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## THE CONFUSION OF IS AND OUGHT IN GAME THEORETIC CONTEXTS\*

JOSEPH B. KADANE† AND PATRICK D. LARKEY†

This paper explores the distinction between normative and positive theoretical statements in a game theoretic context from a Bayesian perspective. Normative and positive theoretical statements are often confused in decision making research. The confusion results from some unique epistemological and methodological problems associated with cognitive behavior as an object for scientific inquiry. The confusion persists because of poor model validation procedures which are themselves further complicated by the confusion. The confusion greatly impedes the development of more useful prescriptions for and predictions of human decision behavior. From a Bayesian perspective which acknowledges the importance of incomplete information and imperfect theories of behavior, the confusion is unnecessary.  
(GAME THEORY)

### 1. Introduction

... it must be remembered that neither opponent is an abstract person to the other, not even to the extent of that factor in the power of resistance, namely the will, which is dependent on externals. The will is not a wholly unknown factor; we can base a forecast of its state tomorrow on what it is today . . . Each side can therefore gauge the other to a large extent by what he is and does, instead of judging him by what he, strictly speaking, ought to be or do.

... Once the antagonists have ceased to be mere figments of a theory and become actual states and governments, when war is no longer a theoretical affair but a series of actions obeying its own peculiar laws, reality supplies the data from which we can deduce the unknown that lies ahead.

From the enemy's character, from his institutions, the state of his affairs and his general situation, each side, using the laws of probability, forms an estimate of its opponent's likely course and acts accordingly.

Carl von Clausewitz  
*On War* (C. 1808)

The development and application of theories of decision making in economics, management science, political science, psychology and statistical decision theory have long been bedeviled by confusion between the "is" and the "ought" in theoretical statements. It is often not clear from either an author's claims for a theory (model) or from the context of its use whether a model is intended to describe how decisions have been made, to explain why they have been made, to speculate about the principles that might be used in making them or to prescribe how they should be made. Although related confusion is found in the physical and biological sciences, the confusion is most pronounced in research on the behavior of human subjects.

In a previous paper [14] we have explored some of the consequences of adopting a modern subjective view of probability for game theory. One important consequence would be clarification of the important distinction between normative and positive theorizing about behavior in games, a distinction that is often lost in the search for determinate "solution concepts" which largely characterizes game theory since the work of von Neumann and Morgenstern.

This paper examines "is-ought" confusion in the theory of games. We discuss what the confusion is, why the confusion arises and persists, why it impedes the develop-

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ment of more useful prescriptions for and predictions of human behavior, and what a cumulatively more useful research program on human behavior in situations of strategic interdependence (conflict and cooperation) might look like.

## 2. The Distinction Between “Is” and “Ought”

The basic distinction in this paper between positive and normative theory was well described by Simon [37, p. 137] as the distinction between declarative and imperative sentences, respectively. Among positive theories, we distinguish three types. *Descriptive* theories are concerned with empirical phenomena, but stop with a description. *Explanatory* theories go further by addressing “why questions.” Finally, *predictive* theories discuss what behavior will be. We also distinguish between two types of normative theory, both concerned with what behavior should be. *Speculative* statements are nonoperational usually consisting of a goal or criterion (e.g., maximize utility or profit) with no precise instructions on how one might accomplish the goal or apply the criterion. *Prescriptions* are operational in that they give both a goal (or criterion) and feasible procedures (an algorithm) for accomplishing it.

Unless you believe that the way in which decisions are currently being made cannot be improved, normative and positive statements about most actors in most situations are different. Predictive and prescriptive theories have their own respective roles in application. These different roles have not been well understood and there are many instances of attempts to use the same theory for both roles.

There is a long history of strong differences of opinion on the relative worth of normative and positive inquiry (see Simon [34]; Friedman [10]; Cyert and March [7]; Lindbloom [16]; Braybrooke and Lindbloom [4]; Winter [42]; March [20]), even when the appropriate relative roles of theoretical types are understood. Consider D. W. Lindley’s particularly strident assertion about relative worth.

... Experiments have been performed which show that individuals do not reason about uncertainty in the way described (by Bruno de Finetti in *Theory of Probability*). The experiments provide a descriptive view of man’s attitudes: de Finetti’s approach is normative. To spend too much time on description is unwise when a normative approach exists, for it is like asking people’s opinion of  $2 + 2$ , obtaining an average of 4.31 and announcing this to be the sum. It would be better to teach them arithmetic. I hope that (de Finetti’s) book will divert psychologists’ attentions away from descriptions to the important problem . . . of how to teach people to assess probabilities. (Lindley [17])

If you are a decision maker faced with a choice of alternatives whose outcomes depend on how people on average perform the arithmetic operation,  $2 + 2$ , it may be very useful to know the 4.31 result, particularly if arithmetic is difficult and expensive to teach. Also, positive theoretical understanding of why people on average calculate  $2 + 2 = 4.31$  may be very important to any efforts to change their calculation behavior. The teaching problem may be different if explanatory analysis shows that 0.31 of the population is perceiving the problem as  $2 + 3$  while 0.69 perceives it as  $2 + 2$  or if it reveals that 0.0655 of the population is solving  $2 + 3$  while 0.345 is solving  $2 + 1$ , or if it reveals that 0.0775 of the population is interpreting the “+” operator as “add the squares.” Descriptive analysis that identifies which classes of people are solving the problem in various ways would, at minimum, enable one to target the teaching efforts. Hence, Lindley overstates the case for normative theory.

The foregoing example is trivial. The point is not. If we design a scheme for pricing electricity on a model of homeowners’ behavior that is prescriptively valid (e.g., they should minimize costs of electricity subject to some constraints) but predictively invalid—they respond differently because we have represented their problem incorrectly—then we are making bad, or at least ignorant, policy unless the policy also provides for teaching all homeowners how to use the model we believe they should use

in determining their response to our new price scheme. In such cases, a positive theory may be much more useful than a normative theory if it is a better predictor of responses, ergo, a better basis for policy formulation. There is a definite relationship between types of theory and styles of theorizing. Generally a deductive style is associated with a normative theory and an inductive style with a positive theory. Here, too, there are strong advocates on each side.

Riker and Ordeshook [28] provide an interesting statement of preferences on styles of theorizing.

To illustrate the difference between observed and postulated regularity, consider the observation of tosses of nine coins all of which show heads. By the empirical standard, this is a regularity and one is justified in the generalization that fair coins fall heads. From this, one would predict empirically that heads would show from the toss of a tenth fair coin. Since, in the now-conventional postulate of Western science, a coin is a mechanism without human or divine preference, there is no reason for it to fall heads more than tails. By the theoretical standard, the observed regularity of nine heads is simply the occurrence of one chance in 512—that is, simply fortuitous—and one cannot really generalize about these nine coins (unless, of course, we question the initial assumption of fairness and examine the possibility that the coins are biased). Contrasting with the empirical prediction, the best theoretical prediction is that the tenth coin will fall heads with a probability of one-half. These are quite different predictions, even though they concern the same (imaginary) experience. In the one case, regularity comes directly from the observation, which is taken at its face value; in the other there is no regularity—even though one is in fact observed—because there is no reason for it. [28, pp. 9–10]

There are a number of obvious problems with the authors' extreme statement on styles of theorizing. As a prescriptive theory for how to play and not to play a coin toss game it fails from a Bayesian perspective because there is no indication of how firm our prior belief in the fairness of the coin is and, therefore, no way of knowing how much we might be influenced by nine consecutive tosses of heads for estimating the subjective probability of a head on the tenth toss. Our view might very well depend on whether we are playing with our own coin, the coin of a rector of a local church known for his honesty or the coin of someone with a ducktail haircut and shifty eyes we just met in a sleazy bar.

Their view of descriptive theory is incomplete because of the widely observed gambler's fallacy. As scientists informed by prior empirical work we would expect a significant proportion of people to believe in fair coins but not understand the independence of tosses and, therefore, they would declare tails on the tenth trial in a belief that the outcome is "overdue."

This paper asserts that both prescriptive and predictive theories are necessary. What is really important to the conduct of social and behavioral science is that researchers understand the distinction and the appropriate respective roles of the differing theoretical types, that they specify a research purpose with sufficient clarity that others in the larger scientific community can see what theoretical type is appropriate, and that researchers test their theories and models in ways that are consistent with their intended uses.

### 3. The Objectives and Focus of Game Theory

Although this paper is primarily concerned with the "is-ought" confusion, it also has much to say of a critical nature about game theory, the domain from which the examples are drawn. Our criticism reflects both disagreement with the objectives of game theory as reasonable for a science of human behavior in conflict situations and disagreement with the procedures (means) that the game theorists use to accomplish their stated objectives. The "is-ought" confusion is manifested in both the objectives of the field and in the pursuit of those objectives.

In such a critical discussion, it is important that we not impute objectives to the field that are not shared by the researchers and then use those objectives as the basis of our criticism. Consider the following statement of objectives for game theory:

. . . it is crucial that the social scientist recognize that game theory is not *descriptive*, but rather (conditionally) *normative*. It states neither how people do behave nor how they should behave in an absolute sense, but how they should behave if they wish to achieve certain ends. It prescribes for given assumptions courses of action for the attainment of outcomes having certain formal “optimum” properties. These properties may or may not be deemed pertinent in any given real world conflict of interest. If they are, the theory prescribes the choices which *must* be made to get that optimum (emphases in original). (Luce and Raiffa [19, p. 63])

Luce and Raiffa [19] are clearly sensitive to the distinction that is the focus of this paper. They disclaim almost any positive (explanatory/predictive) theoretical intent for game theory. They do, however, leave open the possibility that the “optimum properties” derived by game theorists “may . . . be deemed pertinent in any given real world conflict of interest” and assert a necessary prescriptive role for the theory in such cases. They are not fully clear on the operationality of the normative theory for individuals but appear to have prescriptions in mind. They are also not clear in this passage on how to finesse the need for a predictive theory of an opponent’s behavior in advising people on “how they should behave if they wish to achieve certain ends” when the ends are conditional on predictions of the opponent’s action. The normative theory they describe in this passage is only conditional on the preferences of those being advised. Luce and Raiffa’s lack of clarity in this passage on the necessity for predictive theory of the opponent’s behavior is puzzling because the first part of the larger passage from which the quotation is taken deals with the problem of the minimax prescription that it is often suboptimal (irrational) to follow if you believe that your opponent is not playing his minimax strategy.

Another important author on game theory writes:

Game theory is a method for the study of decision making in situations of conflict. It deals with human processes in which the individual decision-unit is not in complete control of other decision units entering into the environment. It is addressed to problems involving conflict, cooperation, or both, at many levels. The decision-unit may be an individual, a group, a formal or an informal organization, or a society. The stage may be set to reflect primarily political, psychological, sociological, economic, or other aspects of human affairs. (Shubik [33, p. 8])

Shubik’s description of game theory is problem and method directed and all encompassing. There is neither distinction nor confusion between “is and ought.”

Yet another author has written:

Game theory is a collection of mathematical models formulated to study decision making in situations involving conflict and cooperation. . . . Game theory attempts to abstract those elements which are common and essential to many different competitive situations and to study them by means of the scientific method. It is concerned with finding optimal solutions or stable outcomes when various decision makers have conflicting objectives in the mind. In brief, a game consists of players who must choose from a list of alternatives which will then bring about expected outcomes over which the participants may have different preferences. The game model describes in detail and potential payoffs which one expects to occur, and it points out how one should act in order to arrive at the best possible outcome in light of the options open to one’s opponents. Game theory attempts to provide a normative guide to rational behavior for a group whose members aim for different goals. (Lucas [18, p. 3])

Lucas’ statement is much more difficult to parse than the previous two statements. He posits multiple objectives for game theory with the greatest weight on prescriptions for individuals and groups. But he also mentions “the scientific method” and “the game model [describing] in detail the potential payoffs which one expects to occur . . . .” In the passage, Lucas is insensitive to the “is-ought” distinction; the types of theory are confused.

#### 4. Is-Ought Confusion in Game Theory

Much of what we have to say in this paper applies to theoretical microeconomics generally and to political science which uses microeconomic models to analyze political behavior. Game theory is the focus of the discussion because it is important to have specific examples if we are to avoid the disconnected, obtuse mode of discussion that occurs so frequently in the debates mentioned above. It is as dangerous to generalize about the social sciences as it is to generalize within the social sciences. Even in a subarea such as game theory with a relatively sharp focus on a particular class of substantive problems using relatively homogeneous methods, there is enough variety to ensure that we will be unjust to some.

It is neither practical nor essential to provide a thorough description of game theory here. The reviews of game theory that we have used for this discussion include Lucas [18], Luce and Raiffa [19], Miller and Steinfatt [22], Rapoport and Orwant [26], Riker and Ordeshook [28], Shubik [33], von Neumann and Morgenstern [41], and Young [43], [44]. Our review of game theory will be very restricted, focussing on the core ideas and aspects related to the is-ought confusion.

Game theorists distinguish between noncooperative theory, which seeks to advise the players, and cooperative theory, which really amounts to a theory of arbitration. In this paper we concentrate on noncooperative theory.

If you are a player (or advisor to a player) you need two theories. First, you need a prescriptive theory to guide your decisions. You want a decision procedure that results in better outcomes (on average) than alternative procedures. Second, you need a predictive theory of the opponent's behavior—a theory to predict his behavior, to formulate premises for your own decisions. The theory of your opponent's behavior, the decision procedure you believe that the opponents will use, may or may not be like the decision procedure you are using. If you have good information about the problem your opponent perceives and reason to believe that he will solve it as you would solve it, your own (prescriptive) decision procedure applied to your understanding of your opponent's decision problem may be a useful first approximation to predicting your opponent's behavior. This approximation should, however, be open to validation and revision as you gain information about your opponent's behavior through successive plays. To the extent that your outcomes depend on your ability to predict your opponent's behavior, there may be a penalty associated with holding an incorrect theory of your opponent's behavior.

The importance of the predictive theory of your opponent is a function of player interdependence. In Game 1 below the theory of your opponent is not at all important because the outcomes to each player are independent of what the other player does.<sup>1</sup> Zero-sum games, however, represent an extreme form of interdependence that makes the theory of the opponent very important.

		Player B	
		1	2
Player A	1	6, 3	6, 7
	2	10, 3	10, 7
		<i>Game 1</i>	

If you are the observer of a game seeking to explain its outcome *ex post* or, more ambitiously and usefully, to predict its outcome *a priori*, you need explanatory/predictive theories of both players and their interaction in the task environment posed by

<sup>1</sup>This game is taken from Martin Shubik [32]. The cell entries represent payoffs from strategy pairs for A and B, respectively.

the game context. You may, as most game theorists do, postulate some form of rational behavior for both players as your explanatory/predictive theories. But, as a rational person, yourself, you should not, in light of a substantial body of experimental and natural evidence, expect these theories to work well predictively except in the most simple, contrived settings.

It is important to understand the necessity of predictive theory in game situations. There is no way meaningfully to prescribe behavior for one or both players without a predictive understanding of human behavior. Prescriptions to a single player that assume a single behavior of the opponent when other behaviors are possible and are, therefore, not conditional on an opponent's behavior, do not provide feasible means of responding to changes in behavior and hence are not satisfactory prescriptions for multiple play games. A classic example is von Neumann and Morgenstern's [41] minimax prescription which is often suboptimal when the opponent is not following the presumed minimax strategy (Luce and Raiffa [19, pp. 62–63]; Ellsberg [8]).

Much game theory has a third party, observer perspective, but there are few situations where it makes sense to prescribe behavior for more than one party to a conflict. "Applications" that consist of advising on institutional design (e.g., voting rules and agenda control procedures) with postulated behaviors are perhaps the closest thing to useful all-party prescriptions, but even these must rest on assumptions about the feasibility of the postulated behaviors. Also, it is rarely useful to interpret history with normative theory. It is too late to advise Napoleon on how to fight at Waterloo. It may not be too late, however, to theorize positively about Napoleon's behavior at Waterloo and adapt explanatory theories developed in historical contexts to contemporary predictive tasks.

The confusion between "is" and "ought" in game theory is widespread and is a serious obstacle to developing the theory along more productive lines. A distressingly large proportion of the research in the social, behavioral and management sciences can be categorized as either cumulatively useless or noncumulatively useless. There are modes of theorizing that have not and are never likely to lead to useful prescriptions for or useful predictions of human behavior. Game theory epitomizes the cumulatively useless. It is astounding that thirty-plus years of (often elegant mathematical) theorizing and experimentation could produce so little of value in instructing people on how they *should* behave in conflict situations and in predicting how they *do* behave in conflict situations.

Even in extremely simple, contrived cases game theory has little to tell us of value in actually playing games or in predicting the outcomes of games as a nonplaying observer unless some very special assumptions hold about player behavior. Consider the following situation. Player *S* has hired you to advise him on how to play the next 100 moves in the following game:<sup>2</sup>

		Player <i>E</i>	
		<i>E</i> <sub>1</sub>	<i>E</i> <sub>2</sub>
Player <i>S</i>	<i>S</i> <sub>1</sub>	+ 3, - 3	- 1, + 1
	<i>S</i> <sub>2</sub>	- 9, + 9	+ 3, - 3

*Game 2*

Players see the full game in normal form, choose simultaneously and know their opponent's choice and the resultant payoff immediately after each play. In the 300

<sup>2</sup>This game is adapted from Lieberman [15]. The cell entries represent payoffs from strategy pairs for *S* and *E*, respectively.

plays already completed,  $S$  has strictly adhered to his mixed minimax strategy of playing  $S_1$  with  $P(S_1) = 0.75$  and  $S_2$  with  $P(S_2) = 0.25$  although it has been awkward flipping coins under the table. His opponent,  $E$ , has also been doing something under the table and appears to have played  $E_1$  with  $P(E_1) = 0.5$  and  $E_2$  with  $P(E_2) = 0.5$  (i.e., there is no obvious pattern beyond frequency to  $E$ 's play). So far  $S$  has exactly broken even in the game which does not surprise him since his calculated expected value for the game assuming minimax strategies was zero. But  $S$  believes that he can do better than following the minimax strategy in the next 100 plays because his opponent does not appear to be following his minimax strategy [ $S$  calculates that  $E$ 's minimax strategy is to play  $E_1$  with  $P(E_1) = 0.25$  and  $E_2$  with  $P(E_2) = 0.75$ ]. Indeed, if  $E$  is truly following a  $P(E_1) = 0.5$ ,  $P(E_2) = 0.5$  strategy and continues,  $S$  believes that he can play  $S_1$  every time and expect  $+1$ . But  $S$  is worried that  $E$  will notice him playing  $S_1$  every time and counter with  $E_2$  every play until  $S$  abandons the strategy.

What is your advice to  $S$  for optimal play? Should he play  $S_1$  or  $S_2$  on the next move and how should he decide? Do you have a general strategy to recommend to  $S$  for the next 100 plays? If so, is your recommended strategy independent of or conditional on  $E$ 's behavior during the 100-play sequence? What literatures would you draw upon for your advice to  $S$  in this situation? Game theory? Classical statistical decision theory? Bayesian decision theory? Psychology?

Alternatively, suppose you have been hired by an observer  $B$  who has been offered the opportunity to be paid for forecasting the outcomes of the next 100 plays of the game between  $S$  and  $E$  (no communication between  $B$  and  $S$  or  $E$  is allowed). For each correct forecast of an observed strategy pair  $(S_i, E_i)$   $B$  will receive 3 and for each incorrect forecast  $B$  will lose 1. The forecasting activity can take two forms. In Game 1, he can forecast the full sequence of 100 moves before any play. In Game 2, he can forecast move by move with full information on prior moves.  $B$  must pay if he wants to engage in one of these forecasting exercises and he wants you to tell him the most he should be willing to pay to play each form. Also, how should he forecast in either or both cases? And again, what literatures do you draw upon for advice?

In the larger scheme of things, the problems posed above are not difficult. They certainly pale beside the problems of a von Clausewitz or those of a commodities trader. What is it then about these "games" or about "game theory" that leaves us after some 35 years of work by excellent minds with no clear prescriptions or predictions?

Much game theoretic work is with simple, potentially tractable examples. Mathematics and experimentation are used extensively. One frequent criticism of game theory is that the situations it models and creates in the laboratory lack "realism" or "external validity." The critics assert that the situations studied are trivial in relation to the "real" conflict situations that ultimately concern us and that these simple situations are not (and never will be) any help in understanding the real situations. The observation about the tenuous relationship between the contrived and the real situations is largely correct (i.e., the games lack "external validity"), but the conclusion that studying the contrived situations is a waste of scientific time and attention is incorrect. The experience with direct study of the real, extraordinarily complex phenomena leads us to conclude that such direct study is too often noncumulatively useless *as science*. There are simply too many variables and complex interrelationships in natural settings for weak theories and weak methods. For us, then, it is a virtue to work with situations that can, at least potentially, be controlled and understood and to extend the analysis gradually to increasingly complex situations. Bernstein [5, p. 237] has eloquently put the case we have against those who criticize game theory on the grounds that the cases it works with are too simplistic and that students of conflict should really be examining the Second World War, Hollywood marriages, and other "real" conflict situations.

... if one takes as primary data products of the nervous system such as Einstein's theory of relativity, Beethoven's Ninth Symphony, and Van Gogh's *Starry Night* and, from these, attempts to deduce the construction of the apparatus that produced them, one is not likely to get very far. It would be like trying to deduce the structure of the elementary particles of subnuclear physics by contemplating Mount Everest. The idea, rather, is to put together a vast array of very primitive objects and to see what such an array working in concert can produce.

We do not, therefore, object to the strategy of studying simple conflict situations. We do, however, quarrel with which simple situations get selected for study and with how they are studied.

A related criticism of game theory has centered on its extensive use of mathematics; for more strident critics, anything that can be studied using the languages of mathematics is not worth studying. We do not share this criticism because there is nothing innately correct or incorrect about any language—it all depends on what one is attempting to accomplish and on how the language is used.

### 5. Why the Is-Ought Confusion Arises and Persists

We will discuss four reasons why the “is-ought” confusion arises and persists in game theory. These are: Introspective Theorizing, Objective and Subjective Rationality, the Third Party Perspective of game theorists and Tractability as a criterion. These reasons are highly interrelated.

Human behavior poses some rather unique and extraordinarily difficult problems as an object of scientific inquiry. Cognitive behaviors including those labelled variously as problem-solving, information-processing, decision making, and choice are not excepted from these problems. The normative-positive confusion results in large measure from the difficulty of these theorizing tasks.

The central problems that decision making behavior poses for positive scientific inquiry are difficulties in accessing (and experimenting with) cognitive mechanisms and the overwhelming complexity of the mechanisms even when we can access the brain (Anderson [1]). The upshot for empirically deriving and testing theories is that we must infer decision processes from behavioral, stimulus-response data often produced in nonexperimental settings. It is in all likelihood impossible to infer necessary, much less unique, decision processes from such data (Anderson [1]). The theoretical search is thus reduced to one for sufficient processes. Even this search is enormously complicated because of the importance of context to behavior. As a number of experiments have shown, contextual factors such as the semantic character of the decision task (Tversky and Kahneman [39]; [40], Slovic, Fischhoff and Lichtenstein [38]; Grether and Plott [11]) and the social setting (Nisbett [24]) can radically alter behavior. Inferring decision processes requires, in the first instance, data about the subject, the context and observed behavior. Given the data, we must be able to attribute behavior. We are not far advanced in this search.

The central problem for normative inquiry is finding demonstrably better ways of representing and solving decision problems. The two most common approaches to prescriptive theorizing are: 1) to represent a problem and devise a solution, hopefully optimal, for it; and 2) understand how a problem is currently represented and solved and attempt to devise superior representations and solutions (i.e., better than those previously employed).

#### 5.1. *Introspective Theorizing*

Many theoretical propositions about individual and collective decision making behavior are the product of deduction and introspection. Thomas Schelling nicely describes the analytic technique.

With people, in contrast to light beams and water, we usually believe we are dealing with conscious decisions or adaptations in the pursuit of goals, immediate or remote, within the limits of their information and their comprehension of how to navigate through their environment toward whatever their objectives are. In fact we can often ascribe to people some capacity to solve problems—to calculate or to perceive intuitively how to get from here to there. And if we know what problem a person is trying to solve, and if we think he actually can solve it, and if we can solve it too, we can anticipate what our subject will do by putting ourself in his place and solving his problem as we think he sees it. This is the method of “vicarious problem solving” that underlies *most* of the microeconomics. (Schelling [31, p. 19]) (emphasis in original).

It is clear how this mode of theorizing contributes to the is-ought confusion. In this mode, the predictive model we posit for our opponent is the best procedure we would know how to follow if confronted with his decision problem as we believe he perceives it. We use our prescriptive model for ourselves to predict our opponent’s behavior.

There are many ways to go wrong in this deductive-introspective approach to theorizing about the behavior of others such as not having a good model for our own behavior, not getting the opponent’s problem representation [“decision frame” in (Tversky and Kahneman [40])] correct or misjudging the person’s values or capabilities. One study (Rapoport, Guyer and Gordon [27]) found important differences in the behaviors of Danish engineering students and American students in playing a simple game. The Danes opted for “equitable” rather than “individual-maximizing” strategies much more frequently than the Americans. The behavioral differences probably stem from differences in national or educational cultures. If these results are replicable and students from the two groups were pitted against each other in the same games, the introspective-deductive approach to theorizing about one’s opponent would be an extremely faulty procedure for either side. Prescriptive and predictive theories for conflict that are not structured so that such behavioral differences can be noticed and utilized are not apt to prescribe or predict very well. In game theory, and microeconomic theory generally, conservatives, liberals, geniuses, morons, heroes, cowards, saints and sinners are all predicted to frame problems and choose in ways that theorists can deduce from the task. The consequences of using the introspective-deductive approach to understanding and predicting an opponent’s behavior are not great when we are working with college students in a laboratory. When the U.S. persists in using this approach to predict Soviet foreign policy and military behavior, there is cause for greater concern.

The study of human decisional behavior inherently involves an organism studying organisms which are not obviously different from himself. Further, the study entails using one cognitive apparatus to study other cognitive apparati where differences in cognitive abilities and propensities are often not obvious. The temptation to introspect in such a situation is overwhelming. Indeed, we hypothesize that the less information one has about an opponent and about differences between an opponent and oneself, the greater the role of introspection will have in predicting the opponent’s behavior.

## 5.2. *Objective and Subjective Rationality*

Most of mathematical game theory is concerned with “solution concepts” from the perspective of a dispassionate, external observer. Solution concepts (e.g., Core, Nash Equilibrium, or Minimax) might be used both prescriptively and predictively. One prescriptive theoretical use would be to work out optimal strategies for the players and to determine what a game’s outcome will be if all players play optimally. One positive theoretical use would be to predict that players in some empirical game context, usually experimental, will play optimally and that the outcome of the empirical game will approximate the predicted outcome from a solution concept.

The empirical results from explanatory/predictive applications are, at best, mixed (McKelvey and Ordeshook [21]; Liebermann [15]; Rapoport and Orwant [26]). Even in many of the relatively simple conflict situations created in the laboratory, subjects depart from optimal behavior as best it can be deduced in a particular game using particular assumptions about the rationality of subjects. These results need not be interpreted as evidence of irrational or nonrational behavior because subjects may still be behaving rationally in a weak personalistic sense (i.e., they are solving the problem they perceive in the best way they can devise). Our theories of behavior at least as expressed in solution concepts are simply not descriptive of how subjects represent and solve all problems. There is very little empirical work (Shubik [30]; and McKelvey and Ordeshook [21]; Axelrod and Hamilton [2] are partial exceptions) that looks at multiple solution concepts as alternative strategies that different subjects may follow in different game task environments or that the same subject may follow at different times in the same task environment.

Most contemporary theories of rationality have subjective elements. The most satisfactory theories in an axiomatic and philosophic sense hold that both probabilities and values (utilities) are subjective. These theories pose some formidable problems in practice because to use them predictively or prescriptively for someone else we need to objectify the theories. Herbert Simon's [35, p. 278] remarks of some 25 years ago are relevant (emphasis in original).

(There is a) necessity for careful distinctions between "subjective" rationality (i.e., behavior that is rational, given the perceptual and evaluational premises of the subject), and "objective" rationality (behavior that is rational as viewed by the experimenter). Because this distinction has seldom been made explicitly by economists and statisticians in their formulations of the problem of rational choice, considerable caution must be exercised in employing these formulations in the explanation of observed behavior.

To the experimenter who *knows* that the rewards attached to the two behaviors  $A_1$  and  $A_2$  are random, with constant probabilities, it appears unreasonable that the subject should not learn to behave in such a way as to maximize this expected gain—always to choose  $A_1$ . To the subject, who *perceives* the situation as one in which the probabilities may change, and who is more intent on outwitting the experimenter (or "nature") than in maximizing expected gain, rationality is something quite different. If rationality is to have any objective meaning, independent of the perceptions of the subject, we must distinguish between the rationality of the perceptions themselves (i.e., whether or not the situation as perceived is the "real" situation), and the rationality of the choice, given the perceptions.

If we accept the proposition that organismic behavior may be subjectively rational, but is unlikely, in a complex world, to be objectively rational, then the postulate of rationality loses much of its power for predicting behavior. To predict how economic man will behave, we need to know not only that he is rational, but also how he perceives the world—what alternatives he sees, and what consequences he attaches to them.

There are active research programs on the elicitation of prior probabilities (Savage [30], Kadane et al. [13], Chaloner and Duncan [6]), the elicitation of preferences (Mosteller and Nogiee [23], Becker et al. [3]) and how subjects represent problems in different domains (Kahneman and Tversky [40]). None of this work to our knowledge is being done in game contexts; indeed this work is irrelevant to game theory as that discipline has been defined.

As long as the emphasis is on a single theory that is to fulfill both normative and positive roles, the is-ought confusion will persist. The theories are not testable as prescriptive or predictive as specified because the theories do not lead directly to behaviors and measures of behavior that can be tested.

### 5.3. *The Perspective*

A third factor contributing to the is-ought confusion in game theory is the third

party perspective adopted by game theorists. Raiffa [25, p. 290] noted this problem

When we depart from (the) extreme case in which there are two players with strictly opposing interests, game theory has very little advice to offer us. True, it does establish a relevant vocabulary and a pattern of thinking, but is silent when it comes to telling us precisely how *we*, as one of the players of the game, should go about analyzing our problem. One difficulty is that the theory attempts to be neutrally prescriptive, to give advice simultaneously to each player of the game, and it cannot accomplish this except in a small subset of strategic conflict situations. (Emphasis in original.)

As we saw in the examples in §4 above, Raiffa is being charitable to game theory in implying that it is useful even in the “extreme case in which there are two players with strictly opposing interests . . .” Even static, single play games with no relevant history of play can pose problems for game theory as positive theory. The theoretical predictions of play outcomes in such situations are often wrong (Rapoport and Orwant [26]; McKelvey and Ordeshook [21]). The theoretical prescriptions are very incomplete and conditional. The prescriptions are valid only if the information exists to calculate your opponent’s rational strategy and your opponent is playing the strategy you deduce. Rather than drawing on statistics and psychology to improve the predictions in the many interesting situations of strategic interdependence, game theory has evolved with rare exceptions to consider increasingly obtuse, hypothetical situations in which players, alike except in preferences and their position with respect to the task environment, interact (Kadane and Larkey [14]).

#### 5.4. *The Tractability Criterion*

A fifth reason for the persistence of the is-ought confusion is the importance of the criterion of mathematical tractability to game theorists in choosing problems and in deciding how to work on them. Problems are selected neither because they promise to shed light on how one could play a game rationally dealing with essential details like gathering and processing information about one’s opponents nor because they promise to shed light on human behavior in conflict situations thus improving our ability to predict that behavior. Problems are selected because a researcher can see how to extend the edifice in closed form. It matters not that the axioms of the edifice are prescriptively incomplete or that they are descriptively false or that the theories cannot be empirically tested. Mathematical tractability is all that counts.

Now it may be that the problems are intrinsically interesting, that the results are elegant and aesthetically pleasing, that the mathematics produced are real contributions to the discipline of mathematics, and that proving theorems in game theory keeps a large number of academics and journal editors employed. It does not follow that the work is contributing to our knowledge of how individuals and organizations do and should behave in conflict situations.

## 6. Reducing Is-Ought Confusion

The confusion of “is” and “ought” is so prevalent in game theory that prescriptions to reduce the confusion are inevitably prescriptions to alter the research agenda in game theory radically. Our prescriptions for reducing is-ought confusion are simple: adopt a Bayesian perspective and validate models in accordance with their use.

### 6.1. *Adopting a Bayesian Perspective*

In a previous paper (Kadane and Larkey [14]) we explored the consequences for game theory of adopting a subjective view of probability. The consequences are large. Distinctions that appear to be important in von Neumann and Morgenstern [41], two-person vs.  $n > 2$  person, zero-sum vs. variable sum, appear not to be critical in a

Bayesian formulation. However, the distinction between single play and repeated plays games seems more important than in the original von Neumann and Morgenstern work.

The Bayesian view of games clarifies the proper, respective roles of prescriptive and predictive theory. Taking the Bayesian norm as *prescriptively* compelling for my play leads me to want the best *description* I can find of my partner/opponent's play. Thus both prescription and description have important roles to play in the Bayesian view of games.

The Bayesian view also raises some fundamental questions about the value of pursuing solutions to games that presume symmetrical behavior in the two-player case and homogeneous behavior for all players in the multi-player case.

. . . the achievement of determinate solutions for two person, non-zero-sum games through the estimation of subjective probabilities requires the introduction of an assumption to the effect that the individual employs some specified rules of thumb in assigning probabilities to the choices of the other player. But this is not a very satisfactory position to adopt within the framework of the theory of games. Logically speaking, there is an infinite variety of rules of thumb that could be used in assigning subjective probabilities, the game theory offers no persuasive reason to select any one of these rules over the others. This problem can be handled by introducing new assumptions (or empirical premises) about such things as the personality traits of the players. But such a course would carry the analyst far outside the basic structure of the theory of games, requiring a fundamental revision of the basic perspective of game theory. (Young [43, pp. 28–29].)

From the subjectivist Bayesian perspective, game theorists are already “(employing) some specified rules-of-thumb in assigning probabilities to the choices of the other player.” Assuming that your opponent will play a minimax strategy which you then attempt to construct from his perspective, given information in any particular game about his pay-offs and preferences, is an example of such a “rule-of-thumb.” At best, this rule-of-thumb is a partial basis for forming your prior about your opponent's likely behavior in certain simple game situations. It is not a logically compelling prescription for your own play (Ellsberg [8]). And it is not a very accurate predictive theory for most games (Rapoport and Orwant [26]; Miller and Stienfatt [22]).

## 6.2. *Changing Model Validation Habits*

If the purposes of theories and models were clearly stated and the theories and models were tested in accordance with their purposes, much of the is-ought confusion would disappear. The confusion between “is” and “ought” exacerbates the already formidable model testing (validation and invalidation) problems of the social and management sciences. Weak testing procedures, in turn, abet the persistence of is-ought confusion. Intended model use is a necessary, if not sufficient, basis for model appraisal.

Those who apply decision theoretic models to the study of politics sometimes blend the different usages of these models, now rejecting a model as descriptive because it seems unreasonable as a prescriptive model (e.g., minimax regret), then accepting a model as descriptive because it seems reasonable as a prescriptive model (e.g., expected utility). (Ferejohn and Fiorina [9])

Many decisionmaking models, particularly formal models, in the social and behavioral sciences are specified, elaborated and never tested or used. Validating models of unspecified use is hard work.

Positive models make empirical claims. They are validated (relative to alternative models) in their ability to explain and/or predict behavior. An important aspect of positive models is their testability, the extent to which their assertions can be empirically checked.

Normative models are validated (relative to alternative models) in their ability to prescribe decision procedures that lead to outcomes which are superior, on average, to outcomes that would result from following an alternative decision procedure. Optimal procedures purport to be superior to any conceivable alternative procedure. Heuristic procedures make the weaker claim that they are superior to other, known feasible procedures. Prescriptive theories and models often must make empirical claims if they are to have value. One important claim is that the procedures are feasible. For example, models of rational decision that advise finding an optimum among an infinite number of alternatives without search instructions often fail on feasibility grounds. Also, prescriptions must usually presume states of positive knowledge. For example, the prescription may presume *a priori* knowledge of levels of demand for a product in future periods or knowledge of which actions an opponent will take on future plays of a game.

The is-ought confusion is a serious problem for game theory because it leads to substantial ambiguity in the standards for appraising theory. When the purpose of a theory is not clearly stated or clearly implicit in some application, it is very difficult to test that theory.

Most of game theory is speculative-normative. The only standards that can be applied to this type of theory are the standards that apply to mathematics research: Is the logic correct (internally consistent)? Are the results new? Do the results suggest further work? Even if the answer to all three questions is "yes" and the results are published, researchers interested in prescription and prediction must then ask, "So what?" There is nothing in these standards to make us sanguine about the utility of accumulated game theoretic results in the short or long run.

Results in game theory are cumulative in a curious way. Results accumulate in a mammoth deductive edifice that neither cumulatively improves our understanding of how the world works nor cumulatively improves our ability to intervene in the world. Unlike theory in the physical sciences, game theory is almost wholly insensitive to the data produced in experimental and nonexperimental settings. When anomalies occur between theory and data as they have frequently over the past 30-plus years there may be a flurry of activity to conduct new experiments with new procedures that produce data consistent with theory; such a procedure asserts that the anomaly resulted from faulty experimental procedures and not faulty theory. There may be revisions to theory that are not fundamental such as devising new "solution concepts" that are still based on rationality assumptions and take a third party perspective. Most of the time there is no contact between game theory and data; the theory evolves without benefit of new, increasingly veridical empirical premises and data is collected without benefit of a useful theoretical framework.

We have discussed why the is-ought confusion arises, why it persists and why it is a serious problem. Our prescription for game theorists is compound but simple: understand the differences in the purposes of theories, be as specific as possible about the purposes of your theory, test the theory in accordance with its purposes, test in ways that invite theoretical failure so that you better understand the conditions under which the theory holds, acknowledge failures, and strive for more prescriptively useful theories and more predictively useful theories, recognizing that these useful theories are apt to differ from each other.

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