

Letters

■ Editor:

Thank you for the opportunity to respond to the letters by Jim Hendler, James Herbsleb and Mike Wellman regarding my survey of the Eighth National Conference on Artificial Intelligence (*AI Magazine*, Volume 12, No. 1). The letters raise many interesting points that can be roughly classified as questioning the validity of the survey and questioning the proposed MAD methodology. Mike Wellman says, "As Cohen acknowledges, a serious problem with the survey is that the AAAI conference proceedings do not accurately represent the field." Actually, I did not acknowledge that AAAI is not representative; I just raised the possibility. Wellman evidently believes it and, further, attributes it to "the general constraints of the conference forum and the length of proceedings papers." But surely Wellman would agree that it requires fewer than six pages to say what one learned or tried to learn in one's research? Only 17% of the papers in AAAI did so. The reason AI researchers say little in six pages isn't the page limit (note that scientists get by with just a few column inches in journals such as *Science* and *Nature*), it is that they are telling us the wrong stuff. To paraphrase David Etherington, AAAI papers tell us what was done, not what was learned. Six pages may be too little space for the former but it is ample for the latter.

Wellman questions my principal result that AI research is either model-centered or system-centered with almost no research in the intersection. He says that much more work is in the intersection but that, due to space limitations, only one aspect of a research project can be reported in AAAI. I found that only eight papers of 150 reported both model-centered and system-centered aspects. I have three reasons for believing that the other 142 papers do not, in fact, represent work in the intersection of model-centered and

system-centered research. First, the split between the neats and scruffies is old and institutionalized, as Hendler points out. Few researchers are trained in both camps. Second, the pathologies that researchers in AAAI-90 themselves attributed to focusing on just one aspect did, in fact, arise. Third, the expected advantages of merging systems and models did, in fact, materialize (although the sample was very small). Wellman says that just because I didn't see system-centered and model-centered research reported together, doesn't mean it wasn't there; I say that I found pathologies (e.g., see my sections Models Without Systems and Systems Without Models) that strongly suggest it wasn't there.

The issue is important because I base the MAD methodology on the assertion that system-centered research needs models and model-centered research needs systems. Neither Wellman nor Hendler disagreed with this assertion but both responded vigorously to what they perceived to be the "folly" of MAD: "Indeed, it is not clear to me that it would be a desirable end for all or even many AI researchers to be performing both types of research" (Wellman). And, "If Cohen is taken (overly) seriously, then every major AI researcher will have to... be conversant with theoretical tools (math, logic, etc.), experimental methods (testing theory, statistics, etc.), and implementation strategies (blackboard architectures, real-time systems, etc.)—a pretty tall order" (Hendler). Well, we have tried dividing AI research among the theorists and the system builders, and my survey suggests this division does more harm than good. Hendler's "tall order" is not actually so tall, especially if you think, as I do, that it could revitalize our research. Wellman and Hendler both call for collaborative efforts between model-centered and system-centered researchers, but this hasn't worked, and it won't, because neither group cares much about what

the other does. No theorist is going to spend his or her time attempting to bring precision to a mess of hacks, kludges and "knowledge," and no system builder is apt to find the attempt informative. MAD does not mean business as usual with occasional collaborative meetings.

MAD means assessing environmental factors that affect behavior; modelling the causal relationships between a system's design, its environment, and its behavior; designing or redesigning a system (or part of a system); predicting how the system will behave; running experiments to test the predictions; explaining unexpected results and modifying models and system design; and generalizing the models to classes of systems, environments and behaviors. The criterion of success is the ability to predict behavior at some level of accuracy useful to designers. These are the basic activities of any engineering science, and they will be accomplished not by collaborations of researchers with completely different ideas of what is interesting, what is evidence, what is a result, and so on, but by individuals who want to do engineering science.

In response to James Herbsleb, let me say that MAD is voluntary, as is the methodology he describes as an alternative. His gibe notwithstanding I present MAD with no more hubris than he and his colleagues present SOAR. That is, I am enthusiastic about MAD because it helps me answer the research questions that interest me, and I hope, as researchers in the SOAR project hope for their methodology, that MAD will prove generally useful.

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■ Editor:

The article by David West and Larry Travis in the Spring 1991 issue will no doubt stimulate some healthy debate about the status and use of the mind/computer metaphor in AI. The authors relied heavily on the analysis of metaphor proffered by Earl McCormic, and his distinction between epiphor and diaphor is useful in the analysis of metaphor in science. Readers of *AI Magazine* should be apprised, however, of other works on metaphor which may help clarify some of the issues.

For one thing, there exists a considerable literature of experimental research on metaphor comprehension which bears on the claim that metaphors can be described as similarity statements. Suffice it to say that the scaling of the fruitfulness of metaphors by means of simple metrics of the features that are shared by the terms in a metaphor is not so straightforward as West and Travis imply.

Furthermore, the distinction between diaphor and epiphor is rather slippery, and the slipperiness is compounded by West and Travis' additional concept of "paraphor." Some traction on these distinctions may be found in the work of linguist George Lakoff, and especially his discussion of the differences between metaphors and "metaphor themes." Illumination can also be found in philosopher Stephen Pepper's discussion of the mechanist, formist, and contextualist metatheories.

Many of these concepts and issues can be found in the pages of the new journal, *Metaphor and Symbolic Activity*.

Apart from overlooking the large, relevant corpus of work on metaphor and on metaphor in science, the West and Travis article is refreshing in that it does not assume the tired old positivist stance which asserts, basically, that "if it's metaphor, and if it's science, then it's bad science." Metaphor analysis can be a useful, if not essential, tool in the analysis of scientific reasoning (and reasoning in general), and the West and Travis article will go a long way in stimulating healthy debate.

One final caution: Explaining metaphor by saying that metaphors are "merely" or "only" analogies or statements of similarity is not an explanation of anything. If I may use an analogy, it is like explaining a person's bizarre behavior by saying

"He's schizophrenic"—it's a description, not an explanation. Analogy is a particular format for laying out semantic relations. As such, it was intended to describe precisely the kinds of semantic relations that can be involved in metaphor. Hence, analogy can be used in post-hoc descriptions of particular semantic relations (e.g., similarity of features) which an interpreter has abstracted from a metaphor. But analogy is not the egg, and metaphor is not the chicken.

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■ Editor:

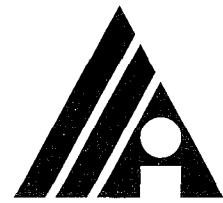
Could, as his article reprinted in the summer 1991 issue states, Marvin Minsky really wonder what caused the misconception that neural networks and AI are conflicting activities? If so, he hasn't read the many attributions of the conflict to his and Seymour Papert's 1969 book *Perceptrons: An Introduction to Computational Geometry*.

Because of that book's influence, say Maureen Caudill and Charles Butler in *Naturally Intelligent Systems* (p. 171, MIT Press, 1990), "neural network research and development was brought to a near-standstill for almost two decades."

Papert knows where the blameful fingers are pointed: In "One AI or Many?" (*Daedalus*, Vol. 117, No. 1, Winter 1988; also *The Artificial Intelligence Debate*, MIT Press, 1988), he uses the metaphor of natural and unnatural sisters. In the early sixties, he says (p. 3), "The artificial sister grew jealous and was determined to keep for herself the access to Lord DARPA's research funds. The natural sister would have to be slain. The bloody work was attempted by two staunch followers of the artificial sister, Marvin Minsky and Seymour Papert..." Papert admits to formerly feeling "some hostility" (pp. 4, 5), and goes on to summarize his continuing misgivings about neural nets.

I found Minsky's article interesting, despite what I took to be a disingenuous opening.

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