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Expert Systems 85

Proceedings of the
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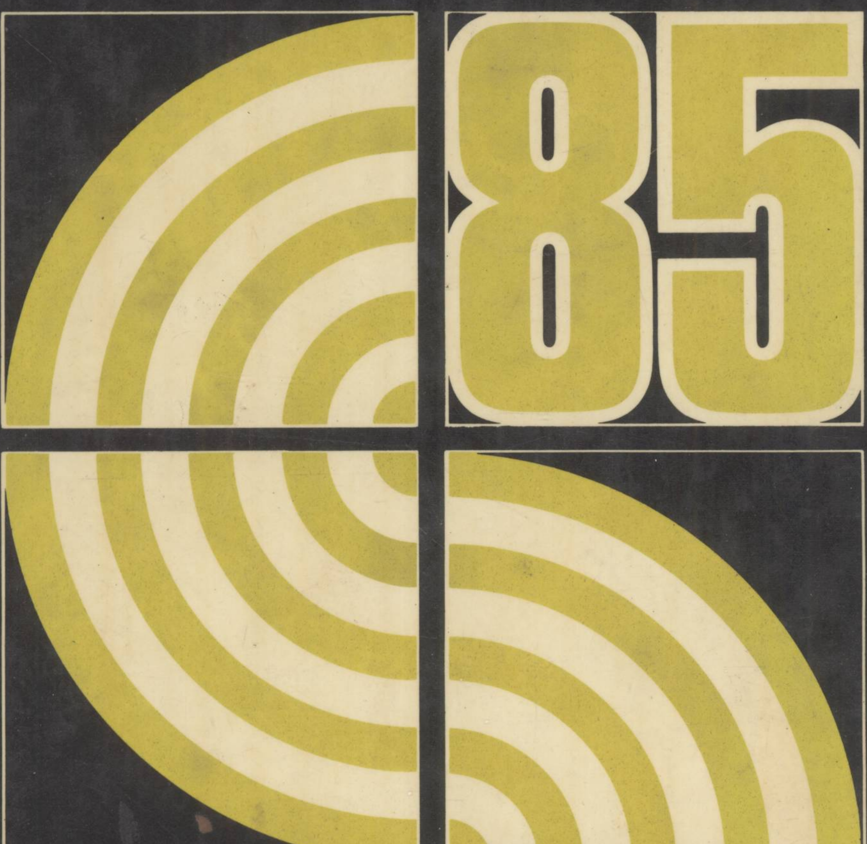

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University of Warwick, 17–19 December 1985

MARTIN MERRY
Hewlett Packard Laboratories

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Preface

This volume is the Proceedings of the Fifth Annual Conference of the British Computer Society Specialist Group on Expert Systems, held at the University of Warwick in December 1985. Following the precedent set in 1984, it includes an introductory paper written by the programme chairman.

The proceedings include all the refereed papers which were presented at the conference, together with the invited papers by Austin Tate and Abe Mamdani; papers from the other invited speakers were not available at the time of going to press.

I would like to thank all those concerned for their work in putting together the programme for this conference: in particular the members of the Programme Committee, and all those who refereed papers.

Martin Merry
Programme Chairman

Contents

EXPERT SYSTEMS - SOME PROBLEMS AND OPPORTUNITIES

Martin Merry
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INTRODUCTION

We are constantly being told that we are living in an electronic age; that we are undergoing the second industrial revolution; and that the information technology era is now upon us. Journalists describe the latest advances in computing in tones usually reserved for slow motion pictures of the Wonders of Nature or close up views of Halley's Comet.

Despite all this reverence, however, computer science is very young. It has roots in a number of different disciplines (engineering, mathematics, psychology, neurophysiology,...) and still forms a rather uneasy synthesis of ideas from these areas. Unsurprisingly, therefore, progress has been much greater in some areas than in others.

As far as hardware performance goes, exaggeration is scarcely needed. The last 30 years have seen 6 orders of magnitude improvement in hardware performance/cost. Expected lifetimes for new hardware products decrease constantly (as anyone who has bought a micro knows - if only you'd waited 6 months you'd have been able to buy something substantially better and cheaper).

In other areas of computing, however, progress has been rather less meteoric. This is due in part to the difficulty of the many problems that need to be solved, but is also due to delays in technology transfer from laboratories to general use. For example, think how long it took Pascal to emerge from universities into widespread use: even now, FORTRAN and COBOL still have a strangle-hold over many sectors of computing. Other examples now reaching a wider audience include: object oriented programming; logic programming; and the "overlapping windows" user interface paradigm. The typical gestation period for new ideas, languages, etc. appears to be between 10 and 15 years.

Expert systems are no exception. The well-known early expert systems, DENDRAL and MYCIN date from the late 60's and early 70's (Buchanan et al 69; Shortliffe 74), and yet the expert systems "boom" has really only appeared over the last two or three years. Most current applications work involves very few substantive new ideas over these early systems. Arguably, the most noticeable advances in current application systems are inherited directly from advances in the underlying hardware - systems can now be developed on reasonably sized hardware and run relatively quickly.

While all this is rather depressing from the point of view of wanting to see new ideas taken up quickly, it should at least make it somewhat easier to predict what's likely to happen in the next few years. In this paper we have a quick glance at three particular topics currently being explored in research laboratories: knowledge-based planning, new architectures for expert systems, and qualitative reasoning. This will hopefully show us what is likely to come into public view in the near future.

KNOWLEDGE-BASED PLANNING SYSTEMS

Most existing expert systems work in *analytic* domains, where problem-solving consists of identifying the correct solution from a prespecified finite list of potential answers e.g. fault diagnosis is concerned with identifying which potential fault is actually present. Many possible application domains, however, do not have this restriction: these domains are *synthetic* - problem-solving involves actually synthesizing a new solution. This is substantially more complex.

Over the past few years there has been increasing interest in building expert systems in these sort of domains, drawing on techniques from AI planning and expert systems. AI planning systems actually predate expert systems - significant work was being done in the mid 60's (e.g Doran & Michie 1966) - but the convergence of the two streams is relatively recent. Expert systems applied to synthetic domains necessarily use planning techniques; modern planning systems use significant amounts of knowledge from the application domain to help formulate their plans. A survey of knowledge-based planning techniques will be found in the review paper by Austin Tate (Tate 86) in this volume.

It is not only in synthetic domains that the techniques of knowledge-based planning are important for expert systems. The control of expert systems themselves is in itself a real-time planning task (Lesser 1984). This will arguably be the dominant use of these techniques as larger and larger expert systems are built.

In my own work on expert systems I have found that the large majority of problems that people have brought to me, looking for expert system solutions, have required knowledge-based planning techniques; I have regretfully sent most of these people away again because the necessary techniques just did not exist or were not sufficiently robust. This is slowly changing. More and more experimental systems are being built to tackle these sort of problems; the number of people working in these areas is steadily increasing. For example, the Special Interest Group on Planning, formed as part of the Alvey Expert Systems Research Theme, has gone from strength to strength over the last two years and now holds regular workshops. In this volume there are more papers on these sorts of systems than in any of the earlier proceedings of BCS Expert Systems conferences; we expect this number to increase again next year.

NEW ARCHITECTURES FOR EXPERT SYSTEMS

The traditional expert system architecture, consisting of a single *knowledge base* and an *inference engine*, is well-known. Within this framework there are many variations - different flavours of knowledge representation, uncertain reasoning, control strategies etc. - for a good analysis of a number of traditional expert systems see Johnson and Keravnou 1985.

Whilst a lot has been achieved within this framework, as more complex problems have been tackled, it has been found to be inadequate, and a number of variations have emerged. Probably the best known is the *blackboard* architecture, where the expert system has a number of distinct knowledge bases, each of which contribute to problem-solving by writing hypotheses etc. about the problem to be solved on a common "blackboard". This architecture was originally developed for the speech understanding system HEARSAY-II (Erman et al 1980).

In general, frameworks like the blackboard architecture are needed when different types of knowledge are used in an expert system, which may need, for example, different forms of knowledge representation. This leads to partitions in the knowledge base and then to different means of drawing inferences from the different types of knowledge. A number of systems tackling such problems have been built, leading to a host of different architectures (many of these are called blackboard architectures but have many differing features; others, like MOLGEN mentioned earlier, use rather different techniques). Different architectures have also been developed for many other reasons e.g. to cluster related pieces of knowledge (Aikins 83).

I have been involved in a number of feasibility studies for systems with the requirement to handle varying kinds of knowledge. Many problems fall into this category; these are not solved by simply putting knowledge into a conventional shell. Something more flexible is needed - today's toolkits are a short term response to the problem.

Like knowledge-based planning systems, we believe systems based round new architectures will become much more prominent over the next few years. However, many of these architectures are in a state of flux - it will probably be a long time before they stabilize. In many ways, this is because they lack any sort of formally defined semantics - they can be criticized as short term technical fixes to problems that need to be understood much better before longer term solutions can be found. The next topic we consider is one direction for research on such long term problems.

QUALITATIVE REASONING

The first two topics we have discussed both appear to be ideas whose time has come; work in both areas has acquired its own momentum and no great predictive skill is needed to see that these topics will become more and more influential. Qualitative reasoning, unfortunately, does not come into this category.

Most current expert systems only represent very shallow expert knowledge - a collection of fragments of compiled expertise. These, in general, form a small fragment of the problem-solving skills an expert brings to bear upon a problem. Typically, in addition to rules of thumb, the expert is able to reason about the deep structure of a problem. If the domain is sufficiently concrete, this reasoning may be completely quantitative e.g. the manipulation of particular mathematical formalisms. In general, however, much of this reasoning is qualitative, and is concerned, for example, with questions of causality.

We can illustrate this by means of a toy example. An expert system for fault diagnosis in a vending machine might have a rule like "IF you have inserted 10p AND a cup has come out AND there is no drink in the cup THEN hit the machine just to the left of the tea-no-sugar button". This rule is highly specific to a particular (model of) vending machine. If one wished to build an expert system to diagnose faults in an arbitrary vending machine, based round a model of the machine in question, it would need to be able to follow a chain of reasoning similar to "the coin is progressing through the machine...it has passed the mechanism which releases the cup...it has not reached the mechanism which releases the drink...it is therefore stuck at a certain place...this is immediately behind just to the left of the tea-no-sugar button...therefore hit the machine here".

Qualitative reasoning is concerned with capturing this kind of problem-solving skill. Slightly blurring the definition, it is concerned with all kinds of representation and manipulation of "deep" knowledge, and also "common-sense" reasoning. It is a problem that has been looked at throughout the whole of the history of Artificial Intelligence (e.g. see McCarthy 58).

Much work has been done; there are now expert systems based round qualitative models (Weiss et al 78; Mozetic et al 83). However, these systems were largely ad hoc; there is still no principled way of handling qualitative reasoning and many extremely hard problems remain to be solved (some argue that these problems are inherently insoluble e.g. see Dreyfus 1981).

Nevertheless, at least a partial solution to some of these problems needs to be found in order to realize many of the claims currently being made for expert systems. For example, one property of human experts is *graceful degradation* - if you ask an expert a question which is slightly outside his domain of expertise you tend to get an answer which is reasonable, if not completely correct; if you ask a similar question of an expert system you get garbage in response. [This of course is true of other computer systems - if you give a Pascal program to a FORTRAN compiler you don't expect it to produce almost working object code, but at least it produces a string of error messages - current expert systems don't "know what they know", and are as likely to produce wrong advice as no advice at all].

Qualitative reasoning, then, is an area in which much work is needed. We do not expect to see many application papers presented at Expert Systems conferences in the near future which are built round qualitative models - we do hope, however, to see more papers on theoretical work in this area.

CONCLUSION

In this paper we have briefly looked at some of the current work on expert systems which is likely to make itself widely felt in the near future. The first two topics, knowledge-based planning systems, and systems which use differing types of knowledge already have many exemplars and are likely to become of great commercial significance relatively quickly. The third topic, qualitative reasoning, is already making itself felt: it is conspicuous by its absence.

In the introduction to the paper we pointed out that a slow rate of perceived progress can be attributed to two things: the time taken for technology transfer, and the difficulty of many of the problems to be solved. While most of this paper has been motivated by the first of these reasons one must not underestimate the second. Qualitative reasoning is but one example of an extremely hard problem area where major theoretical breakthroughs are needed before we will see significant progress (others include: reasoning about time, a *good* way of handling uncertainty, reasoning with defaults, learning from mistakes, etc.).

One advantage of the expert systems boom is that these problems are receiving far more publicity than ever before, and that hopefully therefore more effort is being expended on them; a disadvantage is that expectations have been raised which will need some of these problems to be solved before they can be met. We must be careful not to assume that mere publicity is sufficient to solve these problems; mounting large scale projects which require unsolved research questions to be answered before they can be completed is rather unwise. Raising unjustifiable expectations in the early 70's led, amongst other things to the removal of much of the funding for work on AI in the UK - we must not let this happen again.

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