Reaction to an AI View of The Structural Learning Theory

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In reacting to a commentary, one always likes to begin by emphasizing similarities. In the present case, however, this was very difficult, since so much of what Drew McDermott says is based on an apparent failure to comprehend what structural learning is about. The inadequacy of Drew's analysis is illustrated on page one, where my technical use of the term "rule" is equated with "any program." This is totally at variance with my publications. While the rule concept has been refined somewhat over the years, the term "rule" always has been used to characterize a triple consisting of a domain, a range, and an operation or restricted type of procedure (e.g., Scandura, 1970, 1976, 1981). Thus, for example, the procedures of my rules are NOT equivalent to unrestricted procedures (as would be necessary were rules to be equivalent to programs). Put succinctly, they are structured procedures which do not allow recursion (e.g., Scandura, 1977, 1981); the role of recursion is fully incorporated in the hypothesized control mechanism. Indeed, it is this sharp division of labors which lends the theory its operational modularity and behavioral significance.

More generally, Drew has three main criticisms: 1) that the structural learning theory is "completely vacuous" because its control structure is so vague and general that some version of it is accepted by almost everyone in AI and cognitive psychology; 2) that my writings fail to deal with other people's problems, or to take a stand on "interesting" issues; 3) that my theory of education is too simplistic, and if applied would be dehumanizing.

According to Drew, the structural learning theory simply restates the assumption underlying AI and cognitive psychology, to the effect that computational models are useful. He further equates the structural learning theory with the hypothesized control mechanism (which he calls "cognitive structure"), and, as stated above, appears to believe (falsely) that I equate rules with programs. As empirical justification, he suggests that my demonstration experiments (originally outlined in Scandura, 1971, with the first series detailed in the Journal of Experimental Psychology, 1974) amount simply to showing
that children follow directions. McDermott is wrong on all counts: a) While computational models are useful, I personally have never believed them to be sufficient (e.g., see Scandura, 1970, 1973, 1977, 1981), b) As should be clear to anyone who has taken the time to study the literature in structural learning (rather than taking the intellectually lazy route of accepting "coffee swap" as fact), the hypothesized control mechanism is only one important but relatively small part of a highly integrated theoretical system. c) Finally, McDermott misses the whole point of the experiment (which is not surprising, since to my knowledge he has never conducted one).

The point of the experiment is to demonstrate that if experimentally manipulated lower and higher-order rules are in fact available to an organism (i.e., to his brain) and are independent of the organism's prior knowledge, and if the organism has the requisite encoding and decoding skills, then use of the manipulated rules in particular problem situations is TOTALLY explicable via the hypothesized control mechanism (e.g., see Scandura, 1971, 1975, 1978). This is a strong hypothesis, which is strongly supported by the experimental results. Moreover, the obtained results are NOT explicable via other types of control mechanisms which have been hypothesized, specifically not via production system-based theories in which individual productions are tested for use sequentially (e.g., Newell & Simon, 1972). (As with any other general purpose programming language, of course, one can use production systems to implement ANY kind of control mechanism— including that used in structural learning theories. As observed in Scandura, 1978, pp. 142–145, however, the language of production systems had its origins in mathematical logic and has limited heuristic power insofar as human cognition is concerned.)

There are other oddities in Drew's analysis. These, on the one hand, from falsely asserting that I equate people with their minds, to inferring the equivalence of the structural learning theory and what he calls the production system theory. (The former is a class of theories sharing certain common theoretical and methodological assumptions, whereas the latter refers to a class of theories having a quite different architecture, e.g., John McDermott, 1978, pp. 459–464). On the other hand, there is Drew's failure to make important distinctions such as that between formal equivalence (of computational models) and behavioral equivalence (e.g., their utility for operationalizing human knowledge).

Turning to his second point, Drew is concerned that the structural learning theory does not deal with "the really hard problems" in AI or "to react to the work of others." It may be true that I have not always felt obliged to comment on what some people in that field have felt to be crucial problems. Try as I might, I cannot do everything. But, then, what are considered to be important questions typically varies considerably over fields and schools of thought. Having never been at a loss to identify important questions, one wonders whether research in AI just might benefit from more informed attention to the questions raised and the answers obtained in structural learning.

To illustrate, Drew falsely states that the structural learning theory assumes two types of higher-order rules, idealized composition and generalization rules, on the one hand, and subject (matter) specific rules, on the other. This dichotomy is of his own making, and makes no sense to me at all, aside from the fact that the idealized portion has some parallels in mathematical logic. McDermott then goes on to some obvious statements about possible interactions among components of composite programs and the need for "debugging." I have no quarrel here.

I totally disown, however, Drew's suggestion to the effect that I believe there is a single composition rule. To be sure, some such exists in the idealized world of the mathematician, but not, I think, in the case of human intelligence. To wit: the purportedly large literature on the problem of composition, to which Drew refers (but does not cite), has at best indirect relevance to human behavior.

McDermott next extends his misconceptions to structural analysis, that particular form of cognitive task analysis on which structural learning theories are based. To imply, as Drew does, that structural analysis typically results in higher-order rules which derive SINGLE solution rules is totally erroneous. Even the instance he mentions is misinterpreted and taken out of context. (The point is that the same higher-order rules works for the square root of every prime number, not just 2.) It was reading points like this that made me wish I were not an editor, having to evaluate the paper's value.

Just as my doubts were at their peak, I came across an interesting (indeed, amazing) assertion by McDermott to the effect that "most AI workers would be extremely surprised if a simple learning mechanism turned out to be adequate." As support for his assertion, Drew invokes what he interprets to be the prevailing wisdom in AI — that learning programs NECESSARILY start with a large "innate" structure, from which only a "few tiny facts of anticipated form" can be added (i.e., learned).

Now that may be the best that Drew's friends have been able to do. But if that is the case, then I certainly would not advertise it. Although I would be the first to admit that major problems still exist, Wulfeck and I (Wulfeck, 1975; Wulfeck & Scandura, 1977) accomplished more than than that in our very first attempt to computerize a structural learning-type learning system. As regards the adequacy of this "simple" theory, and ignoring potential philosophical support associated with parsimony, I am tempted to paraphrase a famous scientist's remarks to the effect: If the theory is wrong, then one well-reasoned argument or study will suffice to prove it; but if it is right, then one thousand people saying it is wrong will not change it. (Incidentally, the control mechanism Wulfeck and I actually implemented is not fully modular, i.e.,
independent of the content rules, as required by the theory. Recent theoretical refinements (e.g., Scandura, 1981) provide a basis for resolving that problem but have not been implemented.

Another of Drew's criticisms stems from his failure to understand the crucial distinction between PARTICULAR structural learning theories, which involve specific content and student populations (e.g., “data structures” in computerese), and THE CLASS of structural learning theories taken as a whole. The latter refers to those characteristics (e.g., rule construct, control mechanisms, method of structural analysis) which are common to all structural learning theories.

Having failed to learn anything from what Drew had to say that was relevant to his own field, I was tempted to totally ignore his comments and distortions concerning education; but given our heavy involvement in computer-based instruction during the past several years, I feel obliged to comment briefly. Although working with mechanistic computers in an area called “artificial intelligence”, McDermott and others of his ilk apparently resist the purportedly non-humanistic idea that people's minds can be programmed, at least not in worthwhile ways.

Unfortunately, from Drew's point of view, rightly or wrongly, society has been programming people, both mind and body, for thousands of years — how else to explain the AI mentality espoused by Drew? Indeed, while I agree that restructuring existing knowledge (“debugging”) is an important (but hardly exhaustive) aspect of learning, I would simply remind Drew that “debugging” may be characterized, cognitively speaking, in terms of higher-order rules which operate on available but limited lower-order rules and which generate more adequate versions. (A rule may be inadequate, for example, in the sense that the associated procedure does not always generate a correct response for every element in the domain). Moreover, the method of structural analysis (e.g., Scandura, 1977, 1980, this issue) provides a relatively systematic means of identifying such higher-order rules.

Since so many of Drew's inaccuracies stem from his failure to grasp the central thrust of the structural learning theory, I would call his attention to Scandura (1980). Even cursory reading of that paper should make it clear, even to the most biased reader, that any cognitive theory which purports to be operational must necessarily share certain basic characteristics. Particular structural learning theories, of course, also must include specific rules of content, prototypic of given subject populations, and represented at desired degrees of behavioral detail. In effect, whether theories deal with rote rule learning, complex theories, motivational phenomena, or whatever, depends solely on the scientist's interests, and has nothing whatsoever to do with the scope of the structural learning theory per se.

As pointed out in Scandura (1980), among other places (e.g., Scandura, 1977), AI researchers rarely if ever concern themselves with tying their theoretical constructs explicitly to behavioral observables. Thus, while McDermott admits that people (brains) make plans, form theories, etc., via execution of programs, he indicates that "we (AIers) have little knowledge of what these programs are or how to get at them." The ability to get at these programs (more accurately, rules) is one of the major reasons rules are defined as they are (to allow for assessing individual cognitive potential). Also implicit in the type of thinking espoused by Drew is the assumption that cognitive models must reflect people's actual knowledge. In fact, this is an impossible goal for it would require complete assessment of a person's capabilities. It is necessary only to know enough (about cognition) to explain, predict, or (even) control behavior in the domain of interest. Diagnostic methods sufficient for this purpose are an integral part of the Structural Learning Theory — indeed, it is impossible to distinguish alternative adequate characterizations via observation of behavior within the domain. Moreover, where a domain is enlarged structural analysis allows for building on the existing analysis (i.e., set of rules).

In short, while I agree with the importance of specifying data structures and procedures, and while I have done much of such research in a variety of specific content areas, research in structural learning demonstrates that ALL data structures and procedures must satisfy certain important constraints if the associated theory is to be operational — and that adding detail beyond a point (as in realizing a cognitive model as a computer program) may be illusory with respect to the model's purpose. Many AI researchers seem not to be even aware of the need for such constraints, let alone being cognizant of them in their own research.

Finally, I would call attention to my commentary on AI microworlds and computer-based instruction (CBI) (this issue). While the immediate impetus for writing that commentary derives from our current involvement in CBI, it focuses on many of McDermott's concerns from a philosophical perspective, thereby complementing the above remarks.

Although it hardly needs to be stated, Drew has pulled no punches in commenting on the structural learning theory, and I have done likewise. While outright pugilism is not our goal, the above exchange illustrates the limits to which we are willing to go in encouraging reasoned and informed commentary. Further reaction on the issues raised, or on any other issue of contemporary concern, will be welcomed.

Footnote

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