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The Revolution That Didn't Happen

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I first read Thomas Kuhn's famous book The Structure of Scientific Revolutions[1] a quarter-century ago, soon after the publication of the second edition. I had known Kuhn only slightly when we had been together on the faculty at Berkeley in the early 1960s, but I came to like and admire him later, when he came to MIT. His book I found exciting.

Evidently others felt the same. Structure has had a wider influence than any other book on the history of science. Soon after Kuhn's death in 1996, the sociologist Clifford Geertz remarked that Kuhn's book had "opened the door to the eruption of the sociology of knowledge" into the study of the sciences. Kuhn's ideas have been invoked again and again in the recent conflict over the relation of science and culture known as the science wars.

Structure describes the history of science as a cyclic process. There are periods of "normal science" that are characterized by what Kuhn sometimes called a "paradigm" and sometimes called a "common disciplinary matrix." Whatever you call it, it describes a consensus view: in a period of normal science, scientists tend to agree about what phenomena are relevant and what constitutes an explanation of these phenomena, about what problems are worth solving and what is a solution of a problem. Near the end of a period of normal science a crisis occurs—experiments give results that don't fit existing theories, or internal contradictions are discovered in these theories. There is alarm and confusion. Strange ideas fill the scientific literature. Eventually there is a revolution. Scientists become converted to a new way of looking at nature, resulting eventually in a new period of normal science. The "paradigm" has shifted.

To take an example given special attention in Structure, after the widespread acceptance of Newton's physical theories—the Newtonian paradigm—in the eighteenth century, there began a period of normal science in the study of motion and gravitation. Scientists used Newtonian theory to make increasingly accurate calculations of planetary orbits, leading to spectacular successes like the prediction in 1846 of the existence and orbit of the planet Neptune before astronomers discovered it. By the end of the nineteenth century there was a crisis: a failure to understand the motion of light. This problem was solved through a paradigm shift, a revolutionary revision in the understanding of space and time carried out by Einstein in the decade between 1905 and 1915. Motion affects the flow of time; matter and energy can be converted into each other; and gravitation is a curvature in space-time. Einstein's theory of relativity then became the new paradigm, and the study of motion and gravitation entered upon a new period of normal science.

Though one can question the extent to which Kuhn's cyclic theory of scientific revolution fits what we know of the history of science, in itself this theory would not be very

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[1] Thomas S. Kuhn, The Structure of Scientific Revolutions (University of Chicago Press, 1962; second edition, 1970), quoted below as Structure. This essay is based in part on the author's 1997 Bohner Lecture at Rice University, delivered as part of its yearlong symposium on the work of Thomas Kuhn, and on a 1998 colloquium talk given at the Department of Physics at Harvard University.

disturbing, nor would it have made Kuhn's book famous. For many people, it is Kuhn's reinvention of the word "paradigm" that has been either most useful or most objectionable. Of course, in ordinary English the word "paradigm" means some accomplishment that serves as a model for future work. This is the way that Kuhn had used this word in his earlier book[2] on the scientific revolution associated with Copernicus, and one way that he continued occasionally to use it.

The first critic who took issue with Kuhn's new use of the word "paradigm" in Structure was Harvard President James Bryant Conant. Kuhn had begun his career as a historian as Conant's assistant in teaching an undergraduate course at Harvard, when Conant asked Kuhn to prepare case studies on the history of mechanics. After seeing a draft of Structure, Conant complained to Kuhn that "paradigm" was "a word you seem to have fallen in love with!" and "a magical verbal word to explain everything!" A few years later Margaret Masterman pointed out that Kuhn had used the word "paradigm" in over twenty different ways. But the quarrel over the word "paradigm" seems to me unimportant. Kuhn was right that there is more to a scientific consensus than just a set of explicit theories. We need a word for the complex of attitudes and traditions that go along with our theories in a period of normal science, and "paradigm" will do as well as any other.

What does bother me on rereading Structure and some of Kuhn's later writings is his radically skeptical conclusions about what is accomplished in the work of science.[3] And it is just these conclusions that have made Kuhn a hero to the philosophers, historians, sociologists, and cultural critics who question the objective character of scientific knowledge, and who prefer to describe scientific theories as social constructions, not so different from democracy or baseball.

Kuhn made the shift from one paradigm to another seem more like a religious conversion than an exercise of reason. He argued that our theories change so much in a paradigm shift that it is nearly impossible for scientists after a scientific revolution to see things as they had been seen under the previous paradigm. Kuhn compared the shift from one paradigm to another to a gestalt flip, like the optical illusion created by pictures in which what had seemed to be white rabbits against a black background suddenly appear as black goats against a white background. But for Kuhn the shift is more profound; he added that "the scientist does not preserve the gestalt subject's freedom to switch back and forth between ways of seeing."

Kuhn argued further that in scientific revolutions it is not only our scientific theories that change but the very standards by which scientific theories are judged, so that the paradigms that govern successive periods of normal science are incommensurable. He went on to reason that since a paradigm shift means complete abandonment of an earlier paradigm, and there is no common standard to judge scientific theories developed under different paradigms, there can be no sense in which theories developed after a scientific

[2] Thomas S. Kuhn, The Copernican Revolution (Harvard University Press, 1957).

[3] Kuhn was first trained as a physicist, and despite the presence of the wide-ranging word "scientific" in its title, The Structure of Scientific Revolutions is almost entirely concerned with physics and allied physical sciences like astronomy and chemistry. It is Kuhn's view of their history that I will be criticizing. I don't know enough about the history of the biological or behavioral sciences to judge whether anything I will say here also applies to them.

revolution can be said to add cumulatively to what was known before the revolution. Only within the context of a paradigm can we speak of one theory being true or false. Kuhn in Structure concluded, tentatively, "We may, to be more precise, have to relinquish the notion explicit or implicit that changes of paradigm carry scientists and those who learn from them closer and closer to the truth." More recently, in his Rothschild Lecture at Harvard in 1992, Kuhn remarked that it is hard to imagine what can be meant by the phrase that a scientific theory takes us "closer to the truth."

Kuhn did not deny that there is progress in science, but he denied that it is progress toward anything. He often used the metaphor of biological evolution: scientific progress for him was like evolution as described by Darwin, a process driven from behind, rather than pulled toward some fixed goal to which it grows ever closer. For him, the natural selection of scientific theories is driven by problem solving. When, during a period of normal science, it turns out that some problems can't be solved using existing theories, then new ideas proliferate, and the ideas that survive are those that do best at solving these problems. But according to Kuhn, just as there was nothing inevitable about mammals appearing in the Cretaceous period and out-surviving the dinosaurs when a comet hit the earth, so also there's nothing built into nature that made it inevitable that our science would evolve in the direction of Maxwell's equations or general relativity. Kuhn recognizes that Maxwell's and Einstein's theories are better than those that preceded them, in the same way that mammals turned out to be better than dinosaurs at surviving the effects of comet impacts, but when new problems arise they will be replaced by new theories that are better at solving those problems, and so on, with no overall improvement.

All this is wormwood to scientists like myself, who think the task of science is to bring us closer and closer to objective truth. But Kuhn's conclusions are delicious to those who take a more skeptical view of the pretensions of science. If scientific theories can only be judged within the context of a particular paradigm, then in this respect the scientific theories of any one paradigm are not privileged over other ways of looking at the world, such as shamanism or astrology or creationism. If the transition from one paradigm to another cannot be judged by any external standard, then perhaps it is culture rather than nature that dictates the content of scientific theories.

Kuhn himself was not always happy with those who invoked his work. In 1965 he complained that for the philosopher Paul Feyerabend to describe his arguments as a defense of irrationality in science seemed to him to be "not only absurd but vaguely obscene." In a 1991 interview with John Horgan, Kuhn sadly recalled a student in the 1960s complimenting him, "Oh, thank you, Mr. Kuhn, for telling us about paradigms. Now that we know about them, we can get rid of them." Kuhn was also uncomfortable with the so-called "strong program" in the sociology of science, which is "strong" in its uncompromisingly skeptical aim to show how political and social power and interests dominate the success or failure of scientific theories. This program is particularly associated with a group of philosophers and sociologists of science that at one time worked at the University of Edinburgh. About this, Kuhn remarked in 1991, "I am among those who have found the claims of the strong program absurd, an example of deconstruction gone mad."

But even when we put aside the excesses of Kuhn's admirers, the radical part of Kuhn's theory of scientific revolutions is radical enough. And I think it is quite wrong.

It is not true that scientists are unable to "switch back and forth between ways of seeing," and that after a scientific revolution they become incapable of understanding the science that went before it. One of the paradigm shifts to which Kuhn gives much attention in Structure is the replacement at the beginning of this century of Newtonian mechanics by the relativistic mechanics of Einstein. But in fact in educating new physicists the first thing that we teach them is still good old Newtonian mechanics, and they never forget how to think in Newtonian terms, even after they learn about Einstein's theory of relativity. Kuhn himself as an instructor at Harvard must have taught Newtonian mechanics to undergraduates.

In defending his position, Kuhn argued that the words we use and the symbols in our equations mean different things before and after a scientific revolution; for instance, physicists meant different things by mass before and after the advent of relativity. It is true that there was a good deal of uncertainty about the concept of mass during the Einsteinian revolution. For a while there was talk of "longitudinal" and "transverse" masses, which were supposed to depend on a particle's speed and to resist accelerations along the direction of motion and perpendicular to it. But this has all been resolved. No one today talks of longitudinal or transverse mass, and in fact the term "mass" today is most frequently understood as "rest mass," an intrinsic property of a body that is not changed by motion, which is much the way that mass was understood before Einstein. Meanings can change, but generally they do so in the direction of an increased richness and precision of definition, so that we do not lose the ability to understand the theories of past periods of normal science.

Perhaps Kuhn came to think that scientists in one period of normal science generally do not understand the science of earlier periods because of his experience in teaching and writing about the history of science. He probably had to contend with the ahistorical notions of scientists and students, who have not read original sources, and who believe that we can understand the work of the scientists in a revolutionary period by supposing that scientists of the past thought about their theories in the way that we describe these theories in our science textbooks. Kuhn's 1978 book[4] on the birth of quantum theory convinced me that I made just this mistake in trying to understand what Max Planck was doing when he introduced the idea of the quantum.

It is also true that scientists who come of age in a period of normal science find it extraordinarily difficult to understand the work of the scientists in previous scientific revolutions, so that in this respect we are often almost incapable of reliving the "gestalt flip" produced by the revolution. For instance, it is not easy for a physicist today to read Newton's Principia, even in a modern translation from Newton's Latin. The great astrophysicist Subrahmanyan Chandrasekhar spent years translating the Principia's reasoning into a form that a modern physicist could understand. But those who participate in a scientific revolution are in a sense living in two worlds: the earlier period of normal science, which is breaking down, and the new period of normal science, which they do not yet fully comprehend. It is much less difficult for scientists in one period of normal science to understand the theories of an earlier paradigm in their mature form.

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[4] Thomas S. Kuhn, Black-Body Theory and the Quantum Discontinuity 1894- 1912 (Oxford University Press, 1978).

I was careful earlier to talk about Newtonian mechanics, not Newton's mechanics. In an important sense, especially in his geometric style, Newton is pre-Newtonian. Recall the aphorism of John Maynard Keynes, that Newton was not the first modern scientist but rather the last magician. Newtonianism reached its mature form in the early nineteenth century through the work of Laplace, Lagrange, and others, and it is this mature Newtonianism—which still predates special relativity by a century—that we teach our students today. They have no trouble in understanding it, and they continue to understand it and use it where appropriate after they learn about Einstein's theory of relativity.

Much the same could be said about our understanding of the electrodynamics of James Clerk Maxwell. Maxwell's 1873 Treatise on Electricity and Magnetism is difficult for a modern physicist to read, because it is based on the idea that electric and magnetic fields represent tensions in a physical medium, the ether, in which we no longer believe. In this respect, Maxwell is pre-Maxwellian. (Oliver Heaviside, who helped to refine Maxwell's theory, said of Maxwell that he was only half a Maxwellian.) Maxwellianism—the theory of electricity, magnetism, and light that is based on Maxwell's work—reached its mature form (which does not require reference to an ether) by 1900, and it is this mature Maxwellianism that we teach our students. Later they take courses on quantum mechanics in which they learn that light is composed of particles called photons, and that Maxwell's equations are only approximate; but this does not prevent them from continuing to understand and use Maxwellian electrodynamics where appropriate.

In judging the nature of scientific progress, we have to look at mature scientific theories, not theories at the moments when they are coming into being. If it made sense to ask whether the Norman Conquest turned out to be a good thing, we might try to answer the question by comparing Anglo-Saxon and Norman societies in their mature forms—say, in the reigns of Edward the Confessor and Henry I. We would not try to answer it by studying what happened at the Battle of Hastings.

Nor do scientific revolutions necessarily change the way that we assess our theories, making different paradigms incommensurable. Over the past forty years I have been involved in revolutionary changes in the way that physicists understand the elementary particles that are the basic constituents of matter. The greater revolutions of this century, quantum mechanics and relativity, were before my time, but they are the basis of the physics research of my generation. Nowhere have I seen any signs of Kuhn's incommensurability between different paradigms. Our ideas have changed, but we have continued to assess our theories in pretty much the same way: a theory is taken as a success if it is based on simple general principles and does a good job of accounting for experimental data in a natural way. I am not saying that we have a book of rules that tells us how to assess theories, or that we have a clear idea what is meant by "simple general principles" or "natural." I am only saying that whatever we mean, there have been no sudden changes in the way we assess theories, no changes that would make it impossible to compare the truth of theories before and after a revolution.

For instance, at the beginning of this century physicists were confronted with the problem of understanding the spectra of atoms, the huge number of bright and dark lines that appear in the light from hot gases, like those on the surface of the sun, when the light is separated by a spectroscope into its different colors. When Niels Bohr showed in 1913 how to use quantum theory to explain the spectrum of hydrogen, it became clear to physicists generally that quantum theory was very promising, and when it turned out after 1925 that quantum mechanics could be used to explain the spectrum of any atom, quantum mechanics became the hot subject that young physicists had to learn. In the same way, physicists today are confronted with a dozen or so measured masses for the electron and similar particles and for quarks of various types, and the measured numerical values of these different masses have so far resisted theoretical explanation. Any new theory that succeeds in explaining these masses will instantly be recognized as an important step forward. The subject matter has changed, but not our aims.

This is not to say that there have been no changes at all in the way we assess our theories. For instance, it is now considered to be much more acceptable to base a physical theory on some principle of "invariance" (a principle that says that the laws of nature appear the same from certain different points of view) than it was at the beginning of the century, when Einstein started to worry about the invariance of the laws of nature under changes in the motion of an observer. But these changes have been evolutionary, not revolutionary. Nature seems to act on us as a teaching machine. When a scientist reaches a new understanding of nature, he or she experiences an intense pleasure. These experiences over long periods have taught us how to judge what sort of scientific theory will provide the pleasure of understanding nature.

Even more radical than Kuhn's notion of the incommensurability of different paradigms is his conclusion that in the revolutionary shifts from one paradigm to another we do not move closer to the truth. To defend this conclusion, he argued that all past beliefs about nature have turned out to be false, and that there is no reason to suppose that we are doing better now. Of course, Kuhn knew very well that physicists today go on using the Newtonian theory of gravitation and motion and the Maxwellian theory of electricity and magnetism as good approximations that can be deduced from more accurate theories—we certainly don't regard Newtonian and Maxwellian theories as simply false, in the way that Aristotle's theory of motion or the theory that fire is an element ("phlogiston") are false. Kuhn himself in his earlier book on the Copernican revolution told how parts of scientific theories survive in the more successful theories that supplant them, and seemed to have no trouble with the idea. Confronting this contradiction, Kuhn in Structure gave what for him was a remarkably weak defense, that Newtonian mechanics and Maxwellian electrodynamics as we use them today are not the same theories as they were before the advent of relativity and quantum mechanics, because then they were not known to be approximate and now we know that they are. It is like saying that the steak you eat is not the one that you bought, because now you know it is stringy and before you didn't.

It is important to keep straight what does and what does not change in scientific revolutions, a distinction that is not made in Structure.[5] There is a "hard" part of modern physical theories ("hard" meaning not difficult, but durable, like bones in paleontology or potsherds in archeology) that usually consists of the equations themselves, together with some understandings about what the symbols mean operationally and about the sorts of phenomena to which they apply. Then there is a "soft" part; it is the vision of reality that we use to explain to ourselves why the equations work. The soft part does change; we no

[5] I am grateful to Professor Christopher Hitchcock for a comment after my talk at Rice that led me to include the following remark in this essay.

longer believe in Maxwell's ether, and we know that there is more to nature than Newton's particles and forces.

The changes in the soft part of scientific theories also produce changes in our understanding of the conditions under which the hard part is a good approximation. But after our theories reach their mature forms, their hard parts represent permanent accomplishments. If you have bought one of those T-shirts with Maxwell's equations on the front, you may have to worry about its going out of style, but not about its becoming false. We will go on teaching Maxwellian electrodynamics as long as there are scientists. I can't see any sense in which the increase in scope and accuracy of the hard parts of our theories is not a cumulative approach to truth.[6]

Some of what Kuhn said about paradigm shifts does apply to the soft parts of our theories, but even here I think that Kuhn overestimated the degree to which scientists during a period of normal science are captives of their paradigms. There are many examples of scientists who remained skeptical about the soft parts of their own theories. It seems to me that Newton's famous slogan Hypotheses non fingo (I do not make hypotheses) must have meant at least in part that his commitment was not to the reality of gravitational forces acting at a distance, but only to the validity of the predictions derived from his equations.

However that may be, I can testify that although our present theory of elementary particles, the Standard Model, has been tremendously successful in accounting for the measured properties of the particles, physicists today are not firmly committed to the view of nature on which it is based. The Standard Model is a field theory, which means that it takes the basic constituents of nature to be fields—conditions of space, considered apart from any matter that may be in it, like the magnetic field that pulls bits of iron toward the poles of a bar magnet—rather than particles. In the past two decades it has been realized that any theory based on quantum mechanics and relativity will look like a field theory when experiments are done at sufficiently low energies. The Standard Model is today widely regarded as an "effective field theory," a low-energy approximation to some unknown fundamental theory that may not involve fields at all.

Even though the Standard Model provides the paradigm for the present normal-science period in fundamental physics, it has several ad hoc features, including at least eighteen numerical constants, such as the mass and charge of the electron, that have to be arbitrarily adjusted to make the theory fit experiments. Also, the Standard Model does not incorporate gravitation. Theorists know that they need to find a more satisfying new theory, to which the Standard Model would be only a good approximation, and experimentalists are working very hard to find some new data that would disagree with some prediction of the Standard Model. The recent announcement from an underground experiment in Japan, that

[6] Another complication: As professor Bruce Hunt pointed out to me in conversation, it can happen that two competing theories with apparently different hard parts can both make the same successful predictions. For instance, in the nineteenth century it was common for British physicists to describe electromagnetic phenomena using equations that involved electric and magnetic fields, following the lead of Faraday, while the equations of continental physicists referred directly to forces acting at a distance. Usually what happens in such cases is that the two sets of equations are discovered to be mathematically equivalent, although one or the other may turn out to have a wider generalization in a more comprehensive theory, as turned out to be the case for electric and magnetic fields after the advent of relativity.

the particles called neutrinos have masses that would be forbidden in the original version of the Standard Model, provides a good example. This experiment is only the latest step in a search over many years for such masses, a search that has been guided in part by arguments that, whatever more satisfying theory turns out to be the next step beyond the Standard Model, this theory is likely to entail the existence of small neutrino masses.

Kuhn overstated the degree to which we are hypnotized by our paradigms, and in particular he exaggerated the extent to which the discovery of anomalies during a period of normal science is inadvertent. He was quite wrong in saying that it is no part of the work of normal science to find new sorts of phenomena.

Kuhn's view of scientific progress would leave us with a mystery: Why does anyone bother? If one scientific theory is only better than another in its ability to solve the problems that happen to be on our minds today, then why not save ourselves a lot of trouble by putting these problems out of our minds? We don't study elementary particles because they are intrinsically interesting, like people. They are not—if you have seen one electron, you've seen them all. What drives us onward in the work of science is precisely the sense that there are truths out there to be discovered, truths that once discovered will form a permanent part of human knowledge.

It was not Kuhn's description of scientific revolutions that impressed me so much when I first read Structure in 1972, but rather his treatment of normal science. Kuhn showed that a period of normal science is not a time of stagnation, but an essential phase of scientific progress. This had become important to me personally in the early 1970s because of recent developments in both cosmology and elementary particle physics.

Until the late 1960s cosmology had been in a state of terrible confusion. I remember when most astronomers and astrophysicists were partisans of some preferred cosmology, and considered anyone else's cosmology as mere dogma. Once at a dinner party in New York around 1970 I was sitting with the distinguished Swedish physicist Hannes Alfven, and took the opportunity to ask whether or not certain physical effects on which he was an expert would have occurred in the early universe. He asked me, "Is your question posed within the context of the Big Bang Theory?" and when I said yes, it was, he said that he didn't want to talk about it. The fractured state of cosmological discourse began to heal after the discovery in 1965 of the cosmic microwave background radiation, radiation that is left over from the time when the universe was about a million years old. This discovery forced everyone (or at least almost everyone) to think seriously about the early universe.

At last measurements were being made that could confirm or refute our cosmological speculations, and very soon, in less than a decade, the Big Bang Theory was developed in its modern form and became widely accepted. In a treatise on gravitation and cosmology that I finished in 1971 I used the phrase "Standard Model" for the modern big bang cosmology, to emphasize that I regarded it not as a dogma to which everyone had to swear allegiance, but as a common ground on which all physicists and astronomers could meet to discuss cosmological calculations and observations. There remained respected physicists and astronomers, like Alfven and Fred Hoyle, who did not like the direction of the growing consensus. Some of them attacked the very idea of consensus, holding out instead a sort of

"Shining Path" ideal of science as a continual revolution, in which all should pursue their own ideas and go off in their own directions. But there is much more danger in a breakdown of communication among scientists than in a premature consensus that happens to be in error. It is only when scientists share a consensus that they can focus on the experiments and the calculations that can tell them whether their theories are right or wrong, and, if wrong, can show the way to a new consensus. It was to good effect that Kuhn quoted Francis Bacon's dictum, "Truth emerges more readily from error than from confusion."

Elementary particle physics also was entering into a new period of normal science in the early 1970s. It had earlier been in a state of confusion, not because of a lack of data, of which there was more than enough, but because of the lack of a convincing body of theory that could explain this data. By the early 1970s theoretical developments and some important new experiments led to a consensus among elementary particle physicists, embodied in what is now also called a Standard Model. Yet for a while some physicists remained skeptical because they felt there hadn't been enough experiments done yet to prove the correctness of the Standard Model, or that the experimental data could be interpreted in other ways. When I argued that any other way of interpreting the data was ugly and artificial, some physicists answered that science has nothing to do with aesthetic judgments, a response that would have amused Kuhn. As he said, "The act of judgment that leads scientists to reject the previously accepted theory is always based upon more than a comparison of that theory with the world." Any set of data can be fit by many different theories. In deciding among these theories we have to judge which ones have the kind of elegance and consistency and universality that make them worth taking seriously. Kuhn was by no means the first person who had made this point—he was preceded by, among others, Pierre Duhem—but Kuhn made it very convincingly.

By now the arguments about the Standard Model are pretty well over, and it is almost universally agreed to give a correct account of observed phenomena. We are living in a new period of normal science, in which the implications of the Standard Model are being calculated by theorists and tested by experimentalists. As Kuhn recognized, it is precisely this sort of work during periods of normal science that can lead to the discovery of anomalies that will make it necessary to take the next step beyond our present paradigm.

But Kuhn's view of normal science, though it remains helpful and insightful, is not what made his reputation. The famous part of his work is his description of scientific revolutions and his view of scientific progress. And it is here that his work is so seriously misleading.

What went wrong? What in Kuhn's life led him to his radical skepticism, to his strange view of the progress of science?

Certainly not ignorance—he evidently understood many episodes in the history of physical science as well as anyone ever has. I picked up a clue to Kuhn's thinking the last time I saw him, at a ceremony in Padua in 1992 celebrating the 400th anniversary of the first lecture Galileo delivered in the University of Padua. Kuhn told how in 1947 as a young physics instructor at Harvard, studying Aristotle's work in physics, he had been wondering How could [Aristotle's] characteristic talent have deserted him so systematically when he turned to the study of motion and mechanics? Equally, if his talents had deserted him, why had his writings in physics been taken so seriously for so many centuries after his death?

…Suddenly the fragments in my head sorted themselves out in a new way, and fell into place altogether. My jaw dropped with surprise, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible.
I asked Kuhn what he had suddenly understood about Aristotle. He didn't answer my question, but wrote to me to tell me again how important this experience was to him:

> What was altered by my own first reading of [Aristotle's writings on physics] was my understanding, not my evaluation, of what they achieved. And what made that change an epiphany was the transformation it immediately effected in my understanding (again, not my evaluation) of the nature of scientific achievement, most immediately the achievements of Galileo and Newton.

Later, I read Kuhn's explanation in a 1977 article that, without becoming an Aristotelian physicist, he had for a moment learned to think like one, to think of motion as a change in the quality of an object that is like many other changes in quality rather than a state that can be studied in isolation. This apparently showed Kuhn how it is possible to adopt the point of view of any scientist one studies. I suspect that because this moment in his life was so important to Kuhn, he took his idea of a paradigm shift from the shift from Aristotelian to Newtonian physics—the shift (which actually took many centuries) from Aristotle's attempt to give systematic qualitative descriptions of everything in nature to Newton's quantitative explanations of carefully selected phenomena, such as the motion of the planets around the sun.

Now, that really was a paradigm shift. For Kuhn it seems to have been the paradigm of paradigm shifts, which set a pattern into which he tried to shoehorn every other scientific revolution. It really does fit Kuhn's description of paradigm shifts: it is extraordinarily difficult for a modern scientist to get into the frame of mind of Aristotelian physics, and Kuhn's statement that all previous views of reality have proved false, though not true of Newtonian mechanics or Maxwellian electrodynamics, certainly does apply to Aristotelian physics.

Revolutions in science seem to fit Kuhn's description only to the extent that they mark a shift in understanding some aspect of nature from pre-science to modern science. The birth of Newtonian physics was a mega-paradigm shift, but nothing that has happened in our understanding of motion since then—not the transition from Newtonian to Einsteinian mechanics, or from classical to quantum physics—fits Kuhn's description of a paradigm shift.

During the last few decades of his life Kuhn worked as a philosopher, worrying about the meaning of truth and reality, problems on which he had touched briefly decades earlier in Structure. After Kuhn's death Richard Rorty said that Kuhn was "the most influential philosopher to write in English since World War II." Kuhn's conclusions about philosophy show the same corrosive skepticism as his writings on history. In his Rothschild Lecture at Harvard in 1992, he remarked, "I am not suggesting, let me emphasize, that there is a reality which science fails to get at. My point is rather that no sense can be made of the notion of reality as it has ordinarily functioned in the philosophy of science."

It seems to me that pretty good sense had been made of the notion of reality over a century ago by the pragmatic philosopher Charles Sanders Peirce, but I am not equipped by taste or education to judge conflicts among philosophers. Fortunately we need not allow philosophers to dictate how philosophical arguments are to be applied in the history of science, or in scientific research itself, any more than we would allow scientists to decide by themselves how scientific discoveries are to be used in technology or medicine.

I remarked in a recent article in The New York Review of Books that for me as a physicist the laws of nature are real in the same sense (whatever that is) as the rocks on the ground.[7] A few months after the publication of my article I was attacked for this remark by Richard Rorty. He accused me of thinking that as a physicist I can easily clear up questions about reality and truth that have engaged philosophers for millennia. But that is not my position. I know that it is terribly hard to say precisely what we mean when we use words like "real" and "true." That is why, when I said that the laws of nature and the rocks on the ground are real in the same sense, I added in parentheses "whatever that is." I respect the efforts of philosophers to clarify these concepts, but I'm sure that even Kuhn and Rorty have used words like "truth" and "reality" in everyday life, and had no trouble with them. I don't see any reason why we cannot also use them in some of our statements about the history of science. Certainly philosophers can do us a great service in their attempts to clarify what we mean by truth and reality. But for Kuhn to say that as a philosopher he has trouble understanding what is meant by truth or reality proves nothing beyond the fact that he has trouble understanding what is meant by truth or reality.

Finally, I would like to describe my own idea of scientific progress. As I said, Kuhn uses the metaphor of Darwinian evolution: undirected improvement, but not improvement toward anything. Kuhn's metaphor is not bad, if we make one change in it: the progress of physical science looks like evolution running backward. Just as humans and other mammal species can trace their origins back to some kind of furry creature hiding from the dinosaurs in the Cretaceous period, and that furry creature and the dinosaurs and all life on Earth presumably can be traced back to what Pooh-Bah in The Mikado called "a protoplasmal primordial atomic globule," in the same way we have seen the science of optics and the science of electricity and magnetism merge together in Maxwell's time into what we now call electrodynamics, and in recent years we have seen electrodynamics and the theories of other forces in nature merge into the modern Standard Model of elementary particles. We hope that in the next great step forward in physics we shall see the theory of gravitation and all of the different branches of elementary particle physics flow together into a single unified theory. This is what we are working for and what we spend the taxpayers' money for. And when we have discovered this theory, it will be part of a true description of reality.

[7] "Sokal's Hoax," The New York Review, August 8, 1996.