MEETINGS HELD 1981-82 (at the Institute of Archaeology)

7th October Helen Leach, 'Jigsaw or reconstructive lithic technology.'

18th November A.G.M. Paul Callow, 'La Cotte de St Brelade, Jersey.'

17th February Chris Clayton, 'The anatomy of flint: a close look at an old friend.'
Mike Pitts, 'Approaches to raw material studies.'

10th March (at Durham University)
Caroline Wickham-Jones, 'Techniques of flint knapping: a talk and demonstration.'

17th March Martin Hemingway, 'Aspects of industrial variation in the initial Magdalenian.'

12th May Alison Betts, 'Prehistory in the Jordan Desert.'

LECTURE SUMMARIES

THE ANATOMY OF FLINT: A CLOSE LOOK AT AN OLD FRIEND
by Chris Clayton

Flint has always been treated as a simple homogeneous solid for archaeological purposes such as thermoluminescence (TL) dating, electron spin resonance (ESR) investigation of heat treatment and microwear studies. More detailed examination however reveals almost all flints to be composed of at least three, and up to six distinct morphological and structural types of silica, each with distinct physico-chemical properties.

The basic structure of flint consists of a framework of silicified microfossils and small inorganically precipitated spherical aggregates of quartz, called 'lepispheres'. These are cemented together with a microfibrous quartz phase (chalcedony). Later during flint growth, this chalcedony may recrystallize to give a different (type 2) chalcedony and any remaining voids are infilled with a coarse blocky chalcedony (type 3) and quartz druse.

Most of the silica in flints, and particularly the chalcedony, shows considerable structural disorder and a high water content. It is to this disorder, and to impurity elements associated with it (e.g. Al), that many ESR signals are attributed, including the one which probably corresponds to the TL signal used in dating. On heating,
was found. Forty years later, vast amounts of data have been published by this and other imitative committees and individuals; the largest group of such data is at this very moment wending its way into print. In the original committee's first report, it was observed that 'in all scientific work, many years are occupied with the collecting of facts, after which inductive reasoning weaves these facts into generalisations?' (Keiller et al. 1941, 70). Well, the facts are here. Now the analyses can proceed. Or can they? I would argue strongly that this is not the case. Why should this be?

The answer, I suggest, lies in the nature of research. At the time of the formation of the South Western Sub-Committee the use of inductive logic was very much in vogue. Too much hypothesising or theorising, and one risked an academic rap on the knuckles. The value of deductive reasoning, that is the conscious framing of hypotheses that can be tested by the collection of appropriate data, has been emphasised by some archaeologists in recent years. Extremes, however, should be avoided. Productive research is neither obsessive fact-gathering, nor needless question-asking. Rather, there should be a continual process of questioning, data collection, revision of questions and perhaps generation of new ones, more data collection, and so on. It is this kind of process that has been conspicuously absent from so much of the archaeological petrology conducted in this country.

Specifically what seems to have happened is that the means to the original question, that is the petrological and archaeological sources of stone implements, rapidly became the ends. One reason for this is undoubtedly the circumstance that the question (concerned with prehistoric trade and economy) was, to put it bluntly, not well considered. Knowing that an axe comes from the Lake District is not sufficient equipment for the understanding of the exchange mechanisms that took it to Wiltshire; any more than the knowledge that a tyre in Solihull is made of rubber from India, can lead to an explanation of the rise and fall of the British Empire. One wishes to know why one stone resource was favoured over others; how easy the objects were to produce; how robust the products; and how well they worked or took a new edge. How clearly could potential 'buyers' distinguish between tools from different sources? More importantly, it has to be recognised that exchange cannot be treated in isolation from its social context. Competitive traders with axe-laden canoes are all very well; but there are endless other ways in which the axes could have been moved, and the information on which to base any rational selection of possibilities cannot possibly come from the scrutiny of stone objects alone. In fact, identifying and understanding exchange systems is not a petrological problem. In this sense, the committee's original goal was too ambitious. On the other hand, I would maintain that the form of data collection was not ambitious enough. What data are relevant? What are the questions that implement petrology committees should be asking?

One of the forces behind the growth of interest in petrolog-