Colman's critique of classical game theory is correct, but it is well known. He misses one critique that I consider to be among the most telling. If two "rational" players play a game with a unique, strictly mixed strategy equilibrium, neither player has an incentive to play using this equilibrium strategy, because in a true one-shot game, there is absolutely no reason to randomize. It is easy to explain why one would prefer that one's opponent not know which action we will take, and it is possible to work this up into a full-fledged justification of randomizing. But in a true one-shot, your opponent knows nothing about you, so even if you choose a pure strategy, you do no worse than by randomizing. The evolutionary game-theoretic justification is that in a large population of agents meeting randomly and playing the game in each period, in equilibrium a fraction of the population will play each of the pure strategies in proportion to that strategy's weight in the mixed-strategy Nash equilibrium.

Indeed, most of the problems with classical game theory can be handled by evolutionary/behavioral game theory, and do not need models of "nonstandard reasoning" (Gintis 2000). For instance, in a pure coordination game with a positive payoff-dominant equilibrium, and the payoffs to noncoordinated choices zero, evolutionary game theory shows that each pair of coordinated choices is a stable equilibrium, but if there are "trembles," then the system will spend most of its time in the neighborhood of the payoff-dominant equilibrium (Young 1993).

As Colman notes, many of the empirical results appearing to contradict classical game theory, in fact contradict the assumption that agents are self-regarding. In fact, agents in many experimental situations care about fairness, and have a propensity to cooperate when others cooperate, and to punish noncooperators at personal cost, even when there can be no long-run personal material payoff to so doing. For an analysis and review of the post-1995 studies supporting this assertion, see Gintis 2003.

Evolutionary game theory cannot repair all the problems of classical game theory, because evolutionary game theory only applies when a large population engages in a particular strategic setting for many periods, where agents are reassigned partners in each period. We still need a theory of isolated encounters among "rational" agents (i.e., agents who maximize an objective function subject to constraints). Colman proposes two such mechanisms: team reasoning and Stackelberg reasoning. I am not convinced that either is a useful addition to the game-theoretic repertoire.

Concerning "team reasoning," there is certainly much evidence that pregame communication, face-to-face interaction, and framing effects that increase social solidarity among players do increase prosocial behavior and raise average group payoffs, but this is usually attributed to players' placing positive weight on the return to others, and increasing their confidence that others will also play prosocially. But these are nonstandard *preference* effects, not nonstandard *reasoning* effects. Choosing the payoff-maximum strategy in pure coordination games, where players receive some constant nonpositive payoff when coordination fails, is most parsimoniously explained as follows. If I know nothing about the other players, then all of my strategies have an equal chance of winning, so personal payoff maximization suggests choosing the payoff maximum strategy. Nothing so exotic as "team reasoning" is needed to obtain this result. Note that if a player *does* have information concerning how the other players might choose, an alternative to the payoff-maximum strategy may be a best response.

Moreover, "team reasoning" completely fails if the pure coordination game has nonconstant payoffs when coordination is not achieved. Consider, for instance, the following two-person game. Each person chooses a whole number between 1 and 10. If the numbers agree, they each win that amount of dollars. If the numbers do not agree, they each lose the larger of the two choices. For example, if one player chooses 10, and the other chooses 8, they both lose ten dollars. This is a pure coordination game, and "team reasoning" would lead to both players choosing 10. However, all pure strategies are evolutionary equilibria, and computer simulation shows that the higher numbers are less likely to emerge when the simulation is randomly seeded at the start (I'll send interested readers the simulation program). Moreover, if an agent knows nothing about his partner, it is easy to show, using the Principle of Insufficient Reason, that 2 and 3 have the (equal and) highest payoffs. So if an agent believes that partners use the same reasoning, he will be indifferent between 2 and 3. By the same reasoning, if one's partner chooses 2 and 3 with equal probability, then the payoff to 3 is higher than the payoff to 2. So 2 is the "rational" choice of "ignorant" but "rational" agents.

Colman argues that there is strong evidence supporting Stackelberg reasoning, but he does not present this evidence. Some is unpublished, but I did look at the main published article to which he refers (Colman & Stirk 1998). This article shows that in 2×2 games, experimental subjects overwhelmingly choose Stackelberg solutions when they exist. However, a glance at Figure 1 (p. 284) of this article shows that, of the nine games with Stackelberg solutions, six are also dominance-solvable, and in the other three, any reasoning that would lead to choosing the payoff-maximum strategy (including the argument from insufficient reason that I presented above), gives the same result as Stackelberg reasoning. So this evidence does not even weakly support the existence of Stackelberg reasoning. I encourage Colman to do more serious testing of this hypothesis.

I find the Stackelberg reasoning hypothesis implausible, because if players used this reasoning in pure coordination games, it is not clear why they would not do so in other coordination games, such as Battle of the Sexes (in this game, both agents prefer to use the same strategy, but one player does better when both use strategy 1, and the other does better when both use strategy 2). Stackelberg reasoning in this game would lead the players never to coordinate, but always to choose their preferred strategies. I know of no experimental results using such games, but I doubt that this outcome would be even approximated.

How to play if you must

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Abstract: Beyond what Colman is suggesting, some residual indeterminacy of Nash equilibrium may remain even after individual rationality is amended. Although alternative solution concepts can expand the positive scope (explanatory power) of game theory, they tend to reduce its accuracy of predictions (predictive power). Moreover, the appeal of alternative solutions may be context-specific, as illustrated by the Stackelberg solution.

Analysis of a strategic or noncooperative game presumes that the players are committed to participate. Normative analysis then aims at an unambiguous recommendation of how to play the game. If the analyst and the players adhere to the same principles of rationality, then the players will follow the recommendation; indeed, the players can figure out how to play without outside help. But can they? Like Colman, I shall refrain from elaborating on bounded rationality.

Andrew Colman presents the argument of Gilbert, that common knowledge of individual rationality does not justify the use of salient (exogenous, extrinsic) focal points to resolve indeterminacy. Nor does it justify endogenous or intrinsic focal points based on payoff dominance or asymmetry. This argument is in line with the critique by Goyal and Janssen (1996) of Crawford and Haller's heuristic principle, to stay coordinated once coordination is obtained. It applies as well to folk theorem scenarios, as in the infinitely repeated Prisoner's Dilemma Game (PDG): None of the multiple equilibria is distinguished on the grounds of individual rationality alone. The argument shows that principles other than individual rationality have to be invoked for equilibrium selection

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à la Harsanyi and Selten (1988), and for rationalizing focal points *à la* Kramarz (1996) or Janssen (2001b). Yet, even the addition of several compelling principles need not result in a unique solution for every game. For instance, in the Battle of the Sexes game, the equilibrium in mixed strategies is ruled out by payoff dominance, and there is no obvious way to select between the two equilibria in pure strategies. It seems that there always remains some residual indeterminacy – unless it is stipulated by law how to play certain games. Thus, the ambitious goal of orthodox game theory, broadly defined, to identify a unique solution for each game, has been almost, but not completely, reached.

But do players play as they should? As the author of the target article observes, it takes a further bridging hypothesis of weak rationality – that people try to act rationally – to turn the normative theory into a positive one. Then, as a rule, the recommendations of normative theory are treated as predictions. On a more fundamental level, the common knowledge and rationality (CKR) assumptions may be tested. Although I agree that the literature on experimental gaming testifies to the fruitfulness of empirical research, I would add that empirical research in industrial organization tends to rely on natural rather than laboratory experiments. This is worth noting, because economics, and in particular industrial economics, has been the main area of applied game theory and has immensely contributed to the development and proliferation of game-theoretical modeling.

Obviously, one would not necessarily observe the predicted outcome, if the participants played a game that was different from the one specified by the analyst or experimentalist. This would be the case if the monetary payoffs, or hypothetical payoffs according to the instructions, did not represent the subjects' preferences. Such instances are altruism or fairness considerations not accounted for in the original payoff functions. In such a case, the "neoclassical repair kit" can be applied, to use a popular, albeit somewhat derogatory, term: After a payoff transformation or, more generally, substitution of suitable utility functions for the original payoff functions, the data no longer reject the model. Thus, although the original model proved numerically mis-specified, the theory at large has not been rejected.

Yet, there are plenty of instances where the specified payoffs do represent player preferences, and orthodox and not-so-orthodox game theory is rejected in laboratory experiments. The first response to discrepancies between theory and evidence would be to perform further experiments, to corroborate or reevaluate the earlier evidence. After all, the immediate response to reports of cold fusion was additional experimentation, not a rush to revise theory. It appears that deliberate attempts at duplication are rare and poorly rewarded in experimental gaming. Still, certain systematic violations of individual rationality are abundant, like playing one's strictly dominated strategy in a one-shot PDG and the breakdown of backward induction in a variety of games.

In response to concerns rooted both in theory and evidence, game theory has become fairly heterodox. The recent developments suggest an inherent tension between the goals of explaining additional phenomena and of making more specific predictions (Haller 2000). Less stringent requirements on solutions can help explain hitherto unexplained phenomena. In the opposite direction, the traditional, or if you want, orthodox literature on equilibrium refinements and equilibrium selection has expended considerable effort to narrow the set of eligible equilibrium outcomes, to make more accurate predictions. Apart from the tradeoff mentioned, achieving a gain of explanatory power at the expense of predictive power, novel solution concepts may be compelling in some contexts and unconvincing under different but similar circumstances. One reason is that many experiments reveal a heterogeneous player population, with a substantial fraction evidently violating individual rationality, and another non-negligible fraction more or less conforming to orthodoxy. This raises interesting questions; for example, whether the type of a player is time-invariant or not.

Among the host of tentative and ad hoc suggestions falling under the rubric of psychological game theory, Stackelberg reasoning can explain specific payoff dominance puzzles, but yields detrimental outcomes when applied to other classes of Stackelberg solvable games. For instance, in a Cournot duopoly with zero costs and linear demand, the Stackelberg solution yields the perfectly competitive outcome, which is payoff-dominated by the Cournot-Nash outcome. Hence, the Stackelberg solution illustrates that the appeal of alternative solutions may be context-specific. Incidentally, a Stackelberg solution is a special case of a conjectural variation equilibrium. The latter concept can be traced back to Bowley (1924). It introduces a quasidynamic element into a static game. It has been utilized in models of imperfect competition and strategic trade from time to time, and has seen a revival recently. Despite its appeal, this modeling approach has been frequently dismissed on the grounds that it makes ad hoc assumptions and constitutes an unsatisfactory substitute for explicit dynamics.

Colman's article is thought-provoking and touches on several of the most pressing challenges for game theory, without pretending to be comprehensive or definitive. It will be fascinating to see which new theoretical concepts will emerge to address these challenges, and which ones will last.

What's a face worth: Noneconomic factors in game playing

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Abstract: Where behavior defies economic analysis, one explanation is that individuals consider more than the immediate payoff. We present evidence that noneconomic factors influence behavior. Attractiveness influences offers in the Ultimatum and Dictator Games. Facial resemblance, a cue of relatedness, increases trusting in a two-node trust game. Only by considering the range of possible influences will game-playing behavior be explained.

Whenever a game is played between two people, there are many potential motives for particular forms of behavior. One player may wish to impress or defer to the other. One may feel vindictive towards or sorry for the other player. Such motivations and others, in various combinations, can add many layers of complexity to a game-theoretic analysis of the payoffs. Where people behave in an apparently irrational manner, it is possible that their perception of the payoff does not equate to the economic one because of these other factors. Players may also use cues to predict the behavior of playing partners. For example, images of smiling partners are trusted more than those who are not smiling (Scharlemann et al. 2001).

The Ultimatum Game is one where behavior defies a simple payoff analysis (e.g., Thaler 1988). One player (the proposer) can allocate some proportion of a sum of money to the second player (the responder), who may accept or refuse the offer. If the offer is refused, the money is returned and neither player gets anything. Usually the game is played single-shot, where the players do not know or even see each other. A payoff analysis suggests that any offer should be accepted, but in typical western societies anything less than about 35% is refused. This is usually explained as enforcement of "fair play" by the responder. In the related Dictator Game, the second player has no choice. Now, the first player is free to offer nothing, but in practice, usually does make some offer. It appears that something inhibits purely selfish behavior. The situation is more complicated when the players know something of each other, as the other kinds of factors mentioned above may affect decisions.

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