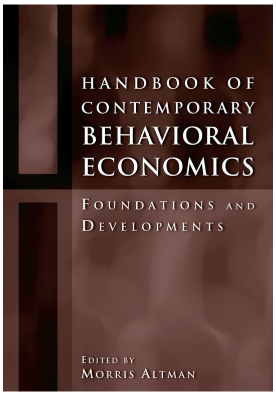

Handbook of Contemporary Behavioral Economics Foundations and Developments

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A Behavioral Approach to Distribution and Bargaining

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Chapter 20

A BEHAVIORAL APPROACH TO DISTRIBUTION AND BARGAINING

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Canonical game theory (as codified in textbooks such as Kreps 1990a, 1990b and Mas-Colell, Whinston, and Green 1995) requires unlimited cognitive and information-processing capabilities. It is obvious that these requirements are at odds with what humans are equipped with or typically have at their disposal.[1] Cowan (2001), updating the message of a famous paper by Miller (1956), has summarized the available evidence and argues that people can remember on average about four chunks of information. Given such cognitive capacity constraints, only a very small set of games can be analyzed ("solved") in accordance with canonical game theory by real people.

Canonical game theory's solution concepts for a given class of games—for example, the class of finite games in normal form or extensive form—is largely based on invariance or covariance with respect to certain sets of transformations, and thus partitions the class of games into *equivalence classes*.[2] Two games from the same equivalence class are said to be strategically equivalent. Most solution concepts, for example, allow for positively affine utility transformations. Experimentally, one can try to induce such transformations by scaling up or down the monetary payoffs ("stakes") appropriately. These changes, which according to canonical game theory ought to be irrelevant, can nevertheless change participant behavior quite dramatically (e.g., Smith and Walker 1993; Hertwig and Ortmann 2001a, 2001b, 2003; see also Laury and Holt 2002 and Harrison et al. 2005 for studies that make a similar point regarding decision making).

Even if we do not transform a game at all but present (or frame) the same game differently, behavior may still react to the change (presentation/framing effects). Consider, for instance, the prisoner's dilemma game, played once or repeatedly, which has been a dominant paradigm of experimentation (Colman 1982, 1995) because it captures succinctly the possibility that individual rationality might contradict social welfare, at least for one-shot or finitely repeated games. This, of course, stands in sharp contradiction to Smith's famous dictum "It is not from the benevolence of the butcher, the brewer, or the baker, that we expect our dinner, but from their regard to their own interest" (Smith 1976, 22).

In the prisoner's dilemma game, if defection always leads to the same payoff advantage when compared to the cooperative strategy (i.e., regardless of what the other player chooses), one can decompose the same game in infinitely many ways by describing for each individual choice how much it grants to the other and how much the individual player assigns to himself. (Unlike a transformation, a decomposition does not change the game.) This leads to a one-parameter family of decomposed prisoner's dilemma games that do not question that the same prisoner's dilemma game is played. Nonetheless, average cooperation rates react quite dramatically to decomposition (Pruitt 1967).

For closely related public good provision problems, Andreoni (1995a) has demonstrated that their positive or negative framing can affect cooperation rates dramatically. Andreoni (1995b) has furthermore demonstrated, also for public good provision games, that what looks like kindness is often, and to a significant degree, subject confusion. McCabe, Smith, and LePore (2000) and Cooper and Van Huyck (2003), building on earlier results (e.g., Schotter, Weigelt, and Wilson 1994), have demonstrated for a variety of games that presenting a game in normal form or extensive form can make a significant difference.

Note that, strictly speaking, in an experiment a commonly known finite upper bound for the number of repetitions cannot be avoided and should be commonly known. If one accepts this reasoning, folk theorems do not apply, and pervasive mutual defection, induced by backward induction, is the solution proposed by canonical game theory (e.g., Mas-Colell, Whinston, and Green 1995 proposition 9.B.3) in repeated prisoner's dilemma games, and in fact in all kinds of social dilemma games such as public good provision or common pool exploitation problems.[3] The same result applies also to asymmetric games of the principal-agent or gift exchange variety (e.g., Fehr, Kirchsteiger, and Riedl 1998).

The robust results, however, of many experiments are that players *cooperate in most rounds,* even when the final round is known and does not have to be inferred, although they defect toward the end (e.g., Selten and Stöcker 1986). Since many participants try to avoid being preempted (meaning that their partner terminates cooperation earlier), they seem to be aware of the backward induction idea. Because of its detrimental consequences, however, they do not follow its recommendations but rather account for it only when the end of interaction is near. Not relying on canonical game theory can be a good idea.

Since folk theorems do not apply in games that are finitely repeated (in some commonly known way; see Neyman 1999), these results have posed quite a puzzle for game theorists and have inspired the innovative *reputation approach* (or "crazy perturbation"; see Kreps et al. 1982).[4] The basic idea is to allow for (a little) incomplete information concerning another player's type: in repeated prisoner's dilemma games he or she may be an unconditional cooperator (i.e., a bit "crazy"). In this way one can try to build up the impression (the other's posterior probability) of an unconditional cooperator. Furthermore, the a priori probability of the other person being "crazy," necessary to justify initial cooperation, can be small when the number of iterations is large.

Although the reputation approach has been rather successful (in the sense of inspiring a large literature), some qualitative aspects of reputation equilibria are supported only poorly, if at all, by experimental data (e.g. McKelvey and Palfrey 1992; Anderhub, Engelmann, and Güth 2002). These include the possibly gradual decline in the probability of cooperation, leading to certain defection in the last period (some participants, for instance, cooperate in the last round), and the specific mixing (the change of mixed strategies over time).

Nevertheless, reputation equilibria illustrate how canonical (game) theory can be enriched by paying attention to robust experimental findings. Reputation equilibria do not question rationality itself, only the idea that rationality is clearly expected in participants. Other applications concern trust, bargaining, and signaling games and, most of all, the classic paradigms of the industrial organization literature.

EXPERIMENTAL RESULTS IN DISTRIBUTION GAMES

Following decades of experimental research on prisoner's dilemma and public good provision problems (much of it done by psychologists, as documented in Colman 1982), in the early 1980s researchers became concerned with deceptively simple models of distribution. Just as in the earlier experiments on prisoner's dilemma and public good provision problems, the experimental results of these simple models of distribution stood in stark contrast to the predictions of canonical game theory, especially in those cases where the predicted distribution was considered unfair. One of these games, the so-called ultimatum game, "is beginning to upstage the PDG in the freak show of human irrationality" (Colman 2003, 147). It is probably no coincidence that this new workhorse of experimental research is a sequential (and asymmetric) game rather than a simultaneous (and symmetric) game.

In ultimatum experiments (Güth, Schmittberger, and Schwarze 1982; see Güth 1976 for an earlier discussion) a positive sum p of money, the "pie," can be distributed by first allowing the proposer to decide on his or her offer o with $0 \leq o \leq p$ to the responder, who can then either accept the offer o (so that the proposer gets $p - o$ and the responder o) or reject it (in which case both players get nothing).

The solution of canonical game theory (assuming that both players care only about their own monetary payoff) predicts that the proposer offers 0 (or the smallest positive monetary unit) and that the responder accepts all (positive) offers. Typical experimental findings (see Camerer 2003, chaps. 1 and 2, for a recent survey), however, are that responders reject even substantial positive offers o in the range $0 < o < p/2$, which they apparently regard as unfair, and proposers shy away from excessively low offers o; the most frequent (modal) offer is usually the equal split $o = p/2$.

These results are quite robust along a number of dimensions such as the financial incentives that subjects face and demographic variables such as gender, race, academic major, and age (Camerer 2003, chap. 2, especially Tables 2.3 and 2.2); they even seem robust across countries and cultures (see Roth et al. 1991, but see also Henrich et al. 2001, which documents exceptions claimed to be due to individual heterogeneity, noise, and culture-specific socialization; for a critique of the experimental procedures employed in that research, see Ortmann 2005). To justify the offer $o = p/2$ instead of $o = \varepsilon$ (with ε denoting the smallest positive unit of money), the proposer must be extremely risk-averse or the responder probably irrational or "crazy," e.g., in the sense of infinite inequity aversion.

A similar challenge, for experimentalists and theorists alike, is provided by the results of so-called dictator games, which were earlier and much more adequately studied in social psychology as reward allocation experiments guaranteeing entitlements (see Shapiro 1975; Mikula 1973). In dictator game experiments (e.g., Forsythe et al. 1994), a positive sum p of money, the "pie," can be distributed by allowing a dictator to decide on his or her offer o with $0 \leq o \leq p$ to the recipient. Here the recipient is just that. He or she cannot veto the proposer's allocation, effectively eliminating the strategic interaction of the ultimatum game. Strictly speaking, the dictator game is not a game.

The solution of canonical game theory for this allocation problem (assuming that the dictator cares only about his or her own monetary payoff) predicts that the dictator offers 0. Importantly, the outcome of canonical game theory for this allocation problem thus coincides with the prediction for the ultimatum game. Typical experimental findings (Camerer 2003, chap. 2, especially Table 2.4), however, find dictators making significant allocations in the range $0 < o < p/2$, although offers on average are clearly lower than in the ultimatum game. Specifically, the equal split $o = p/2$ is no longer the modal offer.

Allocations in the dictator game (as already indicated in Forsythe et al. 1994; see the pay versus no-pay conditions, or the different results elicited by the stakes) have been less robust. Two studies stand out. Hoffman, McCabe, and Smith (1996) studied how social distance (in the form of various anonymity conditions) affected allocation and found that social distance is inversely related to the generosity of offers. In a double-blind treatment meant to control for experimenter effects, the modal response coincided with the prediction of canonical game theory, with about 40 percent, however, still sharing some of the wealth that was bestowed on subjects through the experimenter. More recently, Cherry, Frykblom, and Shogren (2002) demonstrated that the very nature of the wealth—whether it was handed down as manna from heaven, as is typical for almost all (economic but not psychological reward allocation) experiments, or had to be earned—dramatically affects allocation. When wealth had to be earned, 80 to 95 percent of dictators—dependent on the degree of anonymity—followed the game-theoretic prediction. This is a remarkable result because the prediction of canonical game theory for the dictator game is a boundary point that does not allow for noise in the form of subject confusion. While with the benefit of hindsight (e.g., calling back into memory the results of Harrison and McCabe 1985, or Güth and Tietz 1986) this result is not that surprising, it is troubling given standard experimental practices. The key question is whether indeed, as Cherry, Frykblom, and Shogren (2002) claim, their procedure gives us more external validity than the one currently used. If indeed these authors' claim is true, then it would constitute a very damaging critique of the literature on dictator games as well as related literatures such as that on public good provision experiments (e.g., Ledyard 1995, on the verdict of which, regarding the alleged failure of hard-nosed game theory, Cherry, Frykblom, and Shogren 2002 seems to allude to with its title).

A related study for ultimatum games (List and Cherry 2000) was concerned with learning in low- and high-stakes environments and a critique of an earlier high-stakes ultimatum experiment (Slonim and Roth 1998). It did find a downward shift in offers compared to other studies in both conditions (but not nearly as much as the downward shift in dictator game). That seems to have been a rational decision of sorts on the part of proposers, as proportionally smaller offers were rejected more often than larger offers in both in the low- and high-stakes environment. A closer look at the design and implementation suggests that the nature of the earned income was not common knowledge. In fact, responders were simply told that proposers had "earned an amount of money by participating in a previous session." This description must have left open many questions in the responders' minds about how much proposers had to work for their wealth, doubts that proposers very likely anticipated to some extent. It would be interesting to see how the distributions of offers and rejections would shift if indeed the nature of the task—a quiz consisting of seventeen questions taken from the sample section of the Graduate Management Admissions Test—would be common knowledge. More fundamentally, entitlement in the proper sense would have to be based on contributions relevant to the role in the game, similar to the practice of reward allocation experiments. Güth and Tietz (1986) have tried to guarantee this by auctioning the positions in a game.

Apart from testing the robustness of aspects of the experimental design and implementation that according to canonical game theory should not play a role, researchers have also chosen to study related or richer game models hoping that their experimental results add to our understanding of the reasons why proposers usually offer rather fair shares and responders are unwilling to accept meager offers.

One can generalize, for example, the ultimatum game by assuming that nonacceptance of the offer o implies the conflict payoff $\rho\,(p - o)$ for the proposer and λo for the responder with $0 \leq \rho$, $\lambda \leq 1$. The ultimatum game corresponds to $\rho = 0$ and $\lambda = 0$, whereas $\rho = 1$ and $\lambda = 1$ represent dictatorship (the responder has lost all veto power). Similarly, one can study $\rho = 1$ and $\lambda = 0$, the so-called impunity game (for experimental studies of "corner-point games" see Bolton and Zwick 1995 as well as Güth and Huck 1997) but also "interior games" such as $0 < \rho = \lambda < 1$ (see

Suleiman 1996) or $0 < \rho = 1 - \lambda < 1$ (see Fellner and Güth 2003). One general conclusion from this research is that behavior strongly depends on how efficiently the responder can punish the proposer.

One can also combine aspects of ultimatum bargaining and dictatorship. If one includes, for instance, a dummy player in addition to the proposer and the responder, an ultimatum proposal would consist of two offers, o_R and o_D with $o_R, o_D \geq 0$ and $o_R + o_D \leq p$, meaning that the proposer offers o_R to the responder R and o_D to the dummy D and wants to keep $p - o_R - o_D$ and that rejection by R implies 0-profits for all (for experimental studies, see Güth and van Damme 1998; Brandstätter and Güth 2002; Güth, Schmidt, and Sutter forthcoming). The fact that (according to the results of Güth and van Damme 1998) o_D was usually much smaller than o_R and no rejection by R could be attributed to an embarrassingly low asymmetric o_D alone seems to suggest that neither proposers nor responders have a strong intrinsic concern for fairness (see also Bolton and Ockenfels 1998, which explains this by inequity aversion).

Another interesting twist on ultimatum bargaining and dictatorship has been provided by so-called trust experiments. The quickly exploding literature on the trust (or investment) game was initiated by Berg, Dickhaut, and McCabe (1995).[5] In the game (for an early discussion see Kreps 1990a) a proposer makes an initial investment that, on its way to a responder, gets multiplied by a factor greater than 1. The responder then decides how much of what he or she receives—this is the dictatorship aspect of the trust game—will be sent back to the sender (proposer). The prediction of canonical game theory is that the proposer—correctly anticipating that the responder would not return anything—would not invest a thing. The trust game thus predicts an extreme form of underinvestment due to the holdup problem (see Malcolmson 1997).

Experimental results, however, have shown significant investments and returns, with the modal investment being about half of the original endowment and the average return being about what has been invested (but not much more). This result has been shown to be rather robust under a variety of experimental manipulations (Ortmann, Fitzgerald, and Boeing 2000; see Camerer 2003, ch. 2, for a good review of that literature, revealing a striking heterogeneity of subject behavior, and Bolle and Kaehler 2003 for a methodological critique of parameter selection in trust games).

Very recently Cox (2004) has provided a fundamental critique of this research program by pointing out that trust (on the part of the proposer) and reciprocity (on the part of the responder) are not the only candidates for explanation of the fairly robust experimental results in trust games (and the theory developments these results have spawned). Let us define "other-regarding preferences" as preferences that are altruistic (e.g., Andreoni and Miller 2002), inequality-averse (Bolton and Ockenfels 2000; Fehr and Schmidt 1999), quasi-maximin (Charness and Rabin 2003), or maybe even malevolent (Kirchsteiger 1994). Then the behavior of the responder may be reciprocal (i.e., the responder may react to an investment of a proposer), or it may be altruistic, inequality-averse, or whatnot. Anticipating such responder motivations, a proposer may then invest an amount even if he or she has no other-regarding preferences whatsoever. Such investment behavior would reflect trust, or at least the rational expectation that on average other-regarding behavior of responders is likely not to make a reasonable amount of investment a losing proposition.[6] As a matter of fact, that seems about true (see, e.g., the review in Bolle and Kaehler 2003).

Using a triadic design (i.e., comparing giving behavior in the first stage of the trust game with that in a dictator game meant to control for unconditional other-regarding behavior of first movers, and comparing giving behavior in the second stage of the trust game with that in a dictator game meant to control for unconditional other-regarding behavior of second movers), Cox (2004) attempts to separate reciprocity from altruism or inequality aversion, and trust from altruism. He finds significant amounts of trusting behavior and reciprocating behavior but also significant amounts of altruism and/or inequality aversion. Regarding the first stage of the trust game, trust explains about 60 percent of the investment behavior that Cox finds in his study, while other-regarding behavior might explain the rest. Regarding the second stage of the trust game, reciprocity explains about 60 percent, with the rest possibly being explained by other-regarding behavior. Of course, given that the prediction of canonical game theory is a corner-point solution, other explanations (e.g., subject confusion, or curiosity of subjects about what happens if they invest small amounts) are possible.

The experimental results of the very elementary bargaining procedures captured by ultimatum bargaining and the trust game provoked a lively debate among game theorists as to whether or not canonical game theory is just a normative exercise that has little value in application. The simplicity of the tasks suggests that cognitive limitations are not the problem.[7] Putting aside legitimate questions about the implementation of experiments (e.g., the question of earned assets), it seems that the game-theoretic concept of subgame perfect equilibrium points is not descriptively satisfying. As in decision tasks, the question is whether rationality explains experimental behavior (at least by experienced participants) or whether canonical game theory has to be supplemented by a behavioral theory. A premier candidate for a satisfying explanation seems to be social preferences.[8] Such an explanatory strategy, however, poses the question of how such idiosyncratic other-regarding preferences can ever become commonly known (at least probabilistically).

EXPERIMENTAL RESULTS IN (ALTERNATING OFFER) BARGAINING GAMES

One typical reaction to striking experimental findings is to ask how the results would change when the theoretical and experimental setup is enriched. In the case of ultimatum bargaining, it has been argued that fairness may matter less when parties are not limited to only one negotiation round for reaching an agreement. The guiding model for this line of experimental research is that of alternating offer bargaining (e.g., Rubinstein, 1982). In odd rounds t player 1 offers and player 2 responds; in even rounds t the roles are reversed. Agreement is achieved if an offer is accepted. Otherwise one proceeds to the next round (except in the last round, when nonacceptance means conflict, implying zero payoff). Assume, for example, that T, the number of the last round, is a large odd integer (player 1 is the last proposer) and that the same pie p can be divided, regardless of the round $t = 1,\ldots,T$ in which an agreement is achieved (the closest approximation seems to be Güth, Levati, and Maciejovsky 2005). The solution outcome is, of course, the same as in the case $T = 1$. In an experiment, however, participants might learn from unsuccessful offers in earlier periods $t < T$ how issues of fairness matter. Yet the usual assumption in experimental studies has been that delaying agreement is costly (i.e., there are risks posed by a shrinking pie).

There are now several experimental studies of alternating offer bargaining (see Roth 1995 for a survey) that vary the time preferences involved (for example, in the form of equal or unequal discount factors) and the horizon (the maximum number of rounds). The latter is, of course, finite, although one study (Felsenthal, Weg, and Rapoport 1990) tries to create an illusion of an infinite horizon. Other studies (Güth, Ockenfels, and Wendel 1993; Anderhub, Güth, and Marchand 2004) assume that every periodic proposer can declare his or her offer to be an ultimatum, and that the pie p is either increasing or decreasing or even varying nonmonotonically. As in a centipede experiment, both participants here may gain by trusting (i.e., by not terminating early). The explanation of the centipede results in terms of (expected) altruism (in the tradition of reputation equilibria; see McKelvey and Palfrey 1992) cannot account, for instance, for the increasing pie results.

More recently, Johnson, Camerer, Sen, and Rymon (2002) have provided us with an intriguing study that addresses both cognitive limitations and social preferences in insightful ways. The game that they study is a three-round bargaining game where the initial subgame perfect equilibrium offer was \$1.25 and the equal split ("fair") solution was \$2.50. The study is remarkable both for the various treatments that try to insulate the relative contributions of cognitive limitations and social preferences and for the technology used (Mouselab). This technology (which has recently also been used to study normal form games and depth of reasoning, e.g., Costa-Gomes, Crawford, and Broseta 2001) allows the researchers to track the patterns of information acquisition and then to make inferences about the thought process (e.g., to what extent and under what conditions subjects engage in backward induction). The authors study four treatments. The first is a baseline treatment meant to assure the reader that there is nothing about the subject pool of the implementation that is idiosyncratic. Indeed, in this baseline treatment offers average \$2.11, with offers below \$1.80 being rejected half the time. These results replicate earlier results. The Mouselab data make it very clear that subjects do not tackle the problem in a way that would please a game theorist (i.e., by thinking through the problem from the back). In the second treatment, the authors turn off social preferences and let subjects play against robots that they know are programmed in the way that would please a traditional game theorist. While average offers are lower (\$1.84), they remained way off the prediction of canonical game theory, as did the frequent (especially in the first couple of rounds) rejections. In a third treatment, the authors taught their subjects about backward induction, which caused them to make offers to robots that essentially coincided with the prediction of canonical game theory. Of course, providing such commonly known behavior may have made it look like an indoctrination test. In a final treatment, Johnson and his colleagues let untrained and trained subjects fight it out, with this tug-of-war resulting in meeting roughly halfway.

Whereas the models underlying these experimental tests rely on asymmetric bargaining rules, among the symmetrical bargaining models the so-called demand game (Nash game) has received the most attention (Nash 1950, 1953). Here all parties simultaneously choose their demands, which are what they obtain whenever the vector of demands is feasible; otherwise they receive their conflict payoffs. An interesting study (Roth and Malouf 1979) applies the binary lottery technique when studying demand bargaining (allowing, however, for several rounds of simultaneous demands). Parties can earn individual positive monetary prizes and bargain only about the probability of winning their prize (with complementary probability, the other party wins its prize). What is varied systematically is the information available about the other's prize. When prize information is completely private, parties usually agree on equal winning probabilities. If both prizes are generally known, parties often choose winning probabilities that equate their monetary expectations. This, of course, contradicts the axiom of independence with respect to affine utility transformations.

Due to the usually large number of strict equilibria (all efficient vectors of demands exceeding conflict payoffs), participants in the demand game face an additional coordination problem that might justify introducing preplay communication or more strategic possibilities. The main findings are that the (Nash) bargaining solution maximizing the product of agreement dividends must be focal (e.g., as a corner point of a piecewise linear utility frontier) to be selected and that the (Nash) axioms, although normatively convincing, are behaviorally questionable. Experimentally, the monotonicity axiom (Kalai and Smorodinsky 1975) is better supported.

CHARACTERISTIC FUNCTION EXPERIMENTS

Experimental game theory, like game theory, was dominated at first by characteristic function models. A characteristic function for a cooperative game describes for every nonempty subset—that is, coalition C of the player set $N = \{1, \ldots, n\}$ of, say market participants—the sum of profits of the members of C if side payments are possible. A good example for a coalition is a more or less complete cartel, for example, on a market.

It is not at all clear a priori how to implement a given characteristic function as an experiment (it is not, after all, a strategic game). The usual procedure is to permit free face-to-face communication and to let coalitions announce payoff agreements, which become binding if no coalition member withdraws within a certain number of minutes.

Given the usual heterogeneity in individuals' behavior, value concepts were rarely used, although they may become important in accounting experiments; among these, reward or cost allocation experiments (Mikula 1973; Shapiro 1975) may offer early but (too) simple precedents. But in most studies (see Sauermann 1978a, 1978b) the well-known set solutions, such as the core, internally stable, and externally stable (von Neumann and Morgenstern 1947) solution sets or the various bargaining sets, were tested, or new related concepts developed.

Robust results (see Selten and Uhlich 1988; Sauermann 1978a, 1978b) are that:

- Players in the same coalition obey the power structure (by granting a more powerful coalition member at least as much as a less powerful one).
- Equal payoff distributions are frequently proposed and often are used as counterproposals when trying to argue against a previous proposal.
- Coalitions smaller than the grand coalition are formed, even when they are inefficient.

Characteristic function experiments were performed not only by game theorists but also by (social) psychologists. A typical situation is to rely on majority voting games $(w_1, \ldots, w_n; m)$ where w_i with $0 \le w_i \le 1$ and $w_1 + \cdots + w_n = 1$ denotes the voting share of player $i = 1, \ldots, n$ and m with $½ \le m \le 1$, mostly $m = .5$, the majority level which a winning coalition S with $\sum_{i \in S} w_i > m$ must obtain. The characteristic function $v(\cdot)$ allowing for side payments assumes $v(S) = 1$ if S is winning and $v(S) = 0$ otherwise. In case of $n = 3$, $m = .5$, and $w_1 = .49$, $w_2 = .39$, $w_3 = .12$, one has $v(\{i\}) = 0$ for $i = 1, 2, 3$, and $v(S) = 1$ for any coalition S with at least two members. Thus the power structure, as reflected by the winning coalitions, is completely symmetric despite the large differences in voting shares. In such a situation, experimentally observed payoff distributions are often influenced by both the power structure and the voting shares (see Komorita and Chertkoff 1973).

Due to the dominance of strategic models in the industrial organization literature, characteristic function experiments became a less popular research topic. Since strategic models seem to account for every possible result without any serious restrictions on what to assume (see the discussion of repairs, below), there may, however, be a revival of characteristic function experiments for special situations where cooperative solutions are informative, for example, in the sense of a small but nonempty core. The main advantage would be that such informative solutions do not depend on subtle strategic aspects that are behaviorally irrelevant but crucial for the noncooperative solution. An example is the sequential timing of moves in ultimatum bargaining, whose characteristic function is, however, symmetrical in the sense of $v(\{i\}) = 0$ for both players i and $v(N) = p$ for the grand coalition N consisting of both players.

EXPERIMENTS ON (DE)CENTRALIZATION IN WAGE BARGAINING

Several experiments on wage bargaining are concerned with the problem of centralization in bargaining. These experiments were motivated by the empirical results of Calmfors and Driffill (1988), which seem to show that the degree of centralization of wage bargaining procedures in an economy has an impact on macroeconomic performance: Countries with a low level of centralization (e.g., the United States and Canada) or a high level of centralization (e.g., Austria and Sweden) are characterized by low wage levels, while countries with a moderate level of centralization (e.g., Germany) have high wage rates. The opposite relation holds for the degree of centralization and the unemployment level. Up to now a satisfactory theoretical explanation of this phenomenon has been missing. In experiments on centralized versus decentralized bargaining (Berninghaus et al. 2001; Berninghaus, Güth, and Keser 2003) it was investigated whether a tendency to centralized bargaining can be observed at all when trade unions have the choice to centralize.

Berninghaus, Güth, and Keser (2003) assume three players, *X, Y,* and *Z.* These players can negotiate either in a decentralized way or collectively. In decentralized bargaining, *X* negotiates with *Z* about the allocation of a pie P_{XZ}, and, independently, *Y* negotiates with *Z* about the allocation of a pie P_{YZ}. In the case of collective bargaining, *X* and *Y* merge into a new player, *XY*, who then bargains with *Z* about the allocation of the total pie $P_{XYZ} = (P_{YZ} + P_{XZ})$. Whatever *XY* earns is shared by *X* and *Y*. Let *i* and *j* denote one of the two bargaining parties; that is, (i, j) is either (X, Z) or (Y, Z) or (XY, Z). A modified Nash demand game is applied: Each of the two parties $k = i, j$ chooses a demand D_k and a bottom line B_k with $P_{ij} \geq D_k \geq B_k \geq C_k$, where C_k (≥ 0) denotes the conflict payoff of party *k*. Given the vector (D_i, B_i, D_j, B_j) of bargaining choices and the size of the pie P_{ij}, a demand agreement is reached if $D_i + D_j \leq P_{ij}$. A bottom-line agreement is reached in case of no demand agreement and $B_i + B_j \leq P_{ij}$. While both parties $k = i, j$ obtain their demand D_k in case of a demand agreement, their profits are determined by their bottom lines B_k in case of a bottom-line agreement. If neither of these two agreements is achieved, the two parties end up in conflict, with conflict payoffs C_k.[9]

Conflict payoffs C_k depend on the pairing (i, j); therefore, we write $C_k(i, j)$. It is assumed that $C_Y(Y, Z) > C_X(X, Z)$ holds, that is, *Y* is stronger than *X*.

To solve this game theoretically, note that the acceptance borders are the (only) essential strategic variables. Obviously, in an efficient equilibrium the bargaining parties must choose $B_i + B_j = P_{ij}$. To select a unique efficient equilibrium outcome as a benchmark solution, one relies on the Nash bargaining solution, which maximizes the product of the dividends $(B_k - C_k)$ for $k = i, j$. For example, for the pair $(i, j) = (X, Z)$ we maximize $(B_X - C_X(XZ))\ (B_Z - C_Z(XZ))$ subject to $B_X + B_Z = P_{XZ}$. Since the stronger party *Y* has no interest in forming *XY,* condition $B^*_Y > B^*_{XY}/2$ had to be satisfied by the solution choices. Of the three players only *X* has positive incentives for centralizing. The benchmark solution thus predicts decentralized bargaining. However, the experimental results suggest that centralization helps. This might reflect a common experience or belief that one gains in strength by merging, based on factual or expected synergy. This sometimes finds expression in phrases such as "Unity is strength" or, in German, "*Einigkeit macht stark*." Players also might view (the choice of) centralization as signaling "I am tough."

CONCLUSION

We have documented various reactions to the sometimes striking results of experimental tests of the sharp predictions of canonical game theory for simple distribution and bargaining games. Roughly, these reactions can be classified as follows.

First, researchers have extended their study of deceptively simple games such as dictator and ultimatum games to somewhat more complicated games such as trust or alternating offer games. These studies, as the examples of Cox (2004) and Johnson and colleagues (2002) demonstrate in an exemplary manner, have inspired interesting new questions about the importance of cognitive limitations and the impact of social preferences. The Johnson and colleagues study (2002) is of particular interest, as it introduces economists to a noninvasive technique that allows us to better understand (through comparison for look-up patterns of information in various treatments) the reasoning process of subjects. In a related study, Costa-Gomes, Crawford, and Broseta (2001) have applied this technique to identify reasoning types and processes in normal form games. This, in turn, has generated interesting new theorizing attempts for situations that match asymmetrically endowed players (Crawford 2003).

Second, researchers have tried to "repair" the representation of the experimental situation ("game fitting"), for example, by assuming that utilities depend not only on profits but also on their distribution, on a desire for reciprocity, or on what one participant thinks is expected by other(s). These repairs do not question rationality. Since nearly all results can be "saved" in this way, repairs should be at least reasonable and intuitive. For instance, it is obvious that we often care about the distribution of rewards, but when and why we do so is currently poorly understood. Consider, for instance, models of social preferences (e.g., Bolton and Ockenfels 2000; Fehr and Schmidt 1999) that are meant to incorporate what the experimental studies seem to suggest: that people not only are considering own payoffs but also react to what others get. Such concerns are very obvious in close interaction situations (e.g., work teams) but very unlikely when shopping in a supermarket. What is thus required is a kind of cognitive switch that (does not) trigger(s) other-regarding concerns. The same applies to models of intentionality (Charness and Rabin 2003; Dufwenberg and Kirchsteiger 2004; Falk and Fischbacher 2001).

Other researchers (e.g., McKelvey and Palfrey 1995, 1998; Goeree and Holt 2001; Camerer, Ho, and Chong 2004; see also Reny 1992) allow for noise in decision behavior partly in the sense that we rationally anticipate such noise (which might be trembles or indeed altruism) and react optimally to it. While these new models enrich our understanding of cognitive limitations and social preferences, no model addresses successfully where social preferences come from and how boundedly rational decision makers take them into account. Rather, they are just postulated to exist. There exists, however, a rich literature on preference evolution, usually employing the indirect evolutionary approach (see Samuelson 2001 and the collection of articles in that special issue of the *Journal of Economic Theory*), providing some underpinning for which social concerns can be expected to evolve in certain environments.

Third, researchers have tried to understand whether features of standard experimental procedures have contributed to these results. Three developments deserve particular attention. One troubling aspect, as illustrated by the study of Cherry, Frykblom, and Shogren (2002) but also by the tradition of reward allocation experiments in psychology (e.g., Mikula 1973; Shapiro 1975), is the question of the external validity of subject payments that are bestowed on subjects like manna from heaven. Another troubling aspect, as illustrated by Hoffman, McCabe, and Smith (1996) but also by a huge literature in social psychology on expectancy effects (e.g., Rosenthal and Rubin 1978; Rosenthal and Rosnow 1991, 119–25, 128–33; Ortmann 2005), is the potential of experimenter effects. These concerns are of differential importance for various classes of games: they are not likely to play a role, for instance, in guessing games (Nagel 1995), but they warrant concern in distribution and bargaining games. The studies just mentioned, as well as innovative studies such as the one by Johnson and colleagues (2002) go a long way toward a better understanding of the impact of experimental design and implementation and why we see sometimes dramatic deviations from the predictions of canonical game theory.

Yet another troubling aspect is experimental economists' urge to get rid of social context in most of their experiments. There is mounting evidence that this experimental practice, which originally was meant to increase control, often does just the opposite (Ortmann and Gigerenzer 1997). Very simply put, the abstract nature of experimental goods and environments often does not allow subjects to access the inference machines that typically allow them to navigate their "habitats" just fine (e.g., Cosmides and Tooby 1996; Gigerenzer, Todd, and the ABC Research Group 2000). Of course, introducing field referents in various forms in the design and implementation of experiments runs the risk of prompting associations and interpretations of the experimental situation that may be incorrect, a risk that accounted for the usual practice in experimental economics. The advantages and disadvantages of each of these methods are currently poorly understood, although economists can surely learn a thing or two from similar debates that took place in psychology decades ago. A well-known example is memory research, where much of traditional laboratory research initially followed Ebbinghaus (1885) in conducting tightly controlled experiments using even nonsense syllables in an attempt to enhance control. This research paradigm was eventually questioned (e.g., Neisser 1978; Koriat and Goldsmith 1996a; see also Koriat and Goldsmith 1996b, which is of particular interest to experimental economists and psychologists). Closely related to the issue of what subjects bring to the laboratory and what they learn in the laboratory is the progress in developing software packages for computerized experiments. It has inspired a new experimental tradition: participants play the same base game (e.g., a 2-by-2 bimatrix game) repeatedly with randomly changing partners. This research tradition is too recent to permit any general conclusions about how people adapt to past experiences and how such path dependence is combined with undeniable strategic deliberation. One rather robust result is that behavior in two-person coordination games converges to strict equilibria, but not necessarily to the payoff-dominating strict equilibrium (see Camerer 2003). It is striking, however, to observe how closely theoretical exercises of adaptive dynamics and experimental studies are related to each other (e.g., Costa-Gomes, Crawford, and Broseta 2001). If the same simple game is played very frequently, boredom might lead players to seek variety. In studies of robust learning (see Güth 2002 for a selective account), where participants confront repeatedly a variety of related games instead of just one such game, this seems less likely, however.

The present authors disagree on the relative importance of what the experimental evidence tells us. Güth sees them as having established a persuasive case for a descriptive theory. Ortmann argues that more attention ought to be paid to experimental design and implementation issues and the question of the external validity of the (sometimes admittedly striking) laboratory results of distribution and bargaining experiments. That paying attention to experimental design and implementation is a worthwhile enterprise is, to Ortmann's mind, superbly documented in the controversy over the epistemic value of the heuristics and biases program, which reigned supreme in psychology for decades before serious questions were asked about the way the alleged biases had been produced and the heuristics had been formulated (e.g., Gigerenzer 1991; Gigerenzer 1996; Gigerenzer et al. forthcoming; Koehler 1996; Krueger and Funder 2004; Ortmann and Ostatnicky 2004). Ortmann sees in the results of Hoffman, McCabe, and Smith (1996), Cherry, Frykblom, and Shogren (2002), and Johnson and colleagues (2002) and in the emerging debate over the artificiality of our laboratory settings (e.g., List 2004; Harrison and List 2004; Carpenter, Harrison, and List 2005) evidence of methodological problems that warrant more attention than economists have accorded them so far. He also believes that there is a good chance that many of the striking results documented in the literature may be laboratory artifacts in that they are striking only when measured against the predictions of canonical game theory. Ortmann and Hertwig (2000) have pointed out that these striking deviations are overwhelmingly found in social dilemma games of various makes and that their outcomes can be easily rationalized in models that do not assume one-shot or finitely repeated game interactions. The question, then—and it is the question that Ortmann and Hertwig (2000) and others (e.g., Binmore and Samuelson 1994) before them have asked—is whether subjects bring to the laboratory the rules of thumb that serve them just fine in their daily lives and which can be interpreted as series of intertwined indefinitely repeated games.

The present authors agree that the three developments sketched above—the study of more complicated games, the attempts at theory generation, and the questioning of experimental methods—have been fruitful, especially to the extent that they acknowledge that *Homo sapiens* is, at best, rational within limits. Even if one is convinced that humans behave in ways other than those predicted by canonical theory, it is possible to learn a great deal from reasonable refinements of canonical game theory that do not in principle question rationality ("neoclassical repairs"). When a situation is relatively simple, so that even a boundedly rational participant can easily understand it, the neoclassical repairs will often reflect how participants derive their decisions. The tradition of enriching models (fitting games, in particular with assumptions about—far too often commonly known—risk aversion, social preferences, and the like) to match earlier experimental results and testing their solutions with new experiments will therefore continue.

We also agree that much work remains to be done regarding the incorporation of cognitive limitations into our models. We conjecture that, even for relatively simple games, many subjects transform games in simplified decision situations by looking, for example, in gift exchange games at the maximum gain and loss and the likelihood of them occurring.

The basic problem of the rational choice approach is that it assumes all the evaluation problems to be solved, whereas in actual life one often does not know the decisive decision alternatives (in an ultimatum experiment one does not usually consider all offers but focuses attention on a few previously selected ones, such as ½, ⅓, or ¼ of the pie) and how to evaluate them (if I offer only ⅓ of the pie in an ultimatum experiment, what are the chances that this offer will be accepted, and how do I feel if it is accepted?).

Weakening the assumptions of normative decision theory—such as is done by theories of nonadditive utility, regret theories, and prospect theories (see Starmer 2000)—does not help much, since these new theories also rely on given evaluation functions. What all this literature neglects are the dynamics of decision making, even in one-person games where just one decision maker first generates a few choice alternatives that he or she then seriously considers.

A more realistic picture of human decision making would have to incorporate the basic stages of such decision dynamics as checking one's own and others' experiences for guidance as to what one might do and how successful these alternatives have been, and possibly by relying on routines developed for the problem (this allows for path dependence but requires, of course, some theory of qualitative and quantitative resemblance or similarity); developing a cognitive representation of the decision environment that one faces, either by comparing it with previously experienced decision problems or by mentally modeling the basic causality structures (bounded rationality denies perfect rationality but not forward-looking deliberation altogether); generating a few choice alternatives and measures of success (e.g., in an ultimatum experiment, an aspiration level when being the proposer or the responder, and—as the proposer—an aspiration for how likely one's offer should be accepted, e.g., "certainly" when offering ½, "almost certainly" when offering 4/10, and "not sure" when offering only ⅓ of the pie); applying some choice procedure, such as by claiming that one of the success measures should be decisive (e.g., the chances of having one's offer in an ultimatum experiment accepted); and evaluating one's choice ex post, if possible in the light of feedback information, in order to update's one behavioral repertoire.

Process models of dynamic decision emergence that are rich enough easily become rather complex (for a simpler process see Güth 2000; Deutsch and Strack 2004; Güth and Ortmann 2006). These models do not yet offer ready algorithms for generating choice behavior but rather present a general frame on how to combine the various aspects of human decision-making processes. Like neoclassical economics (which only suggests choices when putting in all evaluative judgments like utilities, probabilities, structural assumptions, etc.), an algorithm needs much more information. This, however, should not prevent us from trying hard(er) to develop such algorithms.

NOTES

1. The same can be said of canonical decision theory, i.e., expected utility theory, whose descriptive merits have been questioned (e.g., Camerer 1995; Starmer 2000; but see Myagkov and Plott 1997; List 2004).

2. Since the early nineties an interesting literature has emerged that departs from the heroic rationality and knowledge assumptions of canonical game theory and tries to explain the outcomes (equilibria) of games "evolutively" through dynamic models rather than "educatively." A path-breaking paper in this tradition was Friedman 1991, which established that Nash equilibria, under fairly weak conditions, are the fixed points of dynamic models that incorporate various forms of bounded rationality and limited knowledge. Note, however, that learning and evolution usually demand (indefinitely) repeated interaction. Similar to a tradition in general equilibrium theory, stability of behavior is defined partly by dynamic stability concepts (rest points) and partly by static concepts, e.g., evolutionarily stable strategies. Among noteworthy recent monographs addressing the former are Weibull 1995; Vega-Redondo 1996, 2003; Samuelson 1998; a good introductory text addressing the latter is Hammerstein and Selten 1994. Below we will discuss these developments only in passing, although they do speak to the issue of (the emergence of) social preferences.

3. The authors disagree on this point. Ortmann argues that we might then as well dispute the possibility of indefinitely repeated games during one's lifetime. Surely all of us know that in this life we will reach an endpoint. Güth argues that the neglect of termination is best explained by boundedly rational reasoning, e.g., in a forward induction way ("let's start to cooperate and think about how to terminate when the end is near").

4. Finitely repeated games with multiple equilibria can allow for folk-theorem-like results since they allow for punishing by switching equilibria (e.g., Benoit and Krishna 1985).

5. Binary trust games (where one usually decides between not trusting at all and full trust), which have a somewhat longer tradition, especially in social psychology (see, for instance, Snijders 1996 and the literature review there), are neglected here.

6. The basic idea of this argument is well known (e.g., Reny 1992) and, in fact, goes back at least to Ellsberg's (1956, 1959) critique of a key solution concept proposed by Von Neumann and Morgenstern (1947), the maximin.

7. Although Henrich (2000) reports that several of his subjects, even after thirty minutes of individualized instruction and numerous examples, had to be dismissed because they could not answer control questions. On the other hand, Takezawa, Gummerum, and Keller (2004) report no problem in implementing dictator and ultimatum games with German children ages eleven and thirteen.

8. The authors strongly disagree on this, with Güth viewing this more as a reformulation of the question "Why prosocial behavior?" by asking "Why prosocial preferences?"

9. The reason for splitting up the bargaining choice into demand and bottom line is that although game theory does not account for this, it seems to help the parties to coordinate more easily on how to split the surplus. Behaviorally speaking, demands can aim at an efficient allocation, whereas bottom lines can be seen as a way to avoid conflict. Participants can also try to reach their higher aspirations by high demands and play safe by using more modest bottom lines. A positive difference $D_k - B_k$ might be interpreted as a concession.

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