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Fall and Rise of Aristotelian Metaphysics in the Philosophy of Science

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Abstract The paper examines the fortunes of Aristotelian metaphysics in science and the philosophy of science. It considers the Enlightenment claim that such a metaphysics is fundamentally unscientific, and that its abandonment was essential to the scientific revolution. The history of the scientific revolution and the metaphysical debates involved in it is examined, and it is argued that the eclipse of Aristotelian views was neither complete, nor merited. The evolution of Humeian and positivist accounts of science is described, and it is shown how the severe problems with these accounts, together with a revival of Aristotelian concepts in philosophy, have led to the rebirth of broadly Aristotelian accounts of the metaphysics underlying science.

1 The Enlightenment Dismissal of Aristotelian Metaphysics, and its Neo-Aristotelian Opponents

A certain picture of the history of science runs like this. Science, in the Western world, was first developed by the ancient Greeks. The science of antiquity fizzled out, however, and was extinguished during the Dark Ages and the anti-intellectual Middle Ages. With the Renaissance, ancient science was rediscovered, and was profoundly transformed by the scientific revolution of the seventeenth century. This revolution established science as we know it, and this science brought along with it revolutionary change in human possibilities and our way of understanding the universe. The crucial positive features that made possible the scientific revolution were the notions of the mathematical description of nature as central to science, and the centrality of experiment in establishing and choosing between scientific theories. The crucial negative advance was the rejection of an Aristotelian understanding of the world, which involved a pseudo-scientific metaphysics that was incompatible with genuine science. This picture is an essential component of the larger picture of history promoted by the Enlightenment, in which Aristotelian notions are

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identified as part of anti-scientific scholastic obscurantism, whose overthrow by true science was a crucial part of the victory of reason over Catholicism (in the view of Protestant Enlightenment figures) or Christianity (in the view of deists, agnostics and atheists).

The greater one's knowledge of intellectual history, the less of this account one is inclined to accept. It is still however a picture that has considerable influence. The object of this paper is to give an idea of the current revival of Aristotelian metaphysical themes in the history and philosophy of science, and to argue against this picture of science and its history.

The Aristotelian understanding under discussion is not restricted to the views of Aristotle himself, but extends to the wide tradition that was inspired by him and accepted his basic ideas. Within this tradition, we can distinguish a metaphysics, and a scientific account of the world. (The divisions are not meant to correspond to the way Aristotle himself categorised his thought.) Metaphysics, roughly speaking, is concerned with the most fundamental questions about what things are. Science, as opposed to metaphysics, takes the physical universe alone as its subject matter, and attempts to give a fully detailed account of the universe that would if completely successful provide explanations or predictions for all non-random physical happenings. The Aristotelian position we are considering is in favour of Aristotle's metaphysics, not his science; it holds that Aristotle's metaphysical account of the universe contains the essential elements that must be presupposed by any scientific account, although it does not defend all of his metaphysical views.

An assumption that will be made by this paper is that metaphysical positions are relevant to scientific understanding and progress. This assumption has been questioned by some historians of science, who argue that the essential advances underlying the scientific revolution were in fact achieved by artisans, rather than through any philosophical or metaphysical reflection.[1] If this claim is true, the main theoretical source of the scientific revolution, to the extent that there was such a source, was arguably furnished by Dark Age monks. Aristotle scorned to discuss technology, on the grounds that it was beneath the philosopher's notice. The first Europeans to take technology seriously as a branch of knowledge were Benedictines. Their motto, 'ora et labora', gave work as well as prayer a spiritual status, and their livelihoods depended on it (unlike classical philosophers, who usually handed over physical work to slaves). This led them to be the first European thinkers to classify technology as a legitimate branch of knowledge, and also to be the first to make a practice of leaving written descriptions of it, a key element in technological advance.[2] The project of continually innovating and improving on previous technology did not exist in antiquity, whose technological progress was sporadic. The influence of monks was crucial to its emergence in the Middle Ages.

It is certainly true that artisans, technological advance, and the problems posed by technology, were crucial to the scientific advances of the seventeenth century. In addition to the practical and experimental skills and mentality that were fostered by being an artisan, the experiments that underlay these advances depended on technological innovations such as telescopes; and the role of artillery, for example, in posing problems connected with the flight of projectiles, is well known. However, it will be assumed that

[1] For defences of forms of this thesis see Zilsel (2003), Rossi (1970), Smith (2004).

[2] See Ovitt (1987), Whitney (1990), White (1978). White's contested thesis is that Christian theology is responsible for the development of technology in Europe, a development he sees as a disaster on ecological grounds.

these factors are not a sufficient explanation for scientific advance–they were after all present in other cultures, such as China, which did not reach the same scientific level–and that arriving at a correct metaphysics is related to, and important for, scientific achievement.

The depreciation of Aristotelian metaphysics mentioned above is connected to the fact that in the seventeenth century, Aristotelian science was rejected, and replaced by the far superior Newtonian physics and Copernican astronomy. This rejection has generally been equated with a justified rejection of the whole Aristotelian scheme of things–science and metaphysics all together. The claim that the whole Aristotelian scheme of things was mistaken and unscientific can be called the 'Enlightenment claim'. The contrary claim asserts that the metaphysical structure of the world that is presupposed by science is a basically Aristotelian one. Contemporary advocates of this neo-Aristotelian view do not hold that the entire Aristotelian metaphysics is correct, but they do argue that the correct metaphysics is broadly Aristotelian.

There are degrees of commitment to an Aristotelian metaphysics. The most basic form of Aristotelianism involves accepting things, rather than events, as causes, and attributing their causal activity to their possession of properties that are by nature causal powers. This rules out a conception of laws of nature as simple descriptions of regular patterns, and the claim that being a cause or an effect results from fitting in to some universal pattern. A more specific form adds that claim that things are sorted into natural kinds by their fundamental causal powers; a yet more specific form asserts that the properties that make a thing belong to a given natural kind are possessed necessarily by that thing, and constitute its essence. Scientific investigation, on this view, proceeds by discovering the causal powers that are associated with things of a given kind, and laws of nature, in science, amount to statements about the causal powers possessed by different kinds of thing. More specific still is the assertion that the essence of a thing is its substantial form. This claim is as specific as current neo-Aristotelianism gets; the Aristotelian doctrines of hylomorphism and final causes are rarely defended by current neo-Aristotelians, and will not be construed as forming part of the broadly Aristotelian metaphysics discussed in this paper.

The Enlightenment view, on the other hand, postulates laws of nature, *rather* than causal powers and natural kinds, as the basis of scientific explanation. (The Aristotelian view need not deny the existence of laws of nature; it simply denies that such laws can exist or be understood independently of objects with causal powers.) Support for the neo-Aristotelian position comes from two sources; a new understanding of the history of science, and developments in philosophy itself.

2 Historical Reconsiderations of the Role of Aristotelianism in Science

One factor that contributed to a new understanding of the history of science was the replacement of Newtonian physics by Einstein's theory of relativity. Before this replacement, it was generally thought that science prior to the 17th century was bad science simply because it gave the wrong answers to scientific questions, and that part of the achievement of the scientific revolution–part of what constituted it as a revolution–was just its coming up with true science rather than false science. When Newtonian physics was superseded, this attitude could no longer be maintained. It was not just that Newtonian physics turned out to give the wrong picture of the world; it was that this wrong picture could not be blamed on bad science. Newtonian physics was thoroughly confirmed by rigourous experiment for two hundred years, encountering no serious difficulties until the late nineteenth century. The success of seventeenth-century science could no longer be put down to its having achieved the right description of the world, and, conversely, giving the wrong description of the world could no longer be used as a reason for dismissing pre-Newtonian science. A common medieval understanding of science as having the purpose of 'saving the appearances', that is, giving a theory that would predict all observed phenomena but that would not claim to actually give a description of reality, gained plausibility as a result of this change, since it offered a way of explaining how Newton was a great scientist despite his theory not being correct. (This idea was not in fact original to the Middle Ages, since it was proposed by Ptolemy.)

The idea of 'saving the appearances' as the object of science was adopted by Pierre Duhem (1861–1916), the physicist, philosopher of science, and historian of science. Duhem's argument for this thesis continues to be significant, partly through its influence on W.V. O. Quine, the most influential American philosopher of the second half of the twentieth century. Duhem's position differed from that of current neo-Aristotelians; while himself espousing an Aristotelian metaphysics, he thought that science was in practice unable to arrive at knowledge of the essences of things, and that it was not its function to do so (see Duhem 1987, pp. 90–91). Current neo-Aristotelians espouse scientific realism (a view characteristic of Australian and New Zealand philosophers, who make up a large part of the most significant advocates of this view), and assert that an Aristotelian metaphysics is preferable precisely on the grounds of its suitability as an analysis of scientific theory and practice.

Duhem's importance as a support for the neo-Aristotelian position lies in his revolutionising of the history of science, a revolution that rehabilitated the scientific importance of medieval thinkers.[3] He brought about this revolution through being the first historian to give real consideration to (or even to actually read) key medieval scientific texts, a consideration that led him to conclude that the so-called 'scientific revolution' did not occur in the seventeenth century at all, and was not a revolution, but was instead a steady process that began in the middle ages. This conclusion means that the Enlightenment view is false, and that Aristotelian metaphysics is not incompatible with science, since it was accepted by the medieval scientists who got the modern scientific project off the ground.

Duhem's historical account met with an opposition that was partly motivated by hostility to his positivistic understanding of science as 'saving the appearances'. Edwin Burtt (1892–1989) and Alexander Koyré (1892–1964), the most important of the historians of science who followed Duhem and reacted against him, were both inclined towards scientific realism, and both of them held that abandonment of an Aristotelian metaphysics was essential for the proper development of science—that this abandonment was in fact a true scientific revolution in something like the Enlightenment sense.[4]

An initial problem with this position is that ancient science, whose achievements are not disputed, was itself Aristotelian more often than not, at least in its approach to the physical universe; many important scientists of antiquity adhered to the Middle Platonist synthesis, which applied Aristotle's basic framework to the material world, and a transformed version of Platonic thought to the immaterial world. This is an important fact to keep in mind when considering medieval science, since the Middle Ages read Aristotle through the lenses of

[3] See Duhem (1954, 1969, 1985, 1987, 1996). Duhem also wrote a number of articles for the 1912 Catholic Encyclopedia, which has helpfully been placed online at http://www.newadvent.org/cathen/index.html; they are 'Albert of Saxony', 'History of Physics', 'Jean de Sax', 'Jordanus de Nemore', 'Nicole Oresme', 'Piere de Maricourt', and 'Thierry of Freburg'.

[4] See Burtt (1954), Koyré (1957), Lindberg (1990).

the Aristotelian commentators of mid- and late antiquity (and to a lesser extent through Muslim and Jewish commentators), who were generally working out of this synthesis, and who often added elements to Aristotle's system, or presented alternatives to parts of his system.[5] Epicureanism, on the other hand, whose atomism was the closest equivalent in antiquity to the anti-Aristotelian philosophical positions celebrated by the Enlightenment view, did not make any significant contributions to science. (Epicurus himself was notorious for claiming in his letter to Pythocles that the sun was more or less the size it appears to be to us, about 30 cm across as Cicero recounts it, and his followers were hostile to the very notion of geometry.[6])

Further historical investigation has largely substantiated the view that the essential elements of science were in place during the middle ages, and refuted Burtt and Koyré's claims. The basic outline of science was indeed already present in Aristotle, who saw science as a matter of deriving general principles from particular observed instances—which he called induction—and then using the general principles to explain and predict further particular happenings—which he called deduction. This outline is however too basic on its own to give a satisfactory description of the nature of science, since it leaves unanswered the question of how one gets the general principles from the particular observations. Aristotle made a start on this question, but it was medieval scholars who answered it in its essentials. Robert Grosseteste (c.1168–1253), the bishop of Lincoln, and his pupil the Franciscan Roger Bacon (c.1214–92), added the crucial notions that before moving from particular instances to a general principle, one should accumulate more evidence through experiment, and one should submit the general principle that is proposed as an explanation for the particular instances to experimental testing. Grosseteste also laid down that in addition to seeking particular instances that support a general principle, one should choose between alternative general explanations for particular happenings by attempting to falsify the proposed explanations, and seeing which explanation survives this test. (This method of falsification is better known to current philosophers of science as the central theme of Karl Popper's philosophy of science, although Popper disimproves Grosseteste's account by presenting falsification as the only method for science.) Several of the inductive scientific methods codified by John Stuart Mill, in Mill (1973)—the method of agreement, the method of difference, and the joint method of agreement and difference—were also formulated by Grosseteste, Duns Scotus, and William of Ockham. 'Ockham's Razor', the claim that, other things being equal, the simplest theory should be preferred, is a methodological principle of fundamental importance (see Baker 2004).

The notion that mathematical descriptions of reality are essential to science was explicitly formulated by Grosseteste and Roger Bacon, although it was not original to them–it had its roots in Pythagoreanism and Augustinian Neo-Platonism; the influence of Augustine was decisive in this respect in the Middle Ages, and especially strong in the Franciscan order, which preferred Augustinianism to Aristotelianism, and produced many of the most significant medieval scientists (Bacon was a Franciscan). Significant advances in such mathematical description were made by the Oxford Calculators—Thomas Bradwardine, William Heytesbury, Richard Swineshead, and John Dumbleton—at Merton College in the first half of the fourteenth century (see Sylla (1991)).

This account of the achievements of medieval science should not be thought of as claiming that no significant basic advances in scientific methodology were made in the

[5] For a survey see Sorabji (2005).

[6] This was because Euclidean geometry contradicted some of Epicurus's metaphysical principles. Epicurus's follower Polyaemus roundly asserted that 'all of geometry is false'; see Cambiano (1999), p. 587.

seventeenth century. One such advance was Galileo's assertion of the importance of abstraction and idealisation. Galileo accounted for the behaviour of falling bodies by describing how free fall would work in a vacuum, and then explaining the actual behaviour of falling bodies as resulting from this account modified by the effects of the medium in which the actual bodies we encounter are travelling. Such idealisations may rarely or never occur in the actual world, and hence cannot simply be derived from experience or produced by experiment; they were however necessary for the development of Newton's physics. Another advance was Pascal and Fermat's formulation of the mathematics of probability, an entirely new departure the results of which are now essential to every science.[7] A clearer grasp of the value of a piecemeal approach to scientific problems, that does not attempt to give a universal theory as an explanation for phenomena, was also important for scientific progress.

These advances do not however justify the Enlightenment view that proper science began in the seventeenth century. They greatly extended the power of science, but were not necessary for its existence. This is not to say that the scientific developments of the seventeenth century do not deserve to be described as a revolution. The error of the Enlightenment picture lies in its characterisation of this revolution as consisting in the birth of serious science, rather than as a revolution within science. The advances of the seventeenth century revolutionised an already existing scientific enterprise, whose birth had occurred in the Middle Ages. This is properly described as a birth, not simply as a rebirth of the ancient project of science, because of significant differences between the ancient and medieval scientific enterprises.

One such difference was in their institutional contexts. When the Catholic Church developed universities for the training of clerics, and included Aristotle's scientific works in the university programme of studies, it gave science a central role in an essential institution of society. There was no comparable institutional framework for science in the ancient world. Partly as a result, the medieval scientific enterprise involved a continuous process of investigation, with a view to acquiring more knowledge; whereas scientific progress in antiquity was sporadic, and scientific activity was often confined to learning previously acquired knowledge. The Enlightenment picture of the Middle Ages as a period of scientific darkness is thus the opposite of the truth. Since this is so, it cannot be claimed, as the Enlightenment view does, that an Aristotelian metaphysics is incompatible with good science.

3 The Scientific Revolution and Aristotelian Metaphysics

Of course this is only a partial result from the neo-Aristotelian point of view. The fact that Aristotelian metaphysics is compatible with good science is not yet an argument for its truth, since the same might be true of other metaphysical outlooks. An opponent of the neo-Aristotelian view might point out that in fact the great scientific advances of the seventeenth century were accompanied by the abandonment of Aristotelian metaphysics, and ask why, if the Aristotelian view is the best one, it was abandoned in the course of scientific advance.

There are several things that the neo-Aristotelian can say in answer to this question. One straightforward point is that Aristotelian science and Aristotelian metaphysics fell together because seventeenth-century defenders of Aristotelianism did not usually draw an adequate

[7] For discussion of the evolution of the concept and mathematics of probability, see Byrne (1968), Hacking (1975), Daston (1988), Hald (1990).

distinction between them; but this was due to the inadequacies of Aristotelians, not to deficiencies in Aristotelian metaphysics itself. To this merely defensive point should be added a twofold reply. For one thing, the anti-Aristotelian metaphysics propounded by many seventeenth-century scientists were not in fact ones that anyone would now want to accept, and for another, not all essential Aristotelian concepts were in fact universally abandoned by scientists.

In considering this reply, there are underlying factors that should be kept in mind. One is the danger of a 'Whig' view of intellectual history, that assumes that major changes in intellectual outlook are necessarily for the better, and that influential figures gain their influence through having deep and valuable insights. Consider Spinoza; his odd version of pantheism is not very credible, and never was, but he nonetheless had great influence in the seventeenth century (see Israel 2001). Another is the full emergence, with the publication of Newton's *Principia*, of what Thomas Kuhn has called 'normal science'. This is the practice of developing and applying a particular scientific theory, as opposed to trying to find a new scientific theory that gives a different account of some fundamental aspect of the universe—the latter being the activity that Kuhn describes as bringing about a scientific revolution. Newton's physics was the first theory that was powerful enough to make normal science a worthwhile endeavour; its application and extension was able to occupy scientists for two centuries (and in fact is still practised, for those areas in which it is not practical to apply relativity or quantum mechanics). This produced a certain separation between philosophical thought and science as it is generally carried on. When scientists practice normal science, they no longer need to think about fundamental issues, such as the nature of the basic metaphysical framework that underlies science. That is not to say that they do not make use of such a framework; it is rather that they do not need to consider how it should be explicitly formulated.

The acquiescence, after the eighteenth century, of most philosophers, and some scientists, in Humeian or Kantian accounts of metaphysics, epistemology, and the laws of nature, need not therefore be held to reflect the actual intellectual imperatives of philosophy or scientific practice, as opposed to a simple acceptance of the dominant philosophical trends of their times. A further development that needs to be kept in mind is the increasing separation of philosophers from science after the seventeenth century. Descartes and Leibniz were significantly involved in science, and Locke, while not making any great scientific contributions, at least trained as a physician. Hume, however, was a librarian and historian. This separation meant that while scientists engaged in normal science could disregard strictly philosophical questions, philosophers were increasingly liberated from any need to reconcile their philosophical views with scientific work. All these factors can be cited by neo-Aristotelians in defence of the view that the long eclipse of broadly Aristotelian conceptions in the philosophy of science need not count as a serious objection to these conceptions. To substantiate this defence, however, we need to look at the history of this eclipse in some detail.

Evidence for both sides of the neo-Aristotelian defence can be found in Locke. In one mood, he does not deny the existence of Aristotelian substantial forms, but only rejects the idea that such forms can play a part in science.

> …I have often mentioned a real Essence, distinct in Substances, from those abstract Ideas of them, which I call their nominal Essence. By this real Essence, I mean, that real constitution of any thing, which I the foundation of all those Properties, that are combined in, and are constantly found to co-exist with the nominal essence … Supposing the nominal essence of Gold, to be body of such a peculiar Colour and

> Weight, with Malleability and Fusibility, the real Essence is that constitution of the parts of Matter, on which these qualities, and their Union, depend; and is also the foundation of its solubility in Aqua Regia, and other Properties accompanying that complex Idea … Nor indeed can we rank, and sort Things, and consequently (which is the end of sorting) denominate them by their real Essences, because we know them not. Our Faculties carry us no farther towards the knowledge and distinction of substances, than a collection of those sensible Ideas, which we observe in them (Locke 1975, pp. 442, 444).

Here Locke is following the epistemology associated with the Royal Society. The founding members of the Royal Society rejected the investigation of real essences as too ambitious, and thought that science should confine itself to the systematic description and prediction of observable and quantifiable qualities (see van Leeuwen 1963, pp. 40–41). The idea that this should be *one* of the goals of science is a real advance that puts its finger on a problem with the original Aristotelian understanding of scientific explanation. According to this understanding, one explains why a thing acts as it does by discovering its real essence, and showing how its causal activity flows from this real essence; and the question of why it has that essence is not one that needs an answer, because all there *is* to being that thing is its being a thing of that essential kind.

The trouble with this understanding is that it is often too ambitious. In chemistry, for example, it was only in the twentieth century that such explanations began to be possible, because it was only then that chemical interactions began to be explained in terms of the properties of fundamental particles such as electrons. For such particles, it is arguable that we can give explanations in terms of real essences, because we can claim that all there is to being (for example) an electron, is having its properties of mass, charge, and spin. However, this does not mean that chemistry did not provide any scientific explanations prior to the twentieth century. The notion of scientific explanation thus needs to be expanded beyond the original Aristotelian conception, to include explanations of the kind sought by the founders of the Royal Society. (The approach needed for this expansion was already present in the medieval notion of 'saving the appearances', but this notion tended to be put forward as an alternative to the Aristotelian approach to science, rather than as an addition to it.) In Aristotelian terms, this kind of explanation will involve identifying things of a certain kind, and establishing experimentally that things of this kind behave in characteristic ways, but not trying to account for this behaviour in terms of the real essence of those things.

A simple example would be identifying samples of gold with a spectrometer, and determining that they are insoluble in nitric acid, but soluble in aqua regia (a mixture of nitric and hydrochloric acid). However, contrary to Locke's claim, admitting scientific explanations of this type need not be accompanied by rejecting any scientific role for real essences. Instead, it gives them a further role; we can explain the truth of generalisations like these ones about the solubility of gold by saying that they result from the underlying real essence of gold, and we can pursue an understanding of this real essence by attempting to determine how it can explain such generalisations. This in fact is exactly what scientists do; they look for an underlying structure that confers upon gold both its power to affect spectroscopes in certain ways, and its capacities to dissolve or resists dissolution in various substances. So the real implication of the Royal Society's approach to scientific explanation is that the notion of such explanation should be broadened, instead of altered to eliminate real essences. This broadening will still leave real essences playing a fundamental role, since they will be the ultimate goal and ending point of scientific explanation.

It should be conceded that Locke's approach to scientific explanation had a certain plausibility in the historical circumstances he was in. The prospect of giving real essences as explanations of chemical behaviour did not show even the vaguest promise of fulfilment until Niels Bohr developed his conception of the atom, centuries later–a fact that shows how the case for metaphysical conclusions can be closely related to the state of science. The difficulty of giving such explanations may explain a common objection to Aristotelianism made on the seventeenth century, to the effect that, as Newton said, it postulates 'occult qualities' that 'put a stop to improvement in natural philosophy' (Newton 1952, p. 542). In the absence of any idea of what the real essences of things actually are, an attempt to explain phenomena through appealing to real essences will end up being vacuous.

Locke illustrates the unacceptable elements of the seventeenth-century rejection of Aristotelianism in his rejection of natural kinds.

> And that the Species of Things to us, are nothing but the ranking them under distinct Names, according to the complex Ideas in us; and not according to precise, distinct, real Essences in them, is plain from hence; That we find many of the Individuals that are ranked into one Sort, called by one common name, and so received as being of one species, have yet Qualities depending on their real Constitutions, as far different from one another, as from others, from which they are accounted to differ specifically. This, as it is easy to be observed by all, who have to do with natural Bodies; so Chymists especially are often, by sad Experience, convinced of it, when they, sometimes in vain, seek for the same Qualities in one parcel of Sulphur, Antimony, or Vitriol, which they have found in others. (Locke 1975, p. 443)

Fortunately for chemistry, as Peter Geach points out,[8] chemists paid no attention to Locke's strictures on this topic, assumed that the differences between parcels of substances pointed out by Locke were due to impurities, and developed ways of purifying substances that enabled chemical research to be carried on. In so doing, they developed a science that used natural kinds as a basic notion, a notion ultimately organized—with great scientific fruitfulness—in Mendeleev's periodic table. The 'zoo' of particles postulated by the Standard Model in physics is also a classification into natural kinds of a more fundamental sort.

A somewhat similar combination of epistemological modesty and ontological error can be found in Newton. His modest approach can be found in Definition VIII of the *Principia*:

> I … use the words attraction, impulse, or propensity of any sort towards a centre, promiscuously and indifferently, one for another; considering these forces not physically, but mathematically: wherefore the reader is not to imagine that by those words I anywhere take upon me to define the kind, or the manner of any action, the causes or the physical reason thereof, or that I attribute forces in a true and physical sense, to certain centres (which are only mathematical points): when at any time I happen to speak of centres as attracting or as endued with attractive powers. (Newton 1725, p. 12)

In the following passage, however, we see stated not only a modest view of scientific theories, but a highly contestable presentation of an alternative to the rejected Aristotelian account of the metaphysical foundation of these theories.

[8] Geach remarks that 'As regards natural kinds in the animate world, Locke's scepticism was largely based on a credulous acceptance of old wives' tales: about rational parrots, and about "monsters" or "changelings" produced by the intercourse of bulls with mares, cats with rats, and "drills" with women'. Geach (1961), p. 88.

> All these things being considered, it seems probable to me, that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles … It seems to me farther, that those particles have not only a force of inertia accompanied with such passive laws of motion as naturally result from that force, but also that they are moved by certain active principles, such as is that of gravity, and that which causes fermentation, and the cohesion of bodies. These principles I consider, not as occult qualities, supposed to result from the specific forms of things, but as general laws of nature, by which the things themselves are formed; their truth appearing to us by phenomena, though their causes be not yet discovered. For these are manifest qualities, and their causes only are occult. And the *Aristotelians* gave the name of occult qualities, not to manifest qualities, but to such qualities only as they supposed to lie hid in bodies, and to be the unknown causes of manifest effects: Such as would be the causes of gravity, and of magnetic and electric attractions, and of fermentations, if we should suppose that these forces or actions arose from qualities unknown to us, and uncapable of being discovered and made manifest. Such occult qualities put a stop to the improvement of natural philosophy … (Newton 1952, pp. 541–542)

Here Newton makes the crucial postulation of general laws, rather than Aristotelian forms, as the explanation for the manifest phenomena that fit into the general principles discovered by science. His rejection of Aristotelian qualities as 'occult' trades on an ambiguity in the meaning of 'occult'. If it is taken as meaning occult as opposed to manifest—i.e. not directly observable, as opposed to observable–it is true that Aristotelian qualities (of some kinds) and forms are taken to be occult. However, Newton seems in this passage to be equating 'occult' with unknowable. This is just what Aristotelians deny; they hold that the nature of unobservable qualities and forms can be inferred from observation, and can once inferred serve as the basis for predictions of future observations. To make this equation, one must establish that being unobservable means being unknowable. Neither Newton nor Locke could consistently argue for this view, since both of them believed in the existence of an immaterial God; only with Hume did the claim that knowledge is restricted to observation, in the non-Aristotelian sense of 'observation' used by Locke, get developed in a consistent philosophical framework.

Newton makes it clear in this passage that the general principles discovered by science are not the same as the general laws of nature, but instead result from these laws. He does not give an explicit account of what these laws are, but his understanding of them can be inferred from his letters to Dr. Richard Bentley (1662–1742), an Anglican clergyman and scholar. In his first letter he remarks of his Principia, 'When I wrote my treatise about our system, I had an eye upon such principles as might work with considering men, for the belief of a Deity; and nothing can rejoice me more than to find it useful for that purpose.' (Newton 1782, p. 429) Some of the principles that Newton had in mind were straightforward forms of the argument from design, such as arguing from the ordering of the planets and stars to the necessity of a deity to put them in that order. In his fourth letter, however, he suggests a further argument that casts some light on his understanding of natural law.

> It is inconceivable, that inanimate brute matter should, without the mediation of something else, which is not material, operate upon, and affect other matter without mutual contact; as it must if gravitation, in the sense of *Epicurus* be essential and inherent in it. And this is one reason, why I desired you would not ascribe innate gravity to me. That gravity should be innate, inherent and essential to matter, so that

> one body may act upon another at a distance through a *vacuum*, without the mediation of any thing else, by and through which their action or force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws, but whether this agent be material or immaterial is a question I have left to the consideration of my readers. (Newton 1782, p. 438)

Newton had been formed in the 'mechanical philosophy' argued for by Robert Boyle and Descartes, an understanding of the universe deliberately formulated to replace Aristotelianism. It held that the only properties possessed by material things were the passive attributes of shape, motion, and solidity, with active power belonging only to spiritual beings.

Several things about the mechanical philosophy are worth noting, in the context of a discussion of Aristotelian metaphysics. The general rejection of Aristotelian views was the work of its advocates, and the mechanical philosophy was the view that replaced Aristotelianism. The mechanical philosophy was, however, wrong. The replacement of Aristotelianism by the mechanical philosophy was not, as the Enlightenment picture would have it, in any way a move away from religious belief. Its main proponents, Descartes and Robert Boyle, were sincerely religious; to the extent that the mechanical philosophy had a religious connotation, it was with one theological outlook rather than another. Descartes' spiritual director, Cardinal Pierre Bérulle, the founder of the French school of spirituality, which formed the basis for the training of Counter-Reformation Catholic priests, was an enthusiast for Descartes' philosophy, which he promulgated through the Paris Oratory that he founded. The Jansenists Arnauld and Nicole, authors of the influential Port-Royal Logic, were also enthusiasts for Descartes' physics. Steven Nadler remarks that 'Nicole greatly appreciated the Cartesian absolute distinction between mind and body as providing a solid foundation for proving the existence of God and the immortality of the soul' (Nadler 1988, p. 579). Arnauld attacked Leibniz for reintroducing the notion of substantial form. The Jansenist Pascal rejected Aristotelianism; his important experiments with a barometer were partly intended to refute the Aristotelian denial of the existence of a vacuum. The rejection of Aristotelianism was part of what appealed to the Oratory and the Jansenists about the mechanical philosophy. As extreme Augustinians, they disliked the influence of Aristotle on Catholic theology, which they believed tended to promote an excessively liberal conception of the freedom of the human will.

Most Newton scholars hold that in denying that gravitational attraction could be exercised by material things, he meant to imply that it was immediately produced by divine action.[9] Alternative interpretations are that he thought gravity to be due to the action of other spiritual agents carrying out God's purposes, or to God's directly adding attractive power to material things that are themselves incapable of exercising it.[10] (Newton's speculations about explaining gravitational attraction through an ether do not contradict any of these interpretations. In accordance with his conception of the basic nature of matter quoted above, he thought of this ether as being composed of separate particles, for which the problem of explaining action at a distance would still arise.) Any one of these interpretations serves to explain Newton's contention that God is not only the creator of the universe—the conclusion of his argument from design—but also its governor, continually

[9] See Koyré (1965), pp. 149–163, esp. pp. 149, 152, 149; Cohen (1987); Westfall (1986), p. 233.

[10] See Henry (1994) for discussion of alternative interpretations of Newton's view of gravity.

involved in regulating its activity. Newton's 'general laws', then, are laws in a real sense; they are conceived and enforced by the lawgiver of the universe, God.

Newton's followers expressed this view in more explicit ways. William Whiston, who succeeded Newton as Lucasian professor of mathematics at Cambridge, asserted:

> 'Tis now evident, that Gravity the most mechanical affection of Bodies, and which seems most natural, depends entirely on the constant and efficacious, and, if you will, the supernatural and miraculous influence of Almighty God … I do not know whether the falling of a Stone to Earth ought not more truly to be esteemed a supernatural Effect, or a Miracle, that what we with the greatest surprise should so stile, its remaining pendulous in the Open Air: since the former requires an active influence in the First Cause, while the latter supposes non-annihilation only. (Whiston 1708, p. 284.)

Samuel Clarke, writing in consultation with Newton, defended Newton's view of gravity against Leibniz's objection that it required constant miraculous intervention by God, by claiming that direct divine interventions in the world are only miraculous if they produce an unusual result; 'Natural and Supernatural are nothing at all different with regard to God, but distinctions merely in Our conceptions of things. To cause the Sun [or Earth] to move regularly, is a thing we call Natural; to stop its Motion for a Day, we call supernatural: But the One is the Effect of no greater Power than the other: nor is the One, with respect to God, more or less natural or Supernatural than the other.' (Clarke 1978, p. 362.)

Newton's opposition to Aristotelian metaphysics thus did not involve his offering an evidently superior alternative.[11] The success his views achieved is no doubt due in part to the enormous prestige that attached to his thought, in virtue of his great achievements. But it was also connected to the achievements themselves. One connection has been noted above; the great power of his theory made it possible for scientists to devote themselves wholly to 'normal science'—to developing and expanding a theoretical conception of the world, without worrying about the metaphysical issues it raises. Another connection is to his methodological separation between a scientific account of phenomena and a metaphysical account of the underpinnings of those phenomena. The existence of this separation is contestable; one can argue that science in fact involves an implicit metaphysical picture of the world, and that scientists thus necessarily operate with such a picture when they are doing science, even if the metaphysics to which they assent philosophically is a quite different one. But the *idea* of such a separation was undoubtedly valuable for scientific advance. It meant that scientists could offer a justification for proceeding with their investigations without concerning themselves with the metaphysical issues involved in them, and thus without having their scientific work obstructed by struggles with metaphysical issues. (This approach to science was itself dependent upon the emergence of normal science as a full-time occupation, since revolutionary scientific developments, in physics at least, usually require some reflection on metaphysical issues; thus Einstein's early work on relativity was influenced by his reading of Hume and Ernst Mach.[12])

The dispute between Clarke and Leibniz mentioned above indicates that Newton's views did not meet with universal acceptance. Leibniz was the main opponent of Newton's banishment of active power from the material world, and the source of an alternative line

[11] His view on gravity is now presented as 'Intelligent Falling', a satirical parody of the 'Intelligent Design' position of anti-Darwinists; see http://www.theonion.com/content/node/39512

[12] See Jammer (1999), pp. 40–41.

of thought on the nature of physical things. Educated at first in the Aristotelian school, Leibniz embraced the mechanical philosophy in his youth.[13] He abandoned his adherence to it, however, upon concluding that it was unable to give an account of the observed properties of matter, and explicitly set out to defend and incorporate certain Aristotelian notions in his metaphysics.

Accepting the point that matter conceived of as characterised solely by shape and motion was necessarily passive, he rejected Newton's view that its activity resulted from divine intervention. Instead, he argued that the mechanical philosophy's view of matter was false, that physics required the postulation of active forces that belonged to the nature of physical things, and that the Aristotelian notion of substantial form should be revived to give an account of these forces. Indeed, he went further, and argued that all there is to the nature of physical things are their powers to act and to be acted upon. (This talk of active forces in things gets reinterpreted in his peculiar metaphysics as talk of the harmonised changes within isolated monads, but when Leibniz is actually doing physics, he uses the concept of active force as an explanatory principle.) His views on the active nature of matter were accepted and used by Joseph Priestley and Roger Boscovich, who defined atoms in Leibnizian fashion as centres of fields of force. Boscovich in turn exerted an important influence on Michael Faraday and his development of field theory.[14] Faraday asserted a Leibnizian view of matter, saying of it that 'the substance consists in the powers'. (Harman 1982, p. 77.) Boscovich's influence continued in James Clerk Maxwell[15] and Lord Kelvin, who remarked that 'My present assumption is Boscovichianism pure and simple'. (Whyte 1961, p. 191.) Thus, as the neo-Aristotelian Brian Ellis remarks,[16] there is an important tradition stemming from Leibniz that endorses the Aristotelian notion of active powers in things, and that gave rise to significant scientific achievement.

It is instructive to consider Leibniz's disagreements with Aristotelian views as well as his agreements, since they exemplify the main objections raised to Aristotelianism. He complains that the scholastics thought they could 'explain the properties of bodies by mentioning forms and qualities, without going to the trouble of examining their method of operation: as if someone thought it sufficient to say that a clock has a time-indicative quality which comes from its form, without considering what all that consists in.' (Leibniz 1973, p. 20.) He also criticises scholasticism for holding what he calls the 'physical influx' view of causation; 'the way of influence is that of ordinary philosophy [viz. scholasticism]; but as it is impossible to conceive of either material particles, or immaterial species or qualities as capable of passing from one of these substances to the other, we are obliged to abandon this view'. (Leibniz 1973, p. 131.) This conceives of the Aristotelian understanding of causation as involving the literal passing of some entity from the thing that is a cause to the thing that is being causally affected, with the entities being either material particles, or actual qualities of things—as if, when a seal is pressed on wax, the actual shape of the seal, the particular configuration that belongs to that particular seal, is somehow taken from the seal and passed on to the wax. There are in fact contemporary advocates of theories of causality that have some resemblance to 'physical influx' ones,[17]

13 See Garber (1982) on Leibniz's early views.

14 See Iltis (1973) for discussion of Leibniz and his followers.

15 See Harman 1998, pp. 195–196.

16 See Ellis 2001, pp. 263–268.

17 See Ehring (1986) and (1997), and Dowe (2000).

but Leibniz's description of 'physical influx' accounts of causation corresponds neither to the medieval Aristotelian view[18] nor to the position of contemporary neo-Aristotelians.

4 Hume, Positivism, and the Eclipse of Aristotelianism

Although Leibniz's ideas remained important for science, in philosophy his views were largely eclipsed by those of Hume, who developed the dominant ideas of the seventeenth century in the opposite direction from Leibniz. Hume restricted not just scientific theory, but all belief about causation, to positions about the pattens of manifest qualities. He departed from the previous Empiricists in denying that we could have any experience or concept of the notions of efficacy, agency, power, force, energy, necessity, connection, or productive quality, on the grounds that such notions are nowhere to be found in our experience. In this he was rejecting the Aristotelian account of sense perception; when Aquinas argued against occasionalism, the position that God is the sole cause of all effects, he did so partly on the grounds that we directly observe the causal action of one thing upon another, and hence that occasionalism is contrary to the evidence of our senses.[19]

Recent scholarship has led to two accounts of the position of Hume.[20] The new interpretation of Hume sees him as holding something like the epistemology of the Royal Society and Locke, where he does not deny the existence of causes that are distinct from, and give rise to, our sense experience, but only insists very strictly on our complete inability to know or conceive of what they are. The old interpretation of Hume, the one generally accepted until the 1980s, sees him as insisting that the very notion of anything existing aside from our sense experience is meaningless; the only things that exist are 'ideas'. For the old Hume, all there is to causation *in the objects* is the existence of '… an object, followed by another, and where all the objects similar to the first are followed by objects similar to the second' (Hume 1951, p. 150). Hume concedes that our notion of causation includes more than such regular succession, but holds that this 'more' is furnished by something within ourselves, not in the objects we describe as cause and effect; it consists in a feeling of expectation that an object of the first kind will be followed by an object of the second kind, a feeling that itself always follows when we have enough experience of objects of the one kind being followed by objects of the other kind. Hume points out that on this understanding, we have no rational grounds for believing that objects of one kind will be followed by objects of another kind, but concludes that since we have no choice about forming such beliefs, this lack of rational basis is not a problem. Since the old Hume was the one generally accepted for most of the period under discussion, it is his views that we will consider, without thereby intending to take a position on what the real Hume actually thought.

Hume's views present obvious difficulties for an account of science, but his ideas were so dominant that until the middle of the twentieth century the accounts that were attempted had affinities with his thought, and never rejected its fundamental starting-points. This is

[18] Suarez, for example, simply meant by 'physical cause' a cause that has a real influence on the production of an effect, as opposed to one that was termed a cause but did not actually produce an effect (as for example a factor that is described as a cause on the basis of its not preventing something it can and should prevent); see Suarez (1994), p. 16–17.

[19] See Aquinas (1975), ch. 69, pp. 226–235.

[20] For the new Hume, see Strawson (1989), Wright (1983); for the debate between interpretations, see Read and Richman (2000).

the case even with Kant and his followers. Kant's belief in synthetic a priori knowledge restored in a certain sense some of the knowledge that Hume's scepticism had banished, but only in a certain sense. It did not describe this knowledge as available through sense experience, as the Aristotelian view holds, and it did not assert that it applied to things as they are in themselves, only to things as we understand them—this displaying a clear kinship with Hume's views on sense experience, and on the element of the concept of causation that is contributed by our own minds.

What eventually led to the loss of the pre-eminence of fundamentally Humeian schemes in the philosophy of science was a development that at first was thought to show great promise for them. This development emerged from the direction of nineteenth-century mathematics. This century, which saw the emergence of pure mathematics as an explicitly recognised discipline, was characterised by efforts to place the foundations of mathematics on an explicit and rigourous basis. Eliminating unclarity and appeals to intuition in the foundations of arithmetic and calculus was a principal focus of the work of mathematicians such as Weierstrass, Cantor, Peano, and Hilbert. As part of this movement, the Jena mathematician Gottlob Frege undertook the project of basing arithmetic upon logic. This project required him to develop modern logic, the greatest logical achievement since Aristotle. This achievement was taken up by a general move away from Kant in German-language philosophy (and from Hegel in English philosophy),[21] in the work of the logical positivists (later termed logical empiricists), a group principally composed of the members of Moritz Schlick's Vienna Circle, but also including Hans Reichenbach's Berlin Circle. A. J. Ayer, and Bertrand Russell during the 'logical atomist' stage of his thought, advanced views similar to the positivists; Ernst Mach and Ludwig Wittgenstein, while not members of this movement, should be mentioned as important influences on it.

Russell and Ayer were close to the British Empiricist tradition in their views on epistemology and perception, and they can be described as developing this tradition by applying the new predicate logic devised by Frege to its expression. For the logical positivists, the possibility of using logical and mathematical advances to replace the Kantian notion of the *a priori* was of more interest than traditional empiricist positions on epistemology and perception (on this see Coffa (1991) and Friedman (1999)). A basically Humeian notion of the nature of causation in the objects was however common to all of these thinkers. The opportunity that the positivists thought was presented by the development of modern predicate logic was that of giving a fully rigourous account of scientific theories, in a structure that satisfied the demands of empiricism conceived on Humeian lines. Predicate logic made possible a description of the world in a formal language that had a formal structure comparable to that of mathematical systems.

The grand vision of the positivists was to carry on the project begun by axiomatising mathematics, and produce a complete unification of science by expressing scientific theories in axiomatic form. The appeal of predicate logic for this project was not only its formal structure, but also its extensional nature. In extensional logics, the validity of inferences and the truth of complex statements is solely a function of the truth of the atomic statements that compose them; and the truth of the atomic statements is simply a matter of whether or not the states of affairs that they describe actually obtain. Such a logic is thus well adapted to describe Hume's world of 'loose and separate' existences, where the existence of one thing in no way results from the exercise of the power of another, and where the notion of necessity has nothing answering to it in external reality.

[21] For the move away from Kant, see Coffa (1991), Friedman (1999), and Frederick Suppe (1977), pp. 6–14.

The positivist axiomatisation of science employed three kinds of terms: logical and mathematical terms; observational terms, referring to sensations (in earlier versions) or to directly observable physical happenings or things (in later versions); and theoretical terms, which are defined using the observational terms. (Since mathematical statements were thought, following Frege and Russell, to be explicable in terms of first-order predicate logic and set theory, the addition of mathematical terms to the vocabulary of theories did not make a fundamental difference to their content.) The axioms of the theory were statements of fundamental laws of nature that made use only of theoretical terms.

The intellectual breadth and depth of the positivist project had no parallels in previous accounts of science. The strength of the project, and the intellectual honesty of its main proponents, was displayed in the revisions it underwent. The attempt to apply their approach to the whole of human knowledge was recognised as a failure, and its scope was restricted to an analysis of scientific theory. The attempt to define terms by giving their verification conditions was also found to fail through excess of ambition, and the notion of verification, and the notion of verification was replaced by that of confirmation.[22] The positivist goal of formalisation meant that a formal logic of confirmation was required, a demand that posed the first substantial check for the positivist view. Carnap's initial formulation of such a logic did not succeed, and the whole project of accounting for science in terms of confirmation was attacked by Popper, who proposed its replacement by falsification. It also faced grave difficulties in accounting for fundamental change in science, such as the replacement of Newton's account of gravity by Einstein's. Such change did not in fact happen in the way the positivist account of confirmation demanded, which was through the accumulation of masses of evidence that confirmed the later theories better than the former ones.

The positivist account of science nonetheless retained its popularity, so much so that its later versions earned the description 'the Received View' among philosophers of science.[23] It is precisely the intellectual power and sophistication of the various versions of the Received View that make it important for the neo-Aristotelian position. It is the best effort that could be offered as a defence of a Humeian view, and if it could not succeed, a Humeian account of science has no prospects. It was in fact the very clarity and scope of the Received View that made it vulnerable to criticism; its precise statements made difficulties more evident, and harder to avoid or answer. (Much of the credit for this intellectual power, and for the responsiveness of positivists to well-founded criticism, is owed to the great intellectual honesty of Carnap.) In giving an overview of these difficulties, we move from a narrative of the eclipse of Aristotelian views to an account of the factors that have led to their re-emergence.

Hume's original problem of induction was an obstacle for the Received View (as Popper pointed out), and, unlike Hume, its proponents were not willing to settle for scepticism about inductive and causal knowledge. In addition to their unwillingness to deny the status of knowledge to science, Hume's uncomplicated account of the genesis of our causal beliefs could not be plausibly applied to the more sophisticated means of scientific investigation practised by science. The situation of induction was worsened when Nelson Goodman added his 'new riddle of induction' to the original difficulty posed by Hume. Goodman describes the riddle thus;

[22] On this see Carnap (1936–1937).

[23] An important popularising description of the positivist programme is given in Ayer (1936). For a standard account of the Received View, its evolution, and objections to it, see Suppe (1977).

> Suppose that all emeralds examined before a certain time *t* are green … Now let us introduce another predicate less familiar than 'green'. It is the predicate 'grue' and it applies to all things examined before *t* just in case they are green but to other things just in case they are blue. Then at time *t* we have, for each evidence statement asserting that a given emerald is green, a parallel evidence statement asserting that emerald is grue. The question is whether we should conjecture that all emeralds are green rather than that all emeralds are grue when we obtain a sample of green emeralds examined before time *t*, and if so, why. (Goodman 1955, pp. 74–75.)[24]

Goodman's riddle was an insurmountable obstacle to the positivist project of describing the logic of confirmation in purely syntactic terms.

Further problems with the Received View arose in the area of explanation. Its account of scientific explanation is the 'deductive-nomological model' (DNM), which claims that explanation occurs through the explanandum of a scientific explanation—a statement describing what is explained—being deductively implied by other true statements, that describe laws of nature and initial conditions.[25] This met with a number of difficulties,[26] the most well known one being the case of the flagpole. From laws about the behaviour of light and information about the height of a flagpole and its location relative to the sun, we can deduce—and thus explain—the length of the flagpole's shadow. However, we can equally well deduce the height of the flagpole from the length of its shadow. We want to say that the height explains the length of the shadow, not vice versa, but the DNM provides no resources for doing this.

The Received View's account of theoretical terms was a strict working out of Locke's claim that nominal definitions were the only ones useful in science. The severe problems encountered by this account called into question this Lockean approach. One line of objection came from scientific realists, who wanted to understand statements about theoretical, unobservable entities such as atoms as true claims about the existence of unobservable entitites, rather than assertions whose meaning was exhausted by their implications for observable happenings. Another line of objection called into question the demarcation between theoretical and observational terms. With the abandonment of the Empiricist 'way of ideas', and its modern descendant, phenomenalism, and the replacement of statements about sensation in the object language of positivist theories with statements about physical objects and happenings, it became harder to draw a principled distinction between these categories.[27]

A further objection came from Quine, inspired by Duhem. He pointed out that assertions about theoretical entities cannot in fact be translated into claims about a specific subgroup of observational statements, and then confirmed or falsified according as these statements turn out to be true or false, and asserted that 'our statements about the external world face the tribunal of sense experience not individually but only as a corporate body'. (Quine 1961, p. 41.) An example to illustrate this claim can be taken from astronomy; basing a scientific judgment on what we see through a telescope requires us to assume laws of optics as governing the telescope's image, and is thus not simply an appeal to our visual experience.

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[24] See Stalker (1994) for discussion of Goodman's new riddle.

[25] For the DNM see Hempel (1965).

[26] Discussed in Woodward (2003a, b).

[27] See Suppe (1977), pp. 80–86.

A yet further difficulty comes from the idea of *defining* theoretical terms in an observational language. This implies that if the observations associated with a term change as a result of investigation, then the *meaning* of the term changes. But this then makes it difficult to explain how different theories can disagree—a difficulty that is if anything increased by adopting Quine's claim about the relation of theory to experience; and this in turn makes it hard to explain how preference of one theory over another can be rationally justified. Paul Feyerabend, indeed, drew from the notion of incommensurability the conclusion that such preference cannot be justified, and denounced the imperialism of scientists who claimed that their theories were in any way superior to the views of believers in witchcraft.[28]

5 Rebirth of Aristotelianism in the Philosophy of Science

These problems helped to motivate the first serious reintroduction of the notion of real essences and necessary properties into philosophy since the work of Leibniz. Crucial to this reintroduction was the development of a formal modal logic of necessity and possibility by C. I. Lewis and Ruth Barcan Marcus,[29] which gave precision and power to modal reasoning and the idea of modal properties. Saul Kripke, who developed a semantics for modal logic, led this metaphysical revival.[30] Part of the appeal of Hume's banishing necessity from science arose from the unpalatable nature of the position he and the other British Empiricists were opposing. The rationalist view of Descartes and Leibniz claimed that knowledge of the necessary features of the world was given *a priori*, through reflection upon innate ideas. If a scientific understanding of the world is to include necessary features of things, though, this means that scientific knowledge can be arrived at through simple reflection, without the need for experiment. Descartes and Leibniz said just that, but the Empiricists quite reasonably found this impossible to swallow. Kripke effectively attacked the view that knowledge of necessary truth need be *a priori*, while knowledge of contingent truths must be *a posteriori*; in doing so, he removed a fundamental prop of the Humeian position. He also argued for existence of necessary properties in things, and for a notion of real essences as the properties of things and kinds that science reveals as underlying and explaining their observable characteristics. Hilary Putnam, whose concerns were more focused on the philosophy of science and the problems of giving an account of theoretical terms, also defended the notion of natural kinds, and gave an account of the reference of natural kind terms that paralleled Kripke's account of the reference of proper names.[31] The attempt to argue for essentialism on the basis of Putnam or Kripke's accounts of reference has not been successful,[32] but current neo-Aristotelians do not base their views on versions of Kripke or Putnam's direct theory of reference. Molnar, a leading neo-Aristotelian, is content to adopt the view of 'good old, much maligned John Locke' on nominal and real essence in explaining natural kind terms. (Molnar 2003, p. 22.)

Another important move towards Aristotelianism resulted from attempts to give an account of dispositional properties. Properties of this sort, like charge, spin, and mass, are

[28] See Feyerabend (1975) and (1987).

[29] See Lewis (1932), Marcus (1946, 1947).

[30] See Kripke (1980).

[31] See Putnam (1975).

[32] For criticism of the causal theory of reference as a basis for essentialism, see Salmon (1982) and Shapere (1984), ch. 18.

essential to science,[33] but attempts to explain these properties in the extensional logic used by positivism proved a failure.[34] Any plausible account of scientific laws governing these properties therefore needed to resort to subjunctive or modal statements to describe them. This in itself was a fundamental departure from positivism, since it raises the question of the truthmakers for such statements; what are the features of the real world that account for them? This departure did not, however, prove drastic enough, because analysis of dispositional statements in terms of subjunctive or modal conditionals face severe problems.[35] Philosophers of science have thus moved towards postulating dispositions as real and irreducible properties of things. But this move is simply an acceptance of Aristotelian powers of things as the fundamental principles of causal and scientific explanation.

The step of introducing subjunctive statements was related to fundamental problems in the positivist account of laws. This account, again because of its extensional nature, was unable to rule out laws about nonexistent things being vacuously true, and unable to distinguish between laws of nature and true accidental generalisations.[36] These weaknesses have led philosophers to question the very notion of laws of nature. One line of attack is to ask how these laws, if they are understood as something more than mere uniformities, are supposed to explain or influence what goes on in the world.[37] On Newton's view, there is an intelligible answer to this question; laws of nature are the uniformities that God has decided to bring about, and things conform to them because God makes them. If we do not want to get God involved in science in this way, however, no answer is available. An even more fundamental objection is made by Nancy Cartwright, who asserts that scientific knowledge does not in fact come in the form of laws of nature, if such laws are understood as universal generalisations about one kind of event being associated with another. She gives as an example the concept of mass;

> The relevant vocabulary of occurrent or measurable properties in this case is the vocabulary of motions—positions, speeds, accelerations, direction and the like. But there is nothing in this vocabulary that we can say about what masses do to one another … when one mass attracts another, it is completely open what motion occurs. Depending on the circumstances in which they are situated, the second mass may sit still, it may move towards the first, it may even in the right circumstances move away. There is no one fact of the matter about what occurrent properties obtain when masses interact. (Cartwright 1999, p. 65.)

She concludes that it is the causal powers of objects that science investigates and reveals.

Initially, the proposed alternatives to the Received View focused on the questions of how scientific theories are justified and replace one another, rather than on the question of the metaphysical structure they ascribe to the world.[38] This focus, however, did not give answers to the central problems posed by the Received View. In response to these problems, a movement began towards restoring broadly Aristotelian accounts of causation,

[33] See Thompson (1988) on the necessity of dispositional terms for physics.

[34] See the paper by Carnap, and criticisms of his approach, in Tuomela (1978).

[35] On the problems raised for conditional analyses of dispositions by the problems of masking and finkish dispositions, see Martin (1994), Lewis (1997), Mumford (1998) ch. 3, Bird (1998), Choi (2006); for survey and bibliography concerning dispositions generally, see Fara (2006).

[36] For these difficulties see Armstrong (1983), Tooley (1977, 1987).

[37] For this objection to laws of nature see Mumford (2004).

[38] The main examples of this approach are Popper (1935), Kuhn (1962), Lakatos (1970) and (1978), Laudan (1977).

essence, and explanation; a development that linked up with moves towards Aristotelianism in metaphysics generally, independent of the particular problems of the philosophy of science.[39] The pioneering work here was done by Harré and Madden, and has been extended by Nancy Cartwright, George Molnar, Brian Ellis, John Heil, and Alexander Bird.[40]

An important element in this movement is an idea suggested by Sydney Shoemaker (1980) in response to Goodman's new riddle of induction. Shoemaker answered the challenge of giving a non-syntactic account of the real properties to which induction should be applied, by proposing that the only real properties were causal properties.[41] The idea that all real properties are causal properties has been named the 'Eleatic Principle', and is argued for by D. M. Armstrong among others; '… it seems possible to conceive of a property of a thing which bestows neither active nor passive power of any sort. But if there are such properties, then we can have absolutely no reason to suspect their existence. For it is only in so far as properties bestow powers that they can be detected by the sensory apparatus or other mental faculty.' (Armstrong 1978, pp. 40–41.) The Eleatic Principle is in fact a revival of a central Aristotelian notion. It turns the tables on Hume's asking 'what is causality?', by asking ' what *isn't* causality?' It also helps to understand the error in Leibniz's 'physical influx' account of the Aristotelian understanding of causality. Leibniz, in this account, is portraying Aristotelian metaphysics as identifying causal powers with some other kind of property. In fact, the Aristotelian idea is that the properties that account for the bringing about of effects just *are* causal powers;[42] there is nothing more to the nature of such properties than their capacity to bring about effects of a certain sort.

The neo-Aristotelian philosophers counter the standard seventeenth-century objections to Aristotelianism by arguing that these objections are caricatures, or else invalid, along the lines sketched out in the discussion of Locke's views given above. They assert that their approach solves the problems concerning induction, laws, explanation, and dispositional properties that have been mentioned above. They are supported by the fact that difficulties with Hume's views have been around for some time. Hume's account of the formation of causal beliefs has always met with some scepticism, on the grounds that regular association is neither sufficient nor necessary for the formation of such beliefs.[43] Hume's claim that all we observe in objects is regular succession has also been challenged.[44] The new respectability of Aristotelian ideas has given these objections a philosophical home to go to, since they have a live philosophical option to support. Most neo-Aristotelians have confined their espousal of Aristotle's views to his views on substance, cause, and essence; his hylomorphism has not found any takers. Recent defences of physical intentionality, however, could be seen as advancing a view at least analogous to Aristotle's postulation of

[39] It should be mentioned that Popper introduced the Aristotelian notion of causal powers early on, in an attempt to give an account of notions of probability involved in quantum mechanics; see Popper (1957) and (1959), and Molnar (2003), pp. 105–107.

[40] See Harré and Madden (1975), Bird (2005a, b, 2006), Ellis (2001), Molnar (2003), Heil (2003), Cartwright (1983, 1989, 1999).

[41] He elaborates on this idea in Shoemaker (1984).

[42] Thus, Aquinas, in explicating Aristotle's remarks in *Physics* book VIII 255b17, says that 'to ask why a heavy thing is moved downwards is nothing other than to ask why it is heavy.' Aquinas (1963), p. 511. This of course assumes Aristotle's account of gravity; a Newtonian or Einsteinian account of gravity would rephrase the description of being heavy.

[43] See Ducasse (1969), p. 16.

[44] See Michotte (1946/1963), Anscombe (1981), pp. 136–137; Leslie and Keeble (1987), Fales (1990), p. 15; Sperber et al. (1995).

final causes in things.[45] Neo-Aristotelians are also open as a rule to expanding Aristotle's ontology to include events and processes.[46]

The neo-Aristotelian position is now a well-established option in the philosophy of science. It has not swept all before it; there is a good deal of support for versions of most of the historical alternatives to it, with the exception of pure Humeian regularity theory, which has very few takers nowadays. The position of David Lewis is the nearest thing to an influential Humeian approach in philosophy nowadays, although it is not very popular among philosophers of science.[47] Lewis supplements a Humeian attitude to the actual world with a modal account of causation, explicated in terms of relations to possible worlds. Bas van Fraassen offers a version of 'saving the appearances'.[48] Dretske, Tooley, and Armstrong explain scientific truth through appealing to laws of nature, which are conceived of as contingent relations between universals.[49] Prediction of the future of philosophy is always risky, but there are factors that incline one to think that the neo-Aristotelian view will continue to thrive. Much of the support for its opposition is still based on assuming an anti-Aristotelian tradition whose underpinnings have been removed by historical and philosophical criticism. It is relatively new and in need of further development, which always attracts philosophers interested in making a name for themselves. Its long-term success will depend on how well this further development works out, but we can be confident that the idea of Aristotelian metaphysics as an exploded medieval illusion will not be revived.

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45 For such defences see Molnar (2003), ch. 3; Martin and Pfeifer (1986), Place (1999a, b).

46 See e.g. Ellis (2001), pp. 162–165.

47 As remarked on by Woodward (2003), pp. 3–4.

48 See van Fraassen (1980, 1989, 2002).

49 See Dretske (1977), Tooley (1977, 1987), Armstrong (1983).

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