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LETTERS

K-T Boundary Issues

In his 11 March Research News article (p. 1371), Richard A. Kerr reports on a recent meeting on "Impacts and Catastrophes" in Houston, Texas. He specifically reports on a "blind test" of planktic foraminiferal extinctions at El Kef in Tunisia and separately on a field trip to the Cretaceous-Tertiary (K-T) boundary sections in northeastern Mexico. Both reports contain errors or statements with which I disagree.

1) My initial report on El Kef (*1*) was published in 1988, not 1989.

2) My 1988 paper reported the disappearance of 12 species, or 22%, of the species below the K-T boundary and not 29%.

3) Jan Smit did not publish any species census data on El Kef; thus, he has produced no evidence either for or against species extinctions before the K-T boundary.

4) Robert Ginsburg did not collect the new samples for the blind test at El Kef. In fact, he has never been to El Kef.

5) Smit did not "minimize the influence of rare or misidentified species" by combining all four blind test results. In fact, he extracted a total of only 7 out of 62 possible species that disagreed with my 1988 paper in support of his statement that there is no evidence for pre-impact extinctions.

6) The purpose of the "blind test" was to test Smit's 1982 extinction model of all but one species extinct (*2*) versus my 1988 model with about one-half of the species extinct at the K-T boundary, one-third surviving, and the remainder extinct below the boundary.

7) Kerr does not report that all the blind test investigators reported between 36% and 46% of Cretaceous taxa ranging into the Tertiary, which shows Smit's extinction model to be wrong.

8) It was I who (*3*) could not confirm Brian Huber's 1991 study (*4*) rather than the reverse, as implied by Kerr. Huber's comments are therefore not likely to have been objective.

9) Kerr incorrectly states that while paleontologists discussed the findings of the blind test, sedimentologists worried about deposition. The blind test was discussed only on the last day of the meeting, and many more paleontologists than sedimentologists participated in the field trip and the discussion about deposition.

10) The preconference field trip was not organized by Robert Ginsburg, but by Wolfgang Stinnesbeck from the Universidad Autonoma de Nuevo Leon and myself.

11) The impact tsunami scenario did not win the day, as Kerr states. Sedimentologists generally disagreed with Smit's model of tsunami wave deposition, and all appeared to agree that the outcrops need further study to determine the nature of deposition.

12) Sedimentologists on the field trip found no evidence of up-and-downhill currents.

13) Kerr quotes Robert Dott as speaking for sedimentologists when he concluded that the outcrops show an impact-induced tsunami deposit. In fact, this statement was countered by sedimentologist Donald Lowe from Stanford University who, speaking for the majority of the field trip participants, concluded that the deposit was complex and represented multi-event deposition whose origin could not be determined without further field studies.

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References

1. G. Keller, *Mar. Micropaleontol.* **13**, 239 (1988).
2. J. Smit, *Geol. Soc. Am. Spec. Pap. 190* (1982), p. 329.
3. G. Keller, *Mar. Micropaleontol.* **21**, 1 (1993).
4. B. Huber, *Proc. Ocean. Drill. Proj. Results* **113**, 255 (1991).

Kerr liberally spices his article of 11 March with quotes from paleontologists known to favor the impact-extinction model (Ward, Jablonski, Pospichal), while ignoring many statements to the contrary provided to him both during and after the meeting by equally qualified opponents of the same (myself, Askin, Fisher). Indeed, an inspection of the published abstracts from the Houston meeting reveals that, of the paleontological papers presented, either as posters or orally from the podium, most supported Keller's model of a progressive extinction event that exhibited marked local variation in intensity. In this sense, the fact that no significant rise in extinction frequency is evident in high-resolution studies from several classic K-T boundary sections (for example, Brazos River, Texas; Agost and Caravaca, Spain) is at least as important as the fact that upward of half the planktic foraminiferal fauna, a very minor component of

modern marine biomass, disappear locally at the El Kef section. Perhaps even more so. The El Kef blind test was not designed to resolve various interpretations of the trans–K-T biotic record, but rather to determine the observational pattern of planktic foraminiferal extinctions at this single locality. Smit says he *observed* all Cretaceous species, save one survivor, extending to the boundary and then disappearing together, while Keller says she *observed* disappearances occurring before, at, and after the boundary horizon. The results of both investigators show substantial numbers of extinctions occurring at the boundary in this section. All four blind-test investigators confirmed Keller's general pattern. None confirmed Smit's, whose post hoc attempt to reconcile Keller's pattern with his own "model" of K-T extinctions, by means of unsubstantiated appeals to the Signor-Lipps effect, should be seen for what it is.

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Response: As I reported, each of the four blind testers, to one degree or another, found the same pattern of foram extinction as Keller did. But that does not rule out a sampling problem. In fact, the Signor-Lipps effect predicts that abrupt extinctions will look gradual if some rarer species are missed by a search of the fossil record; but the more intensive the search, the more abrupt the extinction event will appear. By combining the efforts of all four blind testers, Smit intensified the search until all of Keller's gradually disappearing species were found to persist up to but not beyond the impact.

The apparent gradualness of the K-T foram extinctions at El Kef thus shows every sign of being an artifact. On the other hand, the blind test cannot address the question of which forams survived the impact. Many presentations at the meeting addressed this controversial area using a variety of approaches, without any clear resolution.

Keller's recollection of Donald Lowe's remarks differs from my notes taken during that session. Lowe concluded that the Mexican K-T deposits were laid down by "high-energy, pulsating, and probably short-lived events" consistent with the succession of waves from an impact; he added that sedimentologists do not yet fully understand deposits from such huge waves. He specified that he was summarizing the impressions of the half-dozen sedimentologists invited on the trip, not those of all the participants. Of the five sedimentologists on the trip other than Lowe whom I interviewed for the story, four of them agreed that the deposit is consistent with waves from an impact and that no proposed alternative, including that of Wolfgang Stinnesbeck and Keller, can reasonably explain the deposit. Only one reserved judgment until further study.

—Richard A. Kerr

■

Psychopharmacologic Drugs: Mechanisms of Action

Samuel H. Barondes (Perspective, 25 Feb., p. 1102) discusses the changes in psychiatric practice that have occurred with the widespread use of Prozac and comments on its possible mechanisms of action. The mechanisms of action are considered in terms of synaptic information transmission. Other than the conceptual limitations imposed by the present synaptic-dominated model of brain function, however, there is no reason to consider that Prozac or any of the drugs used in psychopharmacology operates exclusively by means of synaptic mechanisms. Accumulating evidence, in