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LETTERS

Magnetic Storm Predictions

Tim Appenzeller's article "Hope for magnetic storm warnings" (Research News, 21 Feb., p. 922) was an excellent overview of coronal mass ejections (CMEs) and their possible effects at Earth, one of the best that has been published on the topic. There is, however, a clarification that should be made.

Although it is well known that intense magnetic storms are strongly associated with CMEs (up to an 80% level of confidence), the opposite association, the one that is actually needed for storm predictability, is weak. Only one out of every six CMEs causes intense storms. We do understand the reason for this poor association. To have an intense storm, a strong southwardly directed interplanetary magnetic field with a long duration (hours) is required to impinge upon the Earth's magnetosphere. The CMEs that do not cause intense storms are composed either of northwardly directed fields or of intense southward fields that are of short duration (fluctuating fields).

At this time, we do not know how to predict the intensity of the field, its direction, or the duration in a particular direction. However, several colleagues are working on this problem, including T. Hoeksema and X. Zhao at Stanford University, N. Crooker at the University of California, Los Angeles, E. Cliver at the Phillips Laboratory in Massachusetts, and D. McComas at the Los Alamos National Laboratory.

We also do not know how to predict a storm's intensity given its interplanetary features, as physical processes within the magnetosphere are not fully understood. As one example, fundamental time scales for the growth and saturation of the Van Allen radiation belt (ring current) are governed by both the amplitude and the duration of the southward interplanetary magnetic field.

It is hoped that a great deal more will be understood within the next few years, so that predictions for magnetic storms can be made and power grids and satellites can be protected.

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Dinosaur Diversity and Extinction

In their report "Sudden extinction of the dinosaurs: Latest Cretaceous, upper Great Plains, U.S.A." (8 Nov., p. 835) Sheehan *et al.* assert that the lack of decrease in *familial* diversity of dinosaurs supports the hypothesis that the effects of an asteroid impact were the sole or dominant cause of dinosaurian extinction. This does not account for the considerable body of evidence of decrease in *generic* diversity and change in structure of dinosaurian faunas (*1*, *2*) before their demise.

Familial and generic diversity have been extensively analyzed (*1*, *2*) in Campanian and Maastrichtian faunas of the northern western interior (NWI) of North America. This region includes the area studied by Sheehan *et al.* These studies (*1*, *2*) have different results because interpretations of familial and generic classifications differ, but general patterns have emerged.

From the Campanian through the Maastrichtian (a period of at least 10 million years), the maximum number of families represented in regional samples from the NWI has remained relatively stable at between 11 to 14 (*2*, *3*). In contrast, the number of genera decreased from approximately 32 to 19. This decrease appears to have been particularly marked among the ornithischian dinosaurs (*2*, *3*).

A major goal for future research is to clarify the evolutionary pattern. Was it one of gradual dwindling, step-wise modifications, or abrupt change long before the end of the Cretaceous? Any viable hypothesis of the causal factors of dinosaurian extinction must account for the evidence of decrease in generic diversity and change in structure of dinosaurian faunas.

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3. D. B. Weishampel, in *The Dinosauria*, D. B. Weishampel, P. Dodson, H. Osmolska, Eds. (Univ. of California Press, Berkeley, CA, 1990), pp. 63–139.

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After 100 years of exploitation, the uppermost Cretaceous Hell Creek Formation of Montana and North Dakota continues to produce spectacular specimens of dinosaurs as they are exposed by erosion. New specimens can be rapidly exposed, but in most areas they are not. Additional pieces of the type specimen of *Tyrannosaurus rex* were recovered in the 1970s, 70 years after the type was first collected. Remains of plaster jackets on the ground from the excavations of dinosaurs some 30 years ago are easily identifiable. Nevertheless, Sheehan *et al.* state that they have obtained statistically meaningful paleoecological dinosaur samples by surface collecting *alone* in areas from which samples *may* not have been collected for 10 years. Even if less heavily exploited surface collections were available, the paleoecological significance of such sampling is highly suspect.

Sheehan *et al.* argue that they are tracking the ecological diversity of eight families of dinosaurs vertically through the Hell Creek Formation—if there is no change in relative abundance of individuals assignable to a family then there is no diversity change; if there is change in relative abundance then there is a diversity change. Even if one accepts their conclusion of no diversity change, their sampling is blind as to whether extinctions occurred in the Hell Creek Formation below the familial level. They tabulate but do not name or include 14 dinosaur genera in their analysis. Instead, they use familial level data because their "generic level data could be misleading." They do not mention that six of their 14 genera could have become extinct without *any* drop in familial diversity, a 43% generic extinction without the loss of a single family. Environmentalists would cause an uproar if such statistical manipulations were used to argue that there have been no human-caused extinctions.

Sheehan *et al.* suggest their total study interval is some 2 to 3 million years, or up to 1 million years long for each of their three sampling levels. Despite their statements to the contrary, I would argue that they cannot detect diversity changes with resolution any finer than about 1 million years. This is longer than all four major ice ages in North America combined. Much could have happened in 1 million years that went undetected by this study, especially because a coarse familial analysis was used.

Sheehan *et al.* question studies that suggest a decrease in dinosaur diversity near the Cretaceous/Tertiary (K/T) boundary. I agree with their concerns, as do other authors (*1*). Sheehan *et al.*, however, have not demonstrated the alternative—that there was no decrease in diversity. A decline of more than 40% in the number of dinosaur species is well documented for the last 10 million years of the Cretaceous in North America (*2*), and a recent abstract of preliminary findings suggests a further decline during the Maastrichtian (*3*). Survival of vertebrate species across the K/T boundary could approach 65%, with dinosaurs representing at most 18% of those that did not survive (*2*). Despite the findings of Sheehan *et al.*, it cannot be determined whether the extinctions of the remaining 35% of the vertebrate species (including dinosaurs) were slow or fast, only that they were not so massive as once thought.

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Response: Our finding was not, as Clemens states, that *familial* diversity did not decrease; this was known before we began our study. Rather than study the ranges of families, we assigned each individual dinosaur to a family and searched for changes in the relative abundances of families (ecological diversity) within facies through the Hell Creek Formation. We demonstrated statistically that ecological diversity was not progressively declining through the formation.

We do not agree with Clemens that there is a "considerable body of evidence of decrease in *generic* diversity . . ." of dinosaurs toward the end of the Cretaceous. The studies cited by Clemens provide ample support for our position. When comparing generic diversity, Dodson and Tatarinov (*1*, p. 60) state, "dinosaur diversity appears to be as high in the Hell Creek, at least in its early part, as in previous stratigraphic intervals. . . ." More dinosaur genera are known from the 8-million-year-long Maastrichtian than from the 10-million-year-long Campanian [(*2*), figure 1]. Dodson concludes (*2*, p. 7612), "there is nothing to suggest that dinosaurs in the Campanian or the Maastrichtian were a group that had passed its prime and were in a state of decline."

To our knowledge, our study was the first field investigation specifically designed to examine the extinction of the dinosaurs. We controlled many variables that have plagued other analyses by examining trends only within individual facies (rather than in the entire formation), by using one set of criteria for identification, and by recording only in situ fossils.

We agree with Clemens that earlier time intervals and other regions need to be examined. Merely reexamining records of upper Cretaceous dinosaurs from databases that are not comparable is of limited value.

In response to Archibald, we note that paleontological studies of vertebrates larger than microvertebrates require surface sampling. We worked in areas where there was little previous collecting. No old plaster casts were found in our area, reaffirming that earlier collection was minimal. Moreover, since additional toe bones of *Tyrannosaurus rex* were found after 70 years, we might have found fossils from previous, unknown excavations. In our census, a single toe bone was as significant as an entire skeleton because either would have been recorded as one individual. Finding similar patterns in North Dakota and Montana gives us further confidence that our data are unaffected by previous collecting.

Archibald raises the specter of a generic extinction of up to 43% going undetected. This would require that genera became extinct while the relative abundance of families remained constant. The numbers of individuals in the extinct genera would have had to have been made up for by a corresponding increase in the numbers of individuals of a surviving genus in the same family. Such compensatory population fluxes would have had to occur independently in six families to produce the data we found, a prospect we consider extremely unlikely.

Division of the Hell Creek Formation into thirds (each about 730,000 years) is the most refined stratigraphic subdivision in which we have confidence. As we noted, a gradual decline during the upper third of the Hell Creek would have been detected in our census as a reduced average of the upper third. Any "gradual" decline of ecological diversity must be confined to the uppermost part of the upper third of the Hell Creek. If such a decline occurred, and there is no evidence that it did, it would have happened during at most a quarter of a million years. If a decline was confined to 250,000 years, rather than the final 10 million years, gradualist extinction scenarios would have to be recast.

Our study focused on ecologic diversity as measured by the Shannon index and rarefaction. Archibald mentions the decline of more than 40% of the dinosaur species in the last 10 million years of the Cretaceous. This is not ecologic diversity, but rather taxonomic diversity—the presence or absence of taxa in a stratigraphic context. As Archibald has pointed out (*3*), examining stratigraphic ranges of dinosaurs is problematic because confidence in ranges of rare taxa is much less than for ranges of abundant taxa.

Ecologic diversity measures how the total number of individuals are distributed among each of the taxa and is more sensitive than taxonomic diversity. Analysis of ecologic diversity detects a decline in the abundance of a taxon, not just the elimination of that taxon.

Archibald suggests that the extinction of vertebrates at the end of the Cretaceous was not massive. The communities of land-dwelling animals, however, changed markedly at that time. Lower Tertiary communities lack the large herbivorous and carnivorous dinosaurs that are so characteristic of the Hell Creek (3). The ecologic change was massive, and we have shown that dinosaurs in Hell Creek assemblages were not gradually declining as a prelude to this changeover. The extinction seems to have been sudden, within the resolution inherent in the study of Hell Creek dinosaurs.

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New Mexico: Technology-Rich

Vincent Kiernan's 10 January article (News & Comment, p. 151) about the proposed consolidation of now separated elements of the Air Force Phillips Laboratory paints a rather negative picture of the move and of New Mexico. Permit me to offer some balancing views.

The proposed move is intended to collocate only those elements of several Phillips Laboratory directorates which have operations that are geographically separated but functionally interacting. It is expected to improve the functioning of all the laboratory's directorates and subsequently lead to operating efficiencies and cost savings, even for those remaining at Edwards Air Force Base and Hanscomb Air Force Base.

The article quotes one person's view that New Mexico is akin to an "intellectual desert." That would be quite a surprise to the many teams of scientists and engineers working at Los Alamos National Laboratory, Sandia National Laboratories, the White Sands Missile Range, and the other institutions that together are often referred to as "the Rio Grande Research Corridor." New Mexico is also the home of several significant geophysics-related entities, many operated for the National Science Foundation (NSF).

In the most recent NSF report (*1*) on patterns of research and development (R&D) performance, New Mexico ranks first in the ratio of R&D performance to gross state product, which, to me, means that technology is relatively more important to New Mexico than to any other state.

Without minimizing the personal disruptions that organizational relocations sometimes cause, let me suggest that the scientists who will move to New Mexico are sure to find a stimulating and vital technical environment, as well as a host of challenging jobs.

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1. *Geographic Patterns: R&D in the United States* (NSF Special Rep. 89-317, National Science Foundation, Washington, DC, 1989), table B-3, p. 31.

Corrections and Clarifications

In the report "Transcription factor loading on the MMTV promoter: A biomodal mechanism for promoter activation" by T. K. Archer *et al.* (20 Mar., p. 1573), the sentence in the legend to figure 2 (p. 1574) which indicated treatment or lack of treatment with hormone should have read, "Samples in lanes 1, 2, 5, and 7 represent DNA from untreated cells, and samples in lanes 3, 4, 6, and 8 represent DNA from cells treated with hormone."