WHAT SCIENTISTS DO

BY STEVEN BENNER

It is easy to be confused about what science is and what scientists do. In part, this is because scientists do so many different things in so many different ways. By way of illustration, I was a Junior Fellow in the Harvard Society of Fellows in the 1980s. I shared this pleasure with many other young scientists who were also launching their careers within the Society.

One member of my cohort was Gary Belovsky, now a professor of biology at Notre Dame. He was interested in how animals search for food, how this search relates to competition between species, and how nutrients were recycled in the ecosystem. To a layperson, however, Gary traveled in Montana chasing moose and analyzing their droppings.

Another Junior Fellow was Lawrence Krauss, a cosmologist interested in the birth and death of the universe. Lawrence, who later wrote *The Physics of Star Trek*, recently assumed leadership of the Origins program at Arizona State University. As he did his science, Lawrence mostly sat in his office working with equations.

Scientists can approach Nature in different ways.

I was a chemist. I was interested in how the phenomenon of life could be understood in terms of the interactions between its constituent molecules, and how this understanding might help diagnose and treat human disease. What I did all day was make molecules, doing something that looked much like what chefs do when they are cooking in a restaurant kitchen.

Each of us called ourselves "scientists". And yet there was scarcely more similarity in what we did in our daily lives than there is between (for example) an auto mechanic and a symphony conductor. Field work, equations, and cooking sample quite broadly all of human activity.

This notwithstanding, each of us belonged to a traditional field of science having a traditional name, biology, physics, and chemistry (in our cases). These sciences are well respected in modern culture. Further, the views of their practitioners are often accorded special standing in the public square, especially when compared with the views of lawyers, advertising executives and politicians, to mention practitioners of a few other noble professions.

This respect is not irrational. Nearly everyone recognizes that biology, physics, and chemistry have empowered society, in the material and manipulative senses of this term. Empowerment by physics is evident from nuclear power plants, spacecraft that land on the Moon, and television sets, *inter alia*. Empowerment by chemistry is illustrated by the colorful fabrics that we wear, the materials used in our hybrid cars, and the medicines that we take to cure our diseases. Biology has identified genes that cause cancer, viruses that cause AIDS, and vaccines that have all but eliminated small pox, polio, tetanus and diphtheria.

We may not agree that these fields of science have produced "knowledge". We may not know what "knowledge" is. Nevertheless, we must agree that science has produced something that behaves like knowledge should behave. Whatever knowledge is, it should confer manipulative control and predictive power upon those who possess it. Physics, chemistry, and biology have done just that.

In this sense, science seems to be special among other intellectual activities that have engaged the human mind over the millennia, including religion, philosophy and art. Many religions, philosophies, and artistic forms claim to confer "knowledge" of some kind. Yet they do not credibly claim the predictive and manipulative empowerment that the sciences claim, even though they might claim other things, such as aesthetic transformation and personal fulfillment.

It shows no disrespect of transformation and fulfillment as human goals to note that the product of the "knowledge" proffered by religious, philosophical, or artistic thinkers cannot be universally recognized, and therefore does not command universal assent, at least not in the same way that scientific knowledge does.

That seems to be largely because religious, philosophical, or artistic "knowledge" does not generate the manipulative empowerment that science does. You may believe that your faith in the virgin birth has empowered you to do good works. An observer might observe those works and choose not to dispute your claim that your faith has been motivating. But the details lie obscured within your psyche. This is not the case when a scientist tells you that water is H_2O, even though you have never seen either an H or an O.

Simple concepts, like "falsifiability", do not explain how science is empowering

So what is special about science that allows it to create the empowerment that is expected from actual knowledge? Certainly, historians, philosophers, and religious thinkers have been no less interested in understanding reality than Galileo, Newton, and Einstein. We all try to state our propositions in language that makes semantic sense. We all use logic in our arguments. We all refer to the natural world.

What we teach in middle school is that scientists apply something called "the scientific method".

No doubt.

But a century of effort has had difficulty defining what that "method" is. This difficulty is illustrated in the context of a suggestion made by Karl Popper, Michael Polanyi and others. These philosophers suggested that scientific propositions could be distinguished from nonscientific propositions by their being "falsifiable".

This "demarcation criterion", as philosophers call it, is widely accepted, even among scientists. Most scientists believe that it is a good idea to make their propositions falsifiable. Yet this cultural belief immediately creates a new debate around a new question: exactly when is a proposition falsifiable?

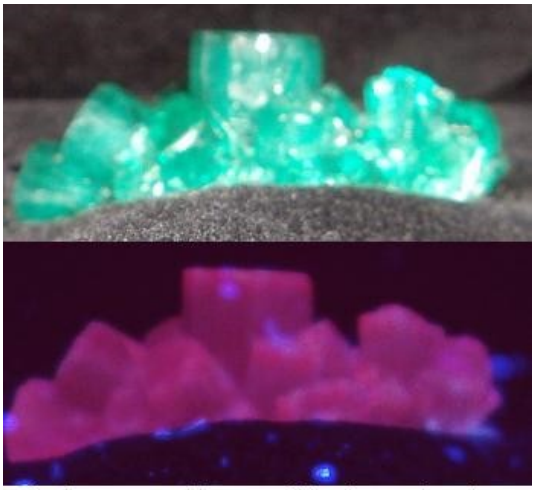
For example, Karl Giberson, executive vice president of the BioLogos Foundation, recently discussed Intelligent Design (ID) with Francis Collins, now Director of the National Institutes of Health. In that discussion, Collins wondered what an Institute of Intelligent Design might study, as "ID doesn't actually propose any falsifiable hypotheses." A clear application of the demarcation criterion, it would seem.

The blogged retort from Casey Luskin from the Intelligent Design community was simple enough. Luskin went to Collins' recent book and found passages where the NIH director had contradicted ID by citing evidence from the structure of the human genome. Collins cannot have it both ways, said Luskin. ID must be falsifiable if observations from the human genome can falsify it. Therefore, ID must be scientific. And so the dispute was not resolved by the demarcation criterion; it simply moved to a new dispute.

As it turns out, falsifiability is not a particularly useful tool for distinguishing scientific and nonscientific propositions. Take a simple law-like proposition that philosophers of science like to discuss: "All emeralds are green". We may regard this proposition as scientific because we can conceive of an observation that falsifies it. We might observe an emerald that is *not* green. Hence, we might conclude that the proposition is "scientific" under the falsifiability demarcation criterion. Not a particularly interesting law, of course. But perhaps we can be satisfied that we are doing "real science".

We can even express this in a syllogism that classical Greek philosophers would recognize (it is called the *contrapositive*). If an observed X is an emerald and X is not green, then the proposition is false.

Unfortunately, things are not so simple in the real world of science. It turns out that whether or not an emerald is observed to be green depends on *how* it is observed, and *who* is doing the observing. For example, an emerald may be observed to fluoresce a red color when observed under ultraviolet light.


Whether or not this emerald is observed to be green depends on how it is observed.

No problem, you say.

The proposition can be changed to read: "All emeralds are green when examined under white light". But even then, the falsification effort does not work if the observer has red-green colorblindness. We must further modify the proposition to read: "All emeralds are green when examined under white light by an observer who does not have red-green colorblindness".

These modifications of the original law constitute *ad hoc* "auxiliary propositions". We invent them to explain away an observation that would otherwise appear to be falsifying. This type of thing seems to defeat the demarcation.

We might say: Fine, scientists are not allowed to modify, *ad hoc*, propositions so that they survive observations that apparently falsify them. Unfortunately, it is not justifiable. We *should* protect propositions from certain contradicting observations. For example, we should not discard a theory if the observing instrument was broken at the time that it generated an allegedly contradicting observation.

The creation of auxiliary propositions *ad hoc must* be done if one does not want to be paralyzed in building models for reality by the exigencies of real world experimentations and observations. Not surprisingly, the history that created the empowerments cited above is littered with *ad hoc* auxiliary propositions.

And this is not even the half of it. The principal atoms in an emerald are oxygen, silicon, aluminum, and beryllium; an emerald is "beryllium aluminum silicate." Now, it is possible to drop on the table a rock that is substantially colorless beryllium aluminum silicate. "Aha!", you say. We have disproven the "law" by finding a non-green emerald. Not so fast. It turns out that mineralogists call colorless beryllium aluminum silicate "goshenite". Mineralogists call aqua-colored beryllium aluminum silicate "aquamarine". Emeralds are green only because they contain a trace of chromium. Thus, the proposition is easily lost in semantics.

These problems become only worse when we move to more complex (and more interesting) scientific propositions, where many propositions are logically connected, including many that we do not even know that we are presuming. For example, it is widely accepted today that the Earth was formed about 4.5 billion years ago and that life has been present on Earth for the majority of the time since it formed.

No problem today, perhaps, but it was a problem in the 19th century as evolutionary theory was being developed. Biologists required hundreds of millions of years of Earth history to explain the observed diversity of life under a model of gradual evolution. Darwin himself used the record of sedimentary rocks to suggest that the Earth was at least 300 million years old.

Some famous physicists disagreed, including the physicist William Thompson, also known as Lord Kelvin. Kelvin was the physicist who helped develop the laws of thermodynamics, one of the most robust sets of laws that physics has ever produced. Kelvin's contributions to physics were so significant that the absolute temperature scale is named for him. We measure temperatures from absolute zero using "the Kelvin", not "the degree Fahrenheit" or even "the degree Celsius".

Starting in 1862 and for forty years thereafter, Kelvin used his understanding of thermodynamics to argue that the Earth could not possibly be as old as evolutionists required. Why? Because the *Sun* could not possibly be so old. Even if the Sun were made of the best coal possible, said Kelvin, it could produce heat at its current rate for only about a thousand years. Kelvin thus held that the laws of physics *disproved* the model of common descent by gradual evolution, key to Darwinian evolution as a theory.

Today, we know that the Sun generates its energy by nuclear fusion and radioactive decay, not by burning coal. This involves the conversion of matter to energy under Einstein's famous $e = mc^2$ equation. Kelvin knew nothing of either. Nor, however, did the evolutionists who stubbornly continued to believe in evolution, despite its having been "falsified" by physics. Illustrating the challenge in defining science as a string of falsifiable propositions, Kelvin's falsification had to be ignored to get the correct answer about the age of the Earth.

The exhortations to "think outside the box" or to "challenge authority" do not serve us here. Had we put Darwin and Kelvin on a stage to have a grand debate, they would never have arrived at $e = mc^2$. Indeed, had someone suggested in 1880 that the Earth was very old and the physicists were incorrect because atoms could fuse to give new atoms with a net conversion of matter into energy, they would have been dismissed as heretics by both camps.

Placing science within the context of the nature of argument

I review these examples of abstract and real science to disrupt the comfortable lessons that you may have been taught about "the scientific method". I also want to emphasize the importance of a more sophisticated understanding of processes within science before people make what they think are "scientific" arguments as they argue in the public square. Once this comfortable complacency is disrupted, we can rebuild a more realistic view of how scientists actually generate empowering knowledge about the real world.

Let us start by recognizing that science is set within a culture. Culture, defined broadly, is a collection of generally-accepted models describing reality (Thomas Kuhn used the word "paradigm"; others have called it a "received view"). Paradigms are so well accepted that members of a scientific community may not even think about them explicitly. Of course, a profound part of the culture underlying science is that something like a "reality" exists. Other examples are more specific to time and field, just as the notion that our Sun can be modeled as a large lump of coal.

Central to the exercise is the recognition that one or more of these paradigms might be wrong. Indeed, individual scientists hope that they might discover that some proposition within the accepted culture is wrong, and become famous for their work that replaces it.

Unfortunately for those hopes, only a subset of the received view is in fact incorrect, and most of that subset is not "ripe" for discovery. Thus, Einstein's general relativity is more correct than Newton's views of how the solar system works, but it was not timely to point this out in 1860; the community was not prepared to discover relativity in 1860 and would not have been able to accept it had it been presented. To paraphrase Clarke, any sufficiently advanced science is indistinguishable from lunacy.

For this reason, practicing scientists who wish to advance their general theoretical framework need to identify parts of the received view that are wrong, but only if that "wrongness" is ready to be found. How do they go about doing this? Generally, scientists start by making observations of the type that have not been previously made. In the case of Francis Collins, one might begin by sequencing the human genome. This is observation on the grand scale, building a model that places over 100 billion atoms in the molecules that support human genetics.

Scientists may then look within these for observations that are puzzling, that are not obviously what is expected given their paradigms, something that needs explanation. Not, for example, that the Sun rose this morning in the East; expected things do not demand explanations. But something that is not, at first glance, like it should be. Explanations are demanded only when things are not as they should be.

Scientists will then attempt to account for the puzzling observation using paradigms within their received view. More often than not, things work out. The puzzle can be resolved. The scientists publish a paper, get promoted within the academy or industry, and move on to the next puzzle.

This exercise, called "normal science" by Thomas Kuhn, sometimes fails. Sometimes, the puzzle cannot be solved by applying the theories and models that are accepted in the culture. This could be, of course, a sign that the puzzling observation being was incorrectly made; perhaps the instrument used to make the observation was broken. Alternatively, the scientist might simply not know enough about currently accepted theory to solve the puzzle. Alternatively, failure to solve the puzzle could indicate that the received view contains an incorrect element.

These three possibilities have different prescriptions. If the observation is wrong, we should repeat the observation; if we do not have enough funding to do the experiment again, we must abandon the puzzle. If we do not know enough physics to solve a problem, we should learn more physics, or perhaps hand the puzzle over to someone else who already knows the physics. But if the underlying theory is wrong, we should try to construct an alternative theory, something new.

In these remarks, we find ourselves knee deep in the sociology of science. What scientists actually do depends on what grants they have, what their university dean is telling them, what is going on at home, or any of many other factors that have nothing to do with the science itself. The most common outcome may be to abandon the puzzle to find another that is easier to solve using extant theory. This sociology accounts in part for the rarity with which scientists actually advance theory. Historically, observations that demand the rejection of a paradigm are often long known long before some scientist picks up the challenge in the way that actually rejects the paradigm.

But let us follow the thread that has a scientist picking up that challenge and actually proposing a new paradigm, one that rejects a paradigm that is part of the culture. At this point, the dynamics change.

First, the scientist who introduced the theoretical innovation becomes interested in seeing that innovation accepted. Those in the community who did *not* introduce the innovation do not have this interest. On the contrary, many are interested in opposing the innovation, perhaps because they themselves introduced the soon-to-be-rejected previous paradigm. Of course, those who did *not* introduce the innovation recognize that they might make *their* career opposing the innovation. If they succeed, then they might be viewed favorably by their peers as "giant killers".

Sociological dangers lie everywhere in this new dynamic. As noted in earlier posts, scientists have control over the data that they consider, but also over the data that they do *not* consider. Scientists control what experiments they do, but also what experiments they do *not* do. Scientists decide when to introduce *ad hoc* hypotheses to explain away apparently negative results, and when *not* to. This leads to what we might call the first law of argumentation:

> *If you control the data to accept and reject from experiments that you can choose to run or not, and if you can apply* ad hoc *propositions as you wish, you can argue yourself into believing just about anything you want.*

And scientists who introduce innovative paradigms generally *want* to believe them, while their opposing scientists often do *not* want to believe them.

This has parallels in the law, advertising, and politics. A lawyer able to freely select the facts can generally convince you that any of his clients are innocent. The salesman in full charge of his message can persuade you to buy just about any product. Any politician in decent command of his rhetorical craft can pick and choose among things known to you to persuade you to vote for him. And any preacher who is allowed to pick and choose can justify any view of the world, and explain away any apparent contradictions.

What science does that is different is to embody a mechanism to manage this process. Science is an intellectual activity that has a process that forces scientists to occasionally come to believe something *other* than what they set out to believe or want to believe.

In most fields of science, the person who sets up an experiment is generally the same person who analyzes its results. Thus, a part of the discipline of training scientists is to get them to understand and manage how they participate in a scientific enterprise that is their own. It is, we teach, not wrong that they have an interest in the outcome of an experiment. Indeed, it is impossible for them not to have an interest. What is wrong is not to acknowledge their own interest, and not to mitigate it using the processes particular for their field.

The processes are different for different fields. In medicine, we insist that pharmaceutical trials be done "double blind", with neither the patient nor the physician knowing who is receiving the drug and who is getting the placebo. In chemistry, we might run an experiment under a range of different, but carefully controlled conditions, something impossible when dealing with human patients.

But in any field, the most successful scientists establish within their laboratories a kind of dialectic. On Mondays, Wednesdays, and Fridays, we believe one thing and act like we believe it. On Tuesdays, Thursdays, and Saturdays, we believe the opposite, and act like it. We might even cherry pick data to make the case "pro" on Mondays, just to see how strong that case is. But if we do, we make sure that on Tuesdays, we cherry pick data to support the "con". Operating throughout is the ability to do experiments, mix reagents, observe stars, or follow moose. These all give reality an opportunity to slap us in the face, to remind us to be not quite so certain that we know what we are doing.

Above all, we teach scientists to distrust all measurements, but to distrust *most* those that confirm what we want to believe. All experiments should be repeated to make certain that their results are reproducible, of course. But the experiments that are most in need of reproduction are those that produced data that support the proposition or theory that the student wants to support.

Scientists can also deliberately set goals to drive discovery. One way is through "synthesis", the act of creating something new following a design based on currently accepted theory. Synthesis has become especially big in biology, where "synthetic biologists" try to create new proteins, or new genetic systems, or new genetic regulatory networks based on what we think we know about living systems. If the theory guiding our design is correct, it should be empowering; the synthetic protein, the synthetic gene, or the synthetic regulators should work. But if the theory is wrong, it might not be empowering. The synthesis will then fail, and fail in a way that cannot be ignored. Thus synthesis drives discovery and paradigm-change in ways that analysis cannot.

But suppose scientists do not establish this kind of dialectic internally? Suppose the scientist, enamored with his innovation, simply becomes an advocate, publishing data to support the innovation while burying data that contradicts it, rationalizing away contradicting observations by introducing *ad hoc* explanations? In this case, the scientist has lost for himself the power of science to discern knowledge. His innovation must now be evaluated by the community.

Fortunately, the community of science offers the opportunity for correction. Unlike in the law, advertising, or politics, science does not have a jury, market, or voter who stands above the dispute, hears both sides, and makes an authoritative decision. Instead, there are rules that deny the existence of an authority. Data are made public. Experiments are open to be repeated in the laboratories who want to attack the innovation. New experiments are designed by others to test the innovation.

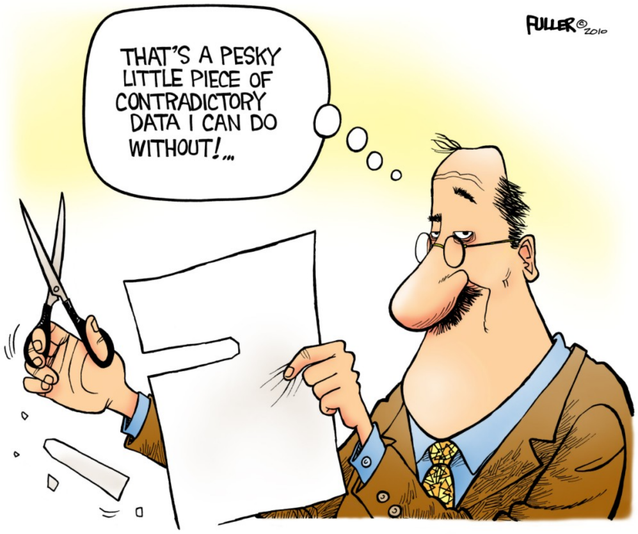
The interaction can be rough and tumble, with advocates on all sides showing little of the dispassionate disengagement of ideal scientists. Sometimes, the dispute is not resolved until the advocating scientists die, to be replaced by a new generation of scientists who can dispassionately evaluate the dispute. But as long as politicians do not intervene, science can be self-correcting.

In this activity, a community can easily become divided into warring parties of advocates. One of these disputes is well known to biologists (but to few other communities of scientist) is the "neutralist-selectionist" dispute, which consumed a generation of evolutionary biologists arguing over whether or not most genetic change influenced the fitness (of a moose, for example).

A dispute well known to chemists (but to few other communities of scientist) is the "non-classical carbocation" dispute, which asked how bonds should be represented in organic molecules carrying a positive charge. Known to physicists (but to few other communities of scientist) is the dispute over the "Copenhagen interpretation", having to do with uncertainty and quantum mechanics. In each dispute, every observation presented by one side was immediately contradicted by data selected by the other. The debates were largely unresolved until the initiating protagonists had left the stage.

How should a community react when our paradigms are challenged? Certainly, we cannot drop everything every time some crackpot decides to challenge what he learned in middle school. Nor can we expect scientific communities, composed of individual humans, to be more liberal than other human communities, where a challenge to orthodoxy is generally met by a response equivalent to "kill the heretic".

But a scientist is not allowed to dismiss this challenge out of hand as ridiculous. Scientists need a process to balance the fact that settled science may be wrong against the need not to waste time with truly ignorant challenges to science.



The first element of this process is a familiarity with the "primary data", the actual observations that underlie the science that is being challenged. A good scientist is always able to answer the question: "So you believe that the Earth is 4.5 billion years old. What are the primary observations that support your belief?" Thus, when a paradigm is challenged, the disciplined scientist can say: "Well, let me see. Suppose our world view is wrong? What primary data must we have misunderstood? What else in our current view of reality would need to be revisited?"

For example, if someone challenges the current paradigm by asserting that the Earth is not 4.5 billion years old, but rather was created by divine intervention 6000 years ago, the correct response is not: "You are crazy".

The correct response is: "Well, maybe. But if that is what happened, then much else of what we think we know must also be wrong. We will need a new explanation for how the Sun gets its energy, as our laws about nuclear physics must be wrong. As this is the physics that has manifestly empowered engineers to build nuclear power plants, we need to explain how they are doing so well even though they are operating with the incorrect laws. The same would go for the empowerment provided by science for the use of radioisotopes in medicine X-rays in dentistry."

All of this empowering knowledge would vanish if the current paradigm concerning the age of the Earth were wrong. Ultimately, it is this interconnection between biology, physics and chemistry, the engineers who are empowered by their laws, and the breadth of observations that are accounted for by those laws, that constrain the search for paradigms in need of revision.

This search is on-going within all of these fields. Evolutionary theory as it is currently structured is not able to explain all of the puzzles that observations of natural biology present us. Physics as it is currently structured is not able to explain all of the puzzles presented by observation of the cosmos. Chemistry as it is currently structured is not able to explain all of the puzzles presented by observations of the molecular world.

It is entirely conceivable that paradigms within these disciplines are ripe for replacement. It is conceivable that the various received views will need dramatic revision. But these replacements and revisions will come from those who create dialectics within their own thinking, are fully conversant with primary data, and are prepared to revisit "settled science" whenever prudent as their views are challenged, not from those who enter the debate as advocates.

Science in the public square

Non-scientists rarely see the kind of uncertainty that drives science forward. The high school science classroom and the distribution science course in college are the end of science education for most lay people. Introductory science courses at both levels are all about teaching fact under the authority of the teacher. A good grade is the desired outcome. Belief in the authority of the teacher is a key to a good grade.

Nor is this perspective on science often on display in the popular press. When scientists appear in the news, they are generally sought for their advice on a matter of public policy. They are asked for certainty, not to express the uncertainty that is at the core of science correctly done.

Accordingly, the public routinely sees scientists as advocates. The supermarket checkout magazines have scientists in white lab coats announcing a new cure for cancer. Should we brush our teeth up and down, or side to side, or in circles? Chances are that someone in a white lab coat has told us to do each of these at some point in our lives. When President Obama appears on television with doctors to support health care reform, his staff has the doctors remove their jackets and don white lab coats. When I first blogged on this site, a principal complaint by intelligent design supporters was that the scientists that they saw were no less advocates than they were.

There is no mystery as to why non-scientist co-opt readily recognized symbols of science. Biology, physics, and chemistry have been empowering in society. Every politician, advertiser, or lawyer wants to have the respect offered to scientists to apply as well to the politics, product, or client that they are advocating. Creation science, Scientology, even social science, the names were chosen to appropriate the mantle of respect that out culture gives to science. It is no accident that Mary Baker Eddy founded the "Church of Christ, Scientist" in 1879, just as our culture was beginning to give science this privileged position of respect.

This provides another reason why it is easy to be confused about what science is and what scientists do. The imagery of science and scientists is widely expropriated in the public square by non-scientists. The temptation to participate in the public dialogue as an advocate is considerable. I myself have been interviewed by reporters who become impatient if I actually practice science before their eyes. It is generally simpler to give an answer rather than to present the context, including all of its uncertainty.

For this reason, it is important, here and elsewhere, for scientists to emphasize that uncertainty is central to science, and advocacy is disruptive of it. When a scientist becomes an advocate, he loses for himself the power to use scientific discipline to discern reality.

So how do things ever get settled in science, at least to the point where personal action or public policy can be based on it? As I described in my book *Life, the Universe, and the Scientific Method*, science proceeds through the successive movement of the burden of proof from one side of propositions to the other as each side meets the culturally accepted standard of proof. That standard is met when a preponderance of evidence favoring one view over another is assembled to the point where it satisfies a community of interested people.

In law, the standard-of-proof is defined by statute. Proof "beyond a reasonable doubt" is required to convict individuals of a felony (O. J. Simpson was *not* convicted in criminal court under this standard). "Preponderance of evidence" is the standard-of-proof used in a civil court (O. J. Simpson lost his civil case to the Brown and Goldman families under this standard).

In science, standards-of-proof are neither legislated nor dictated by authority. Instead, they evolve as part of the culture of a community of scientists. That process is poorly understood, and does not follow clear rules. Because no authority stands above any field to legislate its standards-of-proof, many arguments in science are arguments over what those standards should be.

The intellectual discipline that allows our students to apply process to come to believe things other than what they want to believe, is key to the training of practicing scientists. This process is not easy to teach, not easy to learn, and not painless to apply. It is as difficult for scientists to admit that they were wrong as anyone else. It is as painful to come to believe what one really does not want to believe. But this is the process that leads to knowledge, or at least a view of nature that if not itself knowledge certainly does what knowledge was supposed to do: provide predictive and manipulative power.

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