A Philosophy of Science[1]

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When I speak of science I do not speak as a scientist or even as a science educator. I speak, rather, as a philosopher with a modest background, interest, and teaching experience in the topics of the history and philosophy of science. I also speak as a concerned citizen who has been active in promoting science education. Finally, I speak as one living and teaching in the state of Kansas where, in recent years, a cultural battle has been waging, especially at the level of the state board of education, over the very definition of science. My judgments on the subject of science are tempered by a deep respect for the arduous efforts and the achievements of scientists—there are scientists and science educators in my immediate family. I am also mindful of (if unimpressed by) what Steven Weinberg calls “the unreasonable ineffectiveness of philosophy” (1993, p. 169). In any event, my task here is not to defend philosophy or to add to the body of knowledge regarded as scientific but simply to reflect on the nature and practice of science.

I shall define science as *a special kind of disciplined objectivity*. At least part of what I am aiming at with this broad definition can be expressed in the form of a parable. Three farmers come upon a turtle stuck on a fence post. The first said, “Well it sure didn’t get there by itself. I’ll bet that ol’ prankster Jimmy Watkins put it up there.” The second said, “Maybe that plank in the grass was leaning against the post. The turtle crawled up, the plank fell down, and the critter got stuck.” The third farmer said, “An

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invisible spirit must have put it up there." The other two looked at their friend in puzzlement. As if in explanation the third said, "Well what are the odds that you'd find a turtle on a fence post?"

The first two farmers offer hypotheses that are in principle testable against experience. They could find Jimmy Watkins, for example, and ask about his whereabouts. They could find out whether he has a predilection for putting turtles on fence posts as a practical joke. Or, they could test whether turtles are capable of climbing up an inclined board and why this might be of interest to the turtle. The third farmer, however, merely restates the problem. An invisible spirit may have put the turtle on the post, but the fact that it is unusual to find a turtle on a fence post is no evidence that an invisible spirit put it there. It is that very unlikelihood that poses the question, but does not answer it.

In my definition of science I refer to *objectivity*. What I mean by objectivity is that science aims at truth. Actually, I'm not entirely sure that "truth" is the right word. One of my friends is a retired physics professor at the university where I teach, Dr. Bruce Daniel. Once I asked him a question about the Big Bang. I was having difficulty expressing my question and so I said, "Well, Bruce, I'm just looking for the truth here." His reply was memorable: "Truth is a theological concept. I'm a physicist and we don't care about the truth, only what works." This brought me up short. As a philosopher I am aware of the lively controversies about the nature of truth (see, for instance, Kirkham 1992). I had always assumed, however, that scientists wanted to get a "true" picture of the world, whatever "true" might mean. Until I can hone the point further, perhaps I should say that science aims at something that is at least a close cousin to truth. After all, those farmers want to know how that turtle got stuck on the fence post.

I qualified the word “objective” in my definition with the adjective “disciplined.” What I mean by this is that science has its characteristic methods. I am of the opinion that there is no such thing as *the* scientific method that is or should be practiced by all scientists. There are statistical methods, non-statistical methods, hypothesis testing, data gathering, exploratory research, and even anecdotal approaches in science. The hypotheses proposed by the first two farmers demand very different methods. In one case, the farmers would need to investigate the whereabouts of the Watkins boy and his recent activities. In the other case, they would need to investigate the behavior of turtles —or even this particular turtle—and their (or its) abilities and motivations to climb up inclined planes of the appropriate angle.

As Aristotle understood, one picks the method most appropriate to the subject matter one is studying. To take less fanciful examples: figuring out extinction of learned responses in rats in a maze requires the mazes, the rats, and ways of measuring their responses. But figuring out whether judges and juries tend to accept or deny free will or something else in their approaches to the law may require questionnaires. It would be unhelpful to run the judge and juries through mazes and one cannot give a questionnaire to a rat. Also of relevance is that scientists aren’t always good at following what they understand as proper method. An article by Stephen G. Brush was published in *Science* many years ago with the curious title, “Should the History of Science be Rated X?” (1974). Brush’s point was that much of what goes on in science that leads to fruitful discoveries is not part of what most of us would recognize as proper procedures in science. An excellent example is the feud between the nineteenth century American paleontologists Edward Cope and O. C. Marsh. According to Richard Milner, Cope and Marsh would “try most any underhanded trick to beat the other to a new discovery” including dynamiting fossil beds! Yet, their rivalry advanced knowledge in paleontology through the discovery and naming of numerous new genera and species of fossil animals and by stocking several major museums with dinosaur skeletons (Milner 1990, pp. 94-95).

So, science is a disciplined objectivity, but what kind? Well, a “special kind”; but what is “special” about this kind of disciplined objectivity? What I have in mind is that science offers *explanations* and these explanations should, ideally, have certain characteristics. I identify six of them:

1. Scientific explanations should aim at *testability* by empirical observation. Scientific explanations should be *verifiable* (or confirmable) by experience but this is not sufficient, for many hypotheses are confirmed by experience without being true. Data are generally capable of multiple interpretations. One might say that different curves can be drawn through the same set of data points. Karl Popper famously added—anticipated by Blaise Pascal as early as 1647 (Pascal 1989)—that *falsifiability* is the *sine qua non* of scientific explanations. That is to say, they must be vulnerable to contrary evidence. I think that Popper is correct, but it is not necessary that a hypothesis pass every test of falsifiability. One can always fudge the data or hand off the problem to one’s graduate students for future reference. For example, one of the more compelling arguments for a stationary earth was the lack of an observed parallax. Yet, scientists accepted the movement of the earth around the sun fully two centuries before the parallax was

measured in the 1830s. Or again, Newton, could not explain why the night sky is dark. This was only possible with the discovery of the red shift in the stars, whose importance was recognized only in the twentieth century by Edwin Hubble. In both of these cases there was sufficient evidence to lead reasonable people to set the problem to one side until a solution could be found, but they provide real examples that a single disconfirming instance, important though it may be, does not necessarily spell the death of a theory.

If a hypothesis, to be considered scientific, must be falsifiable, it does not follow that scientists are merely about the business of looking for disconfirming instances of theories. This is not what scientists do, nor is it the only thing they should do (cf. Gardner 2001). Albert Einstein, in his general theory of relativity, first proposed in 1915, predicted a greater bending of light in gravitational fields than what Newton had predicted. In 1919, the two theories were put to the test when separate expeditions—one to Sobral in Brazil and one to the Island of Principe off Africa's west coast—measured the bending of light from the Hyades star cluster around the sun during a solar eclipse. The results confirmed Einstein's theory and disconfirmed Newton's. Alfred North Whitehead was present when Frank Watson Dyson, the Astronomer Royal, announced the results. According to Whitehead, "The whole atmosphere of tense interest was exactly that of the Greek drama: we were the chorus commenting on the decree of destiny as disclosed in the development of a supreme incident" (Whitehead 1967, p. 10). The tense interest was as much in knowing whether Newton's theory failed as it was in knowing whether Einstein's theory succeeded. In this case, confirmation was as important as disconfirmation.

2. Scientific explanations should be consistent with other things we know about the world, including other scientific explanations. Ideally, there should be what William Whewell and more recently E. O. Wilson called *consilience* of explanations, a coming together of mutually supportive conclusions. When this consilience is lacking, there are problems for science. In the nineteenth century Charles Darwin's evolutionary theory met with great success and the scientific community was converted within a few decades. However, the great physicist, Lord Kelvin, had provided an argument from the cooling of the earth that seemed to demonstrate that there was not enough time for evolution to have occurred. He correctly saw that evolution requires vast time scales, but he put the age of the earth at between 20 and 100 million years old. Most evolutionists tried to cram all of evolutionary history into that time scale, but it was an exercise in making a procrustean bed for the data. However, by the end of the century, Marie Curie was burning her fingers on radiation. With the discovery of radioactivity, a new source of heat was found, making the cooling of the earth an unreliable argument for a younger earth. Another example of consilience is the support given to Darwinian evolution by Mendelian genetics. Darwin had only a very unsatisfactory explanation of the inheritance of characteristics and the mechanism of mutation. A better explanation was provided by genetic theory which was originally developed by Gregor Mendel working on problems unrelated to the immediate concerns of Darwinian evolution. These cases, in addition to being examples of consilience, also illustrate the way that science can become a *cumulative enterprise*, as ever more adequate explanations become available, we begin to draw up a more satisfactory view of the world.

3. Scientific explanations should change our ignorance to knowledge. Scientific explanations provide more than verbal solutions to problems. This is often manifested in the technology that is associated with the theoretical side of science. Benjamin Franklin not only gave us the beginnings of a theory of electricity, he gave us the lightning rod which does real work in controlling the powers of nature. A more recent and even more dramatic example of technologies tied to scientific advance is the harnessing of the power of the atom in the form of weapons of terrible destructive potential and in the form of nuclear reactors to provide energy for daily life. A final example: research in genetics holds out the promise of diagnosis, treatment, and cure of a variety of diseases and disorders. One could fill an encyclopedia with examples like these. The prestige enjoyed by science is in large measure a function of its fruitfulness in helping us to manage our lives and shape our environment.

4. Scientific explanations should lead to new and unexpected results. Science has heuristic *value*. When perturbations were found in the orbit of the seventh planet, astronomers were puzzled because this wiggling of the planet didn't match predictions from Newtonian physics. Two astronomers independently came up with the same hypothesis. Reasoning backwards, they argued that a large planetary object outside of Uranus's orbit might cause the anomaly. Sure enough, when they pointed their telescopes to the sky, there was the planet that we now call Neptune. In this case, the discovery was a result of using the very physics that had been put in question. This story illustrates another point about scientific discovery. *Discovery requires recognition*. Neptune, as it turns out, was already on the star charts, but no one had recognized it as a planet. This is like me overhearing one of my supervisors saying to another, "You'll be lucky to get him

to work for you." Not recognizing the irony in his voice I suppose him to be praising me. If I don't realize that he is actually criticizing me, should I be insulted? Arguably, the answer is that I should not. The insult "works" only if I recognize it *as* an insult.

5. Scientific explanations should be *transparent* to the scientific community. In other words, one must always be willing to put one's data forward and invite others to replicate what one has found. The standard way in which this is done is to vet one's results through a peer-review process. In the late 1980s, Martin Fleischmann and Stanley Pons announced in a news conference that they believed they had achieved cold fusion. The announcement was met with much excitement because of the promise of an inexpensive and abundant supply of energy. However, subsequent research by other scientists around the world failed to support the Fleischmann and Pons results. Some scientists went so far as to accuse the two of shoddy research and wishful thinking. I have no opinion on those accusations, but the fact is that the promise of cold fusion never materialized. This example not only illustrates the importance of Popper's emphasis on falsifiability, it also demonstrates Popper's point that the objectivity of science is guaranteed most by the fact that science is *a communal activity*. The lonely scientist working in his or her lonely lab is at best only the beginning of science. That lonely scientist must go out of the lab and defend his or her results in the face of what is found in other scientific labs.

6. Ideally, scientific explanations are *the best available on a given body of evidence*. Science is open ended without being relativistic or merely subjective. The qualification "given the body of evidence" is essential. The body of evidence is constantly changing as new facts emerge from the various arenas of scientific

investigation. For this reason, scientists must constantly be prepared to revise their estimates of the worth of a given theory in light of the newest evidence. Many years ago, I used to listen to Paul Harvey on the radio while I drove to work. In those days, one of Harvey's favorite hobby-horses was the mercurial character of scientists, always changing their minds about what is true. This displayed Harvey's ignorance of the nature of science. It is a virtue to change one's mind when the evidence demands it. It is a vice to stubbornly cling to one's favored theory even when the weight of evidence is against it. For this, and other reasons, I teach my students that the expression "scientific proof" is something of an oxymoron. The *proof* that there is no largest prime will stand for all time. But scientific evidence is always just short of proof in this sense.

The astute reader will have noticed that I have yet to list as a characteristic of scientific explanations that they be naturalistic. This is because I don't know what to say about this. There is surely something right about this since no current well-confirmed scientific theory steps outside the bounds of the natural world to embrace supernatural causes. On the other hand, Darwin is one of my heroes and he treated the theory of special creation of his day as a rival scientific hypothesis to his own theory of descent with modification. In other words, he didn't argue that special creation wasn't scientific, he argued that it was an inferior scientific hypothesis. Reasoning along similar lines, Philip Kitcher speaks of intelligent design as "discarded," "dead," or "failed" science (Kitcher 2007). (Intelligent design also suffers from a severe paucity of peer-review studies.)

The views of Darwin and Kitcher notwithstanding, it is difficult to understand how science could be anything but naturalistic if its explanations are, as I have argued, testable, mutually supportive, productive of real solutions, heuristic, transparent, and open-ended. The third farmer's explanation of the turtle on the post was that an invisible spirit put it there. In what way might this be satisfactorily tested? Are there other lines of evidence pointing to invisible spirits with a propensity to place turtles on posts? What would be the use of such a theory? Would it lead to the solution of other problems or puzzles? Can one investigate such things in a way that is open to critique from one's peers? Is it an open-ended hypothesis, revisable in light of further evidence? My point is not that these questions must necessarily be answered in the negative, but it is difficult to see how they could be answered so as to make them amenable to scientific evaluation. One thing, however, is clear. If the explanation of either of the other farmers were verified, then the third farmer's hypothesis would become superfluous. Even if neither of the naturalistic hypotheses could be verified, it is difficult to imagine why anyone would give any credence to the hypothesis of the invisible spirit.

Victor Stenger, an emeritus professor of physics and astronomy, argues that allegedly supernatural phenomena can indeed be tested scientifically. The examples he cites, however, such as the efficacy of prayer, could better be viewed as paranormal phenomena. It is now acknowledged that extra-sensory perception, psycho-kinesis, and clairvoyance are amenable to scientific investigation. Stenger points out that, after more than a century of study, the results are disappointing for paranormal enthusiasts (2007, pp. 89-93). Had the results been otherwise, would scientists claim to have discovered something about the *supernatural* or would they search for an enlarged theory of the *natural* capacities of human beings? We may safely assume that there is more to nature than current science knows, and some of what we do not know might seem to us to have a supernatural quality. But why would it not be our concept of the natural itself that would need to be expanded? For example, David Ray Griffin, a philosopher, is more sanguine than Stenger (or me) about the results of parapsychology. But his defense of the paranormal supports the point I am making: it is an enlarged view of the natural, not an appeal to the supernatural, that would be required to accommodate these phenomena (Griffin 1997).

What does it mean to say that science is naturalistic or that it is constrained by a *methodological naturalism*? At the very least, it means that supernatural entities and powers cannot be part of its explanatory apparatus. It also means that science assumes that events take place without violating laws of nature. These senses of naturalism are shared by the discipline of history, including the history of science. If one *assumes* naturalism for the purposes of doing science or history—or any activity, such as plumbing or highway construction—it does not follow that there are no supernatural causes or no interruptions of natural law. (Nor should one automatically assume that the activity of a supernatural agent implies, even in the case of alleged miracles, interruptions of natural laws; see Alston 1989, p. 212). It should be obvious that a methodological constraint does not permit one to draw substantive conclusions about what lies outside of the constraint. If I am on a jury and the judge orders me to disregard a piece of evidence, then I am methodologically constrained to ignore that evidence. It does not follow that that evidence is unimportant or irrelevant. It is possible that it could be the decisive reason for swaying my judgment in another direction. Nevertheless, by the procedures of the law, I am obliged to abide by the judge's order.

Let me close with a few remarks on the limits of science. Are there things that science cannot explain? Perhaps, but it is not an easy thing to know what these are. At the very least we can say that there are two extremes to be avoided. One extreme is to suppose that science tells us nothing about the world. The other extreme is to suppose that we can know nothing about the world apart from science. The dramatic successes of science should be enough to lay the first extreme to rest. These very successes, however, tempt some people to adopt the second extreme. According to this view, science alone is the arbiter of what is true and false. This is a philosophy called *scientism* and should be distinguished from the activity of science itself. Scientism is a philosophical thesis about the scope of science; it is not part of the body of knowledge that we call scientific. (An example of one version of scientism is the philosophical school of thought known as logical positivism.) Of course, scientism is itself either true or false; but if scientism is not a deliverance of science, then by its own standards, it cannot be known to be true. I am happy with this conclusion, but it must surely be unacceptable to anyone who wishes to defend the thesis of scientism. More’s the pity.

It should go without saying—though often it does not—that the acceptance of science as important or even as our primary way of knowing the world does not entail the acceptance of scientism. Likewise, the rejection of scientism does not entail the rejection of science. Scientism reminds me of the story of the boy who lost his toy in the back yard but was found by his mother looking for it under the street lamp in the front yard. “Why are you looking for your toy in the front yard if you lost it in the back yard,” inquired the mother. “Because,” the boy replied, “the light is better here.” The light is often better in science, but the world is bigger than it may know. I think that history tells us truths about the world, about the human past, but it is not clear to me that history is scientific. Unlike physicists, for example, historians don’t seek to locate events in a causal sequence governed by natural law. Nevertheless, history, like empirical science, is a kind of disciplined objectivity; it is simply a different kind, one that must take into account human motivations and purposes. I am also fond of my own sub-discipline in philosophy called metaphysics. By the very meaning of the word, “metaphysics,” or “beyond nature,” there is the suggestion that there are questions too big for science to answer. Some of these questions involve the very nature of science itself, the subject of this brief paper. One leaps to the “meta-level” with such questions and thereby one leaps outside the arena of science proper.

Generally speaking, it is unrealistic and unreasonable to require a theory or a discipline to account for itself. As I have said, the philosophy of science is not itself science. One need not be a philosopher of science to practice good science. On the other hand, it is not unrealistic or unreasonable to require that a theory not render impossible the possibility of its own discovery or the possibility of other ways of knowing. For example, Stenger, the professor of physics mentioned above, claimed, without a hint of irony, that empirical evidence is more and more pointing to the inefficacy of minds. Science, he claims, is showing that this form of epiphenomenalism is true (2007, p. 84). Perhaps Stenger only intends to draw attention to the lack of evidence for an immaterial mind substance. But this is not precisely what he says and his proposal has the earmark of a scientist sawing off the limb on which he sits to do his science. Science itself is a purposive, goal-driven, intentionally activated enterprise, a fact born out by even a passing familiarity with the history of science. When carried to its logical conclusion, epiphenomenalism entails that the scientist's own contributions to physics had nothing to do with the mental processes that he had in formulating them, including the arguments he gives on their behalf. As Whitehead said, "Scientists animated by the purpose of showing that they are purposeless constitute an interesting subject for study" (1971, p. 16). Interesting subject for study indeed. Examples like this—there are many others—convince me that the question of the scope and competencies of science should be of continuing concern to the community of scientists (and to non-scientists), especially as the deliverances of science more and more inform public policy decisions at all levels of government.

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