'Pathological Science' is not Scientific Misconduct (nor is it pathological)

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Abstract: 'Pathological' science implies scientific misconduct: it should not happen and the scientists concerned ought to know better. However, there are no clear and generally agreed definitions of pathological science or of scientific misconduct. The canonical exemplars of pathological science in chemistry (N-rays, polywater) as well as the recent case of cold fusion in electrochemistry involved research practices not clearly distinguishable from those in (revolutionary) science. The concept of 'pathological science' was put forth nearly half a century ago in a seminar and lacks justification in contemporary understanding of science studies (history, philosophy, and sociology of science). It is time to abandon the phrase.

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1. The Demarcation Issue

How to distinguish proper science from spurious imitations of it, in other words pseudo-science, has long been discussed. Answers were suggested by logical positivism, later via the hypothetico-deductive definition of science, then using the purported falsifiability of scientific theories, and after that Lakatos proposed the progressiveness or otherwise of research programs.

A satisfactory definition of science or formula for demarcation would make it possible to classify specific investigations on the basis of criteria applicable to real cases. Moreover, the classification should be achievable contemporary with the actual investigation, it should not be necessary to await the benefit of hindsight as to whether valid knowledge was obtained. For example, a good demarcation scheme would have allowed contemporaries of Mendel (rules of heredity) or of Wegener (occurrence of continental drift) to testify that their ideas were worth attending to rather than having them ignored for four decades before being taken up again.

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This author agrees with Laudan (1983) that the search for such means of demarcation will continue to be fruitless, for the following reasons. Any definition of proper science should admit the practices long established and held proper in such fields as chemistry, physics, biology, and geology which, everyone agrees, are indeed sciences. But those practices are polyglot in the extreme (Bauer 1992a; Committee on the Conduct of Science 1989). Practicing scientists hardly ever set out deliberately to abide by some criteria for being properly scientific; and it would therefore be surprising in the extreme if they in fact somehow did all so abide.

Modern science depends on communication among and review by peers. One might therefore suggest that isolation from a given research community would be grounds for classing an investigation as pseudo-science. But isolation is a matter of degree, and some iconoclastic individuals have produced valid scientific breakthroughs while working largely alone – Einstein, for instance, or the previously mentioned Mendel and Wegener.

A less sociological view is that of Kuhn (1970) who contrasts the occasional 'scientific revolutions' with normal science in which puzzles are solved without calling into question long-standing data, methods, or theories. Accepting those three things as the essential aspects of science leads to another possible scheme for demarcation (Bauer 2001a, pp. 9-11). In normal science, no great novelty in any of the three is involved. Scientific revolutions introduce startling novelty in just *one* of the three; typically in theory as with relativity or quantum mechanics, but it might also be in data as with the recognition of radioactivity, or in method as with radio-astronomy. Substantial novelty simultaneously in two of the three facets characterizes such ventures as those of Mendel and Wegener, which Stent (1972) described as "premature" science. Finally, attempting novelty in all three aspects of science signifies wholesale cutting loose from established knowledge and might therefore be classed as practicing pseudo-science – were it not for the fact that valid knowledge might sometimes result from such jumps into the deep unknown; natural history, after all, was largely *ab initio* yet nevertheless led to modern science. The study of such purported phenomena as dowsing or psychic effects may yet yield useful knowledge despite the lack of established data, methods, or theories about them.

Even though widely accepted demarcation criteria are lacking (or, for that matter, general agreement about what defines science), observers and critics of science as well as scientists themselves do freely apply the term 'pseudo-science' to such things as UFOlogy and parapsychology. But a detailed comparison of how investigations are carried out in such areas and in the natural and social sciences fails to turn up clear-cut distinctions (Bauer 2001a). That failure is well-nigh pre-ordained, since certain subjects often labeled 'pseudo' subsequently become, if not accepted science then at least no longer pseudo:

mainstream researchers now study acupuncture, generally despised as pseudo in Western science until the 1970s; ball lightning, long dismissed as non-existent, is now studied by physicists and meteorologists. And so on (Bauer 2001a, pp. 23-24).

2. What is 'pathological science'?

While philosophers have generally employed the term 'pseudo-science', many people interested in what is bad science have applied other terms to various controversial topics: *Fads and Fallacies of Science* by the science pundit Martin Gardner (1957) is a classic. Richard Feynman (1974) talked about "cargo-cult" science. Another physicist has recently titled his book *Voodoo Science* (Park 2000). But the only other term than 'pseudo-science' that has achieved wide use is 'pathological science'; and it has been applied most often to cases best known to chemists and physicists (N-rays, polywater, cold fusion), perhaps because the term was originated by the great physical chemist Irving Langmuir (1932 Nobel Prize).

Langmuir's canonical text on 'pathological science' is not, however, a technical or philosophical treatise but simply a talk given in 1953 and published in 1968. Langmuir described pathological science as "the science of things that aren't so", using as examples the Davis-Barnes Effect, N-rays, mitogenetic rays, the Allison Effect, extrasensory perception, and flying saucers (Langmuir 1968).

Langmuir offered six characteristics of pathological science:

1. The magnitude of the effect is substantially independent of the intensity of the causative agent.
2. The effect is of a magnitude that remains close to the limits of detectability; or, many measurements are necessary because of the very low statistical significance of the results.
3. It makes claims of great accuracy.
4. It puts forth fantastic theories contrary to experience.
5. Criticisms are met by ad hoc excuses.
6. The ratio of supporters to critics rises up to somewhere near 50 percent and then falls gradually to oblivion.

However – in keeping with the genesis of these ideas in an informal seminar for researchers at the General Electric Laboratories – Langmuir made no attempt to justify these characteristics as invariably present, or some of them as being sufficient to diagnose pathology. In either case, they do not provide useful criteria for distinguishing bad science from good science (Bauer 1984, pp. 145-46; *Physics Today*, 1990a, b); many praised pieces of research satisfy one or more of Langmuir's criteria for pathology. Richard Rhodes (1997) has pointed out, for example, that Langmuir's measures of pathological science fit nicely the discovery of prions for which Prusiner received a Nobel Prize. High-energy physics deals increasingly with phenomena that can only be detected by computer manipulation in order to remove background noise, in other words the effects are – Langmuir's second criterion – "of a magnitude that remains close to the limits of detectability" and "many measurements are necessary". The use of atomic frequencies as standards of time "makes claims of great accuracy" indeed, to 10 decimal places and more. Criterion 4, "fantastic theories contrary to experience", describes well much of modern cosmology – Big Bang, black holes, strings, 11-dimensional universes and many-world theories, Anthropic Principles, and so on. Criterion 5 describes as pathological what Lakatos (1976) pointed out to be characteristic of regular science, the *ad hoc* modification of subsidiary parts of a theory in order to maintain the core beliefs. Nor does criterion 6 apply to what not only Langmuir but many other people continue to regard as genuinely pathological, such things as parapsychology, UFOs, water dowsing: the ratio of supporters shows no signs of dwindling over a period of decades. (That last point reveals a lacuna in quantitative data about science: we do not know what may be 'normal' growth in research. Langmuir's notion, repeated by Bennion & Neuton (1976) and Franks (1981, p. 128), that pathological science is like an epidemic, with a rapid rise and then a rapid decline in publications, is a speculation, it is not empirically based. I would speculate by contrast that many non-pathological fields that become 'hot' show a rapid rise followed by a marked decline, high-temperature superconductors for example. Actual data on this score being lacking, no diagnosis of pathology should be based on it.)

Langmuir's criteria, then, are no more valid than the many other suggestions as to how to distinguish good science from pseudo-science (Bauer 1984, chapter 8; Laudan 1983). Certainly, appeals to the classic 'scientific method' are not workable (Bauer 1992a, pp. 57-61). Nevertheless, it remains common for scientists to rely on Langmuir's notions rather than on modern views in science studies and for naive discussions of 'pathological science' to appear even in periodicals that might be expected to draw on referees versed in history or philosophy or sociology of science; for example, in 1992 *American Scientist* had an article castigating as pathological "infinite dilution" studies of the effectiveness of certain biological agents, polywater, and cold fusion (Rousseau 1992).

That Langmuir's ideas have seemed convincing to scientists is illustrated by the publication of his talk 15 years after it was given and by re-publication a couple of decades later (Langmuir 1985, 1989). The 1985 version added such examples of pathological science as water dowsing, the canals of Mars, certain reported photomechanical and electromechanical effects, radar observations of Venus, polywater, biological effects of magnetic fields, and the detection of gravity waves. One or another of these versions of Langmuir's talk continues to be cited as authoritative: several references per year are listed in the *Science Citation Index* through the 1990s; and there are some uncountably larger number in such periodicals as *Skeptical Inquirer* that specialize in discussions of pseudo-science and pathological science but are not scanned for the *Science Citation Index*.

3. Scientific Misconduct

Scientific misconduct is no better defined a concept than is pathological science. An increasing rate of uncovered cases of fraud over the last two decades, chiefly the faking of evidence in clinical medicine (Broad & Wade 1982) led to much discussion of possible ways to prevent and to sanction misconduct by scientists. Journals devoted specifically to issues of ethical research were founded, for example *Accountability in Research* in 1993 (a quarterly, ISSN 0898-9621) and *Science and Engineering Ethics* in 1995 (also a quarterly, ISSN 1471-5546).

It has proved impossible to arrive at a definition of scientific misconduct that could be approved by US government agencies (National Institutes of Health, National Science Foundation) as well as by professional scientific societies and industries engaged in scientific research. A Web-site originally sponsored by the National Science Foundation (The Online Ethics Center for Science and Engineering, http://onlineethics.org/) lists many codes of ethical conduct established by various professional groups in engineering, mathematics, and science; the Center for Study of Ethics in the Professions at the Illinois Institute of Technology makes available a similarly wide range of such codes (http://csep.iit.edu/codes/science.html).

The failure to achieve a science-wide consensus underscores how the approved practices in science vary from specialty to specialty. Perhaps the closest to an 'official' statement about scientific misconduct is the "Model Policy for Responding to Allegations of Scientific Misconduct" developed by the Office of Research Integrity of the Department of Health and Human Services (the Office was originally within the National Institutes of Health):

> O. *Scientific misconduct or misconduct in science* means fabrication, falsification, plagiarism, or other practices that seriously deviate from those that are commonly accepted within the scientific community for proposing, conducting, or reporting research. It does not include honest error or honest differences in interpretations or judgments of data.
> [http://ori.dhhs.gov/html/misconduct/model.asp]

To apply this model, one needs to know what the “commonly accepted” practices in science are. In the following analysis of notorious cases often called ‘pathological science’, it will be shown that the perpetrators followed in their criticized work the same practices as they had in their generally approved research.

4. The exemplars of pathological science

Certain researches have been so disdained as to warrant, in the eyes of many scientists and observers, the epithet ‘pathological’ or ‘pseudo-science’. That clearly implies that the criticized work is in some fashion different from good, proper scientific work. But in what manner?

Rousseau (1992) decries the difference as the “loss of objectivity”. But it is very naive indeed to suggest that unless scientists practice objectivity they are being pathological. Objectivity in science is owing not to its practice by individuals but to the mutual critiquing that decreases subjectivity (Bauer 1992a).

The most frequently cited instances of pathological science are in physical chemistry: N-rays from the beginning of the 20th century, cold fusion from the last decade of that century, and polywater in between (late 1960s). Yet in each of those cases, detailed examination of what the central researchers actually did reveals that they were doing nothing different in the supposedly ‘pathological’ work than in their other, considerably lauded scientific research.

N-Rays

N-rays have been referred to innumerable times, but the best scholarly discussions are by Derek de Solla Price (1975) and Mary Jo Nye (1980).

René Blondlot, in France, at the University of Nancy (hence N-rays), announced his discovery of N-rays in 1903: a new form of radiation, emitted by both living and inanimate bodies, able to penetrate aluminum but not lead, able to be refracted by aluminum prisms as light is refracted by glass. For several years, N-rays were studied by scores of scientists in France and hundreds of papers were published. Yet scientists in other countries were not able to reproduce the radiation. An American physicist, Robert Wood, observed the experiments in Blondlot’s lab: in darkness, visual observation was used to detect on measuring scales the spots of light that N-rays produced. Surreptitiously in the darkened room, Wood removed the aluminum prism. The measurements continued to be read out as before. Evidently optical illusion was causing spots of light to be imagined at expected values along the scales. This demonstration convinced almost all the scientific community that N-rays do not exist; but Blondlot and a few others persisted in their belief that N-rays were real.

So presumably what was pathological here was a reliance on visual observation under conditions – a darkened room – where optical illusions readily occur. (One modern test for glaucoma is to note over what field of view one can detect flashes of light on a dark background. Anyone who has taken such a test knows that one 'sees' some number of flashes that are not actually there.) But Blondlot was a distinguished member of the French scientific establishment. He had been particularly praised for showing that X-rays moved at the speed of light *which he had established by the same method of visual observation*, in that case variations in the apparent intensity of electric sparks. Blondlot was therefore very unfortunate; but how can he be blamed for continuing to use a technique that had been so successful? "The curious error of N-rays is much more a sort of mass hallucination, proceeding from an entirely reasonable beginning" (Price 1975, p. 159).

Moreover, the *facts* Blondlot reported were confirmed by a number of his fellow scientists, not only in his laboratory but also elsewhere in France; which gave Blondlot good reason to think his discovery a genuine one. And early in the 20th century, Blondlot was far from alone in looking for new types of radiation. X-rays and radioactivity had been discovered just a decade earlier, and some years before that Hertz had discovered radio waves.

If pathological science is to be regarded as scientific misconduct, then there would need to be some indication that there had been *willful* deception, or at least *quite egregious* incompetence. The record does not support indictment of Blondlot on either of those scores. In point of fact, if anyone behaved unethically during this episode, it would seem to be Robert Wood, who deliberately and surreptitiously interfered with the experiments in order to deceive the experimenters; yet I know of no discussion of the case that does anything but praise Wood for his demonstration that N-rays are not real phenomena.

Polywater

The most thorough discussion of the polywater affair is due to Felix Franks (1981), who was himself engaged in research on chemical and physical aspects of water for many years. He was not himself involved in any work on polywater, but was acquainted with many of the people who were.

Surface science at mid-20th-century was studied perhaps more intensively in the Soviet Union than elsewhere, including how the properties of water are affected by surfaces. In the early 1960s, Nikolai Fedyakin observed that a column of water sealed in a narrow tube slowly and spontaneously formed a second column that did not freeze or boil like ordinary water. A few years later, the internationally respected Boris Derjaguin brought this phenomenon to wide attention as 'anomalous water', which had a 40% higher density than ordinary water and different refractive index and vapor pressure as well as freezing and boiling points.

As with N-rays, the people who studied polywater used the same techniques and general approach as in their other work. Unlike with N-rays, scientists all over the world reported the preparation and investigation of polywater; indeed the very name is owing to a prominent American spectroscopist, Ellis Lippincott. The renowned British physicist J.D. Bernal called anomalous water "the most important physical-chemical discovery of this century" (Franks 1981, p. 49). Polywater was discussed at several of the prestigious annual Gordon Research Conferences (Franks 1981, p. 124).

So what was wrong about polywater?

It turned out that polywater is actually contaminated water. But before one jumps to the conclusion that those who studied polywater were sloppy in their laboratory technique, one ought to realize that the level of impurities responsible for the effect was lower than could be detected by then-available methods. Moreover, the precise nature and source of the contaminants remained unclear: "several of the questions […] raised have not yet received satisfactory answers [… namely] that water *vapor* reacts with quartz more readily than does liquid water […]. Is water adsorbed from the vapor phase onto silicate surfaces a much better solvent than bulk water? Is it more acidic than bulk water?" (Franks 1981, pp. 145-46). The Russian workers had used quartz rather than glass tubing precisely because glass was known to release impurities into water whereas quartz had not been known to. The first major American publication on polywater had made a point of the lack of spectroscopic evidence of any contamination (Franks 1981, p. 71).

So the polywater researchers can hardly be accused of poor, let alone pathological laboratory practice. But it has also been suggested that polywater should have been dismissed on theoretical grounds: the raised boiling point showed that polywater was more stable than ordinary water, and therefore thermodynamics would decree that all ordinary water would have spontaneously turned into polywater, releasing energy in the process. Nobel laureate Richard Feynman remarked by hindsight (Eisenberg 1981) that there could be no such thing as polywater because if there were, there would also be an animal that need not eat food: it would just drink water and excrete polywater, using the energy difference to maintain its metabolism. Such thermodynamic reasoning is invalid, however. It is not enough that one substance be more stable than another for it to transform readily into the other: there must be some feasible mechanism by which it can do so. Nature affords innumerable examples of substances of different stability coexisting. For instance, diamond is a more stable form of pure carbon than is graphite. By Feynman's reasoning, there should be organisms that get their energy by imbibing carbon in the form of graphite (from the ashes of forest fires, say) and excreting diamonds.

Again as with N-rays, the scientists who were tricked by Nature have been accused of unethical behavior, for example rushing too quickly to publish; yet those who discovered that contamination was the problem were equally guilty of rushing to publication – though they were not criticized for it (Franks 1981, p. 159).

Cold Fusion

The most recent major outcry over 'pathological science' was occasioned by 'cold fusion'. A number of books about this episode have appeared, all of them quite strongly pro- or con-. This author, who himself worked in electrochemistry from the early 1950s to the late 1970s, has discussed the merits and defects of these books in several reviews (Bauer 1991; 1992b, c; 1995).

In 1989, Martin Fleischmann and Stanley Pons announced at a press conference at the University of Utah that they had brought about nuclear fusion at room temperature in an electrochemical cell: they had measured heat production too great to explain by other than nuclear processes.

Many physicists dismissed the claims as impossible from the outset, yet confirmations were being announced from all over the world. Within months, however, many of these were withdrawn; other laboratories reported failures to replicate the effect; and a committee empaneled by the US Department of Energy concluded that there was nothing worth pursuing in these claims. Within a year or two, those working on cold fusion had become separated from mainstream scientific communities, holding separate conferences and often publishing in other than mainstream publications. However, at the present time, a dozen years after the initial announcement, a considerable number of properly qualified people continue to believe the chief claim, that nuclear reactions can be achieved at ambient temperatures under electrochemical conditions (Beaudette 2000).

What have Fleischmann and Pons been accused of that was 'pathological'?

They had announced their discovery at a news conference and not in peer-reviewed publication. They had failed to reveal all details of their procedures. The heat effect remained elusive: no one could set up the experiment and guarantee that excess heat would be observed, sometimes it was and sometimes not. They had performed incompetent measurements of nuclear products and then fudged the results. They had failed to understand that nuclear reactions would inevitably release radiation, and that the level of radiation corresponding to the heat claimed to have been generated would have been lethal. Nuclear theory in any case showed that fusion could not occur under such mild conditions, it required higher temperatures and pressures to many orders of magnitudes, as in the interior of stars.

But of all those criticisms, only the one about fudging nuclear measurements can be sustained, and that does not bear on the issue of whether or not cold fusion is a real phenomenon.

Announcing results first at news conferences has become standard practice in hot fields, for example molecular biology and genetic engineering. It was routine during the initial years of excitement about high-temperature superconductors. Also in that field, some workers quite deliberately put misleading information into their publications, correcting them at the last moment only, in order to preserve secrecy (Felt & Nowotny 1992; Roy 1989).

Lack of replicability does not mean that a phenomenon is necessarily spurious. Semiconductors did not become transistors and microchips in the 1930s because the presence of then-unsuspected, then-undetectably-small amounts of impurities made the phenomena irreproducible, elusive. Certain effects of electromagnetic fields on living systems remained difficult to reproduce for a century or more (Bauer 2001a, pp. 125, 132-33). Perhaps only electrochemists would recognize how vast is the number of experimental variables that might affect reproducibility in cold-fusion systems: almost innumerable variations in the physical characteristics of the electrodes and in the electrical regimen as well as all sorts of possible contaminants, conceivably active at levels that might be virtually impossible to detect by other means than their interference with the looked-for effect.

As to theoretical possibility, "Although cold fusion was, in terms of 'ordinary' physics, absurd, it was not obviously so; it contravened no fundamental laws of nature" (Lindley 1990, p. 376). Physics Nobelist Julian Schwinger was among those who proposed explanations for how cold fusion might occur. It may be well to recall in this connection that lasers and masers were also regarded as impossible before their discovery, and indeed by some eminent people even *after* they had been demonstrated (Townes 1999).

Once again, as with N-rays and polywater, it turns out that nothing occurred that could rightly be called pathological. The leading cold-fusion researchers went at their work just as they had at the other research that had established their good reputation, in Fleischmann's case sufficiently distinguished as to warrant a Fellowship of the Royal Society. Fleischmann had always been known as an adventurous thinker, the sort of person – like the astrophysicist Thomas Gold (1999) – whose suggestions are always worth attending to even when they do not work out. His competence was beyond question, and it was not at all uncharacteristic for him to follow apparently far-out hunches. Sometimes they had paid off for him. Moreover, he had ample grounds from earlier work to look for unusual phenomena when electrolyzing heavy water at palladium electrodes, and he had quite rational grounds for speculating that nuclear reactions might proceed in the solid state under quite different conditions than in plasmas (Beaudette 2000, chap. 3).

The single criticism that is not to be gainsaid concerns how Fleischmann and Pons altered the reported results from initial attempts to measure radiation from their cells. But there is more to be noted here about such apparent instances of scientific misconduct. Fleischmann and Pons were tempted into these actions because they had tried to make measurements without properly learning all the ins and outs of the technique: they thought they could measure radiation by just taking a radiation meter and placing it near their cell. In point of fact, a great deal needs to be known about circumstances that can affect the functioning of such instruments (temperature, for example) and about how to eliminate background signals, as well as about how to interpret the measurements. In this, Fleischmann and Pons were falling into the same trap as many of their critics who, without experience of electrochemistry, thought they could connect together some cells and batteries and palladium electrodes and test within days or weeks what the experienced electrochemists had struggled for several years to bring about.

The transfer of expertise across disciplinary boundaries affords great challenges, and this instance illustrates that a superficial view might label as misconduct what is basically a natural result of failing to recognize how intricately specialized are the approaches of every sort of research. Much of the fuss about cold fusion is understandable as an argument between electrochemists and physicists as to whether empirical data from electrochemical experiments is to be more believed or less believed than apparently opposing nuclear theory (Beaudette 2000). To electrochemists it may seem perverse, possibly even scientific misconduct, to rule out of the realm of possibility competently obtained results because some theory in physics pronounces them impossible. To nuclear physicists, it may seem incompetence verging on scientific misconduct for electrochemists to invoke nuclear explanations just because they cannot understand where the heat in their experiments comes from.

As in the case of N-rays, one can plausibly level charges of scientific misconduct against those who denounced the cold-fusion studies. A journalist baselessly charged a graduate student with falsifying evidence of the production of tritium and this charge was published in *Nature*. The legitimacy of work by a distinguished Professor at Texas A & M University was questioned in two separate, long-drawn-out investigations that ultimately found him innocent of any wrongdoing. One participant in the cold-fusion controversy suggested that critics were guilty of "pathological skepticism" (*Accountability in Research* 2000).

5. Paradigm-threatening research

There is no fussing over instances of 'pathological science' within the realms of what Kuhn has termed 'normal' science, the sort of research that most scientists are engaged in, that adds detail to the existing stock of knowledge without bringing into question accepted modes of explanation. 'Pathological' science is rather *revolutionary* science that has (according to the mainstream view) gone so egregiously wrong as to warrant *passionate* denunciation.

But the mainstream is *always* antagonistic to highly novel discoveries or suggestions, even when they become acceptable later: any suggestion that paradigms need to be changed is *routinely* resisted (Barber 1961), sometimes by effectively ignoring the claims (Stent 1972). Yet until a revolutionary suggestion has been adequately investigated, it cannot be known whether it will in the future become a lauded instance of scientific progress or whether it will be relegated to the dustbin of 'pathological science'. Those who took the risk to follow the new possibility are later praised or denounced according to how lucky or unlucky they were: to become fascinated with an unknown that turned out to be a good lead, or with an unknown that turned out to be a dead end.

In point of fact, a notable number of highly accomplished, creative scientists have suffered varying degrees of calumny from mainstream critics – quite often, critics whose accomplishments do not compare with those of the people they criticize – after some of their most ambitious work failed to find approval or agreement from the mainstream. A short list of such cases includes (Bauer 1996 and 2001b, chap., "Luck – or the lack of it"; Bauer and Huyghe 2000):

- Dual Nobelist Linus Pauling, according to some the greatest chemist of the 20th century, was unconscionably denigrated by some critics for his later work on the value of large doses of vitamin C and other vitamins.
- C.G. Barkla came to grief over the claimed J-phenomenon (Wynne 1976, Wallis 1985) shown by X-rays; yet his Nobel Prize in physics, in 1917, had been awarded for work on X-rays.
- Sir Arthur Eddington, early exponent of relativity and pioneer in theoretical astronomy, produced a 'fundamental theory' that is generally regarded as numerological nonsense (Slater 1957).
- Astronomer Halton Arp was refused further use of telescopes in the United States for denying contemporary dogma concerning red-shifts (Arp 1987, Marshall 1990).
- Hannes Alfvén was awarded the Nobel Prize in 1970 for work on space plasma, yet "many regard his cosmological ideas as belonging to the fringe, and researchers who study his cosmology say they get no public support" (Brush 1990, Marshall 1990).

Just as with Blondlot, Derjaguin, and Fleischmann, there is nothing in the record to suggest that these accomplished scientists had taken a different approach in that part of their work that was called egregiously wrong as they had taken in that part of their work for which they had received high praise. One cannot therefore accuse them of scientific misconduct. It is just the case that seeking new knowledge is fraught with difficulties; and there is no formula for scientific research procedures that can guarantee that false trails will not be followed.

Still, some critics have argued, Blondlot and these other practitioners of 'pathological' science ought to have recognized their errors after their more level-headed mainstream colleagues had pointed them out. Here again, however, one needs to distinguish normal science from (potentially) revolutionary science. The great breakthroughs that are praised by hindsight also came about because their proponents stubbornly, pig-headedly continued to go their own way despite lack of agreement from their peers. As Nobelist Martin Perl put it, "you have to be stubborn and willing to be alone" (Mooney 1996). Even when one's hunch seems not to be borne out by initial experiments, or if success is fleeting or irreproducible, being stubborn can pay off: for example, Jacob and Brenner on the way to discovering messenger RNA, "sure of the correctness of our hypothesis [… we] started our experiment over and over again" (Grinnell 1996).

The most striking potential discoveries bring about revolutionary paradigm shifts. The accepted rules and procedures for doing normal science are not adequate to bring about potentially revolutionary science: as is well known, hard cases make bad laws. Apparently unreasonable persistence and willingness to follow far-out hunches are needed for the great breakthroughs, but they may equally lead to intellectual disasters. Similarly headstrong researchers of similar background, for example Albert Szent-Györgyi and Wilhelm Reich, acquired in the one case a Nobel Prize and in the other the label of crank (Bauer 1992a, p. 61), yet it is far from obvious where and when Reich took a turn that would irretrievably lead him into error (Bauer 2001a, pp. 156-63). As I.J. Good (1998) has remarked, geniuses are cranks who happen to be right; as equally, of course, some cranks may be geniuses who happen to be wrong.

The manner in which research is carried on depends inevitably on the state of the art in the particular specialty. There is inevitably more speculation and persistent sticking with hunches in areas where comparatively little is known than in well traversed fields (Bauer 2001a). The most innovative and exploratory investigations inevitably carry higher risks of going wrong. They will more frequently lead down false trails than to genuine paradigm shifts; but when they succeed, the success is also more significant than are the routine, everyday successes of normal science.

Nothing is to be gained by castigating those who followed false paths in good faith and with the honest determination to add to human knowledge. 'Pathological science' is an epithet applied to *potentially revolutionary* discoveries that did not pan out. The passionate disdain implied by the phrase is not justified by the actions of those who have been so criticized. Rather, it may be an instance of *odium scholasticum*: the criticism is so furious not because the thing is so far removed from the acceptable, but because it comes so infuriatingly close to being remarkably right.

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