Creativity versus Skepticism within Science

More harm has been done in science by those who make a fetish out of skepticism, aborting ideas before they are born, than by those who gullibly accept untested theories.

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When a fantastic idea—such as telepathy—attracts a cult following, it is relatively easy for almost anyone outside the cult to test it and disprove it adequately to satisfy at least a majority of scientists. (I will give some examples later.) On the other hand, it takes a real visionary to recognize—and not kill—a promising new idea that seems to initially violate the current establishment view (what Thomas Kuhn famously referred to as "normal science," the kind of humdrum activity practiced by the majority of scientists, the bricklayers rather than the architects of science).

Karl Popper is often credited with pointing out that an idea deserves the coveted title "scientific" only if it makes testable predictions that are stated in a form that allows them to withstand refutation. (This rules out many social "sciences," including historicism, deconstructivism, structuralism, "isms" in general, and much of the pretentious, postmodern nonsense that tries to pass itself off as science.) This aspect of Popper's idea is well known, yet there is the other aspect of his argument that few appreciate: the fact that revolutionary science often begins with a conjecture—a vision that takes you well beyond the existing evidence rather than being constrained by it. Outstanding science is conducted by those who make imaginative excursions into what might be true, i.e., conjectures that are ontologically promiscuous and not merely consistent with existing data.

They are not made by those who are, to use Peter Medawar's phrase, "cows grazing on the pasture of knowledge." If I am right about this, then the danger of gullibility (even among scientists, not just lay people) through accepting bogus revolutions is vastly outweighed by the danger of novel ideas being ignored by skeptics. More damage was done by those who were "skeptical" of Semmelweiss's or Pasteur's germ theory of disease than by those who believed in spoon bending. As Francis Crick pointed out to me once, "It is better to have nine of your ideas be completely disproved, and the tenth one spark off a revolution than to have all ten be correct but unimportant discoveries that satisfy the skeptics." This seems obvious, but why is it so rarely practiced? In my view, there are two reasons, both of them psychological. (This applies mainly to card-carrying professional scientists.)

Cul-de-sac Skepticism

The first reason is what I call the "cul-de-sac phenomenon." People—including scientists—unconsciously gravitate into a cozy cul-de-sac where they feel safe practicing "normal science." There are great social rewards. People who are in the same club engage in mutual admiration and reward each other by funding each other. Their papers are "peer reviewed" by people in their own clubs, and as a result, no one seriously questions the meaning of the whole enterprise or where it is headed. Anyone who dares to do so is in danger of excommunication by the priesthood, so to speak. In this regard, skeptics are not merely useless; they can be an actual impediment to science. By *skeptic*, I mean one who adopts an overall skeptical attitude, being unreceptive to anything new—not one who practices legitimate skepticism toward claims that are empirically unproven. This should become clearer as we go along.

There are many early warning signs of this phenomenon, but the clearest one is the inability of scientific practitioners to question the axiomatic foundations of their discipline. A second warning sign is when a field is dominated by certain catchphrases or by methodology (fMR, sine waves, reaction-time measures, eye movements, EMG, EEG, "working memory," etc.) rather than by questions. The methodology, phrases, and mantras drive the concepts rather than the other way around. This type of Kuhnian "normal science" would be innocuous were it not for the fact that it siphons off 98 percent of funding from those who embark on bold new adventures or pursue anomalies.

More often than not, skeptics succeed in stifling innovation in science with their "conform-or-perish" approach. This is especially devastating for young scientists entering the field. Even the genuinely talented ones are intimidated into conforming—or at least pretending to conform—in order to obtain jobs, funding, or tenure. With the passage of time, the "mask becomes the man," and any trace of originality is beaten out of them. I have sat on many a committee on my campus when a young scientist has published innovative, internationally recognized work, and some skeptic has tried to block hiring or tenure, arguing, "But why hasn't he got any federal funding?" My usual response to this is that there is something lopsided about that argument: surely, funding should be in the denominator, not the numerator ("more bang for the buck"), in these decisions. (Not to mention the obvious fact that being young, the scientist doesn't have cronies in her club yet; funding committees are usually composed of failed scientists who enjoy being "skeptical." Fortunately, there are exceptions; I have known many eminent scientists who sit on these committees.)

If you think I am overstating all this, go to any annual meeting of the Society for Neuroscience in the United States, attended by 30,000 scientists, and walk along the rows and rows of poster presentations. If you go to two of these meetings in consecutive years, you will be struck by an eerie sense of déjà vu. It's as if someone has taken all the key words from the previous year's meeting and shuffled them around randomly in a computer to create the poster titles of the current year.

What is the harm in all this? To be sure, there is so much of it going on that some of it is going to be important simply by accident. The more serious problem is that it makes science lose its soul; it makes the practice of science no longer enjoyable.

For perspective, I will compare eighteenth- and nineteenth-century science with late twentieth-century science. Victorian science was a grand, romantic adventure for those who practiced it; it was motivated by an unquenchable passion for knowledge. This was true whether you were a fledgling scientist or an eminent one (like Faraday, Huxley, Darwin, Wallace, Cavendish, and countless others). One reason for this was that many of them were financially well off—their livelihood didn't depend on science, so they could pursue science for its own sake. While this is still true for a

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small minority of scientists, it isn't true for most. The funding system is supposed to take care of this, but it doesn't work well in the United States—it's not quite as bad in Canada and the United Kingdom—because it tends to be "top-heavy"; those who already have huge grants get more funding, because they are considered a safe bet. So the rich get richer. There is usually no "trickle down." (It's not a coincidence that the same sort of thing happens in the political realm; the so-called economic revolution in India has benefited only the upper 30 percent—no sign of trickle-down yet.)

Science, in other words, has become "professionalized" into just another nine-to-five "job." The only way to reverse this trend is to hang around the genuinely curious and adventurous scientists so that some of their romantic passion rubs off on you (for there is nothing more contagious than enthusiasm). On the other hand, avoid skeptics like the plague until the final stages of "fact checking." As Sherlock Holmes said, "Mediocrity knows nothing higher than itself, my dear Watson; it takes talent to recognize genius."

One has to do this, even if it means a temporary loss of "pats on the back" from others trapped in your own cul-de-sac. "The quest for respectability," Francis Crick once told me, "is the death of science."

There is a second psychological reason why someone becomes skeptical: it makes him or her look intelligent without too much effort. The practitioner not only recognizes that it is much easier than genuine innovation but also hopes it will be misperceived as a sign of high intelligence—the phenomenon of "Aha! I saw through that, so I must be clever." What such people don't realize is that most clever people in the audience have "seen through" the so-called flaw already but are at least willing to give the scientist who is presenting the idea credit for his boldness—for sending up trial balloons while at the same time recognizing their tentative nature.

These skeptics are easily spotted in the audience at scientific lectures—they are the ones who usually miss the main point of the lecture and try to mask this by pretending to ask questions that seem penetrating but are very often nothing more than skeptical: "Is it inconceivable that the effect you are talking about is really due to X, Y, or Z," and so on.

Pursuing Revolutionary Science

One strategy for pursuing revolutionary science is to ignore the skeptics and be on the lookout for anomalies and pursue them with tenacity. Bear in mind that a certain amount of skepticism is actually healthy—even desirable. In that respect, I am completely in tune with this magazine's main agenda. (Semir Zeki once said, "Referees are swine but sometimes swine can lead you to truffles.") It is easy to portray scientists as being shallow folk skating on the surface, narrow-minded and unreceptive to new ideas. But their skepticism doesn't result from stupidity. On the contrary, there are, in fact, very good reasons for being initially wary of new ideas, because of the simple fact that most "anomalies" turn out to be false alarms. There are many crackpot ideas posing as promising anomalies, e.g., polywater, cold fusion, telepathy, clairvoyance, UFOs, angels, Elvis sightings; one could spend a lifetime pursuing these. (One third of all Americans not only believe in angels but actually claim to have seen one.)

So the question for the young scientist is this: how do I know which anomalies to pursue and which ones to be skeptical of? It has been said that some scientists develop a nose for authentic anomalies. If you are not one of these lucky few, you can also use the trial-and-error method to weed out bogus anomalies, but this is time-consuming. A better option is to adopt the following rule of thumb: if an anomaly has been around for decades, has survived many attempts at experimental disproof, and is regarded as an anomaly for the sole reason that you can't think of a mechanism or that it doesn't fit the "big picture" of science, then go after it, for it can lead to a gold mine (e.g., continental drift and bacterial transformation, both of which I will discuss below). But if it is being ignored because the phenomenon itself has been tested repeatedly and found to be flawed, then don't waste time on it, for otherwise, you could spend a lifetime on a wild-goose chase. Telepathy is a good example. The more careful the measurements, the smaller the effect, and that is always a red flag. (Contrast this with the fact that any ten-year-old anywhere in the world can replicate Galileo's famous experiment by dropping a cannonball and a pea simultaneously from a tall building; unlike the case of telepathy, you don't have to keep making excuses for why the experiment demonstrating gravity doesn't work.)

One final point: if you choose to pursue anomalies, there are bound to be some people whose entire lives' work is threatened by those anomalies and will therefore be offended by your efforts. But as Lord Reith remarked, "There are some people whom it is one's duty to offend."

Legitimizing Anomalies

In general, for an anomaly to make it into mainstream science, it has to fulfill three criteria, *all* of which must be in place. First, it must be true, i.e., reliably repeatable. Second, it has to be explainable it in terms of known principles. Third, it must have broad implications for areas of research beyond that of the researcher. Let's take two examples:

In the late 1940s, Oswald Avery et al. determined that DNA was the factor that permitted bacterial transformation, a phenomenon in which one strain of a species of bacterium (such as pneumococcus A) transforms into a different species (such as pneumococcus B) when A is incubated with fluid that has been extracted from B. This had been observed by other investigators in prior studies, but no mechanism for the transformation had been isolated. That observation, which was published in the prestigious *Journal of Experimental Science*, should have sent a tsunami through biology, but it barely made a ripple. In principle, it was a lot like seeing a pig walk into a room and reemerge as a donkey. Yet, it was ignored by skeptics, partly because it challenged one of the basic principles of biology: the immutability of species.

Avery even hinted that this "transforming principle," the DNA molecule, might carry hereditary information, but his findings were ignored (probably as anomalies) before the replication mechanism of DNA was understood (thanks to Watson and Crick). If someone had seen the significance of those findings earlier, molecular biology might have been born much sooner than it was.

Why was Avery's "anomaly" ignored initially? Because while it fulfilled the first criterion of being reliably repeatable and the third criterion of having vast implications (challenging the idea of the immutability of species), too many skeptics were not yet willing to accept it as fulfilling the second: providing a conceivable mechanism for bacterial transformation. But that, as we have seen, is not a good reason for ignoring the discovery.

A second example is continental drift. Like many schoolchildren, Alfred Wegener noticed that the outlines of the facing coasts of the continents fit together nearly perfectly, and based on that, he posited that the continents as we know them now must have split off and drifted apart from a single, ancient supercontinent. He also noticed that the rock strata on the west coast of Africa perfectly matched those of the east coast of South America. Finally, he pointed out that fossils of an order of Permian freshwater lizards, mesosaurs, are found in only two places on earth—you guessed it, West Africa and the eastern coast of Brazil. And the fossilized remains of identical species of dinosaurs were found on the Atlantic coasts of the two continents. Yet the experts—the skeptics—ignored the evidence that was staring them in the face. They did so because it didn't fulfill their criteria: it didn't fit the contemporary big picture of geology ("*terra firma*" and all that), and they couldn't think of a mechanism for continental drift—plate tectonics had not yet been discovered. So the skeptics argued, believe it or not, that there had been a long, narrow (now submerged) land bridge connecting the Atlantic coasts of South America and Africa, across which all the dinosaurs had migrated and died! One wonders what it would have taken to convince these people: two halves of the same dinosaur skeleton, each on a different side of the Atlantic?

Contrast these two with another "anomaly": telepathy. It fulfills criterion 3 (vast implications) but not criterion 1 (repeatability) nor 2 (a conceivable mechanism). So it is legitimately ignored, except by crackpots. Unlike Galileo's Leaning Tower of Pisa experiment, telepathy becomes smaller and smaller the more rigorously you test it, and that's good enough reason to be skeptical.

I will conclude with two incidents from the life of the great German physician, ophthalmologist, and physicist Hermann von Helmholtz. When he invented the ophthalmoscope to view the fundus of the eye, a royal commission, composed mainly of skeptical eye doctors, was set up in England to "evaluate this new German invention." After considerable deliberation, they reported back to the king: "Your Majesty; this German instrument does enable you to look inside the eye but it is not needed for diagnosing any of the known diseases of the eye." Upon hearing this, Helmholtz is said to have remarked, "But that is the whole point."

Now for the second incident: Helmholtz had just produced the first mathematical formulation of the law of the conservation of energy, which says that energy can neither be created nor destroyed, and applied it to his study of the use of energy by muscle tissue. Other scientists argued that it was applicable only to inorganic objects and not living things, because living things have a "vital spirit." To convince the skeptics, Helmholtz set up an open demonstration at a scientific meeting in Europe, in which he showed that the heat output from a living muscle is exactly what you would expect from an inanimate machine (with no vital spirit). He then wrote back to a friend in Germany: "Not a single scientist in the meeting believed a word of what I said. Now I know I am right." In short, Helmholtz's confidence in his own experiments increased in direct proportion to the number of people who were skeptical of them!

There is surely a moral in this somewhere for every aspiring young scientist: listen to the skeptics, by all means, but have enough confidence—even a touch of arrogance—in your research to recognize that the skeptics are as often wrong as right. □

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