The Journal of Mind and Behavior
Summer 1993, Volume 14, Number 3
Pages 215–222
ISSN 0271–0137

Reply to "The Ability of the Sweeping Model to Explain Human Attention"

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This is a reply to Weinfurt's article (1993, this issue) examining the Sweeping Model. Overall, our positions are not as incompatible as they may seem, although I feel that his conclusion, that the Sweeping Model cannot explain human attention, does not follow from his comments. I will proceed through his article and clarify issues as they arise. Our difference of opinion may result from differing goals, with Weinfurt being concerned with more abstract aspects of cognition, and myself with basic perception and how it may be achieved before proceeding to the more abstract.

This is a reply to a critique (Weinfurt, 1993) of the Sweeping Model (Christ, 1991a, 1991b, 1992). The critique views the material from an interesting perspective, making reference to Logical Learning Theory. I do not agree with the conclusions, so I will proceed point by point through the critique to clarify issues as they arise. I am not an expert on Weinfurt's school of thought, but our differing outlooks on the subject may not be as incompatible as the critique seems to suggest.

Weinfurt begins by saying that, in the Sweeping Model, "the system periodically inhibits some environmental signals from exciting the PDP network while amplifying other signals" (p. 207). This is not what was stated in the Sweeping Model (Christ, 1991b). The sweeps were identified as "lateral inhibition," meaning that units inhibit surrounding units in a competitive way. Nothing is amplified during a sweep except by disinhibition when a unit wins over neighboring units.

I would like to thank the Natural Sciences and Engineering Research Council of Canada, and the Ontario Ministry of Colleges and Universities for their support in this work. Requests for reprints should be sent to Gregory J. Christ, School of Psychology, 125 University, University of Ottawa, Ottawa, Ontario, Canada, K1N 6N5.

There may have been some confusion of terminology, with Weinfurt "interpreting 'computer simulation' as concerning AI, and 'psychological theory' as concerning human cognition" (p. 207). The terms "computer simulation" and "psychological theory" simply refer to the use of a simulation to test a psychological theory. These terms are not meant to designate separate domains. One may not agree that simulation on a computer is useful for testing a psychological theory about humans, and this may be the point that the commentary attempts to make.

It is stated that one of my "underlying assumptions is that human cognition is the same as artificial cognition" (p. 208). I am making an assumption that aspects of human cognition can be modeled in a simplified form, in order to understand how neurons in networks could possibly behave as humans do. Human cognition and "artificial cognition" could be identical if you built an exact model of a human (brain and all), unless you hold a "carbon chauvinist" position. The trick would be to have the model accomplish cognitive tasks in the same, or in a very similar way, to how the human does. Starting with similar hardware (that is, artificial neural networks) seems to me to be reasonable. However, these are just starting points.

Concerning the usefulness of "artificial intelligence" (AI) models, Weinfurt later refers to Rychlak (1991). Rychlak's work deals with traditional AI concepts, only briefly mentioning (but not examining) artificial neural networks. Some of Rychlak's criticisms of traditional AI are the same ones that led me to decide to use a neural network approach. For a basic overview of the history and properties of neural networks, see Anderson and Rosenfeld (1988).

The question is posed: "Could data that are already construed in information-processing terminology ever really contradict an information-processing philosophy of the mind?" (p. 208). Why not? The worth of a theory can be measured by its testable predictions, and ultimately by its practical applications. Therefore, if all the predictions of this model (phrased in whatever terminology) test to be false, it would contradict the underlying philosophy of mind, showing it to be not useful.

The example of two people seeing a Gestalt figure in different ways is used to show that there are problems in postulating information in the environment from which attention selects a subset. I feel this example actually supports my position about attention selecting from information "out there." The environment (signal) "out there" is the same for both, but two different perceivers, with different brains and different histories of learning would almost certainly process the information differently. The organization of their connection strengths would be dissimilar, so accordingly they would perceive somewhat different scenes. Flipping a figure (for example, seeing one face of a cube as the front or the back) could be seen as oscillation between two mutually inhibiting representations that share certain parts.

Intentional flipping would be top-down influence (activity coming from another source in the system) giving one representation an advantage over the other.

Weinfurt does not agree with my use of the term "information," saying that "environmental signals do not become information until a person endows them with *meaning*" (p. 208). Perhaps I should replace the term "environmental information" with "potential information in the environment." My position is that there are patterns of energy "out there" that have varying degrees of regularity; we perceive only a subset of the energy that is "out there," from which our experience is constructed. (I still have not decided if matter is a construct resulting from perceived energy or is something really "out there"; in any case, "matter" is a useful way to describe regular patterns of energy "out there".)

The question is also posed: "Regarding Christ's schematization of the attentional process, why does the environment initiate the process?" (p. 208). The diagram (Christ, 1991b, p. 349) clearly shows the PDP network connecting to a response box, which has an arrow leading back to the environment. In the text, an example of a response is "motor activity." Thus, the diagram shows that the environment is not the start of all activity, but that there is a complete circuit with the system operating on the environment, as well as receiving input. I included the response capability because without this faculty, learning is very restricted, even trivial, with no means of expression (for example, imagine an organism that could not even turn its eyes or change location). Thus the conclusion that the Sweeping Model is based on an inadequate concept of attention does not follow.

The Sweeping Model is criticized as mechanistic, with environmental stimuli taken as signals which are "processed in a Lockean manner" (p. 209). I agree that the Sweeping Model is mechanistic, but am not sure about the "Lockean" label. I do presume an "out there," but as my description of the model shows, the network is not a complete "tabula rasa," and has certain preset conditions in it (see Christ, 1991b, p. 360). Weinfurt describes Lockean as "inputting simple ideas" (p. 209). The Sweeping Model's input is made up of firing rates of sensory receptors, perhaps described as microfeatures of the environment; the original input would have to be significantly processed to lead to representations that could be termed as "ideas." Therefore claiming that the Sweeping Model accounts for simple "signal-chunking" (manipulating pre-formed symbols?) as a computer does, misses the point that its representations would be distributed, overlapping, and constantly changing, unlike symbol manipulating computers. The model should not be confused with what it is being modeled on.

This mechanistic approach is said to emphasize flow and activation of signals "instead of the logical rendering of meanings" (p. 209). How should we

define meaning? Does a nerve impulse have meaning? When does meaning occur? I tried to keep "meaning" in terms of a pattern of activation in the neural network being associated with a type of state in the environment. A pattern's "meaning" could be determined solely in terms of which related representations the activation flows to once a representation has been activated (that is, learned memories and responses on the environment). This is not very different from Rychlak's (1988, p. 324) definition of meaning, since the system's learned representations and responses could also be termed to have "purpose" as much as a person's if they behave in a similar way in that environment. For example, X sees a Y, and X has learned to run from Ys because they aversively increase X's sweep frequency. Y information could be termed to mean (purpose) "aversion," "run" or similar things, but would exist only as certain patterns of activation in X's neural network causing a fleeing response. X may even output information about "wanting" to get away from Y.

Weinfurt asserts that humans "cannot be conditioned, in either the operant or classical Pavlovian sense" (p. 209), unless they are *aware* of the experimenter's intended association between stimuli *and* they cooperate. In the Brewer (1974) chapter that is quoted in support of this claim, I must agree with a comment from the audience: "Your title says there is no evidence of conditioning in adult humans. . . . However, I am not sure that you have succeeded in winning me over to that point. First, it doesn't seem to satisfy the operational definition which you describe: There *is* conditioning" (p. 60). In fact, the data that Brewer reviews show that there is conditioning, but that it can be influenced by verbal information, and that subjects can sometimes determine and articulate what the experiment's contingencies are. There is also the issue of subliminal responding during which adults are *not aware* of the stimulus *or* response (for example, Corteen and Wood, 1972).

Classical and operant conditioning in adult humans are certainly not as straight-forward as in lower animals, but there is ample evidence that they occur. A major goal I had with the Sweeping Model was to find a mechanism to account for behaviorist findings, which are powerful in certain situations, but also to extend this into the realm of more cognitive activity, where the standard behaviorist view falls apart. I proposed how this may happen (Christ 1991b, pp. 362–365). Any pattern of activation can take on the role of a reinforcer; even a verbal instruction could influence or over-ride (top-down) other reinforcers that an experimenter might expect to control a response. The system may even form a representation of the contingencies in an "experiment"—then this representation could influence the original flow of activity in any number of ways.

The next issue concerns questioning the value of PDP models, with the merits of a model of learning irregular past-tense verbs as an example. This particular model (Rumelhart and McClelland, 1986) is quite different from

the Sweeping Model in goal (very specific language task), and structure, and is criticized on language grounds. Whether this language model achieves or fails at its particular goal does not change the more general and established properties of PDP neural networks which are all the Sweeping Model requires (see Christ, 1991b, pp. 348–349), and which are still accepted as far as I know .

Following this criticism of PDP aspects, there is an attempt to classify the Sweeping Model as *mediational* as opposed to predicational. However, I have already noted that stimuli enter the system as firing rates, or microfeatures, which are not "already formed" concepts. This would appear to make the Sweeping Model a "predicational" model, although I am not sure if this is helpful or not. As I outlined my view of "meaning" above, the quote Weinfurt uses about meaning, and needing to frame concepts oppositionally (Bugaj and Rychlak, 1989), may be irrelevant (and it already presupposes some way to do the basic perceiving). How do these oppositional concepts come into the mind? Is the environment bipolar enough in nature for this to be a useful mode of perception? The question to me is not so much how to determine if "Bob is friendly," but how to determine if Bob is "out there" in the first place.

Weinfurt makes a reference to direction of thought being determined by a person's predications, rather than by frequency or contiguity of stimuli. In the Sweeping Model, direction of thought was explained (Christ, 1991b, 1992) as the content addressable spread of activation along the strongest connections (strengthened by previous association) from representations to other representations—the latter ones may be called "targeted referents" if that helps. Content addressable spread of activation is similar to Rychlak's (1988, p. 324) concept of "meaning extension."

Weinfurt then says that predicational and PDP modelling are equally abstract bases for theorizing about cognitive functioning. Admittedly PDP is a simple approximation of the nervous system, but it seems a logical starting point. Philosophical theories are good for generating possibilities. However, the bottom line is that all the cognitive processes described by behaviorists, cognitive psychologists, philosophers, or biologists et cetera, must all be accomplished using (as a minimum) the human body with its nervous system and the environment in which it lives. There may be any number of undiscovered factors that influence these, but I think we should first start working with these minimum requirements.

Weinfurt feels that I *must* invoke a homunculus "to explain the more volitional aspects of attention" (p. 211). The Sweeping Model accounts for volition in terms of prior activation in the network. The prior activation has top-down influences on selection of relevant information, and comes from the ongoing flow of activation that is constantly going through the network. As was previously indicated (Christ, 1992), this flow continues even in the

absence of environmental input, and a state of disconnection from the environment (like sleep) would even be required. Also, the network would be large, such that the input units would be only a small part of the total state of activation; there would be a lot of activity independent of environmental input that would involve flow between already established representations (memory), some connecting with current input, and some not (a "stream of consciousness"?). "Intentionality" would be the top-down influence of this stream of ongoing activity, with no new subsystem being needed.

The evaluation systems of the Sweeping Model are questioned as unclear, but have been outlined in Christ (1991b, p. 360). There are likely separate positive (+) and negative (–) body state evaluation systems, which are prewired to detect "very general sensory input parameters (for example, very intense stimulation)" [p. 360]. Here I postulate that there must be certain innate knowledge, for survival value, of very approximately what is good and bad for survival purposes. Very generally, "good" would be satiation states (dry, warm, full stomach), and "bad" would be damage to the organism (strong energy in any sense modality could cause damage). I find this quite plausible since neonates exhibit such knowledge.

It was also outlined (Christ, 1991b, p. 360) how the evaluation systems lead to attentional shifts. A diffuse burst of activation, related to the evaluation systems, would disrupt ongoing activity, and this uniformly random activity that results would allow a fresh relaxation on the current environmental input to begin, regardless of what previously held the focus. No new mechanisms or homunculi were required.

I would like to add that this diffuse burst of random activation should probably be centered closer to the sensory part of the network, so that the ongoing internal pattern of activation is not excessively interrupted; a complete disruption of activity would cause a discontinuity in the whole system that a shift of attention does not entail. Only the connections to the environment should be strongly wiped and refocused, with the top-down influences only slightly dampened (so bottom-up could direct shift) but still having an effect (so top-down could direct shift). There would be an interplay between the intensity of the bottom-up versus the top-down activity (for example, a very loud sound may over-ride top-down influences; a very focused memory may over-ride bottom-up environmental information). The quote from William James that Weinfurt uses to support his predicational view I feel aptly describes the top-down aspect of the Sweeping Model and why it is necessary for shifts of attention.

My point about using "intrinsically ignorant and mechanical devices" to simulate human cognitive processes was not meant merely to limit theorizing to the level of artificial intelligence, as Weinfurt suggests (p. 212), but to provide a method for theory testing. By using ignorant mechanical devices to

simulate a theory, to test if predicted outcomes really occur, you are forced to rigorously specify the details of your theory. Pure speculation may be a step in the right or wrong direction, but you have no way of knowing which direction you have gone. If one has a good explanation of human functioning, it should be possible to implement a model in an ignorant machine; if one cannot, perhaps the "explanation" was somewhat empty after all. In the interest of falsifying unsound theory, computer simulation can be a useful approach. This does not apply if your philosophy of mind invokes intangible, unmeasurable, unverifiable factors, in which case your philosophy will likely be useful only for "explaining" things after the fact.

General Comments

After examining the arguments raised in this critique (Weinfurt, 1993), I feel that the conclusions drawn do not follow, and that little in the Sweeping Model was shaken. Some of the quoted data even support my view, which suggests that our positions are not overly incompatible. There may be simply a difference of goal, in that Weinfurt's interests seem to be more descriptive and concerned with abstract aspects of cognition (for example, personality attributions) that presuppose how the basic perception was accomplished in the first place. My goal was more to begin with basic perception and how it may be achieved by an animal or human, then to proceed to the more abstract. The Sweeping Model has yet to be tested by implementation, so it remains in the realm of speculation, but it leads to and seeks testing. Logical Learning Theory takes evidence and verification seriously (Rychlak, 1988, chapter 11), so I would think that the test of computer simulation would be seen as an asset. Speculation without regard to testing leaves theory grouped with folklore and fiction: Which version is the correct direction?

References

Anderson, J.A., and Rosenfeld, E. (Eds.). (1988). *Neurocomputing: Foundations of research.* Cambridge, Massachusetts: MIT Press.

Brewer, W.F. (1974). There is no convincing evidence for operant or classical conditioning in adult humans. In W.B. Weimer, and D.S. Palermo (Eds.), *Cognition and the symbolic processes* (pp. 1–42; discussion pp. 57–61). Hillsdale, New Jersey: Lawrence Erlbaum Associates.

Bugaj, A.M., and Rychlak, J.F. (1989). Predication versus mediational modelling and the directedness of cognition in impression formation. *The Journal of Mind and Behavior, 10,* 135–152.

Christ, G. (1991a). Toward a model of attention and cognition using a parallel distributed processing approach. Part 1: Background. *The Journal of Mind and Behavior, 12,* 247–262.

Christ, G. (1991b). Toward a model of attention and cognition, using a parallel distributed processing approach. Part 2: The Sweeping Model. *The Journal of Mind and Behavior, 12,* 347–366.

Christ, G. (1992). Toward a model of human attention and cognition using a parallel distributed processing approach. Part 3: Consequences and implications of the Sweeping Model. *The Journal of Mind and Behavior, 13,* 137–156.

Corteen, R.S., and Wood, B. (1972). Autonomic responses to shock-associated words in an unattended channel. *The Journal of Experimental Psychology, 94,* 308–313.

Rumelhart, D.E., and McClelland, J.L. (Eds.) (1986). *Parallel distributed processing: Explorations in the microstructure of cognition*. Cambridge: MIT Press.

Rychlak, J.F. (1988). *The psychology of rigorous humanism* (second edition). New York: New York University Press.

Rychlak, J.F. (1991). *Artificial intelligence and human reason: A teleological critique*. New York: Columbia University Press.

Weinfurt, K.P. (1993). The ability of the sweeping model to explain human attention: A commentary on Christ's approach. *The Journal of Mind and Behavior, 14,* 207–214.