REFERENCES

OLSON, E. C. 1990. The Other Side of the Medal: a Paleontologist Reflects on the Art and Serendipity of Science. The McDonald and Woodward Publishing Company, Blacksburg, Virginia, 182 p.

RAINGER, R. 1993. Biology, geology, or neither, or both: vertebrate paleontology at The University of Chicago, 1892–1950. Perspective on Science, 1(3):478–519.

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REPLY

COMMENT ON: LACK OF A HIGH BODY COUNT AT THE K–T BOUNDARY

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THIS IS in response to the Comment by Hunter on page 1158 of this issue. Hunter has obviously given considerable thought to the matter and presents several interesting possibilities I had not considered, including a short-term, climate-induced drop in reproductive rates. I agree that such a scenario would result in decreased abundance, but if it played out over a paleontological scale, it would presumably result in a gradual dribbling out above the impact layer.

The accumulation of bone in fluvial channels by lateral migration across the floodplain results in time-averaged deposits which summarize sparsely distributed attritional remains accumulated in the floodplain through time. Such deposits are obvious information destroyers in the sense that they destroy upsection trends, and Hunter is no doubt right in suggesting that such deposits could mask a mass kill. On the other hand, time-averaged deposits are information providers when it comes to census taking. Had there been no change in diversity and a mass kill at the boundary, the stratigraphically higher streams cutting down into the Cretaceous should encounter the same fauna lower ones do.

By and large, I believe that turnover rates are likely to have been rather low in dinosaurs, at least the large ones, and that the taphonomic filter was rather broadly open in both the Judith River and Hell Creek Formations. Since neither is in fact known, I will have to concede another of Hunter's main points. If turnover rates were high, if we had a very tight filter—passing very few remains—or a combination of both, the increase in mortality rates caused by a catastrophic mass extinction might not be enough to produce a readily seen increase at the outcrop.

The difficulty in testing Alvarez's argument is not in framing an adequate test, but in framing a reasonable test that it passes. If an increased body count is not the test of a catastrophic mass extinction, what would be? At the very least one would expect the normal distribution to carry through to the bitter end. It is important to note that while the 2–3-m barren zone is virtually ubiquitous, the normal distribution does not everywhere carry to it (as suggested by the two quotes from Archibald, cited on pages 187 and 189 of my original article).

Hunter is right in suggesting yet another reason why the predicted increase in remains might not occur, but I think this leaves us where we were before. If Alvarez's argument is not falsifiable, upon what other basis are we to judge it, other than the burden of proof?

ANNOUNCEMENT

Dear Colleague:

You are invited to attend the North American Paleontological Convention-VI at the Smithsonian Institution in Washington, D.C., on 9–12 June 1996. The organizing committee welcomes constructive comments on previous NAPC's as well as suggestions for symposia topics and/or format for 1996.

Sincerely yours,
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