males occasionally stopped answering any pattern or flew away, and those tested undoubtedly differed with respect to age, condition of ovaries, number of successful predations, exposure to flashes of foreign males (kinds and numbers), and genetic makeup. Apparently the mimicry is not perfect, although comparative figures cannot be given since attraction rates for conspecific interactions are unknown. One female captured the 12th *macdermotti* male she answered. Another answered 20 congener males, and then moved to a different perch several meters away and answered more than 20 additional males before she captured one. Another female caught the 21st congener male that she was observed to answer. Capture rates were higher for prey belonging to other species: on five occasions I observed the demise of *Photuris* A males; two females captured the first male answered, one caught the second, one the tenth, and one female got the 11th, although she had seized the seventh male and it had escaped. Two other females captured the fifth *tanytarsus* males that they answered.

What is the evolutionary origin of the false signals? Two independent sources are suggested. The flashed responses to *Photuris* A, *Photinus tanytarsus*, and *Photinus macdermotti* males appear to be similar in delay timing to the predator’s own mating responses. False signals could have been derived originally from mating responses and subsequently modified. Responses to the flashes of *Photuris* congener males are similar to the flashes that the predae’s females, and those of many other *Photuris* species, commonly emit when they walk, land, or take flight (9). These “locomotion” flashes would need little if any modification to attract some congener males. (The flashes of the congener female, unlike those of other species, do not bear a specific relation to each flash of the male.) I am able to attract about one male in ten to the 0.08-second flashes of a free-running oscillator with a period like that of the males. I once observed a lycosid spider eating a congener male that continued to emit his rhythmic pattern; two additional congener males were attracted to the flashes of the captive, and were also seized by the spider. I offer this not as an example of a tool-using spider, for I doubt that it is repeated with regularity, but as an indication of how a physiologically inappropriate but trophically fortuitous activation of the locomotion flash mechan-
Phosphorus Dynamics in Lake Water:
Contribution by Death and Decay

In his report Lean (1) appears to extrapolate from results of labeling studies of 1 to 24 hours in duration to conclusions concerning the entire dissolved organic phosphorus (DOP) pool in the natural water system. Although his work is interesting and seems to further elucidate the easily labeled and, thus, apparently the highly labile fraction of the DOP pool, several comments are in order.

First, his words "I identified the forms of $^{32}$P in the filtrate . . . ." are misleading since he has in fact not identified anything. He has characterized three fractions of labeled phosphorus, namely, the original orthophosphate, a high-molecular-weight fraction, and a low-molecular-weight fraction. Each of the latter two could quite reasonably include a host of compounds since Sephadex separations are based primarily upon molecular size differentiation.

Second, Lean proposes that the high-molecular-weight fraction is the result of a combination of the low-molecular-weight organic phosphorus with colloidal material in the lake water. In his model, then, he precludes the direct formation of high-molecular-weight phosphorus in the soluble or colloidal form. For the specific case where only 3 minutes of contact occurred between the organisms and the added $[^{32}]$PO$_4^-$, it is reasonable to argue that the decay of organisms is not a likely source of the soluble organic phosphorus. However, if labeling of organic molecules within the organism can occur within 3 minutes, what evidence is there that release to the surrounding water has occurred by excretion instead of death and cellular lysis? Lean acknowledges that the experimentation took place at maximum biomass, a point where growth and death would be in balance.

On the other hand, if, in fact, not all the cellular organic phosphorus components received $^{32}$P labeling in this short period of 3 minutes (that is, if some specific chemical compounds received no $^{32}$P incorporation), then the model dynamics may indeed ignore a significant segment of the organic phosphorus pool. This nonlabeled segment could be comprised of an entirely different set of compounds from those represented by the "XP" and "Colloidal P" of Lean's model. However, these compounds could be released into solution predominantly by death and decay. Thus, Lean's repudiation of other authors' claims of release by death and decay (his references 12 and 13) based upon his model and sequence of experiments is not valid.

In fact, there is conclusive evidence (2), published prior to the final submission of Lean's report, that a significant fraction of the DOP in both laboratory algal cultures and natural waters is DNA or its fragments (7 to 10 percent of the DOP) capable of exclusion from Sephadex G-75 and G-100 gels. The DNA material represented roughly 50 percent of the total high-molecular-weight fraction. Three distinct responses were used to validate the identity of this isolated, high-molecular-weight material: (i) a deoxyribose-specific fluorescence analysis for DNA, (ii) enzymatic breakdown by deoxyribonuclease of the isolated high-molecular-weight peak, and (iii) conclusive isolation and identification of the bases adenine and guanine by two-dimensional, thin-layer chromatography (including cochromatographing standards and specific color reactions) after perchloric acid digestion of the isolated high-molecular-weight material.

That this material originated from the soluble compartment, independent of cellular damage during processing, was amply demonstrated (2) and in one case demonstration relied solely upon diffusive transport across a 0.22-$\mu$m membrane into sterile culture media. Although direct evidence to differentiate between direct excretion of DNA fragments by living organisms and release into solution by death and by subsequent decay was not sought, the presence of such fragments would certainly evoke tempers in denying the contribution of death and decay to the DOP pool.

Certainly, if Lean's experiments deal solely with excreted compounds originating from viable organisms, then most likely his results do not pertain to the entire DOP pool. In fact, he states, " . . . I concluded that no high-molecular-weight material was excreted, only XP."

Since the existence of DNA fragments in both algal cultures and several natural water systems has been clearly documented (2), these fragments must have been either excreted (contrary to Lean's hypothesis quoted above) or released by death and decay. Lean is
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TD Miale, JL Frias and DL Lawson

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