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STIMULUS-RESPONSE THEORY OF AUTOMATA AND TOTE HIERARCHIES: A REPLY TO ARBIB

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This reply deals with Arbib's comments on the limitations of a stimulus-response theory of finite automata. The reply to his comments and criticisms are organized under five headings: (*a*) Matters of Proof, (*b*) Stimulus Traces and Mediating Responses, (*c*) Number of States, (*d*) Real versus Metaphorical Learning, and (*e*) Problem of Hierarchies. It is contended that a viable alternative to stimulus-response theory has not been defined by Arbib or others, and that explicit negative proofs of the systematic inadequacies of stimulus-response theory to account for complex behavior have not yet been given. The important constructive issue of determining how, in fact, complex learning does take place is emphasized.

Arbib's (1969) comments on my paper (Suppes, 1969) on the stimulus-response theory of finite automata raise in a somewhat different form issues that continue to divide cognitive and stimulus-response psychologists. I am not persuaded by any of his arguments that a viable alternative to stimulus-response theory has yet been defined, although I am willing to admit that a fully adequate stimulus-response theory of complex learning and behavior has yet to be developed. The point of my paper was to show how the theory of finite automata and the theory of TOTE hierarchies in the sense of Miller and Chomsky could be subsumed in a rigorous way within an explicitly and precisely stated stimulus-response theory. It seems to me that Arbib's comments and arguments can be analyzed under five headings. I shall deal with each in turn, and in several cases, will attempt to amplify my own views, as well as to deal specifically with what Arbib has to say. I think that the issues he raises are pertinent to making more definite the issues that divide stimulus-response and cognitive, especially linguistically oriented, theorists.

Matters of Proof

Arbib alleges that he has given a much simpler derivation of my main theorem: given any connected finite automaton, there is a stimulus-response model asymptotically isomorphic to it. He simplifies the proof by assuming that "*for each stimulus-response table there is a stimulus-response model that asymptotically becomes isomorphic* to it [p. 507]"; but as Bertrand Russell said long ago, such procedures of postulation instead of proof have all the virtues of theft over honest toil. In other words, Arbib has not shown how the fundamental result follows from simple general assumptions about stimulus-response connections—the main objective of my paper. To put the matter technically, he is faced with the problem of showing that his very powerful postulate, which corresponds to a theorem in my framework, is consistent with the simple assumptions about conditioning and sampling of stimuli that are an integral part of any standard stimulus-response theory.

Since it is not my main point in these remarks to deal with technical matters of proof, no more shall be said about these questions.

Stimulus Traces and Mediating Responses

Arbib seems to have a rather simple view of how an organism can remember its previous responses. He mentions the natural device used in experimentation with animals of having the previous response externalized by an appropriate stimulus. As noted in my paper, exactly this approach has been used in the conditioning of pigeons to behave as simple automata, but it is not a general approach. Certainly it is not adequate to the problems of language learning to which my own research is primarily addressed, or even to the problems of children learning arithmetic, as reflected in the detailed example given at the end of my paper.

Within classical stimulus-response theory that derives from Hull, for example, it is natural to

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talk about stimulus traces, and I mentioned this possibility in my paper. In terms of later stimulus-response theories, especially those associated with Osgood, Maltzman, Berlyne, the Kendlers, and others, it is natural to talk about mediating responses internal to the organism. Either of these concepts provides a natural framework to replace or to extend the very narrow externalization approach used by Arbib. Arbib objects to having some representation of the previous response in the organism for fear "that the input channels would be completely overloaded if they must carry not only representations of external stimuli but also complete information about the current state of execution of the TOTE hierarchy [p. 509]." This comment, like most of Arbib's other comments, however, is not backed up by a detailed analysis. It is not at all clear that this is a problem. Put in this general way, one may as well express concern over the internal processing also and simply say that it is found to be an overweening mystery of how the organism can work at all.

Number of States

In many ways I think the most serious and important issue raised by Arbib is the query of whether it is possible for an organism ever to have adequate time to learn the apparently large number of stimulus-response connections needed for it to become a suitably large automaton. This is a central issue that has been raised continually, particularly by psycholinguists critical of stimulus-response theories of language learning. The negative claims that Arbib makes concerning a TOTE hierarchy involving eight components with four states each are familiar, and his arguments also assume a familiar theological tone of saying it simply cannot be done.

Because of the many arguments I have had about these matters over the past couple of years, I have come to make a distinction between negative dogma and negative proof. A wide range of psycholinguists and cognitive psychologists have asserted the negative dogma that stimulus-response ideas can never account for complex learning, because suitable conditioning connections could never be learned in sufficient time by organisms exhibiting complex behavior. This negative dogma seems to be an article of faith, and not an article of proof on the part of almost all who assert it, including Arbib. Certainly he does not give a proof that a device with 4^8 states is needed for any cognitive processing he cares to define. In view of his familiarity with the formal literature in the theory of automata on these problems, I suspect he will be somewhat more wary than many of the psycholinguists in venturing to give what would appear to be a detailed argument. For an example of a presumed negative proof as opposed to a negative dogma, but what is in fact a melange of confusions from end to end, see the proposed proof by Bever, Fodor, and Garrett (1968) that a formal limitation of stimulus-response theory or associationism can be established.

It is generally recognized in mathematics and associated disciplines that negative arguments must be formulated in a more formal and explicit fashion than positive arguments. There is also a long history in mathematics and philosophy of establishing explicit and carefully defined systematic standards for evaluating the validity of a negative argument. It seems to me that Arbib does not work at all within that classical tradition. He makes a few casual statements about number of states, but offers no serious negative argument about the size of automata required for any cognitive processes understood well enough to be characterized in relatively exact terms. I will return to this point subsequently.

I think that the problem of the number of states required for various tasks is indeed a fundamental one, and I am certainly not prepared to establish small upper bounds on the number of states needed for a very large number of tasks. On the other hand, I am skeptical of the large claims so often made by psycholinguists or automata theorists. I mention just two examples. At the end of my paper, I gave a detailed analysis of the kind of automaton required for column addition of two numbers, and I showed how it can be related in realistic fashion to the actual behavior of students. The number of states required is just two. Casual thought without detailed analysis might lead to the suggestion that the number of states would need to be large. This is not the case.

My second example, which I will not give in detail, concerns current efforts to write probabilistic grammars for the spoken speech of 2- and 3-year-old children. Although I do not want to claim that these efforts are wholly successful, it is clear from scanning the length of sentences uttered by young children that the number of states required for production of the grammar is relatively small, and probably a larger number of states is required just for the handling of terminal vocabulary. I should add that I have yet to meet anyone, linguist or automata theorist, who would want to claim that a large part of terminal vocabulary can be acquired by any way other than some rather direct process of learning. I would conjecture that the number of states required for any cognitive task, including language comprehension or production, will turn out to be a lot smaller than any of us originally thought. I readily admit that this is simply a conjecture, but I have enough confidence in it to insist that those who wish to claim that stimulus-response theory is inadequate because of the problem of the number of states that must be conditioned should offer negative proofs and not simply negative dogmas. The central focus of my own research efforts is to convert my own positive dogmas into positive proofs. (On the question of number of states, it is perhaps worth noting that a universal Turing machine can be constructed with only seven states.)

A simple example will illustrate an important point in talking about the number of states. If we flip a coin 100 times, the number of possible outcomes is 2^{100}. Each of us can perform this experiment, but we could set the entire population of the world to flipping coins and not approach the number of possible outcomes. The number of *actual* states in a process, it is essential to note, is usually incredibly smaller, by orders and orders of magnitudes, than the number of possible states, and so it is with the relation between the number of actual and the number of possible conditioning connections.

Real versus Metaphorical Learning

Arbib argues in several places that learning simply could not take place according to stimulus-response conceptions because the number of states required is too large and the amount of time needed for conditioning is too restricted. I have already expressed my central argument against his claims about the number of states. I am willing to agree that if the number of states that must be conditioned individually is very large, then there will not be time for the task. What is interesting is the vagueness and weakness of the alternatives considered by Arbib. He expresses clear preference for TOTE hierarchies without considering these hierarchies as special cases of stimulus-response models, according to the first corollary of my main theorem. What he does not say or even sketch is how, within the framework of TOTE hierarchies, a theory can be given of the organism's acquiring cognitive skills. He does mention casually some references to neurons and biochemical changes in the brain, but no serious ideas are set forth in this physiological framework. The reader is left totally uninformed as to what serious alternative Arbib would propose, with the single possible exception of his final remark about the computer as a metaphorical brain. Presumably, metaphorical brains engage in metaphorical learning. If this is what he is suggesting as a theory of learning, he cannot mean for his remarks in this direction to be taken seriously. An account of how organisms learn must be stated with sufficient definiteness and in sufficiently nonmetaphorical terms for it to be tested experimentally. The literature of artificial intelligence over the past decade shows clearly enough that computer theorists have as yet a poor idea of how to think about learning, or in particular, how to solve any special problems of learning in terms of computer hardware or software. Certainly Arbib himself makes no positive or definite suggestions of how to deal with any of the main difficulties facing theories of machine or human learning.

Indeed, it is surprising that in view of his opinions about the adequacy of psychological theories of learning he does not have more to say about the virtues of computer theories of learning. I think the reasons, however, are really clear enough. It does not take a very serious or systematic study of the literature of artificial intelligence to reach the conclusion that, although psychological theories of learning are certainly far from being sufficiently developed, so are theories of how computers should be programmed to learn.

Problem of Hierarchies

I have stated already that the issue about the number of states raised by Arbib is indeed a serious one. I think the other serious issue raised by him is the problem of hierarchies. It seems to be a matter of belief on the part of almost all cognitively oriented psychologists that conditioning theories must treat each simple conditioning connection as separate and equal. The concept of a hierarchy, it seems to be suggested, is contrary to the spirit of stimulus-response ideas. That a clear theoretical counterexample exists is shown at once by the corollary to the main theorem of my paper, namely, the theorem that every TOTE hierarchy in the sense of Miller and Chomsky is isomorphic to some stimulus-response model at asymptote. The abstraction of this result may be unsatisfactory to some readers, but its meaning is clear. Any intuitive hierarchy that may be represented formally by a TOTE hierarchy, a finite oriented graph, a finite tree, or a finite automaton may be represented just as well by a stimulus-response model; in fact, better, for the stimulus-response model also includes an account of how the hierarchy is learned if the stimulus-response connections are not already coded in the genes.

Arbib concludes by stating that my results should not be taken to show that the concepts of intention, plan, and purpose can be subsumed under the central concepts of stimulus-response psychology. He does not challenge the formal result about TOTE hierarchies just discussed. He does challenge the stimulus-response account of how conditioning connections are built up in actual fact.

My own conclusion is this: What he has given us is negative dogma not negative proof, and he has not stated a viable alternative to stimulus-response theory. I join with him in acknowledging the central issue raised by the number-of-states problem. The future task for stimulus-response theorists is evident. We must show in detail, for complex substantive examples, just how learning can take place according to stimulus-response ideas.

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