NEW DIRECTIONS
The Director writes

In the last issue I wrote about the University Museum’s exceptional commitment to field research and listed some of the questions we have to examine in setting clear goals for the Museum’s research in the field in the years ahead. Here I want to start with the basic issue of what we should investigate and why, for as I wrote last time “our work has to be undertaken in the context of rigorously analyzed strategies which will allow us to apply our necessarily limited resources to the solution of precisely defined problems. The days of digging a site ‘because it’s there’ are gone.”

Over the last century or so we have learned a great deal about the past. In many areas we are now aware of at least the main lines of cultural development, and for some periods and some societies we know a good deal more. Elsewhere we are only now beginning to grasp the main outlines as for example in Thailand, thanks to Chester Gorman’s work at Ban Chiang.

Our first problem, then, is to define what we need to know next in the study of each area, culture, and period. It is not just a matter of asking the right questions, but of asking them in the right order. In this way each advance in knowledge leads on to and at the same time itself refines (provides a mid-course correction, if you will) the next question.

The problem is quite as much practical as it is theoretical: there are (usually, not always) many sites to choose from; there are only so many competent people to investigate them; there is only so much money. The best way to make the best use of manpower and money is to define one’s questions very carefully and then set out to answer them. Not that one should necessarily do this with the least possible expenditure of available resources, but rather with an expenditure appropriate in scale (be it small or large) to the nature of the questions being asked and to the effort required to get reliably based replies. A good deal of the digging that gets done is useless not so much because of bad methods or lack of publication (although both are factors) as because it is hopelessly inadequate in scale and scope to answer the questions asked. Such work often raises more questions than it solves, while providing incomplete or misleading answers to the problems it supposedly set out to address.

There are all sorts of other pitfalls. ‘Dramatic’, ‘famous’ and ‘productive’ sites tend to dazzle archaeologists quite as much as their supporters, yet sites are sometimes all three just because the investigator has asked the right questions, decided where they could best be answered, and proceeded to get the answers. There is no magic in ‘how did you know where to dig?’ and not much more than hard work behind ‘how did you know it was there?’

Perhaps the most difficult problem is who sets the questions to be answered and how. Another problem, at least as difficult, lies in the answers, and not only in the extent to which they will reflect what was asked in the first place. To these and other matters we shall turn our attention next time.

MARTIN BIDDLE