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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References

1. F. Skoog, F. M. Strong, C. O. Miller, *Science* **148**, 532 (1965).
2. R. R. Dedolph, S. H. Wittwer, D. C. MacLean, *Sci. Am.* **208** (No. 1), 13 (1963).
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 always our understanding, although perhaps not clearly stated in this early paper, that the mixture was produced prior to the melting process by sedimentary or analogous processes, and did not occur during the processes of tektite formation. Co-workers and I have considered the question of the composition of tektites and their parent material in several papers (*2–8*). From these, the gradual development of our ideas on the nature of the parent material of tektites, and of the "mixing" hypothesis, may be summarized as follows:

The initial observation was of highly significant positive correlations among the concentrations of the alkali elements in australites (*4*). Since such strong correlations are not observed to arise through the operation of terrestrial igneous fractionation processes, alternative mechanisms were sought to produce such an effect. If tektite parent material was a mixture of two phases, one of which contained the alkali elements, then the observed correlations would be accounted for by variations in the amounts of these two phases in different tektites. Study of the available data for major elements was immediately undertaken (*2, 5*). This showed that strong negative correlations existed between SiO_2 and Al, Fe, Mg, Na, and K, and indicated that silica could be one of the phases. This agreed with the observations of lechatelierite particles (*9*). The general dissimilarity between the overall composition of tektites and that of terrestrial igneous rocks, and the existence of the strong negative correlations between silica and the other major constituents (except Ca), led us to postulate the quartz-shale mixture (*2*) as being similar to the parent material of tektites.

The study of the abundance of the trace elements in tektites was undertaken partly to throw further light on this problem, and, eventually, data for 43 australites with 32 new analyses of major elements became available (*6, 7*). These data showed that strong negative correlations existed between SiO_2 and Al, Cr, Mg, Ti, Fe, Na, K, Rb, Cs, Li, Sc, Ba, Ni, and V. These elements were considered to be present in a shale or clay fraction. Ca and Sr showed no correlation with silica, attributed to their presence as an independent component. Zr showed a positive correlation with SiO_2, likewise attributable to a separate component. Elements such as Ga, Cu, and Mn did not show negative correlations with silica. This evidence, taken in conjunction with the absolute abundances, led to the postulate that the parent material of tektites was similar to the composition expected from a mixture of quartz, shale (clay), calcite, and zircon, and that thus a sandy shale would be a suitable parent material. The mixing together of these components occurs during the sedimentary or related processes during the formation of the parent material, and not during the melting. Data of other workers were consistent with this suggestion that the parent material was of a sedimentary type. For example, Haskin and Gehl (*10*) found that the pattern of relative abundance of rare earths was similar to that observed in terrestrial sediments.

In all this work on the chemical composition of tektites and their parent material, a major handicap has been the scarcity of comparative data for the composition of sedimentary rocks, particularly for the trace-element abundances (*11*); this has made difficult the identification of the specific rock type melted to produce tektites. The changes produced in the chemical composition during melting of the parent material appear to be minor; the parent material may therefore be identified from the present composition (*7, 12*).

Further light has been thrown on this subject during an investigation of the glass produced by meteorite impact at Henbury, Australia (*12, 13*). The parent material of the impact glass is a sediment of subgreywacke type, which bears a very close resemblance to the composition of australites (*8*). Analyses of different samples of the sediments show the same inverse relations with silica as are observed in the tektites (*13*), in addition to containing similar absolute concentrations of the elements (*8*). The rock consists of quartz grains ranging in size from 0.01 to 0.2 mm diameter, averaging about 0.06 mm diameter, and set in a fine-grained matrix of chlorite and clay (*13*).

The identification of a common terrestrial sedimentary rock close in composition and in element relationships to the composition of australites is considered to be a confirmation of our interpretation of tektite composition over a number of years. Such studies do not, of course, reveal the place of origin of tektites, but indicate the nature of the material at that place. If tektites are derived from the moon, then the composition of the lunar surface at their place of origin is very similar to that of subgreywacke exposed in the Henbury meteorite craters, Australia, and a study of such sediments should be of interest to NASA scientists. Material of this composition is produced terrestrially only by sedimentary processes following a complicated evolution from primordial solar-system material. If such material occurs on the moon, the controversy over the place of origin of tektites will be replaced by one on lunar development and petrological processes, and our current ideas about the moon will need major revision.

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References

1. L. S. Walter, *Science* **147**, 1029 (1965).
2. R. D. Cherry and S. R. Taylor, *Geochim. Cosmochim. Acta* **22**, 164 (1961).
3. S. R. Taylor and L. H. Ahrens, *ibid.* **15**, 370 (1959); S. R. Taylor, *Nature* **189**, 630 (1961); **195**, 32 (1962); **201**, 281 (1964); *Geochim. Cosmochim. Acta* **26**, 915 (1962); **29**, 65 (1965).
4. S. R. Taylor, *Geochim. Cosmochim. Acta* **20**, 85 (1960).
5. R. D. Cherry, S. R. Taylor, M. Sachs, *Nature* **187**, 680 (1960); S. R. Taylor, M. Sachs, R. D. Cherry, *Geochim. Cosmochim. Acta* **22**, 155 (1961).
6. S. R. Taylor and M. Sachs, *Nature* **188**, 387 (1960); *Geochim. Cosmochim. Acta* **25**, 223 (1961); S. R. Taylor, *ibid.* **26**, 685 (1962).
7. S. R. Taylor and M. Sachs, *Geochim. Cosmochim. Acta* **28**, 235 (1964).
8. S. R. Taylor, *ibid.* **29**, 599 (1965).
9. V. E. Barnes, *ibid.* **14**, 267 (1958).
10. L. Haskin and M. A. Gehl, *Science* **139**, 1056 (1963).
11. F. J. Pettijohn, *U.S. Geol. Surv. Profess. Paper No. 440-S* (1963).
12. S. R. Taylor and P. Kolbe, *Nature* **203**, 390 (1965).
13. ———, *Geochim. Cosmochim. Acta* **29**, 741 (1965).

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