Intertheoretic reduction: a new look at an old project

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A Thesis Submitted for the Degree of Doctor of Philosophy

Department of Philosophy

Durham University

2007

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Abstract

This thesis argues that the core idea of intertheoretic reduction in science is still defendable. The thesis is divided into three parts. In Part One, comprising five chapters, a positive account of reduction is discussed. In Chapter 1, Nagel's classic account of reduction is considered. There are two central themes in this survey: Nagel's account of bridge principles and his non-formal conditions. The former shows that bridge principles are not limited to identity statements, while the latter shows that almost all later philosophers (whether reductionists or anti-reductionists) have ignored these central conditions of reduction.

In Chapter 2, the first wave of objections to Nagel's account, including the problems of meaning variance and inconsistency, are central issues. It will be argued that the former is not a serious objection. However, the latter leads us to Chapter 3, in which reductionists' responses to the problem of inconsistency are considered.

The main tool used to remove the problem of inconsistency is the notion of approximation. I defend approximate reduction, but it needs further metaphysical clarifications to survive the second wave of objections. Before these metaphysical points are addressed, the rest of Chapter 3 is devoted to showing that identity statements between things are one kind, but not the *only* acceptable kind, of bridge principle, and that identity statements between properties are not required for reduction.

The central notion in the second wave of objections against reduction is 'multiple realization'. In Chapter 4, I present a causal analysis of properties and realization, and show that (a) a version of the unity of science is defendable within the causal framework, and (b) arguments against the possibility of general and autonomous special sciences are not valid.

In Chapter 5, an alternative flat analysis of properties and realization, based on the notion of similarity, is discussed. Firstly, I argue that this alternative can save a version of the unity of science. Secondly, I show that both of the discussed metaphysical analyses remove the second wave of objections against reduction.

Part Two, comprising three chapters, concerns negative accounts of reduction. In Chapter 6, explanatory reduction is critically analyzed. I argue that the contrastive nature of explanation prevents us defining reduction in terms of explanation.

In Chapter 7, those accounts of reduction which define it in terms of supervenience are discussed. I argue that supervenience is not even a necessary condition of reduction.

Chapter 8 is devoted to considering functional reduction, which defines the reduction of a property to a domain of more basic properties. Based on two possible ways of functionalization, the inadequacy of functional reduction is discussed.

Finally, in Chapter 9 of Part Three, I return to the version of reduction that I defend. This has two important components: approximation and non-formal conditions. First, I sketch a portrait of this account, and then I consider some of its features, such as its aim and relata, its non-formal nature and its direction.

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2006

Declaration

I confirm that no part of the material has been submitted by me for a degree in this or any other university. No material has been generated by joint work. In all cases, material from the work of others has been acknowledged and quotations and paraphrases suitably indicated.

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Acknowledgements

In writing this thesis, I am greatly indebted to Robin F. Hendry, for many insightful conversations during the development of the ideas in this thesis and for helpful comments on the text. I am also indebted to my Japanese friend Daisuke Kaida, who I have learnt many things from. I would also like to thank commentators and participants in the First Lisbon Colloquium for the Philosophy of Science, The Unity of Science: Non-Traditional Approaches, the First Philosophy Graduate Conference at the Central European University in Budapest, and the Fifth European Congress for Analytic Philosophy in Lisbon for their helpful comments and discussions on some parts of this thesis. Also I would like to thank those members of the Durham Philosophy Department who, through their care and patience, have made the writing of this thesis possible.

For financial support, I would like to thank Iran's Ministry of Science, Research and Technology, and the Iranian Institute of Philosophy.

Last but not least, I would like to thank my wife, Sahar, who married me in the first year of my PhD, and without whose kindness, patience, care and encouragement I would not have been able to complete this work. I would like to dedicate this thesis to her.

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Introduction

It is a dominant belief that the basic idea behind the notions of 'reduction' and 'reductionism' is captured by the clause 'nothing more than ....' For example, if we say that the chemical properties of a substance are reducible to that substance's atomic properties, this means that the former is nothing more than the latter, the former is entirely *dispensable* in favour of the latter, or the latter *absorbs* and wholly *subsumes* the former. In this framework, there is a distinction between 'reductionism' and 'reduction'. Reductionism is a philosophical belief that complex things (objects, properties, theories, meanings...) can *always* be reduced to simpler or more fundamental things. However, reduction is the process by which the reduced thing is shown to be dispensable in favour of the reducing thing. For example, to be a reductionist about scientific theories means two things. Firstly, it means believing that all high-level theories are *always* reducible to more basic theories (and ultimately to the most fundamental theory). In other words, it means believing that high-level theories are *in principle* dispensable in favour of more basic theories. Secondly, it means presenting a process by which high-level theories are reduced to theories that are more basic.

Reductionism can be divided into different types according to the answer that it gives to the question, 'which aspects or features of the reduced thing are dispensable in favour of the reducing thing?' For example, in the case of reduction of scientific theories, we can ask 'which aspect of the reduced theory is dispensable in favour of the reducing theory?' *Methodological reductionism* says that explanations of high-level theories are dispensable in favour of explanations of more basic theories. *Theoretical reductionism* holds that the theoretical assumptions, axioms and laws of the reduced theories are dispensable in favour of theoretical assumptions, axioms and laws of the reducing theories. And *ontological reductionism* says that ontological commitments of the reduced theory are dispensable in favour of their counterparts in the reducing theories.

Answering the question above determines the relata of reduction. Accepting methodological reductionism determines that reduction is a relation between scientific explanations. Similarly, theoretical reductionism says that the relata are theoretical assumptions and laws, while ontological reductionism says that the relata are real-world items. This point shows that we can divide 'reduction' into two main categories: *epistemological* reduction and *ontological* reduction. If the relata of reduction are representational items (theories, concepts, models...), then we have epistemological reduction. In ontological reduction the relata are real world items (properties, events...) (Silberstein 2002, 82). Further classification is possible to sort out different accounts of reduction. As mentioned above, reduction is a process by which the reduced thing is shown to be dispensable in favour of the reducing thing. We can classify accounts of reduction on the basis of the strategies they take towards this process (Silberstein 2002, 82-89). In the case of ontological reduction, the following strategies have been suggested: elimination (reduced things eliminated from our ontology); identity (reduced things identical to reducing things); mereological supervenience (properties of a whole determined by the properties of its parts); and nomological supervenience (fundamental physical laws determine high-level laws). In the case of epistemological reduction, the following strategies have been suggested: replacement (our prior conceptualizations replaced by new ways of describing and conceptualizing); theoretical-derivational (the reduced laws/theories derivable from the reducing laws/theories); semantic/model-theoretic/structuralist analysis (isomorphism between models of the reduced and reducing theories); and pragmatic (the reducing theory about a specific domain provides superior *real-world explanatory* and *predictive* value compared to the reduced theory representing the same domain).

Now let me explain the conception of reduction which I will defend in this thesis, and then compare with the standard definitions above. I consider reduction to be a relation between scientific laws/theories. Therefore, my account is an account of epistemological reduction. Regarding the strategy of reduction, I follow Nagel's *original* suggestion including the derivation of laws *and* the satisfaction of certain non-formal conditions of reduction. Therefore, my account does not belong to the theoretical-derivational category, because, according to this category, derivation is the necessary and sufficient condition for reduction, while in my view, it is only a necessary condition. However, what makes my account radically different is the *aim* of reduction. As mentioned, the aim of reduction in the standard definitions is dispensability/absorption/subsumption of the reduced thing. However, this is not the case in my definition. I do not define reduction of scientific laws/theories as to involve eliminating their concepts/properties or making their explanations dispensable.

In my view, genuine reduction of a scientific law/theory must have epistemic pay-offs, including empirical and historical gains. A valuable reduction either brings empirical achievements (i.e. increases our empirical knowledge, increases degrees of confirmation of laws, or gives clues to new scientific discoveries) or brings historical achievement (i.e. explains why scientists rationally believed in a false theory for a period of time, or why false theories seemed true before discoveries of more comprehensive true theories). On the other hand, as I will argue in detail, even when a law/theory is reduced, its concepts/properties and classifications are not dispensable. Reduced laws/theories can provide explanations that reducing laws/theories cannot. These reasons lead me to say that the aim of reduction is not dispensability, absorption or subsumption. Reduction is an intertheoretic relation that connects laws from different theories and provides epistemic pay-offs. By making local connections between laws/theories, we go in the direction of making the whole of science a connected and unified body. Reduction connects particular laws/theories together, brings epistemic gains and works in the direction of the unity of science. In what follows, we will see how changing the aim of reduction from the dispensability of the reduced items to achieving empirical gains and going toward the unity of science solves many traditional problems with reduction, especially problems with the nature of bridge principles.

Another important difference between my account of reduction and the standard definitions is that I do *not* accept any kind of 'reductionism'. Reduction is a contingent matter, which is sensitive to many formal and non-formal factors, including the reduced and reducing laws/theories and the particular theories available at the time. Therefore, reducibility must be checked case-by-case, and there is no *a priori* reason to think that one kind of law/theory is always reducible to another kind. I will argue that relations such as ontological priority, supervenience, part-whole relation and causation cannot bring about reduction and do not determine the direction of reduction.

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The conclusion of this thesis is that Nagel's *original* account of reduction (presented in Chapter 1), and not later reconstructions of it, has the potentiality to become an adequate account of reduction, although it currently seems out of fashion. However, to remove huge arguments against this account (two of them are discussed in Chapter 2) we have to strengthen it with two supplements. The first one is to introduce the notion of approximation (discussed in Chapter 3). Nagel recognized approximate reduction in his later account of reduction. The second supplement has a metaphysical flavour. We need a metaphysical analysis of the notion of realization (discussed in Chapters 4 and 5). By having firm metaphysical grounds, we will see that the arguments against reduction which appealed to the notion of multiple realization are not catastrophic.

After presenting a positive account of reduction, in the second part of this thesis I will consider some alternative accounts of reduction, and argue that they face serious problems, which do not apply to the version of reduction I defend.

Finally, after giving a general outline of the version of reduction I defend, I will consider some of its characteristics and compare it with an alternative account of reduction.

Part One

A Positive Account of Reduction

1

Nagel's Account of Reduction

Any analysis of the notion of reduction in the philosophy of science should start with Nagel's account. Not because it was the first account, nor because it does not need any amendment, but because it was so influential that most later accounts tried to support, modify, reformulate or reject it. Therefore, to understand the reduction literature, first we have to understand Nagel. Furthermore, I think there are still many important features to whom philosophers of science did not pay attention. Most philosophers simply supposed that Nagel had only one account. However, we will see that he did change some parts of the classic account after criticism. Therefore, it is not *the* Nagelian reduction; there are different Nagelian reductions, and each of them may have different problems. In addition, if we restrict ourselves to his classic account of reduction, there are still many important points to which nobody paid enough attention. Some parts are simply ignored by philosophers and do not appear in any report. Critics did not pay attention to other parts and raised questions that Nagel had explicitly answered. Defenders, who wanted to add some elements and immunize it against criticisms, did not pay attention to its structure and so added elements that are in contrast with the original, and so on. In this chapter, I will start with Nagel's classic account and then in Chapter 3, after discussing some criticisms in Chapter 2, I will say more about his amended account. I think this amended account, supplemented by some metaphysical clarifications, can be the base of a modest account of reduction.

1.1 Prelude

Nagel presents his classic account of reduction between *scientific theories* in chapter 11 of *The Structure of Science* (1961). As a general framework, he supposes that reduction is a kind of *explanation*: 'reduction, in the sense in which the word is here employed, is the explanation of a theory or a set of experimental laws established in one area of inquiry, by a theory usually though not invariably formulated for some other domain.' (Nagel 1961, 338) This general framework says that the aim of reduction is an epistemic achievement (explanation) and not an ontological one (e.g.

economizing our scientific ontology). Therefore, if an activity is directed towards achievements different from explanation of a scientific theory, it is not a reduction. Although, it is quite possible that in going towards explaining a scientific theory, some ontological results may be gained (e.g. identification of entities), but they are by-products of reduction not its central aim.

Nagel divides reduction into two main kinds: homogeneous and heterogeneous. This is a 'language-relative' division (cf. Yoshida (1977, 174)), which is based on homogeneous and heterogeneous vocabularies. Two (descriptive) terms are homogeneous if their meanings are (approximately) the same, and are heterogeneous if their meanings are different (Nagel 1961, 339). The criterion is *meaning* here. Therefore, we need to consider Nagel's account of meaning. However, let us first explain his strategy for dividing reduction into two groups. Suppose that *T1* is the primary theory (science) and that we want to reduce *T2* as the secondary theory (science) to it. (Hereafter we call *T1* the 'reducing theory' and *T2* the 'reduced theory'.) Reduction of *T2* to *T1* is homogeneous if the laws of *T2* employ no descriptive terms that are not also used with approximately the same meaning in *T1*. In other words, the reduction is homogeneous if all descriptive terms of *T2* are also used in *T1*, and each of them has approximately the same meaning with its counterpart in *T1*. Alternatively, the reduction is heterogeneous if there are some descriptive terms in *T2* that either are not used in *T1*, or are used with (radically) different meanings (heterogeneous counterparts). As a result, homogeneous theories have homogeneous vocabularies and their reduction is homogeneous. Whereas, heterogeneous theories have heterogeneous vocabularies and therefore their reduction is heterogeneous.

1.2 Theory of Meaning (I)

As mentioned, Nagel's division of reduction into homogeneous and heterogeneous is based on the notion of meaning, and therefore his theory of meaning is at the heart of his reduction account. What is Nagel's account of meaning? Surprisingly, he does not present an explicit account of meaning in his classic work. However, he speaks about meaning and presents some examples that are clear enough to indicate his intuition. Here is one of them.

> Let us now assume that some person has come to understand what is meant by 'temperature' exclusively in terms of manipulating a mercury

> thermometer. If that individual were told that there is a substance which melts at a temperature of fifteen thousand degrees, he would probably be at a loss to make sense of this statement, and he might even claim that what has been told him is quite meaningless. In support of this claim he might maintain that, since a temperature can be assigned to bodies only on the basis of employing a mercury thermometer, and since such thermometers are vaporized when brought into the proximity of bodies whose temperature (as specified by a mercury thermometer) are a little above 350° **C**, the phrase "temperature of fifteen thousand degrees" has no defined sense and is therefore meaningless. (Nagel 1961, 340)

According to this account, term *'t'* is associated with a procedure. We call this procedure the rule of usage for term *'t'*. This rule tells us that if we want to use the term correctly, we have to follow the procedure. In this example, the rule of usage is the operational procedure of measuring temperature. According to Nagel, the meaning of 'temperature' is identical to (or at least determined by) this operational rule of usage. Now what is the place of the referent of 'temperature' in this picture? Naturally, we can say that if, and only if, a quantity satisfies the rule of usage, it is the referent of the term. What is temperature? Temperature is every state of a body that satisfies this operational rule of usage.

Although this particular example concerns an *operational* definition for a scientific term, Nagel does not restrict rules of usage only to operational ones. Consider the situation of 'temperature' in elementary physics. Here this term has more than one rule of usage; it has a set of rules. One of them is still operational and expresses the relation between the behaviour of instruments such as thermocouples and the state of physical bodies that we call temperature. However, others are not operational. For example, there are some physical laws that express the relations between temperature and other physical states such as volume expansion or electrical resistance. Nagel considers all of them as rules of usage. Therefore, there is a set of rules of usage for the term (including operational, observational and theoretical). This set determines the meaning(s) of the term and every entity that satisfies this set is the referent of the term. (We shortly return to this point and consider its problems.)

Let us consider this theory of meaning in more detail and see how it can affect an account of reduction. The first point is that meaning of a scientific term is fixed only by the theory that it has been appeared in.

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

This point implies that every science determines the meaning of its terms by itself. From the fact that one term is used in two sciences, it cannot be concluded that these two usages have the same meaning and are synonymous.

The second point is a question about the meaning of a term inside a single theory. Does a theory determine only *one* meaning for each term? For example, as we saw, 'temperature' has different rules of usage in the science of heat and so has different meanings there. Are they synonymous? Nagel answers no.

> Accordingly, although 'temperature' is explicated in the science of heat both in terms of theoretical and of observational primitives, it does not follow that the word understood in the sense of the first explication is synonymous with 'temperature' construed in the sense of the second. (Nagel 1961, 351)

This consequence of Nagel's theory of meaning, however, is unclear and in one sense problematic. Suppose that in the science of heat we have two different meanings for 'temperature': 'temperature 1' determined by an observational rule of usage; and 'temperature 2' determined by a theoretical. These two terms, for which we use the same word, are heterogeneous, because according to Nagel their meanings are different. Given Nagel's theory of meaning that rules of usage determine reference of words, it is possible that these two terms refer to two different and distinct states of bodies. Now if we want to use them interchangeably, for example using 'temperature 2' in premise of a deduction and deducing an observational sentence that contains 'temperature 1', we need some additional hypothesis that says the references of these two terms are the same, although each of them has a different meaning. It is because premises and results of a deduction must refer to the same entity and not independent and distinct ones, although they might carry different meanings of it[1] .

Now the question is, what elements of a theory play this role? For example, what parts of the science of heat tell us that the references of 'temperature 1'and 'temperature 2' are identical? One possible answer is that the theory embodies an empirical claim that, according to it, whenever we measure 'temperature 1' and 'temperature 2' for any particular state of body, they have the same values. This means that if we measure temperature by means of a thermocouple or if we measure other parameters (like electrical resistance) and calculate temperature by means of them, we reach the same values. This solution, although it seems natural, has its own problems. One important problem is that the correlation between two parameters, or even having the same values, does not imply that they refer to the same thing. Temperature has a correlation with electrical resistance but they refer to two distinct physical states. Therefore, we need a stronger condition to ensure that there is only one state of body called temperature, although two terms with different meanings can both refer to it. What this stronger condition might be is a serious question that Nagel's account does not answer.

Nagel's theory of reference has strong similarities with the 'description theories of reference'. The description theories can be divided into two groups: description theory of names and description theory of natural kind terms. Let us start with the former. According to the classical form of the theory, *a* descriptive content is associated with each proper name. This descriptive content determines the reference of the name: every particular object or individual that satisfies this description is the referent of the name. Consider for example 'Aristotle' as a proper name. When I use it, I associate a description with it like 'a Greek philosopher (384–322 B.C.)'. According to the description theory, this determines the reference of 'Aristotle': every person who has this description is Aristotle. In addition to this plain version of the description theory, there is an expanded version that tries to unify issues of reference and meaning (Reimer 2003). According to this expanded version, the meaning of a proper name is the mechanism by which the name refers to its referent, whereas according to the description theory, proper names refer to their referents via their associated descriptions, therefore the meaning of a proper name is the same

[1] It is exactly the role of bridge principles, which Nagel requires them for deducing one theory from another with heterogeneous vocabularies. We will consider them later.

description that is associated with it. The meaning (cognitive content) of 'Aristotle' is the description associated with it: a Greek philosopher (384–322 B.C.).

According to modern description theory, which is often called the 'cluster' theory,

> instead of tying a name tightly to one definite description, as the classical theory does, the modern theory ties it loosely to many. This cluster of descriptions expresses the sense of the name and determines its reference; the name refers to the object, if any, that *most*, but not necessarily all, of those descriptions denote. The theory can be made more sophisticated still by allowing some descriptions to have greater weights in the vote than others. Thus, in the cluster associated with 'Aristotle', doubtless 'the systematizer of syllogistic logic' weights more heavily than 'the son of the court physician to Amyntas II'. (Devitt and Sterelny 1987, 43)

Concerning natural kind terms, the description theory suggests the same strategy. Speakers of the language associate various descriptions with each natural kind term, like 'tiger', 'gold' or 'atom'. According to the classical version of the theory, *one* of these descriptions expresses the meaning of the term and determines its reference. However, according to the modern description theory, most of the cluster of these descriptions express the meaning and determine the reference (Devitt and Sterelny 1987, 67).

Similarly, in Nagel's implicit account of meaning which is mainly focused on natural kind terms, every term has some roles in its own theory. This means that, for every term, we have rules of usage which express relations between this term and other scientific terms, or its operational definitions. These rules of usage have similar function to descriptive contents in the description theory. Firstly, they determine the reference of the term, and secondly they express the meaning of it. Although some minor differences could be found between Nagel's implicit account of meaning and the description theory, their overall structures are similar. Especially if we consider the alternative causal theory of reference, we will see that Nagel's account is more similar to the description theory rather than the causal theory.

Nagel's account of meaning is an important component of his work on reduction but most philosophers did not pay attention to it. In the following chapters when we consider Feyerabend's objections to Nagel's account, we will see that he has a similar but more restricted account of meaning. We will also see that how some other philosophers (e.g. Yoshida) tried to adopt a causal theory of reference and present a

Nagelian account of reduction with different assumptions about meaning and reference[2].

1.3 Homogeneous Reduction

Now let us return to Nagel's account of reduction. As mentioned, he distinguishes homogeneous from heterogeneous reduction. He thinks that because the former kind usually happens in the normal development of a science and is unproblematic, there is no need to discuss it in detail. According to Nagel, homogeneous reduction is nothing more than the deduction of one set of sentences from another, in which both use homogeneous vocabularies. For example, according to Nagel (1961, 339), Galileo's laws of free-falling terrestrial bodies reduce to Newtonian mechanics just by deduction. Because they use homogeneous vocabularies, we can easily obtain the latter from the former without any additional sentences.

Sklar has presented an objection to this kind of reduction.

> If one looks for examples of reduction from the history of science, strictly derivational reductions are few and far between. One can *construe* various relationships of a strictly derivational sort as reductions, but an examination of cases of reduction pre-analytically so-called, shows that even in the case of homogeneous theories reduction is very rarely derivation. (Sklar 1967, 110)

Then he considers the relation between the Galilean laws of falling bodies or the Keplerian laws of planetary motion with Newtonian mechanics and concludes that here, the same and exact reduced theories cannot be derived from the reducing theory[3]. However, according to Sklar (1967, 111), 'what can be derived from the reducing theory is an *approximation* to the reduced, where this notion of approximation is suitably relativised to a degree of accuracy, a range of values of the independent parameters, etc.'

I will consider approximate deduction and its relationship with the DN model of explanation in the following chapters, but let me consider the first part of Sklar's objection in more detail here: homogeneous reductions rarely happened in the history of science. It seems to me that we can strengthen this objection and change its nature

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[2] For general problems with the description theory of reference, see Devitt and Sterelny (1987, Ch. 3).

[3] Duhem (1991, 193) also makes this point, 'the principles of universal gravity, very far from being derivable by generalization and induction from the observational laws of Kepler, formally contradicts these laws. If Newton's theory is correct, Kepler's laws are necessarily false.'

from historical to semantic. Can we have two homogeneous theories *at all*? Consider this simplified example: theory *T2* contains descriptive term *'t'*. This term has only one rule of usage that determines its meaning. According to this rule 't' has a nomic relation with descriptive term *'k'*. Term *'k'* has two rules of usage, two nomic relations with terms *'t'* and *'s'* (Figure 1).

t ......nomic relation........ *k* ......nomic relation........*s*

Figure 1: rules of usage in *T2*

Now suppose that theory *T1* contains terms *'t'*, *'k'* and *'s'* too. If these terms in *T1* want to have *exactly* the same meanings with their counterparts in *T2*, they must enter into *exactly* the same nomic relations in both theories. In other words, if the meanings of terms are determined by their roles in a network of laws, having *exactly* similar roles means being in exactly similar networks. Therefore, *T1* must have the same laws as *T2*. There are two possibilities here. According to the first, *T1* has *only* the same laws that *T2* has. In this case, terms in *T1* and *T2* are homogeneous and we can deduce *T2* from *T1*. However, this is a trivial reduction because *T2* and *T1* are the same.

According to the second possibility, *T1* might have some additional nomic relations, but they are such that they do not change the meanings of these three terms (*'t'*, *'k'* and *'s'*). This means that, they must be nomic relations among other terms, not *'t'*, *'k'* and *'s'*. As an example, let us suppose that *T1* has the nomic relations as Figure 2.

t ......nomic relation........ *k* ......nomic relation........*s*

plus

a ..... nomic relation ..... *b* ..... nomic relation ..... *c* ...... nomic relation ..... *d*

Figure 2: rules of usage in *T1*

In this case, we can have homogeneous reduction between *T2* and *T1*, but the point is that *T1* is not a unified scientific theory in the sense that we know in the history of science. *T1* is in fact a conjunction of two separate and distinct set of laws, *T2* plus *T3*. There is no connection between these two isolated parts. However, our concrete examples show that scientific theories are unified sets of laws, in the sense that each two terms in a theory are somehow connected, directly or indirectly. *T1* in our case is not a unified theory; it is simply a conjunction of two theories without any interconnection.

As a result, a homogeneous reduction by means of the terms that have *exactly* the same meanings in both theories is possible only in two cases. Either the two theories must be the same or the reducing one must be an artificial conjunction of two separate theories. In both cases, reduction is trivial.

However, there is another possibility here. As mentioned, Nagel (1961, 339) defines homogeneous terms as terms that might have *approximately* (not necessarily exactly) the same meaning. For example, we might say that terms *'t'*, *'k'* and *'s'* in Figure 1 have approximately the same meanings with their counterparts in Figure 3.

m......nomic relation........ *t* ......nomic relation........ *k* ......nomic relation........*s*

Figure 3: rules of usage in *T1*

This solution may solve the former problems, but brings about new ones. The notion of 'having *approximately* the same meaning' is vague, and Nagel has not explained whether two meanings are approximately similar or are radically different. Therefore, Nagel's criterion of homogeneous reduction is at best incomplete[4].

More importantly, the notion of similarity among meanings is elusive and vague such that articulating a satisfactory account of it is very difficult. For example, consider the fact that similarity is a relation that comes in degrees: a thing might be more similar to a second thing rather than a third one (regarding a particular aspect). This fact implies that apart from a satisfactory account of similarity, we need a limitation to indicate what extent of similarity is acceptable. For example, apart from articulating a theory about similarity among meanings, we need a criterion to indicate the minimum degree of acceptable similarity for homogeneous reduction.

I will consider the issue of similarity in more detail in Chapter 5, but for our present purpose, it is enough to mention that having a satisfactory account of similarity among meanings and a justified threshold to indicate which amount of similarity is acceptable for homogeneous reduction is a philosophical project, as difficult as the

[4] Hempel (1965, Ch. 4) presents an exposition of empiricists' criteria of cognitive significance of sentences, including Carnap's account that was inherited by Nagel. In this exposition, there is no sign of combining the issue of meaning and the concept of approximation to make 'having approximately the same meaning' legitimate.

reduction project. Fortunately, we do not have to solve both problems at the same time. In Chapter 9, when I summarize my account of reduction, I will argue that the homogeneous/heterogeneous dichotomy does not solve any problem of reduction. We will see that by developing a satisfactory account of heterogeneous reduction we can cover the supposed homogeneous cases. In other words, by rejecting homogeneous reduction and considering any reduction as heterogeneous, and by having a strong account of heterogeneous reduction, we can bypass the difficulties of the notion of similarity among meanings.

1.4 Heterogeneous Reduction: Formal Conditions

Nagel thought that homogeneous reduction is unproblematic and so tried to provide an account of heterogeneous reduction. As mentioned, the aim of reduction is explanation of one theory by means of another. Of course, there are different models of explanation and there are different entities in each theory that can be explained. Nagel adopts the deductive-nomological account of explanation (DN), according to which both explanandum and explanans are sentences and the former should be a logical consequence of the latter. Regarding the entities, Nagel believes that laws of the reduced theory must be explained by the reducing theory:

> … [A] reduction is effected when the experimental laws of the secondary science (and if it has an adequate theory, its theory as well) are shown to be the logical consequences of the theoretical assumptions (inclusive of the coordinating definitions) of the primary science. (Nagel 1961, 352)

But for heterogeneous theories there is an obvious problem here. Some terms are used in *T2* and not in *T1*, and deducing a synthetic sentence about a feature of the world containing terms that are not mentioned in the premises is impossible. I call this 'the standard problem' of heterogeneous reduction, the problem that Nagel tries to solve by suggesting some conditions for reduction. His conditions are divided into two groups: formal and non-formal. We start with the formal conditions.

According to the first formal condition:

> It is an obvious requirement that the axioms, special hypotheses, and experimental laws of the sciences involved in a reduction must be available as explicitly formulated statements, whose various constituent terms have meanings unambiguously fixed by codified rules of usage or

by established procedures appropriate to each discipline. (Nagel 1961, 345)

The second formal condition is about meaning again: the elementary expressions used to construct sentences of a science *S* must be '...employed unambiguously in *S*, with meanings fixed either by habitual usage or explicitly formulated rules' (Nagel 1961, 349). The third formal condition is the core of Nagel's account, with which he wanted to solve the standard problem of reduction. Suppose that the reduced theory contains the term *A* that is not used in the reducing theory. According to the 'condition of connectability', 'assumptions of some kind must be introduced which postulate suitable relations between whatever is signified by '*A*' and traits represented by theoretical terms already present in the primary science' (Nagel 1961, 353-4). However, this condition is not sufficient for reduction and we need the 'condition of derivability': 'With the help of these additional assumptions, all the laws of the secondary science, including those containing the term '*A*', must be logically derivable from the primary discipline' (Nagel 1961, 354).

Additional assumptions (bridge principles) are very important in Nagel's account, and many later objections and amendments were about their nature and role. Before we consider them in more detail, let us mention an important point about them. As mentioned, Nagel's distinction between homogeneous and heterogeneous reduction is based on the notion of meaning. In heterogeneous reduction, we have some terms with different meanings. According to Nagel's account of meaning, because meaning determines reference, it is possible that these terms have distinct and separate referents. The role of additional assumptions is to provide some connections between the *referents*. It is important to notice that hereafter Nagel does not deal with meaning. He accepts that there are some terms with different meanings, and now he wants to find bridges to connect their *referents*. This point shows its importance when we see that some philosophers (e.g. Sklar 1967, 119) claimed that Nagel changes heterogeneous theories to homogeneous ones in the first place, and then uses the homogeneous account of reduction to reduce one of them to another. It is not true. Nagel accepts heterogeneous vocabularies, and does not want to make meanings of two heterogeneous terms similar by additional assumptions. He wants to connect their *referents* in some way.

In his classic account, Nagel enumerates three possibilities for the states of bridge principles: they might provide logical, conventional or factual (material) connections. If a bridge principle is a logical connection, it says there is a logical relation (presumably synonymy or some form of one-way analytical entailment) between established meanings of two expressions (Nagel 1961, 354). On this alternative, because the meaning of an expression from the secondary science is explicable in terms of the established meanings of theoretical terms in the primary science, and because the meaning of a term fixes its reference, therefore there is a connection between the referents of the two expressions via the bridge principle.

A bridge principle might be a conventional connection as well (Nagel 1961, 354). This case can be understood well if we suppose that *'A'* is an observational term in *T2*, and by a convention, we connect it to a theoretical expression in *T1*. By this connection, we assign an experimental significance to a certain theoretical term, and say it refers to a state of body that *'A'* as an observational term refers to as well. It is important to notice that it is not an arbitrary connection; it should be consistent with other such assignments that may have been previously made. We can see again that the meanings of two terms are still different here, whereas we have a connection between their referents.

The third kind of bridge principle is factual (material). Here we have '... physical hypotheses, asserting that the occurrence of the state of affairs signified by a certain theoretical expression '*B*' in the primary science is a sufficient (or necessary and sufficient) condition for the state of affairs designated by '*A*'' (Nagel 1961, 354). Nagel (1961, 354-5) puts a restriction on this bridge principle, 'It will be evident that in this case independent evidence must in principle be obtainable for the occurrence of each of two states of affairs, so that the expressions designating the two states must have identifiably different meanings.' In Chapter 3, we will see that Nagel, in his later account of reduction, restricts bridge principles to only the factual ones. Before we go on to consider the second group of conditions (non-formal), let us consider bridge principles in more detail, and see how some philosophers misunderstood their nature.

1.5 Bridge Principles

In this section, I am going to clarify the notion of bridge principles and mention some misinterpretations of them. The first point concerns the *number* of components in a bridge connection. The essential role of a bridge principle is to provide some connection between the referents of two heterogeneous terms. This role may be played by different kinds of connections. Therefore, as long as a connection (or a set of connections) plays this role, it must be accepted as a bridge principle. However, Nagel restricts a bridge connection only to *one* bridge principle and ignores the possibility of a *set* of bridge principles instead of one. In other words, Nagel presupposes that, for connecting two terms, we always need only one bridge principle and not a set of them. For example, when he considers the factual connection, because he has only one bridge principle in mind, he says that the bridge connection must express either *'B' (from the secondary science) is sufficient for 'A'* (from the primary science) or *'B' is necessary and sufficient for 'A'*. However, it is possible that instead of one connection, we have a set of bridge principles in one case, and accordingly it is possible that in one of them *'B'* is a *necessary* condition for *'A'*. Hempel (1969, 188) presents this example: suppose that the primary science includes this law,

$$(x)\ (B1x \rightarrow B2\,x)$$

and the secondary science includes this one:

$$(x)\ (A1x \rightarrow A2\,x)$$

Now the set of bridge principles might have this form:

$$(x)\ (A1x \rightarrow B1\,x)$$

$$(x)\ (B2x \rightarrow A2\,x)$$

Here the secondary science can be derived from the primary science and the set of bridge principles and we have connections between referents of our terms in bridge principles. However, in one bridge connection the term of the primary science (*'B1'*) is a necessary (neither sufficient nor necessary and sufficient) condition for the term of the secondary science (*'A1'*). Therefore, it seems that restricting the number of bridge principles to one and mentioning the logical form of factual connections is not necessary for Nagel's account, and the general requirement that they must provide some connections between referents of the terms is enough.

Now let us turn to an obvious point that caused many philosophers to misunderstand Nagel's account. It is clear that because of the standard problem of heterogeneous reduction, we need some kind of bridge principles. This means that, the secondary theory can be deduced from the primary one ***plus*** bridge principles. Are these bridge principles parts of the primary theory? Nagel gives a negative answer to this question. In his case study of the reduction of the Boyle-Charles' law in thermodynamics to statistical mechanics, he says,

> ... the Boyle-Charles' law cannot be deduced from the assumptions of statistical mechanics unless a postulate is added relating the term

> 'temperature' to the expression 'mean kinetic energy of molecules'. This postulate cannot itself be deduced from statistical mechanics in its classic form .... (Nagel 1961, 372)

In other words, according to Nagel, the laws of secondary science can be deduced from the laws of primary science ***plus*** some additional and independent assumptions, and the whole of this activity is called reduction. Therefore, it is an absurd attempt to discuss whether the reduced theory can be derived *only* from the reducing theory or not. Yoshida (1977, 1) has this point in mind when he says, 'I mention this now because some have thought it worthwhile to discuss the question of whether the less comprehensive is derivable from the more comprehensive theory alone. No one would or should be so foolish as to hold such a view.'

However, some philosophers ignored Nagel's explicit answer and tried to criticize his account on this base. Sklar (1993, 8) says, 'But there is a serious objection to this account. If the reduced theory follows only from the reducing theory and the bridge laws, why say that a reduction to the reducing theory has occurred at all?' In other words, Sklar thinks that *T2* should be derived only from *T1*, and if something else (bridge principles, *BP*) is added to*T1*, there is no reason to say that *T2* is reducible to *T1*. Therefore, Sklar thinks there is a dilemma here. If we want to reduce *T2* to *T1* alone, all of the bridge principles must be derived from *T1*. However, we know this is not possible and so Nagel's account is not feasible. On the other hand, Sklar says; let us accept that we can have some independent assumptions as bridge principles, now reduction is a trivial activity.

> Doesn't the postulation of bridge laws as usable in reduction in fact trivialize the search for reduction? Can't we always derive the reduced theory from the conjunction of the reducing theory and a new bridge theory that simply posits that if the reducing theory holds, then so does the reduced? (Sklar 1993, 338)

Regarding the second horn of the dilemma, I would like to say that Sklar, like most philosophers, did not pay attention to the non-formal conditions of reduction. Nagel puts some non-formal restrictions on the bridge principles that prevent such naive and trivial cases. We will consider these non-formal conditions later.

Needham (1982, 196) has a similar objection,

> ... they [factual bridge principles] are substantial claims extra to the reducing theory. This is the second major problem: if the theory can't be construed as including them, why say that a derivation of the reduced

> theory from the reducing theory together with bridge laws is a case of reduction?

Then, Needham argues that the kind of bridge principles is not important here. Even if we restrict them to only biconditionals, they are still something additional to the reducing theory and therefore 'extend' it.

How can we reply to this objection? The situation here is similar to the prediction of an observational sentence from a theory. Newtonian mechanics can predict planetary motions, this means that the theory ***plus*** some additional information, like initial and boundary conditions, which are not part of the theory, logically imply some observational sentences about planetary motions. Does it mean, because these sentences are not deducible from *only* Newtonian mechanics, that we do not have a prediction? Alternatively, does it mean that the theory is extended by these additional sentences? Obviously not, because nobody says that premises of prediction must be restricted to the theory or adding initial conditions extends the theory.

Similarly, in the case of reduction, as Nagel says, reducing *T2* to *T1* (by definition) means obtaining the former from the latter ***plus*** some additional bridge principles. Nagel does not restrict the premises to the reducing theory, and as he explicitly says, we need something more.

The next point concerns identity statements. We will see in Chapter 3 that many philosophers after Nagel suggested that bridge principles must be identity statements, asserting an identity relation between things or properties. However, Nagel does not require bridge principles to be identities. In his concrete example to reduce a law from thermodynamics to statistical mechanics, we see a bridge principle that makes a connection between the referent of 'temperature' in the former and the referent of 'mean kinetic energy of the molecules' in the latter, both in the ideal gases. However, this is not an identity statement.

Let us consider this particular example in more detail. Nagel (1961, 344-5) says,

> Let us therefore introduce the postulation that $2E/3 = kT$, that is, that the absolute temperature of an ideal gas is proportional to the mean kinetic energy of the molecules assumed to constitute it.

Unlike some other philosophers such as Kripke (1980, 129), Nagel does not say that temperature is identical with the mean kinetic energy of molecules. Kripke's claim has some serious empirical problems that Needham (1982, 206-10) has considered in detail. For example, the notion of negative absolute temperature makes many physicists dubious about the identity of temperature and mean kinetic energy of molecules. Nevertheless, whatever objections threaten the identity between these two entities, they are irrelevant to Nagel's claim about a proportional relation between temperature and the mean kinetic energy of molecules *only* in the ideal gases.

Needham (1982, 209), however, extended his objection and tried to cover Nagel's claim as well.

> There remains a problem even in the special case of the ideal gas for which a proportionality between temperature and energy is derived from the kinetic gas theory. Considering the average kinetic energy of the constituent molecules accommodates the fact that the molecules don't all have the same energy, and gives us a property which, like temperature, is a property of the entire gas but not of the individual molecules. But this leads to the problem that the average kinetic energy might be defined when the gas doesn't have a temperature at all. Such situations obtain when the *distribution* of energy isn't that of an equilibrium distribution- i.e. a distribution corresponding to an equilibrium state, when all parts are at equilibrium with one another. (The notion of temperature is introduced in thermodynamics by providing a basis for maintaining that bodies at equilibrium with one another have the same temperature.) Accordingly, even where temperature does correspond to a definite value for the average kinetic energy of the constituent particles, mere possession of this microfeature is at best only a necessary condition for an ideal gas having a particular temperature. The question is whether this necessary condition can be converted into a sufficient condition by a suitable further qualification without circularity- without presupposing the very notion of temperature which was to be reduced.

In reply to this objection, I would like to emphasise that Needham might have some reasons for converting the necessary condition into a sufficient one. However, whatever these reasons are, they are irrelevant to Nagel's model. According to my interpretation of Nagel (see above), logical forms of bridge principles are not important; the main aim is to provide some connections between the referents, by means of which the secondary science can be deduced from the primary science. This connection, as Hempel showed, may contain a necessary condition. Therefore, the mere fact that we have a necessary condition is not an objection against Nagel's model. If we can deduce *T2* from *T1* by using this necessary condition, it is enough for the purpose of reduction. As a result, Nagel's example is not an identity relation and as he explains, it is either a physical (factual) or a conventional connection[5]. Furthermore, even if we show that having a particular microfeature is only a necessary condition for having a particular temperature in the ideal gases, this does not prevent us *in principle* from reduction. This necessary condition might appear in a set of bridge principles and might facilitate reduction.

The last point about bridge principles is that they are not necessarily bridge *laws,* if we understand laws as empirical claims. As mentioned, Nagel presents three possibilities for a bridge principle. Under two of them, they are clearly not laws, but logical and conventional connections. Many philosophers, including Nagel in his later works, restricted bridge principles to 'bridge laws'. However, in his classic account Nagel does not insist on laws. In Chapter 3, we will see that even if logical and conventional connections are rejected as bridge principles, there are other kinds of bridge principles (e.g. initial conditions) that are not laws. In Chapter 5, I will argue that the essential requirement for a factual bridge principle is to have nomic necessity, not being a law.

1.6 Heterogeneous Reduction: Non-formal Conditions

Apart from formal conditions, Nagel presents some non-formal conditions for reduction. As far as I know, almost all philosophers who wanted to summarize, criticize, amend, apply or reformulate this account, simply ignored the non-formal conditions[6]. It is a dominant belief that Nagel's account of reduction is purely formal and that the formal (logical) structure of theories is sufficient to decide whether the theories are reducible or not. However, this is not true at all. Nagel's account is a non-formal one, which, in addition to logical restrictions, puts non-formal conditions on reduction. Nagel (1961, 358) sees reduction as a genuine scientific activity and thinks that formal conditions cannot separate trivial reduction from noteworthy scientific achievements. I will consider non-formal aspects of reduction in the last

[5] It is interesting to note that according to Nagel (1961, 356-7), a bridge principle can have more than one cognitive status and these statuses are not necessarily inconsistent. In one context, we can assume the relation of temperature and the mean kinetic energy of molecules as a physical hypothesis, and in another context, we can assume it as a conventional one. In the former case, we calculate the value of the mean kinetic energy of molecules in some indirect fashion from experimental data other than that obtained by measuring the temperature. This measurement shows that the temperature is proportional to the value of the mean kinetic energy. In the latter case, according to a coordinating definition we connect the experimental concept of temperature to the theoretical concept of the mean kinetic energy.

[6] See for example, Feyerabend (1962), Schaffner (1967), Sklar (1967, 1993), Nickles (1973), Yoshida (1977), Hooker (1981), Needham (1982), Kim (1990) and Smith (1992).

chapter. Here I only mention Nagel's non-formal conditions and show how these conditions cast light on the notion of reduction.

Nagel has three non-formal conditions. According to the first, the primary science must not be *ad hoc*.

> If the sole requirement for reduction were that the secondary science is logically deducible from arbitrary chosen premises, the requirement could be satisfied with relatively little difficulty. In the history of scientific reductions, however, the premises of the primary science are not *ad hoc* assumptions. Accordingly, although it would be a far too strong condition that the premises must be known to be true, it does seem reasonable to impose as a nonformal requirement that the theoretical assumptions of the primary science be supported by empirical evidence possessing some degree of probative force. (Nagel 1961, 358)

Nagel mentions two kinds of empirical support for the primary science. Obviously, the first kind is those supports that *T1* had them before we consider reducibility of any other theory to it. This means that before we went to discover reduction relation, *T1* was an established theory with different empirical supports, including both direct and indirect. The second kind of support for *T1* is provided after reduction of *T2* to it. For *T1* implies the established, and even new laws of *T2*, supportive evidence of *T2* is now supportive for *T1* as well. In other words, by reduction, we go towards a kind of unity between different parts of science. In the case of reducing thermodynamics to the kinetic theory of gases Nagel (1961, 359) says,

> But the point having greatest weight in this connection is that the combined assumptions of the primary science to which the science of heat was reduced have made it possible to incorporate into a unified system many apparently unrelated laws of the science of heat as well as of other parts of physics. A number of gas laws had of course been established before the reduction. However, some of these laws were only approximately valid for gases not satisfying certain narrowly restrictive conditions; and most of the laws, moreover, could be affirmed only as so many independent facts about gases. The reduction of thermodynamics to mechanics altered this state of affairs in significant ways. It paved the way for a reformulation of gas laws so as to bring them into accord with the behaviour of gases satisfying less restrictive conditions; it provided leads to the discovery of new laws; and it supplied a basis for exhibiting

relations of systematic dependence among gas laws themselves, as well as between gas laws and laws about bodies in other states of aggregation.

Therefore, we can summarize that reduction is a genuine scientific activity in Nagel's account, which goes towards the unity of science. This unity makes the primary science more supported, gathers and connects laws of the secondary science (especially approximately valid laws) together, gives more support to the secondary laws, gives opportunities to discover new laws, and finally shows a systematic dependence between the two theories. By having this non-formal condition, we can consider one of the objections that some philosophers raised against Nagel. In the case of reduction of the Boyle-Charles law to the kinetic theory, Nagel suggests this factual bridge principle:

(*) $2E/3 = kT$

(Which in it E is mean kinetic energy of molecules, T is absolute temperature and k is a constant)

Now this factual claim must be testable. How can we test it? Ager, et *al.* (1974) raised their objection on the basis that it was impossible to test (*). According to them, we can only deduce the Boyle-Charles law or some other well-confirmed laws from (*) plus kinetic theory, and because these laws are well-established empirical ones, their derivation cannot be regarded as testing (*).

> But because the Boyle-Charles law is highly confirmed independently of kinetic theory, the fact that it is deducible from kinetic theory and (*) gives no entrance for testing (*). Indeed, even if other well established empirical laws are deducible from other parts of kinetic theory and (*), the structure of these derivations would similarly insulate (*) from falsification. So long as bridge laws like (*) are conceived of as statements which enable the derivation of well confirmed macroscopic laws from a micro-theory, their adequacy is decided on logical rather than experimental grounds. The reason that [$2/3E^2 = kT$] is an inadequate bridge law is not that it is falsified by experimental work, but that it does not lead to the Boyle-Charles law. (Ager, *et al.* 1974, 120-1)

These authors did not recognize that Nagel had accepted this point and explicitly mentioned it.

> If the Boyle-Charles' law were the sole experimental law deducible from the kinetic theory of gases, it is unlikely that this result would be counted

> by most physicists as weighty evidence for the theory. They would probably take the view that nothing of significance is achieved by the deduction of only this one law. ... Moreover, physicists would doubtless call attention to the telling point that even the deduction of this law can be effected only with the help of a special postulate connecting temperature with the energy of gas molecules– a postulate that, under the circumstances envisaged, has the status of an *ad hoc* assumption, supported by no evidence other than the evidence warranting the Boyle-Charles' law itself. (Nagel 1961, 359)

However, in the actual and significant scientific reductions we have two sets of supportive evidence that change this situation. 'One set consists of experimental laws, deduced from the theory, which have not been previously established or which are in better agreement with a wider range of facts than are laws previously accepted' (Nagel 1961, 360). For example, in the case of thermodynamics, Nagel says that, by adjusting our bridge principles, apart from the Boyle-Charles law, we can deduce the van der Waals law, which is applicable to both ideal and non-ideal gases. Therefore, these two laws are now connected to each other by means of a single theory. This scientific achievement of reduction provides some supportive evidence for both the primary science and the bridge principles.

> In general, therefore, for a reduction to mark a significant intellectual advance, it is not enough that previously established laws of the secondary science be represented within the theory of the primary discipline. The theory must also be fertile in usable suggestions for developing the secondary science, and must yield theorems referring to the latter's subject matter which augment or correct its currently accepted body of laws. (Nagel 1961, 360)

The second kind of supportive evidence is the intimate and frequently surprising relations of dependence that obtain between various experimental laws of two theories by means of reduction. For example, we can show that the different experimental laws of *T2*, which before reduction had been regarded as independent and separate with independent evidential grounds, are now deducible from an integrated theory. Another example concerns the numerical constants that are appeared in different experimental laws of *T2*. By means of reduction, we can show that they are definite functions of theoretical parameters in the primary science.

In summary, those philosophers who criticized Nagel for the impossibility of testing factual bridge principles did not pay attention to the first non-formal condition of reduction, according to which reduction must be a significant scientific achievement, which goes beyond mere deduction of the established laws of the secondary science. Reduction should provide some suggestions for development of the secondary science, and this development provides the possibility of testing both the primary science and the bridge principles.

The second non-formal condition is about the stage of development of theories. The question of reducibility cannot be answered without considering this parameter. The general idea is that scientific theories do not remain unaltered during the development of scientific disciplines and '[a]ccordingly, the question whether a given science is reducible to another cannot in the abstract be usefully raised without reference to some particular stage of development of the two disciplines' (Nagel 1961, 361). For example, Nagel says, thermodynamics is reducible to mechanics only if the latter has postulates about molecules and their modes of actions. This means that thermodynamics is only reducible to mechanics after 1866, the year that Boltzmann gave a statistical interpretation for the second law of thermodynamics with the help of some statistical hypotheses, and it was not reducible to the mechanics of 1700. This point says that the question 'whether thermodynamics is reducible to mechanics or not?' does not have an absolute answer. Reduction needs some logical apparatus, and, if it exists, reduction is possible. According to Nagel, reducibility or irreducibility of one theory to another is not a sign for their ontological connection or separation; it is a mere logical fact and does not relate to the immutable structure of the universe (Nagel 1961, 363-4).

Nagel presents another point in connection with the second non-formal condition. Because reduction must be a significant scientific achievement, it is possible that despite the fact that formal conditions are satisfiable in a particular case, we do not try to reduce one science to another at a certain period of their development.

> Attempts to reduce the discipline to another (perhaps theoretically more advanced) science, even if successful, may then divert needed energies from what are the crucial problems at this period of the discipline's expansion, without being compensated by effective guidance from the primary science in the conduct of further research. For example, at a time when the prime need of botany is to establish a systematic typology of

> existing plant life, the discipline may reap little advantage from adopting a physicochemical theory of living organisms. (Nagel 1961, 362)

The moral of the story is that, according to the current stage of development of *T1* and *T2*, we can make a strategic decision to reduce *T2* to *T1* or not. Whatever this decision is, it has a temporal character and the reducibility or irreducibility is temporally qualified, and so does not say anything about the ultimate structure of the world.

The second non-formal condition helps us to see the non-formal nature of Nagel's account more clearly. According to this account, apart from two theories and their logical relations, some *external* factors are relevant in reducibility. This means that even if formal conditions are satisfied, some non-formal considerations such as the time of reduction or the empirical significance of it, must be taken into account. We might have no reduction between two theories in a particular period, but have reduction between them some time later. This point leads us to distinguish two categories of reduction that I call 'reduction with necessity' and 'reduction without necessity'. According to the former, formal considerations (in particular formal relations between two theories or between their elements) imply a *necessity* for reduction. We consider two theories formally, and if some formal conditions are satisfied, one of them is *necessarily* reducible to another. If the reduction is not achievable now, it is available *in principle*, and we will obtain it in the future when we discover more about the field of study. As an example, consider those accounts of microreduction; according to them, if a theory deals with macro-behaviours, macro-states and macro-properties of a system, it is *necessarily* reducible to theories that are dealing with micro-behaviours, micro-states and micro-properties of that system. If such a necessary reduction is not achievable now, it is achievable *in principle*. As this example shows, here some formal considerations (the micro/macro relation) imply that reduction is necessary[7].

However, in 'reduction without necessity', no formal consideration can imply the necessity of reduction. Nagel's account belongs to this category. According to him, even if all the formal conditions of reduction are satisfied, some non-formal conditions might prevent us from reduction. Believing in reduction with necessity is to believe in a kind of reductionism, which I defined in the introduction. In Chapter 9,

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[7] Another example is reduction by means of supervenience, which I will consider in Chapter 7. Here the formal consideration of supervenience between two sets of properties implies that the theory deals with the supervenient set of properties is necessarily reducible to the theory that deals with the subvenient set.

I will explain the version of reduction that I defend. This is a reduction without necessity. I do not commit to reductionism.

Nagel's third non-formal condition is a general remark about the nature of reduction, more of a real condition. Nagel emphasises that reduction is the deduction of one set of *sentences* from another. In reduction, we derive *sentences* of one theory from another and not *properties* of one subject matter from properties of another. This point is important when we find out that some emergentists (e.g. Broad 1925, 59) rejected reduction, because according to them we cannot even in principle, obtain or deduce novel properties of systems from the most complete knowledge of the behaviour of the components. In reply, Nagel presents two answers. First, we do not have direct access to properties and nature of things, unless we have certain explicitly formulated theories that embody those properties.

> ... the "nature" of things, and in particular of the "elementary constituents" of things, are not accessible to direct inspection and that we cannot read off by simple inspection what it is they do or do not imply. Such "natures" must be stated as a theory and are not the objects of observation... (Nagel 1961, 364)

Therefore, the emergentists claim changes to a new claim that we cannot reduce theories which embody novel properties of systems to theories that embody properties of the components. Nagel's second point says that if we cannot reduce one theory to another, this has only a formal reason and not a metaphysical one. Two theories are irreducible simply because there are no suitable bridge principles to connect them. However, this does not mean that the properties of these two theories are ontologically irreducible. We might find new theories embodying these properties, while they are reducible.

> Accordingly, whether a given set of "properties" or "behavioral traits" of macroscopic objects can be explained by, or reduced to, the "properties" or "behavioral traits" of atoms and molecules is a function of whatever theory is adopted for specifying the "natures" of these elements. (Nagel 1961, 365)

Irreducibility of two theories does not show anything about the nature of the world and its hierarchy. We might have reduction if theories dealing with properties are changed (as thermodynamics became reducible to mechanics only after 1866).

1.7 Nature of Nagel's Account

I finish this chapter by two general remarks about Nagel's account. First, Nagel's original model is suitable only to reduce *true* theories, (those we currently use), or *approximately true* theories (those that are strictly speaking false, but are approximately true in a narrow domain of application). It cannot be used to reduce false theories to more comprehensive theories that replaced them (e.g. phlogiston and Lavoisier's theory). There are two reasons for this claim. First, because the primary science is true and bridge principles either are true or are approximately true (true in a narrow domain), therefore the secondary science that is derived from them is either true or approximately true. Second, because a significant reduction must provide usable suggestions for developing the secondary science (the first non-formal condition), the secondary science must be in use. This means that the secondary science is either true or approximately true. Yoshida (1977) tried to extend Nagelian reduction to cover false theories. I will discuss this possibility in Chapters 3 and 9.

The second remark concerns the point that reduction of *T2* to *T1* does not mean that *T2*'s classifications and kinds are illusory, and we can eliminate them by means of *T1*'s classifications and kinds. Nagel is not an ontological eliminativist. The reason is that by reducing *T2* to *T1*, we find some explanation for the occurrence of the states and behaviours that *T2* deals with. However, this is an explanation, neither a logically necessary connection nor a synonymy relation between two states (Nagel 1961, 366). By this explanation, we assert some conditions, formulated by means of the primitives of the primary science, under which a state from the secondary science *contingently* takes place. The key point is that Nagel takes the relation between properties of the secondary and primary sciences to be a *contingent* relation[8]. His bridge principles are at most coextension relations, rather than property identities. Therefore, he concludes that, although the primary science can imply the secondary one, we still recognize properties of the secondary science as real. Hempel recognizes the point that satisfying Nagel's conditions of reduction does not conclude that we reduce concepts of the secondary to concepts of the primary science and eliminate them from our ontology.

[8] As far as I can see, there is an inconsistency in Nagel's account here. As we saw earlier, he accepts that a bridge principle might be a *logical* relation, i.e. a synonymy or an analytic relation. However, here he insists that bridge principles are *contingent* relations. I have not found any attempt to reconcile these two claims. Perhaps this is the reason that Nagel in his later account of reduction restricted bridge principles only to factual connections and ignored logical and conventional connections.

> Even a set of connective principles powerful enough to permit a reduction of all the principles of a biological theory ***B*** to those of a physicochemical theory ***P*** need not permit a corresponding reduction of concepts; i.e., it need not provide a characterization of every concept of ***B*** by means of concepts of ***P***. (Hempel 1969, 189)

This point is worthy of further clarification. One possible kind of bridge principles is identity statements. Here we assert an identity between two types (kinds), concepts or properties of the primary and the secondary science. For example, we say concept *c2* from the secondary science is identical to (is nothing more than) concept *c1* from the primary science. An identity relation is necessary, although it might be known *a priori* or *a posteriori*. Some philosophers believe that reduction must be achieved by means of identity statements that identify each kind of *T2* that is not used in *T1* with a kind of *T1*. We can call this 'type-type' identity, by means of which we eliminate *T2*'s ontology and replace it with *T1*'s. However, it is important to notice that Nagel's reduction is not so. Even if we restrict bridge principles only to factual (material) connections, they are not identity relations. Identity relations are necessary, whereas Nagel says that factual relations are contingent. Factual connections assert that an occurrence of what is referred to by a term of the primary science is sufficient or necessary and sufficient (or according to my interpretation only necessary) for occurrence of what is referred to by a term of the secondary science. Moreover, this relation might hold only in a narrow domain. For example, Nagel says 'temperature' and 'the mean kinetic energy of molecules' are coextensive *only in the ideal gases* and not in other states like phase changing, and this is enough for the purpose of reduction as long as we want to reduce a law about the ideal gases (the Boyle-Charles law) to the basic theory. Consequently, Nagel's bridge principles, even if we restrict them to factual hypotheses, are not type-type identities. We still commit to the ontology of the secondary science after reduction[9].

[9] In Chapter 6, I will show that some reduced theories can provide explanations that are not obtainable by reducing. This is another reason why we need reduced theories as well as reducing ones.

2

The First Wave of Objections

Nearly one year after Nagel published his classic account of reduction; P. K. Feyerabend (1962) raised some objections on it. These (and others presented by other philosophers with the same nature) made the first wave of objections on Nagel's account. In reply, reductionists made two kinds of response. One group tried to criticise Feyerabend's presuppositions and arguments and show their inadequacy. The other group, although they did not accept Feyerabend's arguments, found important points in them, and so modified their accounts of reduction. In this chapter, I will start with Feyerabend's basic presupposition about the nature of meaning and an exposition of his holistic account of meaning, which has a central role in his criticisms. Then I will consider some problems with it and show that, aside from the debate over reduction, it is not an adequate account of meaning for scientific terms. After that, I will examine Feyerabend's two main objections to reduction: the problem of inconsistency, and the problem of meaning variance. Regarding the latter, I will show that Feyerabend's argument is not clear and has flaws. Moreover, if we ignore the flaws, it is not a serious objection to Nagel's model. The former objection, however, is more powerful and leads us to the next chapter, in which I consider the reductionists' attempts to remove it.

2.1 Theory of Meaning (II)

Feyerabend's account of meaning for scientific terms is similar to Nagel's in one aspect. Both of them believe that every descriptive term used in a scientific theory, has rules of usage that determine its meaning and reference. Therefore, Feyerabend's account, like Nagel's, is in the campus of the description theory of reference.

However, an important difference is that Feyerabend's account is radically 'holistic' or 'contextual'. According to Nagel, a descriptive term (especially an observational descriptive term) might have a rule of usage that is not connected to the theory which the term is used in. In other words, the meaning of an observational term might be determined independently from the theory in hand, for example by an operational rule of usage in a different theory. Meanings of this sort of terms are independent of the theoretical principles of the theory, and therefore can appear in two different theories with two different sets of theoretical principles with the same meaning. (Remember the example of 'temperature' in Chapter 1: its meaning could be determined by an operational definition independently from the theoretical principles of the kinetic theory.)

However, according to Feyerabend, the meaning of each scientific term is *completely* determined by theoretical principles of the theory in which the term appears. As Achinstein summarizes Feyerabend's view,

> 1. A scientific term ***S*** that occurs in a theory ***T*** cannot be understood unless at the least the basic principles of ***T*** are known and understood. (Achinstein 1964, 497)

One logical consequence of this account is that if an old theory is replaced or modified by a new theory, and by doing so, some of its theoretical principles are changed, then common terms in these two theories are not synonymous. Because the rules of usage that determine their meanings include different theoretical principles, they do not have a common meaning. For example, although we use the term 'mass' in both classical and relativistic theories, it has a different meanings in each one, and in fact we speak about two different things by one name. (It might be said that the term 'mass' is ambiguous.) Achinstein puts this logical consequence as follows,

> 2. The meaning of a scientific term ***S*** which occurs in a theory ***T*** will change if ***T*** is modified or if ***T*** is replaced by another theory in which ***S*** also occurs. (Achinstein 1964, 497)

Here is a long quotation from Feyerabend to support these two ideas[10].

> What happens here when transition is made from a theory T' to a wider theory T (which, we shall assume, is capable of covering all the phenomena that have been covered by T') is something much more radical than incorporation of unchanged theory T' (unchanged, that is, with respect to the meanings of its main descriptive terms as well as to the meanings of the terms of its observation language) into the context of T. What does happen is, rather, a complete replacement of the ontology (and perhaps even of the formalism) of T' by the ontology (and the formalism) of T and a corresponding change of the meanings of the descriptive elements of the formalism of T' (provided these elements and this formalism are still used). This replacement affects not only the theoretical

[10] Feyerabend presented these two ideas in almost all of his works. Kuhn (1970, 100-102) also has a similar view about the meaning of scientific terms.

> terms of T' but also at least some of the observational terms which occurred in its test statements. That is, not only will description of things and processes in the domain in which so far T' had been applied be infiltrated, either with the formalism and the terms of T, or if the terms T' are still in use, with the meanings of the terms of T, but the sentences expressing what is accessible to direct observation inside this domain will now mean something different. In short: introducing a new theory involves changes of outlook both with respect to the observable and with respect to the unobservable features of the world, and corresponding changes in the meanings of even the most "fundamental" terms of the language employed. (Feyerabend 1962, 28-9)

2.2 Problems with the Holistic Theory of Meaning

In this section, I will consider some problems with this theory of meaning. First, I will start with a general problem with any description theory that could be applied to Feyerabend's proposal as well. Kripke (1980) enumerates three main problems with the description theories of meaning: the problem of unwanted necessity (sometimes called the epistemic problem), the problem of rigidity (sometimes called the modal problem), and the problem of ignorance and error (sometimes called the semantic problem). According to some people, the second and third problems have more narrow domains of application, while the first problem is more general[11]. In what follows, I will consider only the first problem in detail.

Consider the sentence: 'Aristotle was a philosopher'. Now suppose that for a speaker the associated description with 'Aristotle', which determines its meaning, is 'the last great philosopher of antiquity'. For this speaker, the above sentence says nothing more than 'the last great philosopher of antiquity was a philosopher'. It is clear that

[11] For example, according to Reimer (2003),

> The first and second problems apply only to *expanded* description theories of reference: theories that claim that meaning of a proper name is its reference-fixing description. The third problem applies to the 'basic' versions of the description theory as well: to those versions that claim only that the reference of a proper name is determined by the associated descriptive content, a content which needn't be construed as the name's 'meaning'.

Now the question is whether Feyerabend's account is a basic or an expanded version. There is a reason to consider Feyerabend's proposal as an expanded version: he believes that by changing scientific theories and therefore changing the meanings of their terms, we have a new *ontology*. Therefore, according to him, there is a connection between *meaning* and *reference*. If so, the first two objections may apply to it as well.

such an expression is trivial, analytic, and necessary. However, by saying such a sentence our speaker does not utter a trivial, necessary and analytic truth. For example, he would admit that it was metaphysically possible that Aristotle had not gone to philosophy, and therefore the sentence does not have metaphysical necessity (Reimer 2003).

Now let us change the context of the example and consider the situation of scientific terms. Yoshida has a nice example in this regard. Consider special relativity and a basic equation like '$E= mc^2$'. This is one of our theoretical principles in this theory and therefore, according to Feyerabend, partly determines the meaning of the terms that appear in it: mass, velocity of light and energy. This means that 'mass' in special relativity '... would mean: the measure of the inertial property of a body which satisfies the basic equations of special relativity like $E= mc^2$' (Yoshida 1977, 9). In addition to mass, this situation also holds for energy and velocity of light. Therefore, the equation '$E= mc^2$' would say something like this in special relativity,

> The product of the measure of that inertial property of a body which satisfies $E= mc^2$ amongst other things, and the square of the velocity of light (which velocity satisfies $E= mc^2$, amongst other things) is equal to the energy (which energy satisfies $E= mc^2$ amongst other things). In short, and to put it crudely, $E= mc^2$ would say "those properties that satisfy me satisfy me". (Yoshida 1977, 9-10)

Yoshida says that such an expression is self-referring, necessary, analytic and *a priori*. Whereas, according to him this equation in fact does not refer to itself even indirectly. Yoshida concludes that this false result is a consequence of Feyerabend's false theory of meaning.

Other philosophers have pointed out the same problem. Hempel says,

> Feyerabend's puzzling thesis that successive theories are incompatible and that their key terms differ in meaning may have its root in an overly narrow construal of the idea that terms "obtain their meanings by being part of a theoretical system." If one construes this idea as implying that the meanings of the key terms of a theory must be such as to make the theory true, then the terms are, as it were, "implicitly defined" by the theoretical principles, and the latter are true simply in virtue of the meanings thus assigned to the terms – they are analytic. And a formal incompatibility between two theories with the same key terms must then

be taken to indicate simply that those terms have different meanings in the two systems. (Hempel 1969, 191)

Apart from general problems with the description theory [12], there are specific problems with Feyerabend's holistic theory of meaning[13]. According to Achinstein (1964, 498), Feyerabend's assumptions lead to a paradox. Suppose that I want to understand the meaning of a scientific term. According to Feyerabend, first I have to learn and understand the basic theoretical principles of the theory that the term appears in. It is because they fix the meaning of the term. However, I cannot learn and understand a theory and its theoretical principles before I know meanings of the terms that appear in it. Therefore, we have a circular situation.

This paradox is in fact a disguised version of a 'hermeneutic circle': it is not possible to understand any part of a work until you understand the whole, but it is not possible to understand the whole without understanding all of the parts. In other words, neither the whole (say a text or a theory) nor any individual part (say a phrase or a scientific term) can be understood without reference to one another, and hence, it is a circle. However, it is suspicious to call this a real 'paradox'. In contrast to a *vicious* circle, the concept of hermeneutic circle refers to the dynamic process of understanding, by acquiring more information about the parts, we have a deeper understanding about the whole, and by knowing the whole deeper we understand the parts better. Therefore, what Achinstein's objection can show at most is that we have a *spiral* situation: understanding parts and wholes are mutually dependent. However, this does not imply that the whole process of understanding is impossible.

The second problem (Achinstein 1964, 499) is more interesting. There are cases in the history of science where a new theory exactly *denies* the assumption of the old one. (Achinstein's example is Bohr's theory, which says that the angular momentum and radiant energy of electrons cannot have continuous values, whereas classical electrodynamics says they can have continuous values.) In such cases, meanings of common terms between two theories must be the same. In other words, for one theory to say that a particular entity has one property and the other to deny it, both of them must use the same terms with the same meanings. However, this is not possible

[12] Probably the most powerful argument against description theories of natural kind terms is Putnam's Twin-Earth fantasy (Putnam 1975, 223-7), according to it 'no association of descriptions or mental images will express a sense that is sufficient to determine reference' (Devitt and Sterelny 1987, 70).

[13] Devitt and Sterelny (1987, Sec. 4.2) argue that an alternative causal theory of reference has some virtues over description theories; one of them is that the former avoids the problem of unwanted trivialities, necessities, analyticities and *a priorities*.

according to Feyerabend: here each theory has different theoretical principles and therefore determines the meanings of its terms in a different manner. Consequently, such theories do not speak about similar things and cannot be incompatible. In summary, according to Feyerabend, because meanings of terms are determined by theories, we cannot have two incompatible theories. Incompatibility between two theories means they speak about the same things, but attribute to them different properties. Any change in a theory, according to Feyerabend, changes its ontology and the meanings of its terms. However, we have incompatible theories, and it shows Feyerabend's theory of meaning is not correct[14]. Achinstein formulates this problem under the name of rule B,

> B. It must be possible for two theories employing many of the same terms to be incompatible, i.e., for the principles of one to contain denials of principles of the other. And this presupposes that at least some of the common terms have the same meaning in both theories. (Achinstein 1964, 499)

Feyerabend has replied to this objection. He (1965, 272) presented his own interpretation of Achinstein's rule B, '... I would formulate [rule B] as saying that competing theories must have common meanings'. Let us put aside the fact that it is not an accurate interpretation of rule B, and follow the argument. On the basis of this interpretation, Feyerabend tried to show that in deciding between two competing theories, meanings are not important. We do not consider his method in detail here and simply accept that his method (which is based on making some isomorphism between certain selected semantical properties of some descriptive statements of the two competing theories) works well. However, accepting this method is in contradiction with Feyerabend's key conception of semantic incommensurability.

To see this point let us explore the concept of incommensurability. Two versions of the incommensurability thesis are distinguishable: the semantic incommensurability thesis and the methodological incommensurability thesis. Sankey (1999, 5) formulates the former, which is a consequence of Feyerabend's contextual and holistic theory of meaning and is accepted by him, as follows,

[14] Sklar (1993, 336-7) raises a similar objection, 'Taken to its limit, the doctrine that every change of theoretical assertion generates a change of meaning of the theoretical terms involved leads to paradox, if not absurdity. If one person asserts "*S*" and someone else assert "not *S*," they cannot be contradicting one another, for the terms in "*S*" and "not *S*" have, by the doctrine, different meanings, and the two assertions are not the negations of one another.'

> Because the meaning of the terms employed by scientific theories varies with theoretical context, the vocabulary of such theories may fail to share common meaning. But if theories are unable to be expressed by means of a common vocabulary, the content of such theories cannot be directly compared. For in the absence of a shared, semantically neutral vocabulary, it is impossible for statements about the world asserted by one theory to either assert or deny the same thing as any statement made by the other theory. Theories which are unable in this way either to agree or disagree with respect to any claim about the world are incommensurable in the sense that their content is unable to be directly compared due to semantic variance.

According to the methodological version of the incommensurability thesis, mainly developed by Kuhn,

> … there are no shared, objective methodological standards of scientific theory appraisal. Hence, alternative scientific theories may be incommensurable due to absence of common methodological standards capable of adjudicating the choice between them. (Sankey 1999, 10)

Now let us return to Feyerabend's reply. The point is that if his suggested method exists, i.e. if it is possible to compare the contents of two competing theories, then in the cases where two competing theories are semantically incommensurable, there are ways to compare their contents[15]. However, as mentioned, when two theories are semantically incommensurable their contents cannot be compared. Therefore, if Feyerabend's argument for the possibility of deciding between two theories with no common meanings (including incommensurable theories) is true, then these theories are commensurable. Therefore, Feyerabend can insist on his suggestion only if he gives up the possibility of semantic incommensurability.

The third problem with Feyerabend's account of meaning is that he ignores many different ways that the meaning of a scientific term can be determined, and only mentions determination by means of the role that a term plays in a theory. Achinstein (1964, 500-502) enumerates five possible ways to understand a scientific term. This is possible by (i) knowing an explicit definition for the term, (ii) knowing the derivation of a formula containing an expression denoted by the term, (iii) knowing various characteristics or properties of the item designated by the term, (iv) knowing

[15] I will explain Feyerabend's definition of (semantic) incommensurability in Section 2.4.

the range of application of the term (i.e. the sorts of situations in which it can be employed), or (v) knowing the role the term plays in a theory.

By presenting concrete examples for each case, Achinstein shows that in the first four options there are two alternatives. Sometimes the meaning of a term can be determined without considering principles of the theory that it appears in, and sometimes the only way to understand the meaning is to refer to the theory itself. However, it is only in the last option that we must refer to the theory to understand the meanings of a term.

As an example, according to Achinstein (1964, 502), we understand both the terms 'absolute temperature' and 'pressure' in thermodynamics according to their explicit definitions, i.e. both of them belong to the first category above. However, in the explicit definition of 'absolute temperature' we have to refer to principles of the theory, whereas the definition of 'pressure' is independent from these principles.

Achinstein's examples show that Feyerabend ignores these vast possibilities and only mentions the last option. However, if we consider the real process of scientific enquiry, we will see that some terms can be understood in a theory without referring to principles of that theory, but perhaps by referring to others. This means that, two different theories may have some terms with common meanings.

To sum up, the arguments of this section show that Feyerabend's holistic theory of meaning, apart from reduction debate, is not an adequate theory. It suffers from a general problem (the problem of unwanted necessity), which is avoidable by adopting other alternatives (e.g. the causal theory of reference). More importantly, this theory does not fit well with actual scientific inquires: it cannot account having two incompatible theories, and ignores some possible ways that meanings of scientific terms are determined without referring to the principles of the theories they appear in.

2.3 The Problem of Inconsistency (I)

In this section, I will consider one of Feyerabend's objections to Nagel's account of reduction. Although he only uses one label here, it is worthwhile to note that in fact Feyerabend presents two separate objections under this label. According to the first version, Feyerabend considers particular and paradigmatic examples of reduction in the history of science and shows that the secondary science is inconsistent with the primary and therefore the former is not deducible from the latter. In other words, because the heart of reduction is deduction, and no consistent set of premises can imply an inconsistent set of results, therefore in the alleged concrete examples of reduction we do not actually have reduction.

Consider for example Galilean science and the physics of Newton. A basic assumption in the former is that the vertical acceleration is constant. By some appropriate assumptions, like neglecting the height of a falling body compared with the radius of the earth, we can obtain something *similar* to Galilean laws from Newtonian laws, but what obtained are not the Galilean laws themselves. Although the height of an ordinary falling body in comparison with the radius of the earth is small, their proportion still has some finite value and is not zero. This means that, in what is deduced, we still have inconstant acceleration, and so it is not a Galilean law.

This is the most powerful argument of Feyerabend against reduction, which is independent of his theory of meaning, and caused many philosophers to revise their accounts of reduction. In the next chapter, we will see how philosophers have responded to it. There are two main ways of response here. One group accepted that we cannot deduce the same secondary science, but we can deduce a good approximation to it (approximate reduction). The other group discussed bridge principles and auxiliary assumptions, and showed that we have no reason to suppose them to be true. Therefore, by allowing them to be false we can imply the exact secondary science. Let us leave these solutions to the next chapter and consider the second version of the problem of inconsistency here.

The second version of the problem of inconsistency has two parts. First, Feyerabend (1962, 43) says 'according to Nagel reduction or explanation is (or should be) by derivation'. Then he concludes that 'only such theories are admissible (for explanation and prediction) in a given domain which either *contain* the theories already used in this domain, or are at least *consistent* with them' (Feyerabend 1962, 44). After that, Feyerabend presents long arguments to show that the second claim according to the concrete examples, empirical grounds and methodological considerations, is not defendable. By rejecting the second claim, Feyerabend concludes that the first one is false as well, and therefore reduction cannot be done by deduction.

In reply to Feyerabend, it should be noticed that the first claim could not imply the second one alone. It needs some additional premise such as, 'More general theories are always introduced with the purpose of explaining [reducing] the existent successful theories' (Coffa 1967, 505). Feyerabend's formulation (1962, 43) of this additional premise is as follows; 'the task of science, so it is assumed by those who hold the theory about to be criticized, is the explanation, and the prediction, of known singular facts and regularities with the help of more general theories.' However, this additional premise is not true (at least according to reductionists like Nagel), and therefore the falsity of the conclusion is not due to the falsity of the main premise, it is due to the falsity of this additional condition.

There are two reasons to suppose that reductionists like Nagel do not accept and support this additional claim. First, Nagel did not assert such a condition anywhere, and Feyerabend did not refer to any passage as evidence. The second reason is more persuasive, this additional condition is in contrast with Nagel's account of reduction. To see this, reconsider the argument again which can be summarized as below:

> Main premise: explanation and reduction are done by deduction.
>
> Additional premise: *every* general theory should explain the less comprehensive ones (*all* the less comprehensive theories must reduce to the general theory).
>
> Conclusion: *any* general theory must contain or be consistent with the less comprehensive ones.

As we explained in Chapter 1, Nagel's account of reduction does not have *necessity*. He does not say that *every* general theory *necessarily* reduces the less comprehensive ones. According to him, conditions of reduction are not satisfiable in *all* particular cases, and for a particular case, it depends on the development stage of the theories; we do not have reduction between *all* general and less comprehensive theories. Although this is a virtue for a general theory to explain all possible less comprehensive theories, not all general theories have this virtue. Nagel only describes the conditions of reduction, and says that in any case where these conditions are satisfied, we have reduction. In other words, he does not say that all less comprehensive theories are necessarily reducible at all times. Therefore, by taking non-formal conditions of reduction into account, and by realizing the nature of Nagel's account which does not have any necessity, we see that the additional premise and the conclusion are false, whereas the main premise is true[16].

[16] Another piece of evidence showing the additional premise to be false is 'Kuhn-loss.' According to Kuhn (1970, 169), in some cases in the history of science the new theory could not explain some phenomena that the old theory had explained. Therefore, the task of science is not to provide such theories that explain *all* already explained phenomena. See also Coffa (1967, 506).

2.4 The Problem of Meaning Variance

Feyerabend's second objection to Nagel's model is the problem of meaning variance. According to Feyerabend, what we deduce from the primary science must have terms with the same meanings as the original secondary science. In other words, the process of reduction should not affect the meanings of terms of the secondary science and they must be deduced with their original meanings, not with different (although similar) meanings. Whereas reduction is done by means of deduction, and according to Feyerabend deduction does not influence meanings, this demand is an immediate consequence of Nagel's model that 'meanings are invariant with respect to the process of [deduction and] reduction' (Feyerabend 1962, 33). However, it is not a satisfiable demand. According to Feyerabend, the primary science determines the meanings of its terms by its theoretical principles, and because these principles are different from the principles of the secondary science, the meanings of terms are different in both theories. Therefore, in what are deduced from the primary science terms have meanings assigned to them by the science, and because these meanings are different from the original meanings in the secondary science, what is deduced from the primary science is something different from the original secondary science[17].

> We may generalize this result in the following fashion: consider two theories, T' and T, which are both empirically adequate inside D', but which differ widely outside D'. In this case the demand may arise to explain T' on the basis of T, i.e., to derive T' from T and suitable initial conditions (for D'). Assuming T and T' to be in quantitative agreement inside D', such derivation will still be impossible if T' is part of a theoretical context whose "rules of usage" involve laws inconsistent with T. (Feyerabend 1962, 75)

Two types of reply have been suggested for this objection. In the first, the starting point is theory of meaning. By rejecting Feyerabend's theory of meaning, his objection loses its legitimacy as well. However, in the second, the description theory of reference is still valid, but there are some ways to avoid Feyerabend's objection. Yoshida has a reply with a different theory of meaning, and Nagel provides a reply accepting the same theory of meaning. Let us start with the first.

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[17] Cf. Kuhn (1970, 100-102).

Yoshida (1977, 13) believes that there is one principle that both proponents and opponents of meaning variance accepted: common meaning is a necessary condition for deducibility. We can explain this principle like this. Suppose that theory *T* deduces the sentence '*p**', and suppose that *p* is a proposition and '*p*' is the sentence which says that *p*. Now if meanings of '*p**' and '*p*' are different, then the proposition *p* is not deducible from *T*.

According to Yoshida (1977, 14-15), this principle is false for two reasons. First, it presupposes a theory about propositions that Yoshida does not accept. According to this theory, propositions are the meanings of sentences. Consequently, if two sentences have different meanings, then they correspond to two different propositions. However, we have pairs of sentences that make the same statement (correspond to the same proposition), while having different meanings. If a person asserts that 'John is tall', this sentence refers to the same proposition that the sentence that John asserts 'I am tall'. These two sentences, in their given contexts, assert the same proposition, however, no one would say that they have the same meaning.

Second, according to Yoshida, the identity of the referents, not the identity of meaning, is a necessary condition for deducibility,. In other words, even if '*p**' and '*p*' have different meanings, they might express the same proposition, and if we show that they express the same proposition, we can conclude that theory *T* deduces '*p*' in addition to deducing '*p**'.

Now how can we show that two sentences '*p**' and '*p*', express the same proposition? Yoshida says, if '*p**' and '*p*'

> ... are used to claim that the same relations hold between the same referents, regardless of how those referents or indeed the relations themselves are designated and if the terms which do not refer like "is" and "the" ("syncategorematic" terms) have the same meanings. (Yoshida 1977, 15)

Let us consider what Yoshida says in a concrete example. Suppose that the equation "$F= md^2r/dt^2$", with the help of some assumptions such as $(v/c)^2$ being negligible, can be deduced from the special theory of relativity. Here,

> "F" refers to a vectorial quantity which is a measure of that which causes a change in motion and which acts in the direction of motion (this latter is not generally true for the relativistic force, except under the assumption that $(v/c)^2$ is negligible), "m" refers to the measure of that property of a

body in virtue of which it resists a change in motion, and "d^2r/dt^2" refers to the acceleration of the body. (Yoshida 1977, 16)

Now Yoshida says that the proposition which this equation expresses is,

> ... the magnitude and direction of the measure of that which causes a change in motion and which acts in the direction of the acceleration is equal to the product of the magnitude of that property in virtue of which it resists a change in motion and the acceleration of the body. (Yoshida 1977, 16)

This is exactly the same proposition that the second law of Newtonian physics expresses. Now because these two sentences express the same proposition and one of them is deducible from the special relativity, the other one (the second law) is deducible too, although they might have different meanings or their terms might have different meanings. These two sentences assert the same thing, and the identity of the referents is a necessary condition of deducibility, not the identity of meanings. To sum up, by rejecting a presupposition about meaning, Yoshida concludes that reduction is acceptable as long as what is deduced from the primary science refers to the same proposition that the secondary science refers to, although they might have different meanings, or their terms have different meanings determined by two different sets of principles.

There are two points about this solution. First, it is clear that for reducing *T2* to *T1*, Yoshida allows deduction of some sentences from *T1* with different meanings from *T2*. His main demand is that what is deduced from *T1* should express and refer to the same propositions that *T2* refers and expresses. However, Nagel does not accept this interpretation. What is important for him is that if we want to reduce *T2*, what we deduce from *T1* must have the same *meaning* with the original *T2*. Otherwise, we reduce something else, not *T2*.

> ... [I]f thermodynamics is to be reduced to mechanics, it is temperature in the sense of the term in the classical science of heat which must be asserted to be proportional to the mean kinetic energy of gas molecules. (Nagel 1961, 357)

The second point concerns Yoshida's theory of reference. It seems that he is much closer to the causal theory of reference than the descriptive. According to the causal theory of reference, we fix the reference of a name by dubbing it, either by perception or by description. It means that either we perceive an object and dub it by name '*N*', or we stipulate a description and say 'whatever the unique such-and-such is, will be called '*N*'. After fixing the reference, the name is passed on from one speaker to another, but what makes them successful in referring to something by means of its name is that a causal chain, which stretches back to the initial dubbing of the object with that name, links all uses of the name together[18]. Yoshida has something similar to this in mind when he says that '*F*' in the derived equation from the special relativity refers to the same thing as 'force' in Newtonian physics, although they might have different meanings. Consequently, we can say that Yoshida saves the apparent surface of Nagel's model; however, he changes one of the hidden and central ideas inside it, its theory of reference[19].

Now let us consider the second reply to the problem of meaning variance, presented by Nagel (1970) who did not change his theory of reference. As mentioned, Nagel does not accept that meanings of scientific terms are *exclusively* determined by theories. Some terms have common meanings in different theories.

> Accordingly, although both "theoretical" and "observational" terms may be "theory laden", it does not follow that there can be no term in a theory which retains its meaning when it is transplanted into some other theory. (Nagel 1970, 131)

However, it also does not follow that *all* common terms between the primary and secondary science have the same meaning. As we explained in the last chapter, Nagel is aware that some common terms might have different meanings in two theories. Now Nagel's new point is that from the fact that two terms have different meanings in two theories we cannot conclude that they are *incommensurable*. In other terms, the fact that two terms have different meanings is not equal to the fact that their meanings are *radically different*, and they are *incommensurable*.

To see this point and its relevance to our discussion let us start with Feyerabend's definition of incommensurability[20]. According to him, in two cases we have radical meaning change, and therefore incommensurability.

> [We have incommensurability]... either if a new theory entails that all concepts of the preceding theory have extension zero or if it introduces rules which cannot be interpreted as attributing specific properties to

[18] See Reimer (2003, 2.2) and Devitt and Sterelny (1987, Ch. 4 and Sec. 5.2).

[19] In the next chapter, I will say more about other aspects of Yoshida's account.

[20] For criticisms of this definition, see Fine (1967, 234-5).

> objects within already existing classes, but which change the system of classes itself. (Feyerabend 1965, 268)

As a concrete example, consider 'entropy' in thermodynamics that has a different meaning from 'entropy' in statistical mechanics. Now the question is, 'are these two meanings radically different, and therefore the two terms incommensurable?' Nagel directs our attention to the fact that, according to Feyerabend's definition, 'entropy' in thermodynamics and its counterpart in statistical mechanics are not incommensurable.

In thermodynamics, 'entropy' is defined so that it can be applied legitimately to physical systems satisfying certain conditions. As an example, Nagel (1970, 132) says, '[the term 'entropy' in thermodynamics is applicable] ... to systems such as gases, whose internal motions are not too "tumultuous" — the word is Planck's —, a condition which is not satisfied in the case of Brownian motions.' However, in defining 'entropy' in statistical mechanics we do not need these conditions, and therefore, the extension of the notion of 'entropy' in statistical mechanics (the Boltzmann notion of entropy) includes the extension of its counterpart in thermodynamics (the Clausius notion).

This shows that statistical mechanics does not say that the concept of 'entropy' in thermodynamics has extension *zero*. This only shows that the class of systems for which laws of thermodynamics are valid is more restricted than the class for which the laws of statistical mechanics are valid. Therefore, even according to Feyerabend's definition, although these two terms have different meanings, they are not incommensurable.

The second step in Nagel's solution is straightforward. We have two terms which have meanings that are not radically different; there are common extensions between them. Therefore, there might be bridge principles asserting connections between their common extensions. These bridge principles are accessible because two terms have a domain of application in common. Therefore, by means of this common domain of application, if other conditions are satisfied, we can deduce the narrower theory from the wider one. To sum up, according to Nagel, what can block the possibility of reduction is a *radical* change in meaning (incommensurability), not any change. Feyerabend presents examples of meaning change and concludes that because we have meaning change in all theory change, we cannot have reduction at all. Nagel points out that not all meaning changes are radical (according to Feyerabend's definition of radical change). Therefore, we can still save the possibility of reduction in many cases, although the phenomenon of meaning change is dominant.

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We can conclude this chapter by stating the following claims. (i) Feyerabend's holistic theory of meaning is not adequate. It has some internal problems, and cannot cover what we intuitively have in mind when we speak about scientific terms and their meanings. (ii) As for the problem of inconsistency, the second version that we discussed in this chapter does not threaten Nagel's account of reduction. It is based on a premise that is false according to Nagel's non-formal conditions. (iii) As for the problem of meaning variance, there are two possible solutions. By rejecting a common presupposition about meaning and taking a causal account of reference (Yoshida), or by accepting the description theory but distinguishing between different and radically different meanings (Nagel), we can save the possibility of reduction.

3

Reductionists Make a Response: Modifications

In Chapter 2, we examined the first wave of objections to Nagel's classic account. It contained two main objections: the problem of inconsistency and the problem of meaning variance. Regarding the latter which directly depends on the theory of meaning and semantics, I explained that Feyerabend's contextual theory of meaning and his argument for meaning variance are not strong enough to reject Nagel's account. Thus, Nagel does not need any change in his theory to protect it against the problem of meaning variance, and it is enough to clarify the notion of meaning to show that Feyerabend's schema is very radical. However, the problem of inconsistency makes problems that should be solved by modifications to the original account. In the first half of this chapter, I will consider the nature of this objection in more detail and then compare two close but different alternatives to remove it.

The second half of this chapter is devoted to another modification proposed by reductionists: using identity statements as bridge principles. To look at this proposal, first I will distinguish two kinds of identity: identity between things, and identity between properties. While I accept the former, I consider two motivations for proposing the latter: the explanation of bridge principles, and ontological simplicity. It will be argued that there is no need to admit any kind of property identity in reduction. The non-formal conditions of reduction are enough to remove philosophical worries about the explanation of bridge principles. Therefore, Nagel's account does not need new elements for this purpose. Regarding the second motivation, I will argue that by rejecting the semantical analysis of properties and by accepting a plausible account of properties, philosophical worries about ontological simplicity will be removed. By a plausible account of properties, I mean either analysing properties in terms of causal powers or flat ontology, based on the notion of similarity. Chapters 4 and 5 are devoted to these two accounts respectively. Therefore, I will only mention the outline of my argument in this chapter, and leave details to the following chapters. Finally, I will conclude the chapter by reviewing some proposals for the relation between property identity and correlation. This review shows that even if we accept identity as a necessary component of reduction, the controversial question about the way we distinguish identities from correlations is still present.

3.1 The Problem of Inconsistency (II)

It is clear that if we treat reduction as a strict derivation of the exact secondary theory from the exact primary theory plus some *true* bridge principles (let us put the non-formal conditions aside for a moment), then there are few, if any, examples of reduction in the history of science. As Sklar (1967, 110) says, examples of this kind of reduction are few and far between in the history of science, and more importantly this kind of reduction cannot cover those examples that we pre-analytically considered as reduction. Therefore, to apply our philosophical model to real scientific activities we need revisions in the model.

Moreover, there are cases in the history of science where the secondary theory is inconsistent with the primary one, but we still regard pre-analytically as reduction. Hence, whatever is deduced from the primary theory, even if its observational results are numerically close to the results of the secondary science, is different from (and even inconsistent with) the secondary science. For example, according to the special relativity, the mass of an object is a function of its velocity. This means that in whatever is derived from this theory, mass is a function of velocity as well. However, in classical mechanics, mass is independent from velocity. Therefore, two formulas from these two theories might have very close (or even indiscernible) experimental consequences, but they are two different and inconsistent things. The problem of inconsistency says that we cannot derive a result (the secondary theory) inconsistent with the premises from any true and consistent set of premises (the primary theory) plus true bridge principles. Therefore, if we want to cover such examples in the history of science as reduction, we have to revise in our theoretical model of reduction.

Finally, there are examples of theories that we know are false, but for some practical reasons (like simplicity) we still use them in narrow contexts (e.g. Newtonian mechanics is strictly speaking false but we still use it in many branches of engineering). The problem in such cases is that we intuitively want to reduce them to some more general and true current theories, but according to the standard notion of reduction, it is not possible. It is obvious that we cannot validly derive a *false* conclusion from any set of *true* premises.

This point can be extended more. There are some theories (like the caloric theory of heat, or the early phlogiston theories of reduction, calcinations and respiration) that we know are false; however, contrary to the previous cases we do not use them in any field of science. But we might still want to reduce them, in one sense or another, to more general and true theories. Motivations for this kind of reduction might be different, but one of them is the claim of continuity in the history of science. We might want to show that although we have many theory changes in the history of science, all (or at least most) of them follow a common pattern of reduction, and therefore we have a kind of continuity in the history of science. All of these points force us to modify Nagel's account, and if we could cover all of them by only one common pattern of modification, it would be perfect.

3.2 The Role of Approximation

It is commonly accepted by reductionists that the key notion to solve the problem of inconsistency is approximation. Nagel in his revised version of reduction speaks about approximate reduction (1970, 120-1). Kemeny and Oppenheim (1956), Sklar (1967, 112), Schaffner (1967, 144-6), Hempel (1969, 194), Causey (1972 b, 213-7), Yoshida (1977, 2-7), Spector (1978, Ch. 5), Hooker (1981, 46 and 49), Friedman (1982, 27) and Needham (1982, 194-5) appealed to this notion as well. Of course, there are many disagreements among these philosophers about the definition, limits and extension of approximation. For example, Sklar and Friedman believe that we can use approximation only in homogeneous reduction, whereas Yoshida and Hooker extend it to both homogeneous and heterogeneous reductions. Nevertheless, the notion of approximation became a component of every reasonable account of reduction.

Let us first examine what the idea behind this notion is. Apart from Yoshida, who has a systematically different account of approximation, the other philosophers have such a schema in mind: instead of the original secondary theory (*T2*), we derive a different theory (*T2**) from the primary theory (*T1*). *T2** has two important features; firstly, it is logically consistent with *T1* and so strictly derivable from it; secondly, it has a strong similarity (analogy) with *T2*, i.e. it is a close approximation to it.

How can this model solve some of the problems mentioned in the previous section? It is clear that inconsistency between *T1* and *T2,* and so impossibility of derivation of *T2* from *T1*, is not present in this picture. *T2** is consistent with *T1* and so there is no problem in applying the DN model of explanation (*T1* explains *T2**). Therefore, if we define reduction in terms of the DN model, *T2** might be reducible to *T1*, while *T2* is not. This is true even in the cases where *T2* is false. We derive a true theory (*T2**) from our current true theory (*T1*) which has strong similarities with a false theory (*T2*), and there is no logical problem here.

This schema has one additional virtue. In the cases where *T2* is incorrect and we do not currently use it, the strong similarity between *T2** and *T2* provides an indirect explanation for why we *thought* that *T2* was correct at a certain time. In other words, although *T2* is incorrect, because it is very similar (for example in observable predictions with certain amounts of accuracy that our equipments had in the past) to a correct theory (*T2**), we can explain why scientists believed in *T2*. This point can help us to find a model of continuity in the history of science. For example, we can say that although we had theory changes in the history of science and although the whole process of science is not strictly cumulative, we have a kind of connection between theories such that a new and true theory explains why scientists (rationally) believed in some false theories.

However, the proposed schema has its own difficulty, which needs further philosophical work. The main point concerns the nature of similarity[21]. How can we define similarity in general, and similarity between two theories in particular? We have to define similarity between two things in one or another *respect*. Similarity is not a two-place relation held between two things; it is at least a three-place relation, which aside from two things needs a respect for comparing them. The general question 'are these two things similar?' cannot be answered unless we determine the respect in which we want to compare them: 'Are these two objects similar in respect of their colours?' Now the question is, in what respect do we want to consider similarity between two theories (*T2* and *T2**)? Formal structures, observable predictions, numerical results, conceptual apparatus, simplicity, applicability, and relations with other theories are just a few examples of respects in which we can compare two theories.

Therefore, the claim that two theories must be similar (good approximation/having analogy) is not sufficient. To have an adequate account, first we have to know different respects that two theories can be similar regarding them, and then in each case we need a philosophical account to explain when two particular theories are

[21] I have mentioned this point in Chapter 1, and I will consider it in more detail in Chapter 5.

similar. In some cases, we already have philosophical accounts of similarity[22], while other cases need more work[23].

However, there is still a deeper problem using the proposed schema in concrete cases. In the above schema, it was simply assumed that the bridge principles are *exact* and *true* and we derive a true theory (*T2**) from them (plus the primary theory) which is strongly similar to the original secondary theory. In other words, there is no room for approximation in the bridge principles and we need the notion of approximation only in comparing the two theories (*T2* and *T2**). However, bridge principles are false or approximately true in most concrete examples of reduction. Consider for example the reduction of Kepler's laws to Newtonian mechanics. To derive the mathematical formula of a planet's orbit from Newtonian mechanics, in addition to the general laws, we need some information about the initial conditions of the system (position and momenta of all objects that can affect the planet). However, we do not know all the bodies in the universe, and even if we do know them, we do not know their positions and momenta with exact precision. Therefore, we have to omit some theoretically relevant information and use approximately true bridge principles. Now let us assume that we know all theoretical bridge principles that are necessary for derivation of the planet's orbit. In such case, we will be faced with the n-body problem, for which we do not have a general and exact solution. In other words, not only true and exact bridge principles are not sometimes available, but sometimes we do not *want* to have such things.

Furthermore, allowing approximation in the bridge principles has an additional virtue. By allowing them to be false, we have the conditions under which the secondary theory would be false. In other words, when we know that the bridge principles are false and we know why they are false, we can explain why the secondary theory is strictly speaking false but was as successful as it was. When we know the margin of accuracy of the bridge principles, this margin transmits to the secondary theory and makes the margin that this theory is approximately true. In our example, because we know the margin of accuracy of the bridge principles, we know that the orbit of planet is not a perfect ellipse, but something similar to an ellipse, and it provides an

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[22] For example, structuralist philosophers of science have developed a version of intertheoretical approximation (Balzer *et al.* 1987, 364-383). Moreover, in line with the semantic conception of scientific theories, Gorham (1996) has developed an account of the intertheory relation of comparative structural similarity. His account allows to measure structural dissimilarity between two models.

[23] Schaffner (1967, 146) points out that we have not done much works on the logic of analogy and Sklar (1993, 336) points out some difficulties and complexities in taking a limit in approximations.

explanation of why Kepler's laws *seemed* correct in the past. This explanation corresponds exactly with what we mentioned earlier, that in historical cases we need a kind of reduction to explain why the false theories seemed true[24].

These points lead Yoshida (1977, 2-7) to propose an alternative view on approximation in reduction. According to him, instead of approximate relation between two theories (*T2* and *T2**), we allow the bridge principles to be approximately true. From the conjunction of the primary theory with these approximately (and in few cases absolutely) true bridge principles, we can deduce the *exact* and *original* secondary theory, and not an approximation to it. (From conjunction of Newtonian Laws with *approximately true* bridge principles about the initial conditions, we can derive the exact and original Kepler's laws.)

I would like to consider some aspects of Yoshida's schema here. First, let us discuss different kinds of acceptable bridge principles. Yoshida (1977, 21-28) enumerates three different cases. In the first, we use the mathematical limiting process. By using the limiting operation on the laws of the primary science, we can derive the laws of the secondary one. For example, by allowing N (the number of degrees of freedom) $\rightarrow \infty$ we can show that classical thermodynamics is a limiting case of statistical thermodynamics[25]. In the second case, the bridge principles are contrary to fact, or contrary to the assumptions of the primary theory. For example, by allowing $\hbar$ (Planck's constant) = 0, we can reduce classical mechanics to quantum mechanics. Finally, the third kind of bridge principle is those in which we neglect a limiting variable in comparison with the other magnitudes involved. For example in Lorentz transform, ($x' = x - v.t / \sqrt{1-(v^2/c^2)}$), by the assumption that v/c is negligible, we can deduce the Galilean transform, ($x' = x - v.t$).

Yoshida's schema has a further benefit over the standard account of approximate reduction. As mentioned, for different respects where two theories might be similar, we need different philosophical accounts to explain conditions of similarity. However, in Yoshida's schema, approximation in the bridge principles has a mathematical nature and so is much clearer. The limiting operation, neglecting a

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[24] Allowing the bridge principles to be false has another virtue over the standard view. It clearly shows that reduction does not entail the elimination of concepts of the secondary science (ontological simplicity), i.e. even if we reduce *T2* to *T1*, we still need predicates and concepts of *T2*. For more detail, see Chapter 9.

[25] A controversial point about this alternative is that according to some philosophers (e.g. Nickles (1973, 199)) letting numerical constants change value is mathematically illegitimate. Yoshida (1977, 22-3) considered the issue and presented arguments to show that it is mathematically and philosophically legitimate.

variable in comparison with some others and allowing variables to be infinite are familiar mathematical notions that we have strong theoretical foundations for. Therefore, it seems that this model is much simpler than the traditional model and with stronger foundations. It should be pointed out that Yoshida does not claim that there is no other possible way to make approximation in the bridge principles. He (1977, 74) does not claim that his list is exhaustive. However, the point is that he uses only these three methods and succeeds in covering a vast range of reductions. This can be supposed as being a good confirmation of his proposal[26].

However, Yoshida's proposal faces a logical problem. It is possible that in all three cases (especially in the second one) one of the bridge principles contradicts one assumption or axiom of the primary theory. For example, the assumption that $\hbar = 0$ contradicts quantum theory. Now it could be argued that according to a theorem of classical logic, we can derive anything (not only the secondary theory) from a contradiction. In other words, we can reduce all theories (true or false, with a relevant universe of discourse or irrelevant, scientific or non-scientific) to the conjunction of quantum mechanics and this auxiliary assumption, and this is clearly absurd. Alternatively, by having contrary to the primary theories auxiliary assumptions we can reduce any arbitrary theory to any other arbitrary one.

Yoshida (1977, 73-74) was aware of this point and tried to provide an answer for it. According to him, we must not use contradictions in the premises of a deduction. To avoid contradiction we have to omit those parts of the primary theory that are inconsistent with the necessary bridge principles for reduction. In other words, first we change *T1* to *T1**, whereby *T1** does not contain those parts of *T1* that are inconsistent with the bridge principles, then we derive *T2* from the conjunction of *T1** and the bridge principles. However, can we omit any part of a theory arbitrarily and without any criterion? For example, it does not make sense to omit the principle of restricted relativity[27] from the special theory of relativity and still insist that the

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[26] He covers reductions of impetus theory, Kepler's laws and Galileo's laws to Newtonian mechanics as well as physical optics to electromagnetic theory, thermodynamics to statistical mechanics, classical mechanics to special relativity, classical celestial mechanics to the general theory of relativity, classical mechanics to quantum theory, geometrical optics to wave optics and the special theory of relativity to the general theory. In the case of false theories, his account covers reductions of early phlogiston theories of reduction, calcinations and respiration to Lavoisier's theory as well as the caloric theory of heat to kinetic theory of heat.

[27] This principle says, 'If, relative to K, K′ is a uniformly moving co-ordinate system devoid of rotation, then natural phenomena run their course with respect to K′ according to exactly the same general laws as with respect to K.' (Yoshida 1977, 74)

remaining body of theory can be called special relativity, and that every theory which is derived from that, is reducible to the relativity theory.

To remove this objection Yoshida divides statements of a theory into two parts: essential and nonessential. A bridge principle can contradict with nonessential statements of the primary theory; however, it must not contradict any essential statement. Hence, we can omit nonessential parts of a theory without changing its essence, and use the rest of the theory as a reducing base. Now we face a question about the definition of 'essential parts' and the way that we can separate them from nonessential parts. Yoshida responds with,

> By "essential statement" I mean a statement which is essential to the theory, one without which the theory would not be the theory it is. For example, the principle of restricted relativity [...] is essential to the special theory of relativity. Without that principle one could hardly call the remaining body of statements the special theory of relativity. By contrast, Newton's laws of motion are not essential to statistical mechanics; one can use the quantum laws and still have statistical mechanics. (Yoshida 1977, 74)

Clearly, this definition is not strong enough to carry the burden of reduction, although two particular examples mentioned in it are not so controversial. The definition is circular '...by "*essential* statement" I mean a statement which is *essential* to the theory...', and the remaining words do not remove the circularity. Apart from circularity, the definition is vague. It says that without an essential statement, the theory would not be the theory it is. Every statement, essential or non-essential, can satisfy this condition because when we remove it, the remaining body of statements is not the same theory, as it lacks one statement. In its present form this condition is too weak. We need more restrictions to find which two bodies of statements are *essentially* the same, or alternatively what amount of difference is acceptable to suppose two theories being *essentially* similar.

Surprisingly, these issues are exactly the same that we faced when we considered the alternative view of approximation, according to which *T2** must be similar to *T2* and derivable from *T1*. In other words, after bypassing the issue of similarity between theories and changing the location of approximation from *T2* to bridge principles, Yoshida faces the same problem concerns similarity between theories. He wants to make *T1** with the same essence with *T1*. This means that these two theories must be similar in respect of their essence. Now Yoshida has to answer these questions:

'What is the essence of a theory?' and 'What is the acceptable margin of difference to suppose two theories essentially similar?'

This point leads me to conclude that the two alternative accounts of approximation have the same nature, and face the same problems. Both are completely tied to the notion of similarity between theories, and so need further philosophical work. Therefore, we can summarize the issue like this: to solve the problem of inconsistency between *T1* and *T2*, and reducing the latter to the former by means of a set of bridge principles (*BP*), there are three places for approximation. Either we might have a new but similar primary theory (*T1**), or we might have a new but similar secondary theory (*T2**) or we might have a set of approximately true bridge principles (*BP**). In the case of the first two options, we still need more works on similarity between theories, but the third case has clearer nature.

Therefore, if according to Nagel's classic account, the only acceptable formal structure is $T1 + BP \vdash T2$; according to my account, seven following derivations are acceptable too. (The operator * represents good approximation)

(i) $T1^* + BP \vdash T2$ (ii) $T1 + BP^* \vdash T2$ (iii) $T1 + BP \vdash T2^*$

(iv) $T1^* + BP^* \vdash T2$ (v) $T1^* + BP \vdash T2^*$ (vi) $T1 + BP^* \vdash T2^*$

(vii) $T1^* + BP^* \vdash T2^*$

3.3 Identity between Things

The second half of this chapter is devoted to the issue of identity statements and the huge debates over them. Two kinds of identity statements have been proposed to serve as bridge principles. Let me start with the first, identity between *things*, and leave the second, identity between *properties*, to the next section.

Sklar (1967) was one the first philosophers who proposed thing-identities as bridge principles. First, I will present his account briefly and then examine it critically. He divides heterogeneous reduction into two parts: weak and strong. In a strong reduction, the reduced theory remains well established even after the reduction, and its degree of confirmation would increase because of the process of reduction. However, in a weak reduction, the reduced theory is not a true and current theory and its degree of confirmation is not increased. By its reduction we only want to provide a kind of explanation to show that why it *seemed* true in the past.

Sklar (1967, 118-121) believes that in the case of strong reduction, the mere correlation between two predicates via a (bi-) conditional statement is neither necessary nor sufficient for reduction. In other words, he thinks that Nagel's bridge principles that state (bi-) conditional relations between referents of two attribute-predicates are not strong enough to make reduction. According to him, in some cases, like the Wiedemann-Franz law, we have a correlation between two attribute-predicates (electrical and thermal conductivity of metals) and therefore we can derive laws of one theory from another, but we still do not have reduction. (Both thermal and electrical regularities are determined by a common cause, microphysical arrangements, and therefore both must be reduced to this third theory). Hence, mere correlation is not sufficient for reduction. On the other hand, in some cases we have reduction without any set of correlatory laws. For example, according to Sklar, there is no correlatory law that states a nomological relation between light and electromagnetic radiation, but physical optics is reducible to electromagnetic theory. This example, according to him, shows that in strong heterogeneous reduction, we need a stronger relation between entities than mere correlation, like that which holds between light and electromagnetic radiation: identity.

Sklar (1967, 120) insists that the *only* acceptable bridge principle in strong heterogeneous reduction is identity between *things*. This identity relation says that we have only one set of things whose members (or some of its members) may be referred to by two different labels: light and electromagnetic radiation, or as a macro-micro relation a piece of salt and an array of particular atoms in a particular configuration. Therefore, the logical form of identification may have two forms: universally quantified conditional statements that say every *thing* that is A is also B, or alternatively universally quantified biconditional statements that say every *thing* that is A is also B and vice versa. The crucial point is that in both cases the quantifier only ranges over physical objects, and not properties.

There are some points in Sklar's account which I do not agree with. For example, I see no reason to commit myself to the distinctions of weak/strong and homogeneous/heterogeneous reduction. Moreover, I do not accept Sklar's point that because two macrophysical theories are determined by a common microphysical theory, we cannot reduce them to each other. However, let us leave these points to Chapter 9, and consider Sklar's main point here.

Two questions about Sklar's proposal can be raised. (1) Can identity statements between things be used as bridge principles? (2) Is identity statement between things the *only* acceptable form of a bridge principle? My answer to the first question is affirmative and to the second one is negative. Like Nagel (1970, 126-8) in his revised version of reduction, I think that *in addition to* other kinds of bridge principles we may use identity statements between things. These statements are *another kind* of bridge principle and not *the* unique kind. Here I present an argument to show that restricting bridge principles to identity statements between things contradicts Sklar's presupposition about reduction.

According to Sklar, a philosophical model of reduction and the history of science must be in accordance with each other in significant cases. There are concrete examples in the history of science that we regard intuitively and pre-analytically as reduction. A philosophical account of reduction must recognize most of them as successful reductions, or at least the model must not treat them as irreducible cases *in principle*. If a philosophical model of reduction says that many of pre-analytically cases of reduction are irreducible *in principle*, the model does not fit with the history of science and is not an acceptable account of reduction. Sklar uses this strategy to reject Nagel's classic model. As mentioned earlier, when Sklar discusses Nagel's account of reduction, he believes that when we consider historical and concrete examples of reduction, we recognize that few (if any) of them follow a strictly derivational account. Then Sklar (1967, 110) adds that somebody can construe strictly derivational relations as reduction, but if we do so, we have to exclude those cases that we pre-analytically called reduction from our successful cases. However, this is not acceptable according to Sklar, because it contradicts his discussed presupposition. Therefore, he concludes that Nagel's account cannot cover concrete examples, and so is not acceptable.

The same strategy can be used against Sklar. We can imagine two kinds of theories and therefore two kinds of reduction. In the first group, the reduced and reducing theories are just different in some thing-predicates (or equally sortal or substantival predicates). However, in the second group, the reduced and reducing theories are different in some attribute-predicates. What is the difference between these two predicates? According to Causey (1972 a, 408), 'A thing-predicate is a *name* for a *kind* of elements of the domain of the theory. An attribute-predicate is a *name* for an *attribute*, which, in general, some elements of the domain will possess and others will not possess.' In other words, a particular thing *belongs to* a particular kind, and therefore satisfies the corresponding thing-predicate. (For example, this particular creature *belongs* to the horse kind and so *is a horse*.) However, a particular thing *possesses* (instead of belongs to) an attribute, and therefore satisfies the corresponding attribute-predicate. (For example, this particular cup *possesses* blue colour and so *has blue colour.*)

According to some philosophers, identity between two kinds is extensional, whereas identity between two attributes is not. If anything that belongs to kind *A* belongs to kind *B* and vice versa, then kind *A* and *B* are identical. (For example, roughly speaking because every thing that is water is H2O and vice versa, these two kinds are identical.) However, it is possible that two attributes have the same extension, but they are still different attributes. (For example, every creature, which has a heart, has two kidneys as well, but having a heart is something different from having two kidneys.)

Now where does this story leave us? Sklar's proposal for identity statements has obviously an extensional nature. He suggests conditional (or bi-conditional) quantified statements, their quantifiers ranging only over *things*. This kind of identity statement might be applicable to the first group of reduction, in which the reduced and reducing theories are only different in some thing-predicates. However, if two theories are different in some attribute-predicates (one uses 'temperature' and the other 'the mean kinetic energy of molecules') we cannot identify their attribute-predicates by extensional criteria. Therefore, Sklar's proposal does not work for them. (It is not a new point, Sklar (1967, 123) himself says that we have to avoid using this kind of identity in the case of properties, attributes, events, state of affairs and processes.)

As a result, according to Sklar two theories with different attribute-predicates are *in principle* irreducible. Therefore, his account excludes many pre-analytically cases of reduction as *in principle* irreducible, and this contradicts with his presupposition about reduction. Restricting bridge principles to identity statements between things prevents us covering many cases of reductions.

Identity relations between things have two advantages over mere correlatory bridge principles. Firstly, they bring ontological simplicity. By this relation, we merge two classes of things that we thought were separate into a single class, and therefore reduce our ontological commitments and simplify our ontology. For example, by identifying light with a sub-class of electromagnetic radiation, we simplify our scientific ontology.

Secondly, identity between things exempts us from making some substantive changes in our theories. In other words, to save our theories and avoid changes in them, we are forced to consider some relation between two classes of things as identity and not mere correlation. In these cases, if we consider a class of things over and above the other class, and merely assume a correlatory relation between them, then we have to make unwanted important changes in our theories. Sklar presents an example,

> In many cases, identity, as opposed to correlation, is forced upon us, unless we are willing to make substantive changes elsewhere in our theories. Once we attribute mass-energy to light waves and to electromagnetic waves, the conservation rules for that mass-energy won't leave room for distinct light waves and electromagnetic waves to be present. So if we are going to take light waves as something over and above the electromagnetic waves and merely correlated to them, we will have to so modify our theory to deprive the light waves of mass-energy, of other features (momentum, for example) as well. Once we say that the light waves are just the appropriate electromagnetic waves, the problem is gone. (Sklar 1993, 340)

It is because of this advantage that Sklar (1993, 341) suggests a general methodological principle, according to which we have to identify things whenever we can, unless the assertion of such an identification is blocked by some other features of the situation. For example, our identification is blocked if the reduced entity has some features that are not attributable to the reducing one.

It might be said (Sklar 1967, 121), (Causey 1972 a, b), (Hooker 1981, 204) that another advantage of identity statements between things is that, opposed to mere correlations, these statements need no explanation, although they do need justification. We examine this point in the next section when we discuss identity between properties, and see that it is completely irrelevant to the debate of reduction and must not enumerate as an advantage of identity statements.

Another alleged advantage of identity over correlatory statement (Hooker 1981, 202-3) is that reduction via identities, contrary to reduction via correlatory statements, preserves the pattern of explanation of the reduced theory. This means that if we have only identities, then the reducing theory can explain all phenomena that the reduced theory has explained, and so can preserve its explanatory pattern. I will discuss this point in Chapter 6 and show that in some cases of reduction, even if we have identities, the reduced theory could provide explanations that the reducing theory could not.

Apart from Sklar, other philosophers like Kim (1966), Nagel (1970), Causey (1972 a, b), Enç (1976) and Hooker (1981) accepted identities between things as bridge principles.

3.4 Identity between Properties

The second kind of identity statements that were widely proposed to play the role of bridge principles are identities between properties[28]. I will start by considering two main motivations for proposing such identities and then show that one of them is irrelevant to reduction and the other cannot be satisfied by identities. The first motivation is explanation. It was widely accepted that identities, contrary to mere correlations, need no explanation, although they still need justification. We do not need to explain '*why*' water is H2O, we only need to provide some theories to show '*how*' we recognised that they were identical, and provide some evidence to confirm that they are identical. Therefore, according to the proponents of identities, because in reduction the only things that need further explanations must be the fundamental laws of the reducing theory, we have to use identities (including thing and property identities), and not mere correlations that need explanations[29].

Before I examine this point in more detail, it should be pointed out that it is not an uncontroversial and accepted point that identities need no explanation. Enç (1976, 291), for example, has claimed that identity statements are also explainable and it is possible that they demand explanation in some epistemological frameworks. Enç has gone further and argued that,

> ...unless a specific type of explanatory relation holds between the fact that an individual a is $\emptyset$ and the fact that a is ψ, the theory in which the attribute predicate '$\emptyset$' occurs cannot be said to be reduced to a theory in which the attribute predicate 'ψ' occurs. After the reduction is achieved, the identity criteria that we decide upon for properties may lead us to claim that '$\emptyset$' and 'ψ' designate one and the same property. But this is a claim that has to follow a successful reduction, and not, as Causey has suggested, a claim that makes the reduction successful. (Enç 1976, 291)

[28] See for example (Schaffner 1967), (Hempel 1969), (Causey 1972 a, b), (Ager, *et al.* 1974) and (Hooker 1981).

[29] See for example Sklar (1967, 121 and 1993, 338), Causey (1972 a, 417 and 1972 b, 208) and Hooker (1981, 204).

Here I do not want to support Enç's proposal that identities are results of successful reductions and need explanations, or support his critics (Hooker 1891, 210-2). My point is that this issue is not as obvious as its earlier supporters assumed.

For the sake of argument, let us suppose that identities need no explanation. I shall argue that this point is completely irrelevant to the reduction debate. Firstly, notice that, apart from mere correlation, we have other kinds of bridge principles which, because of their natures, do not demand *explanation*. Consider the reduction of Kepler's laws to Newtonian mechanics. As explained earlier, we need some strictly speaking false initial and boundary conditions to play the role of bridge principles here. We do not seek explanation for a particular initial condition. This point shows that, although mere correlations (as physical hypothesis) may need explanation in other contexts, they do not need explanation when take the role of bridge principles. In the context of reduction, they need something else, something that initial conditions, which take the role of bridge principles, need as well. This is not explanation.

Secondly, we do not demand explanation for all kinds of physical hypothesis. There are fundamental and basic laws, which are axioms of theories, and we cannot explain them by laws that are more fundamental. We explain other laws by means of them and not vice versa. In addition, there are experimental laws, which have a high degree of confirmation but are not explainable in terms of other laws. There is no problem using such unexplainable laws as bridge principles in reduction. This point again shows that what an acceptable bridge principle needs is not explanation.

What is needed for bridge principles is *justification* rather than explanation. We have to justify their selection by showing that they have a high degree of confirmation. Therefore, the main question is 'How can we provide experimental evidence to confirm bridge principles?' In the case of initial conditions, it seems impossible to provide experimental evidence to support an initial condition directly. In the case of mere correlations, which state testable scientific claims, we do not have a better situation. Consider Nagel's famous bridge principle.

(*) $2E/3 = kT$

(E is mean kinetic energy of molecules, T is absolute temperature and k is a constant)

Here, there is no way to test this statement directly. The only available way is to add this principle to an equation from kinetic theory, and then to conclude the Boyle-

Charles law. However, the problem is that because the Boyle-Charles law has high degree of confirmation independent of kinetic theory, the fact that this law is deducible from kinetic theory plus (*) gives no entrance for testing and confirming (*) (Ager, *et al.* 1974, 120-1).

This is exactly the same problem that we discussed in the first chapter, and it arises only for those proponents of reduction which restrict conditions of reduction to formal ones. As I showed, Nagel mentioned this problem explicitly and accepted it. However, he had a solution for it. There are non-formal conditions of reduction. One of them is that reduction must be a significant scientific achievement and goes beyond mere deduction of the former established laws of the secondary science. Reduction must have empirical gain and provide '...usable suggestions for developing the secondary science, and must yield theorems referring to the latter's subject matter which augment or correct its currently accepted body of laws' (Nagel 1961, 360). These new scientific achievements provide (direct and indirect) confirmation for the bridge principles. Therefore, if a deduction can satisfy all of the formal and non-formal conditions of reduction, it provides justification for its bridge principles as well, and therefore their selection is rational.

In summary, we need no explanation for bridge principles, neither for explainable nor for unexplainable ones. We need justification for them. This justification can be provided by satisfying the non-formal conditions of reduction, and it is irrelevant to the point that whether bridge principles are identities or not.

The second motivation for proposing identities between properties as bridge principles is ontological simplicity[30]. In the case of identities between things (via coextensionality of two thing-predicates), it is reasonable to accept that identities can simplify our ontology. In the case of properties, even we do not have such an advantage. To see this point, some metaphysical considerations are required.

A crucial presupposition of those philosophers who suggested identity between properties is that we have a layered picture of properties. At the lower levels, there are properties of more basic sciences like physics, and at the higher levels, there are functional (or multiply realizable) properties of special sciences like psychology. Then reduction must identify high-level properties with basic ones, and simplify this multi-level picture. There are different ways to conclude that the scientific ontology is multi-level. The most dominant one is 'the semantical analysis of properties'.

[30] See for example Schaffner (1967, 143), Hempel (1969, 189) and Hooker (1981, 202).

According to this approach, corresponding to any scientific predicate there is a property, and because we have levels of predicates, we have levels of properties as well.

The best way to show that it is not a task of reduction to simplify the scientific ontology, and therefore we do not need property identities as bridge principles, is to reject the layered picture of reality. (See Section 5.2 for more detail.) In addition, if an alternative metaphysical approach can show that the scientific ontology is simple and flat *per se*, then the second motivation for proposing property identities as bridge principles is irrelevant. In Chapters 4 and 5, I will present two metaphysical frameworks according to which reality is not multi-level. In Chapter 4, I will consider an account that analyzes properties in terms of causal powers. According to this picture, we have a flat level of conditional causal powers (as building blocks of our ontology), and putting these powers together makes properties (See Section 4.2.2). In Chapter 5, I will consider an alternative view according to which we have a flat level of basic properties, and other entities that we thought were high-level and functional properties but are not really properties; they are predicates designating sets of similar basic properties. Both of these approaches, despite their differences, show that the scientific ontology is not multilevel, and therefore there is no need to simplify it by means of reduction.

3.5 Distinguishing Property Identities from Correlations

Although I have argued that two main motivations for proposing property identities as bridge principles are not strong enough, the question about the possibility and criteria of property identity is still attractive. Is identity between properties possible? What conditions should be satisfied to claim that there is identity in a particular case? It is obvious that we cannot answer these questions unless we adopt a metaphysical framework about properties and their natures. Are properties real things or linguistic entities? Are they universals or particulars? Is a property a bundle of conditional causal powers that bestows to its possessor? Different answers to these questions bring different answers to the question of possibility and criteria of property identity. Discussing these metaphysical questions is beyond the scope of this chapter. Putting these questions aside, another interesting topic is the relation between identity statements and their corresponding correlations. Can we distinguish them by means of empirical evidence? In this section, I will review some proposals suggested by philosophers in the context of reduction.

Perhaps Hempel (1969) has the most naive view about the relation between identity and coextensionality. He does not discuss property identity explicitly, but has a similar idea in mind. He believes that in a full reduction, in addition to deducing laws of the secondary science from laws of the primary, we need the reduction of concepts. This means that for every term *B* in the secondary science, we need a connective empirical law of biconditional form, which specifies a necessary and sufficient condition for its applicability in terms of a single concept *P* from the primary science (Hempel 1969, 189). This connection, which can be treated as the definition of *B*, enables us to avoid *B* theoretically. Hempel calls this an 'ontological' feature of reduction.

It can be seen that although Hempel does not mention property identity, his motivation is similar to what we mentioned earlier: ontological simplicity. However, it is obvious that mere empirical coextensionality between two attribute-predicates does not bring about ontological simplicity, and we cannot avoid one concept just because we have a coextension concept with it. Having one heart and having two kidneys are coextension; however, we cannot avoid one of them by having another. In summary, contingent and empirical coextensionality is not equal to identity and does not simplify our ontology.

Kim (1966) was fully aware that there is an essential difference between identity and coextensionality of two properties.

> It is clear that a psycho-physical correlation statement does not entail the corresponding identity statement – at least, the identity must be understood in such a way that it is not entailed by a mere correlation. [...] It is perhaps clearer that the identity entails the corresponding correlation, and at least to this extent, the identity statement has a factual component. (Kim 1966, 227)

In addition to the point that any identity entails a correlation, Kim believes that the corresponding correlation is the *only* factual component of an identity. According to him (1966, 227), an '...identity statement is not confirmable or refutable *qua* identity statement; it is confirmable or refutable insofar as, and only insofar as, the corresponding correlation statement entailed by it is confirmable or refutable by observation and experiment.' We will shortly see some problems with this view, but before that let us continue with Kim and see what he says about reduction.

Generally, Kim believes that reduction does not necessarily need identities as bridge principles. According to Kim (1966, 228-230), two points might be presented as advantages of using identities as bridge principles over using correlations. Firstly, reduction by means of identity enables us to simplify our scientific theories. In reply to this point, Kim says we have simplicity only if we reduce the number of primitive concepts or independent primitive assumptions. However, even if we have identities between properties, the number of primitive concepts are not reduced, because two identified properties are still nonsynonymous and so they are still two primitive concepts (Kim 1966, 230). In addition, reduction by means of identity does not reduce the number of independent primitive assumptions more than reduction by means of correlations.

The second alleged advantage of identity over correlation, according to Kim, is that identity statements need no explanation, while correlations need explanation. In reply to this point, Kim asserts,

> Thus, it turns out that by moving from correlation statements to identity statements we do not explain facts that were previously unexplained; rather, we make them "non-explainable." Now the question is this: In what sense does this achieve scientific or theoretical simplicity of the sort desired in science? [...] I think that the simplicity thus achieved is rather trivial and minimal significance from a scientific point of view. (Kim 1966, 230)

Therefore, according to Kim, reduction by means of property identity does not bring about any special kind of simplicity over reduction by means of correlation.

After considering Kim's arguments, let us return to his point that identities and corresponding correlations are not distinguishable by means of any empirical evidence. This controversial claim motivated many philosophical discussions. Let us review some of them. Causey (1972 a) criticized Kim's view and tried to show that we can separate identities from correlations by means of explanation. He says,

> Suppose that we have empirically justified "*A* iff *B*," where *A* and *B* are attributes. Thus, "*A* iff *B*" is at least a correlation. I claim that it will then be a contingent identity iff it does not require explanation. (Causey 1972 a, 417)

However, how can we recognize that "*A* iff *B*" needs explanation or not? Causey's main tool is the substitution principle, which says,

> Let 'A', 'B' be attribute-predicates, and let $D1$, $D2$ be derivations such that $D2$ is obtainable from $D1$ by uniform substitution of 'B' for 'A', and such that $D1$ is obtainable from $D2$ by uniform substitution of 'A' for 'B'. Then: If A and B are identical, then $D1$ and $D2$ are explanatorially equivalent. (Causey 1972 a, 419)

According to Causey (1972 a, 420), this is a necessary condition for identity, and although it is possible that some correlations vacuously or fortuitously satisfy it, but correlations in general will not satisfy it[31].

Ager (Ager, *et al.* 1974, 129-131) criticized the idea that A and B are identical iff "A iff B" does not require explanation. Causey (1976) replied to this objection. Enç (1976, 288-92) criticized Causey's substitution principle as well. He argues that we do not have explanatory symmetry between all of identical properties. His example is,

> ...there are certain facts about the weight of an object which can be explained by an appeal to its being the gravitational force exerted on the object by the earth, and yet there are no corresponding facts about the gravitational force exerted on an object which are explainable by an appeal to its being the weight of the object. (Enç 1976, 289)

Hooker (1981, 210-2) criticized Enç's view and presented a criterion for property identity, which is very similar to Causey's. According to him (1981, 219) 'Isomorphism of theoretical role, in clarified science, is a necessary and sufficient condition of property identity.' The idea behind this criterion is simple and plausible. If two attribute-predicates are coextensive, there is no guarantee that if we substitute one of them with another in a theoretical context, we still have true statements. Having a heart and having two kidneys are coextensive, however by substituting 'two kidneys' with 'heart' in the sentence 'Heart makes blood circulation', we will not have a true sentence any more.

Considering these arguments in detail is beyond the scope of this section. Nevertheless, intuitively it seems to me that Causey and Hooker's ideas that identities are distinguishable from correlations, in one way or another, are more plausible than their rivals. However, this does not mean that we must have property identity in any reduction. For example, in Section 5.1 I will show that even if we accept Hooker's criterion to distinguish identities from correlations, his main point

[31] It should be pointed out that in some contexts, like modal, we could not substitute one property with its identical counterpart and save explanatory equivalency. However, in the causal and scientific contexts there is no problem with the substitution.

that reduction must be done by means of identity statements is not true. In particular, I will argue that his proposal to make identity statements does not work in a very important case when we have multiple realization[32].

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The arguments of this chapter can be summarized by the following claims: (i) Approximation is a necessary component for any plausible account of reduction, at least to remove the problem of inconsistency. We can have approximation in three places: making an approximate theory close to the primary theory, making an approximate theory close to the secondary theory, and using approximately true bridge principles. In the first two cases, first we need to know in which respects two theories might be similar and then we need philosophical accounts of similarity for each case. Using the notion of approximation in the third case is easier. (ii) Identity statements between things might be used as bridge principles. They simplify our scientific ontology. However, restricting bridge principles to identities between things makes a gap between our philosophical model and concrete examples of reduction. (iii) There is no need for property identity as a bridge principle. Bridge principles do not need explanation, and having firm metaphysical frameworks about properties shows that reduction does not need to simplify the scientific ontology.

[32] Nickles (1973, 192-3) presents three objections to those accounts of reduction that want to save Nagel's structure but use identities instead of correlations. Firstly, introducing identities does not completely solve Nagel's problem. According to Nickles, Nagel's problem is that the DN model of explanation does not have a causal direction and we can explain a cause by its effect as well as explain an effect by its cause. Therefore, using the DN model in reduction also has this problem, i.e. we can reduce the primary theory to the secondary one as well as reduce the secondary theory to the primary one, and introducing identities does not solve it. (I will examine this point in Chapter 9.) Secondly, identifications raise new problems. For example, in reduction of a non-statistical theory to a statistical one we have different alternatives in the statistical theory to identify with a concept of non-statistical theory. Finally, some strong intertheoretic relations, like relation between classical mechanics and quantum theory, do not involve theoretical identifications.

4

Multiple Realization (I): A Causal Analysis of Properties

Realization is one of the most central issues in contemporary analytic metaphysics. Many debates in different areas of philosophy like physicalism, the mind-body problem, consciousness, and the nature of properties connect to it. In the philosophy of science, realization is central as well. After the first wave of objections to the classical model of reduction (the problems of inconsistency and meaning variance that we discussed in Chapters 2 and 3), the second wave of objections were based on the notion of multiple realization. These objections are more discussed and powerful than the first wave of objections such that for many philosophers *the* standard problem with the classical model is multiple realization. It is a dominant belief, and in many arguments taken for granted, that the possibility of reduction and the unity of science is blocked by multiple realization.

Because the realization relation is mainly conceived as a relation between properties, metaphysical discussions about nature of properties are inevitable. In this chapter, I will focus on the nature of multiple realization according to a causal analysis of properties. After that, I will show that in this metaphysical framework we can save a version of the unity of science. In Chapter 5, after mentioning some problems with the causal account, I will consider an alternative analysis of properties and show that this framework can also save a version of the unity of science. After that, I will present an exposition of the second wave of objections to the classical model of reduction, using the notion of multiple realization, and then consider the situation of the classical model of reduction according to the discussed metaphysical frameworks.

Here is a more detailed map of what I am going to do in this chapter: First, I will start with the causal analysis of realization. In the next section, Gillett's reflections on this view and his new account of realization will be presented. I will argue that his new account suffers from problems and so I take the standard causal view as my framework in the rest of the chapter. In the third section, Shapiro's considerations about the possibility of *multiple* realization will be discussed. It will be argued that his worries are only valid when we consider the possibility of multiple realization of *artificial* kinds and not properties or natural kinds.

After these metaphysical points, I will move on to the philosophy of science and examine some points about special sciences. First, I will try to show that given the causal framework of realization a version of the unity of science is still defendable. After that, Kim's considerations about the impossibility of *general* special sciences will be discussed. In reply, I will try to defend the possibility of general special sciences given the causal account of realization.

4.1 Multiple Realization

4.1.1 The Causal Account of Realization

Although philosophers have been using the notion of realization for a long time, few of them have discussed its metaphysics in detail. Normally they consider the intuition that some properties, states or kinds can be realized in different ways and by different physical bases enough to clarify the realization relation. As Gillett (2002, 316-17) says, among early accounts of realization, however, we can find some metaphysical features which would help us to formulate early accounts of realization more precisely. The first demand was that realization must be a non-causal determination relation. This means that the realizing property *e* determines and fixes the realized property *E*, i.e. we have *E* in virtue of having *e*, but *e* does not cause *E*. The second demand was that there should be a connection between the causal roles of the realized and realizer properties. The dominant idea was that when *E* is realized by *e*, *e*'s instantiation should play the same causal roles that *E*'s instantiation plays and not vice versa. In other words, although *e* and *E* are not identical, when *E* is realized by *e* any causal power of *E*'s instantiation is in fact a causal power of *e*'s instantiation. However, some causal powers of *e*'s instantiation might not be causal powers of *E*'s instantiation.

To give an example, Papineau's (1993, 25) definition is a good candidate. His main motivation in appealing to this notion is avoiding the problem of overdetermination. He wants to define the realization relation between a physical property *P* and a mental property *M* such that their common effects, say *e*, are not overdetermined. For this purpose he says,

> I propose that we adopt the following account of realization. In order for a mental or other special type **M** to be realized by an instance of some physical type **P**, **M** needs to be a *second-order property*, the property of having some property which satisfies certain requirements **R**. And then **M**

will be realized by **P** in some individual **X** if and only if this instance of **P** satisfies requirements **R**. In such a case we can say **X** satisfies **M** *in virtue* of satisfying **P**. (Papineau 1993, 25)

However, in recent years some philosophers, without rejecting the earlier accounts, tried to clarify the realization relation more. Here I will focus on Shoemaker's attempt and then mention some others who reached nearly the same results. Shoemaker's formulation of realization is completely dependent on his view of properties, which therefore we start by describing. In 'Causality and Properties' (1980), Shoemaker tries to analyze the notion of property in terms of the notion of causality. First, he defines the notion of a power: power is a function from circumstances to effects. Therefore, for a thing to have a power is for it to be such that its presence in a particular kind of circumstances has certain effects. Similarly, it can be said that for a thing to have a property is for it to be such that its presence in a particular kind of circumstances (i.e. in combination with some other properties) has certain effects. Therefore, a property could be defined as a function from sets of properties to sets of powers. Being 'knife-shaped', for example, is a property, which combined with some other properties like being made of steel, will produce a set of powers like the power of cutting butter. On this basis, Shoemaker defines a conditional power: an object has power *P* conditionally upon the possession of the properties in set *Q*, if it has a property *r* such that if *r* combines with *Q*, they produce *P* (and obviously having *Q* should not be sufficient for *P* alone). For example, an object *O* which has the property of being knife-shaped (*r*), has the power of cutting wood (*P*) conditionally upon the set of properties like being knife-sized and made of steel (*Q*). From these definitions, Shoemaker concludes his theory of properties: *a property is a cluster of conditional powers*. This means that a property, say being knife-shaped, is nothing more than a cluster of conditional powers: if it combines with the properties of being knife-sized and made of steel it will manifest the power to cut wood, alternatively if it combines with the set of being knife-sized and made of wood it will manifest power of cutting butter and so on.

Three points in this account are worth to be mentioned. Firstly, Shoemaker *identifies* a property with a cluster of conditional powers, i.e. the former is nothing over and above the latter. This means that conditional powers are constituents of properties. Secondly, he takes the relation between a property and its conditional powers to be a necessary relation. A property has a particular and fixed set of conditional powers in all possible worlds. In any other possible world that the property exists, it has the same conditional powers. Finally, every property is a set of conditional powers; however, not every set of conditional powers makes a property. For a set of conditional powers to constitute a property, any pair of them should be causally unified. Two conditional powers *X* and *Y* are causally unified if, and only if, it is a consequence of causal laws that either (1) whatever has either of them has the other, or (2) there is some third conditional power such that whatever has it has both *X* and *Y* (Shoemaker 1980, 246).

Nearly twenty years later, Shoemaker presented his account of realization based on the same assumptions. In 'Realization and Mental Causation' (2001), he analyzes the realization relation in terms of conditional powers. However, there are some modifications in the assumptions about properties. Firstly, he no longer *identifies* a property with a cluster of conditional powers (Shoemaker 2001, 94, n. 10). He simply says that instantiation of any property bestows a set of conditional powers on its subject. Secondly, he does not use the 'controversial' part of his former account that a property bestows a particular set of conditional powers, those it does in the actual world, in any other possible world (Shoemaker 2001, 77-8). He simply assumes that in the actual world the same property always bestows the same conditional powers. Finally, concerning causally unified conditional powers in a set, Shoemaker believes that it is not a sufficient condition to constitute a property. He (2001, 87) adds a new condition according to which the set must be closed under nomic and metaphysical entailment: 'for every conditional power contained in the set, the set contains every conditional power nomically or metaphysically entailed by that conditional power'.

Now let us see how Shoemaker analyzes the realization relation by means of causal powers. He starts with the determinate/determinable relation, which many philosophers have agreed has a close relation to the realization relation. He (2001, 78) says that the conditional powers bestowed by a determinable property, like being red, are a proper subset of the conditional powers bestowed by each of its determinate properties, like being scarlet. In other words, although each of the different determinates bestows a different set of conditional powers, all of them have the conditional powers bestowed by the determinable in common. Therefore, the determinate property realizes the determinable property by virtue of the fact that the conditional powers conferred by the latter are a proper subset of the former. The second case that Shoemaker considers is functional properties, on which some philosophers (e.g. Papineau) based their accounts of realization. Here again

Shoemaker thinks that the conditional powers bestowed by a functional property, like being in pain, are a proper subset of the conditional powers conferred by each of its realizers.

Shoemaker (2001, 78) generalizes this requirement to define the realization relation: 'In general, property X realizes property Y just in case the conditional powers bestowed by Y are a subset of the conditional powers bestowed by X (and X is not a conjunctive property having Y as a conjunct).' Naturally, when *Y* is a multiply realizable property the conditional powers bestowed by it are proper subsets of the sets of conditional powers bestowed by each of its realizers.

Accepting this definition, we could have two different interpretations. If we accept Shoemaker's first view that a property is nothing more than a cluster of conditional powers, i.e. if we identify a property with a cluster of conditional powers, we could easily say that the realized property is a *part* of each of its realizers. By accepting Shoemaker's framework of properties and independent from his second paper, Clapp (2001) takes such a position. He first defines realization relation as follows,

> A property *P* of an object (or event) *o realizes* a property *F* of *o* if and only if (i) it is necessary that, if *o* instantiates *P*, then *o* instantiates *F*, and (ii) *o*'s instantiating *P* in some metaphysical sense explains *o*'s instantiating *F* – being *P* is one *way in which* a thing can be *F*. (Clapp 2001, 112-3)

Then he (2001, 127) adds that, 'properties are simply identified with sets of causal powers'. By putting these two ideas together, he moves on to the definition of realization in terms of powers,

> *P realizes Q* if and only if (def.), where *p* and *q* are the sets of powers constituting *P* and *Q*, $q \subseteq p$. (Clapp 2001, 129)

A logical consequence of these premises is that a realized property is *a part* of its realizers. Clapp (2001, 133), for example, says, '...multiply realized mental properties, though real and causally efficacious, are better thought of as *parts* of their physical realizers.' It is a very interesting conclusion that according to this view, not only are mental properties not high-level properties, but also in one sense, they are more basic such that physical properties are made of them.

The second interpretation is obtained when we, with the later Shoemaker, do not identify a property with a set of conditional powers. In this case, we cannot say that the realized property is a part of its realizers. However, according to Shoemaker

(2001, 94. n. 11) we still can say that *instances* of the realized property are parts of *instances* of its realizers[33].

Apart from Shoemaker and Clapp, Kim expresses nearly the same result about the nature of the realization relation. With a general claim about the nature of properties like Alexander's dictum according to which '*To be real is to have causal powers*' (Kim 1993a, 202), it is not surprising to see that Kim analyzes the relation between realization and causal powers as follows,

> [The Causal Inheritance Principle] If mental property *M* is realized in a system at *t* in virtue of physical realization base *P*, the causal powers of *this instance of M* are identical with the causal powers of *P*. (Kim 1992, 326)

However, this definition ignores the point that *P* might have some additional causal powers that we would not like to attribute to *M*. On the other hand, if *P* is a realizer of *M* any stronger property *P** (say, *P & Q*, for a nontrivial *Q* consistent with *P*) is also a realizer of *M*. In this case *P** has some causal powers that are not included in *P* and therefore we do not want to attribute to *M* (Kim 1998, 129, n. 45). For these reasons, Kim has modified his definition. Instead of requiring identity between causal powers of instantiations of *M* and *P*, he allows the subset relation as well:

> If a second-order property *F* is realized on a given occasion by a first-order property *H* (that is, if *F* is instantiated on a given occasion in virtue of the fact that one of its realizers, *H*, is instantiated on that occasion), then the causal powers of this particular instance of *F* are identical with (or are a subset of) the causal powers of *H* (or of this instance of *H*). (Kim 1998, 54)

We can summarise the 'causal' ('received' or 'standard') analysis of realization like Gillett (2002, 317-8) as follows:

(I) A property instance *X* realizes a property instance *Y only if X* and *Y* are instantiated in the same individual.

(II) A property instance *X* realizes a property instance *Y only if* the causal powers individuative of the instance of *Y* match causal powers contributed by the instance of *X* (and where *X* may contribute powers not individuative of *Y*).

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[33] For a critical analysis of this interpretation and Shoemaker's possible motivations for suggesting it, see Section 5.3.

4.1.2 The Dimensioned Account of Realization

Gillett (2002) tried to criticise the causal analysis of realization and propose a new account. Let us start with his concrete example. Suppose that we have a piece of diamond, *S*, which has the property *H* of being extremely hard. In addition, let us suppose that *S1-Sn* are the constituent carbon atoms of *S*, and *F1-Fn* are properties of the atoms or relations between them. It is an obvious point that according to the causal view none of *Fi*s is a realizer of *H* alone. *Fi* and *H* are not instantiated in the same individual (violating requirement I), and their causal powers are quite distinct and independent (violating requirement II). According to Gillett (2002, 320) a plausible candidate for realizer of *H*, in the causal framework, is a structural property (say COMBO), which is a highly complex structure of carbon atoms, their properties and relations. Now Gillett raises the question, what is the relation between COMBO and the fundamental microphysical properties/relations that basic components of carbon atoms like quarks have? Here again it is an obvious point that none of these properties/relations is a realizer of COMBO alone. Gillett inclines to say that here we have a realization relation,

> Although not identical, the sciences again provide evidence that makes it plausible that COMBO is not wholly distinct from such fundamental microphysical properties/relations either. Instead, it appears that such ontologically fundamental properties/relations *realize* structural properties such as COMBO. (Gillett 2002, 320)

However, from the mere fact that according to the causal view none of these fundamental properties/relations is a realizer of COMBO *alone*, Gillett (2002, 320) surprisingly concludes that the causal view '... again cannot class these cases as involving realization'. I cannot understand Gillett's main intention behind this argument. It seems to me that such as in the first case, the causal view prevents us from identifying any single basic property/relation as a realizer of COMBO, but the same strategy could work here. We need another structural property of the diamond, say COMBOBO, which is a highly complex structure of fundamental particles, their properties and relations. This property satisfies both requirements of the causal view and is a possible realizer of COMBO; similarly, COMBO satisfies those two requirements and is a possible realizer of hardness.

It is possible that Gillett's example and his argument for the inadequacy of the causal view do not work well. However, he presents a new account that he thinks adds dimensions to the realization relation. Let us consider this account and the alleged dimensions. His (2002, 322) definition of realization is,

> Property/relation instance(s) **F**1-**F***n* realize an instance of a property **G**, in an individual **S**, *if and only if* **S** has powers that are individuative of an instance of **G** in virtue of the powers contributed by **F**1-**F***n* to **S** or **S**'s constituent(s), but not vice versa.

Gillett thinks that the new definition adds two dimensions to the flat causal view. The first dimension concerns the first feature of the causal view; according to it, the realized and realizer properties are co-instantiated. In the above definition, in addition to the identity of substance, Gillett opens the possibility that instead of one substance (*S*), the realizer properties have a number of substances (*S1-Sn*), which are constituents of the substance of the realized property. The second dimension concerns the second feature of the causal view, according to which conditional powers of the realized property are identical with, or a subset of, the conditional powers of the realizer property. Apart from identity, Gillett opens the possibility that *in virtue of* powers contributed by *F1-Fn, S* has contributed powers of *G*.

Both of these dimensions seem awkward to me. Regarding the first one, I think there is no difference between the causal and dimensioned views. What is the realizer property in Gillett's account? Here we have a set of properties, *F1-Fn,* which plays this role. Now, what is the substance of this set? There are two possibilities here. According to the first, *F1-Fn* contribute their powers directly to *S*. Here we have a situation similar to the causal view and both of the realizer and realized properties have the same substance. Secondly, *F1-Fn* contribute their powers not to *S*, but to *S*'s constituents. In this case, *F1-Fn* are properties and relations of *S*'s constituents. In other words, *F1-Fn* is exactly a structural property at a given level, atomic or sub-atomic. Now, it is obvious that when we want to determine the substance of a set of properties that contribute powers to a set of constituents, we cannot mention one or another constituent. Here the substance of the set of properties is the set of all constituents, i.e. *S1-Sn*. However, *S1-Sn* is nothing more than *S*. Therefore, the substance of the realized property *G* is *S*, and the substance of the realizing property *F1-Fn*, is *S1-Sn* that is identical with *S*. Consequently, there is no difference between the two accounts of realization in this regard.

Regarding the second alleged dimension, I think there is an ambiguity in Gillett's account. The heart of his definition is substituting the notion of '*identity* between conditional powers' with the notion of '*in virtue of*'. Instead of identity between conditional powers of G and a subset of conditional powers of $F1\text{-}Fn$, he says that *in virtue of* the former we have the latter. However, 'in virtue of' is a vague relation that might be used in different ways in different contexts. Even if we take the point that in all cases, 'in virtue of' is a non-causal relation, there are still different usages of it that cannot be unified by one metaphysical account. The whole-part relation and the constitution relation are two examples of usage of this relation: this table has contact with the floor in virtue of the fact that some of its parts (its legs) have contact, and this pen is red in virtue of the fact that one of its constituents is the property of redness. Now, the question is, in which sense does Gillett uses this notion? If he says that powers of the realized property are parts of powers of the realizing property (which would be similar to Clapp's view), this is something quite different from the position that the former is a constituent of the latter. These positions must be first distinguished from each other, and then their plausibility according to more general theories of properties (like the causal model) discussed, a task that Gillett has not done. As a result, there is an ambiguity in Gillett's definition of realization, which might be interpreted in radically different ways. The consistency of these interpretations with more general theories of properties has not been discussed.

Finally, let us close this section with one of the cases that Gillett (2002, 322, n. 9) thinks his account can cover, but the causal account cannot. Suppose S has X and Y, but X contributes none of the powers individuative of Y. However, the powers contributed by X to S are sufficient for S to have powers individuative of Y. According to Gillett, because there is no identity between powers here, the causal account could not cover this case as a realization. However, sufficiency is an example of the 'in virtue of' relation, and therefore Gillett's account covers this case as realization.

To assess this claim let us examine the possible ways that a set of powers might be sufficient for another set. The weakest case (factual sufficiency) is obtained when we have mere factual coextensionality between two sets of powers; in a possible world that all water-soluble things are white, being water-soluble is sufficient for being white. I think Gillett must reject such cases as real realizations; otherwise, he has to accept that having one heart is a realizer of having two kidneys, which seems absurd. Two other possible kinds of sufficiency are available, nomological and metaphysical. In the former, it is a causal law that anything that has X, has Y as well. And in the latter it is a necessary truth that anything that has X, has Y as well. Now remember Shoemaker's new additional condition for a set of causal powers to constitute a property. According to this condition, if a conditional power is (nomically or metaphysically) entailed by another conditional power, then any property that has the latter has the former as well. According to this condition, if conditional powers of Y are entailed by conditional powers of X, then X should confer these powers as well. Hence, the conditional powers of Y are a subset of the conditional powers of X. Therefore, the second feature of the causal account is satisfied and we have realization according to this account. There is no advantage for Gillett's account over the causal account because of this case.

4.1.3 The Possibility of *Multiple* Realization

The *multiple* realization thesis is normally regarded as an argument against reduction. Therefore, apart from the notion of realization we have to consider *multiple* realizability as well. In this section, I will discuss Shapiro's (2000) considerations about the possibility of *multiple* realizability. His main concerns (Shapiro 2000, 636) are that no one has seriously questioned the truth of *multiple* realizability thesis and no one has tried to answer the question about when two realizations are different and distinct from each other. After considering three arguments for possibility of the empirical thesis of multiple realizability, he concludes that none of them can prove it. Here I do not examine these arguments, and start from where Shapiro begins to present his own arguments.

The first crucial point is that, following Lycan, Shapiro thinks that the only things for which the multiple realizability thesis has a chance of being true are *kinds*. What sort of kinds?

> [The multiple realizability thesis], to the extent that it is true, is true of kinds that are defined by reference to their purpose or capacity or contribution to some end. Purpose, capacities, contributions— these specify roles to be played and, accordingly, it may be possible that any of a variety of occupants can fill the role (Shapiro 2000, 643).

The second crucial point is that Shapiro thinks realizers are *objects*, which have some properties. An object is a realizer of a kind, if the object, in virtue of its properties, can fulfil the purpose, capacities, or contributions by reference to which the kind is defined[34]. Now according to Shapiro (2000, 643), 'Some of the properties of realizers

[34] This point seems very unusual. Intuitively, we expect that the realized and realizer entities belong to the same ontological category and have the same nature. For example, we expect that any account of

of multiply realizable kinds are relevant to the purpose, activity, or capacity that define the kind and some are not'. As an example, 'corkscrew' is a kind that can be defined in terms of a capacity; a corkscrew has the capacity to remove corks. Therefore, a piece of steel with a particular shape that could remove normal corks under normal circumstances is a realizer of this kind. The colour of this tool is one of those properties which are not relevant to the capacity. However, the rigidity of the steel is a relevant property.

On the basis of these assumptions, Shapiro (2000, 644) says that two realizers of a kind are distinct only if they differ in the relevant causal properties, properties that are relevant to the purpose and function of the kind. In other words, two realizers are distinct only when they bring about the function that defines the kind in different ways. As a result, a given kind is *multiply* realizable only if it has some distinct realizers.

As an uncontroversial example, it is clear that according to this framework two similar corkscrews that only differ in their colours are not two distinct realizers of the kind corkscrew, because they differ only in colour, which is irrelevant to the function of corkscrew. However, the case will be more complicated if we consider two waiter's corkscrews, one is composed of steel and the other of aluminium. Shapiro (2000, 644) still thinks that they are not two distinct realizers. Both of them fulfil the purpose of the corkscrew with the same causally relevant properties: e.g. rigidity.

However, there is a point here that might help understand Shapiro's account. Steel and aluminium might consider as two distinct realizers of some *other* kinds. Consider 'rigidity' as a disposition. According to Shapiro (2000, 643), because we can define a disposition in terms of some capacities, dispositions (like kinds) are candidates for multiple realizability. Therefore, we might consider rigid objects as members of a kind. Now regarding this kind steel and aluminium are two distinct realizers, because they differ in the mechanisms and the ways that they bring about rigidity.

The oint of this example, as far as I understand, is that any kind has a proper level of abstraction. This level determines which pairs of objects are heterogeneous and therefore make distinct realizers for the kind, and which are homogeneous and so cannot make distinct realizers. A given pair of objects (like steel and aluminium) might be homogeneous according to one level of abstraction, and heterogeneous according to another one. What determines this level of abstraction is the purpose

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realization takes for granted that realizers of properties are properties themselves. However, Shapiro says that concrete objects are realizers of abstract entities like kinds.

and function of the kind. Regarding the purpose of a corkscrew, i.e. removing normal corks under normal circumstances, any two normal rigid objects, like steel and aluminium, are homogeneous. This purpose screens off their differences, because both of them bring about the same effect by the same mechanism. However, regarding the purpose and function of rigidity (if any), they bring about it in different ways and so are heterogeneous.

On the basis of such assumptions, Shapiro (2000, 646-50) presents a dilemma for *multiple* realizability. There are two possible cases. If two realizers of a kind are not distinct from each other, then we do not have *multiple* realizability. We have two instantiations of the same realization. On the other horn, if they are distinct this means they are different in their causally relevant properties and so belong to two different kinds. Therefore, if they are distinct realizers, there is no *single* kind with multiple realizations; there are two different kinds here.

Different points in Shapiro's account might be criticized. However, I will consider only one point, which is directly relevant to our discussion of reduction. The formal conditions of the classical model of reduction concerns the deduction of laws of the secondary science from laws of the primary science. Normally, laws deal with *properties* and *natural kinds*. Consequently, we have to consider the situation of properties and natural kinds according to Shapiro's account. Therefore, the question is, 'can properties and natural kinds be multiply realized?' In other words, can we always characterize a property or a natural kind in the format that Shapiro suggested, i.e. by reference to its purpose, capacity or contribution to some end?

As to properties, Shapiro accepts that *dispositional* properties can be defined by referring to their purpose and capacity. Extending this claim to *all* properties needs the additional assumptions that all properties are dispositional. As to natural kinds, claiming that they can be defined by referring to their purpose and capacity needs the additional assumption that a natural kind is a cluster of dispositions. Both of these additional assumptions might be disputed, but for the sake of argument let us accept them.

However, even if we accept that dispositional properties and natural kinds might be realized, considering two particular realizers as distinct or non-distinct *is not an objective feature of them*, it depends on our point of view and the respects in which we consider them. To see this point, let us return to Shapiro's two concrete examples.

> This moral makes sense of why the waiter's corkscrew and the winged corkscrew do seem to count as multiple realization of a corkscrew. The

> waiter's corkscrew relies on a lever to prise the cork out of the bottle whereas the winged corkscrew uses a rack and two pinions to do the same job. Levers and rack and pinions are different mechanisms that require different manipulations, they are described by different laws, and so on. The causally relevant properties of these two devices differ; a fortiori they qualify as different realizations of a corkscrew. [...]
>
> For all I know, rigidity is a disposition that is multiply realizable. If rigidity is a disposition that can be brought about in various ways, and if steel and aluminum differ in respect to how they produce rigidity, then steel and aluminum are alternative realizations of rigidity. (Shapiro 2000, 644-5)

In the first example, it is quite possible that we take the two corkscrews as *non-distinct,* if we compare them with an imaginary corkscrew that works by laser beams instead of mechanical forces. This imaginary machine first destroys corks by laser beams and then removes them by vacuuming. Here we could say that the first two corkscrews are homogeneous, because both of them work by means of *mechanical forces*. This means that they are not distinct, because both of them use the same mechanism (using mechanical forces) to bring about the purpose. However, the imaginary machine is a distinct realizer, because it brings about the purpose by a quite different mechanism. Therefore, considering two realizers distinct or similar is a matter of our point of view, and depends to other possible and available cases. It is not an intrinsic feature of realizers.

Similarly, in the second example, we could say that steel and aluminum are not distinct realizers of rigidity. Each brings about rigidity in virtue of having fundamental physical particles (e.g. quarks) and the same laws that govern them (fundamental microphysical laws). In this respect, they are homogeneous. However, in another context, when we focus on the type of chemical bonds between atoms of steel and aluminum, they are different and distinct.

Therefore, there is no fixed level of abstraction for a given kind or property. In different contexts and by different points of view, we could have different judgments about two realizers. Shapiro's account is a context-dependent rather than an objective account of realization, according to which homogeneity and heterogeneity of two realizers, apart from them, depend on the context of consideration. A context-dependent analysis of realization might be useful for considering artificial kinds (like corkscrews), but I think is not good for analyzing natural kinds and properties. In my view, a proper metaphysical account of realization for properties and natural kinds must be objective and independent of the context. It must determine exact conditions which under them we have realization, independent of any cognitive, epistemic, or contextual aspects. Shapiro's account is not a proper model for this purpose[35].

Finally, let us close this part by considering a question: 'Given the causal account of realization, are realizers of a multiply realizable property distinct?' My answer is yes and no. In one sense, they are not distinct. All of them realize the realized property by a given, common and fixed set of causal powers. Therefore, all contain a particular set of causal powers, and by virtue of it realize the realized property. However, in another sense they are distinct. Apart from the shared set of causal powers, each of them has some different and un-shared causal powers, which might be unique to that realizer. In this sense, the causal powers of one realizer are different from any other realizer, and so they are distinct. In other words, realizers of a property are different, but are not radically different properties. We will return to this important point shortly and see how it helps to answer some questions.

4.2 Multiple Realization and Special Sciences

Now it is time to return to the philosophy of science and consider the situation of special sciences according to the causal account of realization. In the following section, I will show how we could still defend a version of the unity of science. The next section is devoted to Kim's arguments against general special sciences. In reply, I will try to show that given the causal account of realization we can have *general* special sciences.

4.2.1 The Possibility of the Unity of Science (I)

The idea of the unity of science is one of the old attractive themes for both philosophers and scientists. For some philosophers, the unity of science is more important than particular accounts of reduction. This means that even if Nagelian account cannot be defended against objections, as long as we have a defendable account of the unity of science, that is enough. Therefore, presenting an account of

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[35] For a completely different critique of Shapiro's approach, see Rosenberg (2001).

the unity of science, which is immune against objections to the classical model of reduction, is a promising philosophical task.

At least three components can be distinguished in any model of the unity of science. The first element concerns the aspect in which, according to the model, scientific theories are unified. For example, according to Carnap (1938) in his 'Logical Foundations of the Unity of Science', scientific theories can be unified in respect of their languages. He claims that all physical terms are reducible to the 'thing-language', and if any other science wants to be legitimate, it must only use terms that are reducible to the 'thing-language'.

The second element concerns the strategy that by following it the unity of science in the alleged respect can be shown. Normally philosophers have used an intertheoretic account of reduction as this part. They have suggested ways to reduce more comprehensive to less comprehensive theories in the alleged respect. For example, if somebody believes that scientific theories are unified in respect of their laws, then she might use Nagelian classic account as the second part of her model. However, it is not the case that any model of the unity of science needs a reduction account. We will see shortly that in my favourite model there is no particular reduction account.

The third element concerns the generality of the model. This element asserts that the model covers some theories, and is silent about others. For example, Carnap thinks that his model is applicable to *any* scientific theory. Alternatively, some other models claim that they only cover macro-theories, i.e. they provide accounts for the unity of macro-theories with their counterpart micro-theories.

In the model I will defend, the first element is content of laws: content of a special-science law is a coarse-grained (more abstract) version of contents of a set of basic laws. Regarding the second element, I do not need a particular account of reduction. Metaphysical considerations about the nature of properties are sufficient to show the unity. Finally, the scope of my account only covers special sciences, or more precisely those sciences that use multiply realizable properties. This means that my model at most can show a unity between special and basic sciences. Therefore, this model is silent about the relationship between theories that do not use multiply realizable properties.

To present my version of the unity of science, let us consider a special-science law and its basic (physical) counterparts. Suppose P and Q are two properties belonging to a special science, with two sets of physical realizers respectively: $p1$, $p2$ ... and $q1$, $q2$.... Moreover, suppose that P and Q enter into a law of the special science like P

$\rightarrow Q$, and any pair of pi and qi enter into a law of physics like $pi \rightarrow qi$. Whenever P is realized by pi and Q by qi, we have two laws in two different levels, at the high level we have $P \rightarrow Q$ and at the physical level we have $pi \rightarrow qi$ (Figure 1).

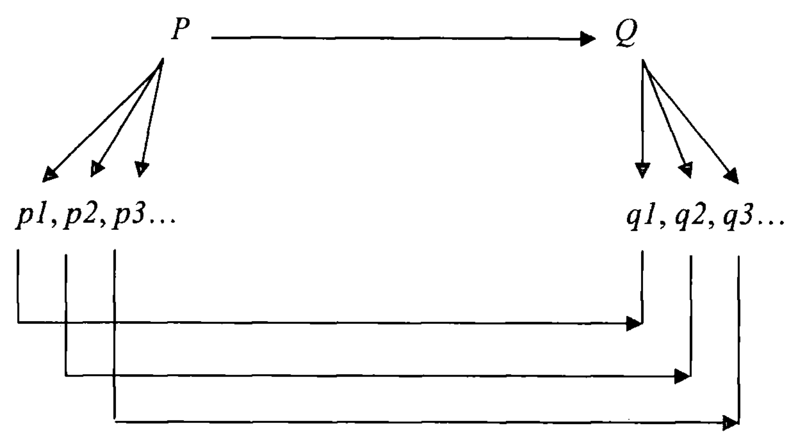

Figure 1: Realization of a special-science law at the physical level

Now, let us consider the conditional causal powers of these properties. Suppose that P and Q's sets of conditional causal powers are α and β respectively. According to the causal account of realization, because pis are realizers of P, α is a subset of their conditional causal powers. Therefore, we can represent their conditional causal powers as table 1. Similarly, for Q and qis we have table 2.

Property	Conditional causal powers
P	α
$p1$	$\alpha + \alpha 1$
$p2$	$\alpha + \alpha 2$
...	...

Table 1: Conditional causal powers of P and its realizers

Property	Conditional causal powers
Q	β
$q1$	$\beta + \beta 1$
$q2$	$\beta + \beta 2$
...	...

Table 2: Conditional causal powers of Q and its realizers

Now we can re-write the laws of Figure 1 according to the conditional causal powers of properties (table 3).

	In terms of properties		In terms of conditional causal powers
A special-science law	(Law 0)	$P \rightarrow Q$	$\alpha \rightarrow \beta$
Physical laws	(Law 1)	$p1 \rightarrow q1$	$\alpha + \alpha 1 \rightarrow \beta + \beta 1$
	(Law 2)	$P2 \rightarrow q2$	$\alpha + \alpha 2 \rightarrow \beta + \beta 2$
	...		...

Table 3: re-writing a special-science law and its physical realizers in terms of their conditional causal powers

My claim is that the content of law (0) is a coarse-grained version of contents of a set of laws (law 1, 2...). To see this point let me review Batterman's (2000) argument and examples, and then borrow from him the concept of an irrelevant causal power. He (2000, 120) introduces the concept of 'a universal behaviour/property': saying that a property/behaviour is a universal feature of a range of systems means that all of the systems, despite their micro-level differences, manifest the same property/behaviour. For example, for a container of fluid, we define the 'order parameter' as the difference in densities of the vapour and liquid. Near critical temperatures, the order parameters of all fluids follow the same mathematical equation. This means that the curve of density vs. temperature for all fluids is the same (a universal behaviour); however, each fluid has a unique microstructure (chemical constitution) and a different critical temperature.

Now with regard to a universal behaviour, some details of the microstructure of a given system are 'irrelevant'. The criterion of irrelevancy is this: if by changing a microfeature we still have the universal behaviour, then that microfeature is irrelevant to the universal behaviour, whereas if by changing the microfeature we will lose the universal behaviour, the micro-feature is relevant. For example, chemical constitutions of fluids are irrelevant to the discussed universal behaviour, while the spatial dimension of the system is relevant. When we have systems with one spatial dimension or quasi-one dimensional systems (e.g. polymers), or when we have systems with (quasi)-two dimensional systems (e.g. films), their curves of density vs. temperature are different from the curve of three dimensional systems like fluids (Batterman 2000, 127-8). Relevant microfeatures are present in all the systems that manifest a universal behaviour, while the systems might be different in their irrelevant microfeatures.

Batterman (2000, 130-134) extends his argument to special sciences as well. According to him, we can explain multiple realization in special sciences with the same strategy. Here we have a universal property at the high-level (say, being in pain), which can be realized by different low-level configurations. All of these different low-level configurations share a set of relevant physical parameters, while they might be different in some irrelevant physical parameters. Although Batterman did not mention the causal analysis of properties, his proposal is very similar to Shoemaker's proposal. What Batterman calls the relevant physical parameters, which are common in all the systems that manifest a universal behaviour, corresponds to the set of causal powers of a multiply realizable property (in our example α). And what he calls the irrelevant factors corresponds to the additional causal powers of the realizers (in our example $\alpha 1$, $\alpha 2$ ...). Now by having these assumptions, we need one further step to obtain the unity of science, the step that Batterman and Shoemaker did not explain and I will explain here.

By using Batterman's terminology, in our previous example the antecedents of laws 1, 2...include *irrelevant* sets of causal powers. αis are irrelevant because when we substitute one of them with another, we still have the common (universal) causal powers (β). Although at the fine-grained physical level, α cannot appear alone and should be with one of αis, which αi is present does not matter. In fact, α is sufficient to bring about β. In other words, αis are irrelevant in the sense that it does not matter which one is present. The key factor is the presence of α with one its supplements; the supplements which in any case one of them (no matter which one) is required to be with α in the actual world, but all are nomologically irrelevant to bring about β.

On the other hand, when α combines with one of these supplements, apart from β they bring about some byproducts like $\beta 1$ or $\beta 2$ .... Similarly, βis are irrelevant as long as our concern is the presence of β. In other words, although β cannot appear without them, it does not matter which one is present. They are irrelevant to our main concern here, which is the presence of a universal behaviour (β).

Now my main claim is that the content of law (0) is a coarse-grained version of a set of nomological contents expressed by laws 1, 2.... Each of laws 1, 2... expresses a nomological content at a fine-grained physical level, however law (0) puts all these contents together and eliminates irrelevant factors from their antecedents and consequents and then present their common content in a coarse-grained version. In other words, at a more abstract level, law (0) only mentions the relevant factors to bring about a universal behaviour. As long as our concern is a universal behaviour (β), law (0) mentions what is nomologically essential for it. However, laws 1, 2..., in addition to the essential information, give more detail about particular configurations and byproducts that we have in particular instances.

The relationship between law (0) and laws 1, 2... can be explained more. According to the discussed interpretation, law (0) says that the only relevant set of causal powers to β is α. Therefore, this law asserts two things: firstly, α is sufficient for β; secondly, the *only* sufficient and relevant factor for β is α. Now it seems that this law has a distinguished situation from the ordinary laws like law 1, 2.... Each of these laws only mentions that a set of causal powers (e.g. $\alpha + \alpha 1$) is sufficient for another set (e.g. $\beta + \beta 1$). However, they do not have the second component of law (0), i.e. they do not claim that the *only* relevant factors to their consequents are those that are mentioned in the antecedents. Based on this distinction, it seems that the method of confirmation for law (0) is different from the ordinary method of confirmation. To confirm ordinary laws, like law (1), we only need positive instances that have both the antecedent and consequent sets of causal powers. Any object that has $\alpha + \alpha 1$ and $\beta + \beta 1$ is a positive instance for law (1). However, this cannot be a positive instance for law (0). To confirm law (0) we must confirm its second component, i.e. we must show that the *only* relevant set of causal powers is α. Now the question is how we can confirm law (0).

One way to do this task seems to be this: we need a vast range of highly confirmed laws as laws 1, 2 .... These laws (probably with the help of some mathematical methods) show that by keeping α unchanged and by changing other factors, we still have β. Therefore, they give us evidence that the only relevant factor is probably α. In other words, it seems that we infer law (0) from a set of basic laws. Now let us suppose that in a particular case we have only law 1. In this case the inference from the law '$\alpha + \alpha 1$ is sufficient for $\beta + \beta 1$' to the law 'α is the *only* relevant factor to β', is not always valid. It is possible that instead of α, $\alpha 1$ is relevant here and brings about β. However, if we have law 1 and law 2, then the inference is more plausible, although it cannot still prove that law (0) is true. As a result, obtaining law (0) from the set of basic laws 1, 2 ... (probably with the help of some mathematical methods) has an inductive nature, which at most can give a high degree of confirmation to law

(0), but cannot prove it. Having more laws, saying that the combination of α with different αis is sufficient to bring about β, gives more confirmation to law (0). Therefore, the claim that real multiply realizable properties have *infinite* possible realizers does not only make a problem for this account of the unity of science, but also increases the degree of confirmation of special-science laws. On the other hand, the degree of confirmation of a generalization that uses multiply realizable properties with *few* realizers is probably low.

Let us consider a concrete example to understand the proposed unity of science better. As a typical law of psychology, consider this one[36]. 'If a person wants something (*w*), and believes she can get *w* by doing some action (*a*), then she will do it.' Let us suppose that properties that are used in this law (e.g. 'wanting something' or 'believing she can get *w* by doing some action') are multiply realizable (or determinable). This means that anybody who has one of these properties has a certain set of causal powers[37]. Imagine that according to neuroscientific discoveries, the brain of anybody who wants something (*w*) has the set of causal powers α, and by believing she can get *w* by doing some action (*a*) her brain has the set of causal powers β, and by doing the action *a* her brain has the set γ. To illuminate these sets let me give an example. Doing the action *a* needs some electrical pulses from the brain to the appropriate limbs. Therefore, the set of causal powers corresponding to this property (γ) has a conditional causal power like *c*, which says if an appropriate voltage-detector connects to the neurons that carry electrical pulses from the brain to the limbs, then the detector will detect a particular electrical pulse when the property is instantiated.

Now let us consider one realization of this law. If Mary wants a glass of water (her corresponding brain state is *s1*), and believes she can get it by opening the fridge (her corresponding brain state is *s2*), then she will open the fridge (her corresponding brain state is *s3*). Being in these brain states has something more than the mentioned sets of conditional causal powers (α, β, γ). For example, when Mary wants a glass of water, in addition to set α, her brain has some additional features, the features that are

[36] In fact, this law needs a *ceteris paribus* clause. For the sake of simplicity, this clause is eliminated. I will examine the issue of exceptions in special-science laws shortly.

[37] A perspicacious reader might say that this claim is in fact the claim of a type-identity physicalist: mental properties are identical with some physical properties. Therefore, at the end of the day the causal analysis of realization rejects the main point of '*multiple realization*' and takes type-identity. I will consider this objection in Section 5.3.

special to wanting a *glass of water* and not wanting something else, like a piece of chocolate.

In the real world, α cannot be instantiated alone. 'Wanting' is always 'wanting a particular thing by a particular creature'. Therefore, at the fine-grained neurological level we have to include all the additional and irrelevant causal powers, while at the more abstract psychological level we can eliminate these powers and only mention the relevant causal powers. Therefore, the discussed psychological law, saying that the only relevant powers to γ are α and β, is a coarse-grained version of a set of infinite neurological laws, laws like Mary's case that says *s1* and *s2* are sufficient for *s3*.

The last point about the proposed analysis of the unity of science is its ability to account *ceteris paribus* clauses in special-science laws. It is a familiar point that special-science laws have exceptions and so are not universally true. In our last example, it is possible that Mary wants a glass of water and believes she can get it by opening the fridge, but she will not do it simply because a doctor prohibited her from drinking water. In such cases, α and β are present, but they cannot bring about γ. Therefore, it seems that these cases falsify the special-science law, which claims the only relevant powers are α and β.

In reply, it might be said that the causal powers of some interfering psychological states (e.g. believing in doctor's advice) can block manifestations of some causal powers belong to α or β. If so, then α and β no longer bring about γ. In other words, it is possible that some irrelevant causal powers block manifestations of some relevant causal powers, and therefore block the manifestation of the universal behaviour. In these cases, the coarse-grained laws have exceptions. A familiar example of this phenomenon is antidote. By covering a sugar cube in a plastic cover, its water solubility will not manifest, although the cube still has the relevant causal powers (e.g. a particular molecular structure).

In summary, given that what a law says about the properties it links is just what it says about the causal powers they confer, and given the causal analysis of realization we can save a version of the unity of science. The content of a special-science law that uses multiply realizable properties is a coarse-grained version of the contents of a set of basic realizer laws. These basic laws mention some irrelevant factors to a universal behaviour, while the special-science law puts all the basic laws together, extracts their common part, and only mentions the relevant factors to the universal behaviour.

4.2.2 Kim's Argument against Special Sciences

In contrast to anti-reductionists who argued for the disunity of science by means of multiple realization, some reductionists appealed to this notion and argued that if we take it seriously it is strong enough to undermine the generality and autonomy of the special sciences. Kim (1992) presented such an argument, which I will first explain briefly and then examine its plausibility according to the causal analysis of realization.

Let us start with the point that according to Kim, bridge principles must be *biconditional* statements expressing coextensionality between terms of the reduced and reducing theories. Kim (1992, 317) is aware that Nagel did not require this. However, he has his own reason to insist on biconditional statements. He thinks that an ontologically significant reduction should reduce higher-order properties, by identifying them with basic properties. In other words, bridge principles must be property identities and not mere property correlations[38] (Kim 1992, 317). Therefore, he concludes that because coextensionality is a necessary condition for property identity, bridge principles must be biconditional.

As I mentioned in Section 3.4, ontological simplicity is one of the motivations for proposing identity statements as bridge principles. In addition, I mentioned that by adopting an appropriate ontology and by rejecting the layered picture of properties, this motivation would be irrelevant. Now it is the right time to see how the scientific ontology, according to the causal analysis of realization, is one-level and flat, such that it is not a task of reduction to simplify it more.

In the causal account of realization, realized and realizer properties belong to the same level. Remember that in this framework, conditional causal powers are building blocks of the ontology, such that in terms of them we analyze properties. There is a flat level of conditional powers. Any property, whether realized or realizer, bestows a set of powers, powers individuate properties, and differences between properties are only differences between their conditional powers. An important relation here is the subset relation. The conditional powers of a realized property are a subset of the conditional powers of its realizers. Therefore, according to one interpretation, a realized property is just a subset of its realizers; while on another interpretation,

[38] Kim in this paper (1992) rejects his former view (Kim 1966). As we discussed in Section 3.5, Kim (1966) first believed that reduction does not need identity statements as bridge principles. However, here he says that an ontologically significant reduction must identify properties. These two views are inconsistent, but what Kim entails from both of them is the same: he insists that bridge principles must be *biconditional.*

instantiation of a realized property is a subset of instantiations of its realizers. All these points show that we do not have a layered picture of properties. By means of the subset relation (one-way conditional relation), we have simplified the ontology, such that there is no mysterious relation between basic and multiply realizable properties. Therefore, we do not need to simplify our ontology by identifying high-level properties with basic ones (two-way conditional relation). Hence, philosophers' justification for requiring identity statements as bridge principles disappears.

Now let us return to Kim's main point. Kim believes that in a biconditional bridge principle, both sides must be *kind* predicates and therefore suited for laws. Assume that M is a high-level property that is identified with Q in a biconditional bridge principle: $M \leftrightarrow Q$. If Q is a non-kind predicate, then M could no longer figure in special-science laws (Kim 1992, 318). The reason is simple: given the bridge principle, we can substitute M by Q in all special-science laws[39]. However, if Q is a non-kind and cannot figure in laws, the results of substitution are no longer laws. Therefore, the original statements are no longer laws and M could not figure in any law. But we want to save special-science laws, so Q must be a kind predicate.

The next step in Kim's account is that the sort of Q that we are dealing with under multiple realizable properties, i.e. disjunction of heterogeneous kind predicates, is a non-kind and unsuited for laws. If so, special-science predicates are non-kind and therefore, special sciences are not general and genuine sciences. Kim has two arguments to show that coextensive predicates with special-science predicates are non-kind. I will present these arguments here.

The first argument has an epistemic nature. Assume that we are told that jade is not a mineral kind, rather it covers two distinct minerals with two distinct molecular structures: jadeite and nephrite. Now consider the following generalization: (L) jade is green. Under the new circumstances, we no longer consider this statement, which we had considered as a well-confirmed law, as a law. It is a conjunction of two separate laws: (L1) jadeite is green, and (L2) nephrite is green. Kim (1992, 319) argues that (L) does not have the 'projectibility' that is a standard mark of lawlikeness, 'the ability to be confirmed by observation of "positive instances"'. Suppose we discover that all of the observed samples of green jade that confirmed (L), were samples of jadeite. It means that they confirmed (L1) and not (L2). Now

[39] If the bridge principle is an identity statement, then apparently substituting M by Q does not affect lawlikeness, i.e. if a sentence containing M is a law, by substituting M by Q we still have a law. However, if the bridge principle is a coextensionality, then the substitution might affect lawlikeness. As Dretske (1977, 250) says, 'If it is a law that all F's are G, and we substitute the term "K" [that is eternally coextensive with "F"] for the term "F" in this law, the result is not necessarily a law.'

what is the situation of (L)? Is it confirmed by these evidences or not? Kim (1993a, 320) thinks that '...we clearly would not, and should not, continue to think of (L) as well confirmed'. However, there is a problem here,

> ...all the millions of green jadeite samples *are* positive instances of (L): they satisfy both the antecedent and the consequent of (L). As we have seen, however, (L) is not confirmed by them, at least not in the standard way we expect. And the reason, I suggest, is that jade is a true disjunctive kind, a disjunction of two heterogeneous nomic kinds which, however, is not itself a nomic kind. (Kim 1992, 320)

In other words, Kim thinks that there is a conflict here. We do not want to consider the supportive evidence of (L1) as supportive evidence of (L), but they satisfy both the antecedent and consequent of (L), and so ought to confirm it[40]. The reason for this conflict, according to Kim, is the disjunctive nature of jade.

Kim's second argument to show that disjunctive predicates are unsuited for laws has a metaphysical nature. First, he introduces 'The Principle of Causal Individuation of Kinds':

> Kinds in science are individuated on the basis of causal powers; that is, objects and events fall under a kind, or share in a property, insofar as they have similar causal powers. (Kim 1992, 326)

By using this principle, he concludes that if *P*1, *P*2 and *P*3 (realizers of *M*), are heterogeneous as kinds, they are heterogeneous as causal powers. The next step is introducing 'The Causal Inheritance Principle':

> If mental property ***M*** is realized in a system at *t* in virtue of physical realization base ***P***, the causal powers of *this instance of **M*** are identical with the causal powers of ***P***. (Kim 1992, 326)

Now Kim argues that the latter principle, in conjunction with the physical realization thesis, includes mental kinds cannot satisfy the former principle and therefore are not scientific kinds.

> Instances of ***M*** that are realized by the same physical base must be grouped under one kind, since *ex hypothesi* the physical base is a causal kind; and instances of ***M*** with different realization bases must be grouped under distinct kinds, since, again *ex hypothesi*, these realization bases are

[40] One reason that we do not want to consider the supportive evidence of (L1) as supportive evidences of (L) is that if we allow them to confirm (L), because (L) is simply a conjunction of (L1) and (L2), they confirm (L2) as well, whatever it is. It means that they will confirm any arbitrary generalization (L2).

> distinct as causal kinds. Given that mental kinds are realized by diverse physical causal kinds, therefore, it follows that mental kinds are not causal kinds, and hence are disqualified as proper scientific kinds. (Kim 1992, 327)

As a result, Kim concludes that disjunctive predicates cannot figure in laws, and because special-science predicates are coextensive with this kind of predicates, they are non-kind as well. Therefore, there are no general special-science laws.

4.2.3 The Possibility of Special Sciences

To reply to the epistemic argument I would like to emphasize that, in the context of realization, we do not discuss the disjunction of *any* two properties, we consider the disjunction of two *realizing* properties that, according to the causal view, have something in common. Suppose that the property 'being jade' bestows a set of conditional powers α. Therefore, the law (L) can be re-written as: (L*) whatever has the set of conditional powers α is green. In addition, suppose that being jade is a multiply realizable property: being jadeite and being nephrite are its realizers. According to the causal view, we can represent their conditional powers as $\alpha + \alpha 1$, and $\alpha + \alpha 2$ respectively. Therefore, laws (L1) and (L2) can be re-written as these: (L1*) whatever has the set of conditional powers $\alpha + \alpha 1$ is green, and (L2*) whatever has the set of conditional powers $\alpha + \alpha 2$ is green. Now according to our previous discussion, we cannot infer (L*) just from (L1*). In other words, supportive evidence of (L1*) *is not* sufficient to support (L*). According to (L1*), we know that having conditional powers $\alpha + \alpha 1$ is sufficient to be green. However, we cannot conclude that having α is the *only* relevant factor to be green. It means that there is no conflict here. We thought that (L*) was true, now we do not know whether it is a law or not. Our current supportive evidence only supports (L1*) and not (L*). So, Kim's contention that any supportive evidence of (L1*) is supportive evidence of (L*) is wrong.

It should be noted that the situation here is different from the situation in table 3. In table 3, we know that law (0) is a coarse-grained version of the set of laws (1, 2 ...). In other words, because we have a vast range of basic laws the inference of the special-science law from them is highly confirmed. This means that $\alpha 1$, $\alpha 2$ ... and $\beta 1$, $\beta 2$... are irrelevant. However, in this example and by limiting evidence that only support (L1*) we do not know whether $\alpha 1$ is irrelevant or not. Irrelevancy of $\alpha 1$

means if $a1$ is replaced by another factor like $a2$, a still brings about greenness. Therefore, we need supportive evidence for (L2*) as well. In other words, to say that (L*) is a coarse-grained version of (L1*) and (L2*) the minimum requirement is supportive evidence for (L2*). In the current situation, it is not the case.

But why does Kim think that supportive evidence of (L1*) supports (L*) as well? Suppose that we have a multiply realizable property *P*, with two realizers *P1* and *P2*. Predicate '*M*' designates *P1* and predicate '*N*' designates *P2*. In addition, suppose that a, $a + a1$, and $a + a2$ are the sets of conditional powers of *P*, *P1* and *P2* respectively. Now if we make the disjunctive *predicate* '*M* or *N*' that designates *P*, its corresponding set of conditional powers is *not* a union of corresponding conditional powers of the disjuncts, i.e. $\{a + a1\}$ U $\{a + a2\}$. However, its corresponding set of conditional powers is the *intersection* of corresponding conditional powers of the disjuncts, i.e. $\{a + a1\} \cap \{a + a2\}$, which is a. (See Table 4)

	Realizer	Realizer	Realized
Property	*P1*	*P2*	*P*
Predicate	'*M*'	'*N*'	'*M* or *N*'
Causal Powers	$a + a1$	$a + a2$	$\{a + a1\} \cap \{a + a2\} = a$

Table 4: The Apposite Direction of Union and Intersection in Disjunctive Predicates and Properties

Kim thought that the corresponding conditional powers of the realized property (being jade) is the union of the conditional powers of the realizing properties (being jadeite and being nephrite), i.e. $\{a + a1\}$ U $\{a + a2\} = \{a + a1 + a2\}$. Therefore, he concludes that the supportive evidence of (L1*) support (L*) as well, the claim that is false in the causal framework of realization[41].

Now let us consider Kim's second argument. Firstly remember that, as mentioned earlier, Kim has modified his Causal Inheritance Principle and added the possibility that the causal powers of the realized property might be a *subset* of causal powers of the realizing property. Secondly, it is true that when two kinds or properties are

[41] Continuing the epistemic argument Kim (1992, 321) says: '... disjunctive properties, unlike conjunctive properties, do not guarantee similarity for instances falling under them. And similarity, it is said, is the core of our idea of property'. I think it should be clear now that in the case of multiple realization we have disjunctive 'predicates', but regarding properties we still have conjunction (intersection) of conditional powers that saves similarity. For a similar point see Clapp (2001, 125-132).

heterogeneous, their causal power sets are heterogeneous as well. However, this point does not mean that these two sets do not have a common subset. In other words, the heterogeneity of two sets of causal powers does not conclude that there is no common causal power between them. As I mentioned earlier, in the case of realization, the causal power sets of two realizers could be distinct (heterogeneous) in one sense, but homogeneous in another sense. If we consider the causal powers common between them, they are homogeneous, and if we consider their unique causal powers, they are heterogeneous.

Kim's argument is fallacious, simply because in the case of multiple realization, although each realizer *P1*, *P2* and *P3* has a unique set of causal powers, there is a common set of causal powers between them. Whenever *M* is realized, no matter whether by *P1*, *P2* or *P3*, *M* will have this common subset of causal powers, and therefore it can be qualified as a property according to "The Principle of Causal Individuation of Kinds".

Kim concludes from his arguments that because a special-science predicate is coextensive with a disjunctive predicate, and the latter is not a kind, therefore the special-science predicate is not kind-coextensive. Hence, there can be no general and autonomous discipline like psychology[42]. My arguments show that special-science predicates could designate multiply realizable properties, with unified causal powers, and therefore they are kind predicates and we could have general special sciences.

I also hope my argument provided an answer to what Fodor (1997, 160-1) calls a 'metaphysical mystery': 'How can there be macroregularities that are realized by wildly heterogeneous lower level mechanism?' (cf. Batterman 2000, 115) According to the causal analysis, there is a common subset of causal powers between all realizers of a multiply realizable property, and in virtue of having this subset, they realize that property. In addition, I hope my argument showed that special-science properties are reducible according to Papineau's (1993, 35) criterion:

> Reducibility to physics does not involve the absurdly strong requirement that the instances of the reduced category should share *all* their physical properties. The requirement is only that there should be *some* physical property present in all and only those instances, which then allows a uniform physical explanation of why those instances always give rise to a certain sort of result.

[42] For different objections on Kim's argument, see Fodor (1997), Block (1997), Batterman (2000 and 2002), Shoemaker (2001), Clapp (2001), and Gillett (2003).

What Papineau calls the requirement of reducibility is exactly the definition of multiple realization according to the causal framework.

***

Taking everything into consideration, I would like to conclude this chapter with the following statements. (i) The causal analysis of realization is one of the available accounts to cover the notion of multiple realization. This account in its standard form is defendable, and its dimensioned version, or fears about the impossibility of *multiple* realization, do not threaten it. (ii) Given the causal analysis of realization, we can defend a version of the unity of science: the nomological content of a special-science law is a coarse-grained version of the contents of a set of its basic realizer laws. (iii) This analysis provides a flat ontology, and therefore takes the burden of simplifying the ontology on the shoulder of reduction. (iv) Given this analysis, general special sciences are available.

5

Multiple Realization (II): A Flat Analysis of Properties

In this chapter, I will present an alternative analysis of the nature of properties, the relationship between properties and predicates, and a new account of multiple realization. The main reason that we need this new account is that the dominant picture of ontology ('the level picture of reality') suffers from serious problems. Firstly, I will explain the level picture of reality that is based on a semantical analysis of properties. In the next section, I will enumerate three main problems with this picture which are sufficient to reject it. After that, some problems with the causal analysis of realization will be discussed. These two sections lead us to the flat analysis of properties, that I will explain its key features and the new interpretation of multiple realization according to this analysis in the next section. The next step is to show the advantages of the flat view over the level picture of reality.

By having enough metaphysical grounds, I will move on to the philosophy of science and consider the possibility of having a version of the unity of science according to the flat analysis. In the next section, I will discuss the second wave of objections against the classical model of reduction, based on the multiple realization notion, and then consider the situation of the classical model according to the two metaphysical frameworks discussed. Finally, I shall finish this chapter by comparing the causal and flat views on properties and a discussion about their advantages and disadvantages.

5.1 The Level Picture of Reality

Following Heil (2003, Ch.2) I call the picture of reality accepted by the vast majority of philosophers 'the level picture'. To understand it, we first need to know more about the basic presupposition behind this. According to this presupposition (The Picture Theory), there is a close relationship between language and reality such that linguistic entities reflect ontological entities and we can discover the structure of reality by knowing the structure of language. As Heil (2003, 23) puts it 'The conception in its most general form is that language pictures reality in roughly the sense that we can 'read off' features of reality from our ways of speaking about it.'

One particular correspondence between linguistic and ontological elements, which is our concern here, is the correspondence between predicates and properties. According to this conception, any predicate that applies, or would apply, truly to objects, designates a property. It means that properties are referents of predicates, and more importantly, objects that satisfy a predicate either share one and the same property (if we consider properties as universals), or have exactly similar tropes (if we consider properties as particulars). For example, because we make true claims using the predicate 'is red', there is a property corresponding to it and both my red pen and a red apple share it or are exactly similar in respect of it. Heil (2003, 26) puts this semantical approach to properties under Principle (Φ),

> When a predicate applies truly to an object, it does so in virtue of designating a property possessed by that object and by every object to which the predicate truly applies (or would apply).

Now it can easily be seen how philosophers could infer the level picture of reality from this principle. We have a layered structure of predicates. At the basic physical level, we have physical predicates that are projectible and truly used for objects. Therefore, there is a level of properties corresponding to them. At a higher level, we have special sciences with their own laws and projectible predicates. Therefore, there are high-level properties corresponding to them. To sum up, corresponding to hierarchical structure of predicates, from the basic fundamental physical to mental, we have a hierarchical structure of properties.

But what is the relationship between these levels of reality? According to a popular view, these levels have two characteristics: the first is that, their entities and properties are not reducible to the lower levels; and the second, that there is a dependency relation between them. This means that we cannot reduce mental properties, for example, to physical properties, (the most important reason for irreducibility being multiple realization); however, the former are dependent on the latter. In this context, dependency means we cannot remove the lower-level items without thereby eliminating the higher-levels. The most popular relation for expressing dependency is *supervenience*: two objects cannot be identical regarding the basic properties while they are different regarding the high-level ones. The following diagram represents a piece of this network.

$$
\begin{tikzcd}
P \arrow[r, "R1"] \arrow[d, "R2"'] & Q \arrow[d, "R4"] \\
p \arrow[r, "R3"'] & q
\end{tikzcd}
$$

Figure 1: A piece of the mesh of high-level and low-level properties according to the level picture of reality

Here P and Q are high-level properties, p and q are lower-level properties, $R1$ and $R3$ are causal relations, and $R2$ and $R4$ are realization relations (p is a realizer of P and q is a realizer of Q).

To see a concrete usage of the semantical approach and a solution to the problem of multiple realization, let us consider Hooker's proposal. He (1981) believes that we have reduction between scientific theories only by means of identity statements between properties. However, the standard problem is that some high-level properties (like boiling in macrophysics) can be realized by different basic properties (like many particular molecular processes), and therefore we cannot identify the high-level property only with one low-level counterpart. To solve the problem he suggests (1981, 498) that we can enrich our high-level language by adding proper predicates (like 'boils in manner Φ') so that for any different manner of boiling we would have a different predicate. Now by the presupposition that there is a correspondence between predicates and properties, we have an enriched set of high-level properties and we can identify any basic property with one of them. Therefore, with a finer-grained view of the high-level properties and by having identity statements as bridge principles, we avoid the problem of multiple realization.

Apart from general problems with the semantical approach that I will examine in the next section, this particular suggestion has the problem that it begs the main question. According to the multiple realization argument, different basic properties could realize the same high-level property. Now, using a finer-grained set of properties at the high level does not change this situation. It is possible, and, according to the multiple realization argument, true, that two different molecular processes bring about boiling in exactly the same way, i.e. both of them realize the fine-grained high-level property *boils in manner Φi.* To show that there is a one-to-one correspondence between basic and high-level properties Hooker needs an argument to show that it is impossible that two different basic properties realize the same fine-grained high-level property (*boils in manner Φi*). In other words, he needs an argument to show that multiple realization is impossible. This is exactly our main concern here, and we cannot simply suppose it to solve the problem of multiple realization.

I will discuss three main objections to the level picture of reality later. However, let me present here a general metaphysical argument against any *unrestricted* semantical approach to properties like Principle (Φ). According to this principle, *any* general term that can be used truly as a predicate designates a distinct property. However, this leads to paradox. Consider for example the predicate 'does not exemplify itself' (Loux 2002, 35-6). This predicate can be used truly. Many objects like the Taj Mahal satisfy it: the Taj Mahal is not self-exemplifying and therefore the sentence 'the Taj Mahal does not exemplify itself' is true. On the other hand, there are things and certain universals that do not satisfy the predicate. The property of being incorporeal, for example, exemplifies itself and so does not satisfy the predicate. Now let us suppose that according to Principle (Φ), there is a property corresponding to this predicate. However, this leads to a paradox (cf. Russell's Paradox), because this property either exemplifies itself or it does not. In the first case, since it is the property a thing exemplifies just in case it does not exemplify itself, it turns out that it does not exemplify itself. On the other hand, if the property does not exemplify itself, then it turns out that it does exemplify itself, because it is the property of being non-self-exemplifying. (A more straightforward example is those predicates that express contradictory or incoherent concepts. There is no property corresponding to them.)

This argument shows that to avoid such paradoxes, we must restrict Principle (Φ) so that only a narrow class of predicates directly designates properties. How can we do that? I will explain Heil's suggestion later, but let me first consider three main problems with the level picture of reality first.

5.2 Problems with the Level Picture

In this section, three main problems with the level picture of reality will be discussed.

5.2.1 The Problem of Causal Powers

Suppose that *P* is a high-level property and *p1, p2* and *p3* are its physical realizers. If an object has either *p1* or *p2* or *p3*, it has *P*, and if it has *P*, it has *p1* or *p2* or p3. There is an important question here: is *P* distinct from its realizers? In other words, how many distinct properties do we have here, three (*p1, p2, p3*) or four (*p1, p2, p3* **and** *P*). Reductionists say that *P* is not a distinct property, over and above its realizers. However, devotees of the multiple realization and level picture say *P* is a distinct and irreducible property. Before we consider this claim, we must have an account of the nature of properties. What is a property? According to Heil (1999, 192) in most (if not all) discussions on the multiple realization notion, it is implicitly assumed that the identity of a property is determined by the causal powers that it contributes to the object which has the property. This account of properties is presented and defended explicitly by many philosophers such as Shoemaker (1980), Kim (1993a), and Mellor (1991). Suppose being spherical is a property. This property has a cluster of causal powers such that when an object has this property it has those causal powers, and by virtue of them can enter into causal relations. For example, a spherical object is typically capable of rolling, of reflecting light in a specific way, and so on. The main point in this account is that we determine and recognize properties by their causal powers and by their effects on other things. If a property does not have any causal power, then we cannot recognize it and then we cannot speak about and use it. In terms of identity condition, it can be said that two causally efficacious properties are identical if, and only if, they have the same causal powers.

Now let us return to our question: if *p1, p2*, and *p3* are realizers of *P*, if we accept that *P* is distinct from *pi*s and that causal powers are essential for properties, then we have to show that the causal powers of *P* are different from the causal powers of *pi*s. Let us examine some different proposed relations between the causal powers of *P* and the causal powers of *pi*s.

The first proposed relation is that the causal powers of *P* make up a subset of the causal powers of each of its realizers. Let us suppose that in object *O*, *p*1 is the realizer of *P* and *p*1 has four causal powers {*c1, c2, c3, c4*}. According to this proposal, the causal powers of *P* must be associated with a subset of this set e.g. {*c1, c2, c3*}. This is the standard causal account of realization, which we discussed in the previous chapter, and has the advantage that it makes clear how each of the realizers of P is sufficient but not necessary for P. If object O has $p1$ then it has $\{c1, c2, c3, c4\}$ and so it necessarily has $\{c1, c2, c3\}$, and therefore having $p1$ is sufficient for having P. Another advantage of this proposal is that it can provide an account of multiple realization that is compatible with functionalism. According to functionalism, different creatures can have one common mental property whereas non-mental effects of this property are different for each of them. If being in pain is a mental property then non-mental effects of it are different in humans and octopuses. Let us assume P is being in pain and has causal powers $\{c1, c2, c3\}$. $p1$ is realizer of P in human beings and has causal powers$\{c1, c2, c3, c4\}$, and $p2$ is realizer of P in octopuses and has causal powers $\{c1, c2, c3, c5\}$. We can see that causal powers of $p1$ and $p2$ are different and so can inter into different causal chains. This point allows the non-mental effects of P in these two realizations to be different. There are some objections to this causal account of realization and more particularly to Shoemaker's revised version of it, but let me leave them to the next part of this chapter, when I will consider problems with the causal analysis of properties in detail, and continue with two other proposed relations between the causal powers of P and its realizers.

The second possible relation is that the causal powers of P include all the causal powers of each of its realizers. Suppose $p1$ has $\{c1, c2, c3\}$. The causal powers of P must include all of these causal powers and also some additional elements: $\{c1, c2, c3, c4\}$. This proposal has the advantage that it does not allow $p1$ to absorb P, because P has some more powers than $p1$.

However, this proposal has some problems. Firstly, it is not clear how we can explain that $p1$ is sufficient and not necessary for P. If object O has $p1$, it must have something more $\{c4\}$ to have P and so $p1$ is not sufficient for P. The second problem is more general. Suppose P is being in pain and $p1$ is its realizer in humans. Now the question is: when P is realized in a human, how can we recognize and detect $c4$? $c4$ is a causal power such that if someone is in pain then she has it, but it is not present in the physical realization of pain in humans. Hence, being in pain has some causal powers over and above those bestowed by its physical realizers. We do not have any way to recognize such transcendental causal powers, because we cannot detect them in physical realizations of pain. If we say P does not have $c4$, then P is identical with $p1$ so P is not a high-level property, but a physical one.

Another possible relation between the causal powers of P and its realizers is that P has some, but not all, of the causal powers of its realizers and, in addition, causal powers that each of its realizers lacks. Suppose $p1$ has causal powers $\{c1, c2, c3, c4\}$

and *P* has {*c2, c3, c4, c5*}. The problem with this proposal is obvious because it is not clear how *p1* can be sufficient for *P*.

The last proposed relation is that each of the realizers of *P* has all of its causal powers but in addition, each of them has some inhibitory causal powers that neutralize some causal powers of *P*. This proposal can solve the problem of sufficiency of *pi*s for *P*, but has another problem. How can we recognize and detect those causal powers of *P* that always neutralize in an object *O*. What is our reason and evidence that when a person is in pain, she has some causal powers but these causal powers are neutralized and are not accessible? It seems that we just assume these causal powers to solve the problem of high-level properties and they do not exist in reality.

As a result, if we accept that properties are identified with their causal powers, the *only* available way to show that there is a level picture of reality, i.e. there are *distinct* high-level properties, is to accept the standard causal analysis of realization: the causal powers of a realized property are a subset of the causal powers of the realizer properties. However, this particular account has its own problems that we will discuss later.

5.2.2 The Problem of Causal Relevance

Heil (2003, Ch. 2) and some other philosophers (e.g. Kim 1993b) believe that the level picture of reality suffers from the overdetermination problem. As mentioned earlier, defenders of the level picture believe that *R1* in Figure 1 is a causal relation, because *P* and *Q* are properties and enter into causal chains. Let us suppose that *R1* and *R3* are causal relations. Now there is a question about whether *R1* is a *distinct* causal relation from lower-level causal relations (like *R3*). We know that *R1* is dependent on *R3,* because by eliminating *R3* we would eliminate *R1,* but here our question concerns its *distinctness*. Some anti-reductionists, such as Fodor, say that *R1* is distinct from *R3* because high-level laws are distinct from and irreducible to lower-level laws. Therefore, high-level causal relations that are supported by high-level laws are distinct from lower-level causal relations. Let us suppose that *R1* is a distinct causal relation from *R3*. In this case, the event of 'having *Q*' is overdetermined: having *Q* has two distinct causes. On the one hand, because *P* is cause of *Q*, having *Q* is caused by having *P*. On the other hand, if we have *P*, we have its realizer *p*, and according to *R3* having *p* is cause of having *q*. Because *q* is a realizer of Q, by having the latter we have the former. In other words, there are two distinct causal ways from P to Q: one directly from P to Q and the other via p and q. To remove this objection, someone might say that having P is the cause of having Q, but not a direct cause. Having P directly causes the object to have q and whereas q is a realizer of Q, the object has Q. This alternative view has two problems. Firstly, we have *causation from above* here (having P is cause of having q). For some philosophers, especially who defend completeness of physics and physical causation; this is not an allowable result. According to them, any physical event like having q has one sufficient physical cause. Causes from outside the physical realm threaten the autonomy of physics.

In addition, it should be noted that in this model having q is overdetermined. Having q has two distinct causes: having P and having p. The reason for the latter is that in our particular case when the object has P it has that by having the realizer p, and there is a causal relation between p and q. Therefore, apart from the direct causal way from P to q, there is a second way from p to q that means overdetermination of q.

As a result, assuming the level picture of reality and the assumption that high-level causal chains are *distinct* from lower-level chains entails the overdetermination of macro or micro-events. Many philosophers believe that there is no systematic causal overdetermination. This means that the level picture is problematic and we need an alternative account for the situation of high-level laws.

5.2.3 The Problem of the Inter-Level Relation

As mentioned earlier, the level picture of reality has two components: irreducibility of high-level entities, and their dependency on lower levels. The last objection concerns the second component. A proponent of the level picture should look for a special relation between levels that is not reduction but captures the notion of dependency. The most commonly proposed relation is supervenience. High-level properties supervene on lower-level properties, i.e. two objects cannot be identical in respect of basic properties while they are not identical in respect of high-level properties. However, many philosophers (e.g. Heil (2003, Ch. 4), Kim (1998) and Horgan (1993)) believe that supervenience is just a modal relation and is silent on the nature of the dependence/determination relation. Kim asserts that supervenience is compatible with reduction, emergence and realization. Similarly, Heil believes that if the set of Ps supervenes on the set of ps, then the supervenience is compatible with all of these possibilities: *P*s are *p*s, *P*s wholly made up of *p*s, *P*s are caused by *p*s, or *P*s and *p*s both have a common cause. Therefore, the fact that high-level properties supervene on lower-level properties does not show that we have the dependency relation here. We need a more sophisticated relation to capture the dependency of high-level on lower-level properties.

5.3 Problems with the Causal Analysis of Realization

In this section, first I will present Heil's objections to the causal account of realization. After that, I will mention some ways to defend the causal analysis against these objections. Finally, I will discuss my own objections to the causal analysis of realization.

Heil (2003a, 21) believes that if we discover *very simple* properties, properties that bestow only one causal power, and if we realize that these simple properties are realizers of some other properties, then the causal account would be in trouble. This is because the causal powers of the realized property are a subset of causal powers of the realizers. Therefore, by rejecting identity between the realized and realizing properties, the latter must have at least two causal powers. This situation is in contrast with the assumption of very simple properties as realizers.

However, Heil does not take this problem very seriously. According to him (2003 a, 22), 'It could easily turn out that the most fundamental properties are not realizers of any property.' In other words, it could turn out that the assumption of very simple realizers is not valid, and therefore we cannot reject a metaphysical model of properties by a conjecture about the structure of actual world.

Apart from this, Heil has two metaphysical objections to the causal model. The first concerns that interpretation of properties that holds a property is identical with a cluster of causal powers and therefore the realized property is a part of the realizing ones. The second objection concerns Shoemaker's revised version, according to which although properties are identified by causal powers but are not identical with clusters of them. Therefore, according to the revised version, although the instantiation of the realized property is still a part of instantiations of its realizers, the realized property is not a part of its realizers. Let us start with the first objection.

According to Heil (2003a, 22), 'Arguments for multiple realization, after all, have typically been advanced against the possibility of 'type identity.'' A common presupposition of these arguments is that finding a *physical* property identical with a multiply realizable property is very unlikely. Now, Heil says, according to the first interpretation of properties (i.e. properties are clusters of causal powers), it turns out that every multiply realizable property is identical with a *physical* property. Suppose that P is realized by $p1$, $p2$ ..., and $c1$ and $c2$ are causal powers of P that therefore are common in all pis. Now there are two possibilities here. If the set of $c1$ and $c2$ does not correspond to a real property, then P is not a real property either. As Heil (2003a, 22) says,

> Perhaps the powers associated with a multiply realizable property do not correspond to a fully fledged property, but only to a proper part of various distinct fully fledged properties. This would mean that predicates picking out multiply realizable properties did not pick out properties, after all, but only collections of powers that go into the makeup of many different distinct properties.

On the other hand, if this set corresponds to a *physical* property, it follows that this physical property is identical with P (because according to the causal account if two properties have the same causal powers they are identical). Heil (2003a, 22) puts the point as follows,

> Now it turns out that every multiply realizable property is identifiable with some physical property, the property corresponding to a collection of causal powers that make up a proper subset of the causal powers that correspond to assorted realizing properties.

In other words, Heil tries to show that, according to the first interpretation, either multiply realizable properties are not real properties, or they are identical with *physical* properties and therefore we have type identity, which seems ironic.

This argument does not seem persuasive. It is obvious that a proponent of the causal account of realization should take the second horn of the dilemma more seriously. He believes that the set of $c1$ and $c2$ corresponds to a property. However, it is not clear what Heil has in mind when he claims that this set corresponds to a ***physical*** property. Firstly, he does not define a *physical* property, and does not distinguish physical properties from other possible kinds of properties like the mental. Secondly, he does not explain why the corresponding property with the set of $c1$ and $c2$ is a physical one. Therefore, in his reply, a proponent of the causal account might say that of course the set of $c1$ and $c2$ corresponds to a property. However, why should we say that this is a *physical* property? The property corresponding to the set is P and nothing else. We do not have two properties here, *P* and a physical property. We have only a multiply realizable property, *P*, corresponding to this set.

This response, I think, is acceptable in the causal framework of properties. Here our basic entities are causal powers. There is a flat level of causal powers, and these powers in different combinations make different properties. The distinctions of physical/mental or realized/realizing properties are not genuine ontological distinctions, and if they do make any sense, they just distinguish those properties that appear as part of other properties (mental, realized) from those that do not (physical, realizing). If this conclusion does not match our previous idea of multiple realization, it is not a problem with this account. It only shows that our previous intuition was wrong and we must replace it with the new one. The claim that the set of causal powers *c1* and *c2* corresponds to a *physical* property, and therefore the multiply realizable property is identical with a physical counterpart does not make sense in the causal analysis of properties. The distinction between physical and mental shows nothing ontologically important here.

It seems to me, however, that the first interpretation of the causal account suffers from a deeper problem. Probably the most important sort of multiply realizable properties are mental properties, like being in pain. In addition to the potentiality of doing something (causal powers), these properties have phenomenal characteristics, which are accessible only to us as owner of them. Any adequate account of mental properties must first recognize and accept these characteristics, then try to put them in the picture and explain their relations with the causal powers of mental properties. The first interpretation of the causal account reduces properties to causal powers and does not explain what the relationship between causal powers and phenomenal (qualitative) features is. Therefore, it simply ignores phenomenal features (or phenomenal properties) and cannot be a proper theory to deal with mental properties. Probably the same reason led Shoemaker to revise his account and withdraw the view that properties are identical with causal powers. Therefore, even if the first interpretation can solve the problem of multiple realization, it is not an appropriate account for mental properties, and the situation of mental properties is still questionable accordingly.

Regarding the second interpretation of the causal model, Heil has other objections. We know that Shoemaker (2001, 94 n.11) rejects his previous view, however he explains his new alternative briefly,

> At one time I thought that one could simply *identify* a property with a "cluster" of conditional powers having a certain kind of unity. If one could do that then, because the conditional powers associated with the property of being in pain is a proper subset of each of the sets of conditional powers associated with the properties that realize pain, one could say that the property of being in pain is literally a *part* of each of the realizers properties. In that case there is certainly no question of the realizer properties "pre-empting" the realized property with respect to causal efficacy – not if the realized property is a part of the realizing property, and is the part that includes the conditional powers involved in the episode of causation. For reasons I cannot go into here, I no longer want to identify a property with a cluster of conditional powers [...]. While rejecting that identification bars me from construing realized properties as parts of their realizer properties, it does not bar me from construing instances of realized properties as parts of instances of realizer properties.

This passage is short and vague enough to allow different interpretations. Heil has his own interpretation: in Shoemaker's new account, properties are distinct and different in kind from powers, however they bestow powers when they are instantiated,

> Properties are not powers; properties bestow powers when instantiated. One way to understand this idea is to suppose that a property's being instantiated is just a matter of its bestowing powers on some particulars. The property (a universal) is one thing, its instances (the power it bestows) are another. Properties are distinct from their instances but act *via* their instances. In describing an object as possessing a property, we are speaking obliquely: the property is not strictly speaking 'in' the object. Rather the object possesses certain powers (these *are* 'in' the object), which are the property's instances. (Heil 2003a, 24)

Let us accept this interpretation for the moment and see Heil's (2003a, 24-5) objections to it. The first objection concerns the reason that we still need properties. What do they do for us that we must keep them in our ontology? They are separate and distinct in kind from their instances; however, the action is all in the instances. Therefore, if we suppose that all a property does is bestow causal powers, and if we accept Heil's interpretation that properties act via their instances, then there is no longer a need to keep properties in addition to their instances in our ontology. An obvious reply to this objection is that a property does something more than bestowing causal powers. This leads us to a different interpretation of Shoemaker's account, which I will discuss shortly.

Heil's second objection is not clear and valid, as far as I can see. As mentioned, Shoemaker explicitly says that according to his new account of properties the realized property is not a part of its realizers. However, Heil (2003a, 24) says,

> Second, it is hard not to see realizing properties as complex properties that include realized properties as constituents. Shoemaker rejects the identification of realized properties with their realizers, but this leaves open the possibility that realized properties are components of realizing properties. Were that so, were mental properties, for instance, constituents of physical properties, then any distinction between mental and physical properties would be difficult to uphold and the point of talk of realization-realization as opposed to reduction – were undercut.

Because this objection is based on the possibility that a realized property is a component of its realizers and Shoemaker explicitly rejects this possibility, I do not take it seriously and conclude that according to Heil's interpretation of Shoemaker's new account of properties, we no longer need properties in addition to their causal powers.

Apart from Heil's interpretation, different readings from Shoemaker's view are possible. As mentioned, a problem with the view that properties are clusters of causal powers is that it ignores phenomenal features of properties. Therefore, it is possible that Shoemaker revised his former view to bring qualities into account and express that properties do something more than bestow causal powers. This is an essential requirement, I think, for any account of properties (especially mental properties) and we must take it seriously. Nevertheless, bringing qualities into account is not enough; we need a clear and coherent picture of them, especially their relations with causal powers. However, even if we understand Shoemaker's revision in this direction, he does not clarify how phenomenal features are related to causal powers and what their role in the nature of properties is.

To mention a problem with Shoemaker's view, let us suppose that properties have phenomenal characteristics in one way, and so are not identical with clusters of causal powers. Now the question is, why does 'instantiation' reduce a property to a cluster of causal powers? Shoemaker asserts that an instantiation of the realized property is a part of the instantiations of its realizers. Hence, a property's being instantiated is just a matter of its bestowing causal powers. However, what happened to its phenomenal aspects and why do we lose them in the process of instantiation? Why can't we say that when a property is instantiated, apart from bestowing powers, we have some instantiated phenomenal aspects as well? If so, because instantiation of a property is not identical with a cluster of causal powers, then instantiation of the realized property is no longer a part of instantiations of its realizers, which contradicts Shoemaker's claim.

To sum up, because Shoemaker does not explain his new account of properties explicitly, there are at least two ways to interpret it; both are problematic. According to the first interpretation (Heil's), there is no room for phenomenal aspects. The essence of a property is bestowing causal powers. However, properties are distinct from causal powers and bestow them via their instantiations. A problem with this interpretation is that we no longer need properties in our ontology. According to the second interpretation, phenomenal aspects are essential for (at least some) properties. Consequently, properties are not identical with causal powers because the former have something more. However, the relationship between these two features of properties (causal powers and qualities) is not clear. More importantly, it is not clear why a property reduces to a cluster of causal powers by being instantiated. In other words, why do we lose the qualitative aspect in the process of instantiation? These points suggest that the causal analysis of realization at most is incomplete. Although this analysis can solve many standard problems of realization and gives a clear picture of realization in terms of the subset relation, it still has gaps. It needs further clarifications about the nature of properties, phenomenal characteristics of properties, and their relation with causal powers.

5.4 The Flat Analysis of Properties

Now it is time to consider an alternative analysis of properties and the realization relation, which does not suffer from problems of the level picture and the causal analysis. Heil has mostly developed this analysis, and I will use his version here. The first principle for Heil (2003a, 13) is that 'Properties are to be distinguished from predicates.' This means that not every predicate (even if it applies truly to objects) designates a property. (Remember our previous examples of predicates that do not and cannot designate any property.) The second principle concerns the nature of properties. When we say two objects share a property, it means that they have something in common. If we assume properties as universals, this principle says that the property is present in both of the objects. Otherwise, if we (like Heil) assume properties as tropes (or modes), the second principle says that the two objects are *exactly similar* in some respect, in respect of that property. Heil (2003a, 13) puts this principle as follows, 'objects share a property only if those objects are precisely similar in some respect.' This principle entails that two properties are the same if they are identical (for the universalists), or exactly similar (for the trope theorists).

Now by applying these two principles we can determine whether a predicate designates a single property or not. If predicate '*P*' designates a single property *P*, then any object that satisfies the former possesses the latter. Hence, for the trope theorists any two objects that satisfy the predicate are exactly similar in some respect (in respect of property *P*). Heil (2003a, 13) expresses this point as follows, 'A predicate names or designates a property only if it applies to an object in virtue of that object's possessing a property possessed by every object to which it truly applies or would apply.' In other words, if two objects satisfy the same predicate, but they satisfy it in virtue of having two different (not exactly similar) properties, then the predicate does not designate a single property.

Now consider the predicate 'is red' as an example. If this predicate designates a single property, then every object that satisfies the predicate must possess this property. However, because of different shades of red we know that objects that we call red are not exactly similar in respect of their colour. They might have different colour tropes, which are similar but not exactly similar. This means that the predicate 'is red' does not designate a single property, and there is no property of being red in our ontology. The situation then is that: we have a linguistic entity (a predicate), which does not designate a property; rather it gathers and groups a set of similar (but not exactly similar) tropes under one name.

Heil (2003, Ch. 3) extends this point to multiply realizable *predicates* (say 'being in pain'). Here we have linguistic entities that gather a group of similar (but not exactly similar) tropes under one name. There are no corresponding properties to these predicates, because objects satisfy them do not possess the same property. Our ontology includes a flat level of basic properties. According to their similarities, these properties are gathered into different groups with different predicates as labels. However, there is no high-level property in the world.

There are two kinds of predicates. One kind designates single fundamental properties. If scientists discover that the most fundamental properties are, for example, quantum-mechanical properties, predicates that designate these properties belong to the first group. The second group of predicates is those that instead of single fundamental properties designate a group of similar (but not exactly similar) properties. Multiply realizable predicates belong to the second group. There is no single property corresponding to them. Therefore, multiple realization is not a special ontological relation between properties, rather this is a familiar concept that one predicate designates a group of distinct but similar properties.

By accepting this framework, one fair question is why are some predicates and concepts that gather particular similar properties together more salient to us than others? For example, why is the predicate 'is red' so salient and useful for us, while we do not gather similar properties of having the length between 5 and 5.1 centimeter under a predicate, say '*X*'? According to Heil, it is a matter of our perceptual system that some predicates are more salient for us.

> This is due, in some measure, to the fact that the properties in question are salient - to us - partly owing to the nature of our perceptual system. Were we built differently, were we made of different materials, the diverse collection of properties that satisfy our concept of redness could well fail to stand out. In that case we should have no use for the concept. (Heil 2003, 44)

The main point of this answer is that usefulness of a concept is not a sign that it picks up a property. As Heil (2003, 44) says,

> Concepts do not 'carve up' the world. The world already contains endless divisions, most of which we remain oblivious to or ignore. Some of these divisions, however, are salient, or come to be salient once we begin enquiring systematically. These are the divisions reflected in our concepts and in words we use to express those concepts.

Now let us see how Heil puts causal powers and qualities in his picture of properties. According to him, properties are simultaneously dispositional and qualitative. Regarding the former, Heil (2003, 76) thinks, '... intrinsic properties of concrete objects are distinguished by distinctive contributions they make to powers or dispositionalities of their possessors.' This means that two properties are identical just so that they make the same contribution to the causal powers of their possessors (Heil 2003, 77). For Heil (2003, 77), inert properties with no powers cannot be traced by us, and so we do not have any epistemic access to them, nor can they make any difference to the causal powers of their possessors. As a result, there is no place for them in our ontology. Therefore, any intrinsic property of concert objects (hereafter written as "property" for simplicity) is a dispositional property that bestow powers on its possessor, and 'it is solely by virtue of possessing a given dispositional property that an object possesses a given property' (Heil 2003, 79). This shows that Heil does not agree with those accounts of properties (e.g. Armstrong's) according to which objects' possession of causal powers depends on laws of nature that could vary independently of objects' intrinsic properties. He assumes that any property is dispositional, and a dispositional property bestows causal powers on its possessor directly and necessarily.

It should be pointed out that Heil's claim that every property is dispositional does not mean that properties are *purely* dispositional (i.e. does not mean that '*all* there is to a property is its contribution to the dispositionalities of its possessors' (Heil 2003, 97)). Heil (2003, Ch. 10) presents some arguments, which are out of the scope of this chapter, to show that this assumption is problematic. According to them,

> … we should want to distinguish empty space from a space occupied by material bodies. If we regard bodies as nothing more than relations or as nothing more than powers to affect other bodies, it is not clear that we have left ourselves with sufficient conceptual resources to make this distinction. (Heil 2003, 100)

To solve this problem he suggests the identity theory, according to which a property is simultaneously dispositional and qualitative, and by 'qualitative' he means intrinsic qualitative properties of objects, those properties that normally classified as 'categorical' (Heil 2003, 79).

> [The Identity Theory] If P is an intrinsic property of a concrete object, P is simultaneously dispositional and qualitative; P's dispositionality and qualitativity are not aspects or properties of P; P's dispositionality, P_d, *is* P's qualitativity, P_q, and each of these *is* P; $P_d = P_q = P$. (Heil 2003, 111)

This principle distinguishes Heil's proposal from those that hold dispositional and qualitative are *aspects* of properties, they are higher-order properties of properties, or that dispositionality is grounded in the non-dispositional. 'Functionalism' is an example of the last group, saying dispositionalities are realized by qualitative properties. Therefore, two objects might be dispositionally indiscernible but differ qualitatively. However, according to Heil (2003, 115), because of the identity theory,

> ...you could not vary an object's qualities without varying its dispositionalities; and you could not vary an object's dispositionalities without changing it qualitatively. In alerting a ball's shape, a quality, you alter its disposition to roll; in changing its colour, another quality, you change its disposition to reflect light in a particular way. Altering the ball's disposition to roll or to reflect light in a particular way involves changing the ball's qualitative make-up.

Let me leave analyzing Heil's proposal and his identity theory to the last section of this chapter and move on to looking at the advantages of Heil's account in the next section.

5.5 Fruits of the Flat Analysis

In this section, we will see how the considered proposal can solve the three problems mentioned earlier for the level picture of reality. After that, another consequence of this model concerning special-science laws will be discussed. Concerning the problem of causal powers, it is clear that there is no such problem with Heil's account. There is no high-level *property* with causal powers for us to worry about. There are high-level *predicates* and predicates are linguistics entities, to which we do not attribute causal powers. Therefore, by rejecting high-level properties, the question of the relationship between causal powers of high and low-level properties seems illegitimate.

Concerning the problem of causal relevance in Figure 1, Heil (2003, 45-6) believes that in one sense we can say that *P* causes *Q*. However, it should be noted that this truth does not hold in virtue of some high-level properties and relations between their causal powers. There is no high-level property in the picture. *R1* holds in virtue of the facts that the truth maker for '*P*' (a predicate) is *p* ('*P'* holds of a particular object at a particular time in virtue of that object possesses *p*), the truth maker for '*Q*' is *q*, and the truth maker for the causal relation between *P* and *Q* is the basic causal sequence between *p* and *q*. It means, 'higher-level causal claims are grounded in causal occurrences involving the truth-makers for higher-level predicates' (Heil 2003, 46). Therefore, by accepting the fact that the truth makers for high-level causal claims between predicates are low-level basic causal sequences between properties, there is no problem of overdetermination and causal relevance[43].

Finally, regarding the problem of inter-level relation, it should be clear that by rejecting high-level properties and introducing high-level predicates the relationship between high-level predicates and basic properties is not mysterious. We have groups of similar properties with a high-level predicate as their label.

Apart from these points, Heil's account has other advantages of which I consider two concerning projectibility of special-science predicates and *ceteris paribus* laws in special sciences. One question about Heil's proposal is that if a special-science predicate does not designate a property shared by objects that satisfy the predicate, how could this predicate figure in genuine special-science laws and in causal explanations? In other words, when we discussed the problem of causal relevance and Heil's answer to it, one might ask why we can have higher-level causal claims *at all*. Why are special-science predicates projectible and why do we have special-science laws?

The identity theory can help us to answer these questions. Properties are identical with their dispositionalities and with their powers. This means that when two properties are similar, in fact their dispositionalities are similar. In other words, under the same circumstances, two objects with two similar properties will manifest similar powers, and therefore behave similarly. Now remember that a special-science predicate designates a family of similar (but not exactly similar) properties. This means that these properties have similar causal powers and objects that possess them will behave similarly under the same circumstances. This is exactly what we mean by the projectibility of a predicate. According to Heil (2003a, 26),

> [Similarities between properties that designated by the predicate 'is in pain'] could be enough to warrant expectations that creatures in pain – creatures satisfy the pain predicate – will be disposed to similar behavior.

[43] Someone might say that if we consider the mental states like belief and desire as high-level predicates whose causal works are done by neurological states, then Heil's account entails epiphenomenalism. Heil (2003b, 45) in reply says,

> Your belief, desire, and intention could be epiphenomenal, however, only if they existed apart from your neurological condition, which they do not. This is not an identity theory. There is no prospect of reducing talk of beliefs, desires, or intentionality to neurological talk. Nor is 'type identity' in the cards. Nevertheless, the truth-makers for mental predicates ('is a belief', 'is a desire', 'is an intention', for instance) including a range of dispositionally similar neurological conditions. On this occasion, it is true in virtue of your being in state *P1* that you have these beliefs and desires, and it is true, by virtue of your being in *P2*, that you have this intention. It is true, as well, that your belief and desire caused you to form the intention, true in virtue of *P1*'s causing *P2*.

> The pain predicate is projectable, not because it designates a single property common to all creatures in pain, but because it is indifferently satisfied by distinct but similar properties bestowing the right sort of dispositionalities on their possessors. The 'causal relevance' of pain to creatures' behavior is accommodated by the uncontroversial causal relevance of the properties in virtue of which the pain predicate is satisfied.

Therefore, the identity theory permits projectible special-science predicates and special-science laws. This means that, contrary to some reductive accounts (e.g. Kim 1992) that do not accept special sciences as general sciences and claim that we cannot have any generalizations about high-level predicates, Heil accepts special sciences.

The same argument can be presented for *ceteris paribus* clauses in special-science laws, which unlike strict and exceptionless basic laws might have exceptions. This is a familiar point that some special-science laws are 'hedged' in the sense that although they are true for most of the objects that satisfy their predicates, however, there are some exceptions, and these counter examples must not be construed as falsifiers of the laws. Heil's account can easily cover these cases. Because a special-science predicate designates a family of *imperfectly similar* properties, this could be enough to expect that some of these *imperfectly* similar properties might bring about slightly different results under the same circumstances. These cases are exceptions of special-science laws and do not reject them (Heil (1999, 203-4) and (2003a, n. 16)).

5.6 The Possibility of the Unity of Science (II)

In Section 4.2.1, we saw how the causal analysis of realization is compatible with a version of the unity of science. In this section, I will show the compatibility of the unity of science and the flat analysis of properties.

As discussed, in the causal account of realization, the ontological building blocks are causal powers. The identity conditions of properties are defined in terms of identity between their causal powers, and the problem of multiple realization is solved by the subset relation between causal powers. Therefore, it is natural to analyze laws of nature, especially special-science laws, in terms of causal powers in this framework.

However, because in the flat framework the central notion is similarity we have a different situation. There are two kinds of predicate: fundamental predicates that designate single properties (any two objects that satisfy the predicate are exactly similar in respect of the property), and multiply realizable predicates that designate a family of similar (but not exactly similar) properties (any two objects that satisfy the predicate are similar in respect of those properties). On this basis, there are two kinds of laws according to the flat view. Firstly, fundamental laws express causal claims connecting two fundamental properties: any object possessing the first corresponding property (i.e. belongs to the exact resemblance class for that property), under the proper circumstances will possess the second corresponding property (belongs to the exact resemblance class for that property). Secondly, special-science laws express causal claims between two sets of similar properties: any object possesses a member of the first corresponding set of similar properties, under the proper circumstances will possess a member of the second corresponding set of similar properties. As we can see, the central notion here is similarity, and we classify laws on basis of the extent of the similarity (perfect or imperfect) between properties designated by predicates that are used in these laws.

The table below shows a special-science law and its corresponding fundamental realizer laws. '*P*' and '*Q*' are two multiply realizable predicates, which designate two sets of similar properties: $p1, p2\ldots$ and $q1, q2\ldots$ respectively.

The special-science law	'P' $\rightarrow$ 'Q' Law (0)
Fundamental realizer laws	$p1 \rightarrow q1$ Law (1) $p2 \rightarrow q2$ Law (2) ... ...

Table 1: a special-science law and its corresponding fundamental realizer laws

Now let us see what the content of Law (0) is. *Prima facie,* this law says that any object possessing one of the similar properties $\{p1, p2\ldots\}$, under the proper circumstances, will possess a specific property from the set of similar properties $\{q1, q2\ldots\}$, via one the fundamental laws $\{pi \rightarrow qi\}$. We can divide this *prima facie* content into three parts: **(a)** there is a similarity relation between *pi* realizing properties: $p1 \approx p2 \approx p3$ ..., **(b)** there is a set of fundamental laws: $\{pi \rightarrow qi\}$, and **(c)** there is a similarity relation between *qi* realizing properties: $q1 \approx q2 \approx q3$ ....

The first point about these three parts is that the last claim is expected from the first two, i.e. if *pi*s are similar, and if any *pi* under the same circumstances brings about a *qi*, then we would expect that *qi*s are similar as well. This is exactly the same point that Heil (2003a, 26) says in answering the question 'why are high-level predicates projectible?'

> [Similarities between properties designated by the predicate 'is in pain'] could be enough to warrant expectations that creatures in pain – creatures satisfy the pain predicate – will be disposed to similar behavior.

The reason of this claim is this; properties are identical with their dispositionalities. Therefore, similarity between properties means similarity between dispositionalities. When an object has a particular dispositionality (a particular set of causal powers), the object manifests particular behaviours under certain circumstances. Therefore, if two objects have similar dispositionalities, they manifest similar behaviours under the same circumstances. If so, clause **(c)**, which expresses similarity among manifestations of a set of similar properties under the same circumstances, is predictable from the conjunction of clauses **(a)** and **(b)**, and the content of Law (0) is reducible to contents of **(a)** and **(b)**.

However, consideration of the actual special-science laws casts a doubt on this *prima facie* interpretation of Law (0). Consider this law as an example of special-science law: 'If a creature suffers from pain (i.e. satisfies the predicate 'being in pain'), and if the creature for some reason does not intend to suffer from pain, then the creature acts to get rid of the source of the pain.' According to the previous interpretation, this law says two things about the world: firstly, it expresses a similarity relation among members of a set of (probably endless) properties that are designated by the predicate 'being in pain', and secondly, it expresses a set of (probably endless) fundamental laws connecting each of these properties to a corresponding fundamental property.

This interpretation has a problem. When we know the mentioned special-science law we do not know all *actual* similar realizers of 'being in pain'. We only know realizers of 'being in pain' in some familiar creatures. For example, we know that 'being in pain' is realized in human beings by the brain state (property) *p1*, and in octopuses by *p2*, and so on. More importantly, even if we claim that we know all actual realizers of 'being in pain', we cannot claim that we know all *possible* (probably endless) realizers of 'being in pain'. By knowing the mentioned law, we cannot claim that we know a possible realizer of 'being in pain' in a different possible world and in a radically different creature. In other words, clause **(a)** is much richer than the special-science law and by knowing the latter we cannot claim that we know the former.

A suggestion to solve this problem might be that we have to limit clause **(a)** to only those actual realizers of 'being in pain' that we know of. Therefore, the law does not enumerate the entire *pi*s, and the entire set of fundamental laws $\{pi \rightarrow qi\}$, but instead expresses a similarity relation between few actual samples of *pi*s and their corresponding fundamental laws. For example, clause **(a)** is something like this: there is a similarity relation between properties $p1 \ldots pn$. However, this suggestion ignores the *projectibility* of the special-science law. We want to have a law which, in addition to the actual and familiar examples of pain in familiar creatures, says something about any possible instance of pain.

Therefore, we need an account of the content of special-science laws that saves their projectibility, but does not mention all possible realizers of their predicates. My suggestion is this: suppose that $p1$ a is realizer of 'being in pain' in a familiar creature (no matter which one, but for the moment suppose human being), and $q1$ is a realizer of 'avoidance behavior' in the same creature. The content of Law (0) can be expressed in two parts. **(A)** A fundamental law expressing that there is a nomological relation between $p1$ and $q1$, (Law (1): $p1 \rightarrow q1$), and **(B)** Under the same circumstances, any property similar to $p1$ (say pi), brings about a property (say qi) similar to what $p1$ brings about ($q1$).

The first clause shows that Law (0) is based on pain experience in some familiar and well-known creatures (like human beings), and the second clause guarantees that Law (0) is projectible and so is applicable to other creatures who experience pain. Clause **(A)** is a fundamental law, a nomological relation between two realizers of the high-level predicates, no matter which one (Law (1) or Law (2) or …). Clause **(B)** is a general principle that is common to all special-science laws, saying that under the same circumstances similar properties bring about similar results. Because this general principle appears in all special-science laws, let me call it 'the similarity principle' and consider it in more detail.

The similarity principle is exactly the same principle that we appealed to in order to claim that the third part of the first interpretation of content of Law (0) is expectable from the first two parts (i.e. **(c)** is predictable from **(a)** and **(b)**). At that stage, I argued that because *pi*s are similar and each of them brings about another property

(q_is) by a fundamental law, q_is should be similar as well. As mentioned, Heil accepts this principle and his explanation for the projectibility of special-science predicates is based on this principle. Therefore, it is not surprising that in the second interpretation of the content of Law (0), I only added the clause **(B)** to save projectibility of the special-science law. In the flat framework of properties, if someone wants to defend special *sciences* with *projectible* predicates, she needs a principle like the similarity principle, saying that similarity among properties that is in fact similarity among their dispositionalities brings about similarity among manifestations of the properties under the same circumstances[44].

Now let us consider the epistemic situation of the similarity principle. It seems to me that this principle is a conceptual truth about the similarity relation. Its justification is not because this principle has been examined many times in many different situations and has enough supportive evidence. In other words, we do not accept this principle as an empirical generalization obtained by *induction*. The reason is clear; induction itself is an application of this principle. We argue, for example, that similar samples of water will behave similarly under the same circumstances; all of them boiling at the same temperature. In other words, in inductive reasoning, by appealing to the similarity principle, we argue that because similar properties under the same circumstances will bring about similar results, and because we have enough evidence that a particular property under particular circumstances brings about a particular result, therefore any other similar property will bring about the similar result.

The reason for accepting the similarity principle, rather than induction, is that it is a conceptual truth, and its truth stems from the nature of the similarity relation. It is built in the similarity relation such that similarity *per se* means that under the same circumstances two similar things behave similarly. Therefore, although the similarity principle has some empirical content and is not tautology, it is not an empirical claim. It is a conceptual truth about the nature of similarity.

Now let us see where all of this leaves us. We analyzed the content of a special-science law into two parts: one is a fundamental law belonging to the basic level (say ultimate physics); another one is a conceptual truth about the nature of similarity, which is common in all special-science laws. By keeping in mind that for our present

[44] The clause 'under the *same* circumstances' is absolutely vital for the similarity principle. We are *not* talking about manifestation of one property (or two similar properties) under the *similar* circumstances. Chaotic systems show that one property (or two similar properties) in two similar but slightly different circumstances (different initial conditions) may bring about radically different results. The similarity principle does not guarantee that similarity among circumstances brings about similarity among manifestations.

purpose it is not important which one of the fundamental laws (Law (1) or Law (2) or …) or which combination of them is placed in the first part of the analysis, we reach this conclusion. There is a unity between special-science laws and fundamental laws. This is the unity of content: the content of a special-science law (i.e. what it claims about the nomological connections in the world) can be analyzed in terms of content of some fundamental laws plus a conceptual truth about the nature of similarity. This means that although a special-science law expresses this content in a unique and different way, what it says (its content) is nothing more than what basic sciences (for example ultimate physics) say about the world. Regarding the content of laws, the special and basic sciences are unified.

This analysis shows that the flat framework of properties, like the causal framework, can bring a version of the unity of science. As mentioned, for some philosophers the idea of the unity of science is more important than particular inter-theoretic accounts of reduction. If so, even if these two metaphysical frameworks cannot solve problems of the classical account of reduction, they can save the deeper idea of the unity of science[45].

5.7 The Possibility of Reduction

This section is devoted to considering the effects of multiple realization on the classical account of reduction. Following our previous terminology, the second wave of objections to the classical model of reduction was based on the notion of multiple realization. These arguments were so powerful that many philosophers considered *the* main problem with the classical model to be multiple realization. In what follows, I will argue that by taking either the causal or the flat view of realization and by considering Nagel's account of reduction (not the accounts that others attributed him), we can save a version of the classical account.

Putnam was one of the pioneer philosophers who introduced the notion of multiple realization into the philosophy of mind to criticize a dominant theory in the 1960s, i.e. the brain-state theory. According to this theory, every mental kind is identical with a (perhaps undiscovered) neural kind. For example, being in pain as a mental kind is

[45] In Chapter 6, I will argue that the unity of special and basic sciences does not mean that by having the latter we do not need the former. In other words, although the content of special-science and basic laws are unified, this does not mean that the special sciences are unnecessary. I will argue that, contrary to the unity of content, special sciences could provide high-level explanations that are not obtainable by fundamental sciences.

identical with C-fiber firing as a neural kind, and every creature having pain has this neural state. Putnam (1967) argues that mental sates are multiply realizable, in the sense that a wide variety of creatures can have, for example, pain. Now if the brain-state theory is true then there must be some physical-chemical kind common to this wide variety of neural structures. According to Putnam, the current scientific evidence is against the hypothesis that *every* mental kind is identical with a neural kind. Although it is *not* impossible that such a hypothesis holds.

> Consider what the brain-state theorist has to do to make good his claim. He has to specify a physical-chemical state such that any organism (not just a mammal) is in pain if and only if (a) it possesses a brain of a suitable physical-chemical structure; and (b) its brain is in that physical-chemical state. This means that the physical-chemical state in question must be a possible state of a mammalian brain, a reptilian brain, a mollusc's brain (octupuses are mollusca, and certainly feel pain), etc. At the same time, it must *not* be a possible (physically possible) state of the brain of any physically possible creature that cannot feel pain. Even if such a state can be found, it most be nomologically certain that it will also be a state of the brain of any extra-terrestrial life that may be found that will be capable of feeling pain before we can even entertain the supposition that it may *be* pain.
>
> It is not altogether impossible that such a state will be found.... Thus it is at least possible that parallel evolution, all over the universe, might *always* lead to *one and the same* physical "correlate" of pain. But this is certainly an ambitious hypothesis. (Putnam 1967, 228)

After this passage, Putnam briefly mentioned a possibility for a brain-state theorist: a mental kind might be identified with the disjunction of its realizers. However, Putnam did not take this option very seriously and regarded it as an *ad hoc* assumption.

> Granted, in such a case the brain-state theorist can save himself by *ad hoc* assumptions (e.g., defining the disjunction of two states to be a single "physical-chemical" state), but this does not have to be taken seriously. (Putnam 1967, 228)

The possibility that Putnam dismissed might be interesting for some reductionists. If we could identify a multiply realizable kind belonging to a special science (say being in pain) to the disjunction of its realizers, then we can use this identity statement as a bridge principle in Nagel's account of reduction and reduce the special science to more basic theories (e.g. neurology or physics). In other words, accepting identity between a multiply realizable kind and the disjunction of its basic realizers means that all special sciences are *in principle* reducible to more basic sciences. Fodor discussed this possibility in detail and tried to show that this option faces serious difficulties.

5.7.1 Fodor's Argument

In his classic paper 'Special Sciences', Fodor distinguishes between physicalism (generality of physics) and reductivism (unity of science). Fodor (1974, 100) accepts token physicalism, according to which '... all the events that the sciences talk about are physical events'. Token physicalism is compatible with the generality of physics and its basic position among the sciences (Fodor 1974, 101); however, it is weaker than type physicalism and reductivism. According to type physicalism, '... every *property* mentioned in the laws of any science is a physical property' (Fodor 1974, 100). It is obvious that type physicalism concludes token physicalism; however, token physicalism does not conclude type physicalism.

What is reductivism? According to Fodor (1974, 100), '... reductivism is the conjunction of token physicalism with the assumption that there are natural kind predicates in an ideally completed physics which correspond to each natural kind predicate in any ideally completed special science'. It is obvious again that reductivism concludes token physicalism, as one of its components, but is not concluded by it. Reductivism is a false doctrine, according to Fodor, which cannot be concluded by any true thesis like token physicalism. To see why reductivism is false, we first need to clarify the notion of a natural-kind predicate. According to Fodor (1974, 102),

> $\boldsymbol{P}$ is a natural kind predicate relative to $\boldsymbol{S}$ [a scientific theory] iff $\boldsymbol{S}$ contains proper laws of the form $\boldsymbol{Px} \rightarrow \boldsymbol{\alpha x}$ or $\boldsymbol{\alpha x} \rightarrow \boldsymbol{Px}$; roughly, the natural kind predicates of a science are the ones whose terms are the bound variables in its proper laws.[46]

[46] Fodor accepts that the murky notion of natural kind is viciously dependent on the equally murky notions of laws and theory. However, he does not see any serious problem in this circularity and thinks that there are interesting things in the circle (Fodor 1974, 102).

Now we need to know why Fodor defined reductivism as a conjunction of token physicalism and the idea that every natural kind is, or is coextensive with, a physical natural kind. According to Fodor (1974, 102), reductivism has the premise '… that every predicate which appears as the antecedent or consequent of a law of the special sciences must appear as one of the reduced predicates in some bridge [laws]'. This shows that Fodor's definition of reduction has three elements: firstly, *every* predicate of the reduced science should appear in a bridge law; secondly, these bridge laws should have *biconditional* forms and connect a natural-kind predicate from the reduced science to a natural-kind predicate from the reducing one (Fodor 1974, 98); and finally, *any* special science is reducible to physics (at least in principle). Now by having this definition and the definition of natural-kind predicates (i.e. that they apply to the bound variables in proper laws), it easily follows that every natural-kind predicate of special sciences is coextensive with a physical natural kind. Therefore, Fodor defines reductivism as conjunction of token physicalism and this conclusion.

The rest of Fodor's paper is devoted to showing that reductivism, as defined above, is false. Accepting that there is very unlikely to be a type-type identity between the special-science and physical properties, Fodor focuses on coextensionality between natural-kind predicates of the special sciences and of physics. He has three objections to this thesis,

> … (a) to paraphrase a remark Donald Davidson made in a slightly different context, nothing but brute enumeration could convince us of this brute co-extensivity, and (b) there would seem to be no chance at all that the physical predicate employed in stating the coextensivity is a natural kind term, and (c) there is still less chance that the co-extension would be lawful (i.e., that it would hold not only for the nomological possible world that turned out to be real, but for any nomologically possible world at all). (Fodor 1974, 104)

Then Fodor moves to the issue of the multiple realizability of the special-science predicates and considers the possibility that we take a disjunction of the possible realizers of a special-science predicate as its coextensive predicate. Let us suppose that '*S*' is a special-science predicate designating a multiply realizable property, and *P1* to *Pn* are realizers of the property. In this case, we have the bridge statement below that looks like a coextensionality;

$Sx \leftrightarrow (P1x \text{ or } P2x \text{ or } \ldots \text{ or } Pnx)$

However, Fodor argues that on the right hand side of this statement there is not a natural-kind predicate, because we have not had any physical law with such a complex predicate (see the definition of a natural-kind predicate above). Therefore, the statement is not a law (one of its predicates is not a natural-kind predicate), and cannot be used as a bridge *law* in the classical model of reduction (Fodor 1974, 108). In summary, natural-kind predicates of the special sciences, because of multiple realizability, are not coextensive with physical natural kinds, and their coextensionality with disjunctions of physical natural predicates is not lawlike and cannot be used instead of bridge laws in reduction. Therefore, natural-kind predicates of the special sciences cannot be reduced, and consequently the special sciences are not reducible to physics.

5.7.2 A Reply within the Fodorian Framework: Local Reduction

There are at least two ways to criticize Fodor's argument. In the first, Fodor's assumptions and arguments are taken for granted, but it is shown that within the Fodorian framework we can still have reduction. In the second, however, Fodor's arguments and his presuppositions are criticized. In this section, I will mention an example of the first group, and in the next one, I will turn to the second group of arguments.

Lewis (1969) presented the earliest reply to Fodor. His main point is simple and straightforward. Take pain as a mental state. This mental state might be identical with a particular brain state in human beings and to some other brain states in other creatures. In other words, a multiply realizable state can have 'local' identities with particular realizers within particular species or kinds of system. On the other hand, the *concept* of this multiply realizable state can be the same in different species or systems. The *concept* of pain is the same in different organisms, but in different organisms, it is identical with different brain (or even nonbrain) states.

> A reasonable brain-state theorist would anticipate that pain might well be one brain state in the case of men, and some other brain (or nonbrain) state in the case of mollusks. It might even be one brain state in the case of Putnam, another in the case of Lewis. No mystery: that is just like saying that the winning number is 17 in the case of this week's lottery, 137 in the case of last week's. The seeming contradiction (one thing identical to two things) vanishes once we notice the tacit relativity to

> context in one term of the identities. Of course no one says that the *concept* of pain is different in the case of different organisms. (Lewis 1969, 233)

Although Lewis himself did not explicitly apply this solution to present an account of reduction, other philosophers developed 'local reduction' on the basis of his suggestion. Kim (1992, 1993b) argues that multiple realization rules out reduction of general (structure-independent) special sciences (e.g. general psychology), but it permits reduction of 'local' (structure-dependent) special sciences (e.g. reduction of *human* psychology). The reason is that within a particular structure-type, we can identify multiply realizable states with particular basic realizers, and these identity statements play the role of bridge principles and facilitate the classical account of reduction.

Many philosophers believe that local reductions are sufficient for any reasonable scientific or philosophical purpose. Kim, for example, thinks that local reduction is the rule rather than the exception in science. Churchland (1986, Ch. 7), Hooker (1981), and Enç (1983) are other philosophers who have presented examples of concrete local intertheoretic reductions where a given reduced concept is multiply realized at the reducing level. In these cases, by identifying the multiply realizable target with a particular physical property, scientists have achieved empirically important local reductions. A familiar example is the concept of temperature. Temperature in a gas is identical with mean molecular kinetic energy. Temperature in a solid, however, is identical with mean maximal molecular kinetic energy, since the molecules of a solid are bound in lattice structures and hence restricted to a range of vibratory motions. Temperature in a plasma is something else entirely. Even a vacuum can have a (blackbody) temperature, though it contains no molecular constituents. However, when we restrict our domain and consider the identity of temperature and mean molecular kinetic energy in a gas, we will obtain a local reduction: the laws of classical thermodynamics (e.g. the Boyle-Charles law) are reducible to statistical mechanics. Therefore, Fodor's argument cannot block the possibility of reduction[47].

[47] Some recent anti-reductionists (e.g., Endicott (1993)) presented a more sophisticated version of multiple realization. According to their account, which goes back to Block (1978), a given mental kind can be realized in a particular structure-type via distinct neural events at different times. As a result, we have to identify a mental kind to one of its realizers in a structure-type at a given time. Therefore, we cannot even reduce a local psychology restricted to a structure-type. In reply, Kim (1992) and Bickle (1998, Ch. 4) argued that a guiding methodological principle in contemporary neuroscience assumes some continuity of underlying neural mechanisms within and across species.

5.7.3 A Different Reply: Rejecting Fodor's Argument

Instead of trying to reconcile Fodor's argument with local reductions, some philosophers have taken a different approach: Fodor's argument is not accurate and cannot block the possibility of reduction[48]. In this section, I will develop my objection to Fodor, which is along the same lines. Fodor's definition of reduction is problematic. As mentioned, Fodor's definition has three elements; (i) *every* predicate of the reduced science should appear in a bridge law, (ii) these bridge laws should have *biconditional* forms and connect natural-kind predicates from the reduced science to natural-kind predicates from the reducing one, and (iii) *any* special science is reducible to physics (at least in principle). There are two possibilities here: either Fodor thought that this is Nagel's definition of reduction, or he suggested this as his own version. Both ways are blocked.

As we discussed in Chapter 1, Nagel does not require conditions (ii) and (iii). According to Nagel, a bridge principle can have a conditional (not necessarily biconditional) form (Nagel 1961, 354). Moreover, there is no necessity in his account where being a special science necessitates reducibility. Nagel only enumerates some (formal and non-formal) conditions, indicating which special sciences at their current stages are reducible and which are not. Therefore, if Fodor supposed that his definition was Nagel's definition, he misunderstood Nagel and what he rejected was a stronger version of reduction[49].

It might be said that Fodor had his own justification for defining reduction so that it requires biconditional connections between natural-kind predicates from the reduced science and natural-kind predicates from the reducing one (i.e. coextensionality of the special sciences and physical natural-kind predicates). As we discussed in Chapter 3, some philosophers argued that because of ontological and explanatory requirements, bridge principles must be identity statements between *properties*. There is no evidence in Fodor's paper to support that he had such a motivation. However, even if we accept that Fodor was concerned about ontological simplicity and therefore suggested coextensionality between natural-kind predicates, our

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[48] See for example Richardson (1979). For different objections to Fodor, see Enç (1983), Wilson (1985), Keeley (2000), and Clapp (2001).

[49] Fodor (1974, 114, n. 2) accepts that his version of reductivism is stronger than what many philosophers of science presented. However, he thinks that many of the liberalized versions of the unity of science and reductivism still suffer from the same basic defects. I do not agree with him on this point.

metaphysical considerations about the nature of properties and the realization relation show that property identities are not required for reduction. As discussed in Section 4.2.2, according to the causal analysis there is a flat level of causal powers as building blocks and we do not have a layered picture of properties to be simplified by reduction.

The same result is obtained from the flat analysis. According to this account, we have a flat and simple ontology of properties. There is no level picture of reality, and therefore there are no layers of properties. Properties may be gathered according to their similarities under different predicates, but there is no corresponding higher-level property. Our property ontology is simple *per se,* and there is no task of simplification of ontology for any account of reduction. This means that, if Fodor's reason to insist on biconditional bridge principles is simplifying ontology, we no longer need such a restriction. The bridge principles need not be biconditional; they must only facilitate deduction of the reduced-theory laws from the reducing-theory.

5.7.4 Disjunctive Predicates and Projectibility

In the last section, I argued that bridge principles can be conditionals and we do not need coextensionality. Therefore, if 'M' is a natural-kind predicate of a special-science designating a multiply realizable property and '$p1$' is a natural-kind predicate designating a realizer of M in a particular system, the bridge principle ($p1 \rightarrow M$) is acceptable, as long as it facilitates deducing the special-science laws from more basic laws. Now, in this section, I would like to consider the situation of bridge principles like (($p1$ or $p2$ or ...) $\leftrightarrow$ M)) that connect 'M' to the disjunction of all predicates designating M's realizers. As mentioned, Fodor in his classic paper (1974) believes that a bridge principle must be a *law*: we need *bridge laws* in reduction. The mentioned bridge principle, according to Fodor, is not a law, because on its left hand side (involving a disjunctive predicate) there is no natural-kind predicate. The reason is that this disjunctive predicate had not appeared in any proper physical law before it appeared in the bridge principle (i.e. it did not have an independent certificate as a natural-kind predicate). Therefore, the disjunctive predicate is not a natural-kind predicate, the mentioned principle is not a *law*, and in the absence of *bridge laws*, there is no reduction.

Kim does not find Fodor's argument persuasive. He (1992, 318) believes that Fodor did not explain why bridge principles must be laws. We can imagine bridge principles that do not necessarily connect kinds to kinds, they connect a kind to a disjunction of kind-predicates and yet with nomic necessity. (For example, the following bridge principle has nomic necessity, but only the first clause is a law and the whole sentence does not express a law. 'All metals either have the property of expanding when heated or are made of green cheese.' (Block 1997, 110)) Kim himself does not accept these as bridge principles, but the point is that he thinks Fodor did not present an independent argument to reject them. According to Kim, we need an independent reason to show that the predicates on both sides of a bridge principle must be suited for laws (kind-predicate).

As discussed in Section 4.2.2, Kim's argument is this. Assume that M is a multiply realizable property that is identified with Q in a biconditional bridge principle: $M \leftrightarrow Q$. If Q is a non-kind predicate, then M could no longer figure in special-science laws (Kim 1993, 318). The reason is simple: given the bridge principle, we can substitute M by Q in all special-science laws. However, if Q is a non-kind and cannot figure in laws, the results of substitution are no longer laws. Therefore, the original statements are no longer laws and M could not figure in any law. But we want to save special-science laws, so Q must be a kind predicate.

The next step for Kim is to show that the disjunction of predicates designating realizers of a multiply realizable property is not suited for laws. As discussed in Section 4.2.2, Kim has two epistemic and metaphysical arguments for this: the disjunctive predicate is not projectible and the disjunctive property does not satisfy the principle of causal individuation of kinds. In section 4.2.2, I argued that both of these objections are not valid in the causal framework of realization. Here I only show that they are not valid in the flat analysis of properties as well. According to this analysis, when two objects satisfy the disjunctive predicate either they have exactly similar properties or they have imperfectly similar properties. These similarities guarantee that we can expect similar behaviours under the same circumstances. Therefore, the disjunction of predicates designating a set of similar properties is projectible in the sense that by knowing the behaviours of an object satisfying one of them we can predict what behaviours another object satisfying another of them would have. Moreover, objects satisfying the disjunctive predicate via similar properties have similar causal powers. Therefore, according to the principle of causal individuation of kinds (see Section 4.2.2); the disjunctive predicate is a kind predicate. As a result, Kim's independent reason to insist on kinds-to-kinds bridge principles does not work, and there is no reason to prohibit us from considering a biconditional connection like (*p1* or *p2* or ... ↔ *M*), which has nomic necessity, as a bridge principle[50].

Sober has developed the same point. Consider the following derivation of a higher-level law:

P iff *A1* or *A2* or … or *An*.

Q iff *B1* or *B2* or … or *Bn*.

If *Ai* then *Bi* (for each *i* = *1*,*2*,…,n).

If *A1* or *A2* or … or *An*, then *B1* or *B2* or … or *Bn*.

If *P* then *Q*.

[*P* and *Q* are multiply realizable properties with *A*is and *B*is realizers]

According to Sober (1999, 552-3),

> Even if laws cannot be disjunctive, why does the above derivation fail to explain why "if *P* then *Q*" is a law? After all, the conclusion will be nomologically necessary if the premises are, and Fodor does not deny that the premises are necessary. Are we really prepared to say that the truth and lawfulness of the higher-level generalization is *inexplicable*, just because the above derivation is peppered with the word "or"?

After this, Sober raises the interesting question 'why must laws be nondisjunctive?' He (1999, 553) mentions laws with disjunctive predicates, e.g. 'water at a certain pressure will boil if the ambient temperature exceeds 100° C. This law seems to be disjunctive – it says that water will boil at 101° C, at 102 ° C, and so on.' One might say, however, that in the law about water different disjuncts bring about boiling water by the same type of physical process, whereas each realizer of a high-level

[50] Fodor (1997) replied to Kim and argued that Kim's independent reason for the unsuitability of disjunctive predicates for laws is not correct. Fodor argues that being jade (a disjunctive property) and being in pain (a multiply realizable property) are not similar, and the inference from the unprojectibility of jade to the unprojectibility of pain is not warranted. Then, he argues that the reason for the unprojectibility of the open disjunction corresponding to pain is that the property that is designated by the disjunction is intrinsically unfit for projection. Block (1997, 116) pointed out that this solution strengthens Kim's argument, 'since that open disjunction is nomologically equivalent to pain, pain is just as non-projectible and non-kind-like as that open disjunction.' Block (1997, 120) suggests the Disney Principle saying 'that laws of nature impose constraints on ways of making something that satisfies a certain description.' According to this principle, realizers of a complex property like thinking are not radically heterogeneous; they are to some extent similar and homogeneous, and therefore we can have deep scientific generalizations about them. Block's suggestion is very similar to the causal and flat analyses of realization, according to which realizers of a multiply realizable property have something in common or are similar, and therefore we can have projectible generalizations using their disjunction.

kind brings it about by a different physical process. According to Sober (1999, 553), 'the point is correct, but it remains unclear why this shows that laws cannot be disjunctive.' However, our previous metaphysical considerations show that the point is not even correct. According to the causal analysis of realization, all realizers of a multiply realizable property bring it about by having the same causal powers. And according to the flat view, they bring it about by having similar causal powers. Therefore, there are not radically different physical processes here, and the situation is similar to the law about water.

In short, there is no reason to restrict bridge principles to biconditionals. Even if we restrict bridge principles to biconditionals there is no reason to suppose that they must be *laws*. A statement with nomic necessity can play the role of bridge principles. There is no reason to show that disjunction of predicates designating realizers of a multiply realizable property are not suited for laws. In addition, there is no reason to suppose that laws cannot have disjunctive predicates. Therefore, any one-way conditional that connects a realizer to a realized property, or any biconditional that connects a realized property to disjunction of its realizers, can play the role of a bridge principle. Multiple realization is not an obstacle for the classical model of reduction.

5.8 A Comparison between Two Analyses of Properties

In this final section, I will compare the causal and flat analyses. As we saw, both of them give clear accounts of the possibility of multiple realization, and the standard problems with the level picture of reality do not apply to them. In addition, both have important effects on the reduction debate: the unity of science is still defendable; there is no need for property identity statements or biconditional bridge principles in the classical account of reduction, and the classical account of reduction for special sciences still works.

Apart from these common points, there are some differences between these two frameworks. As discussed in Section 5.3, the causal analysis needs more clarification about the nature of properties, their phenomenal features, and the relation between phenomenal features of properties (if any) and their causal powers. The later Shoemaker does not present a clear and positive account of properties, especially the place of qualities (if any) in the picture. He only says that properties are distinct from their instances, and when a property is instantiated, we only have a set of causal powers. As discussed, if we accept Heil's interpretation of Shoemaker that properties act via their instantiations, then we face the problem that we no longer need properties. Alternatively, if we take the interpretation that properties do something more than merely bestowing causal powers, and they have qualitative characters as well, then we face the problem that Shoemaker does not present a positive account of qualities and their relations to dispositions. More importantly, he does not answer the question 'why does a property reduce to a set of causal powers and lose its qualitative features when it is instantiated?'

Contrary to this, Heil presents a clear and positive view on properties, and especially on the role of qualities. He has the identity theory: with this view, a property is simultaneously dispositional and qualitative. This theory not only recognizes qualities, but also suggests a relationship (identity) between qualities and causal powers. The identity theory is an alternative for those philosophers of mind who think that instead of focusing on the mere problem of mental causation, we should pay more attention to the phenomenal and qualitative characters of mental states, and their relations to the causal powers of brain states. Therefore, the flat analysis has the advantage over its causal rival that is clear about the nature of properties, and the place of qualities in the picture.

However, the central notion in Heil's account, i.e. (perfect and imperfect) similarity, needs more consideration, while the central notion in the causal analysis, i.e. the subset relation between sets of causal powers, is clear. To see this point, let me present Heil's (2003, 151-2) key points about the notion of similarity. Firstly, similarity (whether perfect or imperfect) between two properties is *intrinsic* to the properties. Secondly, similarity is *not reducible* to any other more basic notion like identity, and so is a *primitive* notion, not explicable further. Thirdly, similarity between properties is an *objective* notion, which is *mind independent*. Lastly, there is an important difference between similarity of properties and similarity of objects: objects are not similar *tout court*, but only in some particular respects. Two objects might be similar in a particular respect because of having similar properties, while they might be different in other respects. However, properties are similar *tout court*; two similar properties are similar *per se* and not in virtue of anything else. 'Objects are similar by virtue of possessing similar properties; properties, in contrast, are not similar in virtue of anything' (Heil 2003, 152). Fifth, as I mentioned earlier, some similarities between properties are more salient to us than others. This fact depends on our perceptual systems, but does not show that similarity between properties is mind dependent. And the last point that is vital for the following discussion is that similarity comes in degrees: p may be more similar to q than to r. According to Heil, this point does not affect the objectivity of the notion.

This formulation of the notion of similarity opens a new horizon in metaphysics of science. Let us start with the point that because similarity between properties comes in degrees, we have a spectrum corresponding to it. At one end, we have perfect similarity. This means that if we consider properties as universals then two properties are identical, and if we consider them as tropes (modes) then they have exact resemblance. If a predicate designates perfectly similar properties (or alternatively identical universals), then it is projectible. This is because when two objects satisfy the predicate, they possess two perfectly similar tropes (or share a universal); therefore, they manifest perfectly similar (identical) behaviours under the same circumstances. In the middle of the spectrum, we have imperfect similarity. In this case, a predicate designates a set of imperfectly similar properties. Special-science predicates belong to this type. As discussed, Heil believes that imperfect similarity can guarantee projectibility of the predicates. Getting closer to the other end of the spectrum, there are predicates designating radically different properties, whose similarity is very low (e.g. 'is a property'). These predicates obviously are not projectible. The reason is that there is nothing (or very little) in common between the properties to guarantee similarity between their manifestations under the same circumstances.

There is a connection between similarity among properties, projectibility of predicates and having exception in laws. To the extent that similarities among a set of properties is high, the designating predicate is projectible. And to the extent that a predicate is projectible, the laws that the predicate appear in have less exception (see the last paragraph in Section 5.5). Therefore, all of these notions are connected together and all come in degrees. This new picture in the metaphysics of science rejects the traditional dichotomies of similar/dissimilar, projectible/non-projectible and exception-less/with-exception. Predicates cannot be categorized into two separate groups of projectible and non-projectible. One the other hand, when two predicates are projectible it does not mean that they are projectible with the same degree. The mentioned schema allows us to have spectrums with different degrees of similarity, projectibility and having exception.

Despite this interesting picture of the three connected notions which can be obtained from Heil's analysis, there is a problem in the heart of this analysis that needs further work. Imperfect similarity, unlike perfect similarity, is not a transitive notion. This means that the degree of similarity between *a* and *b* might be *r*, and the degree of similarity between *b* and *c* might be *r* as well, but it does not follow that the degree of similarity between *a* and *c* is *r*. For example, the colour red is imperfectly similar to the colour orange (suppose that their degree of similarity is *r*), and the colour orange is imperfectly similar to the colour yellow (suppose that their degree of similarity is *r*); however, it is not the case that red and yellow are imperfectly similar with the same degree *r*. This characteristic of imperfect similarity shows that Heil's proposal needs further work to account mapping predicates into properties. Suppose that predicate '*P*' designates two similar properties *p1* and *p2* for which the degree of similarity is *r*. There is a set of properties similar to *p1* with degrees of similarity more than or equal to *r* (circle *c1* in Figure2). Similarly, there is a set of properties similar to *p2* with degrees of similarity more than or equal to *r* (circle *c2* in Figure2). There are properties, e.g. *p3*, which are similar to *p2* with degrees of similarity more than or equal to *r*, but their degrees of similarity with *p1* are less than *r*. Now when Heil claims that predicate '*P*' designates a set of similar properties, which set does he mean? Does this set include *p3*? Given that '*P*' designates *p1* and the minimum acceptable amount of similarity is *r*, then '*P*' does not designate *p3*. However, given that '*P*' designates *p2* and the minimum acceptable amount of similarity is *r*, then '*P*' designates *p3*. Therefore, because imperfect similarity is not transitive we need a more sophisticated relation to map predicates into properties. This relation must provide a criterion to decide whether a given property belongs to a set of similar properties or not. Finding this relation is not impossible, but Heil did not discuss it and so his account needs further work in this direction.

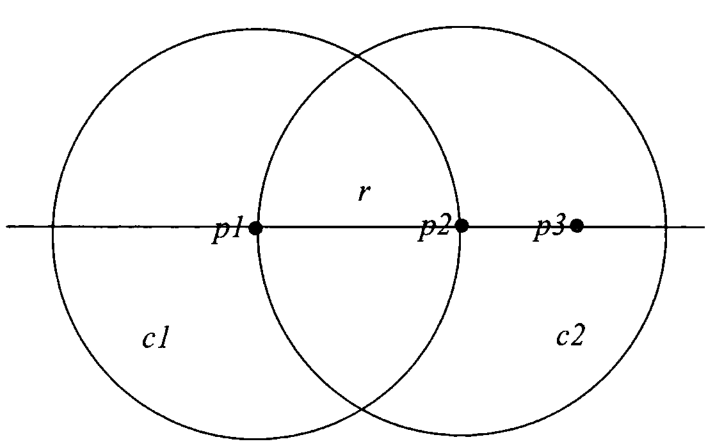

Figure 2: Imperfect similarity is not transitive

Contrary to Heil, Shoemaker's view is clear in this regard. For him, similarity is not the central notion; rather identity is at the heart. If two properties are realizers of a higher-level property, then some of their causal powers are identical. Shoemaker's account does not suffer from vagueness when it comes to the notion of similarity.

In brief, I would like to conclude that both of the causal and flat analyses of properties and realization need further clarification. It is true that both of them can solve the standard problems with the dominant picture theory of reality, both present a clear account of multiple realization, both save the idea of the unity of science, and both have the same consequences regarding the reduction debate. However, each of them has its own problems. The causal analysis needs further clarification of the nature of properties and their qualitative features. The flat analysis needs further clarification of the notion of similarity and its gradualness.

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As a conclusion, the following issues have been discussed in Chapter 5. An exposition from the flat analysis of properties and realization has been presented. It was shown that this analysis does not suffer from the standard problems with the level picture of reality, and can bring about a version of the unity of science. The second wave of objections to the classical model of reduction was discussed and it was argued that both the causal and flat frameworks save this model. Finally, it was argued that the causal analysis needs further clarification of the nature of properties, while the flat analysis needs more work on the notion of similarity.

Part Two

Negative Accounts of Reduction

6

Explanatory Reduction?

In the first part of this thesis, I defended a version of reduction, mainly based on Nagel's classic account. Moreover, I tried to bring metaphysical considerations on the scene, especially the issues concerning properties, and show that either by adopting a flat ontology (e.g. Heil's) or by a more classic approach based on causal powers (e.g. Shoemaker's), we can defend a version of the unity of science: unity of the content of special-science and basic laws. The second part of the thesis, containing three chapters, however, has a different aim. Instead of arguing positively for my approach to reduction, I would like to examine some alternative approaches and show that they are problematic. In other words, instead of saying what reduction is or should be, I would like to investigate what reduction is not and cannot be. This chapter is devoted to critical discussions on those accounts of reduction that define it in terms of explanation (explanatory reduction). In the next chapter, I will consider those accounts that define it in terms of supervenience. Finally, in Chapter 8 I will critically analyze functional accounts of reduction.

This chapter has two main aims. First, I shall try to show that those accounts of reduction that define it in terms of explanation are problematic. Traditionally the reduction of scientific theories has been defined in two ways. Firstly, a necessary condition for reduction of one theory to another is that the latter plus some bridge principles concludes every law of the former. Whereas, according to the second way, a necessary condition for reduction is that anything that the reduced theory can explain, must be explainable by the reducing theory as well. Such a conception of reduction can be found in early literature on the issue (e.g. Kemeny and Oppenheim (1956), Oppenheim and Putnam (1958)), as well as recent works (e.g. Kincaid (1987, 353) and Sober (1999)). It is interesting to point out that philosophers have suggested this concept of reduction almost for reducing macro to micro theories. Therefore, the first aim of this chapter is to show that by analyzing the nature of explanation, particularly its *contrastive* nature, defining microreduction in terms of explanation is problematic.

The second aim is to show that if we reduce a theory to another (according to any valid model of reduction), and if we truly believe that there is a unity between contents of special-science and basic laws, this does not mean that reduced theories and laws are dispensable. This would be a consequence of the first argument. If special-science laws can provide explanations that are not achievable by means of basic laws, then at the end we need to keep our special-science laws, even they are reducible. However, the case of thermodynamics shows that even if we ignore contrastive why-questions there is another reason for indispensability of some macro-theories. Before discussing these issues we need more clarification about the notion of explanation, and especially its contrastive nature. This is the topic of the next section.

6.1 Contrastive Explanation

If we accept the point that explanation is an epistemological activity, increasing our understanding of what-is-explained (the explanandum), a fair question is, what might this explanandum be? It is difficult to explain an *event*, say the last earthquake in Iran. Any event has many (infinite) aspects, and explaining each of them is different from others (cf. Hempel 1965, 421-3). For example, the earthquake happened last year and this aspect needs a separate explanation from others, say the earthquake's happening in the south of Iran. Therefore, it might be said that by different *descriptions* of the same event, we capture different aspects of it, and the object of an explanation is a description of an event that is normally a *sentence*. 'Why did the last earthquake in Iran happen last year?' or 'Why did the last earthquake in Iran happen in the south of the country?'

However, this is not true in all cases. In some explanations, we want to narrow our scope more, and, instead of one plain description of an event, ask "why-questions" about one particular element of that description. For example, we want to know why the last earthquake in Iran happened last year rather than five years ago. Here we have a space of two alternatives: last year and five years ago, our why-question concerns this contrast and its scope is more limited than the previous question. Generally, we call questions with the form 'why p rather than q?' contrastive questions. Contrastive explanations answer contrastive questions.

Another way to reach the contrastive explanations is taken by Lewis. According to his causal theory of explanation (1986, 185) '*to explain an event is to provide some information about its causal history.*' Roughly speaking, for Lewis (1986, 184-5) any event on which our target event has causal dependency is a part of its causal history.

And the criterion of causal dependency is counterfactual dependency. Therefore, explaining an event means providing some information about events that on which the target event counterfactually depends. However, it is obvious that the causal history of any event is a too long, dense and branchy chain of events beginning with the big bang (cf. Mill 1911, 214-218). Therefore, it is not feasible to provide a maximal true answer to any why-question and, generally, it is not required. We have to select some relevant parts of a causal chain and present it as the explanation of the event. But what information is relevant and what is not? Apart from other possible answers, one way is to consider context of the why-question. In some why-questions, we have an explicit or implicit 'rather than...' clause: 'why p rather than q?' These clauses show that main concerns of the questioner are those parts of the causal history that differentiated p from q, and other parts are irrelevant to her question.

Contrastive explanation is an issue in the so-called *pragmatics of explanation*. In contrast with theories of explanation that may focus on *product* of explanation, metaphysics of explanation, nature of explanation and its relation to causation, laws of nature and similar issues, the pragmatics of explanation focuses on the *act* and the *process* of explanation. For philosophers who work on this topic (e.g. van Fraassen (1980), Garfinkel (1981), Lewis (1986), Lipton (1990)), explanation is an answer to a why-question and its relevancy depends on the presuppositions and the interests of the questioner. Generally, the context of a why-question and particularly its space of alternatives are crucial factors in evaluating the relevance and adequacy of an explanation. As an example, van Fraassen invites us to consider an apparently clear plain question 'Why did Adam eat the apple?' When we add various foils to this question, it becomes apparent that it can mean many different things. 'Why did Adam eat the apple rather than doing something else with it?' is a different question from 'why did Adam eat the apple rather than Eve?' There would be no common explanation to both of these contrastive why-questions. In each, by different foils, we have different contexts, and relevancy of any explanation must be considered relating to the contexts. In what follows I shall consider some points about the relationship between contrastive and non-contrastive questions and then review a proposed approach to explain contrastive why-questions.

First, it should be pointed out that existence of contrastive why-questions does not necessarily mean that all why-questions have a contrastive nature. In some apparently plain questions we have some implicit 'rather than...' clause, e.g. when you ask me why I was late for our appointment, you ask why I was late rather than on time. But this is not true for all cases. Some people might suggest that any plain question 'why *p*' can be construed as 'why *p* rather than not-*p*'. Whether this suggestion is true or not depends on the strategy that we adopt to answer contrastive why-questions. For example, according to the 'causal triangulation' method proposed by Lipton to answer contrastive why-questions (I shall explain it shortly), this suggestion does not work. Because according to Lipton (1990, 221), his strategy misbehaves for these 'global' contrasts. Therefore, we do not have any *a priori* reason that any plain why-question is convertible to a contrastive one. However, this does not undermine the important role that contrastive why-questions play in our ordinary and scientific investigations, whether all why-questions are contrastive or not. The point that we need and use contrastive explanations to answer contrastive questions is enough for our purpose in this chapter, which is comparing two theories regarding explanations that they provide for contrastive why-questions.

The second point is about the reduction of contrastive why-questions to non-contrastive, and therefore reduction of contrastive explanations to non-contrastive ones. Lipton (1990, 212-5) considered some proposed non-contrastive questions to reduce the contrastive question 'why *p* rather than *q*?' Here I do not repeat his arguments to show that proposed reductions are not valid. However, I will just mention his two main points, used repeatedly in all of the arguments. The first point (Lipton 1990, 211) is that explaining a contrast is sometimes easier than explaining the fact alone (cf. Garfinkel 1981, 30). Lipton's (1990, 212) reason is that 'to explain '*P* rather than *Q*' is to give a certain type of explanation of *P*, *given* '*P* or *Q*', and an explanation that succeeds with the presupposition will not generally succeed without it.' However, there is no reason to say that explaining a contrast is *always* easier than a fact alone. This is Lipton's (1990, 212) second point that in some cases explaining a contrast is harder than explaining the fact alone. According to him (1990, 212),

> One reason that explaining a contrast is sometimes harder than explaining the fact alone is that explaining a contrast requires giving causal information that distinguishes the fact from the foil, and information that we accept as an explanation of the fact alone may not do this.

These two points help Lipton to argue that we may not be able to reduce a contrastive question to a non-contrastive question, because in some cases explaining the contrast is harder and in some cases easier than the non-contrast.

Now let us briefly review a proposed approach to answer contrastive why-questions. We mainly focus on Lipton's (1990) proposal, which was presented to solve problems of Lewis's (1986) account, and will not consider other proposals (e.g. Sober (1986), Hitchcock (1996 and 1999)). Lewis (1986, 196) suggests that to explain a contrastive why-question,

> ... what is wanted is information about the causal history of the explanandum event, not including information that would also have applied to the causal histories of alternative events, of the sorts indicated, if one of them had taken place instead. In other words, information is requested about the difference between the actualized causal history of the explanandum and unactualized causal histories of its unactualized alternatives.

As an example, if I ask 'why did Lewis go to A's house rather than B's house', one possible explanation would be that A invited him, whereas B did not. This is a relevant explanation because A's invitation is a part of the actualized causal history (his visiting A), whereas this is not a part of the unactualized causal history, i.e. if he had gone to visit B, A's invitation would not have been a part of that causal history.

Lipton (1990, 215-7) criticized this suggestion. One of his reasons is that Lewis's account allows non-explanatory causes. Suppose that both A and B had invited Lewis, but he went to A's anyway. According to Lewis's account, we can still explain this by pointing out that A invited him, since that invitation still would not have been the cause of a trip to B's. Yet if B invited him as well, the fact that he received an invitation from A clearly does not explain why he went there rather than to visit B.

To remove such objections Lipton (1990, 217) proposed his own solution, the difference condition. '*To explain why P rather than Q, we must cite a causal difference between P and not-Q, consisting of a cause of P and the absence of a corresponding event in the history of not-Q*'. As this definition shows, Lipton requires an actual difference between *P* and not-*Q*, in contrast with Lewis who requires a counterfactual difference. In our previous example, Lewis's invitation to A's does not explain why he went there rather than to B's, while there is an invitation in the history of his visit A, there is also an invitation in the history that leads him not to go to visit B.

One important question in contrastive explanation is under which circumstances can a contrast make a sensible contrastive why-question, and is giving it a contrastive explanation required? In other words, what are the requirements for a contrast to make a sensible contrastive why-question? For example, it does not make sense to ask 'why did Lewis go to A rather than I get the job?' According to Lipton, the difference condition imposes some restrictions on contrasts, and only allows some of them to make sensible contrastive why-questions.

> ... [T]he central requirement for a sensible contrastive question is that the fact and the foil have a largely similar history, against which the difference stands out. When the histories are disparate, we do not know where to begin to answer the question. (Lipton 1990, 218-9)

In other words, to the extent that *p* and not-*q* have more similar causal histories, giving an explanation to the question 'why *p* rather than *q*?' is easier. In some cases, if their causal histories are radically separate it is not even possible to provide any answer to the contrastive question. I will use this point in my next arguments concerning the relationships between reduction and explanation.

6.2 Reduction and the Pragmatics of Explanation

In what follows, I shall examine the adequacy of those accounts of reduction that define it in terms of explanation: to reduce *T2* to *T1* it is necessary that the latter can provide an explanation for anything that the former can. In other words, for any why-question that *T2* can answer, *T1* must answer it too. Keeping in mind our previous considerations about the pragmatics of explanation, particularly the point that the relevance of an explanation depends on its context, I would like to highlight one feature of the above definition of reduction. To reduce *T2* to *T1* it is essential that *T1* explains *exactly the same things* that *T2* explains, i.e. answers *exactly the same questions* that *T2* answers. In other words, we have to consider contexts of explanations that *T2* provides, and in each case check whether the counterpart explanation that *T1* provides is relevant in this context. To illuminate this point let us suppose that *T2* can answer the contrastive question 'why *p* rather than *q*?' In this case, a necessary condition to reduce *T2* to *T1* is that the latter answers exactly the same question. If *T1* can only answer the question 'why *p*?', or any other questions rather than 'why *p* rather than *q*?', then the main requirement of reduction is not satisfied. In such case, *T2* provides an explanation that is not achievable by *T1*. Therefore, *T2* is not reducible.

Some philosophers who have defended explanatory reduction simply ignored the pragmatics of explanation. Sober (1999) is among them. He suggests that if *P* is a multiply realizable high-level property that can explain a singular occurrence, its realizer (say $p1$) can explain the same singular occurrence too. Then he (1999, 547) argues that the fact that the latter explanation tells us more than we want to hear does not mean that what is said fails to be an explanation. This is exactly where Sober forgets the pragmatics of explanation. Our concern is not that basic theories can provide some explanation for high-level events (questions). We want to know whether these explanations are relevant in the context of high-level why-questions. We want to know, in the same context and by the same contrastive classes that we have at the high-level, whether more basic explanations can answer the same questions. For example, if the high-level theory can answer the question 'why p rather than q?', while the basic theory cannot, (suppose it can only explain 'why p?'), then we do not have reduction, although each theories provides some explanation of p. Considering the pragmatics of explanation is necessary for any account of explanatory reduction.

6.3 Reduction and Explanation (I): Explaining a Micro-Event

Alan Garfinkel is one of the pioneering philosophers who have paid attention to the pragmatics of explanation. In the first chapter of his book, *Forms of Explanation: Rethinking the Questions in Social Theories* (1981), he considers this issue, and in particular the role of alternative spaces and contrasts in explanation. He concludes as follows,

> To summarize, explanations have presuppositions which, among other things, limit drastically the alternatives to the thing being explained. These presuppositions radically affect the success and failure of potential explanations and the interrelation of various explanations. Call this *explanatory relativity*.
>
> A perspicuous way to represent this phenomenon is the device of contrast spaces, or spaces of live alternatives. The structure of these spaces displays some of the presuppositions of a given explanation. (Garfinkel 1981, 48)

Using this conception of explanation, he devotes the second chapter of the book to reductionism. He tries to show that those accounts of (micro)-reduction that define it in terms of explanation are wrong. It is not surprising to see that he considers the pragmatics of explanation in his assessment of reduction.

> So in order to assess a claim of reduction, we need a notion of when two explanations are explaining the same thing... In particular we can say that if the reduction is to be successful, the two explanations must have the same *object*. This means that they must be about the same phenomena and also that they must construe the problematic in the same way. Not only must they be talking about the same thing ... but they must have contrast spaces that line up in the right way... Otherwise the reduction will fail. (Garfinkel 1981, 50-1)

He begins with a particular concrete example. Suppose that we have an ecological system composed of foxes and rabbits. There are periodic fluctuations in the population levels of the two species. The reason is simple; when foxes begin to eat rabbits, we finally face the situation where there are too few rabbits. This causes foxes to die off, because there are too few rabbits left to sustain them. This new situation causes rabbits to multiply. After a while, there is enough food for foxes and so they begin to multiply, and so forth.

In this system, we have two macro and micro-theories. At the macro-level, we have some differential equations that represent the above fluctuations in populations of the species. However, at the micro-level we have a theory that explains what happens to any particular fox or rabbit, in terms of its behaviour, neighbouring animals, the surrounding environment and so on. For example, this micro-theory can explain why the particular rabbit *r* passed closely to a tree behind which the fox *f* was lurking, why this fox was hungry and how he caught the rabbit and ate it.

Garfinkel presents a contrastive why-question concerning a micro-event. 'Why was this particular rabbit eaten rather than not eaten?' He believes that a macro-explanation, using high-level predicates and vocabularies, can answer this question nicely. According to him (1981, 54), 'The cause of the death of the rabbit was that the fox population was high.' The micro-theory can also provide an explanation for the death of the rabbit, 'Rabbit *r* was eaten because he passed through the capture space of fox *f*'. However, Garfinkel's main point (1981, 55-6) is that the object of the latter explanation is not 'the death of the rabbit', but rather 'the death of the rabbit at the hands of fox *f*, at place *p*, time *t*, and so on.' In other words, the macro-level explanation answers the contrastive question 'Why was this particular rabbit eaten rather than not eaten?' However, the micro-level explanation answers the contrastive questions like, 'Why was this particular rabbit eaten by fox *f* rather than fox *f**?', or 'Why was this particular rabbit eaten at time *t* rather than time *t**?' and so on.

Therefore, Garfinkel concludes that there are some particular contrastive questions concerning micro-events for which macro-theories can provide answers, but micro-theories cannot. It entails that defining (micro)-reduction on the base of explanation does not work.

However, this argument does not seem persuasive to me. Firstly, it relies on a particular example in a particular field of study, and does not give any formal account of the micro-event that we cannot explain. Moreover, we do not know whether this conclusion is applicable to any other macro/micro-theory, or is restricted to this particular case. Secondly, the micro-event about which Garfinkel asks a contrastive question has a probabilistic nature. The death of the rabbit *r* had some degree of probability less than one. This is obvious from both the explanations too; neither claims that the events and facts cited as explanans determined the death of the rabbit. It is possible that the level of foxes was high, or the rabbit passed through the field of the fox, but the rabbit survived. Now the point is that asking contrastive why-questions about probabilistic outcomes that have some probability of not occurring, and therefore providing indeterministic contrastive explanations for them, is a controversial issue in the philosophy of science. Some philosophers (e.g. Lewis (1986, 197)) believe that we cannot provide any causal contrastive explanation for a chancy outcome. Their reason is this: the actual causal history of the actual chance outcome does not differ at all from the unactualized causal history that the other outcome would have had if that outcome had happened. However, some philosophers (e.g. Hitchcock (1999)) have defended the possibility of such explanations. Garfinkel's argument against reduction is based on the possibility of such explanations, but he does not provide any account for this possibility. This means that his argument against reduction is at most incomplete and needs an account of indeterministic contrastive explanation.

Let us put aside these general remarks and ask a simpler question. Can the micro-theory in this particular example provide an adequate answer to the mentioned contrastive question? I think it can. As I said above, the outcome event (the death of the rabbit), had a probability of less than one. First, let us see how the macro-explanation answers this question. As far as I can see, the only information that this explanation provides is that because of the increase in the fox population, the probability of the death of the rabbit increased. It increased as far as the probability of death was higher than the probability of survival. In short, the macro-explanation says that, because the probability of the death was (so) higher than the probability of the survival, the rabbit was eaten.

Now my claim is that the micro-theory can provide exactly such an explanation. The micro-theory can say what the probability of the death of the rabbit *r* by a particular fox *f1* was. If the rabbit was lucky and escaped fox *f1*, then it probably went to the zone of another fox, *f2*. Similarly, the micro-theory tells us what the probability of the death of the rabbit by fox *f2* was, and so on for any particular fox. Now, by having a body of information about the probabilities of the death of the rabbit by different foxes, and by having the exact number of foxes and their distributions, the micro-theory can calculate the accurate probability of the death of the rabbit by *one* of the foxes. This probability has a direct proportion with the number of foxes such that by increasing the latter (under the normal circumstances, not for example in the limiting case that foxes saturate the whole area such that the probability of the death reaches its maximum value one) the former will increase.

On the other hand, the probability of the survival of the rabbit is the complement of the probability of his death.

$$\text{Pr (the survival of the rabbit)} = 1 - \text{Pr (the death of the rabbit)}$$

This shows that, by increasing the number of foxes, the probability of the survival of the rabbit decreases. Now adding these two parts, the micro-theory says that by increasing the number of foxes the probability of the death of the rabbit goes up and up in comparison with the probability of his survival. This is exactly what macro-explanation says about the death of the rabbit. Therefore, in this particular case, the mentioned contrastive why-question is not a good example to show that the macro-theory can provide explanations that the micro-theory cannot.

6.4 Reduction and Explanation (II): Explaining a Macro-Event

In this section, I will present my own suggestion for those contrastive why-questions that we can explain by means of macro-theories. However, explaining them by means of micro-theories is impossible or very unlikely. Suppose we have a complex system, composed of considerable number of interactive elements (e.g. a container of gas composed of billions of molecules interacting with each other). For this system, suppose that we have a macro-theory and a micro-theory. The former deals with the behaviour of the system as a whole, and the latter with the behaviour of the elements individually. (In our example, say thermodynamics and the kinetic theory of particles.) In addition, suppose that the macro-theory uses some multiply realizable predicates each of which has a *literally infinite* number of realizers at the micro-level. Predicates that refer to a particular number as a threshold and classify all numbers below (or above) that in one group are examples of such predicates. For example, the predicate 'having the temperature less than 100 ° C' is an example, because there are infinite real numbers less than 100. Let us suppose that *P* and *Q* are two multiply realizable macro-predicates with infinite micro-realizers. (In our example, say *P* is 'increasing the pressure of the gas', and *Q* is 'decreasing the pressure of the gas'.)

My claim is that contrastive why-questions at the macro-level with the logical form 'why *P* rather than *Q*?' that are explainable by the macro-theory, are not easily explainable by the micro-theory. To consider a concrete example, let us focus on this candidate, 'under the circumstances *C*, why did the pressure of the gas increase rather than decrease?' Here, according to the context of the question, and by keeping the pragmatics of explanation in mind, we are not interested in knowing the exact amount of change in the pressure. Our contrast class is {increasing pressure, decreasing pressure}. Before going to examine the situation of this question at the micro-level, it should be noticed that we are not dealing with an indeterministic event here. According to the macro-theory, it is necessary that under the circumstances *C*, the pressure of the gas will increase. Therefore, the problems with the indeterministic contrastive explanations are not relevant here.

What is the translated version of this question at the micro-level? Here we do not understand *P* and *Q;* therefore, we have to use their realizers, *p1*, *p2*... and *q1*, *q2* .... Instead of 'why *P* rather than *Q*?' we have to ask 'why *pi* (the actual realizer of *P*) rather than {*q1*, *q2*...}?' In our example, the predicate 'decreasing the pressure' is a disjunctive predicate consisting of infinite disjuncts like 'decreasing *r* percent in the pressure', which *r* is between 0 and 100. For any particular *r*, there are many microstate realizers. For example, there are many distributions of molecules that their corresponding pressure is equal to decreasing 2% in pressure.

How can we answer the corresponding contrastive question at the micro-level, i.e. 'why the actual realizer *pi* rather than {*q1*, *q2*...}'? This contrastive question is equal to the following matrix of contrastive questions.

why *pi* rather than *q1* + why *pi* rather than *q2*+ ...

There are two problems with this matrix of contrastive why-questions. First, it is literally infinite, because Q has literally infinite realizers and in some cases (like our example), each realizer of Q has many realizers for itself. Providing such amount of explanation is not feasible in any sense. In reply, one might say that this shows this macro-explanation is available *in principle* at the micro-level. I do not want to consider the clause 'in principle' here. Personally, I think that providing infinite micro-explanations for a simple macro-question is not what we intuitively have in mind, when speaking about reduction. But if someone finds it satisfactory, I have no objection.

The second problem with this matrix shows that the situation is not as easy as the reductionist might think. In the matrix, we put *pi* with any of the *qj*s and made a contrastive why-question. As discussed earlier, however, not every contrastive question has an answer. If I ask a non-sensible question 'why did Lewis go to A rather than I get the job?' there is no answer. As Lipton argues, to make a sensible contrastive question, the fact and the foil must have a largely similar causal history. In our matrix, we do not have any restriction to accept or reject questions. It is possible, and certainly the case, that some of the contrastive questions in the matrix cannot be answered at all. For example, there is nothing in common between causal histories of the actualized realizer (say increasing 2% in pressure via a particular distribution of atoms) and an irrelevant foil like 'a particular distribution of atoms corresponding to decreasing 78.45% in pressure'. This fact shows that we cannot provide answers to all elements of the matrix, and therefore macro-theories can explain some contrastive questions that micro-theories cannot. We cannot define reduction of a macro-theory to a micro-theory in terms of their explanatory equality.

Domotor (1982) suggested that to reduce a macro-theory dealing with a system's behaviours to a micro-theory dealing with behaviours of its elements, we need a third mediating macro-theory that is probabilistic rather than deterministic. For example, he considers the reduction of phenomenological thermodynamics to kinetic theory of particles by means of introducing a probabilistic theory (Domotor 1982, 8-13). Without considering the details of his argument, intuitively it seems promising to me that a third probabilistic macro-theory could solve the mentioned problem. For example, a third theory like statistical mechanics may provide answers to any contrastive question that thermodynamics answers and the kinetic theory of gases cannot. But as I discussed we need more work to reconcile contrastive explanation and probabilistic theories. However, my main point is that Domotor's suggestion is not a response to my argument. It might be shown that the reduction of a macro-theory to another probabilistic theory is possible. But it does not show that we can reduce a macro-theory dealing with systems as wholes to a micro-theory dealing with behaviour of *individual elements*. More importantly, even if we reduce the macro-theory to a third intermediate probabilistic theory, this does not conclude that the main macro-theory is dispensable. The case of thermodynamics and statistical mechanics is an interesting example, one which I shall consider in more detail in the last section of this chapter. I will show that even if we accept the reduction, we still need thermodynamics.

6.5 The Map of Partitions at the Underlying Space

As we saw, Garfinkel presents some contrastive why-questions about micro-events that the macro-theory could answer properly, and the micro-theory cannot. But this is not all he has against microreduction. He wants to make a stronger claim, not only that microreduction is impossible in practice, but also that it is impossible *in theory*. To see this point, let us consider his example (1981, 64). Suppose a car is stopped at a set of traffic lights, the lights change, and the car proceeds. Considering the underlying physics, we had a steady and stable distribution of mass and energy before the lights changed. This distribution was stable against many physical changes. For example, it was stable against changes in the temperature or the shade of the red light. However, by a very small change in the energy distribution (the lights changing from red to green), which was negligible from a physical point of view, we had an enormous effect; a large mass was set into motion. In other words, the underlying space is cut into partitions of irrelevant differences, whereas these partitions are separated by critical points. For example, when the light is red we have an irrelevant partition of all shades of red. Whatever shade of red the light had, there was no difference in the outcome of the system. However, the critical point is turning from red to green. This tiny change, which from a physical point of view is similar to change from one shade of red to another, makes a huge difference at the higher-level. Garfinkel (1981, 64) claims that 'what is necessary for a true explanation is an account of how the underlying space is partitioned into basins of irrelevant differences, separated by ridge lines of critical points'. In other words, to explain the phenomenon at the upper-level we need to know what changes are really relevant to the outcome, and what changes are not. Now Garfinkel claims that by means of micro-theories we cannot discover the sensitivity of the outcome to changes. From a physical point of view, the difference between two shades of red and the difference between a shade of red and a shade of green are similar. According to Garfinkel (1981, 64-5) the partitions at the underlying level do not arise from this level, they are imposed by the upper-level and the macro-theory which governs this level. In our example, physics does not explain why red and green lights produce such radically different outcomes; the system of driving and the rules that drivers are committed to them impose such partitions to the underlying level. Therefore, Garfinkel concludes that, because to explain such phenomena we need to know the sensitivity of the outcome to underlying changes, the micro-theories cannot explain the sensitivity, and the macro-theory imposes them on the underlying level. Hence, microreduction is impossible in principle.

There are two points about this particular example. First, our case here is not a *strictly physical causal* one. This means two things; (1) the light changing from red to green is not a sufficient cause for the driver to move on; it is at best a necessary condition; (2) what is needed to add to this necessary condition to bring about the result has a *normative* nature; according to the driving rules to which our driver is committed, drivers *must* (if the road ahead is clear) move on when the light changes from red to green. The second clause is important for my purpose. This shows that the changing light does not physically cause the movement of the car. Apart from physical causal connections, we have normative elements. If this claim is true, we can conclude that Garfinkel's example at most shows irreducibility of *normative high-level theories*, theories in them apart from physical causal connections some things are imposed by *rules* from the higher to the lower level. Among others, moral theories are examples of such normative cases. However, my concern here is not normative theories. I would like to study the reducibility of physical high-level theories in which only strictly physical causes act. In summary, Garfinkel's example at best is irrelevant to the issue of reducibility of physical theories, and he cannot extend his conclusion to them.

The second point about this example is that someone might extend it to physical theories as well. We know familiar examples of chaotic complex physical systems, in which a small change in the underlying distribution (particularly a small change in the initial conditions) might bring about huge differences at the higher-level. The chaos phenomenon and its relationships with philosophical concepts like prediction, explanation and determination are out of the scope of this chapter. However, I just mention two brief points in this regard. The first concerns the relationship between sensitive dependence on initial conditions and predictability. A chaotic system is predictable *in principle* in the sense that 'if we could exactly specify the initial conditions, all further states would follow from straightforward calculation' (Kellert 1993, xi). However, the problem is that our initial condition specification must be impossibly accurate. This means that to specify the initial conditions we need an infinitely large device for storing and manipulating data (at least for times far enough in the future). Now as Kellert (1993, xi) says we face the question that 'Is our inability to construct such a device really to be seen as a *practical* limitation?' Some philosophers believe that this is a 'practical' problem in nature, and so chaos does not present anything against 'in principle' predictability. Another group deems it as an 'in principle' inability, yet the third group believes that '... there is a place where the line between "in theory" and "in practice" blurs' (*ibid*). Therefore, the point that in some physical systems the outcome is strictly sensitive on the initial conditions at most can prove *unpredictability*. However, to claim irreducibility in the sense that we discuss in this chapter, we need to bridge unpredictability to the impossibility of explanation. The mere fact that we cannot predict something is not enough to conclude that we cannot explain it. It is a controversial claim, which needs lots of philosophizing.

The second point about chaos is its relationship with determinism. Unpredictability does not necessarily mean indeterminism. Some philosophers (e.g. Earman (1986), Hunt (1987), Stone (1989) and Smith (1998)) have tried to show that chaotic systems are deterministic, while unpredictable. On the other hand, some other philosophers (e.g. Kellert (1993, Ch. 3)) argued that if chaos theory is combined with quantum-mechanical considerations, it leads us to doubt the doctrine of determinism itself. Therefore, although Garfinkel may be right that in normative systems the high-level facts impose some partitions on the basic level, this claim is controversial in physics and needs further argument, among them the issues of completeness of physics and the very definition of determinism.

These considerations lead me to conclude that Garfinkel's second point against reduction is not persuasive; it can only work for non-physical and normative theories. In the next section, by focusing on thermodynamics, I shall argue that even if we reduce a macro-theory to another intermediate probabilistic theory, the macro-theory is not dispensable.

6.6 A Case Study of Thermodynamics: An Indispensable Macro-Theory

Thermodynamics and its reducibility to statistical mechanics is perhaps the most discussed case in the reduction literature. Most of the discussions were about the possibility of finding counterpart concepts at the micro-level for key concepts of thermodynamics, e.g. temperature, pressure, equilibrium and entropy. However, in this section I will consider a point that few philosophers have considered. We know that the thermodynamic picture of the world has some special and unique features that we cannot find easily in the other physical pictures, including statistical mechanics. As Sklar (1993, 367) says, at the heart of these features are 'the idea of equilibrium states as attractor states for systems and of the time-asymmetric nature of the approach to equilibrium.' Now the question is, if these features are unique and cannot be found easily in other physical theories, particularly in statistical mechanics, what do we need at the micro-level to do justice to them?

Forgetting technical issues, the dominant answer to this question is that at the basic level we need a basic posit about probability distributions over initial conditions of systems. This means that by having a *structural constraint* on the probabilities of the initial conditions, we can reduce thermodynamics and its unique features to statistical mechanics. As Sklar says,

> ... [T]he most crucial features of the thermodynamic aspects of the world, the features like approach to equilibrium and time-asymmetry that appears as a surprise from the purely mechanical viewpoint of the world, appear at the reducing level only because a basic posit [...] is introduced at the reducing level in order to carry out the reduction. This is the posit of uniform probability over not too small and not too complex regions of phase space for initial conditions of a system.
>
> [...] We can summarize the situation by saying that statistical mechanics successfully reduces thermodynamics by replacing the structural constraints on the world imposed by the latter theory by its fundamental autonomous law – the Second Law – with a structural constraint on probabilities of the initial conditions of systems characterized at the micro-level. (Sklar 1993, 368)

Now let us see what the nature of this structural constraint is. Firstly, it does not contradict our theory at the micro-level. On the one hand, we have a mechanical picture according to which initial conditions of the system are freely choosable. On the other hand, the structural constraint tells us to select one particular initial condition, among many other possible alternatives. Secondly, this structural constraint does not stem from our mechanical theory at the micro-level. It has a thermodynamic nature. From a mechanical point of view, one set of initial conditions does not have any preference over any other set. According to Sklar,

> The underlying compositional and dynamic theories leave open what the initial conditions of a system are to be at the micro-level. And these theories leave open the possibility of there being some interestingly characterizable distribution of those initial conditions in the probabilistic sense. (Sklar 1993, 372)

The only reason that we choose a set of initial conditions is its conforming to the thermodynamic constraint; other physical theories say nothing, neither suorting this choice nor against it. From an ontological point of view, selecting a set of particular initial conditions introduces nothing over and above the ontology of the reducing theory. However, this selection is something autonomous and not part of the reducing theory: only thermodynamic considerations can justify it.

Now what can be concluded from this kind of relation between a macro and micro-theory? In our previous discussion we saw that a macro-theory might be indispensable because it can provide answers to some contrastive why-questions that the corresponding micro-theory cannot. The case of thermodynamics shows that even if we ignore contrastive why-questions some macro-theories are indispensable for another reason. These theories impose structural constraints from the upper-level to the lower-level, so that without them, lower-level theories cannot reduce macro-theories. This is the reason why some macro-theories are indispensable, even if we reduce them to more basic theories, or even if we ignore contrastive why-questions.

Garfinkel (1981, 68-74) points out nearly the same fact. According to him, in some micro-theories, any combination of individual elements is not possible. In these systems, we have *structural presuppositions*, which impose restrictions on the individual elements when they combine to make a collection. These presuppositions come from upper-level theories and do not follow from the nature of individual elements. According to Garfinkel the real micro-level consists of a set of individuals together with a nontrivial *sociology*, and in such systems we do not have microreduction. The micro-theory needs extra elements from outside to explain macro-events.

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In this chapter, I tried to criticize explanatory accounts of reduction, according to which a necessary condition for reduction of *T2* to *T1* is that the latter answers any why-questions that is answered by the former. First, I introduced the notion of contrastive explanation and then argued that in the definition of reduction we have to consider the pragmatics of explanation. This means that two theories answer the same question if, and only if, they explain the same event with the same contrastive classes. By rejecting Garfinkel's suggestion, I suggested a kind of macro-event that the macro-theory can easily explain; however, its explicability according to the micro-theory is problematic. In the next section, I criticized Garfinkel's argument for those partitions at the micro-level that are imposed by the macro-theory. At least, I argued, extending this issue from normative theories to causal ones is not so easy. Instead of that, by focusing on thermodynamics, I showed that in some cases a macro-theory imposes structural constraints on a probabilistic micro theory, so that we need these constraints to reduce macro-theories. This structural constraint ensures that the macro-theory is not dispensable.

7

Reduction by Means of Supervenience?

In this chapter, I shall examine those accounts of reduction that try to define it in terms of the notion of supervenience. Traditionally many philosophers found the notion of supervenience attractive because they thought this notion had the potentiality to combine physicalism and non-reducibility. In other words, to formulate a non-reductive physicalism, allowing autonomy and non-reducibility of higher-level entities and theories (particularly mental entities) and yet allowing their dependency on the physical entities, the notion of supervenience was the most popular candidate. However, some philosophers have argued that strong versions of supervenience that we may use to formulate non-reductive physicalism conclude the possibility of epistemic reduction. If their argument is correct, then we cannot formulate non-reductive materialism by means of supervenience, but also those cases that we thought are non-reducible, are indeed reducible. As discussed in Section 5.2.3, supervenience does not bring about ontological reduction. This means that supervenience is only a modal relation, expressing a kind of covariance, and its modal force is not strong enough to imply ontological reduction. When *A*s supervene on *B*s, for example, this is compatible with all of the following alternatives: *A*s are *B*s, *A*s and *B*s are effects of a common cause, *B*s are parts of *A*s and so on. Now if the argument of philosophers who tried to obtain epistemic reduction from supervenience is correct, then although supervenience is not strong enough to imply ontological reduction, it can give us epistemic reduction.

In what follows I will try to challenge this claim and show that supervenience does not imply epistemic reducibility. I will start with a short exposition of the notion of supervenience, its different types, and those types that have been used in the reduction debate. In the next section, I will consider some accounts of reduction based on the notion of supervenience. After that, I will introduce and analyze some proposed objections to these accounts. I have sympathy with some of them, while others I reject, because of my previous discussions on the nature of properties and multiple realization. I will end this chapter by presenting my own argument showing that supervenience does not bring about reducibility.

7.1 Supervenience and Its Varieties

The core idea of supervenience is the idea of *dependent-variation*. This means that when *A*s supervene on *B*s, then variation in *A*-respects depends on variation in *B*-respects. In other words, we cannot have *A*-respect variation without having *B*-respect variation. Two things cannot be different regarding *A*-respects unless they have some difference regarding *B*-respects. As an example, if we take properties as the relata of the supervenience relation, and if we hold that mental properties supervene on physical properties, then there is no variation in the mental-respects without counterpart variations in the subvenient physical properties. If two persons differ in respect of their mental properties, there must be some difference in their physical states. Equally, if two persons are identical regarding their physical properties, they must be identical regarding their mental properties.

However, this general intuition behind the notion of supervenience could be formulated in different ways, with different modal strength, and according to the objects that we select for comparison regarding their *A* and *B*-respects. The first group of definitions is obtained when we compare *things* regarding *A* and *B*-respects[51]. Therefore, we select two *things* and then compare them regarding two sets of *properties*. However, in the second group of definitions we select two *possible worlds* and then compare them regarding two sets of properties.

Further division is possible in the first group of definitions. We can select two things from the same possible world, or from two distinct worlds. If we select from the same possible world, then we have '*the weak supervenience*': '*A*-respects weakly supervene on *B*-respects $=_{df}$ for any possible world *w*, *B*-twins in *w* are *A*-twins in *w*' (McLaughlin 1995, 24). Otherwise, if we select two things from two different possible worlds we have '*the strong supervenience*': '*A*-respects strongly supervene on *B*-respects $=_{df}$ for any possible worlds *w* and *w** and any individuals *x* and *y*, if *x* in *w* is a *B*-twin of *y* in *w**, then *x* in *w* is *A*-twin of *y* in *w**' (*ibid*). In the second group in which we compare the two possible worlds, we have '*the global supervenience*': '*A*-respects globally supervene on *B*-respects $=_{df}$ all worlds that are *B*-twins are *A*-twins' (McLaughlin 1995, 30).

[51] In the rest of this chapter I simply assume that *A* and *B* are two families of properties. Therefore, comparing two things regarding *A*-respects means their comparison regarding *A*-properties. If they exemplify the same properties from this set they are identical regarding *A*, otherwise they are different in this regard.

All of these three versions of supervenience share one feature: they are holistic in the sense that we do not have a one-to-one relation between properties. Suppose for example that mental properties strongly supervene on physical properties, all that this claim gives us is that if two persons from two different possible worlds have the same physical properties, then they must have the same mental states (both are happy, not in pain, thinking about a same thing and so on). However, this claim says nothing about which particular physical properties are responsible for bringing about a particular mental property, say being in pain. This holistic feature of supervenience makes it inappropriate for the purpose of reduction. What we normally expect in the traditional model of reduction are bridge principles, connecting *one* property (concept) from the reduced theory to *one* property (concept) from the reducing theory. Therefore, we need amendments in the above definitions to obtain one-to-one connections. Here I will only mention three counterpart definitions that change the holistic nature of supervenience to provide one-to-one relations, and will not discuss the ways in which we could obtain them from the original definitions. Two of these new definitions are used to define reduction. In the next section, I will explore the ways of obtaining these new definitions. When I consider the problem of obtaining reduction from supervenience, I discuss problems with these new definitions.

The counterpart one-to-one relation of weak supervenience is this.

> *A* weakly supervenes on *B* =$_{df}$ necessarily, if anything has some property *F* in *A*, then there is at least one property *G* in *B* such that that thing has *G*, and everything that has *G* has *F*. (McLaughlin 1995, 25)

Corresponding to strong supervenience, we have the following definition.

> (*) *A* strongly supervenes on *B* = $_{df}$ necessarily, if anything has some property *F* in *A*, then there is at least one property *G* in *B* such that that thing has G, and necessarily everything that has *G* has *F*. (*ibid*)

Finally, corresponding to the global supervenience this definition is proposed.

> (**) [*A* globally supervenes on *B* = $_{df}$] for each property *F* in *A*, if *F* is exemplified by some object *x*, then there is a condition *C* that is equivalent to a conjunction of all the exemplifications of properties in *B* such that necessarily if *C* obtains, and there is no exemplification of a property in *B* that is not entailed by *C*, then *x* has *F*. (Grimes 1995, 114)

7.2 Reduction by Means of Supervenience

Many philosophers have tried to show that some versions of supervenience imply reducibility. Amongst them, these names can be mentioned: Kim (1978, 1983, 1984, and 1990), Rosenberg (1985), Bacon (1986) and Grimes (1995). In this section, first I will explore Kim's account that claims strong supervenience implies reducibility. After that, Grimes's argument that global supervenience can do the same job will be considered.

As mentioned, we need one-to-one relations between properties (concepts) to proceed with reduction. Let us see how Kim makes the one-to-one version of the strong supervenience. Suppose that the set of properties A supervenes on the set of properties B. This means that if x and y (not necessarily belonging to the same possible world) are indiscernible regarding B-properties, then they are indiscernible regarding A-properties. Now we make the set of *B-maximal* properties. An object x either has a particular property in B (say P) or does not have it. If x does not have P, and if we suppose that the negation of a property is a property (a crucial assumption that I will discuss later), then we can say x has $\sim P$ (not-P). Now concerning the set B, any object x has a chain of properties. It has some of the original properties in B, and the negation of some of the original properties. If we conjoin all the members of this chain, we will obtain a member of the *B-maximal* set. This means that in any member of the *B-maximal* set, every B-property is presented either in its original form or in its negation. These properties conjoin together and make a member of the *B-maximal* set. In addition, the *B-maximal* set contains all possible ways that we can combine these properties. In brief, the *B-maximal* properties are the strongest consistent properties constructible in B. Any object necessarily has one of them, and cannot have more than one. To illuminate this set let us suppose that B has three members $\{P, Q, R\}$. The *B-maximal* set in this case has eight members: $\{(P \,\&\, Q \,\&\, R), (\sim P \,\&\, Q \,\&\, R), (P \,\&\, \sim Q \,\&\, R), (P \,\&\, Q \,\&\, \sim R), (\sim P \,\&\, \sim Q \,\&\, R), (\sim P \,\&\, Q \,\&\, \sim R), (P \,\&\, \sim Q \,\&\, \sim R), (\sim P \,\&\, \sim Q \,\&\, \sim R)\}$.

Another crucial assumption that Kim needs is that the set B must be closed under Boolean operations. This means that any construction of properties that connects members of B by Boolean operations is still a property, and belongs to B. Now it can be seen that if an object has a property (say F) from the supervenient set A, then there is at least one property in B (say G) that this object has. This subvenient property (G) is the same member of the *B-maximal* set that this object has. In addition, necessarily any other object that has G has F. The reason is simple: A supervenes on B, and therefore if two objects are indiscernible regarding B (having the same *B-maximal*) they must be indiscernible regarding A.

We still need one further step to obtain the one-to-one relation between properties. We would like to define supervenience such that it allows multiple realization. This means that, we would like to keep the possibility open that two objects might have different subvenient properties, while they are indiscernible regarding supervenient properties. In other words, two objects might have different *B-maximal* properties, while both have the same supervenient property. This possibility prevents us treating a supervenient property and its counterpart *B-maximal* property as identical or even coextensive. The standard solution to this problem is this: take *all possible B-maximal* properties that an object which has F might have one of them (say $G1$, $G2$...). Make a disjunction of these properties (say G, $G = \bigvee Gi$). Because B is closed under Boolean operations, G is a member of B. Now G and F are coextensive. Any object that has F has one of the disjuncts of G, and therefore has G. On the other hand, any object that has G also has F. The reason is this. When an object has G, it has one of its disjuncts, say $G1$. We have already seen that having $G1$ is sufficient for having F. Therefore, there is a one-to-one relation between properties in the case of strong supervenience. (i.e. we have obtained the (*) definition in the last section from the original definition of strong supervenience). The holistic nature of strong supervenience has been changed into a one-to-one relation between properties.

Now it is time to see how Kim obtains reducibility from this one-to-one relation. As we discussed the issue in previous chapters, some philosophers, including Nagel, do not restrict bridge principles to biconditionals. However, some philosophers, including Kim, believe that the only logical form that can play the role of bridge principles is the biconditional. As mentioned, Kim argues that bridge principles must be identity statements, and because coextensionality is a necessary condition of identity, therefore bridge principles must be biconditionals, expressing coextensionality. Accordingly, it is not surprising to see that Kim construes Nagel's model of reduction just as derivation of laws of the reduced theory from laws of the reducing theory, by means of bridge principles that meet the condition of 'strong connectability':

> Each primitive predicate P of the theory being reduced is connected with a coextensive predicate Q of the reducer in a biconditional law of the

form: "for all x, Px iff Qx"; and similarly for all relational predicates. (Kim 1990, 151)

If this condition is met, Kim says, we can rewrite laws of the reduced theory in terms of the vocabulary of the reducing one. To do this we only need to substitute the original terms with their coextensional counterparts from the reducing theories. Now derivational reduction is guaranteed. Either these new laws belong to the set of pre-existing laws of the reducing theory, or we simply add them as additional laws. In both cases, we can derive laws of the reduced theory from the laws of the reducing one, which means we reduce the latter to the former.

It can be seen, Kim says, that when we have strong supervenience between two sets of properties we have one-to-one connections between properties. This one-to-one connection is exactly what we need in the condition of strong connectability. Therefore, we can conclude that when set *A* of properties supervenes on set *B*, any theory, *T2*, that expresses nomic relations among elements of *A* is reducible to any theory, *T1*, that expresses nomic relations among elements of *B*. In other words, strong supervenience implies epistemic reducibility.

Grimes (1995) presented a slightly different argument to show that strong supervenience implies reducibility. Referring to Nagel's original account, he realized that Nagel did not restrict the bridge principles to biconditionals. According to Nagel, one-way conditionals might play the role of bridge principles. Therefore, Grimes rejects 'the condition of strong connectability', and believes that any one-way conditional, according to which having a property from the reducing theory is sufficient for having a property from the reduced theory, can play the role of bridge principles. Now it can be seen that by having strong supervenience we have such one-way conditionals. As discussed, any *B-maximal* property (say *G1*) is sufficient for having a supervenient property (say *F*). These one-way relations can be used as bridge principles, and therefore we can obtain reduction from strong supervenience. This approach, according to Grimes (1995, 113), has the virtue of not being sensitive to the issue of disjunctive properties. Even if someone rejects disjunctive properties and says that the subvenient set of properties is not closed under disjunction, she can confirm the achievability of reduction from strong supervenience. In Grimes' account, we do not need coextensionality, and therefore we do not need to make one-to-one relations between properties by means of disjunctive subvenient properties.

However, what makes Grimes' article notable is his attempt to obtain reduction from global supervenience. For the first step, he suggests that global supervenience implies condition-to-property conditionals. Suppose A globally supervenes on B. This means that all possible worlds that are indiscernible regarding B are indiscernible regarding A. Now suppose that we select a supervenient property F in a possible world w. When F is exemplified some members of the set B are exemplified in w as well, and some are not. Let us assume that condition C is the conjunction of all the exemplifications of B-properties in w. Now it can be claimed that obtaining the condition C in any other possible world is sufficient to exemplify the property F. For if in any possible world, say w^*, we obtain C, it means that this possible world is indiscernible from w regarding B-properties. According to global supervenience, w^* and w are indiscernible regarding A-properties too. Now, because in w we have the property F, in w^* we have F as well. This means that obtaining the condition C is sufficient to have the particular supervenient property F. Grimes (1995, 114) puts this condition-to-property relation as follows,

> [A globally supervenes on $B =_{df}$] for each property F in A, if F is exemplified by some object x, then there is a condition C that is equivalent to a conjunction of all the exemplifications of properties in B such that necessarily if C obtains, and there is no exemplification of a property in B that is not entailed by C, then x has F.

This one-way condition-to-property conditional is sufficient to bring reduction. As we saw earlier, Grimes does not restrict bridge principles to biconditionals, and accepts one-way conditionals as well. Now he claims that this new relation can work in exactly the same way that one-way relations between properties do, and bring about reduction according to the same arguments[52].

Grimes takes a further step and claims that in addition to condition-to-property conditionals, global supervenience implies one-way property-to-property conditionals. He suggests that global supervenience of A on B implies the following claim:

> For each property F in A, there is a property G in B such that necessarily for every object x, if x has G, and no other object y has a property in B, then x has F. (Grimes 1995, 115)

[52] The only difference is that the conditionals entailed by global supervenience include a certain qualification as part of the antecedent, namely, that there is no exemplification of a property in B that is not entailed by C. Grimes (1995, 114) believes that this minor difference does not make any problem. '... [T]his qualification is not in any way peculiar or untoward. Instead, it simply functions as a type of ceteris paribus clause, a clause that, as many have emphasizes, is essential to almost any genuine law of nature.'

Here G is equal to conjunction of all properties in B that x possesses. This definition says that if the only object that exemplifies a property in B is x, i.e. if comparison between two possible worlds regarding B-properties is equal to comparison between x and its counterpart regarding B-properties, then the condition C in the (**) definition is a conjunction of a set of B-properties exemplified by x. Therefore, C becomes a property and we have a property-to-property conditional. Now we have exactly the same situation that we had in the case of strong supervenience: one-way relation between properties. This relation is sufficient to bring about reducibility. Therefore, Grimes claims that global supervenience implies one-way condition-to-property and property-to-property conditionals, which each is sufficient to imply reducibility.

7.3 Disjunctive Properties and Similarity

In the rest of this chapter, I will consider arguments against the general strategy of deriving reducibility from supervenience. I shall start with some proposed objections that are not acceptable according to my previous discussions of multiple realization. After that, I will consider some acceptable proposed objections. Finally, I will explain my own objection to this philosophical programme. The first objection concerns the issue of similarity. Generally, properties are understood as sources of similarity. When two objects are red for example, either they exemplify the same property (if we take properties as universals), or they have exactly similar tropes (if we take properties as particulars). However, one objection might be that in disjunctive properties we lose this notion of similarity. Teller (1983, 58) presents this example to criticize Kim's account of reduction by means of supervenience,

> ... [C]onsider the property 55 which an object has either by being a molecule with 55 hydrogen atoms in it or by being an object which weights 55 tons. Now what do two objects which have the property 55 have in common? I want to say, absolutely nothing if the two objects are both 55 by virtue of satisfying the two different disjuncts.... Two such objects have nothing in common by virtue of both having property 55 in the way that two red things do have something in common by virtue of their both being red.

According to this objection, a property must ensure some kind of similarity among objects that have it. Now if an alleged property fails to do this, this means that it is not a genuine property. Disjunctive properties, according to this argument, cannot guarantee similarity; therefore, they are not real properties. This means that the subvenient counterpart of high-level properties, which is disjunctive, is not a real property. Hence, strong supervenience cannot provide property-to-property connection, and reduction.

Kim (1990, 153-4) was aware of this objection. However, he does not find it compelling. According to him, we are discussing reduction of theories here, and theories are couched in their distinctive theoretical vocabularies. Then he concludes,

> ... [I]t seems that we allow, and ought to allow, freedom to combine and recombine the basic theoretical *predicates* and *functions* by the usual logical and mathematical operators available in the underlying language, without checking each step with something like the resemblance criterion; that would work havoc with free and creative scientific theorizing. (Kim 1990, 153, Italic mine)

I do not find Kim's reply satisfactory, and more importantly I think it contradicts his own view in the same paper. When he (1990, 152) considers the question 'given that disjunction is a permissible property-forming operation, is it proper to form *infinite* disjunctions?' he replies,

> ... [T]he answer has to be yes. I don't see any special problem with an infinite procedure here .... *We are not here talking about predicates*, or linguistic expressions, but properties; I am not saying that we should accept predicates of infinite length.... (Kim 1990, 152, Italic mine)

As we can see, in the first quotation Kim says that the relata are *predicates* and so there is no need to worry about the criterion of resemblance, which is applicable only to properties. However, in the second quotation Kim says that the relata are *properties* and so we do not need to worry about the length of the disjunction. One possible way to resolve this conflict is to take the view that corresponding to any theoretical predicate there is a property in our ontology. Kim does not take such a view, but we know that this is the standard semantical analysis of properties whose problems were discussed in detail in Chapter 5.

Although I do not accept Kim's reply, I do not find the objection persuasive. Two kinds of case must be separated. In the first case, a disjunctive property is made by disjunction of irrelevant and distinct properties. In this case, the disjunctive property does not guarantee similarity and the objection is valid. However, in the second case we have a disjunctive property where all of its disjuncts are realizers of a multiply realizable property (predicate). We do not discuss disjunction of *any* two arbitrary properties here[53]. In this case, our previous discussions about multiple realization show that the objection is not valid. First, let us consider the issue in the causal framework. Here the multiply realizable property has a set of causal powers, which is a subset of the causal powers of all of its realizers. Therefore, when two objects have that property, they have some real causal powers in common in virtue of which both exemplify the property. This means that Teller's point, that when two objects have the property 55 they have nothing in common, is not applicable to our case. Here we have a core set of causal powers that any object having the multiply realizable property has. Similarly, according to the flat framework it can be said that realizer properties are similar to each other. This means that any two objects that satisfy a multiply realizable predicate have something in common. They might have different realizer properties, but these properties are similar, and this similarity is what the objects have in common.

Now we can conclude that if by defining reduction in terms of supervenience we correlate a disjunction of irrelevant subvenient properties to a supervenient property, then the objection is valid and the disjunctive property cannot guarantee similarity. However, if we correlate the disjunction of a set of realizers to a multiply realizable property (predicate), then the objection is irrelevant. It seems to me that in real and concrete examples of reduction by means of supervenience the second option is the case, and therefore the objection is not valid[54].

7.4 Disjunctive Properties and Laws of Nature

Teller's second objection to Kim's account is that even if we accept that Boolean combinations of properties (including disjunction) are still properties, 'Boolean combinations like the ones we are considering will never figure in physical laws in

[53] This is exactly the point that Clapp (2001, 124-126) presents in reply to Armstrong's point that disjunctive properties violate the principle that a genuine property is identical in its different particulars.

[54] Rosenberg (1985) tried to derive reduction of biology to its lower-level counterparts (chemistry and physics) from supervenience of the former on the latter. His work is based mainly on Kim's account. However, he has two additional assumptions: (i) determinism entails supervenience, and (ii) the finitude of nature, according to which the number of possible realizations of a biological kind is finite. For a critical view of Rosenberg's argument, especially the finitude of nature, and a critical view of obtaining reduction from supervenience see Kincaid (1987). Zangwill (1998) has also discussed the issue of infiniteness of disjuncts in disjunctive subvenient properties. He argues that reduction must have an epistemological dimension, and the issue of infiniteness prevents Kim's model of reduction from having an epistemological dimension.

any science remotely like the physics we know today' (Teller 1983, 59). This means that the counterpart subvenient property coextensive with a supervenient property is very fat and unusual (because of endless and heterogeneous possible realizations of a supervenient property), which could never figure in laws of the subvenient science. If so, and if we keep in mind that Nagel's goal for reduction is obtaining laws of the reduced theory from laws of the reducing theory, then we can conclude that because disjunctive properties at the subvenient level do not figure in laws of this level, laws of the supervenient science could not be derived from laws of the subvenient science. Hence we do not have Nagelian reduction.

In reply to this objection, I would like to appeal to the distinction that I made in the previous section: the disjunction of endless heterogeneous properties is different from the disjunction of realizer properties. The objection might be valid for the first case; however, in the second case which seems closer to real examples of subvenient disjunctive properties, the objection is not relevant. We have discussed this point in Section 5.7.4, and here I only repeat some key points. We have familiar examples of disjunctive laws in physics. Any physical law indicating a threshold is disjunctive. (e.g., 'water at a certain pressure will boil if the ambient temperature exceeds 100° C' is a disjunctive law with literally endless disjuncts.) One might say that in physical examples, all disjuncts bring about the result by the same mechanism, while realizers of a multiply realizable property (predicate) bring about the result by different physical mechanisms (i.e. they are heterogeneous). Our metaphysical reflections show that according to both the causal and the flat analyses of realization this is not the case. According to both of these frameworks, there is something in common among all the realizers, they either have a shared set of causal powers or are similar properties. Therefore, realizers of a multiply realizable property (predicate) are not radically heterogeneous; they bring about the result by the same (or similar) mechanisms. Hence, there is not a clear distinction between normal physical disjunctive predicates and the disjunction of the realizers of a multiply realizable property (predicate): both can appear in laws.

Although Teller's objection regarding disjunctive properties (predicates) is not persuasive, there is a point in Kim's account that makes it problematic. As mentioned, Kim believes that by having one-to-one connections between high-level and lower level predicates (the condition of strong connectability), we can re-write high-level laws in terms of lower level predicates. Now if these 'images' of the high-level laws are already present at the lower-level, then there is no problem in deducing the original high-level laws from the lower level laws. If these 'images' are not present at the lower level, then we *add* them to the lover level laws and derive the original high-level laws from the new set of lower level laws.

This solution, however, is problematic. The main point in Nagelian reduction is to explain the laws of the reduced theory by means of the laws of the reducing theory plus bridge principles, such that this explanation brings epistemic pay-offs (whether empirical or historical). The main aim, however, is not just to do some logical exercises by deducing the former from the latter without any pay-offs. If we add images of the high-level laws to the lover-level laws, and deduce them again from this set, we have not obtained any epistemic gain in fact. Block has also raised this objection against Kim's proposal.

> Laws of the reducing theory together with "bridge" laws or definitions are supposed to explain the laws of the reduced theory. This condition is often ignored in the debate over multiple realizability because of the widespread positivist assumption that explanation is just deduction. If the terms of the upper level theory are all definable in lower level terms, explanation of the upper level laws is said to be trivial. The upper level laws can be deduced from the lower level theory plus definitions, and if the lower level theory isn't rich enough, the "images" of the upper level laws can simply be added to the lower level theory. As images of laws, they will be nomically necessary. But if one has to do *psychology* to discover basic laws of *physics*, the deduction of those laws of psychology from their images in physics won't be as explanatory as one might wish. (Block 1997, 111)

This point leads me to my main objection to Kim's proposal that has a wider scope, and I will discuss it in the last section of this chapter in more detail. In Kim's account, it is not important how a scientific theory connects a set of properties together. The only relevant factor for a theory to be a reducer is that it features a set of properties in its laws. But what kind of nomic relations hold between these properties is not important for the purpose of reduction. In other words, the nomic content of a theory is irrelevant to reduction. For example, as long as a theory figures the disjunctive properties at the subvenient level, it is enough to deem it a reducer theory. We do not consider how this theory connects these properties to each other, and what kind of nomic relations is suggested between them. The nomic content of this theory is irrelevant, as far as it uses some particular vocabulary. However, I would like to emphasise that an account of reduction that is neutral about nomic contents of theories seems too weak. We need more restrictions and conditions on the content of a theory to deem it as the reducer of another theory. The requirement that it must use a set of properties is too weak.

7.5 The Negation Operation

Post (1983) has presented a powerful argument against Kim's account, which can be used against that part of Grimes' account that connects strong supervenience to reducibility as well. Post discusses the negation operation in particular. He (1983, 165) argues that even if we accept that a Boolean construction from *B*-properties is a *property*, it is not clear whether it is a *B*-property. To illuminate this point, consider the negation operation and suppose that *B* is the set of physical properties including *P*: being an electron. Now the negation of this property (the property of not being an electron) is not a physical property. Suppose it is a physical property. This means that anything that has it (e.g. numbers, classes, thoughts, pains) has a physical trait simply because it is not an electron. This is obviously absurd, for example, saying that numbers have some physical properties, like not being an electron, not having mass, and so on. Therefore, the set of physical properties is not closed under the complementation operation. Another example is the set of moral properties that is not closed under this operation. Electrons are not morally good or bad, therefore they have the property of not being good. If we suppose that the set of moral properties is closed under negation, then we have to accept that electrons have moral properties.

To generalize this point, we can say that not every set of properties would be closed under the negation operation. Some sets are closed and some are not. It is something we need to check in any particular case. In addition, we know that the set of (micro) physical properties is not closed under complementation. This means that the *B*-maximal properties that we make by using negation of some physical properties are not necessarily physical. Therefore, the coextensive (or sufficient) property that we construct for a supervenient property is not a physical property. Thus, we have not reduced the supervenient properties to the set of subvenient *physical* properties. We have reduced it to a different non-physical set of properties, which cannot satisfy our aim of reduction.

I have sympathy with Post's argument; however, I think we must avoid treating it as a general strategy that can block *any* reduction by means of supervenience. This argument at most shows that *some* sets of properties (including physical properties) are not closed under *some* Boolean operations (e.g. negation), therefore we cannot use these sets as subvenient base of reduction. However, this argument is silent about those sets that are closed under Boolean operations. Thus, we still can have reduction by these subvenient bases. As Zangwill puts the point,

> The fact that propertyhood (or physical propertyhood) is not preserved under *some* Boolean operations does not show that it is not preserved under *others*. If we are not to be completely liberal about property construction, we need some way of spotting which logical operations preserve propertyhood. (Zangwill 1998, 154)

And I add that the fact that *some* sets are not closed under Boolean operations does not show that others are not closed as well. We need some criterion to check which sets are closed and which sets are not, under Boolean operations.

7.6 Equivalency between Holistic and Non-Holistic Versions of Supervenience

McLaughlin (1995, 29) presents an argument to show that the non-holistic versions of supervenience are stronger than the original definitions. Take strong supervenience as an example. According to the original definition (*S1*): A-respects strongly supervene on B-respects iff for any possible worlds w and w^* and any individuals x and y, if x in w is a B-twin of y in w^*, then x in w is the A-twin of y in w^*. While according to the non-holistic version (*S2*): A strongly supervenes on B iff necessarily, if anything has some property F in A, then there is at least one property G in B (where G is the disjunction of all possible B-maximal properties that are sufficient for F) where that thing has G, and necessarily everything that has G has F. In addition let us suppose that two relations R and R^* are equivalent only if it is impossible for some x to have relation R with y, while it does not have relation R^* with y.

Now it can be shown that *S1* and *S2* are not equivalent. To see this let us assume that the subvenient set B is closed under Boolean operations and contains all possible constructible properties. Now take a proper subset of B that includes no negative properties, no conjunctive properties and no disjunctive properties. The set A still has the *S1* relation with this proper subset, i.e. objects that are indiscernible regarding the proper subset are still indiscernible regarding A-properties. However, A does not supervene on the proper subset according to the *S2* definition. The reason is that this proper subset, according to its definition, is not closed under Boolean operations. Therefore, *S1* and *S2* are not equivalent[55].

What can be inferred from this argument? We can say that the kind of relationship that is required to bring about reduction is not the original relation of supervenience. We can obtain reduction from *some* relations, but these relations are *not* supervenience as we traditionally conceive it (i.e. a *dependent-variation* relation). We need stronger relations. McLaughlin (1995, 30) argues that while the original (holistic) definitions of weak and strong supervenience characterize 'dependent-variation' relations, one-to-one versions of weak and strong supervenience characterize 'dependence-determination' relations. For according to these versions having any property *F* in A requires having at least one property *G* in *B* (the dependency component) such that having *G* suffices for having *F* (the determination component). To conclude, the relation that brings about reducibility is not supervenience in its traditional concept. We might call it 'supervenience', but we should be aware that it involves something more than the idea of dependent-variation[56].

7.7 Supervenience as the Necessary Condition of Reduction

Our previous discussions showed that apart from the problem considered in Section 7.4, other objections to the project of defining reduction on the base of supervenience are either irrelevant or have narrow scope. In particular, we have not had a compelling argument against reduction by means of global supervenience[57]. In this section, I will present my own argument against the whole project of obtaining reducibility from supervenience, which is applicable to global and strong supervenience.

Let us assume for the moment that thermodynamic properties (strongly or globally) supervene on the mechanical properties of atoms and molecules. This means that for any thermodynamic property, we have a mechanical property or condition that is

[55] This is the case even if we, like Grimes, reject coextensionality and only say that for any *A*-property we have a sufficient property in *B*. This is also the case for weak supervenience.

[56] For a similar argument see Post (1983), and for some critical points on it see Teller (1985).

[57] Another objection that is proposed by Seager (1991) and Kim (1992) is that the bridge principle that connects a multiply realizable property to the disjunction of its realizer is not a genuine law; because it cannot be confirmed in the way that genuine laws can be confirmed. This objection is again about multiple realization rather than reduction by means of supervenience. I have discussed this point in Section 4.2.2, and replied in Section 4.2.3.

sufficient for it. Therefore, if supervenience is sufficient for reduction, thermodynamics is reducible to mechanics. However, it should be noticed that this alleged reduction is neutral about the nomic relations (laws) that exist in these theories. To see the point let us assume that apart from thermodynamics, we have another theory THERMODYNAMICS that employs exactly the same terms, predicates, concepts and properties that thermodynamics is using. However, the only difference between them is that they suggest different nomic relations among the predicates. For example, in contrast with thermodynamics that says under a constant volume if the pressure of a gas is increased its temperature will be increased, THERMODYNAMICS says the temperature will remain constant. Now according to the same arguments, if thermodynamics is reducible (by means of supervenience) to any other theory, THERMODYNAMICS is reducible too (given suitable bridge principles which will be different in each case). To be reducible, according to this account, the only requirement is that the set of high-level predicates (properties) supervenes on a more basic set of predicates (properties). Because thermodynamics and THERMODYNAMICS use the same set of predicates (properties), either both are reducible to a particular lower level theory or neither is. Similarly, thermodynamics and THERMODYNAMICS are reducible to any other theory that employs the same set of predicates (properties) that mechanics employs, whatever its nomic content is.

To illustrate this characteristic an analogy might help. Suppose we want to examine the resemblance of two polygons. The first step might be checking the number of vertices in both. If two polygons do not have the same number of vertices, they do not resemble each other. This step is similar to finding some corresponding predicate, property or condition at the subvenient level for each supervenient property. However, this is not enough. Apart from vertices, we need to check sides. For example, we need to check that the ratio of lengths of two corresponding sides is the same for any other corresponding pairs. This is exactly the step that does not exist in reduction by means of supervenience. We do not check the nomic relations among properties, the ways that a theory connects them to each other.

Why is this account of reduction not acceptable? Firstly, in concrete scientific cases we want to consider the reducibility of a particular theory with unique relations among its terms to another particular theory with unique nomic content. Reduction by means of supervenience ignores the nomic contents of scientific theories, and classifies them only according to the properties/vocabulary they use. Theories that use the same properties/vocabulary are in one group, whatever their nomic contents are. Therefore, the answer that this account provides regarding reducibility is different from the answer that we expect, that reducibility should be extremely sensitive to laws of the theories. In other words, the 'reduction' that this account defines has a different meaning from the reduction that we traditionally and intuitively understand. To obtain the traditional conception of reduction we have to put more restrictions on reduction by means of supervenience. We have to change it so that makes it sensitive to the content of laws, in addition to isolated predicates.

The second reason that this account of reduction is too weak and cannot be qualified as a sophisticated account of reduction is that it ignores the non-formal conditions of reduction. As discussed, apart from the formal condition of derivability, Nagel requires more restrictions on reduction. He mentions three non-formal conditions. A review of these conditions shows that all of them concern the nomic content of theories. For example, Nagel says that reduction must have a scientific significance and provide more knowledge and deeper understanding of the two theories. It should help us to discover new laws or increase degrees of confirmation of the present laws.

> In general, therefore, for a reduction to mark a significant intellectual advance, it is not enough that previously established laws of the secondary science be represented within the theory of the primary discipline. The theory must also be fertile in usable suggestions for developing the secondary science, and must yield theorems referring to the latter's subject matter which augment or correct its currently accepted body of laws. (Nagel 1961, 360)

These conditions make Nagel's account different from a logical exercise in which some sentences are derived from others. However, reduction by means of supervenience ignores these conditions, and is therefore too weak and without any epistemic dimension.

In reply to these points, one might say that we can simply add the non-formal conditions to the accounts of reduction by means of supervenience to compensate for their flaws. In other words, one might say that having the supervenience relation is a necessary condition for reduction that must be supplemented by other conditions, like the non-formal ones. I think this option is not as simple as it seems. As we discussed the point in Section 1.6, the non-formal conditions make Nagel's account *a reduction without necessity*. This means that formal considerations do not necessitate reduction. Reducibility is a *contingent* matter that, even by satisfying all the formal conditions, must be examined for any particular theory. Even for a particular theory, its reducibility to another theory should be checked at different stages of its development. However, reduction by means of supervenience is a reduction *with necessity*: if supervenience, then *necessarily* reduction. Adding the non-formal conditions to this account removes this modal force. We could have supervenience without reduction. Therefore, the first result of adding the non-formal conditions to the account of reduction is that supervenience no longer entails reduction. This is not a trivial change. Many advocates of reduction by means of supervenience support it because it can provide a sense for the clause 'in principle' in the definition of 'reductionism': any non-physical theory is reducible to physical theories *in principle*. They argue that supervenience guarantees reduction, if current physics does not use disjunctive subvenient predicates (properties), a complete physics in the future will use them. However, if we accept that supervenience does not guarantee reduction, then it cannot give sense to the clause 'in principle'. An advocate of reductionism must find another reason to support the claim that any non-physical theory is reducible to physical theories *in principle*.

However, someone might accept this change. According to her, we add the non-formal conditions to the accounts of reduction by means of supervenience and only hold that supervenience is a *necessary* condition of reduction, which needs the non-formal conditions to be sufficient, i.e., the supervenience relation does not guarantee reducibility but is necessary for it. This proposal has some problems.

The original holistic definitions of weak, strong and global supervenience cannot cover indeterministic causation. In the case of indeterministic causation, effects do not (weakly, strongly, or globally) supervene on their causes. Two things can be indiscernible regarding the indeterministic causes, but they are discernible regarding the effects. This means that if we define reduction in terms of supervenience, then theories involving properties and predicates relevant to indeterministic causes cannot play the role of reducer theories. In other words, reduction by means of supervenience excludes indeterministic theories as the bases for reduction. For example, we cannot reduce high-level physical theories to many important parts of modern physics, like quantum theory. However, these indeterministic theories are our best and most comprehensive basic theories, and reductionists tend to suppose them to be reducing theories. Incompatibility of reduction by means of supervenience with indeterministic reducing theories is one of the most important problems with this kind of reduction (cf. Crane and Mellor 1990, 205).

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To conclude, I tried to show that defining reduction of scientific theories in terms of the modal relation of supervenience is not possible. Some of the proposed objections to this project are not relevant, and can be answered by having a firm ontological ground. For example, by adopting the causal or flat views of multiple realization, we can show that the objection that disjunctive properties do not save similarity is irrelevant to reduction. Some other objections, for instance the one regarding the negation operation, are acceptable, but have narrow scopes of application. They cannot reject *every* possible reduction by means of supervenience. My main objection to this project concerns the non-formal conditions of reduction, which are absent. I argued that reduction by means of supervenience is not sensitive to the nomic content of theories, and so is too weak. On the other hand, adding the non-formal conditions to this account has its own consequences. Firstly, it takes the modal force of this account and changes it from an account of reduction with necessity to an account of reduction without necessity. Secondly, the supervenience relation *per se* is not appropriate to make a necessary condition of reduction. It cannot be applied to indeterministic causation, and therefore excludes indeterministic theories to be reducers.

8

Functional Reduction?

In this chapter, I shall consider an account of reduction of *properties* by means of functionalizing them. Functional reduction is one of the most recent proposed models of reduction. Proponents of this model think that it can solve problems of the previous models (e.g. Nagel's model or reduction by identity statements), and has further benefits: unification of the issues of prediction, explanation and reductive explanation. Therefore, it can be used in defining 'emergent' properties. The chapter starts with a short exposition of functional reduction, based on the Kim's proposal. Then I shall argue that his account of reducibility is inadequate. Two kinds of functionalization (*MR* and *BA*) will be separated. *BA*-functionalized properties are reducible according to the functional definition of reducible properties. However, in the case of *MR*-functionalized properties, reducibility of a property depends on reducibility of its neighbours in a causal network.

8.1 Functional Reduction

After defending an account of reduction based on the supervenience relation, Kim no longer finds it persuasive and has presented his objections to this account (2003). As a new alternative, Kim now defends an account of reduction according to that *relata* of the reduction relation are properties, rather than scientific theories. He explains this view in different works, and we focus here on his paper 'Making Sense of Emergence'.

As a rule Kim accepts mereological supervenience, according to which 'all properties of a physical system supervene on, or are determined by, its total microstructural property' (Kim1999, 7). By 'microstructural property', he means, 'the system's basic micro-constituents, their intrinsic properties, and the relations that structure them into a system with unity and stability as a substance' (*ibid*). But how can we separate emergent from reducible properties? Kim thinks that the key idea here is *predictability* and so we can separate reducible (resultant) properties from emergent properties by means of their predictability, '... resultant properties are to be those that are predictable from a system's total microstructural property, but emergent properties are those that are not so predictable' (Kim 1999, 7-8).

We can imagine two kinds of predictability: theoretical and inductive. Emergent properties are inductively predictable. This means that if we have enough correct observations that systems with microstructural property *M* have emergent property *E*, we can predict that if this particular system has *M* at time *t*, then *E* will emerge at *t*. However, emergent properties are not theoretically predictable. Kim (1999, 8) argues, 'we may know all that can be known about *M*- in particular, laws that govern the entities, properties and relations constitutive of *M*- but this knowledge does not suffice to yield a prediction of *E*'. Therefore, if we could not predict *E* by complete knowledge of *M* at the basic level, then *E* is an emergent property. Whereas any property is exclusively either reducible or emergent (regarding a given basic level), by converting the mentioned criterion, *the general definition of reducible properties* is obtained:

> The General Definition of Reducible Properties (*GD*): knowing all that can be known about microstructural property *M* at the basic level- such as laws that govern the entities, properties and relations constitutive of *M*- is sufficient to yield a prediction of *E*.

After this general definition, Kim presents two examples to show how this criterion works. According to the first case, if the concept of *E* lies entirely outside the concepts in which our theory of *M* is couched, then *E* is an emergent property. As an example, according to Kim, a phenomenal property of experience (a "quale") is entirely outside the concepts of our physical or psychological theories and so is an emergent property. According to the second case, *E* is emergent in the following situation too,

> it may be that we know what *E* is like – we have already experienced *E*- but we may be powerless to predict whether or not *E* – or whether *E* rather than another emergent *E** – will emerge when a complex is formed with a novel microstructural *M** that is similar to *M* in some significant respect. (Kim 1999, 8)

After this general definition of reducible properties, Kim presents a more operative definition, *the functional definition of reducible properties* (*FD*). Let us assume that *B* is the domain of properties serving as the reduction base. To reduce *E* to *B*, we take three steps (Kim 1999, 10-11):

Step 1: *E* must be functionalized – that is, *E* must be construed, or re-construed, as a property defined by its causal/nomic relations to other properties, specifically properties in the reduction base *B*.

Typically we have something like this, Having $E=_{\text{def}}$ Having some property *e* in *B* such that (i) *c1…cn* cause *e* to be instantiated, and/or (ii) *e* causes *f1…fm* to be instantiated. This means that having *E* by definition means having some property *e* such that *e* has a certain causal role (*C*) in *B*.

Step2: Find realizers of *E* in *B*.

Step3: Find a theory (at level *B*) that explains how realizers of *E* perform the causal task that is constitutive of *E*. (i.e., the causal role specified in Step 1, *C*)

According to this functional model of reduction, if we can causally functionalize property *E* at level *B* (by causal task *C*), and find its realizers at this level, and then find a theory at level *B* that explains how realizers of *E* perform *C*, *E* is reducible to *B*. If not, *E* is emergent.

8.2 Multiple Realization, Functionalization and Inadequacy of *FD*

In this section, I consider Kim's main definition, *FD*, by means of a special kind of functionalized properties. David (1997, 139) introduces two kinds of functionalism, common-sense functionalism and the-best-available-scientific-theory functionalism. In common-sense functionalism our source for the specification of the functional role, which we use to define of the target property, is common-sense knowledge. For example, 'being in pain is the cause of groans' is a part of our common-sense knowledge that we use in functionalizing pain. However, in the-best-available-scientific-theory functionalism our source is the best available scientific theories, for example being in pain (or its neurophysiological realizers) is a cause of secretion of hormone *X*.

Based on this categorization, but not exactly similar to it, I will introduce two other kinds of functionalism: functionalizing *E* in terms of some other *multiply realizable* properties/predicates (*MR*-functionalism), and functionalizing *E* in terms of some *basic* and non-multiply realizable properties (*BA*-functionalism). Let us suppose that we want to functionalize a high-level property/predicate *E*, which can be realized by basic properties. There are two possible ways here. According to the first (*MR*-functionalism), we functionalize *E* in terms of its causal role that it plays between two other high-level properties/predicates C and F, C is cause of E and E is cause of F[58]. The important thing here is that both of C and F are multiply realizable. Therefore, we define E such that each of its realizers is caused by one of C's realizers and causes one of F's realizers (Figure 1).

$$
\begin{array}{ccccc}
C & \rightarrow & E & \rightarrow & F \\
\uparrow & & \uparrow & & \uparrow \\
\text{realization relation} & & \text{realization relation} & & \text{realization relation} \\
\downarrow & & \downarrow & & \downarrow \\
\{c1 \ldots ck \ldots\} & \rightarrow & \{e1 \ldots en \ldots\} & \rightarrow & \{f1 \ldots fm \ldots\}
\end{array}
$$

Figure 1: Functionalizing *E* by means of two other multiply realizable properties/predicates (*MR*-functionalism)

In the second kind of functionalism (*BA*-functionalism), we functionalize E in terms of the causal role that its realizers play at the basic level: having E =def having some property e that is caused by c and causes f (where c, e, and f belong to the basic level B) (Figure 2).

$$
\begin{array}{ccccc}
 & & E & & \\
 & & \uparrow & & \\
 & & \text{realization relation} & & \\
 & & \downarrow & & \\
c & \rightarrow & \{e1 \ldots en \ldots\} & \rightarrow & f
\end{array}
$$

Figure 2: Functionalizing *E* by means of basic properties (*BA*-functionalism)

When Kim speaks of functionalizing E, he allows both of *MR* and *BA*. His main demand is that E must be functionalized in terms of the basic properties. (See step 1 above.) But both of *MR* and *BA* functionalize E in terms of basic properties. In *BA*, having E means having some property e that is caused by c, and causes f (where c, e, and f belong to the basic level). And in *MR* having E means having some property e that is caused by one of the realizers of C and causes one of the realizers of F (where

[58] It seems to me that there is a connection between common-sense functionalism and *MR*-functionalism. When we want to functionalize *our concept* of a property/predicate, say pain, we usually use our common-sense knowledge and define it as a causal link between two other high-level properties/predicates. Being in pain is caused by tissue damage and is a cause of groans. Tissue damage, groans and pain do not belong to the basic neurological level; they can be realized in different ways at this basic level. Most of (not necessarily all of) common-sense functionalizing are *MR*-functionalizing, but this does not exclude the-best-available-scientific-theory functionalizing to be *MR*.

these realizers belong to the basic level). In addition, Kim's two concrete examples show that he accepts *MR* as a way of functionalizing *E*. Kim (1999, 10) functionalizes the property of being a gene like this,

> ...the property of being a gene is the property of having some property (or being a mechanism) that performs a certain causal function, namely that of transmitting phenotypic characteristics from parents to offsprings.

As for functionalizing pain, he says (1999, 13),

> Suppose that pain could be given a functional definition- something like this, being in pain is being in some state (or instantiating some property) caused by tissue damage and causing winces and groans.

Now let us see what *FD* says about the reducibility of *BA* and *MR*-functionalized properties. To be reducible according to *FD*, in addition to step 1 (functionalizing the property), steps 2 and 3 must be satisfied. Step 3, finding a theory, is a purely empirical task and we will not consider it here. However, step 2, finding realizers of the target property at the basic level, is conceptual rather than pure empirical. In the case of *BA*-functionalized properties, there is no problem in step 2. According to the causal role that we functionalize *E* by it, every property at the basic level that is caused by *c* and causes *f* (where *c* and *f* belong to the basic level *B*) is a realizer of *E*. Because *c* and *f* are two particular properties at the basic level and we know them by complete knowledge of *B*, there is no conceptual problem to find realizers of *E* at this level. Any property that is caused by *c* and causes *f* is a realizer of *E*.

However, the situation of step 2 for *MR*-functionalized properties is not so clear. Our definition here (Figure 1) says that having *E* connects to these conditionals: (a) 'if *C*, then *E*' and/or (b) 'if *E* then *F*'. Whereas *C* and *F* do not belong to the basic level *B*, complete knowledge of *B* says nothing about them. (For example suppose that neurophysiology is our basic level, this science says nothing about pain or groans. These properties are not used in this science.) By complete knowledge of *B*, we cannot know whether our present microstructural property is caused by *C* and/or causes *F* or not. This means that we cannot find realizers of *E* at the basic level just by complete knowledge of *B*. To do this task we have to know the sets of realizers of *C* and *F* at the basic level. Then by means of them, we can decide if our present microstructural property is caused by one of *C*'s realizers and/or causes one of *F*'s realizers then it is a realizer of *E*. In other words, decision about a microstructural property in particular, and finding realizers of *E* in general, depends on knowing the sets of realizers of *C* and *F*. But how can we determine the realizers of *C* and *F* (say tissue damage and groans)? The situation of these two sets are exactly similar to the situation of the realizers of *E*, they are realizers of high-level properties. The only way to determine *ci*s is defining *C* as a functional property that has some causal relations with other properties (e.g. *C* is caused by *A* and causes *E*). Now each basic property that can satisfy this functional role is a realizer of *C* and so is one of *ci*s. But, if we want to functionalize *our concept* of *C* (such as the case of *E*), it is very probable that we functionalize it by means of two other *high-level* properties which each have a set of realizers at the basic level (in our example *A* and *E* are high level properties). In other words, it is possible and even very probable that we functionalize *C* in *MR* way.

Where does this leave us? To find realizers of *E* (step 2), we have to be able to decide whether or not our present microstructural property is a realizer of *E*. For this purpose, we have to know the sets of realizers of *C* and/or *D*. *C* is another high-level property that is normally functionalized by some other high-level properties (like *A*). Finding the set of *C*'s realizers requires finding the set of *A*'s realizers. The situation of *A* is exactly similar to *C*, *E* and *D*. And therefore, we have a chain of high-level properties where finding realizers of one of them depends on finding realizers of the rest. These sets of realizers make a network at the basic level and we cannot have only one of them in isolation. To know one of them we need to know some of its neighbours.

To find these sets of realizers at the basic level, we need a starting point. This starting point fixes and determines at least one set of realizers at the basic level, and by this, we can proceed and find the rest. The most likely option for this starting point is that we functionalize at least one of our high-level properties not in *MR* but in *BA* way. For example, if by a conceptual connection we *BA*-functionalize *C*, then finding realizers of *E* will be possible. However, this option changes the nature of Kim's proposal. Kim suggested his proposal to answer questions like 'is the particular property *E* reducible to more basic properties in *B*?' To answer this question Kim requires two things: a functionalized version of *E* and a scientific theory deals with *B*-properties. However, taking the mentioned option means that in addition to these two elements, the reducibility of *E* depends on other factors. The reducibility of *E* depends on *BA*-functionalizability of some other distinct properties. If there is a *BA*-functionalizable property in the chain that contains *E*, *E* is *FD*-reducible. If not, *E* is emergent. In fact, this option changes the reducibility of a property from an intrinsic feature of it to a relational feature. To answer a question about the reducibility of a particular property we must consider *BA*-functionalizability of some other distinct properties. The reducibility of *E* (say being in pain) depends on existence of a conceptual connection between a distinct property (say being happy) and its basic realizers. If *E* appears in a chain that one of its members is *BA*-functionalizable, then *E* is reducible. However, if the same *E* appears in a chains without any *BA*-functionalizable property, then *E* is emergent. This relational account is not what we intuitively expect from a reduction account. We expect that the reducibility of *E* is defined in terms of *E*, the set of basic properties *B* and a scientific theory, independent of other distinct properties.

To conclude, if we reject *MR* as an acceptable way of functionalizing, we add many properties to the list of emergent properties (like our concepts of multiply realizable mental properties, e.g. pain). By rejecting *MR*, we have to accept pain as an emergent property, which obviously contradicts with Kim's overall strategy. On the other hand, accepting *MR* leads us to consider the reducibility of a *MR*-functionalized property as a relational feature of it.

Part Three

What Reduction is and is not

9

A Modest Account of Reduction

In this last chapter, I will try to formulate my modest account of reduction by summing up all the points discussed in the previous chapters. This account is mainly a developed version of Nagel's account, but it has a metaphysical flavour to remove the standard problems with Nagel's account. In what follows, first I will sketch a portrait of this account, and then I will consider some of its features and consequences. In the last section, I will consider an alternative account of reduction and its similarities and dissimilarities with my account.

9.1 A Portrait

As I discussed the point in Section 3.2, approximation is a vital component of any sophisticated account of reduction. Let us suppose that *T1*, *T2* and *B.P* are the sets of reducing laws, reduced laws and bridge principles respectively. When we add (*) to one of them, we make an approximation of it (e.g. *T1** is a set of reducing laws which is distinct from *T1*, but is an approximation to it, and *B.P** is a set of bridge principles that can be approximately true). According to Nagel's classic model, the only acceptable formal structure for reduction is $T1+B.P \vdash T2$, in which *T1* and *T2* are the original reducing and reduced theories and *B.P* is the set of strictly true bridge principles. However, in my interpretation we can also use approximate counterparts of these three elements. Therefore, eight different *formal structures* are available for reduction:

(i) $T1+BP \vdash T2$ (ii) $T1^*+BP \vdash T2$ (iii) $T1+BP^* \vdash T2$

(iv) $T1+BP \vdash T2^*$ (v) $T1^*+BP^* \vdash T2$ (vi) $T1^*+BP \vdash T2^*$

(vii) $T1+BP^* \vdash T2^*$ (viii) $T1^*+BP^* \vdash T2^*$

(For example, according to the seventh option, if we deduce an acceptable approximation of the reduced laws from the original set of reducing laws and a set of approximately true bridge principles, then we have reduction.)

As this schema shows, the notion of reduction completely depends on the notion of approximation (similarity or analogy). As discussed the point in Section 3.2, we have a clearer conception of approximation in the case of bridge principles; however, we still need reliable accounts of similarity (analogy) between laws and theories to capture the different respects in which two theories/laws might be similar. Therefore, despite the work that has been done on the notion of similarity between scientific theories and laws, the formal schema of reduction cannot be regarded as complete unless we have a comprehensive account of similarity between theories in different respects. These accounts must formulate the conditions of similarity, and satisfy some basic requirements, among them the fact that similarity comes in degrees.

Our previous discussions revealed some points about bridge principles. (1) As I discussed the point and presented an example of Hempel in Section 1.5, it is not necessary that to connect a property/predicate from the reducing law to a property/predicate from the reduced law we only use *one* bridge principle. We might have a *set* of bridge principles. A set of bridge principles is acceptable as long as its members are jointly necessary and sufficient for deducing the reduced law from the reducing law. (2) Bridge principles are not limited to laws. Initial and boundary conditions and identities are examples that can be used as bridge principles, but are not laws. (3) Moreover, as discussed in Section 5.7, sentences that have nomic necessity (i.e. they are true in all nomically possible worlds), but are not laws, can be used as bridge principles. For example, the sentence 'all metals either have the property of expanding when heated or are made of green cheese' (Block 1997, 110), is not a law, but has nomic necessity because it is implied by a law. This condition allows us to use sentences that connect multiply realizable properties/predicates to their realizers as bridge principles, without any worry about their lawlikeness. (4) Our considerations in Section 5.7 showed that we could even consider a sentence that connects a multiply realizable property/predicate to its realizers as a law. As discussed, disjunctive laws exist in physics. If the only point that distinguishes disjunctive physical laws from bridge principles is that in the physical laws all the disjunct properties bring about the effect by the same mechanism, then this condition is met in the bridge principles too. Our candidate metaphysical frameworks, whether the causal or the flat conceptions of properties, guarantee that the realizers are not *radically heterogeneous*, and that they bring about the realized property/predicate by the same or similar mechanisms. (5) As discussed in Sections 3.3 and 3.4, there is no need to restrict the bridge principles to identity statements, between things or properties. Bridge principles *can* be identity statements between things, but they are not limited to this kind. On the other hand, two main motivations for restricting bridge principles to property identity are irrelevant. As discussed in Section 3.4, bridge principles do not need *explanation*, and therefore the controversial point that identity statements do not need explanation is irrelevant in the context of reduction. Moreover, we do not expect reduction to simplify our ontology. Two discussed metaphysical frameworks of realization present simple pictures of properties, in which reduction may not simplify our ontology.

Our discussions in Chapters 6 and 7 showed that defining reduction of scientific theories and laws in terms of explanation (explanatory reduction) or supervenience (reduction by means of supervenience) are not successful projects. In the case of explanation it should be pointed out that the cash-value of reduction is not reducible to explanation. We might reduce a macro-theory to a micro-theory, while the former still provides explanation that the latter cannot. Moreover, in Chapter 8, I argued that changing the relata of reduction from laws and theories to properties and defining functional reduction of properties is problematic.

The first wave of objections to Nagel's account, discussed in Chapters 2 and 3, includes two problems of inconsistency and meaning variance. I argued that these objections are not applicable to Nagel's original account, or to any extended version of it. As to the problem of meaning variance, there are two possible answers. By rejecting the presupposition that common meaning is a necessary condition for deducibility and adopting a causal account of reference (Yoshida) we can remove this problem. Alternatively, by accepting a description theory of reference but distinguishing between *different* and *radically different* meanings (Nagel), we can save the possibility of reduction. As to the problem of inconsistency, I argued that by allowing approximation in reduction, this problem is removed.

The second wave of objections to Nagel's account, discussed in Chapter 5, includes the point that those sentences which connect multiply realizable properties/predicates to their realizers are not suitable as bridge principles. In Sections 5.7.1- 5.7.4, I argued that my candidate metaphysical frameworks allow this kind of sentence to play the role of bridge principle. This means that reducing laws of the so-called 'special sciences' does not have any peculiar or unique problem over reducing physical laws. This does ***not*** mean that all special-science laws are reducible to physics *in principle*. It shows that if other conditions of reduction are satisfied (including formal and non-formal conditions), the mere fact that a theory or law uses multiply realizable properties/predicates does not block its reducibility.

Apart from the formal considerations, any valuable reduction must satisfy some non-formal conditions. I explored Nagel's non-formal conditions, to which later reductionists and anti-reductionists did not pay sufficient attention, in Section 1.6. The essence of these conditions is that reduction must be a significant scientific and empirical achievement, rather than a logical exercise between two sets of sentences. Reduction must be in the direction of the unity of science, in the sense that it connects different parts of science together and increases the degree of confirmation for the reduced and/or reducing laws. Reduction must provide further confirmation for the involved laws and theories and provide clues for new scientific discoveries.

As mentioned in section 1.7, Nagel only discussed reduction of a *true* or *approximately true* theory, which is in use, to a *true* more comprehensive theory. Therefore, when he claims that reduction must have epistemic 'gain', he only recognizes scientific and empirical gains and requires that reduction must bring with it valuable empirical achievements. However, as pointed out in Chapter 3 (and will be pointed out shortly), we may want to consider as reductions cases of theory change in which less comprehensive theories are found to be *false*. In these cases, Nagel's non-formal conditions cannot be satisfied. Therefore, I would like to add that, apart from the scientific and empirical gains that Nagel discussed, a reduction might have different gains, e.g. a reduction might have a valuable 'historical gain' and explains why scientists rationally believed in a strictly speaking false theory for a long period of the history of science. I will explain this point in more detail shortly, but here I would like to emphasize that any valuable reduction must satisfy the non-formal conditions to ensure that the reduction has considerable epistemic gains. These include scientific and empirical as well as historical gains.

Finally, the mentioned schemata cover many important reductions in the history of science, and have many concrete examples in its support. It covers the reductions of the impetus theory, Kepler's laws and Galileo's laws to Newtonian mechanics, as well as physical optics to electromagnetic theory, thermodynamics to statistical mechanics, classical mechanics to special relativity, classical celestial mechanics to the general theory of relativity, classical mechanics to quantum theory, geometrical optics to wave optics and the special theory of relativity to the general theory. In the case of false theories, this account covers reductions of early phlogiston theories of reduction, calcinations and respiration to Lavoisier's theory as well as the caloric theory of heat to kinetic theory of heat.

9.2 The Relata and the Aim of Reduction

Silberstein (2002, 82) categorizes different accounts of reduction on the basis of two questions. The first is a question of the relata: 'Reduction is a relation, but *what types of things* may be related?'(*ibid*) The second is a question of the link: '*In what way(s)* must the items be linked to count as a reduction?' (*ibid*) Regarding the question of the relata, he distinguishes two possible answers: real world items and representational items. In an ontological reduction, the relata of reduction are real world items like properties, events or entities. However, in an epistemological reduction the relata are representational items like theories, concepts, models, frameworks or regularities.

With regard to the first question, the account of reduction set out in the previous section is a type of epistemological reduction. It is clear that in this account the relata are scientific theories or laws (epistemologically construed), both of which are representational items [59]. With regards to the second question, satisfying the mentioned formal structure and the non-formal conditions are required for reduction. However, one might raise an interesting point that considering *theories* as the relata of reduction is not so simple. Consider the paradigm example of reduction of the perfect gas laws of thermodynamics to classical mechanics. Friedman rightly points out that this reduction is not the reduction of a theory at all.

> The perfect gas equation is but a highly specialized consequence of equilibrium thermodynamics. To reduce this consequence to mechanics is to accomplish little of real value. For we have not thereby eliminated even the theoretical need for laws and concepts of thermodynamics. We have done nothing to simplify the overall structure of physics. (Friedman 1982, 21)

Two points must be distinguished in this passage. Firstly, reducing one single law from a theory does not mean reducing the whole theory. Secondly, according to Friedman, after the reduction of a theory, we ought not need its laws and concepts anymore. I agree with the first point that the reduction of a single law must not be treated as the reduction of the whole theory. To make this point clearer I would like to say that the primary relata in the discussed model of reduction are scientific *laws*. Reduction in the first place is a relation between scientific *laws*. We normally reduce

[59] Under some construals laws are not representational items, e.g. when they pre-exist their discovery. However, in this context I take laws as statements in scientific theories with representational character.

a law to a body of laws (to a theory). However, this does not mean that a scientific theory cannot be reduced. If all axioms, basic assumptions and scientific laws of a theory are reducible, then the theory is thereby reducible.

However, Friedman's second point is not plausible. He claims that the aim of reduction is to eliminate the theoretical need for laws and concepts of the reduced theory. According to him, reduction must increase conceptual and nomic simplicity. In what follows, I will first show that reduction does not bring about conceptual or nomic simplicity and then present my own version of the aim of reduction. As mentioned in Section 1.7, Nagel believed that reducing *T2* to *T1* does not involve the elimination of *T2*'s concepts. This means that even in the classical form of reduction in which we do not use the notion of approximation; reduction does not mean conceptual simplicity. I would like to strengthen this point with three further considerations. Firstly, in the model of reduction that I defend, we use the notion of approximation. This means that in some cases, instead of an original theory (*T2*) we first make an approximation of it (*T2**), with slightly different concepts, and then reduce this new theory. This process shows that even if reduction brings about elimination of the concepts of *T2**, we have not eliminated the concepts of the original theory *T2*. Secondly, as discussed in Chapter 6, explanatory reduction is problematic. This means that we might have reduction in the sense that I defend, but the reduced theory can still answer contrastive why-questions that the reducing theory cannot. This point shows that, even in the case of reduction we still need laws and concepts of the reduced theories. Finally, as mentioned in Section 6.6, in some cases like thermodynamics, we select particular initial conditions at the level of reducing theory only because of some constraints of the reduced theory. This point shows that some high-level laws, regularities and concepts are indispensable, even if we reduce the theory that they appear in.

Now we face a question about the aim of reduction. If reduction does not entail the dispensability of concepts and laws of the reduced theory, and if reduction of *T2* to *T1* does not show that *T1* is more basic, or entities that are dealt with in *T1* have ontological priority to entities that are dealt with in *T2*, what is the aim of reduction? In my view, the aim of reduction is to make a *network of interconnected laws* and theories. Different laws and theories are connected by reduction. Any genuine reduction has epistemic gains: if the reduced law/theory is still in use, the reduction increases its degree of confirmation and provides new clues for more discoveries. On the other hand, if the reduced law/theory is false, the reduction shows why the scientist had rationally believed in it, why it appeared true, and how an approximation of it can be reduced to the next comprehensive theory. Therefore, in one sense the aim of reduction is to unify science. Reduction can show the unity of science in two ways. Firstly, it provides local connections between true or approximately true laws, from special sciences to fundamental physics (synchronic reduction). Secondly, it shows continuity in the history of science, despite theory changes and paradigm shifts (diachronic reduction). The unity of science in this sense is a contingent fact that must be examined case-by-case[60]. Reduction as defined here does not carry any necessity. The reducibility of a law is a contingent matter that depends on many factors, including the time of reduction. To test the unity of science (in historical or current cases), the defined reduction is a reliable tool. Reduction does not entail conceptual or nomic simplicity, but it produces networks of interconnected laws and theories (whether true, approximately true or false), with valuable epistemic gains.

9.3 Non-Formal Features of Reduction

I mentioned Nagel's non-formal conditions of reduction in Section 1.6. However, non-formal features of reduction are not restricted to these conditions. In this section, I outline three main sources of non-formal conditions for reduction. As Hull (1974, 654) has pointed out, some (logical empiricist) philosophers assume that extracting a scientific theory from the scientific literature and the raw scientific materials is a straightforward and unproblematic process. They assume that received scientific theories exist right out there in nature without any ambiguity and there were not any partially incompatible versions of those theories. However, a close look at the real development of any particular theory shows that at any time in its development 'numerous partially incompatible versions of this theory can be found in the primary literature of science. When these clusters of theories are traced through time, the multiplicity only increases.' (Hull 1974, 654) As an example, Hull considers the case of Mendelian genetics.

[60] The unity of science in this sense is different from the versions that were discussed in Sections 4.2.1 and 5.6. The scope of those accounts was limited to special sciences, while the scope of the contingent unity of science is not. Moreover, metaphysical reflections can prove those accounts, while the contingent thesis of the unity of science needs empirical evidence to be proven. Those accounts of the unity of science at most can insure that searching for contingent unity of science in the case of special sciences is not fruitless.

> At the turn of century, when Mendelian genetics was rediscovered, a half dozen geneticists set out different versions of it. During the first ten years of development of Mendelian genetics, these versions changed radically. (Hull 1974, 654)

However, in reduction debates, e.g. the reduction of Mendelian to molecular genetics, (logical empiricist) philosophers do not consider these raw theories. They have an analysis of what a scientific theory is. According to this analysis, they make a 'rational reconstruction of a scientific theory'. Then they consider reducibility of this reconstructed theory. For example, in the case of Mendelian genetics various versions of this theory, belonging to different times, can be found in the literature. However, by considering the reducibility of this theory, we only discuss one rational reconstruction of these raw theories, which is known as the received Mendelian theory.

The process of making rational reconstructions of scientific theories is one of the issues that we know very little about. There is no explicit account of making rational reconstructions of theories. All we know is that this is a non-formal activity, which does not follow a common structure in different cases. This process is the first source of non-formality in reduction.

The second source of non-formality is the presence of approximation in reduction. As discussed, in many cases it is not possible to reduce a received version of a theory. To reduce it, we have to make some modifications to it, i.e. we have to make an approximation of it, and then reduce this modified version. On the other hand, in some cases the reducing theory needs modifications to fit in our structure of reduction. In other words, like the first case that philosophers make rational reconstructions of theories according to their analysis of scientific theories, in this step they make reconstructions of the received scientific theories according to their analysis of reduction. As mentioned earlier, the notion of similarity (analogy) between laws and theories needs more work in the philosophy of science. For example, we need to know how far we can change a theory and still claim that we have an approximation of it. Currently, we do not have an explicit and sophisticated analysis of this notion to capture its non-formal character. Making approximate counterparts of scientific laws/theories or bridge principles is the second source of non-formality in reduction.

Sklar presents an example of modifying a theory to fit into our analysis of reduction.

> Like nearly all examples, this one [the reduction of the physical optical theory of light to the theory of electromagnetic radiation] fails to be quite adequate. There are predications of traditional physical optics incompatible with some of those of electromagnetic theory, e.g. the latter's predication of the exponentially decaying penetration of electromagnetic waves into the surface of a reflecting opaque object. But the reduced theory can in this case, as in others, be suitably modified, without loss of any of its idiosyncratic concepts, so that the modified version of the theory is properly totally reducible to the electromagnetic theory. (Sklar 1967, 118, n. 1)

Hull (1974, 662) believes that rational reconstruction and modification of scientific theories, which are necessary for reduction, are two main sources of non-formality in reduction. According to him (1974, 662), 'So far very little has been said about these processes. In the absence of any explicit analysis of these currently informal aspects of theory reduction, the claim that Mendelian genetics can or cannot be reduced to molecular genetics seems to be premature.' However, there is another source of non-formality in reduction, which exists in Nagel's original account, although Hull and other philosophers did not pay attention to it. As discussed in Section 1.6, Nagelian reduction is more than the derivation of a set of sentences. According to Nagel, reduction must be a significant scientific achievement that brings valuable empirical gains. His non-formal conditions of reduction are set to guarantee that a reduction has empirical gains: increases degrees of confirmation of laws or theories, and opens new horizons to discover new laws and regularities. The real value of a particular reduction, according to Nagel, is evaluated by the non-formal conditions. To the extent that degrees of confirmation of laws are increased by reduction, and to the extent that a reduction facilitates new discoveries, reduction is valuable.

As pointed out earlier, in addition to Nagel's consideration, in my view a reduction is valuable if it shows continuity in the history of science. In other words, when the reduced law or theory is false, its reduction might still be valuable if it explains why scientists believed in a false theory for a while, and how a theory replaced a false theory with which it is inconsistent, but of which it entailed a good approximation. For these reasons, I would like to emphasize that the epistemic value of reduction is not limited to empirical gains. Historical gains are another alternative. In sum, the non-formal conditions of reduction, as Nagel set, are the third source of non-formality in reduction. In the case of reducing false theories, although these conditions are not satisfied properly, another set of non-formal considerations are present. *A valuable reduction of a false theory must provide evidence for continuity and unity of that discipline during the history, it must explain how scientists rationally believed in a false theory and how the successor theories reduced the false theory and entailed a good approximation of it*[61].

9.4 Reduction as a Spectrum

Many philosophers have tried to distinguish homogeneous and heterogeneous reduction. As mentioned in Section 1.1, according to Nagel reduction of *T2* to *T1* is homogeneous if the laws of *T2* employ no descriptive terms that are not also used with approximately the same meaning in *T1*. In other words, the reduction is homogeneous if all descriptive terms of *T2* are also used in *T1*, and each of them has approximately the same meaning as its counterpart in *T1*. Sklar (1967, 110) believes that 'Two theories will be said to be *homogeneous* if they share the same conceptual apparatus, and *inhomogeneous* if one contains a concept not found in the other.' Friedman (1982, 20) mentions other criteria,

> It has been claimed that in heterogeneous reductions the entities of the secondary theory are collections of entities of the primary theory, while in homogeneous reductions the two theories deal with the same entities; that in heterogeneous reductions at least some of the concepts of the secondary theory are shown to be eliminable, while in homogeneous reductions none of these concepts may be eliminated; that heterogeneous reductions are reductions from a higher level to a lower one and are transitive (if one theory reduces to a second and a second reduces to a third then the first reduces to the third), while homogeneous reductions are intralevel and are not generally transitive.

I see no reason to commit myself to any of these distinctions. As discussed in Section 1.3, Nagel's definition of homogeneous reduction faces problems, among them the fact that there is no articulated account of similarity among meanings and the point

[61] In the case of reducing a false theory, we can use the notion of approximation. This means that the reducing theory might entail an approximation of our original false theory. This point makes my account of reduction consistent with 'Kuhn-loss' phenomenon. According to Kuhn (1970, 169), in some cases in the history of science the new theory could not explain some phenomena that the old theory had explained. In these cases, the new (reducing) theory infers an approximation of the old (reduced) theory, such that this approximation does not explain some phenomena that the original reduced theory explained.

that it is very difficult to find a concrete homogeneous reduction in the history of science. However, the main reason to reject all distinctions between homogeneous and heterogeneous reductions is that the defined schema of reduction in the previous sections can easily cover both homogeneous and heterogeneous reductions. For example, in an imaginary case where we have a pure Nagelian homogeneous reduction, the set of bridge principles is empty and the reducing theory itself concludes the reduced theory (or a good approximation of it). Therefore, Nagelian homogeneous reduction is a special case of our general structure.

Another distinction posed by some philosophers is the distinction between retention and replacement (see e.g., Sklar (1967, 111) and Hooker (1981, 42)). Roughly speaking, in the former, the reduced theory (or as some philosophers put it the ontology of the reduced theory) is retained as correct after the reduction. However, in the latter, the reduced theory is replaced by the reducing theory. Our previous considerations show that I do not agree with this distinction either. I think the proposed formal structure of reduction (a good approximation of the reducing theory and a set of approximately true bridge principles conclude a good approximation of the reduced theory) can cover both of these cases. As to the non-formal conditions, as I pointed out earlier, any valuable reduction/replacement must have epistemic gains, including empirical and historical.

As a result, like some other philosophers (e.g. Yoshida (1977) and Hooker (1981)) I take reduction as a spectrum. This spectrum covers homogeneous and heterogeneous reductions as well as the replacement of theories. All these cases follow the same formal structure. However, what distinguishes them and makes some of them outstanding and valuable is how they satisfy the non-formal conditions. Bringing more epistemic gains means the reduction is more valuable. A reduction of a current theory might be less valuable than a reduction of a false theory, if the former brings few empirical gains while the historical gains of the latter are considerable. Similarly, obtaining the laws of a special science from physical laws might not be qualified as a reduction, if this activity 'diverts needed energies from what are the crucial problems at this period of the discipline's expansion, without being compensated by effective guidance from the primary science in the conduct of further research' (Nagel 1961, 362). In a word, reduction is a spectrum containing homogeneous (if any), heterogeneous and theory changes with a common formal structure, but what makes a reduction valuable is epistemic gains obtained by it.

There is another sense that we can say reduction is a spectrum. As mentioned, approximation is a vital component of reduction. It is an accepted point that approximation comes in degrees; T^* is a closer approximation of T than T^{**}. This characteristic is transmitted from approximation to reduction. Suppose that we have two reductions: $T1 + BP1 \vdash T2^*$ and $T1 + BP2 \vdash T2^{**}$. If $T2^*$ is a closer approximation of $T2$ than $T2^{**}$, then the former reduction is more valuable than the latter. In other words, to the extent that approximate versions of the reduced theory, reducing theory and the bridge principles are closer to their original items, the reduction is more 'reductive'. If a good approximation of the reduced theory is inferred, then the reduction is more reductive. If a weak approximation of the reduced theory is inferred, then the reduction is less reductive. Reduction is a spectrum and comes in degrees. What determines the degree of reduction is the proximity between the approximate items used in reduction and their original counterparts.

9.5 Directions of Reduction

> There is a powerful tendency to take it for granted that physical order and logical order correspond to one another. We often yield to the prejudice that the physical relationship of things determines the logical relationship of properties referring to them. It is usually assumed for instance that propositions describing the properties of a physically 'complex' whole are logically more 'complex' than those referring to the properties of its constituent parts. Statements about the behaviour of the 'elements' or 'simple' parts of a material system are regarded as if of necessity also logically more 'elementary' or 'simple'. (Schlesinger1961, 215)

What Schlesinger describes is an example of a general tendency to connect (micro)-reduction to some underlying ontological relations, and thereby determine the direction of (micro)-reduction and provide 'necessity' for it. We can categorize this tendency into three main groups: ontological priority (atomism), supervenience and determinism. According to atomism, any whole is composed of basic and elementary particles (atoms). A whole is nothing over and above the aggregation of its atoms. Atoms have ontological priority over wholes. We can have a possible world containing atoms and not the corresponding whole. However, we cannot have any possible world containing the same whole without the constituting atoms. Given this ontological relation, an argument for the direction of (micro)-reduction and its necessity would be like this: macro-behaviours, macro-properties or macro-theories relating to a whole are nothing over and above their micro-counterparts. In other words, because of atomism, macro-features are reducible to micro-features and (micro)-reduction has necessity.

The second argument is based on mereological supervenience: 'An object's intrinsic qualitative properties supervene on the intrinsic qualitative properties and (restricted) interrelations of its constituent atoms' (Merricks 1998, 59-60). This means that if two objects are indiscernible in respect of intrinsic qualitative properties and (restricted) interrelations of their parts, they are (necessarily) indiscernible in respect of their intrinsic qualitative properties. An argument might connect supervenience and (micro)-reduction as follows: because of supervenience, macro-behaviours, macro-properties and macro-theories relating to a whole are necessarily (micro)-reducible to their micro-counterparts.

Finally, the third argument is based on evolution and ontogenesis hypotheses. According to Oppenheim and Putnam (1958, 9) scientific theories are arranged in six different levels of universe of discourse: 1) elementary particles, 2) atoms, 3) molecules, 4) cells, 5) (multicelluar) living things and 6) social groups. 'The hypothesis of evolution means here that (for n= 1…5) there was a time when there were things of level n, but no things of any higher level' (Oppenheim and Putnam 1958, 14). 'Similarly, the hypothesis of ontogenesis is that, in certain cases, for any particular object on level n, there was a time when it did not exist, but when some of its parts on the next lower level existed; and that it developed or was causally produced out of these parts'(Oppenheim and Putnam 1958, 14-15).

Now their argument for the necessity of (micro)-reduction is this.

> Let us, as is customary in science, assume causal determination as a guiding principle; i.e., let us assume that things that appear later in time can be accounted for in terms of things and processes at earlier times. Then, if we find that there was a time when a certain whole did not exist, and that things on a lower level came together to form that whole, it is very natural to suppose that the characteristics of the whole can be causally explained by reference to these earlier events and parts; and that the theory of these characteristics can be micro-reduced by a theory involving only characteristics of the parts. (Oppenheim and Putnam 1958, 15)

Two points are common in these three arguments. First, they determine the direction of reduction according to the direction of some other relations. For example, the first argument says that because of the direction of the whole-part relation and because parts have ontological priority over wholes, micro-theories must appear as reducing theories and macro-theories as reduced theories. In other words, this argument allows micro-reduction and rejects macro-reduction (reduction of a micro-theory to a macro-theory) because of the part-whole relation. Second, all of the mentioned arguments attribute necessity to (micro)-reduction. Ontological relations necessitate (micro)-reductions.

I will consider the first point in this section, and leave the second to the next section. As far as I can see, directions of ontological relations do not determine the direction of reduction. As Schlesinger observed, the presupposition of the claim that directions of ontological relations determine the direction of reduction is a deep-rooted conviction that there is a parallel order between physical and logical orders. Whenever we have a physical order (e.g. the whole-part relation), the logical and reductive orders must be in the same direction. However, this presupposition lacks objective foundation. Two things determine the direction of reduction: the possibility of deduction and satisfying the non-formal conditions. The possibility of deduction (the formal structure of reduction) depends on the logical forms of the premises (the reducing laws), the bridge principles and the conclusions (the reduced laws). This is independent of whether or not the entities that the premises and conclusions are dealing with bear particular ontological relations to each other.

As a result, ontological relations do not determine the direction of reduction. We can have micro and macro-reductions. If a micro-theory is drivable from a macro-theory, and this derivation has epistemic gains, then the macro-reduction is acceptable (e.g., Mach's principle according to which the inertial mass, even of micro-particles, depends on how matter is distributed throughout the universe is positively macro-reductive (cf. Crane and Mellor (1990, 190)).

Another relation that does not determine the direction of reduction is causation. If the formal and non-formal conditions of reduction are satisfied, the following cases are acceptable *in principle*: reducing a theory dealing with effects to a theory dealing with causes, reducing a theory dealing with causes to a theory dealing with effects, and reducing a theory dealing with a set of effects to a theory dealing with another set of effects of the same causes. To illuminate this point let us examine Sklar's example that was presented in Section 3.3. According to Sklar (1967, 118-121), in some cases we have a correlation between two attribute-predicates (e.g., electrical and thermal conductivity of metals are correlated by means of the Wiedemann-Franz law[62]) and therefore we can derive laws of one theory from another, but we still do not have reduction. Sklar argues that both thermal and electrical regularities are determined by a common cause, (say microphysical arrangements), and therefore both must reduce to a third theory dealing with the common cause. My point is that there is no *a priori* reason to reject this case as a reduction, simply because it is a relation between two macro-theories dealing with effects of a common cause. Apart from formal considerations of deducibility, we have to pay attention to the non-formal conditions. If by reducing the laws of thermal conductivity to the laws of electrical conductivity we obtain empirical and scientific achievements, for example if this reduction facilitates new scientific discoveries, then it must be qualified as a reduction. On the other hand, if this reduction is simply a logical exercise between two sets of sentences without any epistemic gains, then it is not a reduction.

The same argument can be used to reply Nickles' point, which I presented in note 32 in Section 3.5. According to Nickles (1973, 192-3), Nagel's account of reduction faces the problem that the DN model of explanation does not have a causal direction, i.e. in some cases we can explain a cause by its effect as well as explain an effect by its cause. Therefore, using the DN model in reduction has also this problem, i.e. we can reduce the primary theory to the secondary one as well as reduce the secondary theory to the primary one, which seems absurd to Nickles. Nickles was right to suppose that using the DN model of explanation in reduction makes reduction insensitive to the direction of causality. In fact, my main point in this section is that ontological relations, like causation, do not determine the direction of reduction. However, it does not follow that in any case of reduction we can substitute the reduced and reducing laws/theories. In any valuable reduction, the non-formal conditions must be satisfied. Now, if, by substituting the reduced and reducing theories/laws, the formal structure of reduction is satisfied and the new derivation brings about genuine scientific/historical gains, then there is no reason to hesitate to

[62] $K = \sigma LT$ (K: thermal conductivity, σ: electrical conductivity, T: temperature and L: Lorenz number which is equal to 2.45×10^{-8} $W\cdot\Omega\cdot K^{-2}$) According to this law, the ratio of the thermal conductivity to the electrical conductivity of a metal is proportional to the temperature. Qualitatively, this relationship is based upon the fact that the heat and electrical transport both involve the free electrons in the metal. The thermal conductivity increases with the average particle velocity since that increases the forward transport of energy. However, the electrical conductivity decreases with particle velocity increases because the collisions divert the electrons from forward transport of charge. This means that the ratio of thermal to electrical conductivity depends upon the average velocity squared, which is proportional to the kinetic temperature.

accept the new derivation as a reduction. On the other hand, if the substitution of theories/laws makes problems for the formal structure of reduction or lacks scientific/historical gains (which is more probable in concert examples), then there is no reason to accept it as a reduction. To sum up, contrary to the explanation that the direction of causation may distinguish valid from invalid explanations, in reduction satisfying the non-formal conditions does this task. There is no need to determine the direction of reduction by the direction of causation.

9.6 Necessity of Reduction

As discussed in Section 1.6, Nagel presents a reduction *without* necessity. This means that formal or ontological considerations cannot guarantee the necessity of reduction in a particular case. Reducibility is a contingent feature of laws/theories, which depends on many non-formal factors like the particular theories available at that time. Contrary to Nagel's account, we have explored some accounts of 'reduction with necessity'. The three accounts of reduction mentioned in the previous section are examples of this kind. We have explored one of them (reduction by means of supervenience) in detail in Chapter 7, and have seen that this account faces many problems, among them the problem that it is insensitive to the nomic contents of reduced and reducing theories. This problem applies to other accounts of reduction with necessity. They claim that ontological relations guarantee epistemic reduction, whatever the content of theories. One way to remove this problem is to add the clause 'in principle': if for some logical or practical reasons the current laws/theories are not reducible, the future comprehensive laws/theories are reducible *in principle*.

Nagel's account of reduction, and any developed version of it that commits to the non-formal conditions of reduction, lacks this 'in principle' feature. There is no guarantee to ensure that some future theories are reducible. Reduction is sensitive to the content of theories and must be examined for particular cases at particular times individually.

Although I defend a version of reduction which does not have 'in principle' clause with the mentioned meaning, in the case of reduction of special sciences it has an 'in principle' clause in a very different sense. As I argued in Sections 5.7.1-5.7.4, multiple realization is not a problem for reduction. The mere fact that a law/theory contains multiply realizable properties/predicates does not block the possibility of its reduction. Of course, we do not have any *a priori* reason to suppose a special science reducible, but on the other hand, we do not have any *a priori* reason to suppose it irreducible. Bridge principles connecting multiply realizable properties/predicates to their realizers are suitable for reduction. Therefore, if other formal and non-formal conditions are satisfied we can reduce a special science. In this sense, we can claim that special sciences (or any science with multiply realizable properties/predicates) are reducible 'in principle'. Here 'in principle' means there is no unique and peculiar problem with reducing them. Their situation is similar to physical laws/theories. If the conditions of reduction are satisfied, they can be reduced to other laws/theories.

9.7 An Alternative View

In this last section, I will consider an alternative account of reduction, then compare and contrast it with my account. Smith (1992, 19) argues 'that there is a conception of the unity of science, more modest than some but still far from trifling, which remains untouched by representative arguments for disunity.' His conception of reduction is 'explanatory interfacing': an explanatory interfacing of *T2* to *T1* means 'an explanation in terms of theory *T1* of why theory *T2* works as well as it does' (Smith 1992, 22).

Smith accepts an ontological principle, *P*, which underlies the possibility of reduction. According to *P*, 'the behaviour of wholes is in general causally produced by the behaviour of the parts' (Smith 1992, 25). Now whenever this principle holds, we expect explanatory interfacing (i.e. reduction), i.e. if *P*, then 'our explanatory stories about wholes must be consonant with our stories about the causal mechanism constituted by their parts' (Smith 1992, 25). In other words, in the cases that the principle *P* applies, i.e. where there are lower-level theories describing micro-causal mechanisms and higher-level theories describing macro-behaviours, it is very natural to demand explanatory interfacings (reductions).

Smith puts the point as follows,

> Suppose, in other words, that we hold that there are causal mechanisms fairly well described in terms of *T1*, mechanisms which generate the events that are redescribed and re-explained by *T2*. Then the question is pressing: how in that case do the causal mechanisms, discerned at the level of *T1* and thus specifiable without any reference to *T2*, come to generate the new patterns discerned by *T2* explanations? (Smith 1992, 23)

Smith believes that by showing an explanatory interfacing of *T2* to *T1* we can answer this question. In short, Smith first presents an ontological relation (principle *P*), and then claims that wherever this relation applies we expect a modest reduction, in the sense that the lower-level theory explains why the higher-level theory works as well as it does.

Smith's proposal has strong similarities with my version of reduction, which I will consider first. Smith (1992, 22-3) argues that even in the cases where we have explanatory interfacing, the reducing theory might not answer some why-questions that the reduced theory answers. This is exactly the point that I have argued for in Chapter 7. It is very interesting to point out that Smith's argument, like mine, is based on the point that some why-questions are contrastive and the reducing and reduced theories answer explanatory requests with different contrast classes. Therefore, according to both accounts, reduction does not bring the dispensability of the laws and concepts of the reduced theory. A reduced theory might be autonomously explanatory.

Moreover, Smith asserts that reduction (explanatory interfacing) does not need type-type correlations or identifications between kinds, and therefore does not conclude elimination of the reduced theory.

> ...explanation can come in a variety of flavours. There is no necessary implication, for example, that the mode of explanation in question has to involve type-type correlations or identifications between kinds recognized by the two theories. Nor is there any necessary implication that explaining the applicability of *T2* in terms of an underlying theory *T1* must mean that *T1* absorbs, supersedes, or eliminates the reduced theory *T2*.' (Smith 1992, 21-22)

Similarly, in my account, bridge principles are not limited to biconditionals, identity between things, identity between properties or kind-kind relations, and we do not expect elimination of the reduced theory.

Finally, Smith (1992, 36) holds that '*modest reductions can still subserve the old programmatic aim of demonstrating unity*, i.e. of showing how science hangs systematically together, with higher-level theories being shown to have application in virtue of lower-level facts, with basic physics presumably at the most fundamental level of the hierarchy.' More importantly, he (1992, 38) rejects that the unity of science has an *a priori* basis. This empirical question must be answered by reduction.

Smith's view on the unity of science and the role of reduction is very similar to my view, which was explained in Section 9.2.

However, there is an important point that separates Smith's account from my version of reduction. As mentioned earlier, Smith backs up his account of reduction by an ontological relation (the principle *P*). Therefore, in the cases that this relation does not hold we do not have any reason to expect reduction. The principle *P* is about the part-whole relation; therefore, Smith's account of reduction is applicable only to micro-reductions. However, in some cases, the principle *P* does not hold but we expect reduction. Take for example the reduction of classical celestial mechanics to the general theory of relativity. Here we do not have a part-whole relation and the principle *P* is not applicable. However, we expect an account of reduction to cover this example. In other words, Smith's model depends on an ontological relation and has a narrow domain of application (micro-reduction), while the defended model is independent of any ontological relation and can cover more cases, when the principle *P* is not applicable[63].

[63] Smith (1992, 28) thinks that in Nagel's account of reduction (or even in 'quasi-Nagelian' accounts that the notion of approximation is used), bridge principles must be type-type connections. According to this supposition, in Section 6 of his paper he presents some examples of non-Nagelian explanatory interfacings (reductions). He calls them non-Nagelian because the requirement of type-type bridge principles cannot be satisfied in them. However, Nagel's examples and my previous discussions show that type-type bridge principles are not necessary even for Nagelian reduction. Initial conditions and one-way conditionals with nomic necessity are examples of acceptable bridge principles that are not type-type connections.

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