

ing, whereas fragments of long-bone midshafts are often recovered, thus making the microscopic technique the only reliable aging technique. Since the amount of material needed for analysis by the microscopic technique is small (0.4-cm section), it may be applied to highly fragmentary and poorly preserved fossil finds.

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Terminal Cretaceous Extinctions and the Arctic Spillover Model

Gartner and McGuirk (1) have outlined an Arctic spillover model to account for the observed abrupt marine and terrestrial extinctions at the Cretaceous-Paleocene boundary. Although they devote nearly all of their attention to discussion of climatic effects that might ensue subsequent to the hypothetical spillover event, we would like to redirect attention toward the Arctic Ocean, even though Watts *et al.* (2) have concluded that the sedimentary section that forms the basis for the model is slumped allochthonous sediment.

In chronological order, Gartner and McGuirk's model requires first, a Late Cretaceous cessation of exchange of marine water between the Arctic Ocean and the world ocean. Although available data (3) support a closed connection between the North Atlantic and the Arctic Ocean throughout the Cretaceous, paleocontinental maps (4) and paleoceanic reconstructions (5) depict a wider connection than exists at present between the North Pacific and Arctic oceans during Late Cretaceous time. Patton and Tailleux (6) presented evidence that the compressional tectonics between North America and Eurasia occurred sometime between the middle Late Cretaceous and middle Tertiary but were most probably related to opening of the North Atlantic. Pitman and Talwani (7) stated, on the basis of tectonic studies, that the size of the gap between Alaska and Siberia was significant prior to 60 to 63 million years ago.

Second, the model requires that normal marine organisms within the pre-isolated Arctic Ocean be ecologically replaced by species tolerant of fresh or brackish conditions during the isolation period. Although no deep-sea core from

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- 15 December 1980

the Arctic Ocean has yet recovered a continuous sequence across the boundary, two cores that bracket the Cretaceous-Paleocene boundary have been recovered from the central Arctic Ocean. Core 437 contains a flora that correlates with the Late Cretaceous *Lyracula furcula* silicoflagellate zone, and core 422 bears the Early Paleocene (Danian) *Corbisema hastata* silicoflagellate zone which has been correlated with the *Cruciplacolithus tenuis* coccolith zone. All silicoflagellates are marine. In an earlier exchange (8), Gartner and Keany erroneously cited the reported presence (9) of the Late Cretaceous silicoflagellate species *Vallacerta siderea* at DSDP Site 275 to represent abnormal salinity. Their error was admitted by personal communication on 1 May 1979. In support of their model, Gartner and McGuirk cite Tourtelot and Rye's (10) mollusk isotope data which refer to Campanian and early Maestrichtian mollusks whose most northerly sites are at latitudes 70°; they also conclude that if the isotopic data reflect dilution, the salinity difference was not substantial.

Third, the model requires a trigger mechanism of rifting between Greenland and Norway that is simultaneous with the extinction event. The extinction event has been placed at the base of or just below anomaly 29 (11). Talwani and Eldholm (3), in a study of the evolution of the Norwegian-Greenland Sea, place the initiation of rifting between anomaly 24 and anomaly 25 time: anomaly 24 is present whereas anomaly 25 is missing. Hence, opening of the Norwegian-Greenland Sea postdated the extinction event by millions of years. If causes and effects are not nearly simultaneous, logic

requires that the cause precede the effect.

Finally, the proposed model based on the present volume of the Arctic Ocean utilizes a volume calculation of total water available for the spillover event. The Eurasian Basin, which approximates one-half the present Arctic Ocean (7), originated well after the extinction event. Vogt *et al.* (13) place the time of initial rifting of the Lomonosov Ridge from Eurasia at anomaly 24 time, nearly 10 million years after the extinction event. Further, the Canada Basin, during Late Cretaceous time, was only 1500 m deep (14) as compared to its present depth of 3800 m. Consequently, since volume is a function of both area and depth, the model's estimate of water available to blanket the world ocean with a layer of low salinity water is inaccurate.

To summarize, the hypothetical solution to the problem of the massive terminal Mesozoic extinction event is not useful because it does not account for the boundary limits imposed by available data.

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Clark and Kitchell (1) do not take into account that the conclusion of Watts *et al.* (2) is based on cores not from Ekofisk but from the nearby Abuskel structure and that Perch-Nielsen *et al.* (3) were unable to demonstrate the presence of

Danian coccoliths in the alleged 'mainly redeposited Maastrichtian sediments' at Ekofisk. They also do not recognize that nuances of the North Sea Central Graben stratigraphy are not germane to the Arctic Ocean spillover model except to the extent that they triggered the thinking process that ultimately led to the formulation of that model.

With regard to the passage between the Arctic Ocean and the Pacific, Clark and Kitchell impute temporal and spatial precision to reconstructions that are not warranted. If the size of the gap between Alaska and Siberia was significant prior to 60 to 63 million years ago (4), then presumably, the gap became insignificant at that time. Tarling and Mitchell (5) assign an age of 64 million years to the Cretaceous-Tertiary boundary; McDougall (6) has determined an age of 62.9 to 64.9 million years for basalts overlain by more than 100 m of late Maastrichtian sediment on Ninetyeast Ridge. If we allow for a possible error of a few percent in the above dates and estimates, a sufficient time overlap exists between the various events to accommodate the needs of the Arctic spillover model.

The silicoflagellates from core 437 have been restudied by Bukry (7) who concludes that the age of the assemblage cannot be fixed more closely than Campanian or Maastrichtian, that "... the assemblages of core 437 result from abnormal marine conditions or/and different age than the other known assemblages", and that "... this and other marine silicoflagellate assemblages in the Arctic area does not preclude a brief terminal Cretaceous freshening event." These are substantially the same conclusions advanced earlier (8). Another conclusion that is still valid is that data from core 422 are irrelevant, other than to underscore the prediction of the Arctic spillover model that the Arctic Ocean was indeed a normal marine body in early Paleocene time; this, incidentally, would be unlikely if Clark and Kitchell's reasoning is followed to a logical conclusion (that is, the Bering Strait closed 60 to 63 million years ago and the passage to the North Atlantic did not open until 58 million years ago). Turtelot and Rye's (9) data are of consequence in the Arctic spillover model primarily because the data suggest the capacity of the latest Cretaceous precipitation and runoff pattern to achieve a flushing of the Arctic Ocean.

With regard to the timing of the opening between the Arctic Ocean and the North Atlantic, Clark and Kitchell do not distinguish between rifting and crustal accretion at spreading ridges. It is the

latter that is recorded by magnetic anomalies; the former must precede the latter. In the Greenland-Norwegian Sea, as in the Eurasian Basin of the Arctic Ocean, there exist strips of "deep water" crust landward of magnetic anomaly 24; these strips are of sufficient width to accommodate anomalies 25 through 28, although in both cases these anomalies cannot (as yet) be recognized (10, 11). If these strips of "deep water" crust do represent initial stages of spreading, then crustal accretion began only a scant million years or so after the terminal Cretaceous catastrophe. Rifting, of course, must have come even earlier. To attempt placing tighter time constraints on rifting between Greenland and Norway is, at this time, unrealistic.

As to the volume of the latest Cretaceous Arctic Ocean, most reconstructions require the Amerasian Basin to be underlain by very old crust, crust that probably was already in thermal or isostatic equilibrium in latest Cretaceous time (11, 12). The very large amount of post-Cretaceous sediment fill, therefore, probably has reduced the volume of the basin. Similarly, nearly all reconstructions require some post-Cretaceous compression of the Amerasian Basin. A best estimate for the volume of the latest Cretaceous Arctic Ocean is that it may have been less than the volume of the present-day Arctic Ocean but probably was substantially greater than the present

volume of the Amerasian Basin alone. Three-fourths of the present volume does not seem excessive; but even half the volume of the present Arctic Ocean, given a favorable mixing model with normal seawater, would be more than adequate to achieve the kill of the stenohaline surface plankton.

Although none of the arguments advanced by Clark and Kitchell (1) points to a fatal flaw in the Arctic spillover model, it remains a model, nevertheless, yet to be tested.

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3 March 1981

Food Colors and Behavior

It would be unfortunate if the data presented by Weiss *et al.* (1) encouraged professional nonspecialists and parents to believe that there is a strong association between food colorings in the diet and what Weiss terms "problem behaviors." The danger of misinterpretation of the results of this study derives in part from the authors' interpretation that the data "further strengthen the accumulating evidence . . . that modest doses of synthetic colors . . . can provoke disturbed behavior in children" (1, p. 1488). One clearly responsive child out of the 22 studied represents no more than a rare case of food-color sensitivity. The rarity of this single responder is far greater than 1 out of 22, since the children studied were preselected as "responders" on the basis of open trials with the Feingold diet. If the 22 "responders" represented 50 percent of the subject population in the open trial (a figure frequently mentioned by Feingold) and, further, if this subject population of chil-

dren exhibiting "problem behaviors" represents roughly 5 percent of the general childhood population (2), the finding of one responsive child indicates that about 0.114 percent of the preschool and elementary school children in the United States may be sensitive to food coloring. This is not meant to imply that a disorder occurring at a rate of about 1/1000 is unimportant: on the contrary, if it were not for the concern for such rare disorders, diseases such as phenylketonuria would not today be a manageable disease. However, this still leaves nearly 98 percent of the disturbed children exhibiting "problem behaviors" of unidentified origin, and indicates that the "Feingold hypothesis" has received undue attention.

The study of Weiss *et al.* (1) also raises methodological issues. For example, if the children in the study had not been diagnosed as hyperkinetic, why had they been on the Feingold diet? If they were not hyperkinetic nor had any diagnos-