

**Some of the Ancient History of Experimental Economics and Social Psychology:
Reminiscences and Analysis of a Fruitful Collaboration**

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Abstract

This chapter presents a dialog between the two authors, a social psychologist and an economist, who review, comment, and analyze their joint efforts. Their first project began in 1974. Their experiments investigated coalitions, bargaining, risk aversion, and deadlines. Brief descriptions of their work provide a foundation for observations on the history and progress of experimental economics.

This paper sketches some of the history of the collaboration of an economist (Al) and a social psychologist (Keith). The text is a joint production, with comments and entries from each of us.

Although collaborations between people in these two fields are currently increasing, they were quite rare when we first started working together. Even today, interdisciplinary collaborations of this kind require special attention, including open-minded and supportive co-authors, mutual respect for each other and the two fields, and a willingness to pursue similar issues from different perspectives and in different ways. In other words, not everyone should embark on collaborations like ours.

First, some history. We both got our PhDs in 1974, Al from Stanford's Operations Research department, Keith from Purdue's Social Psychology department. We were both recruited by the Department of Business Administration at the University of Illinois and began our positions as Assistant Professors that fall. Although all this occurred over 30 years ago, it doesn't seem like it's been that long.

Two of our senior colleagues, Bill Zangwill and Lou Pondy, suggested that we meet, as our research touched on similar topics. Al's dissertation was a game theoretic exploration of some questions raised by von Neumann and Morgenstern's (1944) *Theory of Games and Economic Behavior*, about a solution concept in n -person games. Keith's dissertation was an empirical investigation of coalition behavior that tested several social psychological models that had just appeared in the literature.

Our luck at being hired by the same school and the fact that we discovered that we liked each other provided the groundwork for what turned out to be a lengthy collaboration, one that resulted in four research grants and eleven published papers. (This chapter makes the total an even dozen). Our papers have investigated veto games, prisoners' dilemmas, risk aversion, information and bargaining, and deadlines. Neither of us has collaborated with anyone else so many times. Obviously, something went right when we first met, and something went right when we planned and completed our various projects.

Our first project was on the left shoe-right shoe game. This game is based on a story in which one player has a left shoe, two players have right shoes, and the three of them must interact to determine whose shoes will make a pair and how the agreeing players will divide the payoff that the pair can command. This game gives monopoly power to the left shoe player. Thus, its core is the outcome giving the left shoe player the entire payoff.

Our experiment on this game documented the payoff distributions of the players over 12 repeated rounds (i.e., a supergame). We varied the players' information and communication opportunities six ways. We did not use a standard factorial design; instead, we tried to focus on a series of conditions that varied the players' information about their payoffs and their offers, as well as providing some of them with the opportunity to send messages to each other.

The results are probably less important than the process of our collaboration. (It's nice to have a printed copy of the paper that tells us what we actually wrote. Our memories of the process that led to that outcome, however, may diverge.)

Keith: I remember writing a first draft. I was not a particularly good writer at the time, but I was enthusiastic. Thus, I probably gave Al a draft long before I should have shown it to anyone, much less a first-time co-author. I don't remember whether Al changed sentences and edited what I had written. But I do remember entire paragraphs that had a diagonal line drawn through them, with the comment "You can't say this" in the margin!

I suppose that I could have been offended by these comments, but instead, I thought that we simply had a problem that needed to be solved. Thus, this was probably a fine way to start a collaboration, particularly for two researchers whose training had been so different. In essence, I did not speak economics and Al did not speak psychology. (It is still not clear whether either of us has really picked up the other language, but at least we now think that we understand each other.) We had to work our way through a lengthy process to determine how we could express what we wanted to say about our joint work without offending each other or insulting each other's fields in the process. Needless to say, writing this paper required many iterations.

Al: Right, I remember that. (I forget what was wrong with our even numbered drafts, but I distinctly recall that, in the low odd numbered drafts, we claimed to have used experiments to disprove theorems...)

Keith: I don't remember what was wrong with the even-numbered drafts either. But I think that, at the time, I wondered what good a theorem was if it didn't have any relation to actual behavior. (This same kind of thinking was what pushed me away from being a Math major as an undergraduate – that along with my limited mathematical ability.)

When we were thinking of an appropriate outlet, we were faced with another problem, as each of us valued our own field's journals more than the other's. Thus, we followed the recommendation of a senior colleague and sent the paper to Management Science. Rather than the quick turnaround that he had predicted, we waited nine months for a first set of comments. We did our revision, resubmitted, got more comments, resubmitted, got more comments, resubmitted again, and the paper was finally accepted. (A footnote on the first page of the paper indicates that "This paper has been with the authors 2 months, for 3 revisions.") We then waited 18 months for the paper to actually be published.

Al: Two more papers in that line of work followed in the next few years. Because we were both inexperienced, those two papers would have been one if we had known what we were doing. But there weren't many models of psychologist-economist experimental teams in those days, so we had to teach ourselves how to talk to each other, and what to talk to each other about...

Keith: Yes, we studied 12-person veto games and wanted to interpolate between groups of 3 and 12. We were discouraged from doing this by an editor or two – although I don't remember exactly which.

At about the same time we embarked on a project on the prisoners' dilemma. Al had an idea that followed some of Luce and Raiffa's (1957) work in *Games and Decisions*. We studied a model of the repeated prisoners' dilemma with a fixed probability of stopping, and identified the conditions in which mutual cooperation can be sustained at equilibrium. We then conducted an experiment with payoffs and continuation probabilities that would support a cooperative equilibrium with either of two strategies, "tit for tat" and "unforgiving." The data provided neat, clear support for the model.

Al: I have talked about the design of that experiment to my class on experimental economics, and the slide with which I did that most recently lists many elements of the experimental design after noting that "there were aspects of the procedure that I would *do very differently today*, while other aspects have **stood the test of time**. Some could go *either way*."

By this time, even though it was early in both of our careers, we concluded that, because our research was being well received in the journals, we were likely to be get tenure somewhere (if not at Illinois). This emboldened us to submit our prisoners' dilemma paper to Science. Our reviews were positive but the editor indicated that the paper was not of sufficient general interest to be published there. We concluded that it wasn't biological enough, so we then turned to Psychological Review and got exactly the same decision from them. The Associate Editor at Psych Review was also the editor at the Journal of Mathematical Psychology, and he invited us to submit it there. Thus, the paper was published in JMP in April of 1978.

During this process, we discovered two other publications, Goehring and Kahn (1976) and Taylor's (1976) *Anarchy and Cooperation*, that presented one of our two indices for cooperative equilibria. Then, in 1981, Axelrod and Hamilton published the same index as the basis of their widely-cited, award winning paper in Science. Clearly, the zeitgeist was right for these ideas.

As with our coalition research, we followed this first prisoners' dilemma paper with another, one that more comprehensively tested the model, over a large number of different payoffs. The Journal of Conflict Resolution accepted this paper without asking for revisions. This led us to revise it substantially!

It was during this time that Al began working on bargaining games. His first paper on that subject, in Psychological Review in 1979, was a novel experimental design for testing Nash's model of bargaining, which at the time was the model of bargaining most widely used in economics. Nash's model, like many other models in game theory, was framed in terms of players who were expected utility maximizers, and the data the model required was their expected utilities for each possible agreement. Questions had been raised about whether such a

model was testable and whether its predictions could be unambiguously known, since its data were difficult to observe. Al and his student Michael Malouf, therefore designed their experiment as a *binary lottery game*, in which each of two bargainers has only two possible monetary outcomes (one usually being zero, and the other a monetary prize). The bargainers negotiated over their chances of winning their own prize, e.g. by dividing 100 lottery tickets. It is easy to see that if the players are expected utility maximizers concerned only with their own payoffs (as the theory assumes), then their utility for any agreement is simply the percentage of lottery tickets that they obtain. This design allows the payoffs to expected utility maximizers to be known for any possible agreement, and so it allows the prediction of Nash's model to be computed and tested.

This design also provided a test of the prediction of Nash's model that the information contained in the players' von Neumann Morgenstern utility (i.e. utility information up to an arbitrary choice of origin and scale) was sufficient to predict the outcome. In particular, since players' utility information in a binary lottery game does not depend on the size of their monetary prize, the prediction of the theory was that the outcome of bargaining would not be influenced by the size of the players' prizes, and in particular would not be sensitive to differences in their prizes.

Roth and Malouf's (1979) experiment investigated the effects of knowing the other person's prize. The players either knew each other's prize (complete information) or only their own prize (partial information). In addition, their monetary prizes (if they won their lottery) were either equal (\$1 each) or unequal (\$1.25 or \$3.75). The data were as clear as they could be: the agreements divided the lottery tickets at or very near 50-50 when people had equal prizes or did not know their counterpart's prize; when they knew that their prizes differed, the person with the lower prize received, on average, over 60% of the lottery tickets.

Al: I recall that we sent that paper to Psych Review because I had a completely incorrect model of how this kind of science would be done in the future. The idea that economists would theorize and psychologists would experiment seemed to me like a perfectly plausible division of labor. I thought that, in areas of potential mutual interest to economists and psychologists, what had kept economists from properly appreciating psychologists' experiments, and psychologists from properly appreciating economists' theories, might be lack of familiarity. So I thought that if I put an economic experiment in Psych Review, with an experimental design that would address economists' concerns about experimental tests of theories stated in terms of expected utilities, psychologists would pick it up and run with it. Instead, it turned out that psychologists weren't interested in playing that game. While that paper eventually became well cited by economists, I don't think it ever appealed to psychologists.

Keith: As I reflect on Al's failure to stimulate psychologists to use binary lottery games, it is important to emphasize that this new method was strikingly successful for experimental economics, and it has been used repeatedly. But this continues to beg the question of why economists were impressed and psychologists weren't.

My conclusion is that the psychologists were most intrigued, not by the method, but by the data: it was neat, clean, and completely supported a psychological prediction while rejecting a strictly economic prediction.

We followed Roth and Malouf (1979) with two other papers on bargaining games. In Roth, Malouf, and Murnighan (1981) we investigated whether the difference between the complete and partial information conditions of the first experiment might be accounted for just by the way the additional information changed the strategy sets of the players. (When they have more information, they had a lot more to talk about in the text messages that they could send in addition to numerical proposals, for example.) We therefore introduced an intermediate commodity, chips, to see whether effects for complete information about the payoffs was only focused on monetary information, or whether the players would use information about varying distributions of chips (of sometimes unknown value) to formulate self-advantageous messages and strategies.

Once again, the results were crystal clear. The outcome of negotiations proved to be insensitive to the different number of chips in which players' prizes were denominated. Outcomes only reflected the value of the monetary prizes. This supported what we had called "the sociological" hypothesis, i.e., claims for equal (expected) monetary payoffs were more consequential than similar claims made for equal expected payoff in an artificial unit of exchange.

Keith: I enjoyed several things about this experiment. First, the results were beautiful. Second, they confirmed my suspicion that the economics of bargaining could not reasonably ignore the idea that people are naturally curious about their relative outcomes as well as their absolute outcomes. This was a long standing idea that was most centrally described by Adams (1965; 1967) in his presentation of equity theory. And, third, I was delighted to put the word "sociological" in the title and the text of a paper in experimental economics.

Roth and Murnighan (1982) was motivated by the observation that, in moving from a condition in which players knew only their own prize to one in which both players knew both prizes, a complicated change was being made. The effect of informing the player with the higher prize of his relatively privileged position might not be the same as informing the player with the lower prize. And in the full information condition, the prize information had always been part of the common instructions, and hence common knowledge, e.g. each player not only knew the other's prize, but knew that the other player knew his prize, etc. We therefore investigated the effects of information and common knowledge on the bargainers' strategies and outcomes. There were four information conditions in our 1982 paper (both knew each other's payoffs, neither knew, and the high or low prize player was the only one who knew both player's payoffs) crossed with two common knowledge conditions (information was or wasn't common knowledge).

As before, the data were quite clear. Equal expected value outcomes resulted almost only when the low, \$5 prize players knew that their counterparts' prizes were \$20; otherwise, equal probability outcomes dominated. When the \$5 players had information about the \$20 prize and this information was not common knowledge, however, disagreements increased markedly. This is quite logical: the low prize player knew that they were disadvantaged but the high prize players didn't necessarily know this. Subsequent analyses (Roth and Murnighan, 1983), including not only the numerical offers and counteroffers, but also a content analysis of the text messages that were transmitted during negotiations, indicated that the initial demands of the informationally advantaged \$5 players were significantly larger than the initial demands of the informationally advantaged \$20 players. In addition, the informationally advantaged \$5 players most often revealed their information, and they only rarely misrepresented it. This allowed them to demand the equal expected value outcome of 80%. (They rarely asked for more.)

Keith: As I reflect on this last series of studies, I know that I am most intrigued by the consistency and the clarity of the data and what it says about how people negotiate. If I can generalize from myself to other psychologists, this leads me to conclude that psychologists are most interested in demonstrating and documenting behavioral regularities, especially when those regularities are counter-intuitive. Although both psychologists and economists are careful methodologists, the psychologist's ultimate goal is to see what people do. Economists, in contrast, seem to be much more interested in institutions. They enact this interest by constructing beautiful mathematical models and intriguing games. As a particularly potent example, we can note that the prisoner's dilemma was created by an economist (Tucker, 1950) but psychologists spent an incredible amount of effort documenting the behavioral regularities that it generated. At the risk of oversimplification, psychologists loved the fact that the game gave them an opportunity to observe behavior; economists loved the inherent beauty of the game and its analytic possibilities.

Al: Economists are certainly interested in institutions and environments as they influence peoples' opportunities and actions. In the late 1970's, that sometimes opened a big gap between what psychologists and economists were interested in. I recall talking about some experiments in the psych department at Illinois around that time, and being asked if I didn't think that, by paying subjects, we were just coercing them into doing what we wanted. The evidence was that, in some conditions of the experiment, almost all the subjects did more or less the same thing, whether they were men or women, introverts or extroverts... My reply of course was that to economists, the difference between conditions—between e.g. bargaining with and without certain information, between negotiating with one other individual or competing with many, etc.-- was one of the major foci of interest, not only the difference between individuals.

More recently, I think that many psychologists have become more interested in how people interact with their environment, and less with individual differences. So nowadays the difference I notice most between economists and psychologists is how they use theory. Psychologists want theories to be true; they have little tolerance for

theories that can be shown to be false, but a lot of tolerance for theories of limited scope. If different situations require different theories, so be it. Economists seem to view theories more as useful approximations, and they want them to be useful in a broad range of circumstances, and are willing to tolerate that they may not be precisely true, that isn't what they expect of approximations. I was once asked to write a reply to a paper by Amos Tversky in which he asked why economists were so reluctant to abandon the rational model, and I tried to answer in the spirit of sociology of science, and replied with a paper called "Individual Rationality as a Useful Approximation" (Roth 1996).

Keith: One of the ironies of work that relates to utility is that, even for psychologists, the rational model continues to dominate our research. Thus, anytime that we use payoff matrices, awarding points to participants that can then be converted into money or other kinds of prizes, we implicitly assume that the participants view more points as increases in their own utilities and, worse than that, we implicitly assume that these increases are linear. In essence, we constantly ignore Kelley and Thibaut's (1978) long accepted observation that the people in our experiments transform the payoff matrices that we give them, and then they act in ways that maximize their transformed outcomes. Yet we constantly ignore this knowledge, probably so that we don't have to complicate our work too much. Thus, although we know that we are not giving our participants payoffs that represent their utilities, we still act as if that is exactly what we are doing. And this seems to be true of experimenters in general, regardless of their bent (e.g., psychological, economic, political).

Our collaboration on bargaining then turned to a study on the effects of risk aversion (Murnighan, Roth, and Schoumaker, 1988). In this research, we measured players' preferences for lotteries versus certain outcomes and paired a less with a more risk averse bargainer in two negotiations, one with a low and one with a high disagreement outcome if they did not reach an agreement, in three experiments. Classic game theoretic models predicted that the more risk averse bargainer would do better with the high disagreement outcome but worse with the low disagreement outcome. The results only weakly supported this prediction, and this support was only realized after conducting three separate experiments. In other words, this was an elusive effect.

Our last empirical paper was largely serendipitous. In the course of conducting many bargaining experiments, we had noticed informally that many agreements were reached right at the deadline. Thus, our final empirical paper in our work on bargaining focused on the effects of deadlines (Roth, Murnighan, and Schoumaker 1988). This paper went back and examined the agreement times in a series of our earlier bargaining experiments (plus a new one). Although the experiments varied in terms of the bargainers' information, relative prizes, disagreement outcomes, the total time for negotiating, and size of payoffs, the deadline effects were remarkably consistent; almost half of the agreements were reached in the last 30 seconds of bargaining and, of these, over half occurred in the last 5 seconds, and of these, over half occurred in the last second.

Keith: I presented our deadline data at an economics conference. I remember mentioning that this may have been the easiest paper that I ever presented, as all I had to do was show people the data. To be honest, I was less excited about these findings than my audience was. I felt that the deadline effect was much more obvious than my economist colleagues did. As it happened, our paper showed that neither complete nor incomplete information models could account for the pattern of agreement terms, agreement times, and disagreement frequencies.

Al: I'm not sure that is what our paper showed, but it did show a pronounced deadline effect, under conditions that made it difficult to explain. In particular, one popular explanation for deadline effects in labor-management negotiations was the need to bring your constituency along with you, the idea being that it was easy to criticize an agreement reached well before the deadline, and send the negotiators back to the table to try to get a better deal. But, in our experiments, there were no constituencies to convince. Rather, it appeared that negotiators used deadlines to influence each other. This is certainly the view that has predominated in the economics literature following our experiment. (Incidentally, I've since studied similar deadline effects in eBay auctions, see e.g. Roth and Ockenfels, 2002.)

We now turn to some summary comments.

Keith: In a chapter later in this volume, Rachel Croson outlines some of the particular differences between economists and psychologists as they approach experiments. While anthropologists typically focus on tribes, sociologists focus on societies, and political scientists focus on polities, psychologists focus primarily on individuals. They are motivated to understand how and why people act, think, and feel, and how those actions, thoughts, and emotions interact. Thus, a psychologist finds it easy to think about individuals being motivated by many different forces. They are often surprised to find that economists seem to attribute primary motivational forces to money.

Al: I can see that, for a psychologist, talking to economists about individual motivations (or unbounded rationality) must be a lot like talking to your parents about sex. You can't believe that they know as little as they let on, but they exhibit an inexplicable reluctance to talk about details. But for an economist, talking to psychologists about rationality and goal seeking behavior is a bit like talking to your teenage children about sex. They seem to be simultaneously curious and well informed about many details, but less interested in talking about how these fit into a larger view of life.

Keith: It is completely clear that psychologists and economists take decidedly different approaches to understanding the same phenomena. Do they actually think in different ways? Current work on different cultures suggests that East Asians and Westerners do think in fundamentally different ways (cf., Nisbett, Peng, Choi, and Norenzayan, 2001). I am not sure that this is true of economists and psychologists, but I do know that,

whenever I have presented a psychologically oriented paper to an economically oriented audience, the audience tends to nonverbally express concerns, if not about my sanity, certainly about my ability to think about an issue.

It also seems clear that language differences are sizable. To take just two examples, psychologists don't often speak of externalities or equilibria. Thus, I have long concluded that, for a psychologist to publish a paper in an economics journal, it normally helps to add an economist as a co-author. Similarly, for an economist to publish a paper in a psych journal, it normally helps to add a psychologist as a co-author.

Al It's certainly true that a translator helps. Neither Mike Malouf nor I was a psychologist, and when we wrote our 1979 Psych Review article we had never heard of APA style guidelines. We got through the academic part of the editorial process without too many bruises, but once the copyeditors got their hands on it, they clearly figured that we needed a lot of help. The APA was ahead of the curve, and one thing I recall was that everywhere we had written "he," the copyedited version came back "he or she." We felt that some of the particular individuals we referred to might take offense, so we had to change some of them back.

And, if I'm correct that economists mostly regard theories as useful approximations, and therefore tolerate more error than psychologists, in return for simplicity and breadth of potential applicability, that certainly isn't a position that economists have made very clear, either outside of the profession or in it. The growth of experimental economics is bringing that to a head, since it's now hard to ignore that many economic theories aren't precisely true, in a way that was harder to see when only field data were available.

In this connection, when I want to annoy both my psychologist and economist friends, I like to suggest that this difference in approach to theory has something to do with why American presidents have a Council of Economic Advisors, but no Council of Psychological Advisors...¹

¹ The President comes to his Council of Psychological Advisors and says to them "I've got this situation developing tomorrow, and I'd like some advice." They carefully get all the information they can from him about his problem, and tell him "Mr President, you've come to the right place. But this is a bit different than any situation we've studied before, we'll have to do some new experiments. Come back in six months." Then he goes across the hall to his Council of Economic Advisors and says the same thing to them. They say "Mr. President, you've come to the right place. We have a theory that addresses exactly your situation. By the way, what is your situation?"

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