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by

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Bargaining and Negotiations*

What should experimentalists explore more thoroughly?

Werner Güth

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Abstract:

A long time ago most economists would have limited themselves to stating that agreements should be individually rational and efficient and that selecting a specific agreement from that set depends on bargaining and negotiation power whatever that may be. Nowadays hardly any economist will argue that way. The change has been brought about by the strategic approach to bargaining and cooperation and the parallel experimental studies of bargaining and negotiation. When arguing what should be explored more thoroughly, we will point out directions where previous efforts may have been misdirected, where importing new methods may be helpful or even needed, and where new research questions need to be asked and answered.

Keywords: (un)bounded rationality, (non-)cooperative game theory, bargaining and negotiation (theory and experiments)

JEL classification: C90, 92, 93, D63, 64, 71, 74

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1. Introduction

Future directions of research in experimental economics can be (i) predicted as well as (ii) recommended. When attempting (i), one probably does what is usual when trying to predict the future, namely relying on past experiences in order to infer what they might suggest. In case of (ii), one can never be sure how much recommendations are biased by one's own research agenda. Trying to clearly distinguish between (i) and (ii) may be attempted but will probably fail.

In the following, we will therefore simply outline some warnings regarding previous research (section 2), state some conjectures about future directions (section 3), and finally raise some hopes (section 4) before concluding (section 5).

Unlike Camerer (2003), concluding with ten most important research questions/topics, our attitude is to grant not only heterogeneity in experimental behavior but also in experimenter behavior. This renders the notion of clear cut future directions in experimental economics questionable and rightly so: an evolving field of research like experimental economics should satisfy the Darwinian requirements of retention (Don't forget the earlier research!), variation (Try out new designs!), and selection (You should more often than not follow ideas which so far proved to be successful!). We will outline some possible future directions without, however, predicting them.

2. Possible misdirections in the past

Especially by ultimatum bargaining (Güth, 1976) and the extensive tradition of ultimatum (bargaining) experiments (see Güth et al., 1982, and Camerer, 2003, for a recent survey), it became clear that one must clearly distinguish between

- the (game) theoretic prediction based on common(ly known) opportunism in the sense that each bargaining party maximizes her own material payoff and

- actual, e.g. experimentally observed bargaining behavior, influenced by the imported motives, emotions, and perceptions of the – usually student¹ - participants.

Thus it became clear very early that when designing institutions of bargaining and negotiation there will be two directions, namely one based on common(ly known) rationality and one based on empirical findings. Not distinguishing between the two is partly responsible for the still prevailing illusion of some economists that game theory well predicts actual behavior.

Warning 1: Too many bargaining experiments still mainly test the game theoretic prediction without trying to supplement the rational choice approach by a behaviorally more salient alternative.

Of course, in some simple tasks game theory can predict well. But as we should have learned from playing chess this does not extend very far². Nevertheless, widely noticed theoretical studies, e.g., of infinite horizon-alternating offer bargaining (Rubinstein, 1982), inspired experimental tests of such models although an infinite horizon cannot be implemented experimentally (e.g. Felsenthal et al., 1990). But the problem is a much deeper one: bargaining experiments with alternating offers and rather short commonly known horizon and with information retrieval, controlled via the mouse lab technique, have shown (Johnson et al., 2002) that the (game) theoretically convincing idea of backward induction is not in line with the way we reason in multi-stage negotiations. Rather than reasoning backward, we seem to imagine some bargaining play whose reasonableness is then checked without necessarily specifying at the beginning in detail how to end.

Ariel Rubinstein's bargaining model has the advantage of a unique subgame perfect equilibrium. This avoids equilibrium selection as, for instance, by the so-called Nash (1953)-bargaining solution for one-stage demand games with lots of strict equilibria.

¹ We do not discuss experiments using non-student participants (e.g. newspaper experiments like Güth et al., 2003 and 2007) or so-called field experiments.

² Tournament chess is finite and has therefore subgame perfect equilibrium solutions which no one can describe.

But in the light of experimental evidence the difference is behaviorally not so straightforward.

Warning 2: Unique equilibrium predictions usually fail and equilibrium selection in bargaining experiments may be obvious due to imported sharing norms.

The first point is clearly demonstrated by reward allocation/dictator experiments with and without entitlement (Mikula, 1973, Shapiro, 1975, Forsythe et al., 1994) as well as by ultimatum experiments. Furthermore, in one-stage demand game experiments (all parties independently state their demand which is what they receive if the “pie” suffices to satisfy them all whereas otherwise they earn nothing) the equal split is the dominant mode of behavior what justifies the second claim.

More generally, the strategic approach to bargaining and negotiation as propagated by Nash (1950 and 1953) and also Harsanyi and Selten (1972) may have misled experimental economists to

- rigorously detail all subtleties via extensive form or stage games and
- subjugate all information to common knowledge.

Control questions and/or experience might guarantee that all strategic details are well understood. But stressing them might make participants wonder about them and cause demand effects, e.g. in the sense that double blind procedures (the experimenter only learns the distribution of all individual behavior, see Bolton and Zwick, 1995) raise suspicion rather than comfort participants that their individual behavior cannot be observed by the experimenter.

Warning 3: If one already concedes that (game) theoretic predictions usually fail, implementing the games as defined theoretically could turn out to be questionable due to overburdening participants, possible demand effects when providing specific details etc.

Note that the early experimental studies of bargaining and negotiation have used the so-called cooperative approach, e.g. by allowing free face-to-face negotiations based on characteristic function “games” (see the respective Contributions to Experimental Economics, ed. Heinz Saueremann, 1978). Similarly, double auction experiments (Smith, 1962) rely on rules (who bids when?) which are largely unspecified.

If one does not want to give up common (knowledge of) rationality one will, of course, try to align the (game) theoretic predictions with observed behavior (neoclassical repairs or game fitting, Güth, 1995). This inspired concepts of risk aversion³ in stochastic tasks and of all sorts of other regarding concerns in studies of bargaining, negotiation and, more generally, distribution. In view of the rather recent tradition of experimental economics, we think the following is adequate:

Warning 4: In view of the little what so far can be concluded from experimental findings stating universal simplistic, e.g. aversion based theories of bargaining behavior, is premature. The only aversion concept to be encouraged is aversion aversion.

The widely debated concept of inequity aversion (Loewenstein et al., 1989, Bolton, 1991, and mainly Bolton and Ockenfels, 1998 and 2000, as well as Fehr and Schmidt, 1999) has been inspired by ultimatum experiments and proved to account for several experimental findings. But in recent generosity game experiments⁴ (the proposer has a fixed agreement payoff x and can vary the pie size p and thus the offer $p-x$ to the responder) for which inequity aversion predicts equality, the dominant mode of behavior is generosity (the largest p) rather than equality. This may illustrate how new experimental paradigms and probably new findings for old ones can question our interpretation of former results. If a third party is added and thus two agreement payoffs can be exogenously imposed the modal choice of p depends crucially on whether these two exogenous agreement payoffs are equal or not.

³ In bargaining and negotiations the risks are often non-monetary ones, e.g., the risk of losing face, what renders the narrow concept of monetary risk taking by utility of money concepts rather questionable.

⁴ Güth, Levati, and Ploner (2009) and Güth, Pull, Stadler, and Stribeck (2010).

Especially in view of the superior technologies in experimental data elicitation (mouse lab, eye tracking, videotaping, physiological and brain scan data), we should study process models of decision making and not substitute the question “why such behavior?” for one asking “why such preferences, beliefs, aversions?” Process models of boundedly rational decision making need not necessarily rule out game theoretic rationality but could allow for it under special conditions. Hopefully, this will provide the stage for including psychological research, teaching and learning how to bargain and offering negotiation advice.

Warning 5: Explaining all experimental findings by using the rational choice approach immunizes experimental economics against imports from psychological research and must therefore fail in our normative task of helping people to become better negotiators.

3. New methods and questions

The major advantage⁵ of computerized experiments, compared to pen-and-paper experiments, is to allow for extensive learning. It can be expected that this possibility to study learning in the lab will be further exploited. To avoid, however, the same frustration as psychologists – who very actively explored learning behavior much earlier, e.g. Bush and Mosteller, 1955 – one should be open to multifaceted learning rather than mechanically imposing the same type of adaptive dynamics (see Camerer, 2003, for a recent review). When participants, for instance get little information about their decision environment (stochastic and/or strategic), it is natural to first rely on the law of effect (reinforcement learning). This, however, becomes more and more questionable when trying to mentally model the decision situation as feedback information accumulates.

Conjecture 1: Path dependence in negotiations will be studied more extensively but less so by constantly imposing the same single and simple-minded adaptive dynamics and more so by exploring the

⁵ A possible disadvantage could be that participants tend to decide too quickly without thoroughly deliberating what could be relevant when especially investigating the behavior of inexperienced participants.

changing type of dynamic adaptation in long processes of learning.

A more direct application of new elicitation methods like eye tracking or brain scanning is, of course, that the future will allow us to check hypotheses in

- action space (“does observed behavior confirm to predicted behavior?”)
- payoff space (“are the predicted payoffs actually earned?”)
- cognition space (“do the observed indicators of mental representations correspond to the theoretically postulated reasoning process?”) and by
- brain activation data (“do more complex decisions require stronger brain activation in certain brain regions?”).

With latter measurement techniques it will be easy to supplement the usual demographic questionnaire data with physiological characteristics of the probands, e.g. when trying to account for individual differences in behavior. Physiological reactions to stress may, for instance, be more suitable to assess individual attitudes towards risk than simple lottery choices (e.g. Andersson and Holm, 2002).

Conjecture 2: We will test hypotheses more reliably in action, payoff, cognition space and by brain activation data what will in all likelihood offer better methods to account for individual characteristics in negotiation behavior.

Although in bargaining theory the coexistence of

- intraparty conflicts (“do the delegates of a bargaining party really aim at what the clients, which they represent, want them to achieve?”) with
- interparty conflicts (“which party gets more?”)

is an old topic, experimentally this is largely under-researched⁶. Mostly, one either studies bargaining by delegates or negotiations of delegates with their clients in isolation although the intraparty conflict may be crucial for how one interacts with the

⁶ The same applies to intrafirm and interfirm conflicts of firms interacting on the same markets.

other parties at the negotiation table. The huge variety of negotiation processes can in many ways be influenced by how clients can codetermine the negotiation tactics and incentives of their delegates.

Conjecture 3: There will be attempts to capture both, intraparty and interparty bargaining, where one will begin by using familiar bargaining modules, e.g. take-or-leave-it contract offers of clients to their delegates and alternating offer bargaining of such incentivized delegates.

Will the present request for more field experiments inspire more field research, related to bargaining and negotiations? There have been always strong traditions of field research regarding, for instance, international negotiations (Raiffa, 1997), labor negotiations and intrafamily bargaining where the dominant bargaining module is still the asymmetric Nash (1953) bargaining solution (e.g. Ott, 1992). However, field data will continue to be insufficient for distinguishing between bargaining theories differing in subtle details. When bargaining parties more often employ Internet conflict-resolution platforms and when – after anonymizing the process data of such conflict resolution – the data become available, field research may become easier. But whether this will happen and, if so, which data will be provided can be at best speculated so far.

In spite of all the data of bargaining experiments questioning the rational choice approach as a valid tool to explain empirical bargaining behavior, the main trend is still the one of including all sorts of motives, emotions and aversions. This renders a rational choice exercise more complex rather than simpler and – when Pandora's Box of neoclassical repairs can be freely used – even tautologic. The idea of bounded rationality (Simon, 1955) has existed nearly as long as experimental economics (e.g. Sauermann and Selten, 1959, Smith, 1962, Fouraker and Siegel, 1963), but without much influence. This may be due to the fact that the idea of bounded rationality has been propagated without trying to formulate it rigorously. Especially when postulating that aspiration levels have to be revealed by choice

behavior⁷, what one gets are at best well behaved aspiration data but hardly a test of the satisficing hypothesis.

But why should we submit to the spell of the revealed aspiration approach? In experimental economics we often directly elicit action beliefs, e.g. when testing for let-down aversion (one does want to fulfill others' expectations concerning the own behavior) and do not infer them from choice behavior. In our view, there is similarly no reason to refrain from directly eliciting aspiration levels⁸. And for given reliable aspiration data, one can rigorously define and test whether or not a choice is satisficing.

Conjecture 4: With the help of directly elicited aspiration data we expect more bounded rationality attempts to explain and predict bargaining behavior and negotiation results.

The latter conjecture better be true! In our view, teaching and learning bargaining behavior and better negotiation techniques requires reliance on mental models and concepts like success aspirations and satisficing in search. The concepts of the rational choice approach will not do when trying to advise negotiators how to bargain. What could be the concepts behind bounded rationality advice? The advice could be based on answers to the following questions (Güth, 2008):

- Which scenarios (possible combinations of chance effects and/or others' behavior) do you not dare to exclude?
- Which success aspirations have you formed for each of your few scenarios?
- Which action plans are you considering? Please, check successively in your preferred order whether one of the few seriously considered action plans satisfies your aspirations for all your scenarios. If so, you may stop. If not, you have to adapt either your scenarios and/or your success aspirations (scenario and aspiration adaptation, for the latter, see Sauermann and Selten, 1962) or develop other action plans.

⁷ Note that when allowing for all sorts of motives and aversions the rational choice approach is also hardly specific when such motives and aversions have to be revealed in the tradition of the revealed preference approach.

⁸ The problem is more how to incentivize stating aspirations (see Güth et al., 2008).

Scenario formation in this sense does not require but also does not exclude probabilistic reasoning and thereby intrapersonal aggregation of aspirations across scenarios like expected payoff maximization. It also does not rule out optimality but only renders it rather unlikely when confronting a complex negotiation task.

Conjecture 5: When studying bargaining behavior and negotiation styles the rational choice approach will be more and more supplanted by bounded rationality models of bargaining whose reliability can be tested experimentally when directly eliciting the results of scenario generation and aspiration formation and whose concepts are suitable for teaching, learning, and consulting.

This predicted tendency should somewhat close the gap between the methods used and conclusions drawn by (cognitive and social) psychologists and experimental economists (e.g. Fellner et al., 2009, and Güth et al., 2009).

4. New research questions

When inviting participants to play a bargaining game in the lab, we can never be sure which social norms they import and more or less consciously apply. What may change is not the phenomenon as such but how we cope with it. One method could be to study so many task repetitions that the evolution of social norms in bargaining and negotiation behavior would emerge for study. Although participants may initially rely on imported norms, when receiving population feedback (participants learn about all past plays of a session and not only the own one) very likely norms will change by adapting to past average play.

Hope 1: The possibility to run many repetitions and to provide population feedback in computerized bargaining experiments should inspire attempts to study the social norm dynamics and their effects on bargaining behavior.

Rather than distinguishing bargaining only by individuals and groups⁹ the astonishing heterogeneity of lab-observed bargaining behavior suggests a better control and categorization of individual and possibly group types. To elicit the appropriate characteristics, one may rely, for instance, on

- panels for recruiting participants which may provide a lot of socio-demographic background information,
- recruiting different types of participants varying in profession, age, gender and possibly different professional (negotiation) experiences, or
- experiments with a “theory absorbing” pre-phase where participants experience first preprogrammed negotiators¹⁰ or are exposed to theory by attending a lecture before they freely decide whether to follow the theory or their own ideas.

Hope 2: Other and better methods of controlling individual characteristics of participants should lead to attempts to categorize types of participants who behave differently and vary in their bargaining success. This, in turn, can inspire studies on how negotiators try to find out others’ hidden negotiation styles. Or rather than trying to elicit individual characteristics one might prime participants by exposing them to “theory” in order to test the absorbability of, for instance, some “bargaining theory”.

As quite generally true for all binding contracts, most negotiation agreements have to be incomplete. For instance, ambiguous property rights can prevent future efficiency enhancing investments by the negotiators (hold-up problem). So far very few experiments have studied negotiations where

- some aspects of future behavior can be unambiguously agreed upon whereas

⁹ We prefer the more general program of intraparty and interparty negotiations allowing for all forms of intragroup decision processes.

¹⁰ Informing participants that these are programmed and according to which theory avoids any form of deception.

- other aspects necessarily have to remain open and be dealt with unilaterally or by a new round of negotiations.

As an example imagine collective wage bargaining which details work conditions and what wage employees receive but does not anticipate which employees will be kept or laid off when a negative demand shock occurs.

Hope 3: As in contract theory and the experimental studies related to it also bargaining experiments should focus on negotiations for incomplete contracts and how tough negotiations for contractable aspects will affect the mutual goodwill when having to negotiate the so far unsettled aspects.

Although the double oral auction is poorly understood at the level of individual trader behavior (see the attempt of Sadrieh, 1990), it is one of the impressive success stories in experimental economics. The same may apply in the bargaining sphere: we know little about how to strategically model face-to-face negotiations with free communication in an environment where there are no specific rules except for those of declaring an agreement binding.¹¹ But as for the double oral auction the result of such poorly defined negotiation institutions may be reliably predictable. In our view, experimentalists should be open for such findings and may actually engage in systematically exploring such free negotiation procedures and thereby essentially revive the former tradition of characteristic function-bargaining experiments.

Hope 4: Free negotiation procedures may be systematically explored and might prove to be rather efficiency enhancing. The endogenously evolving negotiation styles might also be more relevant for field research, e.g. by telling us which bargaining tactics are more successful.

Lately rather crude designs, e.g. combinations of a public good game with an arbitrary subsequent punishment phase whose technology is in no way related to the

¹¹ For instance, by requiring that an agreement is binding if no party vetoes it during 5 minutes after announcing the agreed upon outcome to the experimenter.

public good environment¹², have received a lot of attention (Fehr et al., 2008). Hopefully, game models of future experiments will capture crucial aspects like bargaining and sanctioning more intuitively by rendering them as natural for some, however stylized, field situation. Punishment options are more often than not crucial aspects of the field situation (e.g. Ostrom et al., 1992) or of the strategic game, e.g. veto power in the ultimatum game or outside options and conflict in bargaining games, whose rules define how effectively one can punish.

When maintaining rational-choice benchmarks, one might want to distinguish between sanctions as part of the equilibrium but not necessarily of the equilibrium play (see Bruttel et al., forthcoming) and non-equilibrium sanctions like rejecting a positive offer in ultimatum bargaining. Equilibrium sanctions may not be used although they could stabilize mutually profitable equilibrium cooperation and non-equilibrium sanctions may strongly enhance cooperation. It seems a good guess that what happens depends on how profitable cooperation and how effective the sanction turns out to be and, possibly, on how much coordination is required for cooperation.

5. Concluding remarks

One rather obvious prediction for the future of experimental economics is that its present boom will finally lead to its more intensive use in public finance (testing rules of public procurement tenders, tax compliance, evaluation of goods and services provided by public authorities, public administration, etc.), business administration, economic policy as well as in the neighboring sciences also exploring economic phenomena. We do not interpret this as a new direction but rather expect that the usual methods will be applied to new problems. This hopefully introduces and suggests some new and innovative experimental paradigms, e.g. bribery games when exploring the behavior of public administrators or inspector games when investigating monitoring in firms and other organizations.

¹² Due to strong reciprocity motives we always were aware of punishment as a way to behave reciprocally and that variations in punishment technology (see, for instance, Bolton and Zwick, 1995, Güth and Huck, 1997, Ahlert et al., 2001) will be crucial whether or not we follow our punishment inclinations.

On the other hand, the present boom of experimental economics might indicate a bubble which can collapse when overly optimistic expectations concerning how much can be learned within a short time are not fulfilled. Such outrageous expectations are partly self-induced, e.g. by predicting a behavioral revolution of economic policy or when claiming that (too) simplistic theories apply universally. But the main problem will be that progress in experimental economics like in other empirical research is slow whereas expectations for its progress and its insights are much more volatile. In our view, we should not be too optimistic. But even more realistic expectations may not prevent a crisis in experimental economics in the sense that its answers more often than not do not provide clear guidance. This could raise questions about the field's acceptance in the community of all economists. Such a crisis would align hopes more closely with actual progress but it will not stop the gradual progress in the field of experimental economics.¹³ For sure, experimental economics will survive.

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¹³ Psychology with its longer traditional of experimental research provides some good intuition for the role of experimental research in the general field.

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