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ISSUES IN COMPUTATIONAL MODELING: INTRODUCTION

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Our goal for this special issue was to examine the impact of formal and computational approaches on psychological research. Although some form of computational metaphor is often accepted in cognitive psychology, there is no consensus regarding the importance, usefulness, or possibility of formal or computational modeling. While some voices enthusiastically support modeling (either formal or computational), other voices have raised serious doubts and expressed them under various forms. Some authors will go so far as to argue that modeling is premature and that the data do not allow a reasonable validation of the models. Other authors suggest that formal and computational models can neither be validated—nor falsified—because of their inherent complexity. Among the critics of the approach, some suggest that modelers are indulging in useless formalism and others complain that "this is no longer Psychology." All these criticisms may be well founded, but they may be completely off the mark as well. Although it may take years to find out the final word concerning the virtues and vices of modeling, it seems worthwhile to try to assess where we stand today.

There are, in principle, many ways to do this, but epistemological or methodological debates can easily yield abstract considerations that make little sense for those who are involved in actual research. Because various research domains tend to favor the adoption of a variety of points of view, there is also a strong risk of misunderstanding between researchers from different domains. When research objectives and strategies differ, one author's meat may be another's poison.

We asked a number of researchers, directly involved in some form of modeling, to describe the value of this approach in their field and what it could be expected to produce. Their only constraint was that the paper had to make a point about the impact of modeling in some domain of cognitive psychology. The emphasis of the paper, however, could be abstract and methodological or very concrete and based on actual results. Of course, opinions were expected to diverge when fields differ, but to converge on domain invariant points. We

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received contributions from authors specializing in language and reading, numerical skills, implicit learning, face recognition, and categorization. Some authors chose to present results whereas some decided to discuss the nature of a modeling effort in their own field and in related domains. Readers will draw their own conclusions, but we wish to raise a note of caution and call attention to a few questions.

The note of caution regards the connectionist nature of most of the modeling efforts considered by the authors. Because they have first hand experience with this kind of modeling, they very naturally—and reasonably—chose examples of this type. However, they usually make points that are valid for most forms of modeling, connectionist or not. In any case, this is not a systematic effort at defending connectionist modeling against the other, symbolic, variety. Most of the observations made by Content and Frauenfelder regarding re-description or identifiability problems, or by Cleeremans and French regarding the necessity of specifying a description level apply to both symbolic and connectionist modeling, as well as to the use of animal models (however different the perspectives may be). This is the consequence of a sincere effort to bring out the specific characteristics of the modeling enterprise itself.

We have tried to make some questions salient by the organization of the issue. We have left the introduction, in a sense, to Cleeremans and French. They make explicit a utilitarian view of the modeling approach in which most problems can be cast. In particular, they emphasize the fact that any model is partial and must be used, and also assessed, with very specific goals in mind. They also list most of the difficulties the modeling approach encounters.

Edelman et al. then describe concretely how connectionist modeling has approached the knowledge representation problems involved in learning and retrieving arithmetical facts. They show how this approach is useful to analyze representational problems but, also, how our ignorance regarding the most appropriate form of representation generates identifiability problems. Simple models like Anderson's "Brain-State-in-a-Box" require very specific assumptions regarding the input representation. More elaborate models, such as MATHNET, work with more general or "natural" representations but make a set of more complicated and seemingly ad hoc assumptions about the architecture or the inner workings of the model. Whether specific assumptions should be made concerning the representation of the input or the architecture of the network, and how arbitrary they can be, is an example of the problems faced by modelers.

Some crucial assumptions, though, may easily remain implicit and therefore untested. As an illustration of this problem, Valentin et al. explore—in the context of face processing—the assumption that Hebbian learning is inferior to Widrow-Hoff learning. They show that Hebbian learning is weaker than Widrow-Hoff learning only in relation to the specific constraints jointly imposed by the task and the statistical characteristics of the stimulus set. Actually, they show that Hebbian learning can, in some specific cases, outperform Widrow-Hoff learning. As a consequence, they show that the complexity of some tasks can be misevaluated. In this case, it turns out that learning a small number of faces is easier than generally assumed. Valentin et al. conclude with some speculations about early development, suggesting that infants' learning of faces may be likened to Hebbian learning of a small set of (lowpass filtered) faces. This paper demonstrates why a model must be assessed only with a well-defined use in mind. It even shows, for instance, that the adequacy of the performance measure depends upon the task at hand, as well as upon statistical properties of the stimulus set.

The statistical interpretation of connectionist models, as well as the relationship between statistical properties of the task and the performance of various classes of algorithms, is highlighted by Rosseel in his paper on categorization. Rosseel shows how the category formation task can be treated as a statistical problem and that, while most formal models can be recast as statistical techniques, neural network models are "natural" implementations of these techniques. He continues by providing a neural network implementation of a generalized statistical approach that relates very clearly to existing psychological models. Besides the fact that it gives a general framework for categorization, Rosseel's paper exemplifies how statistical analysis can be used effectively to solve the re-description problem: the problem of understanding the model itself and explaining its behavior. Just as symbolic models tend to capture the linguistic or logical competence of human subjects, at least some neural network models seem to work by developing some statistical competence.

Content and Frauenfelder are left with the difficult task of concluding this special issue. Their goal is to clarify the relationship between human experimental data and the modeling effort. Their perspective is slightly different from the perspective adopted by Cleeremans and French but they concur on the points that any modeling effort will be fundamentally partial, should be assessed in reference to very specific and well-defined objectives, and can only be expected to pay off if an appropriate re-description of the model is provided to complement it. They show that if these conditions are met, the power of formal and computational modeling can be brought to bear on important problems facing cognitive science. We hope that the papers presented in this special issue support this cautiously optimistic outlook.