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THE METAPHYSICS OF SCIENCE

An Account of Modern Science in terms of Principles, Laws and Theories

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CONTENTS

INTRODUCTION

This book, in its attempt to depict the metaphysics of science, has a form which differs in a number of ways from that of most other contributions to the philosophy of science. Part of this difference is already implied in the book's title, for few modern writers would want to say that science has any metaphysics at all. What does it mean to say that science has a metaphysics?

Metaphysics itself may be thought of as having two main aspects which, following Kant, we shall call the transcen*dent* and the transcenden*tal*. Both of these notions are important for the message of the present work. The transcendental, as it is to be understood here, may be seen as consisting of a person's most fundamental convictions or beliefs about the nature of reality. These are beliefs – such as, for example, belief in the existence of God – which affect the whole of a person's conception of reality, and which, psychologically speaking, are the most difficult to give up. Furthermore, they are beliefs of which a person may not be conscious. In distinction from Kant's view, however, we do not take the transcendental to be independent of experience; rather, while the beliefs that compose it are not just generalisations of experience, they are nevertheless arrived at through some combination of experience and thought. What is important however is that once arrived at, they constitute the very preconditions for the way one afterwards experiences the world. And, as follows from this, the transcendental need not take a predetermined form, as it did for Kant. As conceived by him, the very constitution of humans was such that they experience nature in terms of, for example, cause and effect. Here, the transcendental is open to reform – in terms of our example, though one's belief in God may be deep, it may come to be given up.

As regards science then, the transcendental may be seen as concerning the most fundamental beliefs scientists as a group have regarding the nature of reality, as these beliefs are manifest in their scientific endeavours. Or, moving from psychology to epistemology, we should say that the transcendental for science consists of the most fundamental *presuppositions* of science. In being transcendental with respect to science they cannot have been arrived at through the pursuit of science, but must be, in a definite sense, pre-scientific, or metascientific. And, as in the example given above, they can be revised or abandoned in favour of alternatives. Thus the idea that science has transcendental presuppositions does not conflict with the idea that science is a dynamic, evolving enterprise, but rather directs the philosopher's attention to analysing the dynamics and evolution of science in terms of changes in its presuppositions. These changes may be more or less drastic, which can lead us to say that changes in science, or scientific revolutions, are more or less total. And at the end of the day we may still find that certain basic presuppositions – or core *principles* – have continued to function as regulative ideas for science throughout its history, and furthermore that this transcendental heart of science is what makes science what it is and not another thing. It is just this transcendental aspect of science, and how it affects the enterprise of science as a whole, that is focussed on in the present work.

The other aspect of metaphysics is the transcendent, which, broadly speaking, is that which lies beyond the limit of some generally accessible realm, whether it be, for example, experience, knowledge, understanding, language, or thought. Of immediate relevance for the study of science is the idea of something's lying beyond the limit of knowledge, where knowledge is understood as empirical knowledge. It is largely with regard to the issue of whether science should be restricted in its investigations to what can be empirically known, or whether it should also delve into the transcendent realm of theoretical entities, that the long-standing battle in the philosophy of science between empiricists and realists has been waged (whereas the issue concerning the transcenden*tal* may be seen as the focal point of the debate between empiricists and rationalists). Our difference from Kant with regard to the transcendent, apart from his not considering its possible application as limiting realms other than that of empirical knowledge, is that what is transcendent at one point in time need not remain so – what is at one time hypothetical may become factual. So we have more of a pragmatic view of the transcendent than does Kant, at least as regards its application to science. And further, on our notion there are *levels* of transcendence, such that, for example, physical atoms may be viewed as transcendent with respect to particular empirical laws concerning gases, while quarks and leptons may be considered transcendent with respect to physical atoms. It may here be noted that, properly understood, the realist is not necessarily advocating that one can have knowledge of any particular transcendent realm, but that it is through theorising about the nature of such a realm and its relation to the non-transcendent or empirical that the latter can be made intelligible. This issue is the topic of the first chapter of the book, and constitutes a theme throughout the work which is rounded off in the penultimate chapter.

So what is here meant by saying that science has a metaphysics is that it has a transcendental aspect, the question of whether it also has or ought to have a transcendent aspect being one investigated in the book against the background of the presupposition that the transcendental aspect has the particular form specified in Chapter 2.

A second way in which this book differs from most other contributions to the philosophy of science is in its emphasis on 'paradigm-thinking.' There are various ways in which the relation between the transcendental and science may be conceived, and the way in which it is conceived here is perhaps novel. During the nineteenth century it was common among philosophers and scientists to think in terms of the principles of science, but for them the principles were to constitute the *basis* of science, whereas here principles are to constitute the *core* of science – a distinction to which we shall return directly. Furthermore, for most thinkers at that time, and even today, science was conceived of as a monolithic enterprise providing the one and only sure route to truth. Here, on the other hand, as is in keeping with the view that the transcendental can take different forms, science is conceived as one particular epistemological activity which may be compared with others. Moreover, what is intended by *science* in the present work is restricted to what is normally considered *modern* science, i.e. science since the time of Galileo and the Scientific Revolution. Thus modern science can be compared with other ostensible means of gaining knowledge or understanding of reality – such as Aristotelian science, or magic – such a comparison to be made first in terms of similarities and differences in their core principles.

What is meant by saying that particular principles constitute the core rather than the basis of science is that they are not general self-evident truths from which particular empirical truths can be formally deduced, but are rather ideal conceptions of reality which guide scientists' investigation of actual reality. From this perspective, what makes a particular activity scientific is not that the reality it uncovers meets the ideal, but that its deviation from the ideal is always something to be accounted for. In this way transcendental principles constitute *paradigms* in much the same sense as this term is intended by Kuhn (and Wittgenstein), it being understood however that they are conceptual and ontological rather than concrete and methodological in nature. Thus, as distinct from Kuhn's view, principles constitute a paradigm for modern science in that they are mental constructs depicting, in broad outline, an ideal reality, rather than being instances of scientific practice embodying an ideal method. Similarly to Kuhn's view, on the other hand, an enterprise focussed on a particular ontological paradigm can go through a number of historical phases. On the present account, the paradigm of modern science as a whole had its golden period during the nineteenth century – a period that has been termed 'the age of science' – while during the twentieth century greater difficulty has been experienced in the attempt to assimilate the results of scientific enquiry to its transcendental ideal, particularly in its core discipline of physics.

Speaking of the 'core discipline of physics' brings us to another sense in which transcendental principles constitute the ontological paradigm of modern science. Thus, just as science had an historical period during which the reality it revealed was most similar to the ideal depicted in the principles, so too can we say that various scientific disciplines lie closer or further from the transcendental core of science depending on how similar the reality they uncover is to the reality of the principles. Due to the nature of the principles of science on the one hand, and reality on the other, the greatest success in applying the principles has been had in physics and chemistry, while biology lies further from the core, and the social sciences further still.

Another way that paradigm-thinking enters the present work is in the claim that this sort of thinking actually occurs in science. Thus on the present view both scientific theories and the expressions of empirical laws constitute in science itself intellectual paradigms intended to capture the *essence* of particular aspects of reality; and these essences are not necessary or sufficient conditions that reality must meet in order for the relevant laws or theories to be applicable, but *idealised* states of affairs which as a matter of fact might never have real correlates. One area in which this paradigm-thinking is particularly clearly manifest is in the treatment of natural kinds in biology (as examined in Chapter 7), where difference of natural kind is not an all-or-nothing affair. In this context, paradigm-thinking involves the taking of certain real things or intellectual constructs as each constituting the ideal of a particular type, such that individual entities are seen as gravitating more or less to one paradigm or another depending on their characteristics; in this way such an entity may thus be considered to be of some particular type, or perhaps to constitute a borderline case between types.

The use of paradigm-thinking in the present work does not stop there however, but lies in the background throughout. Thus, when in the book we speak of the function of theories as being to provide causal explanations of laws, we mean that this is their *paradigmatic* function, which does not exclude their being used, for example, to provide information about a deeper-lying reality as such; or when we say that the aim of the empirical aspect of science is the discovery of empirical laws, we mean this is its *paradigmatic* aim, and do not intend to deny that the empirical aspect of science may also involve e.g. the determination of the existence of particular entities. The conception of science presented here is, it is hoped, a coherent whole in which various concepts occupy particular nodal points, thereby making it also a system. In this system these concepts, the most important of which are principles, laws and theories, function at these nodes as conceptual paradigms.

So the notions of principles, laws and theories constitute the nodal or paradigm concepts in terms of which the present account of science is conducted. This account, in broad outline, runs as follows. Modern science, as presented in Chapter 2, is a particular epistemological enterprise which consists in the application of, and thereby obtains its nature from, particular *fundamental metaphysical principles.* In order to find clear application to reality, these fundamental principles are refined in various ways, giving rise to different groupings of what may be termed *refined principles*, each grouping defining a different science or scientific discipline. Where the fundamental principles are normally implicit in the doing of science, the refined principles are explicit.

The assumption that reality has the basic nature depicted by the implicit fundamental principles leads to its being investigated according to a particular method – the experimental method – resulting in the discovery of *empirical laws* (the topic of Chapter 3). There is no guarantee however that the laws discovered by employing this method will be in keeping with the fundamental principles as a whole, nor, more particularly, with the refined principles of the science or scientific discipline in question. In order to show that and how they are so, one or more depictions of the reality being investigated is advanced, depictions each of which is more detailed than that provided by the refined principles.

Such depictions are of hypothetical realities which on the one hand naturally give rise to the empirical laws that are of interest, while at the same time are constrained by limits set by the refined principles. This constraint consists in the depicted realities' not transgressing the refined principles, as well as in the depictions' employing only concepts taken from them. Such ontological depictions are *theories* which, if they achieve their aim, may be said to have scientifically *explained* the laws in question by showing them to be but an empirical manifestation of the principles underlying the science in question (the topic of Chapter 4). In this way, where empirical laws provide scientific *knowledge*, theories, by linking the laws to the principles, provide scientific *understanding.*

This, then, is the central message of this book. Following its presentation in Chapters 2 to 4, it is employed in various ways. First, in Chapter 5, it provides the structure for a model of scientific explanation. This model is the Principle-Theory-Law (PTL) model, which involves a further development of the law/theory distinction in introducing notions of the *nominal* vs. the *real* aspects of the domain of a theory. While it is intended that this model capture the essence of explanation in modern science, it is possible that it also has application outside this realm. In any case, it is applied in Chapter 6 to a case study taken from modern microeconomics, where it appears to fit rather well. There, according to the core ideas of the book, the key difference between mainstream economics and the natural sciences, apart from their subject-matter, lies in their having different conceptions of causality.

Against the background of one of the fundamental principles of science presented in Chapter 2, the distinction between the nominal and the real developed in the PTL model is presented in Chapter 7 as the key to understanding the modern-scientific conception of natural kinds. There it is suggested that for modern science natural kinds are to be conceived of as having both nominal and real essences, where a real essence can be a nominal essence relative to some even deeper-lying real essence. This notion of difference of level, mentioned above with regard to the transcendent, stems from the relativisation of the law/theory distinction; it is also a key aspect of the discussion of probability in Chapter 8. In that discussion, for which another fundamental principle is central, the distinction is made between nominal and real probability determinations, where nominal probability is based on empirical samples while real probability is based on ontological theories.

In Chapter 9, as mentioned, the realist/empiricist discussion is brought to a close; and the distinctions between epistemology and ontology, and between knowledge and understanding, are discussed. In the final chapter, Chapter 10, the modern-scientific worldview is compared with other historical worldviews, and the question is raised whether for both epistemological and pragmatic reasons it may be time to change to some fundamentally different epistemology, whether or not the name "science" be applied to it.

As is clear from the above, this work is one in the philosophy of science as distinct from epistemology. We are here focusing on the nature of modern science, and not on how best to obtain knowledge of reality in some wider context. Though basic epistemological issues are broached in Chapter 3, this is done only with the aim of determining the fundamental conception of knowledge acquisition of modern science. This marks a third difference between this work and most other contributions to the philosophy of science. In them it is implicitly assumed that modern science constitutes the best way of obtaining knowledge about reality, the question being what is the best way to conduct science. In other words there is an implicit faith that humankind has been constantly moving forward along the one road to Truth, that road being Science, without consideration being given to the thought that modern science might appear just as wrongheaded in the future as alternative forms of science do now. The interest of the present volume, on the other hand, is primarily in clarifying the nature of science, though questions as to its value are taken up in the final chapter.

As is implied in that chapter, that the nature of modern science be clarified is becoming a pressing need in an age where science, while helping provide most people in industrialised nations with a high standard of living, has at the same time been an essential factor in the development of nuclear and chemical weapons, and a contributing factor to the spread of pollutants threatening future human life on earth. In spite of these trends, science shows signs of becoming the first world-wide religion. The scientific enterprise is in serious need of demystification.

The present work is intended to reveal the nature of modern science as an intellectual enterprise. If successful, it should thereby explain such central aspects of modern science as the way in which the Scientific Revolution of the seventeenth century actually was a revolution with respect to preceding epistemological approaches. Similarly, it should explain both why the nineteenth century may be considered the golden age of science, as well as why physics and chemistry lie at the core of the enterprise while the social sciences continue to struggle to obtain scientific status. It should also afford a means of demarcating science from non-science (in terms of paradigms), and explain the nature of scientific revolutions, whether major or minor. Furthermore, the present work should clarify the nature of the foundational problems in physics today, as well as the nature of such activities as scientific explanation and classification.

We begin our excursion into the metaphysics of science by examining the historical debate regarding the role of the transcendent in science, as manifest in the empiricism/realism issue in the philosophy of science.

CHAPTER 1

EMPIRICISM VS. REALISM – THE PERENNIAL DEBATE IN THE PHILOSOPHY OF SCIENCE

The issue over empiricism and realism, presently the focus of much discussion in the philosophy of science, is but the manifestation of an age-old perplexity. The perplexity is over the relation between one's experiences, and the world, or between phenomena and reality.

Now of course phenomena may themselves be considered real, and the realm they constitute be considered a world. But such a world differs from the reality with which it is being contrasted here by the fact that the latter is conceived to be the only one of its kind, to exist independently of being experienced, and to be common to us all.

At one time or another virtually every conceivable line has been taken on the issue, from the view that there is no reality other than phenomena, to the view that reality, while different from phenomena, alone causes and is perfectly represented by them. The interest of the present chapter will be in presenting some of the more conspicuous forms the issue has taken in the philosophy of science since the time the subject began as a relatively autonomous discipline, and in critically appraising some of the more recent contributions against the background of the debate as a whole.

Empiricism in the philosophy of science is, broadly speaking, the view that scientific investigation be confined to phenomena and their formal relations, while realism is the view that it is also the task of science to investigate the *causes* of such phenomena and relations, conceived as emanating from the real world.[1] **Auguste Comte**

[1] This is the conception of realism involved in the historical debate. As will be argued below, the modern conception of 'scientific realism,' according to which theories are statements and the aim of science is to produce theories ever closer to the truth, is essentially an empiricist conception, and fails to come to grips with the

(1798–1857) strongly advocates a variant of the empiricist view, which, following Saint-Simon, he terms *positivism*:

Our study of nature is restricted to the analysis of phenomena in order to discover their *laws*, that is, their constant relations of succession or similitude, and can have nothing to do with their *nature*, or their *cause*, first or final, or the mode of their production. Every hypothesis that strays beyond the domain of the positive can merely occasion interminable discussions, by pretending to pronounce on questions which our understandings are incompetent to decide.[2]

Comte's main point is epistemological, not ontological, which we shall see also to be the case with most commentators on the issue in the philosophy of science. He is not denying that the domain of phenomena has a causal basis in a world transcending such phenomena (and might thus be considered an ontological realist by implication), but is arguing that the inaccessibility of this world makes it idle to posit hypotheses concerning its nature.

Comte sees his positivistic approach to the investigation of nature as the third stage of a process which first passes through theology and then metaphysics. In metaphysics we find a "vain search after absolute notions, the origin and destination of the universe, and the causes of phenomena."[3] He asks:

What scientific use can there be in fantastic notions about fluids and imaginary ethers, which are to account for phenomena of heat, light, electricity and magnetism? Such a mixture of facts and dreams can only vitiate the essential ideas of physics, cause endless controversy, and involve the science itself in the disgust that the wise must feel at such proceedings. . . . These hypotheses *explain* nothing. For instance, the expansion of bodies by heat is not *explained* – that is, cleared up – by the notion of an imaginary

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epistemological issue. In this regard, cf. Sober (1980), p. 371, and Blackmore (1983), p. 34.

[2] Comte (1830–42), p. 147.

[3] Ibid., p. 72. Next quote, pp. 148–149. In spite of this view of metaphysics, however, Comte claims elsewhere that "the human mind could never combine or even collect [*recueiller*] observations unless it were directed by some previously adopted speculative doctrine" (*Discours sur l'Esprit Positif*, Paris, 1844, p. 6; cited in Laudan, 1981, p. 146). Another point difficult to reconcile with the main thrust of his work, namely an instrumentalistic acceptance of theorising, is taken up in Laudan (1981), pp. 150–156.

fluid interposed between the molecules, which tends constantly to enlarge their intervals, for we still have to learn how this supposed fluid came by its spontaneous elasticity, which is, if anything, more unintelligible than the primitive fact.

For Comte, explanation is not to involve the positing of the existence of unobservable entities: "an explanation of facts is simply the establishment of a connection between single phenomena and some general facts, the number of which continually diminishes with the progress of science,"[4] though it is not evident whether Comte expects explanation in this sense to suffice to 'clear up' e.g. why heated bodies expand. Since such connections and the relations constituting the laws of phenomena have nothing to do with the cause of the phenomena, neither they nor the explanations in which they figure are causal. Here we see how the view that the only relations of epistemological relevance are formal is a natural consequence of empiricism (positivism), and serves to complement it. Comte, however, does not see these relations as being of formal logic, but of mathematics, "the most powerful instrument that the human mind can employ in the investigation of the laws of natural phenomena." On this view, then, such issues as that concerning action at a distance do not arise, for there is no requirement to indicate the cause of the phenomena in which this principle appears operative, but merely to show the phenomena to be formally connected to some general facts.

Comte's opposition to metaphysics and the investigation of causes is antithetical to the position of **William Whewell** (1794–1866). On Whewell's view, the notion of *force* in physics is nothing other than the empirical specification of the more general notion of *cause*. Thus, for example, each of Newton's three axioms or laws of motion is the empirical determination of a more fundamental causal maxim – a maxim whose empirical determination could take some other form in a different field of inquiry. Newton's first law of motion – that a body will continue in its state of rest or uniform motion in a straight line unless acted upon by a force – is in this way considered

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[4] Comte (1830–42), p. 72; next quote, p. 100.

by Whewell to be an empirical specification of the maxim that nothing can take place without a cause.[5]

With reference to Comte, Whewell says:

> Again, some persons condemn all that we have here spoken of as the discussion of ideas, terming it *metaphysical*: and in this spirit, one writer has spoken of the 'metaphysical period' of each science, as preceding the period of 'positive knowledge.' But as we have seen, that process which is here termed 'metaphysical' – the analysis of our conceptions and the exposure of their inconsistencies – (accompanied with the study of facts) – has always gone on most actively in the most prosperous periods of each science. There is, in Galileo, Kepler, Gassendi, and the other fathers of mechanical philosophy, as much of *metaphysics* as in their adversaries. The main difference is, that the metaphysics is of a better kind; it is more conformable to metaphysical truth.

As distinct from metaphysical truth, Whewell distinguishes two kinds of *inductive* truth: laws of phenomena, and theories of causes – the former describing an order which the phenomena follow, and the latter explaining *why* they follow that order by indicating the cause or causes of which the order is the effect.[6] Thus Kepler discovered that the planets describe ellipses, and Newton explained why they do so by reference to his (causal) laws of motion and the law of gravitation. But it may be that our empirical specification of causes be improved upon with the progress of science, so that what we take at any one time as the particular cause of a certain phenomenal order be mistaken. Again with reference to Comte, Whewell says:

> Since it is thus difficult to know when we have seized upon the true cause of the phenomena in any department of science, it may appear to some persons that physical inquirers are imprudent and unphilosophical in undertaking this Research of Causes; and that it would be safer and wiser to confine ourselves to the investigation of the laws of phenomena, in which field the knowledge we obtain is definite and certain. Hence there have not been wanting those who have laid it down as a maxim that 'science must study only the laws of phenomena, and never the mode of production.' But it is

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[5] Whewell (1847), Part 1, pp. 217–218; quote following, Part 2, p. 378.

[6] Whewell's distinction between metaphysical maxims (or axioms, or principles), laws of phenomena, and theories of causes is essentially the same distinction as that made in the present study between principles, laws and theories.

easy to see that such a maxim would confine the breadth and depth of scientific inquiries to a most scanty and miserable limit. Indeed, such a rule would defeat its own object; for the laws of phenomena, in many cases, cannot be even expressed or understood without some hypothesis respecting their mode of production. How could the phenomena of polarization have been conceived or reasoned upon, except by imagining a polar arrangement of particles, or transverse vibrations, or some equivalent hypothesis? . . .

To debar science from enquiries like these, on the ground that it is her business to inquire into facts, and not to speculate about causes, is a curious example of that barren caution which hopes for truth without daring to venture upon the quest of it. This temper would have stopped with Kepler's discoveries, and would have refused to go on with Newton to inquire into the mode in which the phenomena are produced. It would have stopped with Newton's optical facts, and would have refused to go into the mode in which these phenomena are produced. And, as we have abundantly shown, it would, on that very account, have failed in seeing what the phenomena really are.[7]

On the other hand there is a particular point on which it may appear that Whewell and Comte would agree, namely that regarding the fruitlessness of positing the existence of what we would today call theoretical entities. With regard to the atomistic theories prevalent at his time, Whewell says, "in explaining the properties of matter as we find them in nature, the assumption of solid, hard, indestructible particles is of no use or value." An attitude shared not only by Whewell and Comte, but by almost all early contributors to the debate, is a predilection for the attainment of *certainty*,[8] and this disposes Whewell to view the atomistic doctrine with a critical eye. But it should be noted that in the present context Whewell is not actually denying that something like atoms may exist and play a causal role in the manifestation of phenomena – his concern rather is over the claim that such entities be indestructible. Ironically, Comte himself appears to accept the notion of atoms where he suggests that "We say that the general phenomena of the universe are *explained* by [the doctrine of gravitation], because it connects under one head the whole immense variety of astronomical facts, exhibiting the con-

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[7] Ibid., Part 2, pp. 103, 104; next quote, Part 1, p. 438.

[8] On the philosophical background to this predilection, see Manicas (1987), pp. 12–15.

stant tendency of atoms towards each other in direct proportion to their masses, and in inverse proportion to the squares of their distances;"[9] a doctrine which he considers to be a mere extension of the perfectly familiar fact of the weight of bodies on the surface of the earth.

J. S. Mill (1806–1873), while agreeing with Comte on fundamentals, sees a problem with his approach as lying in his objecting to the application of the word "causal" to relations of succession. In the spirit of Hume and in defiance of common sense, Mill at one point identifies causality with succession, pronouncing a Law of Causation, which "is coextensive with the entire field of successive phenomena, all instances whatever of succession being examples of it."[10] However, like Comte, he does not wholly deny the existence of causes which, in his terms, actually produce their effects, but considers them to be of no relevance to induction: "I premise then, that when in the course of this inquiry I speak of the cause of any phenomenon, I do not mean a cause which is not itself a phenomenon; I make no research into the ultimate or ontological cause of anything. ... The only notion of a cause which the theory of induction requires, is such a notion as can be gained from experience."

Thus for Mill, all laws of nature are causal in his sense, and no distinction is to be drawn on that ground between e.g. Kepler's laws and what is commonly termed Newton's theory of gravitation. Their indistinguishability on this ground, however, is not to exclude our speaking of Newton's 'theory' *explaining* Kepler's laws, for explanation on Mill's view consists in resolving laws into other laws. But explanation for Mill, unlike for Whewell, is not to answer the question *why?*

> The word explanation is here used in its philosophical [*earlier eds.*: a somewhat peculiar] sense. What is called explaining one law of nature by another, is but substituting one mystery for another; and does nothing to render the general course of nature other than mysterious: we can no more assign a *why* for the more extensive laws than for the partial ones. The ex-

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9 Comte (1830–42), p. 75.

10 Mill (1881), Bk. III, Ch. v, § 1; next quote, § 2. In this regard cf. McCloskey (1971), pp. 39–40: "Mill's concept is not the ordinary concept and . . . the account he offered is not what is ordinarily meant by 'cause.'"

planation may substitute a mystery which has become familiar, and has grown to *seem* not mysterious, for one which is still strange. And this is the meaning of explanation, in common parlance.[11]

Another notion Mill treats in a noteworthy way is that of being physical. The notion of physical is normally applied to what is considered to be real and not merely phenomenal, but Mill uses it in terming the relations of succession between phenomena *physical* causes.[12]

Ernst Mach (1838–1916) also uses the term "physical" in a phenomenalistic sense, where he not only speaks of physical *experiences*, which are to consist solely of sensations, but goes so far as to identify physical objects (bodies) with sensations.[13] Mach, like Comte and Mill, is an empiricist; but his empiricism is more extreme than theirs, being ontological as well as epistemological. For him, the world consists solely of sensations and mathematical relations among them. "What we represent to ourselves behind the appearances exists *only* in our understanding, and has for us only the value of a *memoria technica* or formula, whose form, because it is arbitrary and irrelevant, varies very easily with the standpoint of our culture."[14] Thus, "In conformity with this view the ego can be so extended as ultimately to embrace the entire world."[15] Since everything is part of the ego, "Bodies do not produce sensations, but complexes of elements (complexes of sensations) make up bodies." This view serves greatly to simplify the scientific enterprise:

For us, therefore, the world does not consist of mysterious entities, which by their interaction with another, equally mysterious entity, the ego, produce sensations, which alone are accessible. For us, colors, sounds, spaces, times, etc. are provisionally the ultimate elements, whose given connexion it is our

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[11] Mill (1881), Bk. III, Ch. xii, § 6.
[12] Ibid., § 2.
[13] See e.g. Mach (1906), pp. xii and 29.
[14] From Mach's *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit*, Prague, 1872; cited in Blackmore (1972), p. 86. See also e.g. Mach (1906), p. 363.
[15] Ibid., p. 13; next quote, p. 29.

business to investigate. It is precisely in this that the exploration of reality consists.[16]

For Mach, reality does not extend beyond what can be known; all that can be known are sensations and their relations; and that unknowable something which purportedly lies behind sensations is the concern of metaphysics. Thus, like Comte, Mach is led to denounce metaphysics.

I should like the scientists to realize that my view eliminates all metaphysical questions indifferently, whether they be only regarded as insoluble at the present moment, or whether they be regarded as meaningless for all time. I should like then, further, to reflect that everything that we can know about the world is necessarily expressed in the sensations, which can be set free from the individual influence of the observer in a precisely definable manner. ... Everything that we can want to know is given by the solution of a problem in mathematical form, by the ascertainment of the functional dependency of the sensational elements on one another. This knowledge exhausts the knowledge of 'reality.'[17]

One might wonder, however, whether Mach's view itself contains metaphysical elements. Not according to Mach: "[O]ur view has no metaphysical background, but corresponds only to the generalized expression of experiences."

Mach does not assume himself to be the first to advocate such a view, and refers to Berkeley and Hume in this context. It also seems that he believes a good deal of science actually to have been conducted in a way which is in keeping with his conception: "Science has always required self-evident propositions as a safe foundation upon which to build;"[18] and he sees his foremost opponents as being those who assume the *existence* of a non-perceptible molecular or atomic realm.

All the same, Mach is apparently not against theorising about such a realm,[19] in that it may lead to the awareness of important relations

[16] Ibid., pp. 29–31.
[17] Ibid., p. 46; next quote, p. 61.
[18] Ibid., p. 56.
[19] Though he does say in his (1883), p. 337, that "A thing that is beyond the ken of knowledge, a thing that cannot be exhibited to the senses, has no meaning in natural science."

among phenomena. But once these relations have been determined, the theory and its realm of imperceptibles is to be discarded.[20]

Like Mill, Mach identifies causation with succession; and like both Comte and Mill he conceives of explanation as at most consisting in indicating particular formal relations among phenomena: "a so-called 'causal' explanation, also is nothing more than the statement of an actual fact [sensation] or of a connection between facts."[21] Thus for Mach no non-causal constant relations are possible; and in explanation no reference need be made to unseen agents or forces.

A number of questions arise concerning this view, such as how it is to avoid solipsism, and the extent to which scientific practice actually has been in keeping with it. Concerning this latter point we might consider as an example an aspect of science apparently accepted as legitimate by all empiricists, namely Kepler's three laws of motion – or more particularly the first law – which states that the orbit of each planet is an ellipse, with the sun at one focus.

According to Mach, the value in Kepler's first law lies in its ability to economise our thinking about the motions of the planets. But is this economising of thought consistent with the view that it is fundamentally the phenomena that are 'real'? From a broadly phenomenal point of view the planets do not describe ellipses, but involve retrogradation and so forth. In order to conceive of them as describing ellipses – in order to obtain 'economy of thought' – one must *abstract* from the phenomenal realm.[22] And this abstraction implies a form of conceptualisation in which the phenomenally manifest motions are illusory.

As regards solipsism, one might ask how on Mach's view there can exist anything more than one thinking subject and the sensations experienced by that subject, for the bodies of other people are only

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[20] In this regard, see Laudan (1981), pp. 211&n–213. A fundamental point with respect to the empiricism/realism issue however is whether such a stance is actually consistent with an otherwise empiricist approach. This point will be returned to below.

[21] Mach (1906), p. 335.

[22] Cf. Whewell (1847), Part 1, p. 25: "When we see a body move, we see it move in a path or *orbit*, but this orbit is not itself seen; it is constructed by the mind."

sensations experienced by the ego, and the sensations experienced by those bodies in turn are not perceived by the ego at all. Mach's reply to the latter point is that each of us is justified in positing the existence of the sensations of others on the basis of analogy with our own case.[23]

Ludwig Boltzmann (1844–1906) uses this line of reasoning against Mach, saying that we can then just as well use analogy to argue for the existence of a non-perceptible realm of atoms.[24] To this Mach has responded by saying: "I never had it in mind to replace the language of the vulgar or even the everyday speech of scientists. Also, I hope I will be credited with having long been familiar with the simple considerations which Boltzmann has raised."[25] Whether Boltzmann's considerations were simple or not, history has refuted Mach on the question of the existence of atoms, in the sense then at issue.

But the basic difference between Boltzmann and Mach lies deeper than this. We can begin to obtain an idea of this difference by comparing Mach's saying that what we represent to ourselves behind the appearances exists only in our understanding, with Boltzmann's disavowal of solipsism:

> Solipsism is the view that the world is not real, but a mere product of our [*sic*] fantasy, like a dream object. I too once hankered after this whim, which led to my failing to take the right practical action and caused me great damage, to my immense delight, for this provided me with the desired proof that the external world exists, a proof that can consist only in showing that if we doubt this existence we are less able to act appropriately.[26]

Here we see that Boltzmann differs from Mach in his belief in the *existence* of a non-phenomenal or real world – a point central to

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[23] Mach (1906), pp. 24 and 27.

[24] Boltzmann, 'On the Question of the Objective Existence of Happenings in Inanimate Nature,' 1897, in his *Populäre Schriften*, Leipzig, 1925, p. 175; cited in Blackmore (1972), p. 206.

[25] From *Die Principien der Wärmelehre*, Leipzig, 1896; cited in Blackmore (1972), p. 206.

[26] Boltzmann (1900, 1902), pp. 150–151.

understanding the debate between them.[27] In this light we can understand why, for example, Boltzmann makes no claim to the effect that the atomic theory must be correct, i.e. that atoms in particular must exist or be real. His position is rather that *something* must be real, and that scientific advance on the phenomenal level requires theorising about the nature of that something. The line he takes is thus that:

> All these [previously mentioned] achievements and many earlier attainments of atomic theory are absolutely unattainable by phenomenology or energetics; and I assert that a theory that achieves original insights unobtainable by other means, that is moreover supported by many facts of physics, chemistry and crystallography, such a theory should not be opposed but cultivated further. As regards ideas about the nature of molecules it will however be necessary to leave the widest possible room for manoeuvre.[28]

And we can also understand Mach's position, that atoms *cannot* be realities behind the phenomena, since for him *nothing* can:

> [I]t would not become physical science to see in its self-created, changeable, economical tools, molecules and atoms, realities behind phenomena' . . . . The atom must remain a tool for representing phenomena, like the functions of mathematics. Gradually, however, as the intellect, by contact with its subject matter, grows in discipline, physical science will give up its mosaic play with stones and seek out the boundaries and forms of the bed in which the living stream of phenomena flows.[29]

Where realists claim that there is a reality lying beyond or behind the realm of phenomena, a reality which is at least partly responsible for the nature of that phenomenal realm, strict phenomenalists say that it is quite pointless to postulate even the existence of such a reality, since nothing could be known about it in any case. It would

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[27] Thus issue is here taken with Larry Laudan, who believes the fundamental difference between Mach and Boltzmann to lie in their divergent *methodological* views with respect to the aim and structure of theory (1981, p. 218). As will be shown here, their differences with regard to theory can more profitably be seen as a manifestation of their more fundamental *ontological* differences – differences which Laudan neglects and which are difficult to imagine being explained on his view.

[28] Boltzmann (1899), p. 99; see also e.g. Philipp Frank's recollection of a conversation with Boltzmann, quoted in Brush (1968), p. 207.

[29] Mach, *Populär-wissenschaftliche Vorlesungen*, Leipzig, 1896; pp. 206–207 in the translation: *Popular Scientific Lectures*, La Salle, III., 1943; quoted in Laudan (1981), p. 224.

at best be a noumenal world of *Dinge an sich*, the subject of unending metaphysical speculation. Realists, on the other hand, feel that the only way one can make sense of the world of phenomena is by assuming the existence of just such a real world. They are thus forced to go beyond experience in their considerations, and to treat of what is not certain. In science this means *theorising* about reality, while recognising the tentative nature of one's efforts. In this regard Boltzmann is perhaps one of the first explicitly to acknowledge the value of the transitory in science, and implicitly to suggest that the attainment of certainty or truth need not be science's sole aim:

[H]ypotheses that leave some play to fantasy and go more boldly beyond what is given will give constant inspiration for novel experiments and thus become pathfinders to totally unsuspected discoveries. Such a theory will of course be subject to change and it may happen that a complicated theoretical structure will collapse and be replaced by a new and more effective one, in which however the old theory as a picture of a restricted field of phenomena usually continues to find a place within the framework of the new one; as for instance emission theory for describing catoptric and dioptric phenomena, the hypothesis of an elastic luminous aether for representing interference and diffraction, the theory of electric fluids for describing electrostatic phenomena.[30]

That the attainment of truth be the primary, if not the only aim of science is a presupposition of virtually all non-realist contributors to the debate. In this respect, **Henri Poincaré** (1854–1912) is no exception: "The search for truth should be the goal of our activities; it is the sole end worthy of them."[31] Poincaré's ontological position is that of a strict positivist or phenomenalist:

[A] reality completely independent of the mind which conceives it, sees or feels it, is an impossibility. A world as exterior as that, even if it existed, would for us be forever inaccessible. But what we call objective reality is, in the last analysis, what is common to many thinking beings, and could be common to us all; this common part, we shall see, can only be the harmony expressed by mathematical laws. It is this harmony then which is the sole objective reality, the only truth we can obtain.

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30 Boltzmann (1904), p. 161.
31 Poincaré (1914), p. 11; next quote, p. 14.

In considering the nature of science in more detail, Poincaré makes a distinction similar to Whewell's distinction between metaphysical maxims, theories of causes, and laws of phenomena. Poincaré's distinction is between three categories of hypothesis: the first category contains general principles, the second what he calls 'indifferent hypotheses,' and the third 'real generalisations,' which are the mathematical laws referred to in the quote above. Unlike Whewell's metaphysical maxims however, Poincaré's fundamental principles are not to be metaphysical but are to be arrived at by the experimental method.[32]

Hypotheses of the second category, like Boltzmann's 'theories,' constitute attempts to conceptualise the reality Poincaré considers inaccessible. With regard to them he says: "There is a second category of hypotheses which I shall qualify as indifferent. In most questions the analyst assumes, at the beginning of his calculations, either that matter is continuous, or the reverse, that it is formed of atoms. In either case, his results would have been the same. On the atomic supposition he has a little more difficulty in obtaining them – that is all."[33]

One might wonder whether Poincaré is correct in his assumption that all rival hypotheses (theories) which might fall into his second category necessarily are 'indifferent,' or, in modern terminology, necessarily are 'underdetermined by the data.' Almost anticipating Poincaré's very words, Whewell says:

> If any one holds the adoption of one or other of these theories [of emission and undulation] to be indifferent, let him express the *laws of phenomena* of diffraction in terms of the theory of emission. If any one rejects the doctrine of undulation, let him point out some other way of connecting double refraction with polarization. And surely no man of science will contend that the beautiful branch of science which refers to that connexion is not a portion of our positive knowledge.[34]

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[32] See Poincaré (1902), pp. 105, 110, 129, 138, 166. Poincaré's general principles are more similar to Whewell's empirical specifications of metaphysical maxims and to what in the present study are termed *refined* principles, while Whewell's metaphysical maxims are more similar to what we term *fundamental* principles.
[33] Ibid., p. 152.
[34] Whewell (1860), p. 231.

Like Mach (and Comte in places), Poincaré admits that theories or 'indifferent hypotheses' may have a place in science, in that they may give satisfaction to the mind;[35] but also in the spirit of Mach he warns against their being taken too seriously: "These indifferent hypotheses are never dangerous provided their characters are not misunderstood. They may be useful, either as artifices for calculation, or to assist our understanding by concrete images, to fix the ideas, as we say. They need not therefore be rejected." At another point he adds: "These hypotheses play but a secondary rôle. They may be sacrificed, and the sole reason this is not generally done is that it would involve a certain loss of lucidity in the explanation [*exposition*]."[36] Elsewhere, though, he considers them to be "useless and unverifiable."

We find an expression of Poincaré's *instrumentalism* in the context of a discussion of Maxwell's electrodynamic theory, where he says: "Two contradictory theories, provided they are kept from overlapping, and that we do not look to find in them the explanation of things, may, in fact, be very useful instruments of research."[37]

In his philosophy of science Poincaré not only believes that we cannot have knowledge of real objects,[38] but considers questions regarding their nature to be meaningless. "To those who feel that we are going too far in our limitations of the domain accessible to the scientist, I reply: These questions which we forbid you to investigate, and which you so regret, are not only insoluble, they are illusory and devoid of meaning." On the other hand, the true relations *between* real objects *can* be known.[39] According to Poincaré these relations take the form of mathematical (empirical) laws, and not scientific theories; and for him it is to the attainment of the knowledge of such laws that science ought ultimately be devoted. "The day will perhaps come when physicists will no longer concern themselves with questions which are inaccessible to positive methods,

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35 Poincaré (1902), p. 164; quote following, p. 153.
36 Ibid., p. 212; next quote, p. 156.
37 Ibid., p. 216; for the Perspectivist conception of theory conflict and the realist-instrumentalist controversy, see Dilworth (1994a), pp. 116–117 and 164–166.
38 Poincaré (1902), p. 161; quote following, p. 163.
39 Ibid., p. 161; quote following, p. 223.

and will leave them to the metaphysicians. That day has not yet come; man does not so easily resign himself to remaining forever ignorant of the causes [*le fond*] of things."

Pierre Duhem (1861–1916) distinguishes between two kinds of theory: what may be called *explanatory* theories, which are similar to Whewell's theories of causes and Poincaré's indifferent hypotheses, and what might be termed *representational* theories, which are systematisations of experimental laws and are similar to Poincaré's hypotheses containing general principles.[40] Where Comte, Mill and Mach would call systematisation explanation, Duhem keeps the two cases separate. According to Duhem, "While we regard a physical theory as a hypothetical explanation of material reality, we make it dependent on metaphysics. In that way, far from giving it a form to which the greatest number of minds can give their assent, we limit its acceptance to those who acknowledge the philosophy it insists on."[41]

Where Poincaré sees the search for truth as the sole aim worthy of science, Duhem's view is similar, though he expresses this aim as a striving to obtain universal assent. He therefore sees his own task as being to provide a conception of theories which allows of their being impartially judged, i.e. judged independently of metaphysical bias. On Duhem's conception, "A physical theory is not an explanation. It is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws."[42]

Thus for Duhem,

> a true theory is not a theory which gives an explanation of physical appearances in conformity with reality; it is a theory which represents in a satisfac-

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[40] In this regard see Giedymin (1982), p. 79: "The distinction between these two types of theory (and theorists) had in fact been made by Poincaré . . . in his 1888–9 lectures on electricity and optics" – cf. the Introduction to Poincaré's *Electricité et optique: les theories de Maxwell et la théorie électromagnetique de la lumière*, J. Blondin (ed.), Paris: G. Carré, 1890.

[41] Duhem (1906), p. 19. Often in this work Duhem speaks as though he were advocating a particular way of viewing *any* physical theory whether it be explanatory or representational, when in fact he is advocating a way of theorising which excludes the former.

[42] Ibid., p. 19; next quote, pp. 20–21, 26–27.

tory manner a group of experimental laws. A false theory is not an attempt at an explanation based on assumptions contrary to reality; it is a group of propositions which do not agree with the experimental laws. *Agreement with experiment is the sole criterion of truth for a physical theory.* . . .

Thus, physical theory never gives us the explanation of empirical laws; it never reveals realities hiding under the sensible appearances; but the more complex it becomes, the more we apprehend that the logical order in which theory orders experimental laws is the reflection of an ontological order, the more we suspect that the relations it establishes among the data of observation correspond to real relations among things, and the more we feel that theory tends to be a natural classification.[43]

Duhem thus takes issue both with the use of picturable mechanical models, and with atomistic theories. The former satisfy only those with a "need to imagine concrete, material, visible, and tangible things." In this context Duhem also shows an aversion to a particular form of instrumentalism. For the tangibly inclined, theory is "neither an explanation nor a rational classification of physical laws, but a model of these laws, a model not built for the satisfying of reason but for the pleasure of the imagination. Hence, it escapes the domination of logic. . . . To a physicist of the school of Thomson or Maxwell, there is no contradiction in the fact that the same law can be represented by two different models."[44]

Atomistic theories, for their part, presuppose the metaphysics of atomism:

[E]ach time the fortunate daring of an experimenter discovers a new set of experimental laws, he will see the atomists, with feverish haste, take possession of this scarcely explored domain and construct a mechanism approximately representing these new findings. Then, as the experimenter's discoveries become more numerous and detailed, he will see the atomist's combinations get complicated, disturbed, overburdened with arbitrary complications without succeeding, however, in rendering a precise account of the new laws or in connecting them solidly to the old laws; and during this period he will see abstract theory, matured through patient labor, take possession of the new lands the experimenters have explored, organize these con-

[43] Ibid., p. 70.

[44] Ibid., p. 81; with reference to Poincaré, cf. also Duhem (1905), p. 294. Duhem's conception of models here corresponds to what will be treated as *analogues* beginning in Chapter 5 of the present work.

quests, annex them to its old domains, and make a perfectly coordinated empire of their union.[45]

Duhem's position leads him to criticise Galileo, whom he terms a realist, for suggesting that the earth *really* revolves about the sun, a view which transgresses Duhem's injunction not to take theories as telling us anything about reality. Duhem sides rather with Cardinal Bellarmine, whom he quotes: "It is one thing to prove that by assuming the sun at the center of the world and the earth in the heavens one saves all the appearances, and quite another thing to demonstrate that the sun really is at the center, the earth really in the heavens."[46]

Galileo's reply to this line of reasoning is:

> Granted, it is not the same thing to show that on the assumption of the sun's fixity and the earth's mobility [all?] the appearances are saved and to demonstrate that such hypotheses are really true in nature. But it should also be granted, and is much more true, that on the commonly accepted system there is no accounting for these appearances, whence this system is indubitably false; just so should it be granted that a system that agrees very closely with appearances may be true; and one neither can nor should look for other or greater truth in a theory than this, that it answers to all [*sic*] the particular appearances.[47]

Duhem believes Galileo's reasoning here actually to lead to the view Duhem himself endorses. It seems however that Galileo, unlike Duhem, does not intend that our knowledge cannot extend beyond the phenomenal realm, but that it is in precisely the manner here

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[45] Ibid., p. 304.

[46] Excerpted from letter of Bellarmine to Foscarini, dated April 12, 1615; pp. 121–125 in *Copernico e le vicende del sistema copernicano in Italia nella seconda metà del secolo XVI e nella prima del secolo XVII*, Rome, 1876; p. 107 in Duhem (1908a). Note that the issue here is not over the existence of 'theoretical' entities, but over two conceptions which are being considered only with regard to their kinematic differences.

[47] *Copernico* . . . (op. cit.), pp. 129–130; pp. 108–109 in Duhem (1908a). The notion of saving the phenomena originates with Plato and is almost always used in astronomical contexts. In Plutarch, for example, Cleanthes is depicted as thinking "that the Greeks ought to lay an action for impiety against Aristarchus the Samian on the ground that he was disturbing the hearth of the universe because he sought to save [the] phenomena by assuming that the heaven is at rest while the earth is revolving along the ecliptic and at the same time is rotating about its own axis." (Plutarch, p. 55).

favoured by Duhem that it does just that. Both Galileo and Duhem admit the *existence* of a non-phenomenal reality, and Duhem in the present context allows that we can *assume* it to be of a particular nature if by so doing the phenomena can be saved. Galileo says that our assumption may be true, which Duhem cannot deny, and he may be suggesting that the sole criterion for its being so is whether it saves *all* the phenomena.

These two views are very similar, and one would expect that the methodological consequences of adopting either would differ little from those of adopting the other. The question remains, however, whether the method of theorising advocated by Duhem would naturally lead to the making of assumptions of the sort at issue here, and whether the making of such assumptions, if only hypothetically, is not itself a form of realism.[48]

But then Duhem has himself expressed views which seem more sympathetic to realism than to positivism:

> If, on the other hand, [the physicist] yields to the nature of the human mind, which is repugnant to the extreme demands of positivism, he will want to know the reason for, or explanation of, what carries him along; he will break through the wall at which the procedures of physics stop, helpless, and he will make an affirmation which these procedures do not justify; he will be metaphysical.
>
> What is this metaphysical affirmation that the physicist will make, despite the nearly forced restraint imposed on the method he customarily uses? He will affirm that underneath the observable data, the only data accessible to his methods of study, are hidden realities whose essence cannot be grasped by these same methods, and that these realities are arranged in a certain order which physical science cannot directly contemplate.[49]

And though Duhem's original aim was to provide a conception of theory free from metaphysics, at the end of the appendix to his main work in the philosophy of science he gives expression to a conception reminiscent of Whewell: "[T]he physicist is compelled to recognize that it would be unreasonable to work for the progress of physical theory if this theory were not the increasingly better defined and more

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48 We shall return to this issue later in the present chapter when we take up the views of Bas van Fraassen.

49 Duhem (1905), pp. 296–297.

precise reflection of a metaphysics; the belief in an order transcending physics is the sole justification of physical theory."[50]

N. R. Campbell (1880–1949), like Duhem, considers the attaining of truth to be an important aspect of science, and the universal provision of assent to constitute the criterion of whether a judgement is part of the subject-matter of science.[51] But he attributes a different significance to this assent than does Duhem, taking it not to concern the realm of phenomena, but an independent reality. He says: "of course scientific reality must be independent of perception in a certain degree. It must be independent of the perceiver. This is what we mean when we say that there must be universal agreement concerning the laws which define reality." And with respect to Berkeley, considered e.g. by Mach to be a "far more logically consistent thinker than Kant,"[52] Campbell says: "Now his view of reality seems exactly contrary to that we have just considered; reality, so far from being independent of perception, consists of nothing else than its being perceived. To me, and I believe to most men of science, the idea is unintelligible."[53]

Campbell also largely removes himself from the view of Mill, attacking for example his (Humean) conception of causality as succession, pointing out that common sense shows it to be too wide:

> Thus, there is the hackneyed instance of the relation of day and night; each day invariably precedes the following night, and yet we do not regard that day as the cause of that night. Or, to take an even simpler example, if we allow a body to fall freely, its fall through the first foot precedes invariably its fall through the second; and yet the fall through the first foot is not usually regarded as the cause of the fall through the second. Every fact which precedes invariably another fact is not its cause.

Further, unlike Duhem, Mach, Poincaré and others of a phenomenalist persuasion, Campbell does not consider truth to be science's sole or ultimate aim. As he sees it, "The search for truth alone never has and never will lead to any science of value. The spirit which must be so carefully curbed in the search for truth must be given free

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50 Duhem (1908b), p. 335.
51 Campbell (1920), p. 21; quote following, p. 254.
52 Mach (1906), p. 368.
53 Campbell (1920), p. 255; next quote, p. 58.

rein when truth is attained."[54] What is required apart from truth is what Campbell terms *meaning*. He says: "a proposition is true in so far as it states something for which the universal assent of all mankind can be obtained; it has meaning in so far as it gives rise to ideas which cause intellectual satisfaction."

In this context Campbell distinguishes between laws and theories, saying that "The value of a law lies chiefly in its truth, that of a theory chiefly in its meaning."[55] Laws, he believes, are almost always suggested by theories,[56] which are of two kinds – 'mechanical' and 'mathematical' – corresponding to Duhem's explanatory and representational theories.[57] Campbell however considers both kinds to be explanatory – to provide intellectual satisfaction – not of individual phenomena, but of laws, it being "the business of science not only to discover laws but also to explain them." Mechanical theories accomplish this by indicating that the laws to be explained are analogous to other laws with which we are more *familiar*; and mathematical theories by unifying them under a more general proposition, thereby *simplifying* them. Campbell suggests however that this latter form of explanation might actually be better termed generalisation,[58] and that "For those to whom theories of the first type are of supreme importance, the importance of those of the second type consists chiefly in the prospect of their ultimate conversion to those of the first."

Where Duhem's distinction between explanatory and representational theories is clearly one between realist and phenomenalist approaches to science, Campbell's distinction is less evidently so. That he has something like this in mind, however, shows itself in his consideration of the relative values of 'mechanical' and 'mathematical' theories. Here he mentions such points as that mechanical theories, even if false, have nevertheless stimulated research, and that though

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[54] Ibid., p. 224; next quote, p. 218.
[55] Ibid., p. 219. We recall here Poincaré's allowing that 'indifferent hypotheses' (theories) might give satisfaction to the mind, while considering this to be of little consequence to science.
[56] Ibid., pp. 88, 104–105.
[57] Cf. ibid., pp. 150n., 151n.; quote following, p. 113.
[58] Ibid., p. 146; quote following, p. 148.

they may be more liable to error than mathematical theories, this can be excused by the fact that they state more. He also implies that a generalisation based solely on the phenomena to be explained does not provide those phenomena with the certainty they would receive were they explained by a conceptually independent theory. Thus:

> The test of 'purely phenomenal' gives exactly the opposite result to that imagined by its proposers; the more purely phenomenal a proposition is and the less the element of theory associated with it, the less is its certainty. From phenomena and phenomena alone we can deduce only negative conclusions, never a positive; it is theory which gives us positive certainty.[59]

While admitting that "A taste for mechanical theories can no more be forced by argument on one who does not possess it than a taste for oysters," Campbell considers what might lie behind the intellectual satisfaction so many scientists find in mechanical theories. Taking what he considers to be a paradigmatic case, in which the laws to which analogy is made in a mechanical theory are Newton's laws of motion, he suggests that the satisfaction provided by the theory stems mainly from its involving the notion of bodies moving under the influence of forces, which we can each of us relate to our own voluntary actions of causing physical objects to move.[60] Unlike most authors, however, Campbell does not present the issue as though the decision lies solely with us, but grants a role also to nature, recognis-

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[59] Ibid., p. 153; next quote, p. 151.

[60] Ibid., p. 155; see also pp. 63–64. This view is also expressed by Whewell, who examines the etymology of the term "force": "The original meaning of the Greek word [for *Force*] was a *muscle* or *tendon.* Its first application as an abstract term is accordingly to muscular force:

> Then Ajax a far heavier stone upheaved, He whirled it, and impressing Force intense
> Upon the mass, dismist it.

"The property by which bodies affect each other's motions, was naturally likened to that energy which we exert upon them with similar effect: and thus the labouring horse, the rushing torrent, the descending weight, the elastic bow, were said to exert force. . . .

"Thus man's general notion of force was probably first suggested by his muscular exertions. . . ." (1847), Part 1, pp. 185–186.

ing that "It is possible that in the future we shall have to choose between the advantages of simplicity and those of familiarity."[61]

In keeping with the view of Whewell,[62] and in direct opposition to Mill, Campbell sees formal logic as being of no relevance to science.

> Of course the province and power of logic have been very greatly extended in recent years, but some of its essential features . . . have remained unchanged; and any process of thought which does not show those features is still illogical. But illogical is not synonymous with erroneous. I believe that all important scientific thought is illogical, and that we shall be led into nothing but error if we try to force scientific reasoning into the forms prescribed by logical canons. [S]cientific thought is fundamentally different from logical thought.[63]

Campbell supports his conviction by suggesting that where for logicians *words* or equivalent symbols are the instruments by means of which the process of thought is conducted, in science words have nothing to do with the operation whereby one passes from one set of thoughts to another. For the scientist, thoughts are primary, and words are simply a convenient means of calling thoughts to mind. As regards e.g. the idea of providing formal definitions, for example one of *silver*:

> No student of science has ever felt the smallest need for a formal definition of silver; our words are perfectly effective in calling up the thoughts we desire without one, and in admitting the right of anyone to ask for one we are encouraging a very dangerous delusion. . . . When we are merely trying to call up ideas, a jumble of words quite outside all the rules of grammar may often be more effective than the most accurately turned sentence.[64]

And, for example, whether two statements call up essentially similar ideas cannot be ascertained by studying their grammatical form. In summary, Campbell concludes that "the reasoning processes of classical logic have no application to science."

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[61] Campbell (1920), p. 157.
[62] See e.g. p. 67, n. 31, of the present work.
[63] Campbell (1920), p. 52. This, together with the rest of Campbell's text cited here, should dispel the myth that Campbell is one of the fathers of logical empiricism.
[64] Ibid., p. 53; next quote, p. 38.

Quite the contrary of this view was adopted by the more influential members of the **Vienna Circle** and their successors, who wrote as though they believed science to be essentially a linguistic phenomenon, and all epistemological relations to be between sentences in 'the language of science.' In conjunction with this, according to their programme of logical *positivism* – later to evolve into logical *empiricism*,[65] the subject-matter of science was to consist of what can be directly observed. Thus, theoretically, the 'language of science' was to consist of logical operators and terms referring to what is directly observable, while practically, it was taken to be that part of everyday language capable of being represented in terms of first-and second-order predicate logic.

This basic view was so influential that even those who considered themselves the severest critics of logical empiricism, such as Karl Popper and Imre Lakatos, did not challenge the programme as regards these fundamentals.[66] In fact, the view has been so widely held during the twentieth century as practically to constitute the discipline of the philosophy of science itself; and its influence on those who believe themselves to have moved beyond it is still strong today, as will be seen below.

What is remarkable from the point of view of the survey of the present chapter is the dogmatic acceptance of this approach in the early work of such of its proponents as **Rudolf Carnap**, **Ernest Nagel** and **Carl Hempel**, who not only provide no argument for it, but tacitly assume even theoretical science, as actually practised, to be in keeping with it. We shall here look at the work of **Hempel**, in which themes of relevance to the empiricism/realism issue are most clearly developed.

Hempel is perhaps best known in connection with the deductive-nomological model of scientific explanation, his general account of which he admits, citing Mill and others, to be 'by no means novel.' Though he remarks in his text that there exists a method of explaining phenomena using e.g. micro-theories, thereby obtaining insight into certain inner mechanisms, he believes this method to be

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65 On the conceptual development of logical empiricism, see Dilworth (1994a).

66 In this regard, see ibid.

but an instance covered by his own conception.[67] Apparently unaware of the existence of an issue in this regard, he assumes the extreme empiricist stance that causality is captured by statements of empirical regularity, that (causal) explanation in actual science consists in nothing other than systematisation under such statements, and that explanation and prediction have the same (syllogistic) form.[68]

Thus on Hempel's way of thinking, "Scientific laws and theories have the function of establishing systematic connections among the data of our experience, so as to make possible the derivation of some of those data from others." Some ten years after making this claim, however, he wonders: "Why should science resort to the assumption of hypothetical entities when it is interested in establishing predictive and explanatory connections among observables?"[69] And after lengthy considerations in terms of elementary and second-order logic, he concludes that it is not in fact the sole purpose of scientific theory to establish deductive connections among observation sentences – though he does not tell us what other purpose he thinks it might have.

Later in his career, when he recognises an alternative to his original view, Hempel argues against what he believes this alternative to be. While admitting that actual explanations of working scientists are not in keeping with his covering-law model, he claims this to be due to their being formulated with a particular kind of audience in mind. Thus, according to Hempel, such explanations are not objective, and consequently their existence does not suffice to show his model of (audience-independent) explanation to be "hopelessly inadequate."[70]

In a similar vein he criticises the idea that explanation involves what he terms a 'reduction' to the familiar, since: "what is familiar to one person may not be so to another." Furthermore, "instead of reducing the unfamiliar to the familiar, a scientific explanation will often do the opposite: it will explain familiar phenomena with the

[67] Hempel & Oppenheim (1948), p. 259.
[68] Ibid., pp. 246–251; quote following, p. 278.
[69] Hempel (1958), p. 179.
[70] Hempel (1965), pp. 427–428; next quote, p. 430.

help of theoretical conceptions which may seem [*sic*] unfamiliar and even counter-intuitive, but which account for a wide variety of facts and are well supported by the results of scientific tests."[71] However, as regards what should constitute an adequate scientific explanation, this just begs the question. So too does Hempel's stating, without drawing an essential distinction between models and analogies, that "For the systematic purposes of scientific explanation, reliance on analogies is . . . inessential and can always be dispensed with."

Though in still later work Hempel does come to admit that models as distinct from analogies are of more than heuristic value,[72] and that his conception of objective cognitive significance is much too restrictive,[73] he never makes the step to realising that his problems of scientific explanation and of theoretical entities are but manifestations of a deeper philosophical issue concerning empiricism and realism. This deeper issue is of central concern, however, to **Rom Harré**, who reacts strongly to the underlying presuppositions of logical empiricism, and develops a comprehensive realist conception of science in its stead.

The empiricists and Harré both believe that there is nothing fundamentally wrong with the way science is actually practised. But for Harré actual scientific practice is more important than it is for the empiricists, and he stresses that "the philosophy of science must be related to what scientists actually do, and how they actually think."[74]

In this light Harré, like Whewell and Campbell, takes the empiricists to task for their unquestioned assumption that formal logic provides the appropriate tools for the analysis of science: "Traditional logic recognises only truth and falsity, and these are very peripheral concepts in science. We must not fall into thinking that an intellectual process, like the process by which theories are formulated, is not capable of an analysis to reveal its rationale, just because the simple principles of truth and falsity cannot capture it."[75]

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[71] Ibid., p. 431; next quote, p. 439.
[72] Hempel (1970), pp. 157–158.
[73] Hempel (1973), p. 377.
[74] Harré (1972), p. 29.
[75] Harré (1970a), p. 281. Wesley Salmon advances a similar view: "attempting to give a logical characterization of scientific explanation is a futile venture, and..."

Harré questions not only the notions of truth and falsity as engendered by logic, but also the formalistic idea that science aims to establish deductive relations among statements.

> If it is maintained that criticisms of deductivism which hinge on the divergence between that theory and practice are beside the point since the theory depicts an ideal form of knowledge, then deductivism as an ideal should be free from 'problems.' But it is just the very theory which runs fastest into difficulties. To hold that [the idea that] scientific knowledge should develop towards a deductively organized system of conditional statements, describing regularities of succession among types of events, is the *logical* ideal, leads us straight into a situation in which we have to say that in their ideal form scientific theories are not confirmable (Problem of Induction), are about regularities of sequences of types of events indistinguishable from accidental sequences (Problem of Causality), and change their plausibility relative to evidence under purely logical transformation (Problem of Instance Confirmation), and that scientific laws are indistinguishable logically from accidental generalizations (Problem of Natural Necessity), and are such that their component predicates are logically independent (Problem of Subjunctive Conditionals).[76]

Rather than aiming to systematise descriptions of phenomena, according to Harré, "science *above all* seeks to answer the question *why?* . . . The main problem of science is to discover by what means the non-random patterns in nature are produced."[77] This involves more than the determining of empirical correlations.

> It is hardly a scientific explanation of phenomena merely to describe some other phenomena with which they are associated, unless one has some conception of how this association comes about. Then that conception is really what is doing the explaining and is the heart of the theory. For instance, it is not a scientific explanation of the Aurora to instance the increased activity of sun-spots which regularly antedate the appearance of the glow in the sky. A scientific explanation will tell you why and how the sun-spots are associated with the Aurora, and this involves discussions of the nature of sun-spots and of the paths of electrons which leave the sun. These discussions are relevant only because we have some idea about the nature of the Aurora,

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little of significance can be said about scientific explanation in purely syntactical or semantical terms." (1984, p. 240).

[76] Harré (1970b), pp. 28–29.

[77] Harré (1970a), p. 279.

and know a good deal about the discharge of electricity in tenuous gases. In short, to explain the Aurora we describe the mechanism which produces the phenomenon, and so come to see *why* sun-spots are associated with the Aurora.[78]

At another point Harré clarifies what he means by the term "mechanism":

In ordinary English this word has two distinct meanings. Sometimes it means mechanical contrivance, a device that works with rigid connections, like levers, the intermeshing teeth of gears, axles, and strings. Sometimes it means something much more general, namely any kind of connection through which causes are effective. It is in the latter sense that I mean the word, [and it] is in the latter sense that the word is used in science generally, in such diverse expressions as the mechanism of the distribution of seeds and the mechanism of star formation.

And, for Harré, mechanisms are depicted by *models*:

Scientists, in much of their theoretical activity, are trying to form a picture of the mechanisms of nature which are responsible for the phenomena we observe. The chief means by which this is done is by the making or imagining of models. Since enduring structures are at least as important a feature of nature as the flux of events, there is always the chance that some models can be supposed to be hypothetical mechanisms, and that these hypothetical mechanisms are identical with real natural structures.[79]

Following this line, Harré also suggests prediction and explanation in actual science to be more fundamentally different than is recognised on the logical empiricist view. Sometimes prediction is possible where explanation is not, and vice versa:

Consider the course of a disease. Long before any explanation of what happens is available the empirical knowledge of experienced doctors may enable them to foretell the course of the disease with great accuracy from the symptoms. We would hardly call the description of the symptoms the explanation of the later stages of the disease. Nor indeed would we call the predictions made from nautical almanacs the explanation of the risings, settings, and conjunctions of the heavenly bodies, Characteristically, giving an explanation involves describing the mechanism, usually the causal mecha-

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[78] Harré (1972), p. 24; next quote, p. 118.

[79] Harré (1970b), pp. 34–35.

nism, responsible for a series of happenings, and this may not be enough to predict just what will happen. We know the causal mechanism of evolutionary change pretty well, but until we actually observe what happens we are unable to predict the appearance of new forms of plants and animals, because of the presence of the random (unpredictable) element of mutation in the system.[80]

So, for Harré, providing a scientific explanation consists essentially in depicting a relevant causal mechanism by means of a model. But the empiricist can respond to this approach by saying that the analysis it provides has not gone any deeper than that in terms of constant conjunction, this notion simply reappearing in the conception of the functioning of the causal mechanism.

Note that such a criticism does not question that scientific investigation is actually conducted in the way depicted by Harré, and if we grant that his characterisation is correct, then the criticism should be directed at scientists themselves. But since Harré accepts his conception of science as also being that of the best way to go about investigating the world, he might be expected to defend it on this point.

Harré does provide a reply to the empiricist here, namely that the acceptability of the depiction of a causal mechanism is not dependent on its suggesting a constant conjunction, but in there being "a pair of components, in [the] mechanism, which are related by the basic interactions of the science we are using. Any scientific enquiry with which the reader is familiar can provide instances of this procedure, e.g. physics supplies the mechanisms for chemical phenomena, chemistry for many physiological phenomena, etc., etc."[81]

Harré's realist conception has received relatively little attention in mainstream philosophy of science, in spite of the fact that the empiricism/realism issue has once more become a topic of popular concern. **Bas van Fraassen**, for example, an influential contributor to the current debate, takes no notice of Harré's views in his main work devoted to the issue. In it van Fraassen argues for what he terms 'constructive empiricism,' according to which:

[80] Harré (1972), p. 56.
[81] Harré (1970b), pp. 109–110.

> Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate . . . with the preliminary explication that a theory is empirically adequate exactly if what it says about the observable things and events in this world, is true – exactly if it 'saves the phenomena.'[82]

The implication here is that what the theory 'says' about *un*observable things and events need not be true, and so a distinction must be made between what is observable and what is not. In this regard van Fraassen claims that though "'observable' is a *vague predicate*,"[83] the distinction can nevertheless be drawn. "A look through a telescope at the moons of Jupiter seems to me a clear case of observation, since astronauts will no doubt be able to see them as well from close up. But the purported observation of micro-particles in a cloud chamber seems to me a clearly different case."

He contrasts the above position with what he calls scientific realism, namely the view that "Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true."[84]

Each of the views depicted by van Fraassen has two parts, one concerning the aim of science and the other concerning people's beliefs. The first is apparently intended as a factual claim about science – "The question is what aim scientific activity has"[85] – while the second seems more prescriptive, and involves the (counter-intuitive) assumption that we can *decide* "how much we shall believe when we accept a scientific theory."

Judging from his terminology in his depiction of constructive empiricism, it would appear that van Fraassen wishes to defend a position similar to Duhem's with regard to the Bellarmine-Galileo dispute. That is, he would admit that it is legitimate to construct theories referring to what transcends experience, but that the correctness of such a theory with respect to empirical matters would not imply its correctness with respect to the realm beyond experience.

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[82] van Fraassen (1980), p. 12. As in the case of Duhem, it is not altogether clear here how 'saving' the phenomena is to differ from hypothetically explaining them.

[83] Ibid., p. 16; next quote, pp. 16–17.

[84] Ibid., p. 8.

[85] Ibid., p. 18; next quote, ibid.

There is a difference between van Fraassen's and Duhem's views however which is hinted at in the former's suggestion that 'observability' is a vague predicate. Unlike Duhem, Mach, and other phenomenalists, who claim that all that can be known are phenomena (and the relations between them), van Fraassen adopts a form of *naive realism* which admits that we can know of the existence of actual physical objects if they are observable. This means that we must distinguish here between the trans-empirical or real world, distinguished from the phenomenal world as a matter of principle, and van Fraassen's unobservable world, which is neither more nor less real that his observable world, and is distinguished from it only contingently. And we note that this is what leads van Fraassen to suggest that the notion of observability is essentially vague.[86]

But, even setting this realist element in van Fraassen's thinking aside, as intimated earlier with regard to Duhem, the very idea of 'saving the phenomena,' in that it admits the *relevance* of reference to what is beyond experience, is actually more a form of realism than of empiricism. Furthermore, as regards van Fraassen's depiction in particular, what a theory referring to unobservables 'says about observable things and events' is that they take the form they do as a consequence of the nature and behaviour of the unobservables. So his own manner of expression suggests that a theory's being 'empirically adequate' implies its also being 'theoretically adequate,' i.e. that it correctly depict what is unobservable.

Not only is van Fraassen's 'constructive empiricism' actually a form of realism, but his 'scientific realism' is really a form of empiricism. He claims that the realist considers the aim of science to be to provide 'a literally true story of what the world is like,' i.e. to provide a correct description. But as Duhem recognised in distinguishing between explanatory and representational theories, for the realist the aim of science, as far as its theoretical aspect is con-

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86 On this point, cf. Ernan McMullin (1984a), p. 20: "Why could there not, in principle, be organisms much smaller than we, able to perceive microentities that for us are theoretical and able also to communicate with us? Is not the notion 'observable in principle' hopelessly vague in the face of this sort of objection? How can it be used to draw a usable distinction between theoretical entities that do have ontological status and those that do not?"

cerned, is primarily to explain and not to describe. It is the empiricist who sees truth as the sole or primary aim of science, and who wants to limit 'theorising' to the establishment of the non-controversial, representational type of theories where he believes such truth can be attained.[87] The realist, on the other hand, in attempting to explain, is forced to speculate. But it is to be admitted that in order for the realist's proposed explanation actually to be an explanation, the trans-empirical realm depicted by his or her theory should *in essence* be as the theory suggests it to be; and to this extent the realist aims to have theories which correctly depict the trans-empirical. But for the realist, unlike the empiricist, 'truth' is not the sole aim of science, and a theory which is not *known* to be correct will be accepted, so long as there is no reason to believe it to be e.g. essentially incorrect, or nonsensical, or incompatible with some other theory the realist accepts.[88]

van Fraassen's characterisations of 'constructive empiricism' and 'scientific realism' thus indicate that he has failed to come to grips with the real issue. Perhaps most notable in this regard is his lack of awareness of the fundamental idea that the primary purpose of constructing theories which depict unobservable entities is not to de-

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[87] It is of some interest to note here that, as has been pointed out by Brian Ellis (1985, pp. 55, 56) and admitted by van Fraassen (1985, pp. 275, 287), the latter's view originates with studies of representational or systemic theories as versus causal or explanatory theories. On having this be pointed out, however, van Fraassen, rather than consider whether there may be epistemologically important differences between the two kinds of theory, instead takes issue with the (metaphysical) notion of causality. Not seeing it as expressing anything more than asymmetric regularity, he believes it can be eliminated; and in the case of science he asserts that to see causal theories in the same way as representational theories "gives us the best hope of eliminating metaphysics from our interpretation of science" (ibid., p. 288), as though this were a goal shared by parties on both sides of the debate.

[88] On this point cf. McMullin, who supports a realist view: "I do not think that acceptance of a scientific theory involves the belief that it is true. Science aims at fruitful metaphor and at ever more detailed structure. To suppose that a theory is literally true would imply, among other things, that no further anomaly could, in principle, arise from any quarter in regard to it. At best, it is hard to see this as anything more than an idealized 'horizon-claim,' which would be quite misleading if applied to the actual work of the scientist." (1984a, p. 35).

scribe those entities, but to explain observable phenomena.[89] In fact, we might say that it is just this distinction that van Fraassen is himself groping towards in his characterisations of constructive empiricism and scientific realism, the former emphasising the explanatory function of a theory, and the latter the theory's ability to provide a correct description of the world.

A perusal of van Fraassen's work shows that the basic reason for this misunderstanding is that his thought is wholly restricted to the conceptual framework of logical empiricism. This is revealingly expressed, for example, in his liberal and unreflecting use of the notion of 'the language of science,'[90] a notion which arose with logical empiricism, and whose viability is far from evident to anyone critical of its fundamental presuppositions. But more substantially, his depiction of realism primarily in terms of truth is realism as it might be conceived *within* logical empiricism, where the expressions of both laws and theories are assumed to be statements having 'truth-values.' Further, van Fraassen himself conceives of theories in this way throughout, without noting that this conception is highly problematic with respect to the realism/empiricism issue.[91] Nor, like the early Hempel, does he realise that the notions of cause and explanation are similarly problematic; for though he treats of them, he does not touch on the question central to the debate, namely whether the empirical is to be conceived of as being causally based in the trans-

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[89] This point has been widely missed, even by those advocating various forms of realism. Thus with regard to 'truth,' for example, even if we were to describe realism as involving a search for true theories, the truth of such theories would not lie simply in their providing correct descriptions of the trans-empirical, but in their providing correct descriptions of how the empirical is a manifestation of the trans-empirical.

[90] Cf. e.g.: "Not every philosophical position concerning science which insists on a literal construal of the language of science is a realist position." van Fraassen (1980), p. 11. It may be noted that Whewell also uses this expression, though not in the all-embracing logical empiricist sense, but rather in referring to the technical terms of science.

[91] Cf. again McMullin: "The realist would not use the term 'true' to describe a good theory. He would suppose that the structures of the theory give some insight into the structures of the world. But he could not, in general, say how good the insight is. He has no independent access to the world, as the antirealist constantly reminds him." (1984a, p. 35).

empirical, or whether scientific explanation requires reference to such a realm.

Other contemporary writers, such as **Larry Laudan**, have had similar difficulty in coming to grips with the issue. According to Laudan "the realist maintains that the goal of science is to find ever truer theories about the natural world;"[92] and "Realists in particular argue that scientists should and do seek true theories and, moreover, that they often find them (or at least close approximations to them)." At another place he criticises what he considers to be a form of realism according to which, among other things, "Scientific theories (at least in the 'mature' sciences) are typically approximately true, and more recent theories are closer to the truth than older theories in the same domain."[93]

Here too theories are conceived along logical empiricist lines to be entities which are either true or false, and whose function is to describe rather than explain; and Laudan's reliance on this tradition is also manifest in his frequent use of such turns of phrase as "any language rich enough to contain negation,"[94] and "even highly successful theories may well have central terms that are nonreferring." So, much of what has been said above with regard to van Fraassen can also be said of Laudan, including that his arguments are against realism as it might be seen *within* the context of logical empiricism, and that they in fact support realism.

We see this distortion when we look, for example, at Laudan's discussion regarding theories which have largely had correct empirical consequences, but whose trans-empirical ontologies we do not today accept. In his terms, this is a consideration of the realist claim that "If a theory is explanatorily successful, then it is probably approximately true."[95] With the ultimate aim of supporting the thesis that realism has not in fact been a dominant force in science, he cites

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[92] Laudan (1984a), p. 106; next quote, p. 104. Note that it is not being claimed here that no one holds views similar to those Laudan is criticising. But those 'realists' to whom he refers are all his contemporaries, and he does not question their logical empiricist presuppositions any more than they do themselves.

[93] Laudan (1984b), p. 219.

[94] Ibid., p. 224; next quote, p. 227.

[95] Ibid., p. 228.

such examples as the crystalline-sphere theories of ancient and medieval astronomy, the phlogiston theory, and the theory of the electromagnetic ether. Laudan's argument here consists essentially in pointing out the fallacy of asserting the consequent (*A* implies *B*; *B*; therefore *A*),[96] coupled with his historical claim that such theories outnumber theories whose ontologies we today accept by about six to one.

First, Laudan does not distinguish between those of the theories he cites which were put forward with the intention that their theoretical entities be considered actually to exist, and those which were not. Eudoxus, for example, warned against taking the crystalline spheres of his astronomical theory as actually existing. Second, it should be pointed out that such trans-empirical theories as Laudan refers to would never even have been proposed had their propounders not adopted a realist attitude. Third, so long as the theoretical entities to which the realist's theory refers are inaccessible, he should recognise as well as anyone that the theory is merely hypothetical, that is, that it is a theory and not a fact. Why should he be expected to claim it to be correct as a whole (to be true or approximately true), when he sees it in this light?[97] This idea of *establishing* the correctness of theories, expressed in terms of determining them to be *true*, is rather that of the logical empiricists, who have this aim with regard to theories conceived as universal statements (which has led to their problem of confirmation). Fourth, it could be argued that each of the theories Laudan mentions has constituted a step on the way to what have generally been considered scientific advances of either an empirical or theoretical nature. Fifth, the theories Laudan refers to are today generally considered to be incorrect with regard to the ontologies they suggest, not because this (realist) way of thinking is itself mistaken, but on the contrary, because this way of thinking has led to a revision of judgement. It is largely because technological advance has shown those particular ontologies not to exist and/or because theoretical advance has led to more realistic ontologies, that

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[96] What is missed by Laudan here (but also by Galileo as quoted above), as will be treated in detail in the following chapters, is that there are fundamental constraints on theorising other than simply that a theory produce correct empirical results.

[97] Cf. above, p. 40, n. 91.

the ontologies of the theories Laudan mentions have been discarded. And sixth, as a matter of fact many theories of this type, again through technological advance, have been shown to be essentially correct.

In sum, what is at issue is the very idea of conceiving of the empirical as having a causal basis in the trans-empirical. In the face of Laudan's argument a realist might well respond that the fact that theorising of a realist type has been done on such a scale as suggested by Laudan's examples supports the view that realism *as a matter of fact* has had an important place in science; that such theorising has paved the way to scientific advance supports the view that realism *should* have an important place in science; and that technological advance has actually shown many such theories to be essentially correct should *secure* realism's place in science.

As is becoming clear, it is unfortunately the case that the modern debate concerning realism and empiricism is largely being carried on within the context of the basic presuppositions of logical empiricism,[98] with realism being misleadingly described as essentially the view that scientific theories are true or approximately true.[99] A variation on this confusion is also manifest in the work of **Nancy Cartwright**, who believes realism in physics to entail what she terms the 'facticity' view of *laws*, according to which all laws of physics are true. She says: "The view of laws with which I begin – 'Laws of nature describe facts about reality' – is a pedestrian view that, I imagine, any scientific realist will hold."[100] In further considering what she takes to be the realist view she says: "A long tradition dis-

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[98] As suggested by McMullin: "[E]mpiricism is one of the most commodious of all philosophic mansions; so large is it, indeed, that few ever find their way outside it." (1974, p. 129).

[99] This is also the basic view of William Newton-Smith, who believes that " 'Realism' has been used to cover a multitude of positions in the philosophy of science, all of which, however, involve the assumption that scientific propositions are true or false where truth is understood in terms of a cleaned-up version of the correspondence theory of truth." (1981, pp. 28–29). This approach, together with an emphasis on the notion of *sentences*, may also be found in Newton-Smith's more recent work: "Some [scientific realists] state that the sentences of science, be they theoretical or observational, are true or false in virtue of a correspondence or lack of it between what the sentence says and how the world is." (1989, p. 31).

[100] Cartwright (1983), p. 55.

tinguishes fundamental from phenomenological laws, and favours the fundamental. Fundamental laws are true in themselves; phenomenological laws hold only on account of more fundamental ones. This view embodies an extreme realism about the fundamental laws of basic explanatory theories."[101] And with regard to her main work she states: "The primary aim of this book is to argue against the facticity of fundamental laws."

Nowhere, however, does Cartwright identify even one individual who is to belong to the tradition she mentions. The only person the present author can think of who might possibly be ascribed such a view is Whewell, but his name does not appear in the index of Cartwright's work, and her discussion does not meet the issues Whewell raises. Unlike van Fraassen and Laudan, who are arguing against real opponents, it would appear that Cartwright is fighting a straw man. But her straw man, like their opponents, looks more like an empiricist than a realist.

In fact the whole of Cartwright's critical discussion is based on a fundamentally logical empiricist conception of science. She focuses on and accepts without question the notion of truth; she similarly accepts explanation to be paradigmatically performed by laws, not theories; and she does not at all question whether there is an important epistemological distinction between explanation and description, or between explanation and prediction.

Thus, with regard to causality for example, she assumes causal explanation to be "explanation by causal law,"[102] (where causal laws are those which "have the word 'cause' – or some causal surrogate – right in them"). She distinguishes such explanation from theoretical explanation, and speaks of "the tension between causal explanation

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[101] Ibid., p. 100; cf. also p. 65; next quote, p. 152. It may be mentioned that Cartwright's 'fundamental laws,' in that they are to hold between theoretical entities, bear a certain affinity to scientific causal principles, to be treated in the sequel.

[102] Ibid., p. 29; next quote p. 21. In this general regard cf. also McMullin's critical comments in his (1987), p. 69: "[S]he makes a number of questionable assumptions about the nature of explanation in science. . . . She assumes, for example, that the basic explanatory function in science is carried by laws, not by theories, and that the criterion of explanation is what she calls 'organizing power.'. . ."

and theoretical explanation,"[103] where causal explanations "have truth built into them."

In her more positive contribution however, which she terms the 'simulacrum' account, she begins moving over to a view of science very similar to Harré's, and at the same time revising her conception of causality. She says: "On the simulacrum account, to explain a phenomenon is to construct a model which fits the phenomenon into a theory. The fundamental laws of the theory are true of the objects in the model, and they are used to derive a specific account of how these objects behave."[104] Furthermore, "The simulacrum account is not a formal account. It says that we lay out a model, and within the model we 'derive' various laws which match more or less well with bits of phenomenological behaviour."

And with regard to causality she says: "One important thing we sometimes want to do is to lay out the causal processes which bring the phenomena about, and for this purpose it is best to use a model that treats the causally relevant factors as realistically as possible. . . . But this may well preclude treating other factors realistically."[105] And: "All sorts of unobservable things are at work in the world, and even if we want to predict only observable outcomes, we will still have to look to their unobservable causes to get the right answers." *This* is much closer to realism than what Cartwright takes realism to be, viz. the view that 'fundamental' laws must be true. But Cartwright is not aware that her view here is not novel, and towards the end of her book she still thinks that "The emphasis on getting the

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[103] Ibid., p. 12; next quote p. 91.

[104] Ibid., p. 17; next quote p. 161.

[105] Ibid., p. 152; next quote p. 160. It should be noted that realist model building requires abstraction and idealisation, even with regard to causally relevant factors. Cf. Harré (1970b), pp. 41–42 for his treatment of these notions. There he says, for example: "An abstraction has properties . . . fewer than its source-subject. Which properties are chosen depends largely on the purposes for which the model is created. This is because the properties which are not modelled are those which are irrelevant, and relevance and irrelevance are relative to purposes." (p. 42).

Cartwright frequently makes reference, either implicitly or explicitly, to idealisation in her criticism of what she takes to be realism. But see Nowak (1980) for an anti-*empiricist* philosophy of science based on idealisation. This topic is treated in Chapter 5 of the present study.

causal story right is new for philosophers of science; and our old theories of explanation are not well-adapted to the job."[106]

Another contemporary philosopher, **Ian Hacking**, distinguishes between what he sees as being two forms of realism, one which he calls realism about entities and the other realism about theories.

> Realism about theories says that we try to form true theories about the world, about the inner constitution of matter and about the outer reaches of space. . . . Realism about entities – and I include processes, states, waves, currents, interactions, fields, black holes, and the like among entities – asserts the existence of at least some of the entities that are the stock in trade of physics.[107]

It is Hacking's intention to defend this latter sort of realism, and the main way he does this is by suggesting that such things as electrons must exist since as a matter of fact they are manipulated in laboratory situations. "Electrons are no longer ways of organizing our thoughts or saving the phenomena that have been observed. They are now ways of creating phenomena in some other domain of nature. Electrons are tools."[108]

But as an argument concerning the existence of electrons this just begs the question. Experimenters do not manipulate electrons – and if we admitted that they did we would certainly also have to admit that electrons exist – what they manipulate are pieces of experimental apparatus. They *assume* such manipulations to have an effect on a trans-empirical reality of which electrons are a part – and so they are themselves realists; but nothing in Hacking's argument supports a stronger conclusion than this. This conclusion *is* noteworthy though as regards the extent to which actual scientific practice is realist in its orientation.

This question-begging carries over to Hacking's distinction between what he sees as being two sorts of realism, for he believes that what he calls realism about entities is justified mainly by the fact that such entities are actually manipulated by experimenters. But since as a matter of fact such entities as electrons are not actually

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106 Cartwright (1983), p. 162.
107 Hacking (1984), p. 155.
108 Ibid., p. 156; also in Hacking (1983), p. 263.

manipulated, his distinction falls in the wrong place for his own purposes. A distinction which *might* be made in this context would be between realism about theoretical entities,[109] such as electrons, and realism about what might be termed empirical entities, such as laboratory instruments. A pure phenomenalist such as Mach should deny the independent existence of both sorts of entity, while a naive realist such as van Fraassen might deny the former while accepting the latter.

While Hacking's views are not so imbued with logical empiricism as are those of most of his contemporaries, he nevertheless conceives of what he calls realism about theories in empiricist terms (that we try to form true theories), and quite misses the point that the very formulation of theories is a realist occupation whose aim, as mentioned above, is not primarily to describe the trans-empirical but to explain the empirical.

To conclude this survey we shall take a brief look at the thoughts of **Hilary Putnam** on the issue. In a recent contribution, with the title 'What is Realism?,' he expresses a view which is steeped in the logical empiricist tradition. An immediate indication of this is the recurrence of such phrases as "true observation sentences," and "observational vocabulary," as well as e.g. his suggestion that "we formalize empirical science or some part of empirical science – that is, we formulate it in a formalized language *L*, with suitable logical rules and axioms, and with empirical postulates appropriate to the body of the theory we are formalizing."[110] Accordingly, his conception of realism is also thoroughly empiricist.

With regard to realism, Putnam states: "Whatever else realists say, they typically say that they believe in a 'correspondence theory of truth.' " The correspondence theory of truth to which Putnam is referring is based on the ideal-language, referential-theory-of-meaning idea that all terms occurring in true sentences should have counterparts in reality. On this view, realists claim this correspondence to

[109] Here one should keep in mind a further distinction, namely between theoretical entities and hypothetical entities. While theoretical entities are normally hypothetical when first introduced, they need not remain so – electrons perhaps being a case in point.

[110] Putnam (1984), p. 140; next quote, p. 148.

obtain even when the sentence in question cannot be verified or falsified, while empiricists limit meaningful sentences to those whose correspondence can be empirically checked.

What we see here, however, is not that empiricists and realists are being distinguished with regard to *correspondence*, but with regard to verificationism or falsificationism on the one hand, and some transcendent view on the other. In other words, on Putnam's own way of thinking, both empiricists and realists can hold to a correspondence theory. What distinguishes them is whether they are willing to think in terms of a trans-empirical reality.

In the next chapter the first move will be made in the laying out of an alternative conception of science against the background of which the empiricism/realism issue can be resolved. It will consist in the depiction of the core of modern science as consisting in certain particular principles.