A Further Test of Noncooperative Bargaining Theory: Reply

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This note is to comment on "A Further Test of Noncooperative Bargaining Theory," by Janet Neelin, Hugo Sonnenschein, and Matthew Spiegel (1988). They conclude by rejecting both the "Stahl/Rubinstein theory" and the "equal-split model" for the bargaining games they studied.

Consider first the rejection of the "Stahl/Rubinstein theory." To begin with, I. Stahl (1972) and A. Rubinstein (1982) should be cleared of the implicit charge of naiveté. They simply proved the existence of unique subgame-perfect equilibria in certain noncooperative bargaining models. And, just as the current behavior of Kasparov and Karpov does not refute Zermelo's theorem that Chess has an optimal solution, so the experimental results of Neelin et al. (1988) are irrelevant to the theorems of Stahl and Rubinstein. Their results, however, are not irrelevant to our own experimental work, reported in K. G. Binmore, A. Shaked, and J. Sutton (1985). We studied a very simple two-stage bargaining game and observed a marked tendency for equal-split proposals to be made by inexperienced subjects, but a much stronger tendency for subjects who were slightly more experienced to make subgame-perfect proposals. Although we were much more naive then than we are now about the behavior to be expected from experimental subjects, we were very guarded in the conclusions we drew from these data. In the working paper, Binmore et al. (1984), in which our results were reported in full, we went so far as to deny explicitly that subgame-perfect equilibrium behavior should be expected in general. The reason is very simple. Even knowing the theory and being provided with the parameters of the game in a user-friendly graphical form on a microcomputer display, as in our experiment, we, ourselves, had to think quite hard to come up with the "proper" proposals in multistage versions of the Stahl/Rubinstein bargaining model. One has to find a suitable rule of thumb which relates the form in which the problem is presented to the abstract theory. Without extended opportunities for learning and ample incentives to make the necessary time-and-attention costs to the subjects worth bearing, it therefore seems unreasonable to suppose that game theory is likely to be very useful as a precise predictor of the behavior of Princeton undergraduates in this context: even if they knew game theory. This is *not* an admission that game theory is useless in economics. Game theorists defend the use of their trade in positive economics by arguing that, in the *real* economic world, there *will* often be the necessary incentives and opportunities for learning to take place. But here we are concerned with *laboratory* behavior.

But, if we were not trying to prove that game theory is a sharp predictor for laboratory bargaining behavior, what were we trying to do? Economists do not generally seem to be aware that there is large psychological literature on these matters. And, although one may have reservations about the general usefulness of psychological theory in explaining economic behavior, presumably it will be admitted that psychologists know how to run laboratory experiments. One therefore has to take their claims that "equity theory" provides a good explanation of a great deal of laboratory behavior seriously. (See, for example, Eiser, 1980; and Adams and Freedman, 1976.) In particular, Reinhard Selten (1978), the inventor of subgame-perfect equilibrium, is convinced by these claims and has his own laboratory data to offer in support of his views. It was in

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response to a particularly striking experiment by members of his school, namely, W. Güth, R. Schmittberger, and B. Schwarze (1982), that we were led to attempt some laboratory work of our own.

The point being made here is that the issue was not, and is not, the extent to which "gamesmen" are able to solve strategic problems from cold in the laboratory. The issue is whether "gamesmen" exist at all. That is to say, whether subjects sometimes *try* to take account of strategic issues in deciding what to do. Equity theory suggests otherwise: that subjects do not act as "gamesmen" but as "fairmen." Precisely what "fair" means in any given context is a question often open to more than one interpretation. Moreover, there is the more general theory of "focal points" as described, for example, by Roth et al. (1981) to be contended with. But, although such explanatory devices are too hydra-headed to properly deserve being called "theories," the laboratory data gathered by their adherents have been replicated too often to be brushed aside as irrelevant. We, ourselves, have observed behavior that certainly looks "fair," not only from the inexperienced subjects in the study mentioned earlier, but also from subjects in later studies (Binmore et al., 1985, 1987), even though we have tried hard not to offer cues that might precipitate such behavior. This extends even to the behavior reported by E. Hoffman and M. Spitzer (1982), who observed pairs of subjects agreeing to split $14 equally, although one bargainer could guarantee himself $12 simply by withholding agreement.

Our contention in Binmore et al. (1982, 1985) was not that this behavior does not exist or is unimportant. Our contention is that it can be displaced by what we would regard as more thoughtful behavior, *given a suitable environment*. Superficially, the Neelin et al. (1988) results would seem supportive of this conclusion. But the reason that no follower of equity theory will regard their results as relevant in this context, is the manner in which their experiment was "framed." The subjects were students in an intermediate microeconomics class, who were told that they were to test a theory to be explained later. They therefore "knew" a priori that optimizing behavior was expected of them. And the evidence that subjects seek to behave "as expected" is overwhelming. For this reason, we went to great trouble (and much expense) in recruiting subjects and in designing the experimental environment, to offer as few cues as possible as to what behavior was "expected." In particular, in contrast to many studies, our own connection with the experiment was unknown to the students. Moreover, contact with our assistants and with each other was minimized by placing the subjects in separate booths and requiring them to communicate through computing equipment. Without similar precautions, even the two-stage bargaining game results of Neelin et al. are not properly comparable with ours, especially since these were preceded by a (four-stage) practice game whose results were not reported. One might remark that the experiments of W. Güth and R. Tietz (1987) are also not properly comparable with ours. (Unlike Neelin et al., they find that economics students tend to split equally in their two-stage bargaining games.)

The results of Neelin et al. are therefore perhaps best seen, not as a contribution to the debate described above, but to a theory of "bounded rationality" in simple bargaining games. The consistency with which the two-stage optimizing strategy was thoughtlessly carried over to the three- and five-stage games is certainly not a result it would have been easy to anticipate.

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