

cated the pattern of fine-scale cuticular folding illustrated by Wolfe, identical patterns also appeared on *Nelumbo* and *Nuphar* leaves affixed to herbarium sheets, as well as in the epidermis of both previously frozen and unfrozen leaves allowed to dry on glass slides. The folding varied in its extent and was most commonly found near the margin, larger veins and folds, but was not seen on the cuticle of decaying *Nuphar* leaves taken directly from a water bath. After death, the mesophyll of both *Nuphar* and *Nelumbo* could be seen to undergo rapid breakdown into a gelatinous mass. From these observations we infer that such folding is the result of boundary shear stress generated by the slippage of the degrading mesophyll over the more integral epidermis, whose movement is constrained in some way, as by the leaf margin, veins or by adhesion to an interface such as that with the enclosing sediment. Thus we believe that Wolfe's implication that freezing is a unique cause of his cuticular folding is not correct.

We also take issue with some of Wolfe's palaeobotanical identifications and biostratigraphical assertions: for example, leaves of the form-genus *Nelumbites* lack the hexagonal areolation possessed by all modern species of the genus *Nelumbo*; the alleged seed shown in Fig. 3k cannot be positively identified; and the elements in Wolfe's Fig. 3h cannot be reliably recognized either as "growing tips" or "Nelumbonaceae" as labelled. Further, leaves of the extinct form *Paranymphaea crassifolia* differ significantly from those of *Nuphar* in secondary vein spacing and areolation. It thus seems rash to ally *Paranymphaea* with *Nuphar* and to attribute the climatic tolerance and even time of blooming and seed-set of *Nuphar* to the fossil. We also question Wolfe's ability to determine mean daily temperatures using unstated methods, especially in view of the significant objections that have been raised to his leaf physiognomic methods for determining mean annual temperatures⁷.

Far from being anomalous, ponded-water sediments are common⁸ and the assemblage of plants that Wolfe describes is a normal part of the basal Palaeocene sequence for tens of metres or more above the Cretaceous/Tertiary boundary in this region^{9,10}. Despite an extensive fossil record, *Paranymphaea crassifolia* has not yet been reliably identified anywhere in Cretaceous sediments. The assertion that the rhizomes of *Paranymphaea* and *Nelumbites* were lifted out of their growth position in the latest Cretaceous mudstone beneath Wolfe's bed 1 and redeposited in bed 5 of the sequence (Palaeocene) by freezing of the higher parts of the plants into an ice layer that was later buoyed by flood

waters appears highly improbable in view of the lack of disruption of the Cretaceous mudstone or any evidence of remaining leaves or rhizomes in it. Additionally, our observations show that the leaves, petioles and rhizome tips of the modern analogues lose all fundamental strength upon being frozen or shortly after death, and that the leaves sink rapidly after death and are readily disaggregated after as little as 1–3 weeks at 20 °C in the laboratory. Thus, it is highly unlikely that the leaves would have continued floating long after thawing or could have had the coherency to collect sediment on their upper surfaces.

LEO J. HICKEY

LUCINDA J. McWEENEY

Department of Geology and Geophysics
and Department of Anthropology,
PO Box 6666,
170 Whitney Avenue,
New Haven,
Connecticut 06517, USA

WOLFE REPLIES — I proposed¹ a scheme for Cretaceous/Tertiary boundary events based on the Teapot Dome section, which represents an *in situ* lily pond and is the only known boundary section that contains determinable plant megafossils in the bolide fallout layers. All megafossil taxa, which represent pond lily (*Paranymphaea*) and lotus (*Nelumbites*), were discussed and illustrated; these megafossils were interpreted as remains of Cretaceous plants that suffered mass-kill. I reported a series of laboratory experiments on modern lotus (*Nelumbo nucifera*) leaves; these experiments, documented by photomicrographs, indicate that structural deformation like that in the fossil cuticle was produced by freezing but not by other environmental disturbances, including dessication. Further, the deformed cuticles are restricted to the fallout layers, which contain no evidence of dessication. Only two types of pollen occur in antherial masses and hence probably came from plants proximal to the depositional site; these pollen have characters that I interpret to ally them to extant relatives of the megafossil taxa.

Is *Paranymphaea crassifolia* in fact absent in the Cretaceous? If true, the *Paranymphaea* leaves I found 0–5 cm above the impact layer could not represent Cretaceous plants, and *P. crassifolia* must have originated *de novo* at the boundary, a most remarkable and improbable event.

Pollen morphology can be difficult to interpret. Nichols and I interpret the morphology of the spinose Teapot Dome pollen differently, and he asserts that this pollen cannot be nymphaeaceous. This pollen consistently has a single 'fold' and, despite my requests, Nichols was unable to find a 'pore' that was not

observed by a spine. I interpret this pollen as monosulcate and nonporate. Nichols also seemingly does not appreciate that different plant organs can evolve at different rates; the nelumbonaceous pollen, which is distinct from the Cretaceous taxon he mentions, does not have all specialized characters of extant *Nelumbo*, unlike the associated megafossil organs. He denies the significance of the occurrence of nelumbonaceous pollen as tetrads; if this is not significant, why did the typically terse Erdtman⁵ mention this? Nichols' palynological arguments result in the improbable conclusion that in this lily pond neither *Paranymphaea* nor *Nelumbites* was represented by pollen.

If Hickey and McWeeney are attempting to show that my statements on cuticle are false, why did they experiment on *Nuphar* and not *Nelumbo*? *Nelumbites* constitutes about 98% of the megafossils, strongly suggesting that this plant was the source for the structurally deformed fossil cuticle. To observe the minor folds, I had to prepare the leaves chemically, whereas Hickey and McWeeney state that such folds "... appeared on *Nelumbo* and *Nuphar* leaves affixed to herbarium sheets ...". Hickey and McWeeney's folds varied in distribution, whereas my freezing folds are ubiquitous in modern and fossil cuticles. I question whether Hickey and McWeeney are reporting the same kind of structural deformation. I also question the observations on the supposed lack of strength of aquatic organs following freezing: in my experiments, thawed *Nelumbo* leaves underwent some degradation but remained intact and floating for at least 4 months.

Determination of events at the Cretaceous/Tertiary boundary is important, and I thus allowed Nichols to examine my preparations before publication. Similarly, had they asked, Hickey and McWeeney could have examined the megafossils. Why do they now contradict my megafossil determinations without having examined the specimens?

JACK A. WOLFE

US Geological Survey,
Mail Stop 919, Box 25046,
Denver,
Colorado 80225-0046, USA

1. Wolfe, J. A. *Nature* **352**, 420–423 (1991).
2. Müller, J. *Bot. Rev.* **47**, 1–142 (1981).
3. Leffingwell, H. A. *Geol. Soc. Am. spec. Pap.* **127**, 1–64 (1970).
4. Jerzykiewicz, T. & Sweet, A. R. *Can. J. Earth Sci.* **23**, 1356–1374 (1986).
5. Erdtman, G. *Pollen Morphology and Plant Taxonomy* (Chronica Botanica, Waltham, Massachusetts, 1952).
6. Tschudy, B. D. *US Geol. Surv. Prof. Pap.* **770** (1973).
7. Dolph, G. *Proc. Indiana Acad. Sci.* **99**, 1–10 (1990).
8. Fastovsky, D. E. & McSweeney, K. *Geol. Soc. Am. Bull.* **99**, 66–76 (1987).
9. Hickey, L. J. *Univ. Mich. Pap. on Paleont.* **24**, 33–49 (1980).
10. Johnson, K. R. & Hickey, L. J. *Geol. Soc. Am. spec. Pap.* **247**, 433–443 (1991).