

LETTER TO THE EDITOR

COMMENTS ON: THE ALTERATION OF SOME AROMATIC AMINO ACIDS AND POLYHYDRIC PHENOLS BY CLAY MINERALS

I would like to make a few comments on the interesting paper by Thompson and Tsunashima (*Clays and Clay Minerals* **21**, 351–361, 1973), which apparently lends support to my work on asymmetric reactions of the optical isomers of amino acids on clay-mineral surfaces.

In the first place, I wish to draw attention to the full-length report of my research (Jackson, T. A., *Chem. Geol.* **7**, 295–306, 1971). The papers in *Nature* and *Experientia* cited by Thompson and Tsunashima are merely brief preliminary notes.

In the second place, I feel that further work needs to be done to verify the hypothesis that abiotic oxidative degradation occurred. No mention is made of the temperature at which the experiments were performed, and I assumed that it was room temperature. It seems surprising that abiotic degradation catalyzed by clay in an aqueous system should have occurred so rapidly under such mild conditions, whereas it would not be at all surprising if microbial growth

had occurred. The authors' arguments invoking CMP-acetate and $Al(OH)_3$ may strengthen the case for an abiotic reaction, but I have misgivings about unsterilized solutions of readily metabolized organic compounds left standing at room temperature for days on end. If the authors plan to do any further work, one might suggest that they routinely use experimental conditions that exclude microbial growth absolutely. Another complicating factor is that the observed decrease in absorbance at $275 \mu m$ and increase at $300 \mu m$ could be due, at least in part, to adsorption of the amino acid by the clay, accompanied by desorption or dissolution of small amounts of humic matter associated with the clay. Humic matter absorbs strongly in the u.v., is soluble at alkaline pH, and is commonly associated with clay minerals.

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