This letter is our reply to Dr. Estes’ comments concerning our recent journal publication.

Our use of the term “labelled P” can be explained as follows. The young plant has a given pool of $^{32}$P inside its cells before being exposed to the treatment solution. At the end of the treatment period this pool now consists of three fractions: (i) the original $^{31}$P, (ii) $^{32}$P radioactive isotope, and (iii) the $^{31}$P absorbed during the treatment period. The latter two fractions represent the only part of the total P pool whose origin was the treatment solution and the part of interest in this study; hence, we call this the labelled P fraction.

Understanding this, Fig. 1 and 2 (2) show an uptake of 0.06 mg, not of $^{32}$P, but of $^{32}$P and $^{31}$P in the same ratio as present in the treatment solution. This technique represents a common application of radioisotopes to the study of salt absorption.

The 600 mg of uptake discussed by Dr. Estes represents a 10,000-fold misinterpretation of the ordinate units. The labeling system used in this paper is coincident with other authors in this volume and is further explained in (1).

As explained in the methods section, Fig. 3, 4, and 5 (2) are reproductions of continuously charted activity accumulations, representing an infinite number of data points. Thus, these figures are copies of the original data and not products of randomly selected data points.

We feel the use of comparative assay methods conforming to accepted radiotracer assaying techniques to be understood. However, identical sample geometry was assured by common preparation techniques, detection efficiency is peculiar to individual instrument arrangements and its inclusion would be of no value to the paper. The ash material of 10-day-old corn plants was of insufficient quantity to require self-adsorption correction.

References

Reply to “Fiction in Physics”

The particular statement in my review to which Dr. Childs refers is: “Regarding air-water interface effects . . . , the explanation reverts to the fictitious notion of an actual physical tension presumed to be lying tangentially within the interface, whereas the analysis could have been carried forth just as well on the basis of the less artificial concept of energy per unit area of surface.” In view of the debatable connotation of the word “fictitious,” I would forthwith suggest its deletion. I do, however, reiterate a preference for the surface-energy approach, primarily because its thermodynamic character does not require a mechanistic model or specification as to just where a “force per unit length” would be acting. Therefore, I favor the derivation of the pressure-curvature equation from the standpoint of surface energy (Adam, 1941, p. 8, 9), rather than from the explicit use of force vectors drawn tangentially in the interface surface (Childs, 1969, p. 150–152). There is, of course, the mathematical sense in which the units of surface energy cancel out to those of a tension (Adam, p. 2, 3), but there appear to be serious difficulties in ascribing physical reality to such a tension (Adam, p. 3–5).

References