Vaulting optimality

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The target article provokes three comments and one more general criticism. First, the reason it is not surprising that "[the optimality heuristic] is used most systematically and successful-

ly in the physical sciences . . . and least in the social sciences" is not its increased plasticity, as is claimed. Rather, it is the greater accuracy and wider applicability of mathematical models in the physical sciences. As Schoemaker amply points out, the equations governing many physical dynamical systems can be viewed through the smoky glass of optimising principles. But in the social sciences it is rather easy to provide myriad examples to refute a hypothesis with substantial quantitative rather than just qualitative import. Optimality hypotheses, dealing in quantities, are hard to come by because quantitative hypotheses in general are hard to come by.

Second, equilibria and optimality bear a more complex relationship to each other than is revealed by viewing the former as just "minimising a difference function defined on the actual and ideal states," as suggested in the context of chemical equilibria. For example, Maynard Smith's (1974) evolutionarily stable strategies (ESSs), which are indeed equilibria, might often be better viewed as suboptimal, in terms of different criteria.

A third point is that the issues the target article highlights are slightly obscured by the absence of a distinction between the messages that might be better directed at general explanation in science and those that are specific to optimality principles. For example, of the eight features of the optimality heuristic, only the fourth, "teleological description of the system," is really confined to optimality; all the others seem to be perfectly general. "Confirmation bias" is also not restricted to this heuristic.

The more general criticism can be seen clearly in a paradigmatic environment for the optimality heuristic – cognitive science. Humans are entities for which both teleological and causal explanations may genuinely invoke processes of optimisation, whereas with water or exchange rates, only the teleological explanations trade on optimisation. As seen in the target article, optimality assumptions are rife at the "higher" cognitive levels, for instance in postulates concerning rationality; but they have also been made about the "lower" subpersonal levels, for example in postulates concerning energy minimisation for constraint satisfaction. Unfortunately, only the significance of levels of explanation and description is ever alluded to. The claim is made, for instance, partly in the context of economic explanation, that "each optimality principle, it seems, begs for an associated process explanation that describes causally, within the constraints of an organism or system, how it operates." Surely this confuses the levels.

For concreteness (rather than correctness, Foster 1990), consider Marr's three levels (Marr 1982). At the computational level, the task a system performs is described and possibly justified on the grounds of appropriateness; at the algorithmic level, representations commensurate with the task and the algorithm by which it is carried out are defined; and at the implementational level, the precise physical realisation of the algorithm is described. As Fodor teaches (Fodor 1975), the fact that psychology has an independent existence at all is a function of the different modes of theoretical explanation at these different levels. The target article suggests an unhappiness with a computational-level optimality principle unless its algorithmic and/or implementational level are also evident. This is unlikely to be a fruitful methodological restriction.

In this context, questions about the use of optimality should be directed at the computational level. How felicitous is it to suppose that human cognition is optimising some measure? The quick-footedness Gould and Lewontin (1979) note is also evident in the discussion of planning under the assumptions of bounded rationality, as seen in a dispute between Dennett and Fodor (Dennett 1987; Fodor 1981). Fodor criticises Dennett for being too wedded to predicting others' actions on the basis of assumed rationality, meaning rationality in the narrow sense defined in the target article. Dennett responds that rationality is inevitably bound in terms of the time and space available for processing, and it is therefore appropriate to predict assuming these bounds. But this threatens to make the notion of optimisation too trivial to be of value.

In drawing our attention back to the uses and abuses of optimality, the target article raises a number of important hurdles that users of optimality principles should vault. Needless to say, cognitive science is "tripping" happily.