

LETTERS

Parapsychology—A Correction

I am writing to correct what I said about J. B. Rhine of Duke University in the question-and-answer period after I gave my paper "Not consciousness, but the distinction between the probe and the probed, as central to the elemental quantum act of observation" at the panel session "Physics and Consciousness" on 8 January 1979 at the annual meeting of the AAAS in Houston. The tapes of that session, distributed under the sponsorship of the AAAS, carried my prepared paper. They also carried the two appendices I prepared for my paper when I discovered to my dismay that the other three participants were speaking on the so-called field of "parapsychology." One of these appendices was called "Put the pseudos out of the workshop of science," and the other "Where there's smoke, there's smoke" (both reprinted in the 13 April issue of the *New York Review of Books*, along with my February letter to the board of directors of the AAAS suggesting that the AAAS disaffiliate the Parapsychological Association).

In response to one of the questions from the floor, I unwisely repeated a secondhand, and as it turned out, incorrect account of the experiments of Rhine and McDougall purporting to show that descendants of "educated" rats do better at mazes than the descendants of "uneducated" rats. Rather than repeat here my inaccuracies, let me give references to the literature (1) in which the interested reader may get the story correctly.

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References

1. R. Robinson, *Genetics of the Norway Rat* (Pergamon, New York, 1965), pp. 697-720; T. M. Sonneborn, *Am. Nat.* **65**, 541 (1931).

I am glad to see John Wheeler's letter of retraction of the charge he made against me in Houston on 8 January and am pleased to have this opportunity to reply. It is also good to know that his statement of retraction will be sent to all those who have already purchased tapes containing a record of Wheeler's charge against me, and further that the Wheeler charge will be deleted from tapes and records of the symposium being distributed by the AAAS in the future.

As may be seen, however, Wheeler's letter does not identify just *what it is* that he retracts; it could be *any* little thing; he vaguely calls it "inaccuracies." I have

therefore to insert here a brief abstract of this missing part of the story, condensed from the official taped record:

After Wheeler ended his critical remarks on the Parapsychological Association (PA), he was asked to be more specific. In reply he gave an account of an experiment from McDougall's rat research at Duke of 50 years ago, work in which I had a part. According to Wheeler a postdoctoral assistant in the experiment intentionally altered the conditions so as to produce spurious positive results. However, subsequent consultation with Dr. S, a distinguished geneticist (whose name was given to the audience), led to the disclosure to McDougall of these false-positive results and in consequence they were never published. Wheeler concluded his story at this point by saying "The only thing I haven't mentioned here is the name of the assistant who did the experiment. It was Rhine . . . Rhine—he started parapsychology that way."

No one was present who was prepared to respond to this unscheduled accusation; so it went unchallenged. It was some weeks before I got it from a transcription of the AAAS tape of the seminar. Dr. S., the witness cited, was the first to respond. He rejected the Wheeler charge against me as wholly untrue. I sent his letter to me (which is necessarily confidential) to William D. Carey, the Executive Officer of the AAAS and received on 19 March a prompt and cooperative response from him. In the meantime, Dr. S., however, wrote Wheeler directly to set him straight. Finally, Wheeler wrote me a note of apology (on 20 February) and, on 12 April, he wrote the letter of retraction to *Science* to which I am now replying.

An acceptance of Wheeler's retraction might be expected at this point to wind up this "Houston affair," but for one more item so far not discussed. This is a letter from Wheeler to Carey dated 12 January in which he pursued his plan announced at Houston to "run the 'pseudos' out of the workshop of science." This letter, only 4 days after the "blast-off" in Houston was, of course, based on the impressive case Wheeler was evidently still confident he had made at the seminar in identifying parapsychology as a "pseudo," and for which I had been chosen as an example. The dates show that Wheeler could hardly have known of his mistake at the time he made the appeal to Carey. The collapse of his plan right on the launching pad, as it were, may reasonably be assumed to have left the "Houston affair" to history.

But in science, mistakes are seldom completely fruitless. No sooner had the

PA been "read out of the status of eligibility" for affiliation with the AAAS than the new president of the association, Kenneth Boulding, was asked in an interview by the *Washington Star* (9 January) where he stood on the issue of the attack on the PA by Wheeler. These few courageous words of President Boulding as quoted by the *Star* will I think make the Houston meeting of the AAAS a memorable one long after the controversy over the PA affiliation is deservedly forgotten. This is the "Boulding Declaration," as I would like to call it. "The scientific community has to be kept open." "The evidence of parapsychology can't just be dismissed out of hand." "One has to subject their methodology to something." I am "in favor of keeping them in."

These words put new meaning into AAAS affiliation and give this great organization an added responsibility for the advancement of its more difficult, venturesome sciences, such as parapsychology.

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Drug Safety: Phenacetin

The assertions presented by Johanson and Angervall (Letters, 13 Apr., p. 130) to support their point of view that phenacetin (P) is a carcinogen need further examination. The steps between clinical indication of risk and identification of a carcinogen need to be based on sound scientific evidence.

The case reports in the literature associating abuse of P-containing analgesics with renal pelvic tumors can hardly be classified as sound epidemiologic data. At the time of the initial case report associating renal pelvic carcinomas with the abuse of P-containing analgesics (1), the results of a negative 2-year study in the Charles River CD rats (Sprague-Dawley) were available (2). Phenacetin was administered (20 to 200 milligrams per kilogram per day) in a meal-form (unpelleted) rodent diet. The results of an additional negative 31-month study in Berlin-Druckery rats (>100 mg/kg per day (3)) were also available.

The Burroughs Wellcome study, mentioned in Cuatrecasas' letter to *Science* of 5 January (p. 6), was initiated after the Bengtsson and Angervall letter to *Lancet* in 1970 (4). In this negative 18-month study, C57BL/6 mice were supplied up to 754 mg/kg per day of P in meal-form (unpelleted) rodent diet; the drug-diet mixes