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LETTERS

Ice Age "Venuses"

The Grimaldi Ice Age figurines referred to in Random Samples of 18 February (p. 923) were shown in 1914 to George McCurdy of the Peabody Museum at Harvard University by the daughter of the excavator, L. Jullien. In 1939 they were offered for sale to the American Museum of Natural History, who sent photographs to the Peabody Museum. Hallam Movius bought the most important of these for the Peabody. It is important, not because it is a "masterpiece," but because it is the only Ice Age carving depicting *two* females, one side pregnant, and the other not, thus confirming the presence of both concepts in Ice Age female imagery (Fig. 1). The long archaeological argument as to whether pregnancy and nonpregnancy were depicted appears to have been settled by my analysis of the figurine published in 1986 (*1*). The remaining Grimaldi figurines are patently "crude" carvings. It was likely for this reason that they were not purchased earlier.

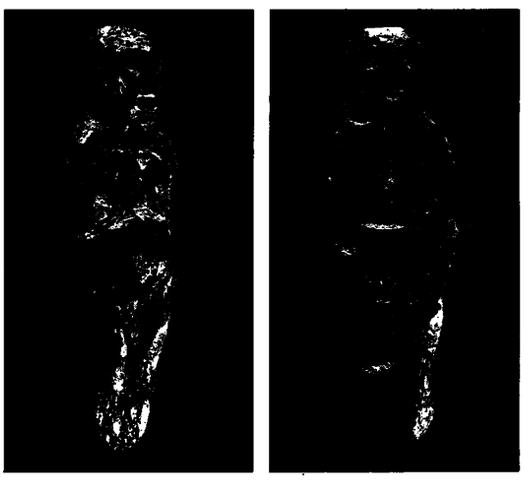

Fig. 1. Grimaldi steatite figurine, three-quarter (left) and rear (right) view, from (*1*, p. 809) (Courtesy A. Marshack).

Early in the 20th century, museums and collectors were primarily interested in purchasing obvious "masterpieces." The best of the Grimaldi figurines had been purchased early in the century for the Musée des Antiquitées Nationales in France. Such "masterpieces" had long shaped archaeological theories concerning the quality and meaning of the so-called "Venus" figurines.

The new set of Grimaldi figurines may, therefore, be important precisely because of their ostensible "poor" quality. There are many classes and types of female imagery with different apparent meanings and uses, and one class, patently "crude" and often relatively quickly made (but not works in progress), was apparently produced by less skilled carvers for short-term, or even one-time, ritual use (*2*). The "masterpieces," on the other hand, often show evidence of long-term curation, use, and reuse.

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I have read with some amusement the speculations concerning the purposes of the 20,000-year-old "Venus figurines." It seems to me that the simplest and most obvious explanation for them has been overlooked, namely, that they are the Epipaleolithic equivalent of the centerfold.

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■

The Meaning of Models

Three cheers for Naomi Oreskes, Kristin Shrader-Frechette, and Kenneth Belitz for their much needed critique of "verification, validation, and confirmation of numerical models . . ." (Articles, 4 Feb., p. 641).

Two points require amplification. First, the impossibility of "verifying" or "validating" models is not limited to computer models. All theories invoke auxiliary hypotheses and rely on imperfectly measured data. Any nontautological theory (that is, any theory that refers to the world) is underdetermined and thus unverifiable, whether it is embodied in a large-scale computer model or consists of the simplest equations. The differences between traditional, analytic theories and large simulations are differences of degree only. Large-scale models, because they require estimation of many more parameters and boundary conditions, have been the focus of the

debate over validation, but the limits on the certainty of our knowledge described in the article apply to all.

Second, as the authors note, the principles discussed apply to models outside the earth sciences, including economics and the social sciences. Although positivism may have "collapsed resoundingly in the 1950s" in philosophy and physics, it continued to wash over the social sciences, cresting with Milton Friedman's still influential 1953 *Essays in Positive Economics* (*1*). Despite numerous critiques (*2*), positivist empiricism remains the leading theory of knowledge in economics today. One need only substitute "demand elasticity" for "porosity" and "marginal propensity to consume" for "hydraulic conductivity" and the article would describe well the state of model assessment in economics. Economic models are routinely described as "being valid" or "having been validated," and the principal (and often only) criterion for "validity" is the correspondence of simulated and actual data. The concept of data, furthermore, is usually restricted to constructs for which numerical measures are available, aggravating the problem of underdetermination. Worse, because it is time consuming to manipulate model inputs until one gets the result one desires, leading economic forecasters have long resorted to "add factoring"—the practice of adding a fudge factor to the *output* of the model so that it corresponds better to the modeler's intuition, thus avoiding the troublesome bother of actually using the model to reach conclusions from well-documented assumptions (*3*). In one study (*4*), the General Accounting Office found that a leading econometric forecasting firm add-factored interest rate projections in a scenario of restricted monetary growth from the model's output of 34% per year to the more sensible value of 7% per year. Such practices persist in part through the failure of model consumers (academics, policymakers, managers, and citizens at large) to look behind the "tests" of "validity" offered by the model makers.

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3. J. D. Sterman, in *Foresight and National Decisions*, L. Grant, Ed. (University Press of America, Lanham, MD, 1988), pp. 133–169.
4. *Three Large Scale Model Simulations of Four Money Growth Scenarios* (Joint Economic Committee, Subcommittee on Monetary and Fiscal Policy, 97th Cong., 2nd Sess., Washington, DC, 1982).

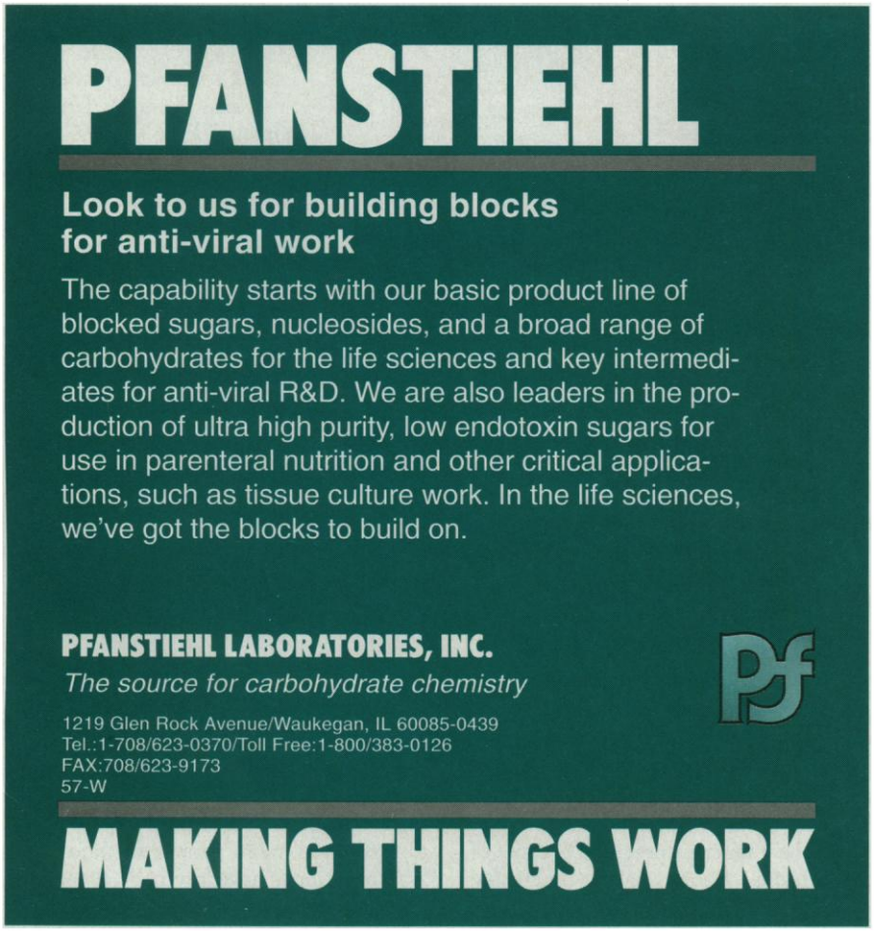
Oreskes *et al.* correctly point out some of the limitations of models and the ways in which models can be tested. However, they set up and convincingly slay a straw man. The straw man is the claim that verification and validation prove that the scientific content of a model is "true." With possible rare exceptions, I know of no case where a modeler has made this claim. "Validation" amounts to acceptance testing. If a model meets specified performance criteria, it is accepted as a "credible representation of the real system" (*1*), and perhaps further as the best technology currently available. That is all that "validation" means in the technical jargon of simulation modeling. For example, a definition of "validation" is "substantiation that a computerized model within its domain of applicability possesses a satisfactory range of accuracy consistent with the intended application of the model" (*2*).

A complex computer model is not reducible to a simple syllogism. However, the correct syllogism in this case is, If this model meets the specified criteria (p), then it is acceptable for use in the problem domain (q). Validation is testing to demonstrate that the criteria are met, thus asserting p and validly concluding q (the model is acceptable for use). This is not a case of affirming the consequent.

Oreskes *et al.* do not define what they mean by "truth" presumably because we all know what that is. However, they use the term "truth" in in the special context of formal logic, and it has other meanings that are consistent with the technical meanings of "verification" and "validation" (*3*).

Semantically, there is little to choose among the terms "verify," "validate," "confirm," "authenticate," "corroborate," and "substantiate," because they are synonyms in ordinary language (*3*). The term "confirmation" has no more claim to probability or provisional acceptance than any of the others. Thus, "verification" and "validation" (and "confirmation") acquire special disciplinary meanings for testing simulation models.

Modelers themselves should take the lead in asserting the restrictions and limitations of models and should draw some important lessons from Oreskes *et al.*: (i) make clear that "verification" and "validation" are used in a technical sense; (ii) if necessary, don't use the terms if they are likely to be misunderstood and create a false sense of truth rather than consensus; (iii)



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take care to specify the context of the model; and (iv) use model acceptability and performance indices rather than simple declarations of validation to describe the results of model testing.

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Response: We fully agree with Sterman that the points raised in our article are limited neither to computer models nor to the earth sciences. We focused on numerical models in the earth sciences because the more general point about underdetermination of scientific theories has been made eloquently elsewhere, because earth science is the area of our own experience and expertise, and because the issues of verification and validation are active topics of discussion among earth scientists.

Rykiel sees little semantic difference between "verify," "validate," and "confirm." While acknowledging overlap in the many meanings of these terms, we disagree that they are synonyms in common usage. For example, one can *verify* that a parking permit has been *validated*. Nuances of meaning do matter, particularly when terms are shared in scientific and lay discourse. Our discussion of the terms "verify" and "validate" is an accurate representation of the way many earth scientists use these terms (*1*), and our use of the term "confirm" follows decades of scholarship in logic and in philosophy of science (*2*). Nevertheless, the primary objective of our article was substantive, not semantic. If modelers were to change only language and not practice, then our article would not have achieved its objective. Current usage *is* misleading and can create a false sense of truth, particularly in practical policy applications.

Rykiel says we slay a straw man, but the "straw people" in this case include the International Atomic Energy Agency and the U.S. Department of Energy. The terminology that we critique comes directly from the scientific guidelines of these agencies and from published scientific literature (*3*). The syllogism that Rykiel puts forward as the correct logical construct for evaluating models begs the fundamental questions at stake: Who decides what the specified criteria are? What are the limits of the problem domain? and Can they change with time?

Rykiel concludes that "modelers themselves should take the lead in asserting the restrictions and limitations of models." Insofar as two of us are modelers (*4*), and all three of us routinely use and evaluate models (*5*), this is precisely what we tried to do.

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Corrections and Clarifications

In the Random Sample "Venuses reappear" (18 Feb., p. 923), Patricia Rice of West Virginia University is incorrectly identified as "Patricia White." Randall White of New York University is the source of quotes attributed to "White."

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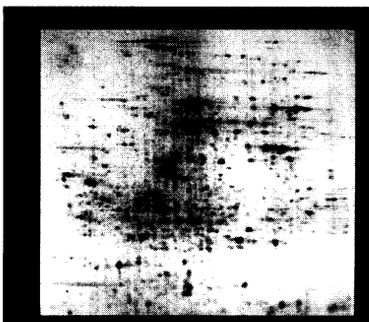

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