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A CONTEXTUAL APPROACH TO SCIENTIFIC UNDERSTANDING

ABSTRACT. Achieving understanding of nature is one of the aims of science. In this paper we offer an analysis of the nature of scientific understanding that accords with actual scientific practice and accommodates the historical diversity of conceptions of understanding. Its core idea is a general criterion for the intelligibility of scientific theories that is essentially contextual: which theories conform to this criterion depends on contextual factors, and can change in the course of time. Our analysis provides a general account of how understanding is provided by scientific explanations of diverse types. In this way, it reconciles conflicting views of explanatory understanding, such as the causal-mechanical and the unificationist conceptions.

1. INTRODUCTION

In a recent review of debates about the nature of scientific explanation, Newton-Smith (2000) observes that fifty years of discussion have not led to consensus, but, on the contrary, to many rival models of explanation. Acknowledging that the various models provide insight into different aspects, Newton-Smith (2000, 130–131) emphasises the need for "some deeper theory that explained what it was about each of these apparently diverse forms of explanation that makes them explanatory". The present situation, in which we lack such a theory, is "an embarrassment for the philosophy of science" (ibid., 132). What could the "unifying" concept in the desired theory be? Newton-Smith briefly considers the notion of understanding: all explanations supposedly give understanding, and a general theory of understanding might tell us how. However, he immediately rejects this again on the grounds that understanding is a subjective, psychological and therefore unfruitful concept (a well-known objection: cf. Hempel 1965, 413). The present paper argues, *pace* Newton-Smith, that understanding can play the desired unifying role. It presents an analysis of the nature of scientific understanding and proposes an answer to the question of how different types of explanation can provide understanding.

What is scientific understanding and when is it achieved? Some philosophers of science claim that science provides understanding by presenting a unified picture of the world (Friedman 1974; Kitcher 1981, 1989; Schurz

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and Lambert 1994). A competing view is the causal conception of understanding (Salmon 1984, 1990, 1998; Humphreys 1989; Dowe 2000). It is remarkable, however, that none of these philosophers starts by giving a clear account of exactly what understanding consists in, in order to show how it is achieved by either unificationist or causal analysis. Typically, authors simply affirm that a particular form of explanation provides understanding and make no attempt at justifying their claim.

Scientists are not unanimous about the nature of understanding either. Lord Kelvin famously stated: "It seems to me that the test of 'Do we or not understand a particular subject in physics?' is, 'Can we make a mechanical model of it?' "(Kargon and Achinstein 1987, 3 and 111). But while this view of scientific understanding had strong appeal and was widely supported in mid-nineteenth century, today no physicist will defend it: classical mechanics has long lost its paradigmatic position. As a second example, consider the fact that today hardly any scientifically educated person will judge Newton's law of inertia unintelligible, whereas to most of Newton's contemporaries it was mysterious (see Dijksterhuis 1950, 512–513). The history of science shows great variation in what is and what is not deemed understandable. Even at one particular moment in history opinions about what is understandable often diverge (more examples of such variation will be supplied below, in the course of our argument).

Should we rely on the views of practising scientists, with the danger of being led into a view according to which understanding is merely a matter of fashion? Or should we look for a philosophical, generally valid conception of scientific understanding? Our strategy is to bypass this dilemma by proposing an approach that offers a *general* characterisation of scientific understanding, but one that can encompass the historical variation of specific intelligibility standards employed in scientific practice. While acknowledging the pragmatic nature of understanding and its associated context-dependence, we formulate a generally applicable, non-trivial criterion for the attainment of understanding. We will argue that understanding, in the sense of this criterion, is epistemically relevant and transcends the domain of individual psychology.

The article is structured as follows. In Section 2 we defend the idea that understanding is part of the central aims of science, arguing against the view that it is merely a (philosophically irrelevant) psychological by-product of scientific activity. In Section 3 we consider philosophical theories of explanation that are based on specific standards of scientific understanding, discussing the merits as well as the problems of these theories. Our discussion in Sections 2 and 3 is not only critical but naturally leads up to a new notion of scientific understanding. In Section 4, we make this new analysis explicit, defend it, and apply it to concrete examples. Although most of these examples are from physics, we believe that our approach applies to natural science in general.

2. UNDERSTANDING: AN EPISTEMIC AIM OF SCIENCE

Nowadays few philosophers of science will contest that they should take account of scientific practice, both past and present. Any general characteristic of actual scientific activity is in principle relevant to the philosophical analysis of science. Thus, if the majority of scientists considers understanding to be among the central cognitive aims of science, then this cannot be ignored. But is there such a consensus about the significance of understanding? Is achieving understanding an aim in scientific practice? This is not a trivial question: one might even wonder whether there are central aims of science at all. Detailed historical study has revealed a variety of aims of scientists in different periods of history, and some historically-minded philosophers have concluded from this that science does not have any universal aims. Thus, Laudan (1984, 138) claims that it is impossible to state what the central cognitive aims and methods of science are or should be, because "we have seen time and again that the aims of science vary, and quite appropriately so, from one epoch to another, from one scientific field to another, and sometimes among researchers in the same field" (cf. Laudan 1990, 47–54, and Longino 1990, 17–19).

We agree with Laudan that the practice of science should be taken seriously, and that there is a variation in the attitudes of scientists. But this does not entail that a general characterisation of the aims of science is impossible or meaningless. To see this, it is helpful to distinguish between three levels on which scientific activity can be analysed: the macro-level of science as a whole; the meso-level of scientific communities; and the micro-level of individual scientists. Elsewhere one of us (De Regt 1996b) has argued that this three-level structure allows us to account for the variation in heuristic philosophical influences on science. We propose an analogous analysis for the variation in scientific aims.[1] For example, all scientists will agree that they aim to produce knowledge that is supported by experience; this is a macro-level aim of science. However, when it comes to the question of exactly how, and how strongly, scientific knowledge has to be supported by experience, the answers given by scientists from different communities, and sometimes even by scientists within the same community, will differ; these are meso- or micro-level differences. The three-level distinction reconciles the existence of universal aims of science with the existence of variation in the precise specification and/or application of these general aims. The macro-level characterisation of aims must necessarily be general in order to accommodate meso- and micro-level differences, but it may still provide us with significant information about science. We will argue that achieving understanding is among the general (macro-level) aims of science. This leaves open the possibility that scientists in different historical periods or in different communities have quite different specific views about precisely how scientific understanding is to be achieved (cf. Kuhn's analysis of the role of values in science; see Kuhn 1977). Early historicist philosophy of science (Kuhn, Lakatos and the early Laudan) focused on the meso-level, but further historical research showed that their units of analysis (paradigm, research programme, research tradition) were not as monolithic as presumed and that also at the micro-level relevant differentiation may exist. Nonetheless, with respect to understanding and intelligibility the most important variation appears at the meso-level: within a particular community standards of intelligibility are usually shared (see e.g., Rouse 2003, who argues that Kuhnian paradigms provide shared ways to understand the world, rather than shared beliefs about the world).

Let us now consider in more detail why understanding is essential to the epistemic aims of science. Some philosophers deny understanding this status, claiming that it is an epistemically irrelevant, psychological by-product of scientific activity. Thus, Hempel (1965, 413) recognised the relation between explanation and understanding, but argued that the latter notion cannot be fundamental: "such expressions as 'realm of understanding' and 'comprehensible' do not belong to the vocabulary of logic, for they refer to the psychological and pragmatic aspects of explanation". (It may be noted that there is an analogy here to certain discussions in the foundations of mathematics: it is often argued that the only thing of epistemic relevance to mathematics are *proofs*, whether or not they provide understanding of the result that is proved.) Hempel explains his point as follows:

> Very broadly speaking, to explain something to a person is to make it plain and intelligible to him, to make him understand it. Thus construed, the word 'explanation' and its cognates are pragmatic terms: their use requires reference to the persons involved in the process of explaining. [...] Explanation in this pragmatic sense is thus a relative notion: something can be significantly said to constitute an explanation in this sense only for this or that individual (Hempel 1965, 425–426).

For Hempel this entailed that an analysis of 'understanding' is not relevant to the *philosophy* of science.[2] Below we will argue against that conclusion. But we agree with Hempel that the notion of understanding is pragmatic, in the sense that it concerns a particular purpose or effect of a scientific theory (or statement) for the person who uses it. One can use the term 'understanding' only with – implicit or explicit – reference to human agents: scientist *S* understands phenomenon *P* with theory *T* in hand.[3] That understanding is pragmatic in this sense implies the possibility of disagreement and variation based on contextual differences. For example, in 1926 physicists in the Copenhagen-Göttingen circle believed that atomic phenomena could be understood with the theory of matrix mechanics, while most other physicists – notably Schrödinger – disagreed. Such differences may be traced back to different scientific, philosophical or social backgrounds (see De Regt 1997, 2001).

A present-day writer with ideas that are in some respects comparable to those of Hempel is Bas van Fraassen. On his account of science, explanation is part of a pragmatic dimension, which he explicitly contrasts with the epistemic dimension that he deems central to science (van Fraassen 1980, 4 and 87–96). He defines pragmatic reasons as "specifically human concerns, a function of our interests and pleasures"; they are contextual factors which are "brought to the situation by the scientist from his own social, personal, and cultural situation" (ibid., 87–88). Van Fraassen (1977, 144) rejects the "false ideal [...] that explanation is the *summum bonum* and exact aim of science". Explanation is not an aim of science itself but a human activity in which one may *employ* scientific knowledge. Although the issue of the character of scientific understanding is barely touched upon by van Fraassen, one may safely conclude that his analysis of the nature of explanation as extra-scientific holds *a fortiori* for understanding. In a similar vein, Trout (2002) has recently argued against the epistemic relevance of understanding; he claims that understanding is a purely subjective phenomenon that should not be allowed to play a role in the epistemic evaluation of scientific theories and explanations (see De Regt 2004 for a critique of Trout's argument).

Contra Hempel, van Fraassen, and Trout, we hold that the pragmatic nature of understanding is not inconsistent with it being epistemically relevant. On the contrary, we submit that understanding is an essential ingredient of the epistemic aims of science; without understanding these aims will remain out of reach. According to empiricists such as Hempel and van Fraassen, the epistemic aim of science is (roughly stated) the production of factual knowledge of natural phenomena. However, not even a radical empiricist will be satisfied with a mere list of true descriptions or predictions. Philosophers of science, as well as scientists themselves, agree that the aim of science transcends merely listing facts: one needs at least some general law or theory that produces these descriptions or predictions. But possessing a theory is not enough: in addition one should *be able to* *use* the theory to derive predictions or descriptions of the phenomenon. And this implies that not only knowledge of laws and theories (and background conditions) but also particular *skills* of the user of this knowledge are involved in achieving the epistemic aim of science. This introduces a pragmatic element that, we will argue, is part of an epistemically relevant notion of understanding.

Contrary to what may seem at first sight, this practical aspect is not trivial. In modern science the application of a theory to empirical reality is typically not a simple deductive procedure. The usual situation is not one of directly generating successful empirical predictions from theories plus boundary conditions. Instead, there are intermediate stages in which pragmatic aspects play an important role, a theme extensively discussed by Nancy Cartwright. Analysis of examples from scientific practice leads Cartwright (1983, 58–59) to conclude that theories are true of models, which fail to correspond exactly to reality. Applying a model to a real system is a matter of intricate approximation. Formal principles, telling us how to get from a theory via a model to a description of a real system, do not exist: "There are just rules of thumb, good sense, and, ultimately, the requirement that the equation we end up with must do the job" (ibid., 133). Accordingly, the generation of scientific knowledge of empirical reality is inherently bound up with pragmatic skills, evaluations, and decisions. Achieving the epistemic aim of science is not simply a matter of writing a theory and determining its empirical content, but a complex process that unavoidably has a pragmatic dimension.

Not only skills of scientists but also properties of theories play a role in this dimension: whether scientists are able to apply a theory to a particular phenomenon depends both on their skills and on the pragmatic virtues of the theory, e.g., visualisability or simplicity. These virtues may contribute to the intelligibility of the theory, thereby facilitating the use of the theory in the construction and application of models, and accordingly they contribute to the achievement of the epistemic aims of science. The appropriate combination of scientists' skills and intelligibility-enhancing theoretical virtues is a condition for scientific understanding. Understanding in this sense is not purely subjective and individual: the acquisition of required skills, attuned to preferred theoretical virtues, is typically relative to scientific communities rather than individuals; i.e., established at the meso-level (Kuhn 1970; Rouse 2003).

Understanding is an inextricable element of the aims of science. As another illustration of this claim, and as a further step towards a characterisation of scientific understanding, contrast a scientific theory with a hypothetical oracle whose pronouncements always prove true. In the latter case empirical adequacy would be ensured, but we would not speak of a great scientific success (and perhaps not even of science *tout court*) because there is no understanding of how the perfect predictions were brought about.[4] An oracle is nothing but a 'black box' that produces seemingly arbitrary predictions. Scientists want more: in addition they want insight, and therefore they need to open the black box and consider the theory that generates the predictions. Whatever this theory looks like, it should not be merely another black box producing the empirically adequate descriptions and predictions (on pain of infinite regress). In contrast to an oracle, a scientific theory should be intelligible: *we want to be able to grasp how the predictions are generated, and to develop a feeling for the consequences the theory has in concrete situations*. In Section 4, we will develop this observation into a general criterion for scientific understanding.

We conclude that while description and prediction are essential, understanding is an equally indispensable element of the epistemic aims of science. Moreover, we have argued that theories are essential for achieving scientific understanding of nature. The question of how precisely theories can provide understanding will be the central theme of the remaining part of this paper.

3. CURRENT CONCEPTIONS OF EXPLANATORY UNDERSTANDING

Above we have argued – against Hempel and van Fraassen – that understanding is epistemically relevant. Of course we do not want to suggest that we are the first to say so. Nowadays many authors claim that scientific explanations are the means to achieve understanding, and defend a particular model of explanation by appealing to its alleged understanding-providing virtues (e.g., Salmon 1984, 259; 1998, 79ff.; Schurz and Lambert 1994, 109; Cushing 1994, 10; Weber 1996, 1). However, none of them provides an account of what understanding consists in, in order to show *how* it is produced by scientific explanations. Usually, authors merely state that their favourite type of explanation furnishes understanding without any justification. Barnes (1992) rightly criticises unificationists for this reason, but typically fails to provide substantial arguments for the causal view favoured by himself. Our analysis is meant to fill this lacuna by presenting the outline of a general theory of scientific understanding, which intends to give a comprehensive account of the various types of explanation defended in the literature. As a preparation, we will review the currently most influential views of explanatory understanding.

In general, theorists of scientific explanation claim that there exist theoretical virtues that guarantee that theories provide understanding. Usually, they favour particular, allegedly objective standards of intelligibility to which explanatory theories should conform; examples are causality, visualisability, locality, and determinism. Currently, the causal-mechanical conception is very influential, especially as a result of the work of Salmon (1984; 1998); it is discussed in Section 3.1. Its most important rival is the unificationist conception, developed by Friedman (1974) and Kitcher (1989), which asserts that scientific theories provide understanding by unifying other theories and/or phenomena. Unificationism does not impose demands on the form of theories, but sees unifying power as the understanding-providing theoretical virtue. The unificationist conception is examined in Section 3.2 below.

3.1. *The Causal-Mechanical Conception*

According to Wesley Salmon, knowledge of causal relations is crucial for scientific understanding. His 1984 book *Scientific Explanation and the Causal Structure of the World* presents a detailed theory of causal-mechanical explanation, which was later refined and elaborated in discussion with others (see Salmon 1994, 1997, 1998; Dowe 1992, 1995, 2000; Hitchcock 1995). The two key elements of this theory of causality are, first, causal interactions, generating and modifying causal structure, and second, causal processes, by which causal influence is transmitted; see Salmon (1998, Chapters 8 and 16) for summarising accounts of the theory. Salmon's theory is intended to be generally applicable; most importantly, it should be compatible with indeterminism. According to Salmon, we need a causal theory of explanation because "underlying causal mechanisms hold the key to our *understanding* of the world" (Salmon 1984, 260, our italics). This is because "causal processes, causal interactions, and causal laws provide the mechanisms by which the world works; to understand why certain things happen, we need to see how they are produced by these mechanisms" (ibid., 132). In his later work Salmon has put even stronger emphasis on the importance of scientific understanding (see Salmon 1998, 3, 9, 387, and Chapter 5 *passim*).

Thus, Salmon treats causality as a standard for intelligibility: in all cases where a causal theory is available or possible, such a theory provides the best way to generate scientific understanding of phenomena. It should be noted that Salmon does not claim that causal-mechanistic explanation is an *a priori* condition for scientific understanding. Like most present-day philosophers he rejects apriorism and acknowledges the logical possibility that his favourite type of explanation may not be applicable in all situations

(Salmon 1984, 239–240; 1998, 313; cf. Hitchcock 1995, 309). Still, he assigns a superior status to causal-mechanical explanation by claiming that if an explanation of that form is possible, then this is the best explanation. Therefore, his position is not pluralistic: he remains committed to the view that causal analysis is the privileged road to scientific understanding.

It seems to us, however, that even if Salmon's qualifications are taken into account, present-day scientific developments cast severe doubt on the alleged privileged status of his model of causal explanation as the way to scientific understanding: not only are there domains of reality where causal-mechanical explanation fails, but moreover, scientists sometimes do not use it in situations where it is in principle applicable.

At the deepest levels of physical reality Salmon's concept of causality is highly problematic. The main obstacle is the failure of the notion of a causal chain; causal connections of this type, namely continuous space-time trajectories along which energy and momentum are transported, do not exist according to quantum theory in its standard interpretation. The best known example in which quantum physics and Salmon's analysis of causality conflict is the Einstein–Podolsky–Rosen (EPR) situation. A central thesis of Salmon's theory is that every empirical correlation requires a causal explanation, either in terms of a direct causal connection or in terms of a common cause. In the EPR case, however, a direct causal connection is impossible because the two correlated events are too far apart in space and too close in time for a signal to connect them; and the violation of Bell's inequality entails that an explanation of EPR correlations in terms of a common cause is impossible as well.[5] Accordingly, Salmon has problems in applying his model of explanation to present-day fundamental physics, which induces him to add disclaimers such as: "If quantum mechanics requires non-causal mechanisms, they also explain what goes on" (Salmon, 1984, 133; cf. 258, and Salmon 1998, 76, 325). Salmon's dilemma is that he wants to remain faithful to modern science and to quantum mechanics as it is usually interpreted by scientists (e.g., he wants to leave room for indeterminism), while his analysis of causality is not a natural part of modern physics.

That quantum theory presents fundamental difficulties for a causal analysis is well known, but sometimes not considered as decisive because of the interpretational problems surrounding the theory. However, we need not restrict our attention to quantum theory. Physics is full of examples that show that causal-mechanical explanation is not always the actually preferred manner of achieving understanding. Consider, e.g., the way the special relativistic Lorentz contraction is usually understood. This example is particularly interesting because here a causal-mechanistic account *is* possible: such an explanation was given by Lorentz himself in his electron theory, and the same explanation is possible within the framework of special relativity. In this account the changes in the intermolecular forces that occur if a body is set into motion are identified as the mechanism that makes the length contractions understandable. However, this explanation is not the standard one and will not be found in relativity textbooks. The usual way of making the contractions intelligible is by connecting them deductively to the basic postulates of special relativity (the relativity postulate and the light postulate): length contractions must necessarily occur if these postulates both apply to physical reality – this is a matter of logic. Causal reasoning is not involved.

To be sure, there are areas of contemporary science in which causal-mechanistic explanation is prevalent; see e.g., Machamer et al. (2000) on the use of mechanisms in the biological sciences. But, as Berger (1998) shows by means of a case study in population biology, even in biology causal analysis is not always the preferred explanatory strategy. In sum, Salmon's model fails to apply to the most fundamental physical theories, and is in some other cases not used even where it can be applied (N.B.: even quantum theory can be regarded as an example of the latter type, because a causal version of it is available – Bohmian theory – but is not preferred by the general physics community). These facts are sufficient to cast doubt on the core idea that causality has a special status as *the* fundamental, privileged standard of intelligibility.

A proposal for a specific intelligibility standard such as Salmon's always faces the danger of being superseded by science. Historically-minded readers will observe that the preference for causal mechanisms can plausibly be related to the success of this concept within nineteenth-century science.[6] But even in pre-twentieth-century physics causal-mechanical explanation was not always the norm. It is true that Newton's theory of gravitation was criticised because it failed to conform to the Cartesian intelligibility ideal of contact action; its implication of *actio in distans* was unacceptable to most seventeenth-century physicists. But between 1700 and 1850 action-at-a-distance rather than contact action and causal chains dominated the scientific scene (van Lunteren 1991, 126). It was only after 1850, as a result of the success of ether theories, that contact action and causal processes à la Salmon became an acceptable intelligibility standard again (see Section 4.3. for a more detailed account of this example).

We conclude that, in the light of actual scientific practice and historical evidence, causality does not qualify as a universal standard of intelligibility. It is undeniably an important tool for achieving scientific understanding, but not the only one. Indeed, changes in the structure of scientific theories make it difficult to maintain that the same intelligibility standards remain valid throughout the history of scientific thinking. Although it is evident that certain standards play a dominant role in particular episodes of scientific practice, they do not possess a privileged status in an absolute sense: their importance and function can vary in history and across fields or disciplines. Thus, causality is only one 'tool' for achieving scientific understanding, applicable in some situations, but not in others. The idea of 'tools for understanding' will be elaborated in Section 4.

3.2. *The Unificationist Conception*

The most influential rival of the causal-mechanical conception is the unificationist conception of explanatory understanding. It assumes that science provides understanding by presenting a unified picture of the world: a theory that achieves a unification of other theories or of the phenomena provides *ipso facto* scientific understanding. Unificationism was defended by Friedman (1974), and further developed by Kitcher (1981, 1989) and many others (e.g., Schurz and Lambert 1994; Weber 1996; Jones 1997). According to Friedman,

> science increases our understanding of the world by reducing the total number of independent phenomena that we have to accept as ultimate or given. A world with fewer independent phenomena is, other things equal, more comprehensible than one with more. (Friedman 1974, 15)

Friedman's initial attempt to elaborate this intuition into a precise account of explanatory unification proved to be untenable, however (see e.g., Salmon 1990, 94–101). Kitcher thereupon improved the approach and presented an account of unification in which argument patterns are central:

> Understanding the phenomena is not simply a matter of reducing the 'fundamental incomprehensibilities' but of seeing connections, common patterns, in what initially appeared to be different situations. [...] Science advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same patterns of derivation again and again, and, in demonstrating this, it teaches us how to reduce the number of types of facts we have to accept as ultimate (or brute). (Kitcher 1989, 432)

The unificationist conception of understanding has several merits. Most importantly, its applicability is very general, since it does not require that theories are of a specific form. This implies that no theory is in principle incapable of providing understanding. No matter what its specific features are, if a theory turns out to be the maximally unifying systematisation of a particular body of knowledge (i.e., if the set of derivations employs fewer argument patterns than any other systematisation) it provides genuine explanations and understanding. Accordingly, the objection we raised against the causal-mechanical conception does not apply to the unificationist view. For example, unificationism allows for the possibility that quantum mechanics provides understanding, a feature recognised as a major advantage by Friedman (1974, 18) and Salmon (1998, 76).

The quest for unification has indeed been playing an important role in the history of science, for example in the development of Maxwell's theory of electromagnetism (see Morrison 2000, Chapter 3) and in present-day theoretical physics (see e.g., Wayne 1996; Cat 1998). However, it is not clear that pursuit of unification was always motivated by a desire for understanding (see Barnes 1992, 7, and Morrison 2000, 106, for arguments against a historical connection between unification and understanding). Moreover, even if this were the motivation, it would not yet follow that unifying power is the quintessence of scientific understanding. For drawing this conclusion would ignore the historical variation in scientists' intelligibility standards, and the fact that theoretical virtues like causality and visualisability at some points in history, or by some members of the scientific community, have been deemed essential for achieving understanding. It often happens that newly proposed scientific theories are regarded by the contemporary scientific community as supplying no understanding at all, notwithstanding their potential for unification; e.g., Bohr's 1913 atomic theory or Newtonian gravitational theory. The rejection of these theories as 'non-explanatory' was not based on an alleged lack of unifying power but on their lack of other qualities, such as visualisability and continuity.

In response to this objection, unificationists might try to give rational reconstructions of actual history. For example, they might regard Kelvin's remarks on the indispensability of mechanical models for scientific understanding as false consciousness, and argue that Kelvin's belief was ultimately rooted in the unifying power of mechanics and not in any particular understanding-generating property of such models themselves. Kitcher (1989, 436) seems to endorse such an approach when he argues that the concept of causal dependence is a derivative of unification, but that this does not imply that all individuals are aware of this (instead "our everyday causal knowledge is gained by absorbing the lore of our community", and is thus only indirectly linked to unifying power). It seems to us, however, that naturalistic philosophers of science should not distort history and appeal to false consciousness. If scientists explicitly base their judgments about theories on particular criteria, this should be taken seriously *unless* it can be proved that in fact other criteria are at work.

Of course, we do not deny that unification is important in science; we only reject the thesis that unifying power is the universal standard of scientific understanding. Unification appears to be an effective tool for achieving understanding, but like causality it is one among a variety of tools. Interestingly, this conclusion can be drawn also from Kitcher's own analysis of the role of argument patterns. In order to support his claim that unification is essential for scientific understanding, Kitcher (1989, 437–438) presents an argument that resembles our reasoning in Section 2 above. Citing Kuhn, he suggests that "[s]cientific knowledge involves more than knowing the statements" (of a theory). In order to apply the theory successfully to concrete situations, one needs to understand it, and this requires an extra cognitive ingredient. According to Kitcher, this extra ingredient is the set of argument patterns associated with the theory: internalisation of these argument patterns is a prerequisite for producing scientific knowledge. In other words, Kitcher acknowledges the crucial role of skills. He is right, but it is a *non sequitur* to conclude from this that unification (in Kitcher's sense of *reducing* the number of argument patterns) is the only way to produce increasingly more scientific understanding. Obviously unification can be an effective tool to achieve this goal: seeing analogies between theories in the form of similar argument patterns is a way of extending the range of a particular skill. But understanding can equally well be increased by internalising two or more different argument patterns instead of one unified argument pattern. The former scenario may even be preferable, in cases where scientists are better equipped to employ the separate argument patterns than the unified one.

Finally, note that Kitcher's last-mentioned argument is based on the idea that understanding *the theory* is a condition for generating scientific knowledge, that is, for producing scientific understanding *of nature*. In Section 2, our analysis of understanding as an inextricable element of the aims of science led to the same conclusion. We will elaborate this idea in the next section, where we present a general theory of scientific understanding that explains the role of various intelligibility standards, such as causality and unification, in achieving understanding.

4. A CONTEXTUAL THEORY OF SCIENTIFIC UNDERSTANDING

A satisfactory conception of scientific understanding should reflect the actual (contemporary and historical) practice of science. It should therefore allow for variation in standards of understanding. This variation can be accommodated in a natural way if it is acknowledged that scientific understanding is pragmatic and context-dependent. As was argued in Section 2, this conclusion is not inconsistent with the idea of understanding as a universal epistemic aim of science: while achieving understanding is a macro-level aim, there may be contextual meso- and micro-level differences in scientists' views on precisely when understanding is achieved. A look at scientific practice teaches us that the various intelligibility standards endorsed by philosophers (causality, visualisability, etc.) have indeed played a role at various times and in various situations. They are therefore certainly relevant to the analysis of scientific understanding. However, they do not have the status of exclusiveness and immutability that is sometimes ascribed to them: their importance and content depend on the context and are subject to change or development. In the present section we will give a macro-level account of scientific understanding that encompasses, at the meso- and micro-level, context-dependent intelligibility standards.

4.1. *Criteria for Understanding Phenomena and Theories*

In Section 2 we argued that understanding is an essential element of the aims of science, and we concluded that scientific understanding of phenomena requires theories, which in turn have to be intelligible themselves. Intelligibility of theories is needed because scientists have to be able to use theories in order to generate predictions and explanations (recall that intelligibility is a context-dependent feature referring not only to theoretical virtues but also to scientists' skills). Before expanding on intelligibility, let us first state our Criterion for Understanding Phenomena:

CUP: *A phenomenon P can be understood if a theory T of P exists that is intelligible (and meets the usual logical, methodological and empirical requirements).*

Two preliminary remarks should be made. First, CUP implies that it is the general theoretical framework accepted by the scientific community that determines whether a phenomenon is understandable *in principle* (cf. Toulmin 1963; Kuhn 1970). In this sense, the understandability of phenomena is already context-dependent, and determined by meso-level factors. Second, in order to provide *scientific* understanding, any proposed theory must conform to the 'usual logical, methodological and empirical requirements' mentioned in CUP. Accordingly, our criterion does not entail that, for example, astrologers possess scientific understanding of personality traits of their subjects if – as may be the case – they have an intelligible (in a sense shortly to be specified) theory about these personality traits.

The crucial question is now: When is a theory intelligible? We need a general criterion for the intelligibility of theories, which allows for a contextual role of the various intelligibility standards actually employed in scientific practice. In Section 2, when comparing scientific knowledge with predictions of an oracle, we suggested that what one wants in science is the ability to grasp how the predictions are brought about by the theory. This does not imply that proficiency in handling the theory is a *sine qua non*. It is possible to have technical proficiency in manipulating formulas and symbols in the theory, without possessing understanding; having a computer available, or an electronic calculator, does not automatically lead to understanding. Conversely, it is possible to understand how a theory works without being able to make precise calculations with it. Familiarity with the global structure of the theory may be enough. This demand for a global knowledge of the way the theory works is still vague. It can be given more substance by making use of an observation of Heisenberg (1927, 172) about what understanding in physics amounts to in actual practice (Feynman et al. 1965, vol. 2, 2-1, endorses essentially the same view and attributes it to Dirac). We propose a modified version of Heisenberg's formulation as a general Criterion for the Intelligibility of Theories:

CIT: *A scientific theory T is intelligible for scientists (in context C) if they can recognise qualitatively characteristic consequences of T without performing exact calculations*.[7]

First of all, it should be noted that CIT captures the pragmatic and contextual nature of intelligibility (and accordingly of understanding). CIT explicitly refers to the scientists who use the theory, and to the particular context C in which they operate. Whether theory T is intelligible depends not only on the virtues of T itself but also on such contextual factors as the capacities, background knowledge and background beliefs of the scientists in C. Thus, CIT allows for the possibility that a scientific theory considered unintelligible by some (in one scientific community, at one time) will be regarded as intelligible by others (in another community, and/or at another time).

In Section 2, we discussed the importance of *skills* in achieving scientific understanding. CIT provides a more precise interpretation of the nature of these skills. It should be emphasised that although reference to skills introduces a pragmatic, non-objective element, this does not reduce intelligibility to a purely subjective and individual affair. On the contrary, as Kuhn (1970) has argued, skills are acquired within a community and assessed by that community (cf. Rouse 2003). In Section 3, we introduced the idea of *tools*, in order to characterise the role of causality and unification in achieving understanding. The availability and acceptability of such conceptual tools is context-dependent, and typically determined at the meso-level as well. With the proposed criteria CUP and CIT we can give a more specific account of the role of skills and tools, and particularly their combination, in the generation of scientific understanding. The remainder of this paper is devoted to developing and defending that account.

Let us illustrate the proposed criteria CUP and CIT by applying them to a concrete case: the explanation of Boyle's law by the kinetic theory of gases. This example is generally regarded as a paradigm case of an explanation that increases our understanding, and has often been invoked to promote various, allegedly universally valid, intelligibility standards; e.g., causality (Salmon 1984, 227; Barnes 1992, 8), visualisability (Cushing 1994, 14), and unifying power (Friedman 1974, 14–15). We claim, however, that by means of CUP and CIT we can give a better account of how gaseous phenomena, like those described by Boyle's law, can be understood with the kinetic theory.

Consider the way Ludwig Boltzmann introduced the kinetic theory in his *Lectures on Gas Theory*. Boltzmann (1964) devotes the introductory section to a purely qualitative analysis that leads to the conclusion that a gas can be pictured as a collection of freely moving molecules in a container. In a quite straightforward way, this molecular-kinetic picture can give us a qualitative feeling for the behaviour of macroscopic properties of gases. First of all, heat can be identified with molecular motion and it follows that an increase of temperature corresponds with an increase of the (average) kinetic energy of the gas molecules (Boltzmann 1964, 28–30; cf. Feynman et al. 1965, vol. 1: 1–2ff.). Moreover, the picture immediately gives us a qualitative explanation of the fact that a gas exerts pressure on the walls of its container. If a gas molecule collides with a wall of the container, it gives it a little push. The total effect of the pushing of the molecules produces the pressure. In more formal terms: molecules exert forces on the wall and the total force of all molecules on a unit area equals the macroscopic pressure.

In this way we obtain qualitative understanding of the relations between temperature, pressure and volume of a gas. If one adds heat to a gas in a container of constant volume, the average kinetic energy of the moving molecules – and thereby the temperature – will increase. The velocities of the molecules therefore increase and they will hit the walls of the container more often and with greater force. The pressure of the gas will increase. In a similar manner, we can infer that, if temperature remains constant, a decrease of volume results in an increase of pressure. Together these conclusions lead to a qualitative expression of Boyle's ideal gas law. It is important to note that the above reasoning does not involve any calculations. It is based on general characteristics of the theoretical description of the gas. Its purpose is to give us *understanding* of the phenomena, *before* we embark on detailed calculations. Such calculations are subsequently motivated, and given direction, through the understanding we already possess. We stress that our approach does not intend to downplay the value of detailed calculation; we do not want to replace exact calculation by intuitive understanding. Our proposal should not be read as a campaign for a more qualitative approach in the sciences, or for the superiority of *Verstehen* over *Erklären*. Exact mathematical techniques and logical rigour in argumentation are obviously essential in modern science. What we emphasise is the importance of understanding as an *additional* epistemic aim of science.

The example of the kinetic theory illustrates how scientific understanding of a phenomenon is based on the ability to recognise qualitative consequences of the theory without performing exact calculations (criterion CIT). In this case, the general picture of moving gas particles allows us to make qualitative predictions of macroscopic properties of gases in particular situations. Even without endowing atoms with specific properties (which has to be done to construct a theory with which precise calculations can be made), the kinetic hypothesis provides a tool that can give us understanding of the phenomena.[8]

One might ask why this account is superior to a causal-mechanical account, or even whether there really is a difference. After all, the story just told does employ causal reasoning. The answer is that our account *explains how* in this case understanding can be achieved by means of causal arguments; it does more than merely *affirming that* causality supplies understanding. Causal reasoning is used to achieve the goal of qualitative prediction; it is an instrument that helps us to get a feeling for the situation. Newton-Smith's question, cited in Section 1, is therefore answered: we have made it clear what it is that makes the kinetic explanation explanatory. The example shows that causality functions as a tool for achieving understanding. But this does not imply that it is a necessary condition: the possibility to gain understanding by other means is left open.

As a second example, consider meteorology, a science concerned with highly complex systems. Weather predictions are obtained by means of computer calculations in which the Navier–Stokes equations are solved for very large systems, using many auxiliary theories to incorporate small-scale effects. If meteorologists merely were occupied with making correct predictions in this manner, they would fail to understand the weather. But this is not the case: meteorologists are concerned not only with 'brute force' computer calculations but also with formulating intelligible meteorological theories and models. An example is 'PV-thinking', based on Ertel's equation of potential vorticity (see Ambaum 1997, 3–10). The goal of PV-thinking is to provide qualitative understanding, in the sense of facilitating the recognition of qualitative consequences of applying the Navier–Stokes equations to the atmosphere by means of a relatively simple picture. For example, in 1951 Kleinschmidt applied PV-thinking in order to gain a qualitative understanding of the behaviour of cyclones, adding that a representation of cyclones in terms of potential vorticity should be regarded as merely "an illustrative picture" [*anschauliches Bild*] (quoted in Ambaum 1997, 4). Even though the PV-picture cannot be interpreted realistically, it is a useful tool for understanding cyclones (and atmospheric evolution in general). Therefore, there is more to meteorology than brute force calculation: meteorologists also aim at scientific understanding in the sense of criteria CUP and CIT.

We want to emphasise that on the proposed conception of understanding, reduction and understanding are not essentially related. As the first example shows, a reductive theory may provide understanding but does not do so *by virtue of* the reduction. The fact that kinetic theory helps us to understand the behaviour of gases and that thermodynamical laws can be reduced to kinetic theory, are two different achievements. Indeed, one can understand the thermal phenomena in question also without reduction, namely by means of thermodynamics itself (see e.g., Feynman et al. 1965, vol. 1: 44-1ff). The second example illustrates this even more clearly: meteorology, as a branch of applied physics, should in principle be reducible to elementary physics, but it is not this reduction that guarantees understanding. On the contrary, in this case it is primarily the non-reductive approaches, such as Ertel's theory, that provide understanding.[9]

4.2. *Conceptual Toolkits*

CIT can accommodate the variety of ways in which understanding is achieved in scientific practice. Qualitative insight in the consequences of a theory can be gained in many ways. Above, we have suggested that an appropriate combination of skills and tools is essential, where both skills and tools are contextually determined. So far we have given examples of tools, but we have not yet presented a general account of how precisely they function in the attainment of scientific understanding. First, note that in order to recognise consequences of a theory intuitively, and to be able to argue about them, a conceptual framework is required in terms of which one can argue qualitatively. There is an instructive analogy with problem-solving methods, in which heuristic strategies often play an important role. Heuristics can guide the search for solutions in a variety of ways, not only "by drastically restricting the number of possible roads" but also "by positively suggesting which general directions to take in the searching process" (Radder 1991, 196). Analogous to problem-solving heuristics, the 'intuitive' recognition of theoretical consequences requires conceptual tools. These should make it possible to circumvent the calculating stage and make the intuitive jump to the conclusion. As it turns out, scientists have often advanced explicit ideas about the conceptual tools that can be used to obtain such insight in the consequences of scientific theories.

An important example is visualisation, which has been regarded by many a scientist as an almost indispensable help in doing science. Well-known physicists such as Faraday, Maxwell, Schrödinger, and Feynman have emphasised the essential role played by visualisation in scientific practice (see De Regt 1996a, 1997, 2001). Indeed, Feynman's attitude shines from almost every page of his famous lectures on physics (Feynman et al. 1965). A simple illustration is the use of 'field lines' in electrostatics (ibid., vol. 2, 4–11). Although intuitive application of this concept is possible only in simple situations, it is quite useful to get a feeling of how electrostatic systems behave. And this, according to Feynman et al. (1965, vol. 2, 2-1), is precisely what it means to have physical understanding of the situation in question: "if we have a way of knowing what should happen in given circumstances without actually solving the equations, then we 'understand' the equations, as applied to these circumstances". In the cases analysed in Section 4.1 visualisation appeared to be an important tool as well.

Even in the most abstract areas of contemporary science visualisation is still used as a tool for achieving understanding. Consider the way in which the so-called MIT bag model is employed for dealing with quark confinement (Hartmann 1999). The model was put forward in the context of quantum chromodynamics (QCD), the fundamental theory describing hadron structure. The problem with QCD is that reliable tests are possible only by means of 'black box' numerical techniques (so-called lattice QCD), which do not immediately give us insight. In order to obtain such insight, hadron physicists use models, of which the MIT bag model is an example. This model was proposed in order to achieve understanding of quark confinement: quarks supposedly exist only in pairs or triplets, confined to a very small spatial volume. The existence of quark confinement is empirically supported by the fact that no single quarks have been observed, and moreover, it seems (but has not rigorously been proven) to be a consequence of QCD.

The MIT bag model describes hadrons as 'bags' in which (two or three) quarks are spatially confined, forced by external pressure (similar to nucleons in the nuclear shell model). With the help of boundary conditions and suitable approximations, the single model parameter (bag pressure) can be adjusted to fit hadronic observables (e.g., mass and charge). Hartmann (1999, 336) observes that the predictions of the model only modestly agree with empirical data, and asks: Why do physicists entertain the model, despite its shortcomings? His answer is that this and similar models provide "plausible stories". After emphasising the importance of qualitative stories, Hartmann (1999, 344) addresses the question of how their quality is to be assessed. In addition to conforming to various straightforward criteria, the story should provide understanding, but Hartmann concludes that it is "very difficult to explicate how a model and its story exactly provide understanding".

Our theory of understanding supplies the explication that Hartmann is looking for. The MIT bag model, and the story that goes with it, provides scientific understanding by allowing us to make qualitative predictions about hadron structure without carrying out calculations by means of lattice-QCD. The visualisable MIT bag model is a *tool* for hadron theorists, in the sense discussed above. Accordingly, our analysis substantiates the observation of particle physicist T. Cohen (quoted by Hartmann 1999, 329): "models of the hadrons are essential in developing intuition into how QCD functions in the low energy domain". Interestingly, in this case the relation between visualisable models and the theory at issue (QCD) is much looser than in the example of the kinetic theory of gases. Therefore, the models function primarily as tools for understanding, rather than as exact physical interpretations of the mathematical theory.

These examples show that visualisation is an important tool. However, it would be erroneous to maintain that visualisation is essential for obtaining understanding. Other conceptual devices can be used as well to facilitate qualitative reasoning. For example, many theoretical physicists have developed a familiarity with, and intuition for, the general behaviour of the solutions of the mathematical equations they use. This enables them to acquire a feeling for the qualitative behaviour of the described systems *without* invoking picturable physical mechanisms. For instance, it is possible to get an intuitive feeling for how quantum-mechanical systems in two-slit-like situations behave, by familiarity with the linear character of the Schrödinger equation. The claim such physicists often make, namely that they really understand the theory they are working with and the phenomena described by it, is on our analysis perfectly legitimate. Our approach implies that such different tools as visualisable physical mechanisms on the one hand and abstract reasoning on the other, can fit in with the same central aim of obtaining understanding. The various intelligibility standards proposed by philosophers of science (e.g., visualisability, causality, and continuity) find a place in our approach as 'tools' for achieving understanding: they can help to 'see intuitively' the consequences of a scientific theory.

Another instructive example is the deflection of light by matter predicted by general relativity. There are at least three well-known ways of making this phenomenon understandable. All strategies use general relativity, but employ different conceptual tools for bringing out the consequences of the theory. The first approach focuses on the variation of the velocity of light in a gravitational field; this is a consequence of general relativity that can be made understood intuitively by the use of the equivalence principle. The deflection of light can subsequently be made intelligible by means of an analogy with the propagation of light in a medium with variable refractive index. No specific causal mechanisms are invoked (the light signal itself can be considered a causal chain, but we are talking about the explanation of the *deflection*, and no mechanism in the ordinary sense is specified for that). The second approach is more abstract. It does not mention the variation in the velocity of light, but invokes the non-Euclidean character of the spatial geometry to explain the non-Newtonian curvature of the light rays (see Eddington 1923, Chapter 6, for a clear exposition of these different ways of understanding the deflection of light). Finally, one can describe the light rays as null-geodesics in the four-dimensional metrical field. This can be done in an abstract mathematical way, but the procedure is often made visualisable via analogies with two-dimensional curved surfaces. It is noteworthy that these different ways of obtaining a feeling for how light propagates according to general relativity can be used next to each other. Depending on the kind of problem to which one wishes to apply the analysis, on the calculational techniques one favours, or on other preferences, one or the other can be chosen.

The just-mentioned example undermines the causal conception of understanding, because no causal chains were identified that are responsible for the deflection of the light. However, a causal way of obtaining understanding is also available. Popular expositions of general relativity, but also informal arguments among scientists, often make use of conceptual tools that have proved their understanding-providing value in Newtonian gravitational theory and are therefore 'imported' into relativity theory. This leads to accounts according to which masses pull, and 'attract' light. It is difficult to give this notion of an attractive gravitational force a definite meaning within the formal framework of general relativity, and a realistic interpretation therefore appears problematic. However, the Newtonian causal mechanisms can still fulfil the role of conceptual tools providing understanding.

There are many more examples in which the causal-mechanistic worldview makes itself felt in the terminology used in obtaining understanding, but has lost most of its substance and cannot be taken literally. Many 'mechanisms' in physics are no mechanisms in the ordinary sense of the word at all. An interesting case is the 'mechanism' adduced in quantum field theory to explain how particles acquire mass (the so-called Higgs mechanism). This mechanism consists in the introduction into the Hamiltonian of an additional field, with a potential that has a ground state energy that is less than zero. Mathematical considerations involving spontaneous symmetry breaking, and the introduction of new fields which are shifted with respect to the old ones, lead to a rewritten Hamiltonian that can be interpreted as describing massive particles. The whole reasoning is based on the form of the equations and analogies, and certainly no classical causal mechanism, transporting mass to particles that were massless before, is involved. Moreover, the quantum fields do not correspond to particles in the classical sense and that the same problems with the notion of a causal chain arise as in the EPR-case. The Higgs mechanism might be considered an example of the "noncausal mechanisms" that Salmon (1984, 133) alludes to. But it is unclear how such a 'mechanism' would fit into Salmon's theory of explanation. By contrast, our approach has no problem in accounting for the explanatory power of this kind of argument.[10]

The examples given above support our theory of scientific understanding, based on criterion CIT, and they show that the contextuality of understanding is essentially related to the question of which tools are available and deemed suitable. There is no universal tool for understanding, but a variety of 'toolkits', containing particular tools for particular situations. Which tools scientists have at their disposal, depends on the (historical, social, and/or disciplinary) context in which they find themselves. This context-dependence is typically of a meso-level nature, i.e., it is the scientific community that determines what tools are available and which skills are required to achieve scientific understanding.

An interesting implication of CIT, and the associated 'conceptual toolkit thesis', is that theories may become intelligible when we get accustomed to them and develop the ability to use them in an intuitive way. In this sense, the present view rehabilitates an intuition that has often been discredited: the idea that understanding is related to familiarity. Friedman (1974, 9), for example, deems the view that "scientific explanations give us understanding of the world by relating (or reducing) unfamiliar phenomena to familiar ones" – the so-called 'familiarity view' of scientific understanding – "rather obviously inadequate". Indeed, there is a well-known argument against it: science often explains phenomena that are directly known and quite familiar to us by means of theories and concepts of a strange and unfamiliar (sometimes even counter-intuitive) character; see Toulmin (1961, 60–61) and Hempel (1965, 256–258) for similar criticisms. Nonetheless, the intuition behind the familiarity view should not, and need not, be relinquished completely. The idea of conceptual toolkits, associated with CIT, implies that only if tools are familiar to their users, can they be used for their purpose, namely making predictions without entering into explicit calculations. However, this familiarity does not necessarily involve the idea that the user should be acquainted with the concepts in question from everyday experience. One can get accustomed to the mathematical concepts used in a theory or to an initially weird physical interpretation of a theory, to such an extent that it becomes possible to reason in the associated terms in an intuitive manner. In this way, CIT incorporates the intuition on which the familiarity view is based; but it does not lead to the consequence that we should understand phenomena in terms of things which are more familiar from everyday experience than the phenomena themselves. CIT therefore gives a more sophisticated account of the familiarity aspects of understanding than the standard familiarity view and is not vulnerable to the objections that demonstrate the untenability of that view.

4.3. *The Dynamics of Understanding: Contextual Variation of Tools*

Can general conclusions be drawn about the contextual variation of tools, and the factors that influence it? Is a general model of the dynamics of understanding possible? This question is akin to the one discussed by James McAllister in his book *Beauty and Revolution in Science* (1996). McAllister analyses the role of aesthetic factors in theory evaluation, citing visualisability and symmetry as examples. He argues that scientists' aesthetic preferences are formed and updated by the 'aesthetic induction': "A community compiles its aesthetic canon at a certain date by attaching to each property a weighting proportional to the degree of empirical adequacy then attributed to the set of current and recent theories that have exhibited that property" (McAllister 1996, 78). Although standards of intelligibility are not simply examples of aesthetic criteria (see De Regt 1998), McAllister's theory may also shed light on the development of the former. Conceptual toolkits are partly overlapping with aesthetic canons and may be formed in a similar inductive manner. Moreover, like aesthetic canons, conceptual toolkits are first and foremost established at the intersubjective, meso-level of scientific communities; they transcend the individual level. Extending McAllister's thesis, we believe that not only empirical success of theories plays a role in the inductive process but also the earlier understanding-providing success of the tools. For example, if visualisation has successfully functioned as a tool for rendering earlier theories intelligible (in the sense of CIT), then visualisability will be a prominent part of the present conceptual toolkit for achieving scientific understanding.

As an example, consider the case of Newtonian gravitational theory, which was already cited in Section 3.1. It is well-known that Newton's contemporaries Huygens and Leibniz expressed grave doubts about the intelligibility of this theory because of its implications of action-at-a-distance. And also Newton himself was unhappy with his own theory in this respect. In 1693, in a famous letter to Bentley, he stated: "It is inconceivable that inanimate brute matter should, without the mediation of something else which is not material, operate upon and affect other matter without mutual contact"; and he added that this idea is "so great an absurdity that I believe no man who has in philosophical matters any competent faculty of thinking can ever fall into it" (Newton 1961, 253–254). Apparently, Newton had difficulty with the metaphysics now associated with Newtonian theory: there was no room for *actio in distans* in the corpuscularist worldview to which he adhered. For that reason he deemed it "inconceivable", and did not accept it as a tool for scientific understanding.

But *actio in distans* has not always been regarded as incomprehensible, as a historical study by van Lunteren (1991) shows. On the contrary, opinions oscillated: after a time when it was considered unintelligible, the period between 1700 and 1850 was dominated by proponents of action-at-a-distance theories, among whom were Kant, Boscovich, Laplace, Helmholtz, and Weber. At the end of the eighteenth century, the tide had turned completely in favour of *actio in distans*, according to van Lunteren (1991, 126): "The former truism 'nothing can act where it is not' was changed for the canon that 'a thing can only act where it is not' ". In 1847, in his famous *Über die Erhaltung der Kraft*, Helmholtz explicitly stated that action-at-a-distance is a necessary condition for intelligibility:

> The task of physical science is finally to reduce all natural phenomena to unchanging forces of attraction and repulsion, the intensity of which is dependent upon distance. The possibility of solving this problem is a prerequisite for the complete conceivability of nature (Helmholtz 1983, 17, our translation; cf. van Lunteren 1991, 142).

In this period, attempts to formulate theories of gravitation based on contact action (e.g., by Euler, Le Sage, Young, Ampère, and Herapath) were often ignored by the scientific community. Only in the second half of the nineteenth century contact action became an acceptable explanatory resource again, as a result of the empirical successes of ether theories.

The controversy about the intelligibility of Newton's theory of gravity hinged on the acceptability of action-at-a-distance as a tool for understanding. Seventeenth- and eighteenth-century physicists who objected to Newton's theory endorsed a corpuscularist ontology that implied contact action (cf. McAllister 1996, 56–58). This preferred metaphysics was a 'canonisation' of the tools that previously had contributed to the achievement of scientific understanding. Thus Huygens rejected action-at-a-distance as unintelligible. Huygens's adherence to corpuscularism was rooted in the fact that he had learned to understand the natural world scientifically (i.e., according to CIT) by means of corpuscularist principles and models. The same applied to Newton himself. The generally accepted tool of contact action – canonised in corpuscularist metaphysics – did not enable him to understand his own theory. Scientific understanding of it was still possible, though, in a different, more mathematically oriented way (cf. Gingras 2001, 398–399, who argues that Newton's *Principia* induced a change in the meaning of 'explanation': from mechanical explanation to mathematical explanation). But just like Huygens, Newton held fast to the previously successful tool of contact action so strongly that he did not accept the possibility of achieving scientific understanding of the phenomena by means of the *actio in distans* instrument.

We conclude that there is contextual variation in the availability and acceptability of tools for understanding. Metaphysics is one element in that context: it may supply or prohibit particular tools, as the example shows: the tool of *actio in distans* was available but not acceptable at first. However, as Newtonian theory turned out to be very successful, scientists abandoned their metaphysical scruples and accepted action-at-a-distance as a tool for scientific understanding. Around 1800 action-at-a-distance had replaced contact action as the preferred tool. But the tide turned once again in the nineteenth century. Since the idea of contact action proved helpful for qualitative reasoning in the theory of electromagnetism (think e.g., of Maxwell's mechanical model of the ether), it became a prominent element of the nineteenth-century conceptual toolkit.

Incidentally, rejecting a scientific theory as unintelligible in the way Newton and Huygens did is not necessarily unproductive. As McAllister (1996, 81–85) explains for the related case of aesthetic canons, such an attitude can be a useful form of conservatism. However, holding on to metaphysics or aesthetics too strongly amounts to confusing the means with the end: a metaphysical worldview may be useful as a source of tools for producing scientific understanding and of heuristic strategies, but achieving harmony between scientific theories and pre-existing worldviews is definitely not an epistemic aim of science.

Today, the situation in quantum mechanics furnishes another illustration of the relations between worldpictures on the one hand and understanding scientific theories on the other. There are many ways of interpreting the quantum-mechanical formalism; e.g., the Copenhagen interpretation, the many-worlds interpretation, the Bohm interpretation, modal interpretations. Each interpretation provides its adherents with a set of conceptual tools with which it is possible to render the theory intelligible. These conceptual expedients offered by the various interpretations differ very much. The Bohmian scheme comes closest to the kind of picture we know from classical physics, with particle trajectories and fields guiding the particles. The other interpretations operate with notions which are much less amenable to visualisation. The fact that there is not one generally accepted interpretation of quantum mechanics and that physicists belonging to different schools claim to understand the theory by means of their own conceptual framework (and often accuse other approaches of unintelligibility), illustrates once again the contextuality of scientific understanding and the fact that understanding is not bound up with fixed explanatory categories.[11]

4.4. *Barometers, Flagpoles, and the Contextuality of Intuitions*

While our contextual approach is grounded in empirical (historical) data, most philosophers justify their conception of scientific understanding by referring to intuitions. Barnes (1992, 8), for example, after having reproached Friedman and Kitcher for not supplying any argument for the understanding-providing virtues of unification, defends the causal conception by arguing that it rests on a "sound intuition". And Cushing (1994, 11) defends visualisability as a criterion for understanding by referring to "the intuition ... that understanding of physical processes involves a story that can, in principle, be told on an event-by-event basis". More generally, it appears that existing theories of explanation all rest upon particular intuitions about what a good (read: understanding-providing) explanation is. These intuitions are employed as 'basic facts': for example, the problem of asymmetry involves the intuition that the length of a flagpole can explain that of its shadow, and not the other way around. Our contextual analysis of scientific understanding does not intend to deny the value of these intuitions. But we refuse to accept them as rigid directives given once and for all. Intuitions depend partly on the historical and scientific context, and may therefore be subject to change or development.

By acknowledging the contextuality of intuitions, our approach does not fall prey to the kind of criticism that has proved effective against the deductive-nomological (D-N) model of scientific explanation. At first sight, it might be thought that all familiar counterexamples to the D-N model (deduction of causes from effects, deduction of a law from the conjunction of itself and another irrelevant law, causally irrelevant deductions, etc.; see Salmon 1990, 46–50, for an overview) are also counterexamples to our proposed account. Indeed, it would seem that we could just add the stipulation that a scientist arrived at the results of the deductions intuitively (i.e., without detailed calculations) without weakening the strength of the counterexamples. Let us discuss some examples to see how our account of scientific understanding evades this potential criticism.

In the well-known barometer case the standard objection is that the mere deduction of the future occurrence of a storm from the reading of a barometer does not yield an explanation of the storm and does not make its occurrence understandable. Instead, a common cause is responsible and provides the explanation, namely the very low air pressure at present. First, note that the prediction of an upcoming storm from present barometer readings does not satisfy our criterion for scientific understanding, because it does not refer to any scientific theory. Actually, there is no theory about relations between barometer readings, as theoretical quantities, and weather conditions: according to the meteorological theory that actually connects barometer readings to weather conditions, barometers are just indicators of air pressure. On our criterion CIT, this theory is intelligible if one is able to foresee qualitatively that, for example, the future occurrence of a storm can be the consequence of a very low air pressure obtaining now. A meteorologist able to make a global deduction *on the basis of meteorological theory*, and using the data provided by the barometer, will possess scientific understanding.

But suppose, to get closer to the heart of the matter, that situations exists in which there are correlations between different events (analogously to those between barometer readings and weather conditions) but in which no theoretical explanation in terms of a common cause is available. In that case one can deduce and predict the occurrence of an event from the occurrence of its correlate, but can this lead to scientific understanding? On our analysis, this is perfectly well possible. We have merely required that the correlations are embedded in a scientific theory; this theory does not have to be causal (so the correlations do not necessarily have to be explained in terms of either direct causal connection or a common cause). The theory may equally well contain the correlations as scientifically basic, and not as substitutes for something else (as in the barometer case). Such a theory is not *per se* unintelligible, and scientists who understand it (according to CIT) can acquire scientific understanding of the phenomena in its domain. Perhaps these scientists would proceed in a rather abstract mathematical way, having developed an intuitive feeling for the equations describing the correlations. In fact, this is the case for quantum physicists who have developed intuitions about the outcomes of present-day experiments with so-called entangled states, in which case causal mechanistic reasoning fails and the equations are the main guide.

So our response to the barometer case is twofold. First, this example itself provides no real challenge, because it does not deal with actual scientific theories and scientific understanding. Second, in situations in which correlations between events *do* figure in basic scientific theory, it is not clear why deductions using these correlations could not lead to scientific understanding. The EPR correlations (in which the entangled states that we just mentioned play an essential role) constitute a case in point. The outcome at one wing of the experiment can be understood on the basis of the outcome at the other side (plus quantum theory), even though there is no causal contact between these events. In this case our causal intuitions fail as tools for understanding and the context requires that we develop alternative mathematical intuitions.

What about other putative counterexamples? Cases of irrelevant deductions (the hexed salt example, or Salmon's case of men consuming birth-control pills) seem to fall in the same category as the barometer problem. There is no scientific theory correlating these (redundant) conditions to the effect in question. If such a theory *did* exist, it would not be evident that the correlations could not provide understanding. Putative counterexamples in which laws or theories are trivially derived from themselves do not hit the mark either, since our analysis pertains to the understanding of natural phenomena on the basis of theories, and not to the derivation of theories themselves.

The asymmetry problem, exemplified by the flagpole case, is more interesting. In line with what we have argued above, we hold that it depends on the context whether the length of the flagpole makes it understandable how long the shadow is, or vice versa. In this respect we agree with van Fraassen (1980, 130ff.). The common sense intuition of asymmetry is based upon a preference for everyday causal reasoning; and in many scientific contexts (e.g., classical optics) such causal reasoning is useful as well. But, again, it would be unjustified to attribute a context-independent privileged epistemological role to it. As we have seen many times now, causal explanations do not possess a unique position in modern science, even if we disregard the notorious case of quantum mechanical EPR-correlations (see also Ruben 1993, 10 and Berger 1998 for arguments concerning this point). If non-causal tools of the kinds we have discussed are admitted, there is no reason to hold on to the idea that there is a fixed direction in all understanding-providing arguments. Moreover, even if preference is given to causal reasoning, physical science does not automatically fit in with everyday causal thinking and does not always single out earlier events as the causes of what happens later. A famous example in which causal chains can be read in both time directions is furnished by electromagnetic theory in the version of Wheeler and Feynman (Wheeler and Feynman 1945; see Panofsky and Phillips 1969, 394–398, for an introduction). In this time-symmetric theory of electrodynamics the force on a charged particle is considered to be caused both by antecedent conditions and by circumstances in the future. Actually, a similar interpretation can already be given within the context of ordinary electrodynamics: the electromagnetic field at one instant can both be seen as coming from the earlier motions of charged particles and as originating from later motions of these particles (so-called 'retarded' and 'advanced' solutions of the equations). That such ideas have been received as serious proposals by itself already shows that there are no *a priori* absolute obstacles to giving scientific causal explanations in a direction we are not accustomed to consider in everyday life. In our view, this everyday preference is a contextual matter.

5. CONCLUSION

Achieving understanding is a generally acknowledged aim of science. In this paper, we have presented an analysis of the nature of scientific understanding that takes account of scientific practice and accommodates the historical diversity of conceptions of understanding. Its core idea is a criterion for the intelligibility of scientific theories (CIT, see Section 4.1) that incorporates the pragmatic and contextual features of understanding. While accounting for the contextual variation of intelligibility standards observed in scientific practice, our approach retains a general, non-trivial specification of what it means to possess scientific understanding of a phenomenon. This is possible by distinguishing between different levels at which science can be analysed. At the macro-level achieving understanding is a universal aim of science, but at the meso-level, and sometimes even at the micro-level, one observes variation in the specification of how to achieve scientific understanding: scientists in different periods of history or in different communities often endorse quite different intelligibility standards.

We have criticised current philosophical theories of explanation for assigning an unjustified privileged status to one particular standard of intelligibility (e.g., causality, visualisability, unifying power). We have argued, by contrast, that such intelligibility standards function as 'tools' for achieving understanding in particular contexts: they help the scientist to see intuitively the consequences of a scientific theory (leading to the fulfilment of CIT). By depriving these standards of their universal status and by giving them a contextual role instead, our approach leads to an overarching theory that explains "what it was about each of these apparently diverse forms of explanation that makes them explanatory" (Newton-Smith 2000, 131–132).

It has not been our intention to deny that causal-mechanical pictures play an important role in achieving scientific understanding. Quite often they do, as the examples discussed in Section 4 illustrate. However, their role is fundamentally different from what is usually asserted: causal-mechanical reasoning and visualisation should not be regarded as necessary conditions for understanding, but as (contingent) tools that can be useful in particular contexts. Our overarching analysis of scientific understanding remains applicable even when causal or visualisable interpretations of a theory have become problematic, like in modern physics.

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NOTES

1 Actually, variation in philosophical heuristics and in scientific aims are often closely related. For example, the controversy between Maxwell and Boltzmann about the specific heat anomaly (see De Regt 1996a) can be analysed either in terms of conflicting philosophical heuristics or in terms of differing aims. Although from a macro-level point of view both physicists had a common aim, namely solving the anomaly, on a micro-level their aims were different because they had different philosophical criteria for what would count as a good solution (see De Regt 1996b, 146).

2 Although Hempel denies that 'understanding' is a philosophically relevant notion, he does have something to say about the way in which science may provide understanding. Hempel (1965, 337): "the [deductive-nomological] argument shows that, given the particular circumstances and the laws in question, the occurrence of the phenomenon was to

be expected; and it is in this sense that the explanation enables us to understand why the phenomenon occurred". In this way 'understanding' is reduced to 'rational expectation'. However, well-known examples, like the barometer prediction of a storm, show that rational expectation is insufficient for understanding: we do not understand the occurrence of a storm by merely referring to the barometer's indication. See Section 4.4 for our analysis of this example.

[3] Of course one might claim that also explanation itself is pragmatic in this sense and that one can only say that theory T explains P for scientist S (actually this is van Fraassen's view, see below). However, this deviates from the standard use of the term 'explanation'.

[4] The historiography of science supports this idea. Toulmin (1963, 27–30), for example, stresses the generally acknowledged point that Ionian astronomers were as scientific as their Babylonian contemporaries. The latter "acquired great *forecasting-power*, but they conspicuously lacked *understanding*". By contrast, while its predictive power was meagre, Ionian philosophy of nature is traditionally considered as the beginning of natural science.

[5] See, e.g., van Fraassen (1991, 81–98). It might be thought that EPR-correlations can be explained by means of common cause analysis in terms of an interactive fork (instead of the conjunctive fork); see Salmon (1984, 168–172). However, the problem is that the particle trajectories do not count as causal processes at all, according to Salmon's criteria. So retreating to the (weaker) interactive fork does not help in the EPR situation. Recently, Hofer-Szabó et al. (1999) have argued that on a more general interpretation of the common cause principle the Bell inequalities do not rule out the existence of common causes in an EPR situation. However, Hofer-Szabó et al. abandon a crucial assumption of standard common cause explanation (viz., that there should be a single common cause for all correlated pairs).

[6] Even though Salmon himself is at pains to avoid this impression. Cf. his plea for "causal-mechanical understanding (but not the nineteenth-century English version satirized by Duhem)" (Salmon 1998, 87).

[7] If one wants to apply our analysis to non-mathematical, qualitative theories, we suggest to replace 'exact calculation' by 'complete logical argumentation'. Intelligibility of such theories then implies the ability to recognise consequences without following all the steps the 'formalism' of the theory requires.

[8] In De Regt (1999), this example is discussed in more detail, particularly in relation to Boltzmann's *Bildtheorie* of scientific theories.

[9] See Dieks and De Regt (1998) for more arguments against the reductionist view of scientific understanding.

[10] Interestingly, elementary or popular expositions of the Higgs mechanism do sometimes employ causal analogies, in a way comparable to the use of causal metaphors in popular treatments of light deflection discussed above; see for example: http://atlas.web.cern.ch/Atlas/documentation/EDUC/physics10.html

[11] The defended conception of scientific understanding is sufficiently broad to allow for the possibility that a theory is understood without a realistic interpretation. Indeed, such instances frequently occur in the history of science; the MIT bag model furnishes an example, and understanding quantum mechanics via the Copenhagen interpretation is arguably another example. A realistic interpretation of a scientific theory may facilitate understanding; but it is not indispensable. Therefore, the fact that scientific understanding is an aim of science does not entail scientific realism.

REFERENCES

Ambaum, M. H. P: 1997, *Large-Scale Dynamics of the Tropopause*, unpublished Ph.D. dissertation, Technical University Eindhoven.

Barnes, Eric: 1992, 'Explanatory Unification and Scientific Understanding', in D. Hull, M. Forbes and K. Okruhlik (eds.), *PSA 1992*, Vol. 1, Philosophy of Science Association, East Lansing, pp. 3–12.

Berger, Ruth: 1998, 'Understanding Science: Why Causes are not Enough', *Philosophy of Science* **65**, 306–332.

Boltzmann, Ludwig: 1964, *Lectures on Gas Theory*, University of California Press, Berkeley.

Cartwright, Nancy: 1983, *How the Laws of Physics Lie*, Clarendon Press, Oxford.

Cat, Jordi: 1998, 'The Physicists' Debates on Unification in Physics at the End of the 20th Century', *Historical Studies in the Physical and Biological Sciences* **28**, 253–299.

Cushing, James T.: 1994, *Quantum Mechanics. Historical Contingency and the Copenhagen Hegemony*, The University of Chicago Press, Chicago.

De Regt, Henk W.: 1996a, 'Philosophy and the Kinetic Theory of Gases', *The British Journal for the Philosophy of Science* **47**, 31–62.

De Regt, Henk W.: 1996b, 'Are Physicists' Philosophies Irrelevant Idiosyncrasies?', *Philosophica* **58/2**, 125–151.

De Regt, Henk W.: 1997, 'Erwin Schrödinger, *Anschaulichkeit*, and Quantum Theory', *Studies in History and Philosophy of Modern Physics* **28B**, 461–481.

De Regt, Henk W.: 1998, 'Explaining the Splendour of Science', *Studies in History and Philosophy of Science* **29**, 155–165.

De Regt, Henk W.: 1999, 'Ludwig Boltzmann's *Bildtheorie* and Scientific Understanding', *Synthese* **119**, 113–134.

De Regt, Henk W.: 2001, 'Space-Time Visualisation and the Intelligibility of Physical Theories', *Studies in History and Philosophy of Modern Physics* **32B**, 243–265.

De Regt, Henk W.: 2004, 'Discussion Note: Making Sense of Understanding', *Philosophy of Science* **71**, in press.

Dieks, Dennis and Henk W. de Regt: 1998, 'Reduction and Understanding', *Foundations of Science* **3**, 45–59.

Dijksterhuis, E. J.: 1950, *De Mechanisering van het Wereldbeeld*, Meulenhoff, Amsterdam.

Dowe, Phil: 1992, 'Wesley Salmon's Process Theory of Causality and the Conserved Quantity Theory', *Philosophy of Science* **59**, 195–216.

Dowe, Phil: 1995, 'Causality and Conserved Quantities: A Reply to Salmon', *Philosophy of Science* **62**, 321–333.

Dowe, Phil: 2000, *Physical Causation*, Cambridge University Press, Cambridge.

Eddington, Arthur: 1923, *Space Time and Gravitation*, Cambridge University Press, Cambridge.

Feynman, R. P., R. B. Leighton, and M. Sands: 1965, *The Feynman Lectures on Physics*, Addison-Wesley, Reading.

Friedman, Michael: 1974, 'Explanation and Scientific Understanding', *Journal of Philosophy* **71**, 5–19.

Gingras, Yves: 2001, 'What Did Mathematics Do to Physics?', *History of Science* **39**, 383–416.

Hartmann, Stephan: 1999, 'Models and Stories in Hadron Physics', in M. Morrison and M. Morgan (eds.), *Models as Mediators*, Cambridge University Press, Cambridge, pp. 326–346.

Heisenberg, Werner: 1927, 'Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik', *Zeitschrift für Physik* **43**, 172–198.

Helmholtz, Hermann: 1983, *Über die Erhaltung der Kraft*, Physik-Verlag, Weinheim.

Hempel, Carl G.: 1965, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*, Free Press, New York.

Hitchcock, Christopher R.: 1995, 'Discussion: Salmon on Explanatory Relevance', *Philosophy of Science* **62**, 304–320.

Hofer-Szabó, G., M. Rédei, and L. E. Szabó: 1999, 'On Reichenbach's Common Cause Principle and Reichenbach's Notion of Common Cause', *The British Journal for the Philosophy of Science* **50**, 377–399.

Humphreys, Paul: 1989, *The Chances of Explanation*, Princeton University Press, Princeton.

Jones, Todd: 1997, 'Unification, Reduction, and Non-Ideal Explanations', *Synthese* **112**, 75–96.

Kargon, Robert and Peter Achinstein (eds.): 1987, *Kelvin's Baltimore Lectures and Modern Theoretical Physics*, MIT Press, Cambridge, MA.

Kitcher, Philip: 1981, 'Explanatory Unification', *Philosophy of Science* **48**, 507–531.

Kitcher, Philip: 1989, 'Explanatory Unification and the Causal Structure of the World', in P. Kitcher and W. C. Salmon (eds.), *Scientific Explanation*, University of Minnesota Press, Minneapolis, pp. 410–505.

Kuhn, Thomas S.: 1970, *The Structure of Scientific Revolutions*, The University of Chicago Press, Chicago.

Kuhn, Thomas S.: 1977, 'Objectivity, Value Judgment, and Theory Choice', in *The Essential Tension*, The University of Chicago Press, Chicago, pp. 320–339.

Laudan, Larry: 1984, *Science and Values*, University of California Press, Berkeley.

Laudan, Larry: 1990, 'Normative Naturalism', *Philosophy of Science* **57**, 44–59.

Longino, Helen: 1990, *Science as Social Knowledge*, Princeton University Press, Princeton.

Machamer, Peter, Lindley Darden, and Carl F. Craver: 2000, 'Thinking about Mechanisms', *Philosophy of Science* **67**, 1–25.

McAllister, James W.: 1996, *Beauty and Revolution in Science*, Cornell University Press, Ithaca.

Morrison, Margaret: 2000, *Unifying Scientific Theories. Physical Concepts and Mathematical Structures*, Cambridge University Press, Cambridge.

Newton, Isaac: 1961, in W. Turnbull (ed.), *Correspondence, Volume 3: 1688–1694*, Cambridge University Press, Cambridge.

Newton-Smith, W. H.: 2000, 'Explanation', in W. H. Newton-Smith (ed.), *A Companion to the Philosophy of Science*, Blackwell, Oxford, pp. 127–133.

Panofsky, W. K. H. and M. Phillips: 1969, *Classical Electricity and Magnetism*, Addison-Wesley, Reading.

Radder, Hans: 1991, 'Heuristics and the Generalized Correspondence Principle', *The British Journal for the Philosophy of Science* **42**, 195–226.

Rouse, Joseph: 2003, 'Kuhn's Philosophy of Scientific Practice', in T. Nickles (ed.), *Thomas Kuhn*, Cambridge University Press, Cambridge, pp. 101–121.

Ruben, David-Hillel (ed.): 1993, *Explanation*, Oxford University Press, Oxford.

Salmon, Wesley C.: 1984, *Scientific Explanation and the Causal Structure of the World*, Princeton University Press, Princeton.

Salmon, Wesley C.: 1990, *Four Decades of Scientific Explanation*, University of Minnesota Press, Minneapolis.

Salmon, Wesley C.: 1994, 'Causality without Counterfactuals', *Philosophy of Science* **61**, 297–312; reprinted in Salmon (1998), pp. 248–260.

Salmon, Wesley C.: 1997, 'Causality and Explanation: A Reply to Two Critiques', *Philosophy of Science* **64**, 461–477.

Salmon, Wesley C.: 1998, *Causality and Explanation*, Oxford University Press, Oxford.

Schurz, Gerhard and Karel Lambert: 1994, 'Outline of a Theory of Scientific Understanding', *Synthese* **101**, 65–120.

Toulmin, Stephen: 1963, *Foresight and Understanding. An Enquiry into the Aims of Science*, Harper and Row, New York.

Trout, J. D.: 2002, 'Scientific Explanation and the Sense of Understanding', *Philosophy of Science* **69**, 212–233.

Van Fraassen, Bas C.: 1977, 'The Pragmatics of Explanation', *American Philosophical Quarterly* **14**, 143–150.

Van Fraassen, Bas C.: 1980, *The Scientific Image*, Clarendon Press, Oxford.

Van Fraassen, Bas C.: 1991, *Quantum Mechanics. An Empiricist View*, Clarendon Press, Oxford.

Van Lunteren, Frans H.: 1991, *Framing Hypotheses*, unpublished Ph.D. Dissertation, Utrecht University.

Wayne, Andrew: 1996, 'Theoretical Unity: The Case of the Standard Model', *Perspectives on Science* **4**, 391–407.

Weber, Erik: 1996, 'Explanation, Understanding and Scientific Theories', *Erkenntnis* **44**, 1–23.

Wheeler, J. A. and R. P. Feynman: 1945, 'Interaction with the Absorber as the Mechanism of Radiation', *Reviews of Modern Physics* **17**, 157–181.

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