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Expert systems and archaeology: what lies ahead?

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22.1 Introduction

What will be the impact of expert systems on archaeological science over the next decade? This question is not as easy to answer as might at first appear. The development both of artificial intelligence and expert systems and also of the use of mathematical and computer methods in archaeological science has been far from smooth. There is no reason to believe that the future will be more straightforward than the past. Any assessment requires consideration of the state of the art both in expert systems research and development and in archaeology.

22.2 The current expert systems scene

A good starting point is to ask the question: what *are* expert systems? The question is all the more appropriate since almost anyone with a computer program they wish to promote is tempted to call it an expert system. Strictly, however, an expert system is a computer program which uses non-numerical domain-specific knowledge to solve problems with a competence comparable with that of human experts (though not necessarily by the same methods). The significance of 'domain-specific knowledge' is that the program knows a lot about a little. It may be quite good at the diagnosis of some particular class of diseases of the liver, say, but have no competence whatsoever at anything else.

There are other typical properties of expert systems beyond their use of non-numerical knowledge about the task domain. Normally there is a clear distinction between the 'knowledge base' and the 'inference engine' which does useful reasoning using that knowledge—though this distinction is sometimes blurred by the use of 'meta-knowledge', that is, knowledge in the knowledge base which is about how the inference engine should do its job, rather than about the task domain itself. Expert systems are normally expected to be able to reason with uncertainties ('He's probably got malaria but I could be wrong') and also to justify and explain the conclusions they reach. No human expert will be happy to accept the decision of a computer program without understanding how it has been obtained.

Expert systems do not all do the same kind of job. For example, some interpret evidence, some generate designs or plans. Accordingly, there are a number of different expert systems architectures: for example, rule-based systems, frame-based systems, 'blackboard' architectures, and hierarchical non-linear planners. The relatively simple rule-based systems are the best known but may well ultimately prove the least powerful.

In fact, hundreds of expert systems have been built. But how well do they perform? The answer seems to be: not as well as might sometimes appear. It is only very recently that more than a handful of systems have found their way into routine service and for most of these their commercial viability remains unproven. Most of the systems described in the research literature did not get beyond the experimental prototype stage.

Not only are there many expert systems to read about (if not quite to use), there are also scores of 'tools' available to support their construction. These range from artificial intelligence programming languages such as Lisp and Prolog, through expert system shells such as SAVOIR and KES (that is, expert system frameworks with domain-specific knowledge yet to be added), to 'advanced' expert system building toolkits such as ART and KEE. But, of course, since the systems themselves are largely experimental and unproven, the tools offered for their construction are necessarily difficult to evaluate and compare.

Much current research on expert systems design concerns the difference between 'shallow' empirical knowledge which current expert systems can work with, and 'deep' knowledge which they cannot. Shallow knowledge refers to observed regularities which can be relied upon if not understood (my car will usually not start if the temperature is below zero). Deep knowledge involves understanding the causal mechanisms involved (my car will not start if the temperature is below zero because the formation of ice leads to...). Designing computer programs which can perform causal reasoning is not easy.

Jackson 1986 is a good introduction to expert systems studies, and Winston 1984 is a highly readable textbook on more general artificial intelligence techniques.

22.3 The current archaeological scene

Most archaeology is concerned with the exacting task of meticulous excavation followed by interpretation of the evidence obtained. This interpretation typically depends upon nothing more rigorous than everyday knowledge and the insights of cultural anthropology, but is often supported by specialised scientific work, for example soil analysis and radiocarbon dating.

However, over the past two decades there have been many attempts to make archaeological interpretation more 'rigorous' and more 'objective' by the use of a variety of mathematical, statistical and computer based methods. The more important broad categories of methods and techniques which have been deployed are the following (for details see Doran & Hodson 1975; Doran 1986):

- computer data bases, sometimes with on-site microcomputers;
- techniques for automatic classification such as *K*-means cluster analysis;
- combinatorial seriation techniques typically used for chronological seriation;
- multivariate scaling techniques such as Principal Components Analysis and Correspondence Analysis;
- spatial analysis;
- mathematical and statistical modelling; and
- computer simulation including Monte Carlo methods.

The total amount of experimentation has been substantial. Alas, there is a general feeling that the deployment of these formal methods has *not* delivered either substantial new archaeological insights or reliable tools for archaeologists to use. All too often the introduction of a technique has not gone beyond the initial 'look it can be used and does seem to be helpful' stage.

One current reaction to this disappointing state of affairs is merely to be less ambitious, to concentrate on low-level exploratory data analysis: graphical investigation of univariate and bivariate distributions and the like. However, one must surely ask the question: *why* have twenty years of experimentation with formal methods yielded so little real benefit?

An answer sometimes given to this question is that the difficulty lies in archaeologists' almost complete lack of expertise in the domain of, for example, multivariate statistics and the communication difficulties that arise when they attempt to enlist the aid of professional statisticians. But such difficulties seem to me at most contributory.

The heart of the matter, I suggest, is the lack of reliable sociocultural theory both at the 'micro' and 'macro' levels. By the former I mean explanatory understanding at the level of, say, the socioeconomics of farms or the cultural forces manifested in burial grounds. By the latter, I mean explanatory theory covering the emergence and long-term dynamics of social and cultural systems. Such theory must address, for example, the significance of the structure and belief systems of simple hunter-gatherer societies, the underlying causes of the emergence of chiefdoms as a social formation, and the reasons for the emergence and, sometimes abrupt collapse, of early states. At both levels there have been many attempts to build effective sociocultural theory including some drawing upon the mathematical and computational conceptual repertoire. But there is no agreement. There is not even agreement on the broad theoretical approach to be followed.

Such fragments of sociocultural theory as do exist are inevitably poorly related to the interpretation of archaeological data. This paucity of 'linkage' (or 'middle-range theory' as it is sometimes called) means that an archaeologist with excavation data to interpret must achieve this interpretation in the absence of any agreed and even half rigorous theoretical framework. He or she must rely upon everyday knowledge and uncertain anthropological insight. So it is hardly surprising that formal aids to interpretation make relatively little progress. Of those in use, most are 'model free' which in fact means that they embody weak general purpose models without specific theoretical content and so can at best identify abstract structure in the data which itself then requires interpretation. Alternatively, they employ explicit models which are at worst plainly inadequate, at best theoretically highly controversial.

22.4 The likely impact of expert systems

Given this background, what is the impact of expert systems on archaeology likely to be? I assume that there will be an impact. Indeed, it has clearly already begun (for example, see Huggett & Baker 1985; Doran 1986; Gardin *et al.* 1987); Baker, in this volume). But what form will it take and will it be beneficial?

I *hope* the impact will be:

- to make economic and social sense by automating a range of frequently arising, relatively straightforward and knowledge intensive tasks, thereby freeing scarce human expertise to concentrate on the more difficult problems;
- to shift attention away from weak general purpose techniques of statistical data analysis and interpretation to problem domain-specific non-numerical knowledge and its use in

data interpretation, examining at the same time the relationship of the former to the latter; and

- to provoke the development of more rigorous and formalised sociocultural and linking theory. This can happen not by transferring relevant theory from expert systems work to archaeology (what is needed is precisely the 'deep' theory which expert systems development itself lacks) but by challenging archaeologists to design expert systems knowledge bases incorporating aspects of sociocultural theory. Each time this challenge is accepted existing theoretical deficiencies will become all too apparent as the attempt is made to organise theoretical knowledge into a coherent formal structure capable of supporting computer-based formal inference.

It may be, of course, that agreed and substantial sociocultural theory is an impossibility. That might be the consequence of a fundamental inadequacy in the available evidence. Alternatively, the very idea of objectivity in social contexts may be misconceived. But it seems unduly pessimistic (and unscientific) to assume without trial that no significant theory can be formulated and agreed. And the tendency of expert systems work to provoke theory development has already been noted in disciplines other than archaeology.

But hopes are not expectations. I *expect* the impact of expert systems on archaeology to be:

- Another archaeological illustration of the 'Law of the Hammer': give a small child a hammer and he or she will immediately try to pound everything in reach with it. Moore & Keene 1983 have discussed the damage already done by the operation of this law within archaeology.

- A 'boundary dispute' between statisticians and computer scientists. Statistical inference and diagnostic expert systems are both concerned with interpreting evidence, and should be complementary. But given the difference in disciplines of origin, a degree of misunderstanding and rivalry seems inevitable.

- *Delay* in the development of rigorous sociocultural and linking theory caused by the ill-considered allocation of scarce archaeological computing expertise to the development of rule-based classificatory and diagnostic expert systems in contexts which can contribute little or nothing to theory formation.

Perhaps these expectations are too pessimistic. I hope so. In any case my remarks apply only to the next five to ten years. In the longer term I am convinced that artificial intelligence concepts can and will make major contributions to the achievement of archaeological goals.

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