

phenotypic variation may not be reflected in the sediments. If, as seems likely, phenotypic differences are superimposed upon the supposed evolutionary sequence, then one can hardly hope to elucidate the problem by using coarse sampling methods. Referring to Hallam's present graphs, I agree that readers must judge for themselves whether the dots and crosses have the same trend, line or whether perhaps the crosses tend to lie towards the right of the dots in the lower part of the scatter.

In his additional statistical work Hallam has used the breadth of the left valve as an alternative size dimension, but finds little difference from his previous results. This improvement in the choice of the size dimension is wasted unless he also abandons the length of the periphery of the left valve as a measure of coiling.

I concluded my previous article (1959) on this subject with the statement: "Reviewing the various quantitative methods which have been applied to the study of the *Ostrea-Gryphaea* series has led me to the opinion that most of the statistical evidence is open to criticism and does not provide a sound basis for any conclusion. Until further evidence on the relationship between size and coiling becomes available, the only possible verdict is Not Proven." Despite Hallam's additional statistical work, I still hold this opinion. With reference to the statistical evidence it is impossible to judge whether it is Trueman or Hallam who has reached the right conclusion for the wrong reasons.

Some who have read my *Biological Review* article have expressed regrets that I reached no conclusion. But this is not correct. I concluded that the available evidence does not justify most of the conclusions which have been drawn by previous workers. I used the existing data to make a number of suggestions, and drew attention to some of the difficulties which would inevitably arise in the interpretation of the results of any further investigation. It is true that I reached no solution to the *Gryphaea* problem. This raises the question whether any solution is possible. Oysters are notoriously variable and few would regard them as ideal material on which to base a study of an evolutionary series. In the present case it is demonstrable that the extent of phenotypic variation at the top of the succession is comparable with the supposed evolutionary change throughout the succession. Under these circumstances one must be realistic and accept that the Liassic *Ostrea-Gryphaea* series is intrinsically unsuitable material for this type of investigation. While it might be instructive to try to obtain some more data on the relationship between size and coiling, the results of any further work are likely to be amenable to more than one interpretation.

ADDITIONAL REFERENCES

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"DELTAIC" CYCLOTHEMS

SIR,—In principle, steady even subsidence *by itself* seems capable of determining the birthplace and subsequent cyclothemic life-history of a "delta". No more need be put into a general theory. All "deltas" can be considered as possessing innate tendencies to overlengthen and short-circuit

repeatedly. *Leave one alone in a subsiding region and it will produce regression-transgression cyclothems without further interference.*

The simple theory is deducible from the classic Mississippi researches of Fisk, Russell, and the workers they inspired. Dr. A. J. Wells (*Geol. Mag.*, 1960, xcvii, pp. 389–403) just fails to formulate it, though usefully comparing the importance of various factors known to influence “deltaic” sedimentation.

Dr. D. Moore has shown that certain Yoredale cyclothems are substantially explained “without recourse to more fanciful ideas” (*Journ. Geol.*, 1959, pp. 522–539). Prof. A. Bersier proved the same for the Molasse (*Eclog. Geol. Helv.*, 1959, li, pp. 854–893, esp. fig. 4). Twelve years ago (XVIIIth Int. Geol. Congress) I applied the idea to Wealden cyclothems. New knowledge since has necessitated upgrading certain factors (e.g., eustatic changes of base-level) and my position is now close to Dr. Wells’s.

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REVIEWS

THE EVOLUTION OF NORTH AMERICA. By PHILIP B. KING. pp. 189 with 96 text figs. and one folding map. Princeton, Princeton and Oxford University Presses, 1959. Price 60s.

Formulated as a series of lectures, this book is the first coherent and readable account of the geology of North America. The undergraduate audience required the frequent explanations of general principles and concepts in geology which, to a foreign reader not using this as his first approach to geology, seem oddly out of place. The lecturing style has not been concealed and this allows a free and colloquial expression of views, often controversial yet without laborious qualification. There is indeed textual evidence of the stenographic mode of production which from lesser lips is becoming a menace. On the other hand, the elementary nature of the exposition makes for simplicity and the book can be read straight through with rewarding clarification and without the burden of over much detail.

In using North America as a type continent to develop his ideas on crustal evolution, one of the most distinguished American geologists gives us the benefit of a sustained and mature study of his region. The concepts which this uses, and to which it is fitted, make a coherent philosophy rooted in a century of Appalachian and wider studies and illustrate well how thought is influenced by environment. The tectonic giants of the European Alps by their very success tempted others too long to view crustal structures and processes in the light of an atypical geosyncline and orogeny. So one can foresee this newly integrated view of the evolution of the American continent exercising a similar tyranny in geological thought. Jargon is fashioned by vivid examples, and the structural units of the American continent or its variety of geosynclines (Kay’s terminology suitably translated by King p. 57) have a parochial flavour: thus “ancient rocks, covered by later sediments which remain nearly flat-lying” are *interior lowlands*—interior in the U.S. but not necessarily so. However, the author helps to liberate us from some prevailing fashions. Island arcs have been worked harder on less basis than most concepts and King uses the more general term *tectonic lands* when he needs an area giving rise to sedimentary (and pyroclastic) material. Other prevailing fashions still find their place in his synthesis. Continental drift is considered,