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Debate

Scientific Dissent: Reception and Fate of Premature Claims and Hypotheses

Ernest B. Hook

Editors note: this article is, in part, a response to Henry Bauer's provocative article in the summer 2021 issue, "Fact Checking Is Needed in Science Also," in which Bauer argues that the stifling of dissent has impeded scientific advance. Following Hook's critique, Henry Bauer responds.

Recent concern about "fact-checking" of political claims, has led to a similar concern about statements allegedly supported by "science." A recent article in *Academic Questions* maintained that "minority views in science are given no shrift, that they are *automatically* dismissed . . . " (my emphasis), and deplored this phenomenon.[1]

This raises at least two questions: 1) Is this a true description? 2) If so, is it justified, or as the author implied, deplorable?

I argue here first that the statement a "minority view" in science is *automatically* dismissed is simply false; second, that the issue of the fate of minority views in science is nuanced and complex, and cannot be simply characterized; and third, when "minority" claims or hypotheses are dismissed by many in the scientific community, this has happened because of very good reasons internal to the discipline, either because of failures to replicate claims, or the inconsistency of hypotheses with accepted doctrines, even in the rare cases when they eventually turn out to have been correct.

But what exactly is a "minority" view in science? Literally, it's a viewpoint which more than fifty percent working in a specific field oppose. But the term is often used to refer to a view supported by only a very few, by scientific "dissenters," so to speak.

1 Henry H. Bauer, "Fact Checking is Needed in Science Also." *Academic Questions* 34, no.2 (2021):18-30 (notably 23).

Ernest B. Hook is professor emeritus, School of Public Health, University of California, Berkeley; may be contacted at ebhook@berkeley.edu.

Variation in the Fate of Minority Positions in Science

Note any scientific claim or hypothesis in a field when first enunciated is in one sense, at the time first proposed, a "minority view," and may have a number of fates: a) it may of course be immediately accepted by a majority of workers in the field, or at least go unchallenged; b) it may be more slowly accepted as others successfully repeat the work or confirm the hypothesis; c) it may be ignored as having little interest to workers in a field; d) it may be rejected by most scientists because the claim cannot be replicated and/or appears implausible, or the proffered hypothesis seems too far-fetched; e) it may remain an area of ongoing dispute. The outcome may change categories, for example, when work is initially rejected by many is later accepted, and vice versa. But there may still be a sizable proportion who support the view or at least tolerate it, i.e. do not actively oppose it. And even if the minority position is rejected by the vast majority in a field, a vigorous but small group may continue to pursue the matter, publishing in journals and other outlets confined to the field which circulates among its small band of denizens.

Furthermore, most scientists in a field work in a narrow specialty. New claims or hypotheses, however dramatic, are often not directly relevant to their own work, not likely to be of great interest, or are outside their area of specific expertise.

I emphasize that opposition to a viewpoint is often not static, in the sense that the viewpoints of scientists working in a field may—and often have—vary markedly over time. As one example, consider the views of evolution among biologists. First was the issue of whether evolution of flora and fauna occurred at all, and species were not fixed as described in scripture. Then among those who accepted secular variation in species, were different proposed *mechanisms*. Lamarck, for instance proposed transmutation of species, as well as inheritance of acquired characteristics. Darwin's subsequent view of evolution through natural selection, postulating a branching tree of life, went through wide fluctuations in acceptance by biologists. After initial enthusiasm among many who did not accept the Bible literally, there was wide skepticism around the end of the nineteenth century. Only developments starting early in the twentieth century culminating in what was termed "the modern synthesis" integrating genetics led to more widespread acceptance, although debate persists about numerous aspects, e.g. how *much* contribution Darwinian mechanisms make to evolution.[2]

This change occurred over many decades. Recognition of DNA as the genetic material occurred much more quickly after the report of Oswald Avery and his collaborators in 1944 on transformation of bacteria with DNA extracts. This provided what has been regarded as the first good evidence that DNA made up the gene, and not proteins, as had been widely believed.[3] The episode illustrates the nuances in discussing "minority" views. Here the question revolves about the field involved. Avery and his collaborators were microbiologists and clinicians, working with bacteria. Other microbiologists quickly replicated their work on transformation in other bacterial species. Protein chemists, however, especially one of Avery's eminent colleagues, argued that a small amount of protein contamination in the extract used could have been present in the sample, and it, not DNA was the informational molecule. This despite the fact that enzymes that destroyed proteins did not affect DNA or the achieved transformation effect, nor later work that the enzyme that destroyed DNA but had no effect upon proteins, also abolished transformation. The vast bulk of geneticists worked in higher organisms, had no biochemical expertise, and could not evaluate this criticism. Most neither rejected nor accepted the claim for DNA mediated transformation if they were aware of it, but ignored it. There was little they could do with the finding. A few were prompted to investigate further, and within a decade other evidence, notably the double helix model proposed by Watson and Crick, which indicated how the structure of DNA provided a plausible model for gene replication, convinced many of its genetic role. Yet as late as the 1960s the eminent ecologist Barry Commoner, not a geneticist, expressed skepticism about the accepted role of DNA and even as late as 2002 he continued to challenge the central dogma of molecular biology in highly misleading ways,[4] but has, appropriately, been ignored.

Alleged Examples of Automatic Dismissal of Minority Views

Consider some specific examples offered in a recent issue of *Academic Questions* as evidence that "minority views are given no shrift, [are]

2 Peter Bowler. *Evolution: The History of an Idea* (Berkeley and Los Angeles: University of California Press, 2009).

3 Maclyn McCarty, *The Transforming Principle: Discovering that Genes are Made of DNA* (New York & London, W.W. Norton & Company, 1985).

4 Barry Commoner, "Unraveling the DNA myth," *Harpers* 304, no.2 (2002): 39-47.

automatically dismissed," even though emanating from eminent scientists, and the implication that advancement of science has suffered thereby.[5]

Linus Pauling, "*the* outstanding chemist of the twentieth century," was maligned, it is said, for advocating the health benefits of much larger intakes of vitamins than the official Recommended Daily Allowances (emphasis in the original). But even in his own field, despite his eminence, Pauling blundered badly at least once, making an elementary mistake in chemistry when he proposed a three stranded structure for DNA. The proposal contained such obvious errors, that as James Watson wrote, it was immediately repudiated.[5] In the field of nutrition and preventive medicine, Pauling had no expertise, and his enthusiasm for huge doses of Vitamin C, and allegations that it could cure cancer, *was* investigated by Mayo Clinic investigators (who reported negative results).[6]

Pauling challenged their study, but they had not dismissed his claim out of hand. If Pauling was maligned, it was for going beyond the available evidence, and embracing enthusiastically what was and is widely regarded as quack medicine.

Nobelist Kary Mullis was "maligned for pointing to absence of proof that HIV causes AIDS." Mullis, a biochemist, neither a virologist, an epidemiologist, nor a clinician, like other AIDS denialists, was too focused on the fact that the interval between HIV infection and AIDS may be very prolonged, and that symptom complexes similar to AIDS may have other etiologies. He ignored the fact that, ethically, some causal associations involving human disease cannot be proved as in a laboratory investigation of chemical reactions. For example, as tobacco companies liked to claim, it's never been proven that a single person got lung cancer from smoking. Indeed, it would be impossible to prove after the fact. Moreover, even to attempt to prove *sensu strictu* that smoking causes lung cancer in humans would require an unethical controlled study, with *random* assignment of exposure. (This would overcome the objection of the eminent statistician R.A. Fisher, himself a heavy smoker, who postulated that a gene that predisposed people to lung cancer and to smoking tobacco caused the association. He was simply wrong.[7])

5 Bauer, "Fact Checking is Needed in Science Also," 23.

6 Edward T. Creagan et al., "Failure of high-dose vitamin C (ascorbic acid) therapy to benefit patients with advanced cancer: a controlled trial." *New England Journal of Medicine* 301, no. 13 (1979): 687-690; Charles G. Moertel et al., "High-dose vitamin C versus placebo in the treatment of patients with advanced cancer who have had no prior chemotherapy: a randomized double-blind comparison," *New England Journal of Medicine* 312, no. 3 (1985): 137-141.

7 Sander Greenland, "Invited commentary: Science versus public health action: Those who were wrong are still wrong," *American Journal of Epidemiology* 133, no. 5 (1991): 435-436.

Similarly, because of the nature of the virus, and absence of a good animal model, to prove strictly that HIV causes AIDS one would need to infect randomly selected human beings with the virus and compare this group with a sham infected random control group, in a double-blind experiment. Who would volunteer for such an experiment? Who would even consider doing it? It would be analogous to Nazi experiments on humans.

Peter Duesberg, a "ground-breaking cancer researcher, was effectively ex-communicated for pointing out that HIV could not cause AIDS." Duesberg *claimed* this, he did not "point it out" as if it were an overlooked fact, which it isn't. His background is in biochemistry and molecular biology, not in medicine or public health. He hypothesized initially that AIDS was caused by chemicals taken by gay men to enhance sexual experience, and later expanded his claims to include other substances. Had he had any epidemiological or medical experience, he might have realized how unlikely this explained those who got AIDS after self-injection with unsterilized needles, or received a blood transfusion or a blood product to treat hemophilia. After isolation of the HIV virus he continued with undiminished vigor to claim AIDS had his postulated etiology. This despite the sudden drop in AIDS cases in those receiving transfusions after blood banks began to screen for HIV in the blood supply, and in hemophiliacs after appropriate measures were taken. Duesberg's view implied that costly public health measures to prevent or treat AIDS were unnecessary. The South African government happily followed his advice, avoiding considerable expenditures on effective preventive or treatment measures.[8] This resulted in avoidable deaths of over three-hundred thousand people.[9] Independent work has not supported his claims, which is inconsistent with the epidemiology of AIDS. The claim he was "ex-communicated" implies he was cast out of some academic community. But he was not terminated at the University of California, Berkeley despite outside complaints that he be disciplined, nor expelled from the distinguished National Academy of Sciences, membership in which gave prominence to his allegations. Certainly, many in the scientific community regard him as a pariah. But this has not prevented his theory of malignancy originating in

8 Philip Alcabes et al., "Incubation period of human immunodeficiency virus," *Epidemiologic reviews* 15, no. 2 (1993): 303-318; Pride Chigwedere, Max Essex. "AIDS denialism and public health practice," *AIDS and Behavior* 14, no. 2 (2010): 237-247.

9 Nicoli Nattrass, "AIDS and the Scientific Governance of Medicine in Post-Apartheid South Africa," *African Affairs* 107, no.4 (2008): 157–176.

changes in chromosome number and not in gene mutations from receiving a respectful hearing.

Nobelist Luc Montagnier (who received the prize for discovering the HIV virus that causes AIDS) was "laughed at" for giving credence to "possible efficacy of drugs at homeopathic level dilutions," i.e. levels so small there could hardly be one molecule in an ordinary dose. Montagnier, in work completely unrelated to his expertise in virology, has claimed that DNA "solutions" so diluted they could not contain a molecule of anything but the solvent were producing electromagnetic signals![10] Incredulity characterizes the general response of the vast bulk of the scientific community aware of this claim, rather than laughter.

Roger Pielke "withdrew from publishing about climate change because he had been so viciously harassed for skepticism about the role of carbon dioxide." The reference provided in the *AQ* article indicates this refers to Roger Pielke Jr. a political scientist working in public policy and sports governance, and is not a trained meteorologist (unlike his eminent father Roger A. Pielke Sr.). The original article indicates it was his skepticism about the extent of historical changes in climate (not carbon dioxide) that provoked disagreement, and his web site indicates he still appears to be publishing on this matter.[11]

Martin Fleischmann, the "highly respected electrochemist . . . was sent beyond the pale for claiming to have evidence of nuclear reactions at ordinary temperatures," i.e. he had achieved cold fusion. If he was correct, then this would imply vast amounts of very cheap energy could be generated readily. Fleischmann's first report was atypically presented at a press conference, and with much hoopla, rather than in a peer reviewed article, or at a conference, completely in violation of accepted scientific practice. Moreover, neither at the press conference nor in his subsequent publication were sufficient details available. Virtually all who attempted to replicate his results failed to do so, most of those who did later retracted claims, and his eventual published paper was regarded as sloppy and incomplete. It was denounced as pathological science and a result of incompetence and delusion. Fleishmann's initial results were hardly ignored when initially reported. If anything they got immense attention, and many tried to replicate his results without success. Moreover, he was not

10 Luc Montagnier et al., "Electromagnetic signals are produced by aqueous nanostructures derived from bacterial DNA sequences," *Interdisciplinary Sciences: Computational Life Sciences* 1, no. 2 (2009): 81-90.

11 Roger Pielke Jr., "My Unhappy Life as a Climate Heretic," *Wall Street Journal*, December 2, 2016; Roger Pielke, Jr. website, https: // rogerpielkejr.com, accessed 06/28/2021.

exiled from the scientific community, but continued in a research position until his retirement.[12]

Despite the fact that Fleishmann's and Pauling's claims got great attention, and were *not* dismissed out of hand, failure to confirm them led to widespread dismissal. Especially in the field of cold fusion, there still are many true believers espousing their doctrines, and publishing in journals out of the mainstream.

Prematurity in Scientific Discovery: An Approach to Dissident Science

To slightly reformulate a definition proposed by Gunther Stent, one may define as "premature" a scientific claim or hypothesis if its implications *cannot be connected by a simple series of logical steps to canonical, or generally accepted knowledge in a field*.[13]

Stent suggested that it was appropriate for scientists to ignore such claims or proposals to avoid being overwhelmed by scientific cacophony. The examples I have reviewed indicate that however eminent a scientist may be, hypotheses or claims of an experimental result that are "premature" are appropriately rejected or ignored by the scientific community until such time as supportive or confirmatory work of a different nature appears.

The chemist and philosopher of science, Michael Polanyi, maintained members of a scientific discipline must have a shared view of the nature of things, and conduct their research in that shared light. Moreover, they must presume that evidence or hypotheses offered at variance with shared view is incorrect. To be accepted, a proffered claim or concept must have "a sufficient degree of plausibility."[14] In fact he believed it appropriate that an earlier scientific report of his own had been appropriately rejected and not followed up, even though it was later found to be correct.[15] He wrote:

> Scientific publications are continuously beset by cranks, frauds and bunglers whose contributions must be rejected if journals are not to be swamped by them. This censorship will not only eliminate obvious

12 Martin Fleischmann, Stanley Pons, "Electrochemically induced nuclear fusion of deuterium," *Journal of Electroanalytical Chemistry* 261, no.2A (1989): 301–308; Dennis l. Rousseau, "Case studies in pathological science," *American Scientist* 80, no. 1 (1992): 54-63.

13 Gunther S. Stent. "Prematurity and uniqueness in scientific discovery," *Scientific American* 227, no. 6 (1972): 84-93; Ernest B. Hook, ed. *Prematurity in Scientific Discovery: On resistance and neglect*, (Berkeley and Los Angeles: University of California Press 2002), 8.

14 Michael Polanyi, "The Republic of Science: its Political and Economic Theory," *Minerva* 1, no.1 (1962): 54–73.

15 Michael Polanyi, "The Potential Theory of Adsorption," *Science* 141, no. 3585 (1963): 1010-1013.

> absurdities but must often refuse publication merely because the conclusions of a paper appear to be unsound in the light of current scientific knowledge.[16]

Moreover,

> [even if] " . . . a paper has come out signed by an author of high distinction in science, it may be totally disregarded, simply for the reason that its results conflict sharply with the current scientific opinion about the nature of things."[17]

Linus Pauling's proposed triple helix for DNA provides an example of the latter. Of course, some premature claims or hypotheses *are* eventually accepted by the community of scientists after further work has established their validity, despite initial skepticism. Notable examples include not only the informational role of DNA rather than proteins as the genetic substance, discussed above, but Ida Noddack's correct but ignored claim in 1934 that Enrico Fermi and his colleagues might have achieved what was later termed "nuclear fission" rather than produced transuranium elements as they had suggested, and Warren and Marshall's suggestion that a bacterium, not acidity caused gastric ulcers. With regard to the latter two episodes one may defend the view that initial skepticism was appropriate until later work supported the suggestions.[18]

As Polanyi eloquently summarized matters, this is a necessary price the scientific community must pay to avoid being overwhelmed by cacophony.

> For scientific opinion may, of course, sometimes be mistaken, and as a result unorthodox work of high originality and merit may be discouraged or altogether suppressed for a time. But these risks have to be taken. Only the discipline imposed by an effective scientific opinion can prevent the adulteration of science by cranks and dabblers."[19]

16 Polanyi, "The Republic of Science," 1010-1013.
17 *Ibid.*
18 Ernest B. Hook "Interdisciplinary Dissonance and Prematurity: Ida Noddack's Suggestion of Nuclear Fission," in *Prematurity in Scientific Discovery*, 124-149; . Robin Warren, Barry Marshall, "Unidentified curved bacilli on gastric epithelium in active chronic gastritis," *The Lancet* 321, no. 8336 (1983): 1273-1275.
19 Polanyi, "The Republic of Science," 1010-1013.

How then one may ask, can a proponent of a premature hypothesis or claim seek to have his view made acceptable to the relevant scientific community? Simply by seeking to connect the hypothesis or claim to the implications of generally accepted knowledge in the field. Or in the case of a claim that others cannot consistently be replicated, to seek out the explanations for the discrepancies.