Letters

Zombification

I would like to draw attention to William Booth's News & Comment article "Voodoo science" (15 Apr., p. 274), which discusses my research on the Haitian zombie phenomenon. According to Booth, C. Y. Kao of the State University of New York Downstate Medical Center in Brooklyn notes that "this is an issue of fraud in science." Kao's cavalier use of the word "fraud" must be addressed.

While pursuing the ethnobiological investigation of zombification in Haiti, I observed that the "bokor" (sorcerer, or negative priest) consistently included several marine fish as ingredients in a dry powder known as the *poud zombi*. At certain times of the year these fish may contain lethal levels of tetrodotoxin (TTX), a neurotoxin capable of inducing apparent death. In the fall of 1983, when I approached Kao with my findings, he enthusiastically cited a 1978 Japanese case in which a poisoned individual recovered 24 hours after suffering all the symptoms associated with brain death. He noted that such a case, although rare, was not unprecedented—an observation supported by a review of the biomedical literature.

In December 1984 I provided Kao with two samples of the reputed zombi powder for analysis by Takeshi Yasumoto of Tohoku University, Sendai, Japan. One, which was 2½ years old, yielded no evidence of TTX. The other sample, which had been collected 6 months previously, contained 1.1 micrograms of TTX per gram of powder, an amount too small to result in significant pharmacological activity. On the basis of the results of this assay, Kao suggested that my hypothesis be abandoned. My understanding of the emic reality of the Haitian zombie, as well as the complexity of folk preparations, led me to suggest that his conclusion was premature. I raised the following issues for consideration.

First, this powder is not made by a pharmaceutical laboratory. In all my publications I have mentioned the variability of toxin levels within natural populations of the fish, as well as the diversity of formulas concocted by the bokor. Researchers have found that even during the season of greatest toxicity only about 50% of tested specimens from a single site are toxic. Given this variability, the fact that one of the samples contained *any* TTX is significant.

Second, analysis of the powder requires putting it in solution. If those doing the analysis do not put it into a buffered solution, the analytical procedure itself might inadvertently destroy most of the powder's TTX. As Booth notes, Laurent Rivier of the Université de Lausanne reports having found between 5 and 20 micrograms of TTX per gram of powder from a portion of one of the same samples I sent Kao, a concentration that Kao has said is "getting into the ballpark of feasibility."

Third, Kao's many excellent publications about the in vivo effects of TTX involve microgram-milligram amounts of the drug, administered intravenously or interperitoneally. Given alone, very little, if any, TTX crosses the blood-brain barrier. However, if TTX is administered directly into cerebrospinal fluid, nanogram amounts have been found to have more dramatic effects than do microgram amounts administered intravenously or interperitoneally (*1*). It may be that some of the powder's other ingredients enable increased transport across the blood-brain barrier and that this enables a reduction of three orders of magnitude in the effective dose of TTX.

Fourth—and this consideration weighs heavily—it is only when the bokor succeeds that his machinations become apparent—only when he causes others to believe the victim is dead and then revived. One success in dozens of attempts would be sufficient to support the cultural belief in the zombie phenomenon.

No one has suggested that there is an assembly line producing zombies. Given the complexities inherent in the process, zombification is likely to be an extremely rare event. Given the latitude afforded the bokor, the pharmacological efficacy of any particular batch of the powder is not so critical that the proportions of ingredients in two samples can be used to pass judgement on the plausibility of the entire endeavor. The issue that must be addressed is the observation that the Haitian bokor seeks out and uses in his preparations several species of fish known to contain lethal levels of TTX.

Finally the most serious allegation specifically mentioned in the article "Voodoo science" is that I deliberately withheld data concerning the laboratory analyses of the zombie powder. This is incorrect. As soon as the results of Yasumoto's first assay became available, I informed my major professor, Richard Schultes of the Botanical Museum of Harvard University as well as Rivier, who was already involved in experimental work with samples of the powder. Both advised me to continue to pursue my hypothesis. In following this advice I made no effort to conceal Yasumoto's results. To date I have published two books on the subject. *The Serpent and the Rainbow* (*2*) is a popular account of my ethnographic experience in Haiti, and its narrative ends 6 months before Yasumoto's work began. My second book, *Passage of Darkness* (*3*), presents my dissertation research. Both the results of Yasumoto's analysis and the subsequent work of Michel Lazdunski, director of the Center for Biochemistry at the Université de Nice, and Rivier are noted and discussed in chapter 6.

In *Passage of Darkness*, as in my dissertation, the 1982 experiments by Leon Roizin of Columbia Presbyterian Hospital in New York City are given one paragraph (*3*, p. 7) in the introduction as part of a review of the early progress of the zombie investigation. The late Nathan Kline, then director of the Rockland State Research Institute in New York, provided Roizin with a fresh sample of the powder. Even though Roizin now asserts that the preliminary experiment was "just among friends" and not for circulation, Roizin himself discussed the results in a British Broadcasting Corporation documentary "The Living Dead" that was broadcast in 1984. His question, "How do I know that something was not added to that material?" is innuendo, and I question the propriety of Booth's writing it in the absence of supporting evidence.

I also object to the insinuation that, in contrast to the tests conducted on animals by Roizin, I attempted to conceal the preliminary experiments conducted by John Hartung and myself at the Downstate Medical Center in 1985. Hartung and I had no results worth reporting. Our experiments were invalid because we placed the powders into a nonbuffered solution without realizing at the time that the resulting *p*H would denature TTX. Having recognized our mistake, we elected to pursue the matter if and when a fresh batch of powder became available. Our experiments had been aborted. Booth, however, seems to imply that we had obtained results that we then deliberately ignored. In my view, that Hartung "defends the silence" concerning the failed attempt we made to replicate Roizin's findings does not suggest that we actively sought to conceal the results of those experiments. On the contrary, Hartung's supporting comment should indicate his understanding of the insignificance of those experiments, as well as his understanding of the limited significance of the initial assays completed by Yasumoto. Moreover, Hartung's statement that "absence of evidence is not evidence of absence" is not an abdication of intellectual and scientific responsibility, but a cautionary warning that in the evaluation of these preparations the possibility of a false-negative conclusion is extremely high.

It is on this point that Kao and I disagree. I do not question his experience and authority, but I believe that his argument in this case falls wide of the mark. He and I do appear to agree, however, on three important points: (i) TTX can cause a person to appear to be dead even though that person subsequently revives; (ii) TTX was an ingredient in one of the samples of the zombie powder that Yasumoto analyzed; and (iii) occasional Japanese victims of fugu (TTX) poisoning appear to be dead but are not. The causal hypothesis may be wrong or in need of substantial revision, but none of the objections raised by Kao changes its status relative to its alternative.

The most serious issue raised in this controversy is that there is a vast difference between an unresolved or even false hypothesis and a fraudulent one. For Kao to suggest, after a complete review of the research, that my theory linking TTX to zombification is wrong would fall within the ordinary domain of science; but for him to disseminate unwarranted allegations of fraud lies within another domain. In Booth's article, John Moore of the Duke University Medical Center in Durham, North Carolina, is quoted as noting that the burden of proof of any hypothesis lies with the scientist. It is precisely because of this that I have chosen to continue to pursue what seems to me to be the most viable explanation of an exceedingly complex cultural phenomenon. But it is equally true that, in the case of a public accusation of fraud, the burden of proof lies on the accuser. Should the accusations prove false, as in this case, the individual responsible for the inflamatory statements should be held accountable.

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2. W. Davis, *The Serpent and the Rainbow* (Simon & Schuster, New York, 1985).
3. ———, *Passage of Darkness* (Univ. of North Carolina Press, Chapel Hill, NC, 1988).

Response: Most points raised by Davis appeared in the article, including one item which I consider central to understanding the whole affair: that an anthropologist in the field and a chemist at the bench approach a highly sensational subject such as zombies in very different ways. This clash of research cultures has been exacerbated by the long-standing publicity surrounding Davis and by the relative absence of published data on the presence of tetrodotoxin in zombie powder.—WILLIAM BOOTH

Translation of Einstein Papers

John Walsh's article on "Editorial changes for Einstein papers" (News & Comment, 15 Apr., p. 278) comments that the translation of the documents of volume 1 "has been excoriated by reviewers on the grounds of both gracelessness and inaccuracy and for lacking notes." Similarly, the review of volume 1 by Peter Loewenberg (29 Jan., p. 510) complains that the translations "are often awkward in the rendering of Einstein's clear and pungent style, and are sometimes misleading."

As the consultant on the translations, I am dismayed that various reviewers did not take note of the preface of the translation volume. Its second paragraph stated (*1*):

> The purpose of the translation, in accordance with the agreement between Princeton University Press and the National Science Foundation, is to provide "a careful, accurate translation that is as close to the German as possible while still producing readable English," rather than "a 'literary' translation." This type of translation should allow readers who are not fluent in German to make a scholarly evaluation of the content of the documents as well as obtain an appreciation of their flavor, in particular that of the correspondence. If some of the passages sound awkward, it is usually because the original passages were awkward—both because many of the letters and notes were obviously written in haste, and because the writers (especially Mileva Marić, whose native language was not German) did not always express themselves in correct, not to say literary, German.

It then noted a number of "particular problems that arose in translating the correspondence." It is of course quite proper for reviewers and reporters to question the decision of the National Science Foundation and Princeton University Press to publish a raw translation without notes and other editorial material, which "should be read only in conjunction with the documentary edition," as noted in the preface. But the translator should not be faulted for carrying out the mandate of NSF and the Press precisely as it was intended. While we do not claim perfection, we have yet to be furnished with a specific example of inaccuracy, apart from Loewenberg's statement that in the report of a detective on the financial assets of the Einstein family we should have used "fortune" rather than "real property" ("eigentliches Vermögen"). Although "fortune" may not be wrong, we consider our translation of the detective's bureaucratese to be more accurate. *Science* is not the place to engage in a debate on whether Loewenberg's other translations are any better or more faithful to the original than ours; we do not thnk so. But his advice for "readers who wish to appreciate Einstein" to provide their own translations is beside the point; if they could, they should not consult the translation volume at all.

Furthermore, one should not make the mistake of expecting the "clear and pungent" style of the *mature* Einstein to be always present in all the communications of the young one (volume 1 ends in 1902, when he was 23). We have not attempted to provide versions we wished Einstein had left us with if English had been his native language and if he had always written with posterity in mind. To do this would have been a disservice to scholars who want to study Einstein's life and the development of his ideas, but are not sufficiently fluent in German (scientific, colloquial, as well as some of its dialects, as used almost a century ago) to be able to rely on the original documents alone. The problems of a literary translation are quite different as, for example, shown by a current debate (*2*) on and the retranslation (*3*) of *L'étranger* by Albert Camus. Readers of translated novels or poems have a right to demand a re-created work of art. Scientists who intend to make a scholarly evaluation of documents they cannot read in the original require a translation as close to the original source as possible.

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2. H. Mitgang, *New York Times*, 18 April 1988, p. C21.
3. A. Camus, *The Stranger* (Knopf, New York, 1988).

Gaia Modified

Concerning Gaia and Richard A. Kerr's discussion of 22 April (Research News, p. 393), the tendency of Earth's spheres of activity to maintain or systematically renew a harmonic balance within themselves and among one another is well known among observers of nature. We observe the centennial of this observation as the Le Châtelier principle this year.

Although Earth may remind one, in poetic moments, of a living system, it does not metabolize, replicate, mutate, or reproduce mutations as living systems do. Gaia in its current mystical sense invokes poetic license. With the modifications described by Kerr it becomes a junior synonym of the Le Châtelier principle; or, biologically speaking, homeostasis.

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