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COMMENT ON "THE CONFUSION OF IS AND OUGHT IN GAME THEORETIC CONTEXTS"*

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Kadane and Larkey in two stimulating articles, the first on "subjective probability and the theory of games" [2] and the second on "the confusion of is and ought in game theoretic contexts" pay considerable (even if unintentional) homage to the influence of the general methodology of the theory of games. This can be seen by observing that the essence of most of their remarks would still apply if we crossed out game theory in the title and replaced it by moral philosophy (see especially the assumptions made in Smith [5]), microeconomics, social philosophy, political science, sociological theory or the use of formal constructs in the social sciences in general.

In spite of setting up a straw man and then emphasizing a false dichotomy both of the papers call attention to deep problems and difficulties in the application of mathematical methods to models of multiperson interactive behavior.

The straw man they set up is the single undifferentiated game theory with no distinction made concerning the modeling assumptions, the rules of the game, or the domain of application. The false dichotomy is between the normative point of view of game theory and the subjective viewpoint to which they subscribe. There is no clear-cut dichotomy. There are a great many positions ranging from *ad hoc* selection of subjective priors combined with limited abilities to process the information perceived to be at hand, to Harsanyi's carefully stated resolutely normative rationalistic approach.

By the tenor of both articles it appears that to Kadane and Larkey game theory purports to be for the most part the normative theory of multistage multiperson decisionmaking where the von Neumann assumption of probabilities as frequencies is somehow critical.

In a basic way game theoretic man is no more and no less than rational utilitarian man placed within a specifically stated multiperson interactive environment. All the limitations on the model of man underlying much of economic and political theory apply.

Clearly the basic assumptions made concerning the decision units and their ability to communicate and calculate will be important in determining the development of various theories. In particular the assumption that an individual has a well-defined utility function which reflects preferences among risky alternatives may be hard to accept. It is not needed for all game theoretic analysis. Important results in the game theoretic study of the economics of exchange can be obtained without making this assumption.

Those of us concerned with the applications of game theoretic methods to the social sciences are well aware of the importance and the limitations of our assumptions concerning the perceptions, preferences and abilities of individuals. Furthermore we must be sensitive to the domain of definition of the models constructed and our confidence in their adequacy as representations of the phenomena to be studied. In particular the assumptions and the model are not independent of the question being

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asked. Can we predict the behavior of Yale undergraduates playing in a sequence of repeated 2×2 matrix games with incomplete information, is one type of question. Can we construct a sociologically neutral index of the relative importance of voting strength in a nonsymmetric voting game is a highly different type of question.

Shapley and Shubik have stressed the importance of the steps taken in modeling prior to specifying solution concepts (see Shubik [4, Chapters 1–4]). They stress that the assumption of "external symmetry" is extremely strong; i.e. any differences between the players must be specified explicitly in the rules of the game, otherwise it is assumed that all individuals are psychologically and sociologically identical. Furthermore there is an assumption that all the rules of the game are specified. In many situations involving high levels of uncertainty this is hard to accept.

Given that assumptions have been made about the players we are still confronted with at least four distinct ways of modeling their environment. We may model in the three ways suggested by von Neumann and Morgenstern. They are the coalitional form, the strategic form or the extensive form. All are representations of games with a well-defined beginning and end. A fourth way of modeling is to find a representation without a specified start and termination.

The three forms utilized by von Neumann and Morgenstern are rated in order of suppression of information. The coalitional form wipes out details concerning strategies and the strategic form removes details concerning moves and information. A true dynamics requires at least the study of the extensive form. Yet following the clearly stated advice of von Neumann and Morgenstern much of the development and application of game theory has been devoted to statics.

> We repeat most emphatically that our theory is thoroughly static. A dynamic theory would unquestionably be more complete and therefore preferable. But there is ample evidence from other branches of science that it is futile to try to build one as long as the static side is not thoroughly understood. von Neumann and Morgenstern [6, p. 44]

Associated with the different forms of representing games are different questions and solution concepts. Let us consider games in strategic form dividing them into four categories: (a) two-person constant sum games, (b) two-person nonconstant sum games, (c) n-person games with $n > 2$, and (d) games against Nature.

TABLE 1

		Player 2: 1	Player 2: 2
Player 1	1	10	0
	2	−20	10

The maximum solution for two-person constant sum games is, in my opinion, clearly and sensibly normative. Damon Runyon phrased the attitude succinctly "The race is not always to the swiftest, nor the battle to the strong, but that is the way to lay your dough". I am sufficiently convinced of this point that, playing the role of Player 1 I would be happy to play the following game with Kadane and Larkey as often as they choose at $1 a point. I am also willing to precommit myself to the strategy (3/4, 1/4).

The most popular solution suggested for both two-person and n-person nonconstant sum games with low communication conditions is the Nash noncooperative equilibrium. What it yields is an outcome with consistent expectations. In experimental games with nice structure and context such as Cournot oligopoly models it has some predictive value. But not too much. When the noncooperative equilibrium has other "nice properties" such as Pareto optimality and symmetry it is a better predictor.

When there are only two individuals playing, I see little if any reason why they should play with low communication except when trapped by experimental psychologists in artificial environments. The normative theories of Nash, Shapley, Harsanyi and Selten appear to offer attractive fair division prescriptions.

When there are many players the reasons for low communication are much better, but then the assumptions concerning complete information about the rules of the game become shaky. At this point we could argue that a reasonable way to model the competitive environment is as a set of parallel one-person games against nature.

We may wish to ask perfectly reasonable and interesting questions about how people do and how people should play the following games:

(a) a one-shot game with many players and low direct communication.

(b) a game with many players, low direct communication repeated many times with a known termination.

(c) a game as in (b) but with an unknown termination.

Items (b) and (c) require a dynamic theory and they appear to be the questions that concern Kadane and Larkey. They are by no means the only questions of concern to the game theorist but they are good questions. Furthermore in common with the psychologists and statisticians the game theorists have no answers even for how people do or should play in a one-person game against Nature. Milnor [3] presents a valuable shopping list of properties one might want such behavior to have; but there is certainly no universally accepted behavioral theory, and although Harsanyi offers a normative theory, how the original subjective probabilities are obtained is still not clear to some of us.

Table 2 shows five games with different levels of uncertainty concerning payoffs and strategies. How should they be played? The first one maximin, and for the others I am not yet convinced, although Harsanyi has a normative prescription. How are they played? I believe that the answer depends heavily upon context, as it is the context which provides the clues for evaluating the bounds on the uncertainty. It also matters whether Nature is played by the weather pattern over New Haven, a group of individuals, a madman, a stranger much like you, an old friend or a two-year old.

In Table 2b payoffs are unknown; in 2c Nature is known to have a strategy with unknown consequences. In 2e we do not know how many extra strategies Nature might have. In 2d the player may have an extra strategy he is not aware of. For example his control room may have a large wall with two buttons to press, but somewhere hidden in the wall there might be a third button.

The theory of games has provided a methodology which has enabled us to pinpoint and to clarify where many of the difficulties in describing and analyzing multiperson decision processes lie. An understanding of the problem is not as gratifying as a solution, but it is an important step towards eventually obtaining a satisfactory solution.

I believe that we do not have a satisfactory general behavioral theory of strategy taking into account limited rationality. An attempt to replace other players by a

TABLE 2

	1	2
1	1, − 1	− 1, 1
2	− 1, 1	1, − 1

(a)

	1	2
1	1, ?	− 1, ?
2	− 1, ?	1 ?

(b)

	1	2	3
1	1, ?	− 1 ?	? ?
2	− 1, ?	; ?	? ?

(c)

	1	2
1	1 ?	− 1 ?
2	− 1 ?	1 ?
3?	10 ?	10 ?

(d)

	1	2	3	. . N?
1	1 ?	− 1 ?	?	:
2	− 1 ?	1 ?	?	:

(e)

mechanism leads to logical difficulties with "he thinks I think" regressions. Shapley has a story about a stupid child who at a party, when offered a dollar bill or a five dollar bill by his father took the one dollar . . . and he did it again and again. Merely replacing an n-person game by n parallel one-person games with subjective probability updating black boxes solves no problems, it slurs over them, as has been pointed out already by Harsanyi [1].

In the behavioral sciences in general not enough attention has been paid to the problems of model building and validation. Many shades of the "is" or "ought" distinction stressed by Kadane and Larkey have already been selected in the initial assumptions concerning the model, long before a solution concept has been suggested.

I share with Kadane and Larkey, von Neumann and Morgenstern, and many others a great desire to see a valuable dynamic positive theory useful for prediction in economics and elsewhere. Not only do we not have a good dynamic theory we are still some distance from completing the program suggested by von Neumann and Morgenstern, which was to obtain a reasonably complete and adequate static theory before concentrating on dynamics.

I am in complete agreement with the last paragraph of Kadane and Larkey's paper; I believe that they encapsulate the spirit that has led to the development of a diversity of models and solution concepts by game theorists and I hope that both Bayesian statisticians and experimental psychologists will follow this advice.

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