SECOND OPINIONS ON 'PATHOLOGICAL SCIENCE'

If Irving Langmuir's posthumous article (October, page 36) is interpreted as providing some characteristics of those experiments that *may* be "pathological" and therefore should be scrutinized with more than the usual care, it will give the scientific community a valuable tool.

The language and tone of the article, unfortunately, encourage quite another interpretation of those characteristics: one in which the characteristics are in and of themselves sufficient to brand an experiment as "pathological," thereby sidestepping any need for "bothersome" critical analysis of the methods and claims of the experiment. Since the characteristics constitute acceptable practice, trivial, incidental errors or features wholly external to the experiment, this interpretation does science a considerable disservice. Already I have seen the label "pathological science" applied to experiments without any attempt at real analysis.

The underlying pathology that Langmuir identifies is subjectively biased measurement or data selection. His second characteristic—that the effect is close to the perceptual threshold, or a small per-trial effect size is made statistically significant by combining many observations—boils down to there being an opportunity for this underlying pathology to enter. But the existence of the opportunity does not establish the existence of the fault. The necessary precautions (blind judging or including unambiguous criteria in the initial design) are now routinely taught as part of undergraduate laboratory practice and are generally applied. Indeed, if this characteristic constitutes a fault, then a very large proportion of all contemporary experimental science would have to be discarded.

The fourth characteristic—"Fantastic theories contrary to experience are suggested"—could be rephrased in a more positive way as "Exciting new theories invalidating previous thinking are suggested." Unless one can point to a logical flaw or to an actual experimental result contradicted by the new theory, one has no objective basis for deciding whether a theory is pathological or "merely" revolutionary. In any case, truly revolutionary experiments are frequently accompanied by flawed attempts to explain them. It is ironic that Langmuir's primary example of a theory that isn't "very sensible"—"the whole idea of an electron being captured by an alpha particle when the alpha particles aren't there, just because the waves are there"—appears to be a simple statement of quantum tunneling as expressed in the language of the then current de Broglie pilot-wave formalism.

In my experience, new ideas, especially seemingly revolutionary new ideas, are generally only universally accepted if supported by overwhelming experimental evidence. If the experimental evidence is merely "exceptional" it is met with a mixed reception. Unless the experimental technology is quickly developed to produce overwhelming evidence, enthusiasm wanes. This loss of enthusiasm occurs even with the production of independent good evidence. Langmuir's sixth characteristic—"The ratio of supporters to critics rises up to somewhere near 50% and then falls gradually to oblivion"—may therefore simply be rephrased "The experiment is controversial"—hardly damning to any but the most conservative.

Langmuir's third characteristic is that "there are claims of great accuracy." There are many reasons why such claims or apparent claims might be made. The most obvious is that the claims are justified. Another is that the authors might be presenting their raw measurements without intending to imply that all the digits are significant. Still another reason is for rhetorical effect (as in, for example, Langmuir's unrealistic claims of great accuracy implicit in his verbatim quotes of conversations occurring decades previously). Unless it can be demonstrated that a claim of great accuracy is unjustified, no flaw has been demonstrated. Un-

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less it can be shown that the inflated accuracy is crucial to supporting the theoretical claims, the flaw is minor. In fact this flaw is commonplace and generally is simply ignored in uncontroversial work.

The fifth characteristic—"Criticisms are met by *ad hoc* excuses thought up on the spur of the moment"—is of no diagnostic use, since it is simply characteristic of any scientist defending a pet theory, conventional or unconventional. Langmuir gives instantly apparent, plausible explanations for experimental results seemingly in contradiction to established theory. Those he criticizes give "*ad hoc* excuses thought up on the spur of the moment." Both are, in fact, doing exactly the same thing: demonstrating that a plausible explanation for the apparent anomaly does exist, so that it does not constitute a falsification of the defended theory. Neither, in all likelihood, feels any great commitment to the proposed explanation's being the correct one. It is sufficient merely to demonstrate that *some* plausible explanation exists. It is then up to the critic (either of conventional or of new theory) to show that, in fact, no plausible and adequate explanation exists. This is the essence of orderly scientific dispute.

The first characteristic—"The magnitude of the effect is substantially independent of the intensity of the cause," and the minimum causal intensity is at the threshold of detectability—is the only one with real, independent diagnostic use. One has good reason to be suspicious of any experiment that shows this characteristic. But reason for suspicion is not equivalent to reason for rejection. We certainly would not wish to categorize Galileo's experiments with inclined planes, George Biddell Airy's experiments with a water-filled telescope, Albert A. Michelson and Edward W. Morley's interferometer experiments or Heike Kamerlingh Onnes's discovery of superconductivity as pathological.

I do rather strongly suspect that Langmuir's characteristics can be useful in alerting us to the need for deeper analysis of an experiment. While, as I attempted to show above, each of his characteristics is individually innocent, their appearance together in a single experiment should raise serious doubts. They must be applied strictly, carefully and as objectively as possible, however, and they should not be taken as a basis for rejecting an experiment. The need for rigor here is acute: I was unable to think of a single controversial experiment, however eventually resolved, to which its critics could not have applied the label "pathological science," taking no more liberties with these characteristics than Langmuir did in some of his examples. If we are not careful, his message simply becomes "Reject all controversial claims."

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I was delighted to see that Irving Langmuir's informative and enjoyable lecture on "pathological science" has finally been made available to the general scientific community. Teachers at universities tend to spend all of their time informing their students of the success stories of science. However, if students are to develop into creative and discriminating scientists, they would do well to have some passing familiarity also with some of the failures. It may help them sort out which current new ideas are going to last and which are going to disappear.

The last section of Langmuir's talk, dealing with "flying saucers" (now referred to as UFOs), needs some clarification.[1] The Air Force did not close down and declassify its entire research project with the ending of Project Sign in 1949. Sign was replaced by Project Grudge, which in 1952 was in turn replaced by Project Blue Book. All three projects were classified, but not at a high level. The Air Force finally closed Project Blue Book in 1969, on the recommendation of Edward Condon, who directed a study on behalf of the Air Force at the University of Colorado from 1966 to 1968. (It is unfortunate that the Condon report,[2] the basis of the recommendation, is marred and weakened by serious inconsistencies.[3])

The article in *The Saturday Evening Post* that Langmuir referred to was probably that of Sidney Shallett.[4] The general mentioned by Langmuir, with a name that sounds like "Schwartz," was probably General Carl Spaatz, Air Force Chief of Staff from 1947 to 1948.

A more general commment is the following: Very few young scientists have heard of the Davis and Barnes experiment, N rays, mitogenetic rays or the Allison effect. By contrast, all young scientists have heard of extrasensory perception (a part of "parapsychology") and UFOs. It is worth inquiring whether this difference is a reflection of a basic difference between the two groups of topics.

When we look at Langmuir's list of "symptoms of pathological science," we find that the parapsychology and UFO problems do not now present all

continued on page 108

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those symptoms, nor did they present them at the time Langmuir gave his talk. For instance, in neither case have there ever been "claims of great accuracy," and in neither case has the ratio of supporters to critics (among the scientific community) risen to anything approaching 50%.

I suggest that the parapsychology and UFO problems are, in fact, *not* examples of pathological science. I have suggested elsewhere[5] that they may better be understood as examples of "heretical science." I have in mind the interpretation that in addition to being based only on shaky and shifting evidence, a topic of pathological science arises from *within* a scientific discipline (presenting mainly an intellectual challenge, but also to some extent an internal political challenge), whereas, by contrast, a topic of heretical science arises or has strong support *outside* the scientific community (presenting the scientific community with both an intellectual challenge and an external political challenge).

It seems likely that problems of pathological science are resolved more rapidly than those of heretical science partly because they are more sharply defined and partly because it is possible (though not easy) to cover them in scientific meetings and publications. Problems of heretical science would no doubt be resolved more rapidly if they were discussed freely, fully and objectively in scientific journals. Most scientific journals are not open to such discussion; an exception is the *Journal of Scientific Exploration*, published jointly by the Society for Scientific Exploration and Pergamon Press.

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11/89

Irving Langmuir's lecture on pathological science, transcribed and edited by Robert N. Hall, contains numerous errors in its section on extrasensory perception that need to be corrected. Much of what Langmuir says about his meeting with J. B. Rhine, who founded the institution of which I am director, is misleading and even false.

First, as to the statements of fact, Langmuir says that he met Rhine "probably about 1934," that Rhine "published a new volume of his book" that contained "a chapter on the sealed-up cards in the envelopes," that the ESP scores obtained using these cards "all come up to around [an average of] 7" per 25 trials and that "nothing is said about the fact that for a long time they came down below 5."

It is fortunate that the records of Rhine's experiments and his extensive correspondence are kept in the archives at Duke University Library, so that we can check on the accuracy of some of the statements made by Langmuir.

▷ Langmuir did not recall the date of his meeting with Rhine correctly. The meeting was not in 1934, but rather in 1937, when Langmuir was in the Durham area in connection with a meeting of the American Chemical Society.

▷ Rhine published *New Frontiers of the Mind* in 1937. It contains no chapter on sealed-up cards. In fact, there is no such *chapter* in any of Rhine's books.

▷ It is false to state that Rhine ever wrote in any of his books that the mean run score obtained with sealed-up ESP cards was around 7.

▷ Contrary to what Langmuir says, Rhine and his associates reported in table 7 of their 1940 publication *Extra-sensory Perception After Sixty Years* six series of experiments in which subjects were tested with ESP cards enclosed in sealed opaque envelopes. One of these series, conducted by Rhine, gave an average of 4.89. This was the series Langmuir was referring to. Langmuir was therefore clearly wrong in implying that Rhine did not report his negative results. The average run score of the six series together is 5.21, which is significantly different from the mean chance expectation of 5.00 ($z = 7.74$).

The implication of Langmuir's statements on parapsychological research is unambiguous. The reports of successful ESP experiments, it is suggested, are due to selection of data. He implies that parapsychologists report only significant results and find an excuse to discard and reject results contrary to their expectations. Even the most vociferous critics of parapsychology today do not subscribe to this view because there are numerous published studies that simply cannot be explained away by such an assumption.

Langmuir refers to the declining scores in the ESP experiments of his nephew David Langmuir. If his account of these experiments is accurate, they seem to fit very well with the general pattern of results in this field. Such declines are quite common and are to be expected unless the experimenter is careful to introduce novelty and other incentives to keep up proper motivation of the subjects. In fact, even Rhine's star subjects eventually failed to keep up their levels of high scoring. David Langmuir wrote to Rhine on 14 April 1937 acknowledging the latter's sending him test materials, including cards, and promising to keep Rhine informed of the test results. If David Langmuir had not been scared away from continuing his contact with Rhine by such skeptics as Irving Langmuir, perhaps the outcome of those experiments would have been different.

It would seem that Irving Langmuir was a man with a mind of his own, a mind that was tricked by his own preconceptions. For a man of his scientific stature and intellectual capabilities, Langmuir did surprisingly little to ensure that his statements about ESP research were backed up by facts, and he was unwilling to face up to those facts that he did find. For example, in May 1938 he gave a lecture at Union College in which, among other things, he criticized Rhine's research. In the *Schenectady Gazette* dated 5 May 1938, Langmuir is quoted as saying, "Although I cannot as yet find anything wrong with the experiments of Professor Rhine, after having spent two days in studying the methods and experiments, I can't help feeling that it has all the indications of being wrong scientifically."

On learning about the Union College talk, Rhine wrote to Langmuir for clarification, but as far as we know he never heard from Langmuir. It is clear that Langmuir's critique of Rhine's work was based not on anything he found wrong with it, but on his perception that it fit his "symptoms of sick science," which themselves are quite arguable, to say the least. I do not believe any serious student of philosophy of science today would agree with his criteria for pathological science. Clearly, his diagnosis relative to ESP research was wrong.

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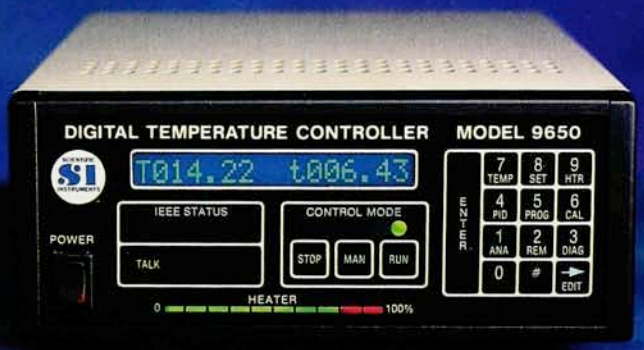

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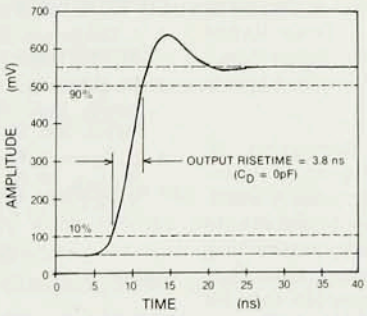

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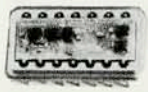

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small group at General Electric, has now reached a much larger audience through its publication in PHYSICS TODAY. I first came across a copy of this lecture in 1972 and was especially struck by the vigor with which Langmuir attacked J. B. Rhine, on the basis of an interview that had taken place almost 20 years before the lecture was given. Since Rhine was still alive and active I decided to ask his opinion of Langmuir's accusations, guessing (correctly) that neither Langmuir nor anyone else had thought to send him a copy. I received a prompt reply. Since this letter was definitely not intended for a wide audience I will limit myself to two short quotations from it: "[Langmuir] has not hesitated to make up things to suit his pattern," and "He quoted me as saying things I could easily convince a fair-minded, honest third person I would never have said." Langmuir evidently felt that he could dispose of Rhine (and, presumably, the whole field of parapsychology) on the basis of his interpretation of this one interview; Rhine, however, was prepared to defend himself if necessary, and confident that he could do so effectively.

In view of this profound disagreement between the two men I would urge readers not to base their own assessment of parapsychology on Langmuir's account but to look for more information. It is certainly not true, for example, that parapsychology has come to an end, as Langmuir's portrayal of it would have led us to expect. But the opposition between the minority of scientists who support parapsychological findings and the much more powerful group that actively opposes them is as strong as it was in Langmuir's day, and apparently no closer to being resolved. Just as Langmuir's lecture is incomplete without Rhine's reply, so the recent report by the National Research Council[1] should only be read in conjunction with the reply from the Parapsychological Association.[2]

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11/89

Robert Hall's transcription of Irving Langmuir's talk on pathological science was entertaining; however, there is now reason to doubt Langmuir's belief that selective reporting can sufficiently explain the statistical significance of (presumably) all types of parapsychological experiments.

For example, an article by Roger D. Nelson and myself addresses the problems of selective reporting and methodological quality in a large body of experiments studying the possibility of consciousness- or observer-related effects on physical systems.[1] Several other recent articles in the physics literature lend further support to the likelihood of such effects.[2]

One of Langmuir's symptoms of pathological science is that support for it "falls gradually to oblivion." Four years after Langmuir's talk, J. B. Rhine and his colleagues formed the Parapsychological Association. Twelve years later, the Parapsychological Association was elected an affiliate of the American Association for the Advancement of Science, and it remains an active scientific society today. Within the past two years, the National Research Council and the Office of Technology Assessment have both examined the parapsychological literature in some detail. That doesn't sound like declining interest to me.

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10/89

Irving Langmuir's description of cases of "pathological science" is interesting as history not usually available to anyone not close to the events described. It is especially interesting because at about the same time that Langmuir gave his recorded talk he was the central figure in an illusory interpretation of the results of a series of cloud-seeding experiments. As part of Project Cirrus, conducted jointly by the Signal Corps, the Office of Naval Research and General Electric from 1947 to 1953, silver iodide was introduced into clouds in New Mexico at seven-day intervals. Statistical analysis of rainfall and temperature records covering much of the US convinced Langmuir that periodic seeding was capable of producing marked and very widespread effects on atmospheric circulation and rainfall.[1] He also was convinced that seeding with dry ice had resulted in a drastic shift in the path followed by an Atlantic hurricane. Langmuir's claims were met with disbelief or skepticism by nearly all meteorologists, and a panel appointed by the American Meteorological Society found that Langmuir's claims had not been demonstrated "if the available evidence is interpreted by any acceptable scientific standards."[2] An extended and illuminating account of these events has been published by Horace Byers.[3]

The Weather Bureau, in response to the extravagant claims Langmuir made for Project Cirrus, launched the Cloud Physics Project in 1947. The consistently unspectacular results stimulated challenges by Cirrus adherents. I recall an AMS meeting in the early 1950s at which Langmuir got into a shouting match with Ross Gunn, a Bureau of Standards physicist who represented the Cloud Physics Project.

There is no evidence that Langmuir ever was convinced by the criticism of his interpretation of the Cirrus results. His final paper on the subject was published as a GE Research Laboratory Report in 1955. Langmuir had made fundamental contributions to the understanding of cloud physics as well as to innumerable topics in basic chemistry and physics. Yet in this case he was unable to see around his own idiosyncratic interpretation. There are lessons here for all of us.

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10/89

Near the close of Irving Langmuir's interesting account, reference is made to the planet Venus's being mistaken for "a comet about to collide with the Earth, or somebody from Mars."

Venus caused some concern at Los Alamos in late 1944 or early 1945. We had heard of the balloon bombs that the Japanese had launched, which were supposed to drift eastward across the Pacific and then land and do damage in the United States when they exploded. We learned later that one such bomb had landed in Oregon and that several people were killed when it exploded. The military people, who were responsible

for safety and security, were concerned that such a bomb might arrive at Los Alamos.

One bright sunny day as I walked back from lunch to the Technical Area I noticed many people looking up at a bright point in the sky. There, drifting overhead, was one of the dreaded bombs. Some people on the street were using binoculars and thought they could see the payload attached to the spherical balloon. Rumors abounded about triangulation measurements that gave numbers for the elevation and direction of motion of the balloon. Interest remained high through much of the afternoon. But at sunset the object had drifted well to the west, and the next morning the object rose in the eastern sky. We were all amused that a planet had caused so much concern.

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I took exception to the article "Pathological Science." While the article is informative and interesting, I could not help but feel from its timing and tone that it was a thinly veiled indictment of the researchers who have reported observing cold fusion. It is still premature to dismiss those findings. Perhaps when and if the cold fusion results are retracted, this article will provide an interesting perspective on the phenomenon. Until that time it should be noted that cold fusion is not unlike many other new phenomena that seem at first to be counterintuitive, controversial and irreproducible. One need only think back to the appearance of high-temperature superconductivity for one example. Perhaps PHYSICS TODAY might review the initial resistance to acceptance that other landmark discoveries have faced, rather than hopping on the bandwagon of those who find it rewarding to engage in a cold fusion witch-hunt.

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Shapley's Poor Seeing of Astronomy's Future

According to *The Diary of H. L. Mencken* (Charles A. Fecher, ed., Knopf, New York, 1989), Harlow Shapley's crystal ball was no better than Ernest Rutherford's. Add astronomers, female scientists, fans of Robert Millikan and those who do not think of Rutherford as an astronomer to those who will now be sore at Mencken. The following is a portion of the entry in his diary for 25 April 1931:

Alfred Knopf and I arrived in Boston night before last and, after a placid sleep at the Ritz–Carlton, went out to Cambridge yesterday morning to see various bigwigs. . . .

We had lunch with Harlow Shapley, the astronomer, at the Faculty Club. He turned out to be an inconspicuous and somewhat rustic looking man, apparently in the late forties. But the more he talked, the more his rusticity vanished. He said that the new 200-inch reflector, now being made, will be of very small value to astronomers save as an advertisement to their profession. He said that practically everything it may be expected to accomplish could be accomplished by the existing telescopes. The latter have already revealed millions of stars, and studying them will occupy astronomers for the better part of a century. Shapley said that the Harvard Observatory needed no more than two or three really competent astronomers. The rest of the work is done satisfactorily by persons with relatively meager equipment. Some of them are girls from the women's colleges. Shapley said that he was opposed to training astronomers in any number. He said that the number of places open for really competent men is small, and that it would be very easy to overcrowd the profession. He expressed strong disapproval of Robert A. Millikan, and especially of Millikan's efforts to reconcile science and religion. I gathered from his talk that he himself is a thorough-going skeptic. He told us of a devastating saying, at Millikan's expense, by Sir Ernest Rutherford, the English astronomer. Rutherford said that publicity grabbing has become one of the learned sciences and a great force in modern life, and that it has become necessary to set up a unit to measure it. This unit, he said, is the *kan*. It is, however, so large that it has become necessary to resort to a workable fraction of it. This fraction is the *millikan*.

When I was a student and junior faculty member at Johns Hopkins in the 1940s, I encountered Mencken a couple of times at the Hamilton Street Club when I was taken there by my adviser and friend Gerhard H. Dieke. I remember Mencken as acerbic, opinionated and egocentric, but outrageously amusing. He wasn't particularly well informed about science, but he certainly professed to be well informed about important people, eminent scientists among them. There does not seem to be any way he could have invented the contents of the conversation with Shapley, although, of course, his interpretations may have been somewhat hyperbolic.

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Cold Fusion Theorist's Unwithdrawn Papers

In the news story "Doubts Grow as Many Attempts at Cold Fusion Fail" (June 1989, page 17), Barbara G. Levi writes, "An MIT theorist has withdrawn a paper suggesting how this [helium-4 production in a palladium lattice] might occur." I assume Levi was referring to Peter L. Hagelstein of the department of electrical engineering and computer science, who in April submitted four related papers on a theory that attempts to explain evidence for what some believe to be cold fusion. He submitted his papers to *Physical Review Letters* and subsequently made a number of revisions to them—resubmitting the modified versions each time. In a conversation with Hagelstein on 2 June, he assured me—rumors notwithstanding—that at least as of that date he had not withdrawn his papers.

(*Note added in proof:* Hagelstein delivered a paper on a modified version of his theory, "Coherent Fusion Theory," on 12 December 1989 at the cold fusion session of the winter annual meeting of the American Society of Mechanical Engineers in San Francisco. This new version of his theory suggests that tritium and heat are the byproducts of possible cold fusion reactions.)

EUGENE F. MALLOVE
MIT News Office
6/89 *Cambridge, Massachusetts*

Corrections

December, page 9—Armand Wyler's formula for the fine-structure constant was

$$\alpha = \frac{9}{8\pi^4}\left(\frac{\pi^5}{2^4\,5!}\right)^{1/4}$$

December, page 17—In the hydrogen maser developed by Norman Ramsey and Daniel Kleppner, atoms were confined by means of a quartz storage bottle coated with Teflon, not a "magnetic bottle." ■