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Experimental Success and the Revelation of Reality: The Miracle Argument for Scientific Realism

Martin Carrier

Abstract. The paper addresses the so-called miracle argument in favor of scientific realism and examines the viability of scientific realism as an explanation for the success of science. Scientific realism is committed to the claims that the theoretical terms in the mature sciences typically refer to real objects and that the theoretical laws in such sciences are typically approximately true. Instrumentalism or non-realism draws on the principles that factual claims need to be confirmed empirically and that experience fails to single out true assumptions. The miracle argument says that if a theory referred to fictitious objects, it would be miraculous that it is able to correctly predict observable effects. This argument transforms scientific realism into a hypothesis that is testable by the history of science. I perform such tests and conclude that the only type of realism that appears to be in agreement with the historical record is "realism of natural kinds." Theories that enjoy distinguished explanatory success truthfully establish equivalence relations among phenomena.

1 Instrumentalism and Realism

It is part of the human condition that man's striving may come to grief. This experience of real resistance is conceded by everyone. Sometimes reality is such that our aspirations are thwarted. But this experience of impediment is of limited import in a double respect. First, it is purely negative. One only recognizes what cannot be achieved. Second, it is merely empirical. One is restricted to learning about the observable conditions of failure. Conversely speaking, no trustworthy information is obtained as to how the observations in question come about. Nothing positive is known about the machinery that keeps the action going. On the non-realist or instrumentalist position, this restriction to experience characterizes the scientific endeavor as a whole. No scientific account can reliably penetrate the inner workings of nature and disclose the contrivances behind the appearances.

According to this instrumentalist approach, scientific theories are nothing but tools or instruments that allow us to capture experiences systematically and economically and to control the phenomena. The instrumentalist contention is that these aims characterize the justifiable epistemic ambitions of science *completely*. Nobody would deny, after all, that economy of thought and capacity of intervention are among the aims of science. In particular, all scientific realists would unhesitatingly agree. The issue is whether further epistemic aims can legitimately be attributed to science.

On the realist stance, the primary epistemic aim of science is to arrive at true theories. For this so-called scientific realism, quantities that are non-observable but successfully assumed in science can be expected to possess an approximate counterpart in reality. In particular, theory realism claims that such non-observable quantities which figure in successful scientific theories refer to real entities. Such theories manage to disclose the reality behind the appearances. An important reason advanced in favor of this position is that successful theories in the mature sciences are simply too good not to be true. For instance, using the hypotheses of electrons and electromagnetic fields, scientific theory explains a large number of diverse observations about electricity in a precise and unified fashion. This excellent achievement, as the argument goes, makes it inevitable to assume that the entities invoked exist in reality.

2 *The Instrumentalist Argument*

Non-realism is frequently adopted on two sorts of principles, namely, empiricism and the so-called Duhem-Quine thesis. It is stated, first, that all knowledge about matters of fact is to be judged empirically. Experience is the only basis for testing and confirming assumptions with factual bearing. It is claimed, second, that the truth of theoretical hypotheses cannot unambiguously be appraised on this empirical basis alone. This latter view arises from Pierre Duhem's argument against the conclusiveness of theory testing in the early 20th century (Duhem 1906, chap. 10) and is stressed by Willard Van Orman Quine from the 1950s onward (Quine 1953; Quine/Ullian 1970, chap. 6-7; see Gillies 1992, chap. 5). The combination of their views is often denoted as Duhem-Quine-thesis; it states, roughly speaking, that logic and experience fail to single out true hypotheses.

The pivot of the pertinent argument is the limited import of empirical tests of theoretical hypotheses. Two obstacles stand in the way. No theory can uniquely be positively distinguished on empirical grounds alone since there are always empirically equivalent, but theoretically distinct and incompatible theoretical alternatives. If a hypothetico-deductive theory test is passed, the result does not provide a basis for the exclusion of all alternative accounts. The inference from the truth of the observed consequences to the truth of theoretical principles employed for their derivation would fall victim to the fallacy of affirming the consequent. False premises may imply true consequences. Ptolemaic astronomy could accommodate eclipse data, and Newtonian corpuscular optics accounted for the reflection, refraction and dispersion of light.

The room left for positive confirmation of theories by this underdetermination argument is paralleled with respect to refutation by what is called Duhem's problem. Duhem argues that no theoretical hypothesis can be refuted using only logic and experience. The reason is that each empirical test of a hypothesis proceeds by invoking a large number of additional assumptions and auxiliary hypotheses so that a deviant observation fails to identify unambiguously the mistaken principle. The only legitimate inference is that an error lies somewhere in this network of intertwined hypotheses; nothing is implied as to its location. Notwithstanding declarations to the contrary, Foucault's 1849 experiment concerning the velocity of light in media of different indexes of refraction failed to refute the assumption of the particulate nature of light. It generated an anomaly for the corpuscular theory, to be sure, but it is safe to assume that theory and data could have been brought into accordance by adapting auxiliaries referring, for example, to the interaction between light and matter or to the mode of operation of the indication processes (the measuring instruments) (Duhem 1906, sec. 10.2).

The upshot of the instrumentalist argument is that the test procedures employed for evaluating scientific theories do not allow for a cogent determination of the truth value of hypotheses. Hypothetico-deductive testing leaves room for a falsely positive and falsely negative assessment alike, which uncertainty thwarts any claim as to the supreme trustworthiness and reliability of theoretical principles. Given these limitations, the argument says, it is unfounded and mere hubris to assume that successful theories mirror reality.

3 *Realism as an Explanation for the Success of Science*

Realists attempt to counter the instrumentalist argument by contending that there can be convincing empirical grounds for transcending experience and for making existence claims concerning theoretical quantities. The prototypical argument to this effect is the so-called "miracle argument" which says that scientific realism is the only conceivable explanation for the success of science. Without assuming that science takes hold of the real world, the predictive success of science would be an utter miracle. But since there are no miracles anymore, realism is left as the only explanation for this success. The classic modern formulation of the argument is due to Hilary Putnam (although earlier versions are found in the methodological literature).

> The positive argument for realism is that it is the only philosophy that doesn' t make the success of science a miracle. That the terms in mature scientific theories typically refer (this formulation is due Richard Boyd), that the theories accepted in a mature science are typically approximately true, ... these statements are viewed by the scientific realist ... as part of the only scientific explanation of the success of science. (Putnam 1975, p. 73)

> And the typical realist argument against idealism is that it makes the success of science a *miracle*. ... And the modern positivist has to leave it without explanation (the realist charges) that 'electron calculi' and 'space-time calculi' and 'DNA-calculi' correctly predict observable phenomena if, in reality, there are no electrons, no curved space-time, and no DNA-molecules. ... But if these objects don't really exist at all, then it is a *miracle* that a theory ... which speaks of curved space-time successfully predicts phenomena. (Putnam 1978, pp. 18-19)

In its canonical formulation (likewise due to Putnam), scientific realism is characterized by two principles:

> (1) The theoretical terms in the mature sciences typically refer to real objects.
> (2) The theoretical laws in the mature sciences are typically approximately true (Putnam 1978, pp. 20-21).

The realist interpretation concerns the theoretically introduced concepts and theoretically assumed mechanisms. These concepts and mechanisms are supposed to correspond to really existing objects or processes. Scientific realists usually add the clause that the currently accepted theories in physics, chemistry and biology represent mature science so that their central theoretical concepts can be expected to refer to reality.

The miracle argument in favor of this view says that if a theory referred to fictitious objects, it would be puzzling that it is able to correctly predict observable effects. Predicted observations are obviously not taken from experience but rather derived from theory. Non-realism is at a loss to account for the fact that fictitious "calculi" were able to anticipate truthfully empirical phenomena. After all, it would be an amazing feat if a theory about unicorns and dragons arrived at true predictions about horses and rhinos. It would be likewise highly astonishing if a theory that referred to actually inexistent curved space-time structures could adequately accommodate the observed rotation periods of pulsars. By contrast, it is a natural explanation for such predictive success that curved space-time actually exists and that it is grasped at least approximately correctly by the theory.

Scientific realism thus explains the empirical success of science. This involves a major shift in its conceptual status. The assumption that successful theories in the mature sciences correspond to reality becomes an empirical hypothesis that refers to the history of science and is buttressed by the actual explanatory accomplishment of scientific theories.

4 *Strong Predictive Success and Genuine Reference*

The miracle argument suggests an empirical test of the realist contention to explain the success of science. Realism assumes that truth implies reference so that reference of the theoretical terms is a necessary precondition for the truth of the theoretical postulates. Further, a theory's truth is supposed to be sufficient for its predictive success. A true theory mirrors reality and is empirically successful for this reason. Consequently, the realist explanation for the success of science suggests that there should be no successful theories that lack reference.[1]

Larry Laudan has taken seriously this explanatory claim and subjected it to historical examination. He came up with numerous examples of theories that were once empirically successful but turned out later to be thoroughly mistaken in ontological respect (Laudan 1984a, pp. 225-226). Take 18th century phlogiston chemistry which furnished an account of combustion and the phenomenon called calcination (i.e., the oxidation of metals in modern terms). The pivot of this account was that a "principle," a nonmaterial, property-bearing entity, left the body during the processes in question. The phlogiston theory enjoyed important explanatory successes in the decades around 1700. Likewise, the fluid theory of electricity was built around the assumption of a weightless substance that was thought to be able to penetrate bodies and

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[1] This inference appears to be fallacious since the transition from success to truth seems to involve the fallacy of affirming the consequent. But realism contends that a theory's truth is the only conceivable explanation for its success, and with this auxiliary clause appended the argument becomes deductively valid.

to be a source of forces. This theory was empirically successful in the 18th century.[2] Analogously, the optical ether was part of the highly successful optical theory of the 19th century and formed an undisputed constituent of the ontological inventory of science. In his Preface to Heinrich Hertz' *Principles of Mechanics* Hermann von Helmholtz gives credit to the experiments of the latter on electromagnetic waves and states as one of the conclusions to be drawn from these experiments that "there can no longer be any doubt that light-waves consist of electric vibrations in the all-pervading aether" (quoted from Hacking 1983, p. 256).

Laudan's objections have established beyond reasonable doubt that the miracle argument in its original, comprehensive form cannot be sustained. Since empirically successful theories with nonreferring theoretical terms can be identified in the history of science, reference to real entities is not necessary for empirical success. However, the force of this conclusion suffers from Laudan's extremely broad notion of empirical success. A theory is said to be successful if it offers confirmed predictions and explanations of a variety of phenomena. Laudan adopts such a broad notion for the reason that a more tight conception would tend to make science unsuccessful, with the result that the realist explanandum might be in danger to evaporate (Laudan 1984a, p. 222). Laudan's counterexamples consequently refer to theories that were once successful in this weak sense and that according to present lights fail to refer to reality.

But scientific realism cannot convincingly be grounded on such an undemanding notion of success. Consequently, scientific realism cannot be undermined by counterexamples of this sort. A theory that is constructed such that it fits certain known phenomena qualifies as successful in this sense. Kepler's laws of planetary motion or Boyle's law of ideal gases were more or less read off from the available data and indeed matched these data—including future observations of the same phenomena. But we aren't struck by such

cases of explanatory and predictive success. These generalizations did what they were designed to do. There is no miracle involved here. And if there is no explanandum, realism can hardly be credited with providing the explanans.

Accordingly, scientific realism cannot be grounded on explanatory and predictive success *simpliciter*. Rather, a more demanding variant of such success has to be brought to bear. What matters is the successful expansion of a theory's domain of application beyond its former boundaries. I call this distinguished type of predictive achievement "strong empirical success." What is really miraculous is William Whewell's consilience of inductions (1858) and Pierre Duhem's theoretical prediction of hitherto unknown laws (1906). It is only the explanation of these two sorts of empirical success that could support the realist contention (Musgrave 1988, 232; Carrier 1991, pp. 25-26).

Consilience of inductions is characterized by the following two features.

(1) Laws that were taken to describe different kinds of phenomena are unified by a theory. After the formulation of the theory these phenomena are taken to result from a common underlying process.

(2) This unification was not achieved by deliberate adaptation. Rather, a theory designed to cope with one class of facts was later found to accommodate an additional, apparently different set of phenomena. Unification was not achieved by hand, as it were, but came about as an unexpected and surprising coincidence.

One of Whewell's examples is Isaac Newton's hypothesis of universal gravitation. As Whewell tells the story, the hypothesis was introduced to account for Johannes Kepler's third law and later found to explain the remaining Keplerian laws, along with lots of other seemingly unrelated astronomical phenomena like the precession of the equinoxes (Whewell 1858, pp. 153-154).

Whewell goes on to argue that consilience reliably indicates that the theory has grasped an aspect of the real world. Hypotheses that are merely constructed to match the data will fail when new phenomena are discovered. If a hypothesis applies to these novel circumstances as well, it must represent an element of reality.

> But when the hypothesis, of itself and without adjustment for the purpose, gives us the rule and reason of a class [of facts] not contemplated in its construction, we have a criterion of its reality, which has never been produced in favour of falsehood. (Whewell 1858, p. 155)

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[2] It might be objected that these theories are not part of the "mature" sciences. It could be said that neither chemistry nor the theory of electricity in the 18th are "mature" in the sense required by the realist so that the alleged counterexamples miss the realist contention. However, I argue below that phlogiston chemistry was empirically successful (see sec. 6); moreover, it exhibited methodological virtues like unifying power that are considered today as important distinctions. Denying maturity to the phlogiston theory around 1700 would deprive much of present-day science of maturity as well. In addition, as Worrall has pointed out, without a more detailed elaboration, the maturity clause has the air of an ad-hoc device. Whenever the ontology of a theory is abandoned completely, it was immature in the first place (Worrall 1989, p. 153).

Only if we assume that the theory reflects the real state of affairs, do we understand that and how consilience can arise.

This train of thought is instantiated by Jean Perrin's argument in favor of the reality of molecules (advanced between 1908 and 1913). Perrin drew attention to the fact that the quantity now known as Avogadro's number (and referring to the constant number of molecules per mole of any substance) could be measured in divergent ways. The relevant methods relied on such apparently unrelated phenomena as Brownian motion, electrolysis together with Robert Millikan's determination of the electron charge, or Max Planck's radiation law (Brush 1976, p. 697). Inspection of the results revealed a remarkable numerical agreement which Perrin took as a cogent reason for the existence of molecules (Salmon 1984, pp. 214-220).

> Our wonder is aroused at the very remarkable agreement found between values derived from the consideration of such widely different phenomena. Seeing that ... the numbers ... agree among themselves, without discrepancy, for all the methods employed, the real existence of the molecules is given a probability bordering on certainty. (Perrin, quoted in: Salmon 1984, p. 219).

Put in Whewell's terms, Perrin's argument comes down to an inference from successful consilience of inductions to veracity. Molecular theory establishes a relation between regularities that would appear completely unconnected otherwise. If Avogadro's number did not refer to some underlying molecular reality, the numerical agreement among the different results would seem utterly miraculous. Only by assuming that the diverse methods capture the same aspect of reality, can resort to a miracle be avoided.

A similar argument is developed by Duhem who, unlike Whewell, focuses on the successful prediction of novel facts, i.e., of empirical generalizations formerly unknown to science. If a theory is regarded as a purely artificial system, Duhem argues, we cannot expect it to entail successful predictions of hitherto undiscovered regularities. On this view, such a prediction would have to be considered a "marvelous feat of chance." If, by contrast, the theory is assumed to grasp the real order of things, it is natural to expect that there are some previously undetected novelties hidden in it.[3]

> The highest test, therefore, of our holding a classification as a natural one is to ask it to indicate in advance things which the future alone will reveal. And when the experiment is made and confirms the predictions obtained from our theory, we feel strengthened in our conviction that the relations established by our reason among abstract notions truly correspond to relations among things. (Duhem 1906, p. 28)

Duhem's example is the prediction of "Poisson's spot" by Augustin Fresnel's wave theory of light. In 1819, Denis Poisson derived a seemingly grotesque consequence from Fresnel's recently proposed theory that regarded light as transverse oscillatory motion in a mechanical ether. According to this theory, Poisson argued, a bright spot should appear in the middle of the shadow of a circular screen lit by a point source of light—a result he deemed absurd. When this startling prediction was actually verified, the theory was considered to contain an element of truth (Duhem 1906, pp. 29-30).

In light of these considerations, a more specific and more tenable version of the miracle argument can be given. What seems astonishing from a non-realist point of view is the capacity of some theories to successfully predict formerly unknown effects (Duhem) or novel relations between known generalizations (Whewell) without adjustment for this purpose. Such predictions refer to new types of phenomena or new types of relations between them. The amended miracle argument says, then, that without assuming that science captures reality, *strong* empirical success remains inexplicable and miraculous. Put this way, there is some plausibility in the claim that the success of science generates an explanatory challenge to the non-realist.

5 Non-Realist Explanations for the Success of Science

Naturally enough, the diverse brands of non-realism or instrumentalism attempt to meet this challenge. Let me briefly review some non-realist explanations for the empirical success of science.

5.1 Methodology Instead of Truth

The empirical success of theories is attributed to the strengths and virtues of scientific method. Bas van Fraassen and Larry Laudan point out that theories are subjected to a large number of demanding examinations. They are confronted with the evidence and only accepted if they pass tests that would have

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[3] It will appear amazing that Duhem who was presented above as one of the authors of the instrumentalist argument (see sec. 2) should have committed himself to such seemingly realist views. But there are important differences between Duhem's "natural classification" and realism proper. I will come back to this issue in sec. 8.

detected empirical inadequacies if there were any. Successful theories have come out first in a "fierce competition" among rivaling approaches. Only those accounts can be expected to survive in this methodological battlefield that latch on to the real regularities of nature. That is, only truly empirically adequate theories pass this tough selection process unscathed. What eventually counts in the miracle argument is nothing but empirical adequacy, and this feature can readily be explained by recourse to demanding standards for judging theories. It is no wonder from an instrumentalist perspective that accepted theories are empirically successful. They were selected for precisely this property. Empirical success can thus be explained by appeal to the high standards of evaluation employed in science; recourse to truth is gratuitous (Van Fraassen 1980, 39-40; see also Laudan 1977, 127; Laudan 1984b, pp. 97-101).

However, what is lost in this anti-realist explanation is the difference between simple empirical adequacy and strong empirical success. The methodological explanation aims at predictive success in general and misses the outstanding impact of strong success. The selection for empirical adequacy must always proceed on the basis of known evidence. Theories cannot possibly be judged in terms of future achievements. The successful anticipation of a new kind of effect or a new sort of relation among effects cannot be confirmed using presently available data. The surprising and unforeseen character of these future distinctions is missed by taking them to be part of simple empirical adequacy.

5.2 *Reciprocal Adaption Between Data and Observational Consequences*

The miracle argument proceeds on the assumption of a close connection between theoretical principles and observational consequences. Only on that assumption can empirical success clearly be credited to the principles. As Barry Barnes argues, however, this assumption is somewhat remote from reality. The application of a theory to experience is an intricate procedure into which a large number of auxiliaries, models, and analogies enter. All these mediating constructions are fashioned with an eye on bringing theory and experience into harmony. Empirical success can at most to a small extent be attributed to the virtues of the corresponding theory; it is rather due to the practice of developing the theory in view of the data and to specify the data in light of the theoretical claims (Barnes 2004, in this vol.).

But this approach equally misses the exceptional status of strong success. Neither confirmed novel predictions nor unintended unifications can be the result of tailoring a theory to the extant evidence. It is precisely the fact that no such explicit adaptation can produce strong success that underlies the intuition of its special and distinctive character. It is true, Barnes might reply that the novel predictions could speciously be borne out by adapting the data to the theoretical requirements. But this option would demand, first, an extreme and fairly implausible malleability of facts and, second, would in any event be ruled out regarding novel unifications. After all, Whewell's consilience distinguishes cases in which theory and data are at hand before their mutual agreement is recognized.

5.3 *The "Round-of-Shot" Argument*

Non-realism might object that strong successes are indeed nothing but chance hits and do not reveal any underlying conformity between theory and reality. However, as the objection proceeds, these cases still do not involve a miraculous feat of chance since they are supplemented with lots of failed attempts. As a matter of fact, a large number of assumptions advanced in science are misguided and empirically inadequate; they are abandoned quickly. Others only serve to account for those phenomena they were designed to cope with. All these hypotheses fall short of the demanding standards implicit in the amended miracle argument. Given such a vast number of less than persuasive theoretical endeavors, it can safely be expected for statistical reasons alone that some theoretical innovations not only capture the known data but also anticipate novel types of facts or relations. Strong success is really due to chance, but unamazingly so since these rare lucky events are surrounded by an ocean of abortive attempts.

This "round-of-shot" argument is based on the assessment that science is far less successful than the advocates of the miracle argument presuppose.[4] Scientific theorizing for the most part produces defective and flawed accounts—as most of the shots go astray. This immense number of misses is ignored in typical historical reconstructions. Focusing on the celebrated achievements of creative geniuses covers all the flimsy ideas and ramshackle

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[4] I understand Holm Tetens to say that the world outside the laboratory is governed by a vast number of intertwined influences whose complexity severely restricts the descriptive and predictive success of science (Tetens 2004, in this volume, p. 88).

conceptions with benign neglect that make up the mainstream of scientific research.

It is true, since the miracle argument for scientific realism relies on the empirical success of science, scientific realism would be severely threatened if science turned out to be rather unsuccessful in the relevant sense. The viability of the miracle argument and the round-of-shot rejoinder alike depend on how successful science really is. Both parties to the dispute rely on a historical premise so that empirical research could contribute to mitigating the contrast. The critical parameter is the frequency or fraction of strong success in the mature sciences. I admit to not having conducted a systematic survey to this effect. Yet judging from the circumstantial historical record I am familiar with, the idea that scientific creativity largely resembles a random walk that once in a while stumbles upon a grain of truth haphazardly and otherwise gets lost in a maze of error and misjudgment does not strike me as overly plausible empirically.

6 *Strong Empirical Success and the History of Science*

I conclude that non-realist accounts of strong success are unconvincing whereas realism offers a natural explanation. This explanation amounts to the claim that reference is necessary for strong success which translates into a historical hypothesis: theories whose central theoretical terms lack reference to real objects or processes must neither enjoy Duhemian predictive success nor Whewellian consilience of inductions. The next step is to examine how this hypothesis fares in light of the history of science. An empirical rebuttal of the amended miracle argument requires cases of nonreferring, strongly successful theories.

Whether or not a theory refers to reality can be judged noncircularly on the basis of present scientific knowledge. The reason is that scientific realism includes the commitment that the presently accepted theories in the mature sciences are typically approximately true (see sec. 3). This realist contention entails the following *retention requirement*: for a theory component to be interpreted realistically, it is necessary that it be retained across scientific change. If science is supposed to successfully capture reality, the insights gained on such occasions must not disappear in the ensuing development. A realist will surely be content with an approximative retention of a theoretical feature involved in the production of strong success. But some sort of cumulative theory change is required in order for scientific realism to be a viable position. Conversely, what is excluded is that a theory which once enjoyed strong success is jettisoned completely without leaving any ontological trace in present-day science (Carrier 1993, pp. 393-394; see Worrall 1989, pp. 144, 146).

One important caveat is to be added before I enter history. In many cases the predictive success of wrong theories stemmed from their correct aspects. Fresnel's strongly successful prediction of Poisson's spot arose within an ether theory of light which is mistaken and nonreferring for this reason alone. But the prediction did in no way rely on this ether mechanical framework, but rather on the hypothesis that light is a transverse wave. And this hypothesis is approximately true in light of the present state of knowledge. Thus, the retention requirement is to be brought to bear solely on those features of a theory that were responsible or unavoidable for the production of strong success.

Philip Kitcher distinguishes between working posits and presuppositional posits of a theory. The former are essential for the explanatory achievements of the theory, whereas the latter merely express a commitment to entities whose existence appears to be a prerequisite of the truth of the theory. Kitcher's example of a presuppositional posit is the ether in Fresnel's theory. This all-pervasive mechanical medium was only extremely rarely put to explanatory work. It was rather thought to be a necessary precondition in order for a wave account of light propagation to be true. The existence of the ether appeared as a presupposition of the truth of any undulatory optical explanation. Kitcher surmises that historical arguments for scientific realism should only address the trustworthiness and retention of working posits, whereas presuppositional posits should not be taken seriously in ontological respect (Kitcher 1993, pp. 142-149).

Consequently, possible historical counterexamples to the amended miracle argument need to present cases of strong success as produced by erroneous working posits. The complication is that a working posit might be correct in the relevant sense while being part of a thoroughly flawed account. Consider the prediction of the phases of Venus by both Copernican and Tychonic astronomy. Both theories entailed the startling novelty that Venus should exhibit the full cycle of phases just like the Moon. This feature was ruled out within Ptolemaic astronomy; its discovery by Galileo in 1610 thus constituted strong empirical success. Yet the two thus distinguished accounts are profoundly mistaken. Neither is the solar system composed of a superposition of uniformly rotating spherical shells that carry the planets along (as Copernicus supposed), nor is the Earth located at the center of the Sun's orbit while the other planets revolve around the sun (as Tycho would have it). This episode does not militate against the amended miracle argument since the novel prediction was brought about by the hypothesis that Venus revolves around the Sun rather than the Earth. This hypothesis constitutes the working posit, and it is considered true till the present day.

Viable counterexamples to the miracle argument can only refer to strongly successful theories whose relevant working posits first produced the strong success and were yet later given up completely and left no vestiges in the science of today. I will present two such examples.

7 *History as the Touchstone of the Miracle Argument*

The first example is taken from the history of the later phlogiston theory. I briefly mentioned that phlogiston was taken as an immaterial bearer of the property of combustibility (among others), that it was thought to be contained in combustible materials and in metals and to be released in burning and calcination (see sec. 4). Combustion and calcination thus involve a decomposition into phlogiston and a residue specific for the substance at hand.

In 1766, this account was borne out empirically by Henry Cavendish. He dissolved some metals (iron, tin, and zinc) in some acids (hydrochloric acid, diluted sulfuric acid) and found that an extraordinarily light, extremely combustible gas escaped that burned without any recognizable residue.[5] Cavendish tried to ascertain that the inflammable air, as he called the new gas, did not originate from the acid and concluded that it was released from the metal. Given the background knowledge of the period, a light, combustible gas, burning without residue and contained in metals could be nothing other than phlogiston. Consequently, Cavendish believed he had produced pure phlogiston. Judged by present lights, Cavendish had managed to set free hydrogen from the acid (contrary to what he thought he had established). One of the pertinent chemical reactions is: $Fe + 2HCl \rightarrow FeCl_2 + H_2$.

Proceeding from Cavendish's account, Joseph Priestley in 1782 managed to predict successfully a formerly unknown effect. If inflammable air is pure phlogiston, so his reasoning went, it should be able to supply the phlogiston necessary to transform a calx into the corresponding metal. It should be possible to turn a calx into the metal through absorption of inflammable air.

Priestley succeeded in confirming this novel prediction. He heated several calxes in inflammable air and observed that the gas almost completely disappeared and that the calxes were converted into the respective metals. Priestley believed he had shown beyond reasonable doubt that the calxes had taken up the inflammable air and thereby regained their metallic properties.

Put in modern terms, Priestley had unwittingly synthesized water in his experiment. As a result of the heating, the metallic oxides gave off oxygen which combined with the surrounding hydrogen to form water. One of the reactions that occurred was: $FeO + H_2 \rightarrow Fe + H_2O$. Priestley noticed the emergence of water, but assumed it was moisture already contained in the inflammable air.

Priestley managed to correctly predict a novel regularity that was unknown to science before and not to be expected prior to the formulation of Cavendish's version of the phlogiston theory. This theory was essential for arriving at the prediction; it was clearly a working posit, not a mere presupposition. However, the underlying account seems wrong on all counts. Priestley's result expresses the reductive properties of hydrogen which are due to the fact that hydrogen easily gives off electrons. And this is a far cry from the capacity to supply calxes with phlogiston. Priestley achieved a strong success using a theory completely deprived of reference by the later course of scientific progress (Carrier 1991, pp. 29-30).

The second example is taken from the caloric theory of heat according to which heat is a particular substance. Because of its material nature, heat is governed by a conservation law. Temperature is identical with the concentration of caloric; i.e., the matter of heat is more dense in warm bodies than in cold ones. Caloric is assumed to be composed of particles that attract the particles of other substances with forces specific for the substance involved and diminishing with increasing distance. This approach was held to apply to the particles of ordinary substances as well, but with one exception. The particles of bodies attract one another, as is evidenced by cohesion, whereas the caloric particles repel one another, as thermal expansion demonstrates. In thermal expansion the heat particles are pushed apart by the repulsive force between them and carry along the particles of the heated body.

On this account, the solid state is characterized by an equilibrium between the attractive forces among caloric and body particles, on the one hand, and the repulsive forces among the caloric particles, on the other. The former forces are specific to the substance involved which entails that the thermal expansion of solid bodies varies from substance to substance. This was well

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[5] Cavendish noticed that water appeared in burning the gas but he interpreted this water as moisture dissolved in the gas and deposited in combustion.

known for long. Gases are characterized by the accumulation of caloric; after all, they can be produced by heating. This means that caloric repulsion is entirely dominant. In the gaseous state the particles of the body are pushed so far apart that the short-range attractive forces among them are no longer effective and the repulsive forces among the caloric particles prevail. The elastic properties of gases bear witness to this prevalence of the repulsive forces within the matter of heat. This means that the thermal expansion of gases is nothing but expansion of caloric. It follows that the rate of expansion is the same for all gases; it does not vary with the chemical nature of the gas. In 1802, John Dalton and Joseph-Louis Gay-Lussac independently recognized that this was indeed true and thereby confirmed a novel prediction of caloric theory (Carrier 1991, pp. 30-31).

The modern explanation of the equality of the thermal expansion of gases is that the intensity of substance-specific electric forces between the molecules is negligible (which is in turn due the great distances between the molecules and their large kinetic energies). For this reason, gases can be viewed as collections of colliding point mass particles, which processes can be analyzed using the laws of mechanics. These laws indiscriminately apply to particles of all substances, and the analysis indeed yields the law of Dalton and Gay-Lussac.

This case likewise exhibits the features of strong success. A generalization is anticipated on theoretical grounds and borne out empirically. The prediction depended critically on the caloric account. If this account is renounced, there is no basis to expect any difference in the thermal behavior of solids and gases, respectively. The properties of the matter of heat are actually invoked in the explanation, not merely presupposed; they are working posits. Still, the central concepts of this caloric explanation are entirely nonreferring. Its pivot is the repulsive forces of the matter of heat. But there is no matter of heat, and no other repulsive forces play any role in the modern account of the effect.

Both case studies show that the miracle argument is inappropriate in its amended form as well. They make it clear that strong empirical success may occur although the pertinent theoretical terms lack reference. Strong success is possible without retention of the relevant laws or explanatory mechanisms. Since retention is necessary for attributing reference to the theoretical quantities in question, these cases constitute counterexamples to the realist claim that strong empirical success demands reference of the theoretical terms used. The upshot is, rather, that reference is not necessary for strong success. Although there is nothing in nature that resembled phlogiston or caloric, the corresponding theories enjoyed strong predictive success. It follows that strong empirical success cannot be explained by reference. There is no basis for the judgment that these theories were successful because their central concepts referred to reality. The realist explanation of the success of science cannot be sustained.

8 The Retention of the Classificatory Structure

The argument has come to a deadlock. On the one hand, the miracle argument, at least in its amended version, is intuitively sound. It seems undeniable that there has to be a reason for strong success; the latter should not count as miraculous. It certainly constitutes a highly remarkable achievement if a theory looks further in empirical respect than all observers and experimenters before. The realist explanation affords such a reason and is *prima-facie* persuasive. But these attractive prospects count for nothing if the argument is not in conformity with the historical record and thus fails as an explanation for strong success in the history of science. The nadir is reached: we are at a loss to understand how strong success is possible.

But there is a path leading upward again. It is shown by the recognition that the two explanations did not miss the point in such a large measure as the foregoing presentation might suggest. In fact, one feature is preserved across the theoretical change, namely, the ordering or classification of the phenomena covered. Put more concretely, phenomena bound together by and viewed as results of a common process or law in the outdated approach continue to be related in the up-to-date framework. The theory-based classification of the phenomena as being alike is retained across the theoretical shift. What has once been connected through strong success remains to be viewed as being of the same kind afterward.

As regards the phlogiston case, Cavendish's experiment was interpreted as the release of phlogiston which involved the calcination of the metal employed. Priestley's empirical demonstration of the reductive properties of hydrogen was seen as the reversal of this process: the calx takes up phlogiston and thereby turns into metal. Thus, both reactions were regarded as instantiations of the same chemical process; their only difference was that this process was going off in opposite directions.

Both phenomena are still considered equal in kind in modern chemistry. Cavendish has produced an oxidation, Priestley its converse, i.e., a reduction. It is true, this judgment is based on the modern concepts of "oxidation" and "reduction.". An oxidation in this sense involves a loss of electrons, a reduction is tantamount to electron gain. Viewed from this perspective, the metal is oxidized under the action of the acid in Cavendish's experiment. In chloride formation, for instance, electrons are shifted from the iron to the chlorine (see sec. 7). In Priestley's reversal, by contrast, the oxide releases oxygen and is reduced to metal. This involves an electron shift from oxygen to metal. Correspondingly, both reactions are still regarded as equivalent in kind and as merely proceeding in contrary directions. Their classification is retained in spite of the drastic changes in the content of the pertinent theories.

The same lesson accrues from the caloric example. The strongly successful explanation drew on a connection between interpreting a gas as a universal state of matter and deriving the equality of themal expansion for all gases. The argument attributed gaseous properties to the accumulation of caloric with the result that all substances could in principle become gasiform. It was often believed that gases are particular substances; some (like air) are elastic by their nature, and others (like earth or water) are not. On the basis of this same process of accumulation of heat matter, caloric theory predicted the universal agreement of the expansion rates. It is the physical nature of a gas that accounts both for its being a general state of matter and for its like expansion. Both facts are consequences of a common underlying mechanism.

This connection is faithfully preserved in the kinetic theory of gases. The mechanical analysis of the molecular structure of gases suggests that gases are a state of matter in general, and it entails at the same time that their thermal expansion rates should agree. Again, although the content of the respective theories has changed dramatically, the classification established among the phenomena has remained unaltered.

This discussion suggests the following conjecture: If a theory predicts a novel effect by relating it to another effect, i.e., by assuming both effects to be produced by the same underlying process or mechanism, the two are tied together truthfully. That is, a common bond of this sort reflects a real connection. It is tempting (though not supported by the above examples) to generalize this conjecture to cases of consilient unification. If two laws are related by consilience of inductions, this relation captures a real sameness in kind (Carrier 1991, pp. 32-33).

This conception resembles Duhem's notion of a "natural classification." I portrayed Duhem as one of the authors of the instrumentalist and the miracle argument alike (see sec. 2, sec. 4)—which might appear inconsistent. But Duhem does not employ the miracle argument so as to show that concepts used in strongly predictively successful theories refer to real entities. For Duhem it is the relations that such concepts induce among the phenomena that reflect the ontological order. Strongly successful theories tend to establish natural classifications that grasp the real relations of equality in kind. Strongly successful theories reveal the actual organization of the phenomena, not their real nature, as standard scientific realism would have it (Duhem 1906, pp. 24-27).

The realism of natural kinds I advocate takes strong empirical success as a suitable basis for a realist taxonomy. It is the relation of sameness in kind that provides the continuity at the theoretical level that is required by the miracle argument. Relations among the phenomena are the stuff that reality is made of. The historical claim associated with this position is that phenomena once connected by strong success continue to be considered in science as being equal in kind. Such bonds are forged by nature. Let no man put them asunder.

9 Family Stories, or: Kind-Realism and Related Breeds

The miracle argument in its standard form embodies the inference from success to truth. I take it that the preceding discussion has made it clear that this reasoning is in need of further refinement or amendment. The tenability of the miracle argument crucially depends on how the notions of falsity and success are conceived precisely. One option for preserving the miracle intuition is to say that the wrong but successful theories that apparently militate against this intuition were not completely off the mark, but rather contained an element of truth, and that it was this element that was responsible for the success of the theories in question. One might also say that not just any old confirmed prediction counts as success, but only exceptional achievements. In the foregoing sections I combined both these options and argued to confine the proper import of the miracle argument to those parts of a theory that were essential for the production of strong empirical success.

Howard Sankey rightly feels that the type of scientific realism that might emerge from a qualified miracle argument of this sort is much more restricted than many realists would wish to defend. After all, the argument grants realist bearing only to a comparatively small fraction of the theoretical content of advanced science. Sankey suggests to avoid such restrictions by focusing on the methods of science. As he argues, "the rules of method are truth-conducive tools of inquiry, which serve as reliable means for obtaining truth" (Sankey 2004, in this volume, p. 70).

I grant at once that it would be nice to have truth-conducive rules for theory judgment and theoretical progress. But no such rule I'm aware of is justifiably of the requisite sort. We can't possibly select theories according to their truth or verisimilitude. The methodological rule to identify significant truths is entirely void. What we do instead is to subject theories to demanding tests as to their empirical adequacy and other methodological virtues. This is precisely the alternative to the realist explanation of scientific success that Van Fraassen and Laudan suggest: it is scientific method rather than truth that accounts for success (see sec. 5). On this approach, the appeal to truth is an idle wheel that does not yield an explanatory extra for the success of science. Sankey's account needs to be supplemented with an argument that explains by virtue of which property a methodological rule is conducive to truth.

This concern is deepened by turning to historical examples. Classical gravitation theory should certainly meet Sankey's truth-conducive standards—or there aren't any. Still, the theory was superseded by general relativity theory which entails a quite different ontology with respect to gravitational phenomena. Gravitation is no longer considered as a force that drives bodies off their inertial course; it is rather thought to influence the structure of space-time and to determine in this way what inertial motion looks like. In spite of their small divergences at the empirical level, Newtonian and Einsteinian accounts of gravitation employ deeply contrasting explanatory mechanisms and devise divergent schemes of the nature of gravity.

It seems, then, that Sankey's rules cannot be shown to be conducive to truth; following them may yet lead one astray in ontological respect. For this reason I fail to see a justification for his more comprehensive realism. The amended miracle argument restricts realism in scope, to be sure, but puts it on a more solid foundation. It seems that we have to choose between the broad scope of realist claims and the strength of the argumentative underpinning of realist ambitions. Realism can be conceived as a broad but weakly backed position and, alternatively, as a narrow but strongly supported one. The line I am following can be understood as a plea for the second option.

While the kind-realism I defend is distinguished from more sweeping or comprehensive brands of realism, it is similar to John Worrall's structural realism (which is derived from Henri Poincaré rather than Duhem). What is preserved across ontological disruptions is the mathematical structure used for capturing the phenomena. The mathematical form of the laws provide an element of continuity at the theoretical level over and above the reproduction of empirical content. Consider the transition from Fresnel's optics to James Maxwell's electrodynamics. Fresnel conceived of light as mechanical ether vibrations whereas Maxwell regarded it as electromagnetic field oscillations. The theoretical content and the relevant entities differ drastically. Fresnel was quite wrong about the nature of light and about the explanatory mechanisms. What has remained intact, however, is the structure of the equations. The relevant laws are formally similar. Although they relate different entities in each theory, the relations themselves coincide. The predictive success of Fresnel's theory rested on the fact that it had captured the relations between optical phenomena (Worrall 1989, pp. 158-159).

The transition from Fresnel to Maxwell is a special case in that Fresnel's equations were adopted unchanged—if reinterpreted. A much more common pattern is that the old equations reappear as limiting cases of the new ones. This approximate continuity of structure explains why otherwise mistaken theories managed to score exceedingly well empirically. In summary, Worrall presents structural realism as combining harmoniously two *prima-facie* incompatible features: it does justice to the miracle argument while at the same time recognizing the depth of the theoretical rifts running through the history of science (Worrall 1989, pp. 160-161).

Worrall's structural realism and my realism of kinds share the same basic orientation. Both views stress the reality of relations, whereas no such reality is attributed to the related quantities. But structural realism highlights mathematical relations as the focus of scientific realism, whereas kind-realism is intended to be more ecumenical. Equivalence classes of phenomena that are established by nonmathematical explanations are accepted as candidates for real equality in kind as well. Further, I restrict attribution of reality to theoretical elements connected by strongly successful explanations. Not any structural continuity counts. All depends on the explanatory scores gained by the pertinent theory.

10 Establishing a Natural Taxonomy of Kinds

It is in order to briefly recapitulate the structure of the argument and to roughly indicate the overall view emerging from these considerations. Here is what the argument comes down to. Adopting scientific realism entails a commitment to a realist interpretation of at least one aspect involved in contemporary scientific theories. Relevant aspects could be theory-induced explanatory mechanisms, or entities or equivalence classes among the phenomena (i.e., natural kinds). Such a realist interpretation suggests that earlier theories that were once confirmed in roughly the same fashion as their successors are now should likewise be trustworthy ontologically with regard to the aspect in question. From this follows the retention condition: any feature of a scientific theory that is to be counted as real has to be retained across scientific change. This condition is accepted by realists and non-realists alike. Of course, one is free to say that the reality out there is distinct from everything science has come up with in the past. But this contention would no longer qualify as "scientific" realism. The latter position involves a commitment to the ontology suggested by successful scientific theories—which entails the retention condition. The anti-realist argument from scientific change likewise proceeds from this condition. Laudan's criticism (see sec. 4) operates precisely on this basis: since nothing significant is retained, the realist claims are mistaken.

The second step of the argument brings to bear the judgment that strong empirical success is an extraordinary, astonishing achievement and deserves explanation. The miracle argument provides a possible reason, namely, that the relevant aspect of the thus distinguished theory reflects something real. The argument in and of itself does not specify what constitutes this veridical aspect. The retention condition serves to single out whether laws and mechanisms, entities or natural kinds allows for a realist interpretation. Features of strongly successful theories that are later abandoned are thereby excluded from the inventory of nature. The historical record tentatively suggests that only the last item, i.e., natural kinds, may legitimately be interpreted realistically.

The claim is, then, that the miracle argument is all right in principle; it has only been applied to the wrong subject matter. The argument supports a realism of kinds that focuses on relations between phenomena and is distinct from the more common brands of realism that involve commitment to the existence of entities. Kind-realism claims that science sometimes manages to sort phenomena into the right equivalence classes; nothing is thereby said about the criteria used for this purpose. As the two case-studies were intended to show, the properties and mechanisms that served to tie the various phenomena together are subject to drastic historical alteration. My contention is that the classification induced by strong success corresponds in its relevant aspects to the natural order among the phenomena. This entails no claim as to the reference of the concepts and criteria brought to bear in the classification procedure. There is no referential tie between phlogiston and electrons or between the repulsive forces of caloric and the kinetic energy of molecules. It is the relations they impose on the phenomena, not their natures, that coincide.

The realism of kinds I advocate involves a markedly non-Aristotelian approach to kinds. In the Aristotelian tradition, kinds are individuated through their essential properties. The class of human beings is rightly determined by the property "rational animal," not by the attribute "featherless biped." I suggest to individuate kinds without commitment to any particular set of properties. My claim is that only the induced taxonomy, but not the theoretical means used for establishing it, is (in specific cases) justifiably to be considered real. It is true, our prime epistemic access to kinds is through theories; kinds are determined by relying on theories. Still, it is only the results, and not the means used for their production, that are arguably reliable ontologically. It is the relation of sameness in kind among phenomena that sometimes deserves our ontological confidence. No such case can be made for the explanatory mechanisms employed for specifying such relations, nor for the entities involved in them. It is clear that the phenomena contained in these equivalence classes are similar in some particular respect. But precisely what this respect is cannot reliably be specified. Only the relation of sameness in kind, not the properties underlying this equivalence, is trustworthy ontologically.

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