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Discussion

Comments on Vernon Smith's—"Theory and experiment: What are the Questions?"

Werner Güth*, Hartmut Kliemt

Max Planck Institute of Economics, Kahlaische Strasse 10, D-07745 Jena, Germany

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ABSTRACT

When commenting on Vernon Smith's inspiring paper, we first argue that game theory in its "reasoning about knowledge" tradition is not truly behavioral and try to categorize different approaches. We then go on by considering specific topics, discussed by Vernon Smith, before concluding with some methodological reflections.

1. Introduction and overview

"This paper deals generally with testing questions that arise both when experimental observations are in accord with the actions we predict, and when they are not" (see Smith, introduction). Vernon Smith discusses these questions against the background of the Duhem-Quine problem, and rightly so. Before we comment on this (in 3) we will, however, voice our disagreement with the interpretation of classical game theory as a behavioral theory (2). On specific behavioral issues we shall then focus basically in the same sequence as Vernon Smith does (4) before we conclude (5).

2. Is game theory behavioral?

Though prevailing in particular among American game theorists, the interpretation of game theory as yielding direct behavioral predictions is not necessarily that of its founding fathers. Von Neumann was framing his original problem in terms of optimal or ideal rather than real play while Oskar Morgenstern originally was fascinated by problems of self-refuting or self-fulfilling prophecies. The aim of their "reasoning about knowledge" (see Fagin et al., 1995) or "eductive" (see Binmore, 1987/1988) approach to game theory was that of forming an ideal theory. In their *conceptual* exercise they intended to explicate ("spell out") what it means to be fully rational when interacting with fully rational beings who are also in command of unlimited capacities of reasoning. In view of its contrary to fact assumptions the ideal theory emerging from the conceptual effort could not be assessed according to the classical criterion of "adaequatio intellectus et rei". On the other hand it was not pure mathematics either. Therefore, it was not clear what would distinguish a "better" such theory from a "worse". As a minimum criterion of adequacy such a theory had to be conceivably "absorbable" (see Morgenstern, 1972; Morgenstern and Schwödiauer, 1976; Dacey, 1981; Güth and Kliemt, 2004); that is, it had to be possible that the

* Corresponding author.
E-mail address: gueth@econ.mpg.de (W. Güth).

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Table 1

Duhem-Quine-Again		Predicted overt behavior	
		Observed	Not Observed
Circumstances	Fulfilled	1-corroboration	2-refutation
	Unfulfilled	1-?	2-?

theory could be commonly known without providing any of those reasoning about the game a reason to plan otherwise than specified by the commonly known ideal theory.[1] Any equilibrium theory (Cournot, 1838; Nash, 1951) is absorbable among fully rational players whose beliefs are rational in that they expect the choices of others and optimally respond to their beliefs (Aumann and Brandenburger, 1995). The Seltenian stipulations of small trembles (see Selten, 1975) – for which, of course, justifications in purely mathematical terms can be given as well – can be interpreted as additional adequacy criteria. The continuity in approximation—for instance when taking the limit of a sequence of (strategy) trembles is imposed to prevent the game theorist who envisions these plans from what may be called a "free flight of fantasy".

Continuity in approximation is a restriction on theory formation that does not amount to a claim of approximate behavioral truth of the ideal type behavior envisioned by the theory.[2] It only makes sure that the solutions proposed are robust under arbitrarily small perturbations and thus could at least conceivably be approximated by real world processes that induce the strategies under considerations.[3] However, if the ideal type is not meant to approximate real behavior the good empiricist might ask why she should bother at all. Yet it is a fact, too, that humans are not merely interested in what is. They are as well interested in what might be even though it never will be.[4] Therefore theorists "naturally" engage in the thought experiment of what a world of fully rational beings would be like and what kinds of plans would be consistent in such a world. These *thought experiments* are counterfactual in content. As "thoughts" they exist as a matter of fact and can have an indirect factual behavioral impact on the real world. They can serve useful purposes. For instance they can be used as heuristics when real world individuals who went through the mental exercise of ideal type theory are seeking for improvements.[5] Yet, we believe that it is futile to defend game theory on empirical grounds. The role of "classical game theory" within "the economy of theoretical minds" is more indirect than implicitly assumed by most economists, including Vernon Smith. The fact that most economists nowadays are trained in game theoretic ways of thinking helps to integrate the field by facilitating the exchange of ideas and the division of labor among several specialized researchers but it is not the case that the theory – except for extremely simple cases – is in any plausible sense "approximately true".

We have good – we think overwhelming – reasons to conclude that the ideal type "reasoning about knowledge" premises of classical game theory are so much contrary to the facts that any implications derived from them amount to prophecies rather than predictions. As opposed to prophecies that may turn out true or false as well, empirically founded predictions must be based on empirical laws that are corroborated to some extent or at least cohere with the body of empirical knowledge at a given time.[6] As a piece of a priori reasoning, pure game theory does not fulfill that requirement.

The preceding reservations notwithstanding, we strongly sympathize with Vernon Smith's basic aim of critically assessing empirical claims in an experimental process of "conjectures and refutations".[7] In view of the history of experimental economics Smith's way of organizing his discussion seems useful and so we will go along with it in our further comments.

3. In the spirit of Duhem-Quine

Vernon Smith intends to probe more deeply into the question of how theories are tested against experimentally rendered observational data. Consider (Table 1). The two cases of 1-corroboration and 2-refutation are standard, though, as the author convincingly indicates, in the end less simple than commonly assumed. The two other cases further illustrate why the appearance of simplicity may be deceptive. In case "2-?" we do not know whether the theory itself is wrong or some of the conditions for its application did not hold true. Even if observed behavior is as predicted as in the "1-corroboration"

[1] The multiplicity of equilibria with regard to the theory itself is to be acknowledged. There could be alternative such absorbable theories. In those cases in which games have unique equilibria all of the theories must suggest equilibrium plans to be absorbable among ideally rational planners.

[2] It is a constraint on the kind of ideal rationality chosen and not an idealization of an empirical behavioral theory.

[3] To justify exclusive reliance on strategy rather than, say, payoff trembles raises separate issues which we cannot pursue here; see (Harsanyi, 1973).

[4] In a more philosophical vein we might allude to the Kantian conception of citizenship in two worlds. The Kantian homo noumenon represents ideal homo oeconomiucs behavior while the real behavioral type of the boundedly rational homo sapiens corresponds to the Kantian homo phaenomenon; see on this somewhat weird Latin-Greek bit of terminology (Kant, 1996).

[5] They also might suggest certain ideas of what adaptive processes could conceivably bring about.

[6] A first testing of a newly conceived theoretical conjecture if not backed by already corroborated knowledge is more like a prophecy than a prediction. After pre-testing and preliminary corroboration it gradually turns into a prediction.

[7] Though we are rather confident that a behavioral interpretation of the premises of classical game modeling is quite absurd, we agree that this is the best anchor we have at present. However, the justification for using this benchmark cannot be some prima facie behavioral credibility but must be found in the cognitive psychology of the research process. Another reason may be that adaptive processes will single out the same results as the optimization of ideally rational individuals would and we can therefore use the theory to generate explananda, i.e. phenomena to be explained (by behavioral laws), rather than explanations.