to be complimented on a significant and valid contribution to the understanding of phosphorus dynamics in natural waters. However, it should be emphasized that these results do not preclude a significant contributory role by death and decay in the overall DOP dynamics, as he implied.

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In my model (*1*) I presented observations consistent with the kinetics of $[^{32}\mathrm{P}]\mathrm{PO}_4$ movement between the dissolved and particulate forms in lake water.

Death and decay are processes that are more important in conceptual models and in batch cultures than in lake communities. Minear's contention that at maximum biomass growth equals death is reminiscent of elementary explanations for the growth curves of batch cultures. I prefer to think of the biomass in a lake as being maintained through a balance between growth and zooplankton grazing, heterotrophic activity and sedimentation. Instead of a static biomass persisting throughout the summer months, a system is maintained in logarithmic phase and death rarely occurs. Grazing rates in eutrophic waters often exceed 80 to 100 percent each day; hence, death and decay are no more common in lakes than in chemostats with high turnover times.

Minear's criticism that I "characterized" the forms in the filtrate rather than "identifying" them is justified. The original draft of my report included comments on the existing techniques. The measurement of "soluble reactive phosphate" had been considered equivalent to orthophosphate, but it is now known to include artifacts which possibly include labile organic phosphorus as well. The "soluble unreactive forms," otherwise known as "dissolved organic phosphorus" (DOP), may be neither soluble nor dissolved but rather particles smaller than 0.45 μm (*2*). Furthermore, this fraction may not be organic. It is simply a substance that tests as phosphorus after perchloric acid digestion. By combining radioisotope tracer kinetics with gel filtration techniques [as stated in (*1*)], I identified the biologically important forms in a functional way. Space limitations did not permit such an extensive introduction in the final report.

One of the complications that has confused research on phosphorus in lakes is the lack of sensitive analytical techniques. By the time one has collected enough sample, cell damage has undoubtedly occurred and the products are difficult to distinguish from those produced through "death and decay."

Since 1970 when the original work was done which led to the model (*1*) in question, I have extended the experimental period from between 1 and 24 hours to 2 months in both hard water and soft water lakes. Some modifications in the existing model will need to be made, but the formation of so-called DOP seems to be the product of an excretion process and not the result of the "death" of the cell.

One should not get too upset over the lumping together of several compounds in a compartment I term "colloidal phosphorus" when I have committed an even greater "blunder" by lumping all living organisms, detrital material, clay particles, and other particulate materials under the heading "particulate phosphorus." My only excuse is that the model appears to be consistent with the observed kinetics.

The attention that Minear has given to DOP is certainly justified. Better identification will not only advance our knowledge of the role of phosphorus in lake water but may also provide some new insights into cellular metabolism.

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Primate Evolution: Were Traits Selected for Arboreal Locomotion or Visually Directed Predation?

In criticizing the proponents of the arboreal theory of the origin of primates, Cartmill (*1*) appears to overlook the principal reason why their theory was adopted, and if he did not make such an oversight, the differences between his view and theirs would be reduced. The arboreal theory was not postulated by Smith (*2*) and Le Gros Clark (*3*) to explain the replacement of tree shrew–like morphology with primate-like morphology, but rather to account for the emergence of tree shrew–like morphology from that of a terrestrial insectivore. Neither Smith nor Le Gros Clark believed that arboreal life per se accounted for primate differentiation, as is evinced by Smith's view (*2*, p. 39) that the elaboration of man from apes involved ". . . a continuation of those processes of evolution which we have been examining in the lowlier members of the Primate series." Thus, the proponents of the arboreal theory would readily agree with Cartmill's remark that tree dwelling, by itself, is not sufficient to transform an arboreal insectivore into a primate; some additional conditions, perhaps predation, as Cartmill suggests, is necessary for primate differentiation. In any case, both Cartmill and those he criticizes believed that increased reliance on vision was a key to understanding the evolution of primates.

It turns out that neither predation nor tree dwelling alone can provide a complete picture of changes in the visual nervous system in the various mammalian lines of descent. Cartmill uses the cat to illustrate the contention that predation results in the evolution of a highly developed visual system. In fact, the cat might be better used to show that predation does not result in a primate-like brain and visual apparatus. In presenting the cat as a highly visual animal, he overlooks the fact that not only is the acuity of the cat (*4*) and the dog much less than that of primates (*5*), but also, in psychological testing, visual stimuli have been shown to be much less compelling to carnivores than are auditory stimuli (*6*). It also should be noted that, irrespective of stereopsis, the visual anatomy of the cat is not very similar to that of the primate (*7*).

Finally, Cartmill argues that the case of the squirrel shows that an arboreal habitat does not produce primate traits. However, the brains of both the tree shrew and the squirrel do have many striking primate-like features; both possess enlarged occipital lobes and visual projections to the temporal lobe (*8*). Furthermore, although, as Cartmill states, the squirrel possesses only 60° of binocular overlap, this does not imply that it lacks significant stereopsis since the major proportion of the visual cortex and the dorsal lateral geniculate receive this binocular input (*9*). Thus, it appears that a squirrel-like habitat may very well have set the stage for the evolution of primates since, as Smith wrote 50 years ago (*2*, p. 31), ". . . some small arboreal shrew-like creature took another step in advance, which was fraught with the most far reaching consequences, for it marked the birth of Primates."

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Both W. E. Le Gros Clark and G. E. Smith believed that prosimian and anthropoid morphology was generated from tree shrew–like morphology by the same set of selection pressures (and resulting evolutionary trends) that led to the differentiation of the tree shrews. This point is clearly stated in the peroration from *The Antecedents of Man* (*1*, p. 349), where Le Gros Clark asserts that the adoption of arboreal habits by "the first little tree-shrew-like creatures" initiated the gradual development "in progressive stages" of the distinctive primate characters, including enhanced visual acuity, olfactory regression, brain expansion, and improved prehensility of the hands and feet. Smith's concurrence in this view is clearest on pages 31–35 of the cited work (*2*). To be sure, both Le Gros Clark and Smith believed that many of these evolutionary trends, once initiated by the adoption of arboreal habits, were partially self-perpetuating through a positive feedback process—particularly in lineages of the anthropoid grade. Raczkowski's quote from Smith deals with these later phases of primate evolution; in context, it is clear that by "lowlier members of the Primate series," Smith meant Old and New World monkeys.

K. U. Smith's (*3*) cats were able to discriminate line widths at 75 cm corresponding to critical visual angles as small as 0.45 minute of arc, whereas the minimum value established by Weiskrantz and Cowey (*4*) for rhesus monkeys was 0.57 minute of arc. The studies cited by Raczkowski thus support Smith's (*3*) conclusion that there are "no great differences between the visual capacities of the cat and the monkey in the discrimination of single lines." Even if this conclusion proves to be false, no differences between primates in general and felids in general can be established by comparing a 3-kg nocturnal felid with a 10-kg diurnal monkey. These studies are therefore irrelevant to the conclusions Raczkowski wishes to draw. The work of Jane *et al.* (*5*) is doubly irrelevant, since comparable data for primates were not collected.

I do not assert that predation per se "results in the evolution of a highly developed visual system"; the predatory behavior of many mammals (and probably of the ancestral mammals as well) relies chiefly on olfaction, hearing, and vibrissal contact. I agree (*6*) that primates resemble squirrels but differ from cats in some features, and that these may represent specifically arboreal adaptations. I also agree (*7*) that squirrels may well have stereopsis. If so, this is further support for the notion that the lateral orbital orientation seen in squirrels and tree shrews is a good adaptation for arboreal acrobatics (since it permits maximum parallax and hence maximum stereoscopic depth), while the wide visual-field overlap found in cats and primates originated as an adaptation for activities which demand a broader stereoscopic field over relatively short distances.

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