CORRESPONDENCE

THE GROWTH AND FORM OF BRACHIOPOD SHELLS

SIR,—I should like to congratulate Dr. M. J. S. Rudwick on his recent paper, " The growth and form of brachiopod shells " (Rudwick, 1959) and express appreciation of his analysis of the different components involved in their growth. I cannot agree, however, with his statement that dimensions such as length, breadth, and height of the shell form an unsuitable basis for statistical analysis. His statement appears to be founded on three points. There is undoubtedly an element of truth in each, but I shall attempt to demonstrate that each contains an element of misconception which has, I believe, led him to draw a false conclusion.

(1) It is true that statistical analysis based on the frequency distribution of the ratio of height and length may fail to discriminate between two species of brachiopod which demonstrably differ in the profile of the shell. But it is not true that in statistical treatment they " would be regarded as identical in this character ", because it is fundamental in the logic of statistical comparison that absence of a significant difference does not prove identity. It is true that statistical comparison has sometimes been misused in this respect, but only an illogical statistician would really consider that he could prove identity. The absence of a significant difference may be used to supplement qualitative observations that also fail to reveal a difference, but in such a case, the possibility always remains that further statistical analysis using larger samples might reveal a difference. Statistical analysis is an ideal tool for those seeking differences between populations, but it cannot be used to demonstrate identity.

(2) In the case of a species which changes during ontogeny from a gently convex to a highly convex shell, it is true that a frequency distribution graph of the ratio between height and length might show a bimodality, when a high proportion of young shells is present in the sample. But even in the absence of a thorough qualitative examination of shell forms, it is not true that " this bimodal distribution would certainly be taken to reveal the presence of two species ". It is unlikely that a statistician would jump to this conclusion because it is well known that bimodality may be produced in a number of different ways and that the interpretation of such graphs has many pitfalls (Joysey, 1956). The frequency distribution method was used in some of the earliest applications of statistical analysis to palaeontology, but it has now been generally abandoned in favour of relative growth studies. If the relative growth method were applied in the case cited by Dr. Rudwick the difficulty which he has described would not be encountered. If one plotted a graph of height against length the resulting scatter diagram would indicate the relationship between the young and adult stages of each species in a mixed assemblage. It would be possible to fit a trend line to the scatter diagram of the young stages of each species and use the slope and intercept of these lines as a basis of statistical comparison.

(3) It is true that dimensions such as length, breadth, and height of a brachiopod shell are each a complex resultant of many different growth components, and that any particular magnitude may have been produced by any of an infinite number of different growth patterns. It is also true that these dimensions cannot adequately express the subtly complex shell forms of brachiopods. But to conclude that these dimensions are unsuitable for statistical analysis involves a *non sequitur* in the argument, because it is not intended that they should even attempt to describe the many different growth components of which they are a product. The inherent complexity of a biological dimension does not destroy its reality. The dimensions of a brachiopod shell are characters in their own right, and are amenable to statistical analysis which may be used to detect differences between populations.

The application of statistical methods to palaeontology is still in its infancy, and some of the methods which were used in the earlier studies were incorrectly applied. Many recent studies give the impression that a sledge-hammer

is being used to crack a nut, because they are largely concerned with the development and trial of new techniques. Although still based on such simple dimensions as length, breadth, and height of the shell these new techniques have already overcome the difficulties described by Dr. Rudwick, and so it is certainly premature to reject these dimensions as a basis for statistical analysis.

On a broader issue, there is at present a tendency for palaeontologists to prefer either qualitative or quantitative methods and to follow one almost to the exclusion of the other. In consequence, they sometimes have an incomplete appreciation of the available evidence relating to a problem, and are liable to become engaged in controversies which have no valid basis. The final solution of any problem must satisfy the evidence derived from all sources. In the field of palaeontology, I believe that qualitative and quantitative methods should not be regarded as alternative to each other, but rather as supplementary to one another.

REFERENCES

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SIR,—I am grateful to Dr. Joysey for his comments on my critique of the use of parameters in brachiopod statistics and I welcome the opportunity to clarify the position.

(1) In the first case, it is true that a competent statistician would regard the shells as " not significantly different ". But if he is more than a competent statistician—if he is a palaeontologist competently using statistics for a palaeontological purpose—he must necessarily go beyond his judgment as a statistician and interpret the absence of statistical difference in palaeontological terms. This involves his judgment as a palaeontologist, which is logically discontinuous from his judgment as a statistician. It is my contention that the absence of any significant statistical difference is liable to be interpreted as a morphological identity in the character, and thereafter as a taxonomic identity of the shells. This is not a question of good or bad statistics, but of good or bad palaeontological judgment.

(2) I hope that Dr. Joysey's cautionary comments on the use of frequency distribution graphs, in the paper to which he refers, will help to raise the standards of the statistics used by palaeontologists. But even the use of a scatter diagram would fail to eliminate the ambiguities and confusions that I described. The adults of the two species would still be grouped indistinguishably together in the same part of the scatter ; and the young of the second species would occur in a scatter separate from the corresponding adult shells and separate from the young of the first species. Certainly it would be possible to fit trend lines to these different scatters ; but the palaeontological *meaning* of the lines could be established only by comparing the scatter diagram with the actual shells, that is, by a qualitative study of shell form. This would reveal the metamorphosis undergone in the ontogeny of the second species, and hence would lead to a true interpretation of the trend lines. The example I used in my paper was of course an extreme case ; and probably no palaeontologist would in practice fail to notice the great difference in form between the two shells described. But the application of statistics, using the final length, breadth, and height of the shell as parameters, is liable to lead to analogous false interpretations even when the differences in shell form are less immediately obvious. A more fruitful application of

quantitative method would be to plot the changes in certain parameters during the ontogeny of many individual shells, deriving a separate curve from each shell. This would make the differences between the shells graphically apparent, and would be the quantitative analogue of the " dynamic " modes of description which my paper was designed to advocate ; but it would not be, strictly, a statistical technique.

(3) It is true, of course, that *any* parameters can be used as a basis for statistics, regardless of their ontogenetic derivation. But, as in the other points, we are concerned with the palaeontological interpretation of the statistical results, not with the competence or incompetence of the statistician. The question at issue is again one of palaeontological judgment : are statistics based on these particular parameters so far divorced from the mode of growth of the shells, and so far abstracted from the observable shell forms, that they are liable to overlook or to obscure the palaeontologically significant relationships ? Certainly *any* parameters will give *some* statistical results ; but do these particular parameters lead to palaeontologically significant results ?

It is my belief that the only basis for adequate palaeontological judgment is a thorough qualitative knowledge of the shell forms ; and that this knowledge should not only govern the interpretation of the statistical results, but should also determine beforehand which of all possible parameters are most suitable for systematic discrimination. I have no wish to disparage the use of statistics or to deny their usefulness in their proper place, but only to affirm their true status as a technical tool in palaeontology, and as methodologically subordinate to qualitative studies. The present fashion for statistics is undoubtedly beneficial in so far as it encourages quantitative precision wherever that is possible ; but it is pernicious in so far as it sometimes leads to disparagement of the unquantifiable element in the discernment of systematic differences.

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