Kinins, Cytokinins, Phytokinins

Skoog, Strong, and Miller (*1*) urge the adoption of the term "cytokinin" in place of "kinin" in order "to avoid further confusion." I am of the opinion that the confusion was at least partially ameliorated by the following letter, published in *Scientific American* in 1963 (*2*):

> The term "kinins" has for some time been applied by our group and other workers in plant physiology and growth-regulator mechanics to the group of substituted purine derivatives promoting kinetin-like responses in plant tissues. In the August 1962 issue of *Scientific American* H. O. J. Collier published an article entitled "Kinins." The article dealt with polypeptides that possess hormonal activity in animal tissue.
>
> Thus we have two identical words in the literature that refer to vastly different chemical entities. For this reason we suggest that in the future the plant kinins be called "phytokinins" to avoid much confusion.

My opinion is evidently shared by others, for the more explicit term phytokinins has subsequently been used and accepted by editors and referees of several journals (*3*), *Science* among them. The ill-chosen term kinins was abandoned on a basis of precedence of usage. Logically the suggested new term cytokinins should be abandoned before adoption in favor of phytokinins on the basis of precedence of definition, usage, and acceptance.

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3. D. C. MacLean and R. R. Dedolph, *Am. J. Botany* **51**, 618 (1964); R. R. Dedolph, *Bioscience* **14**, 53 (1964); T. Shirakawa, R. R. Dedolph, D. P. Watson, *Proc. Am. Soc. Hort. Sci.* **85**, 642 (1964); H. C. Dostal, V. Tuli, R. R. Dedolph, *ibid.*, in press; V. Tuli, D. R. Dilley, S. H. Wittwer, *Science* **146**, 1477 (1964); D. A. Gilbart and R. R. Dedolph, *Proc. Am. Soc. Hort. Sci.*, in press.

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Wright Valley: Conjectural Volcanoes

Two reports in recent issues of *Science* relating to Wright Dry Valley in Antarctica pose problems which may have a single, simple explanation.

H. T. U. Smith [*Science* **148**, 941 (1965)] infers that the scabland (Bull and others; see Smith's reference *1*) in the western head of the valley was carved by a castastrophic flood with an origin like that of the Spokane Flood, which many believe carved the Channeled Scabland of eastern Washington. This inference, as he points out, predicates a much greater deglaciation of Antarctica than that for which geologists have found other supporting evidence. Such a degree of ice retreat would probably have caused a greater world-wide rise of sea level than any for which clear evidence is available in lower latitudes.

Ragotzkie and Friedman [*Science* **148**, 1226 (1965)] infer from the low deuterium content of Lake Vanda, down valley from Smith's area, that the deep, salty water in this lake came from Upper Wright Glacier, in contrast to the present inflow of meltwater, Onyx River, from Lower Wright Glacier. This leaves unexplained the composition of the salts in Lake Vanda. Calcium chloride is not the major constituent of the dissolved solids in most glacier ice. Angino and Armitage (their reference *2*) ascribe the high Ca^{++} and Mg^{++} content (Ragotzkie and Friedman omit 7684 parts per million of magnesium ion from the analysis they quote from Angino and Armitage) to erosion of dolomite exposed in the eastern part of Wright Valley, but the dolomite does not occur to the west (see McKelvey and Webb, 1962, Smith's reference *2*). Angino and Armitage point out that the chloride could be accounted for by admixture of modern or ancient sea water, but that this would not explain the composition of the salt as a whole.

I suggest that both these anomalies —the major flood in Wright Dry Valley and the anomalous deuterium and solute content of the water in Lake Vanda—can be explained if volcanic eruption occurred under the thick ice west of Wright Dry Valley. Evidence of postglacial volcanism occurs in the adjacent Taylor Dry Valley, and active Mt. Erebus is not much farther away.

Steam erupting under several thousand feet of ice would melt the ice and would itself be condensed, thus forming a considerable volume of water in a very short time. Initially, perhaps, this water would form a lake, hemmed in by surrounding ice until it found an escape route. By the time an escape route opened up, several cubic miles of water might be present. All this water would then rush down the valley.

The large volume of water from such a lake could have carved Smith's scabland channels in the absence of extensive deglaciation. The explanation is catastrophic, but the evidence calls for a catastrophic cause, and the much-feared *jökulhlaup* of Iceland is surely catastrophic, even though in that area a volcano has a much smaller volume of ice to work on.

Moving on down the valley, the volcano-melted floodwater would fill the Lake Vanda basin. It would undoubtedly be low in deuterium and have a much higher content of solute than ordinary glacial meltwater; this solute might be high in the Cl^- ion. The high Ca^{++} and Mg^{++} and low Na^+, K^+, and SO_4 concentrations are less easily accounted for; kenyte, present in the McMurdo Volcanics, is an alkalic rock, and one might expect high Na^+ and K^+ in the associated and derived waters. However, the suggestion seems worth consideration.

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Tektites: Origin of Parent Material

By his discovery of coesite in tektites from Phaeng Dang, Thailand, L. S. Walter (*1*) has helped to establish their origin during a highly energetic event from material containing quartz. The purpose of this note is to correct the following misstatement, in his report, of my views on the origin of the parent material of tektites:

> The material from which the tektites were formed was either glass or extremely fine-grained before the quartz was transformed to coesite by impact metamorphism. This limitation on the source material places a severe restriction on the terrestrial origin of tektites. Certainly the Phaeng Dang tektites could not have formed through mixing of the melts of various sedimentary rocks as proposed by Taylor and this casts a doubt of such a source material for other tektites.

In the paper referred to, Cherry and I (*2*) were led, from a consideration of the major element relationships in tektites, to postulate that the chemical composition of tektites was similar to that of a mixture of 75 percent shale and 25 percent quartz. It was