Multilateral Bargaining in Political Science Experiments: A Survey

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The political science literature has long been concerned with the behavioral properties of alternative bargaining institutions. The focus of most of this research has been on the way in which committees make decisions, but there has been some interest in legislative and electoral processes. From the outset this literature has concerned itself primarily with multilateral bargaining settings, as distinct from the economics literature which has displayed an odd reluctance to go beyond the bilateral bargaining setting.

It is appropriate to begin our survey with a detailed review in Section 1 of the Fiorina and Plott [1978] experiments. Although these were not the first experiments to look at committee behavior, nor the first to study "spatial" preferences[1], this study has influenced virtually all of the subsequent literature. It provides an important benchmark of comparison for any new institution.

The most important conclusions of Fiorina and Plott [1978], hereafter FP, are: (i) that the Core does provide a good descriptive model of committee outcomes when it exists; (ii) that the absence of a Core solution does not herald behavioral indeterminacy, as some theoretical models allow; and (iii) that an extraordinarily simple set of committee rules, akin to Roberts Rules of Order, suffices to bring about Core outcomes. Each of these conclusions applies to the "spatial environment" studied by

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[1] See Halfpenny and Taylor [1973] for the first series of such experiments that I am aware of. Their experiments do not, however, meet many of the procedural standards of Fiorina and Plott [1978] and the literature reviewed here.

FP, and may not generalize beyond that environment as we shall see.

In Section 2 we examine a number of experiments that have been designed to study a popular solution concept amongst political scientists: the Competitive Solution of McKelvey, Ordeshook and Winer [1978]. This concept has two important advantages over the Core as a solution principle: it exists over a much wider domain of preferences, and it provides specific predictions about coalition formation and divisions within coalitions. The basic idea, familiar from older cooperative game-theoretic solution concepts, is that potential coalitions must competitively bid for "pivotal agents" whose vote can mean the difference between surviving as a coalition and being voted out by some other coalition. This process of competitively coaxing agents to join different coalitions determines potentially asymmetric payoffs for winning coalition members. The Core is silent on these matters, although the data suggests that such asymmetries are consistently observed.

In Section 3 we consider the first of two sets of departures from the strong results of Fiorina and Plott [1978] and McKelvey, Ordeshook and Winer [1878]: the demonstration that some notion of "fairness" is needed to provide a complete description of behavior in certain settings.

The first set of results is that the Core does not account for the propensity of many experimental committees to adopt outcomes by a vote of the committee of the whole. This tendency, termed "universalism" in the political science literature, holds that committees will find agreements that provide "something for

everybody", even if it means that agents in the Core must forego some payoff. The key finding here is that there is some level of foregone payoff at which the propensity to seek a universalist outcome diminishes: the taste for "a fair outcome that includes everybody" is demonstrably bounded.

A second set of results that are disconcerting for the Core turns on the role of specific procedures. Do there exist procedures, such as might be suggested by the theoretical literature on Machiavellian agenda-setters[2], that can stop a committee from reaching the Core even when it exists? It appears that there are such procedures, although the evidence is hardly convincing. We summarize some components of this research in Section 4.[3]

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[2] There is a small experimental literature which we do not review here on the role of strategic agenda-setting behavior in committees. Various forms of agenda-setting procedures have been studied, ranging from the simple "convener games" of Isaac and Plott [1978] to the complex alternatives allowed in Cohen, Levine, and Plott [1978], Eckel and Holt [1989], Herzberg and Wilson [1988] and Wilson [1986]. The general finding here has been that agenda-setters in experiments do not appear to be able to strategically manipulate outcomes as one might expect from theoretical considerations. Moreover, voters do not appear to react to the possibilities for sophisticated (i.e., non-sincere) behavior that might be theoretically expected in such settings. It is not clear at this stage whether this is the result of stupidity on the part of agenda-setting subjects, conjectured or actual sophisticated responses by some committee members, or the applicability of some alternative solution concept.

[3] There are many other procedures in the political science literature which have been devised for quite different reasons and are of less interest here. One example is "approval voting", discussed in detail by Brams and Fishburn [1983] and the subject of experimental investigation by Niemi and Bartels [1984] and Koc [1988]. Another example is "four-fifths majority rule", investigated experimentally by Laing and Slotznick [1987].

In Section 5 we draw a number of conclusions from this literature of relevance to the experimental study of alternative multilateral bargaining institutions. The most important conclusion concerns the importance of undertaking a comparative evaluation of any such alternative in the spatial environments studied by political scientists. A related conclusion is the relevance of comparing the performance of structured institutions with the relatively unstructured legislative rules used in many of these studies. The main insight of the political science literature, embodied in FP, is that one can often do quite well in simple spatial environments by using relatively unstructured institutions.

1. THE CORE

1.1 Fiorina and Plott [1978]

FP designed their experiments to evaluate the comparative predictive performance of a wide range of heuristic and formal models from game theory, economics, political science and sociology. Given this wide net, it was appropriate for them to try to set up voting and amendment rules that were plausibly found in field committees. Their instructions are a model of simplicity, despite a number of procedural ambiguities that have subsequently been found to be of little behavioral importance by Salant and Goodstein [1990]. It is useful to read their instructions in full:

> **General.** You are about to participate in a committee process experiment in which one of numerous competing alternatives will be chosen by majority rule. The purpose of the experiment is to gain insight into certain features of complex political processes. The instructions are simple. If you follow them carefully and make good decisions, you might earn a considerable amount of money. You will be paid in cash.
>
> **Instructions to Committee Members.** The alternatives are represented by points on the blackboard. The committee will adopt as the committee decision one and only one point. **Your** compensation depends on the particular point chosen by the committee (see attached payoff chart). For example, suppose your payoff chart is that given in Figure 1 and that the committee's final choice of alternative is the point (x,y) = (170,50). Your compensation in this event would be $7,000. If the policy of the committee is (140,125) your compensation would be computed as follows: [THE INSTRUCTIONS CONTAIN A FIGURE SHOWING INDIFFERENCE CONTOURS]
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> The point (140,125) is halfway between the curve marked $7,000 and the curve marked $8,000. So, your compensation is halfway between $7,000 and $8,000, i.e., $7,500. If the policy is one-quarter of the distance between two curves, then your payoff is determined by the same proportion (i.e., at (75,50) which is one-quarter of the way between $8,000 and $9,000, you get $8,250).

The compensation charts may differ among individuals. **This means that the patterns of preferences differ and the monetary amounts may not be comparable. The point which would result in the highest payoff to you may not result in the highest payoff to someone else.** You should decide what decision you want the committee to make and do whatever you wish within the confines of the rules to get things to go your way. **The experimenters, however, are not primarily concerned with whether or how you participate so long as you stay within the confines of the rules.** [Under no circumstances may you mention anything **quantitative** about your compensation. You are free, if you wish, to indicate which ones you like best, etc., but you cannot mention anything about the actual monetary amounts. Under no circumstances may you mention anything about activities which might involve you and other committee members after the experiment, i.e., no deals to split up afterward or no physical threats.] [THIS SECTION WAS OMITTED IN THE NO-COMMUNICATION CONDITION]

Parliamentary Rules. The process begins with an existing motion (200,150) on the floor. You are free to propose **amendments** to this motion. Suppose, for example, (170,50) is the motion on the floor and you want the group to consider the point (140,125). Simply raise your hand and when you are recognized by the chair, say "I move to amend the motion to (140,125)." The group will then proceed to vote on the amendment. If the amendment passes by a majority vote, the point (140,125) is the new motion on the floor and is subject, itself, to amendments. If the amendment fails the motion (170,50) remains on the floor and is subject to further amendment. Thus, amendments simply change the motion on the floor. You may pass as many amendments as you wish.

At any time during the consideration of an amendment of the motion on the floor a **motion to end debate** is in order. If there are no objections, an immediate vote will take place. If there are objections, the motion to end debate will itself be put to a majority vote. If the motion to end debate fails, the amendment process continues. If it passes, a vote on the amendment or motion will take place.

To sum up, the existing motion on the floor is (200,150). You are free to amend this motion as you wish. The meeting will not end until a majority consents to end debate and accept some motion. Your compensation will be determined by the motion on the floor finally adopted by the majority. So there is no time limit, and no explicit disagreement point.

Are there any questions?

We would like you to answer the questions on the attached page. These should help you understand the instructions.

Test

1. At _____ I would make the most possible money. The amount I would receive is _____.

2. At _____ I would make the least possible money. The amount I would receive is _____.

3. Suppose (200,150) is the motion on the floor and an amendment to move to point (199,149) passes (fails), then the new motion on the floor is _____,_____ (_____,_____)?

4. Suppose an amendment to move to (100,100) passes and no further amendments pass. If the motion on the floor is then adopted by a majority, my compensation is _____.

The crucial aspect of the FP procedures is that there always exists a motion on the floor. An agent that is recognized to be the proposer may either move that the current motion be amended or that it be adopted. In each case a simple majority vote decides whether or not to proceed. The fact that a motion is always on the floor, and that the present set of decisions that agents face is a function of the history of decisions made previously, makes the extensive form of this game proliferate with time.[4]

There is one ambiguity in the FP instructions which deserves

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[4] This is in stark contrast to the extensive form representation of the multilateral bargaining institution studied by Harrison, Rausser and Simon [1990], for example, which simply repeats each stage game with no effect of history whatsoever. In this general context one should also note recent experimental work by McKelvey and Palfrey [1990] on the "centipede game", which focusses directly on the irrelevance of a history of irrationality for current behavior.

some comment: there is no rule as to how proposers get recognized by the chair. This was undoubtedly resolved in an unbiased manner, to the extent that such things are possible at a conscious level. Nonetheless, it makes it difficult[5] to formally analyze the game implied by the FP experiments.

FP devised three series of experiments. For present purposes the significant aspect is that Series 1 and 2 contained a Core outcome, and Series 3 did not. In Series 1, which was the base series of experiments, two treatments were crossed. The first treatment was the level of payoffs, or more accurately the speed at which agents payoffs declined from their ideal points. The second treatment was the provision of information about the preferences of other agents. The default was for there to be no provision of such information, although agents would reveal some ordinal features of their preferences through their (fully observable) votes.

In all of the experimental committees considered by FP there were five members. All were drawn from the student populations of CalTech, USC, Pasadena City College, Cal State LA, and LA City College. This is likely to be a mixed bunch! All subjects were inexperienced in these experiments, and presumably in others as well.[6]

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[5] But not impossible: a plausible assumption, using a diffuse prior on the abilities of committee members to get themselves recognized, is that each member had an equal chance of being called on.

[6] As far as I am aware there have been no studies testing whether or not experienced subjects behave differently than found by FP. There have been many studies replicating the basic findings of FP with inexperienced subjects.

In Series 1 there were ten experimental sessions in each of the four environments. In each case the unique Core outcome was (39,68). The low payoff experiments had average outcomes of (47,72) in the Communication case and (36,70) in the No Communication case, with standard deviations of 21.9 and 17.3 respectively. The high payoff experiments had average outcomes of (37,68) in the Communication case and (38,69) in the No Communication case, with standard deviations of 5.2 and 8.3 respectively.

The first point to note about these results is that the outcomes may seem to be closer to the Core in the high payoff case, but they are in fact no closer from a statistical point of view since the variance is so much higher in the low payoff experiment. In other words, irrespective of the experiments one looks at, the data does not lead us to reject the Core as a hypothesis that predicts these outcomes.

The second point to note is that the variance in outcomes seems to be much lower in the high payoff condition (for either communication treatment). This is also somewhat illusory, since the high variance in the low payoffs experiments is almost entirely due to one or two "wild" outcomes that should arguably be dismissed as outliers.

These comments may seem to be unimportant, since the data seem to be moving towards the Core as we increase payoffs and one should perhaps leave well enough alone. However, the fact that one gets very close to the Core even when payoffs are "low" may be suggestive of a lack of strong financial incentives in these

experiments. Of course, this argument still requires that we explain the excellent performance of the Core, but at least we are on notice that this performance **may not** be due to the effects of enhanced financial incentives. Fiorina and Plott [1978; p.586] have a particularly revealing discussion of their casual evidence from observation of subject behavior that these concerns were important. They even note that in "...the low-payoff condition the experimental atmosphere may best be described as: 'Choose any reasonable point and let's go'." Salant and Goodstein [1990] take this general point up in a more formal manner.

In Series 2 the Core outcome is (61,69), and the average outcome over ten experiments is (60,72) with a standard deviation of 7.3. The main difference between Series 1 and Series 2 was that the preference contours of individuals were elliptical rather than circular. Again, the data is consistent with the Core prediction.

1.2 Berl, McKelvey, Ordeshook and Winer [1976]

Another study with impressive support for the Core is Berl, McKelvey, Ordeshook and Winer [1976], hereafter BMOW. They employed three spatial environments. The first was a five-person committee with Euclidean preferences. The second and third were three-person committees with "City Block" preferences in which iso-payoff lines were squares around ideal points, rather than being circles or ellipses as with Euclidean preferences. All games had a Core, but in the last two environments it was not geometrically transparent.

BMOW employed 39 graduate students from Carnegie-Mellon in a

relatively uncontrolled design. All of the subjects appear to have taken some formal intermediate microeconomics course, given that BMOW note that they were familiar with technical concepts such as indifference contours. All subjects were trained in a three-person committee experiment with no Core, but we are not told if they were paid for their training performance. Some subjects played the research experiment more than once, but no two subjects played against each other more than once. We therefore do not know what level of combined experience applied in any particular experiment.

The procedures in these experiments were also relatively unconstrained. There was no time limit on bargaining, no prescribed status quo point as in FP, no predetermined order for proposers to be recognized nor any chairman, and bargaining was face-to-face.

The results strongly support the Core. The most interesting feature of their results, however, is that they administered a post-experiment questionnaire to subjects. Responses to several of the questions indicated that subjects who ended up at a Core outcome, and even some subjects who were members of the winning Core coalition, thought that there was a better coalition for those who voted it in! In other words, a large number of subjects[7] do not appear to have understood the basic logic of the Core even though they had just participated in an experiment resulting in a Core

[7] It is impossible to say from the report how many subjects answered which question in a particular way, since it appears that subjects could check of as many answers as they liked rather than just selecting the most appropriate.

outcome.[8] Strike up one more success to positivist "as if" methodology!

[8] This suggests a fertile environment to apply the ideas of Gode and Sunder [1990] on the construction of lower bounds to the efficiency of institutions. Their idea is to simulate the behavior of "zero intelligence" automata in a given institution, to see how well such limited-rationality subjects would perform. This provides a valuable benchmark to evaluate the performance of human subjects against, so as to determine how much of the observed efficiency is due to their individually rational behavior and how much is attributable to the constraints that the institution places on that behavior. One important corollory of a "payoff dominance" or "flat maxima" problem is that this lower bound is quite likely to be close to the upper bound of efficiency, since subjects do not need to display inordinate amounts of individual rationality to make "good" decisions.

2. THE COMPETITIVE SOLUTION

2.1 McKelvey, Ordeshook and Winer [1978]

The Core need not exist in any particular game. With more than two alternatives to choose from and three or more voters, the conditions required for the existence of the Core are in fact extremely severe. Moreover, the Core is generically silent on the coalitions that might be expected to form. McKelvey, Ordeshook and Winer [1978], hereafter MOW, develop an alternative solution concept known as the Competitive Solution (CS) to address these issues.

The CS is defined in terms of viable majority coalitions. A viable coalition is one that cannot be upset by any other coalition, in the sense that no member of the proposed coalition can do better for himself by voting down the coalition and it's proposal. Of course some agents may be indifferent between joining any one of several coalitions, but no agent has a positive incentive to leave any viable coalition. The implication of this is that agents who are proposing coalitions will be certain to offer "pivotal agents", who would otherwise swing their vote to another coalition and it's proposal, enough to induce them not to swing. The fact that some agents may be pivotal for several coalitions means that these coalitions will competitively bid for them to be in their coalition. This competitive bidding process will determine the set of viable coalitions as well as the specific proposals that they make.

One of the key predictions of the CS in simple spatial games

is that one will observe "minimal coalitions" forming. These coalitions will not, in general, include all agents, since all agents are rarely pivotal in simple majority rule games without a Core solution. A simple game, which has been implemented in experiments reported by Oppenheimer and Miller [1979], illustrates the predictions of the CS. Table 1 lists the preferences induced, as well as the CS, which consists of five possible proposals. Note that this is an ordinally symmetric game in the sense that all agents are treated equally with respect to payoffs. Each agent has three coalitions in the CS and each agent receives the same return over all three coalitions.

The key feature of this example is that the CS treats agents asymmetrically. Proposal 1, for example, excludes agents 4 and 5 and selects a policy outcome F which is very poor for agent 4 (his next to worse outcome) and is the worst possible outcome for agent 5. On the other hand, note that proposals 4 and 5 allow these agents to reverse the goods on agents 1 and 2! Thus the CS can include some "extreme" outcomes: the set of outcomes in the CS need not be "connected" in any sense, as one often finds with multiple Core outcomes.

We return to evaluate the results of experiments using these preferences in the next section. MOW generated experimental results of their own which strongly support the CS. They conducted eight experiments with graduates from Carnegie-Mellon GSIA. All subjects had received training in a three-person spatial experiment. The research experiment had five agents bargaining over the spatial

location of two policies. Bargaining was relatively unstructured, even compared to FP since there was no presumption that the motion on the floor needed to be modified or put (i.e., agents could use these rules if they chose, or simply propose any outcome at any time for adoption). Ordinal preferences were complete information, but no cardinal payoff information was provided and communication during the session could not explicitly reveal that information.

The specific predictions and outcomes of the MOW experiments are extremely difficult to evaluate, since they do not completely report the preference structures of agents or the outcomes. They display the preference structures in their Figure 8 (p.612), but they do not allow one to infer with any certainty the ideal points or rate of reduction of payoffs away from the ideal points. Similarly, only the coalition data is reported fully in Table 3 (p.613), although with full specifications of the preferences it would be possible to infer the selected policy outcomes.

We simply note here that MOW report that their experiments provided strong support for the CS. As we shall see below, however, others have been critical of this conclusion.

2.2 Ordeshook and Winer [1980]

Another series of experiments purporting to test the CS are reported by Ordeshook and Winer [1980]. What is interesting about these experiments is the use of "real world" spatial maps representing the preferences applicable to political parties. There are, however, several questionable aspects of this study.

The first problem is that one is never told exactly whose preferences these maps are supposed to represent: voters, party members, or parliamentary members? The second problem is that these preferences do not generate particularly sharp or interesting predictions from the perspective of testing the CS: it is geometrically apparent which parties should form coalitions and they do. Third, and related to the second point, all of the alternative models perform "well" relative to a pure chance model of coalition formation, notwithstanding the ludicrous conclusions drawn by the authors that the data clearly supports the CS.[9]

Nonetheless, this experimental design does forge new ground in establishing that one can use "realistic" preference maps in experiments, making the results more credible and relevant to policy makers. General procedures for constructing such "real world" preference maps are discussed in Aldrich and McKelvey [1977] and Poole and Rosenthal [1984]. Another experimental study to use "realistic" preference maps and rules of procedure in committee experiments is Grether, Isaac and Plott [1979]. They find that subjects select Core outcomes, which can be shown to exist in their environment.[10]

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[9] In their Table 4 Ordeshook and Winer [1980] represent four probabilities that certain hypotheses could have occurred by chance. These probabilities are all below 0.0000000558, but one is said to do significantly better than all of the others because it has about fifteen more zeroes!

[10] Caves and Salant [1987] propose, but do not conduct, an interesting series of committee experiments using "realistic" preferences estimated from the observed voting patterns of agricultural marketing boards in California.

3. FAIRNESS AND UNIVERSALISM

3.1 Miller and Oppenheimer [1982]

Miller and Oppenheimer [1982], hereafter MOpp, took issue with the strong support for the CS claimed by MOW. Their concern is somewhat broader than simply an attack on MOW, however. They begin by noting that the potential instability predicted by theoretical models of multilateral bargaining in committees with no Core has not been born out experimentally. FP were the first, in their Series 3 experiments, to make this point, but it comes out yet again in the MOW experiments which were quite stable vis-a-vis the CS. The general conclusion is that the logic of competitive coalition-breaking underlying the CS "organizes" the laboratory data well.

Political scientists have arrived at different explanations for perceived stability in field committees: the notion that committees of the whole will tend to form and seek outcomes which are best for them, ignoring the possibility that smaller majority coalitions could vote in outcomes that are better for its members (see Weingast [1979]). Given that controlled lab experiments seem to support the competitive process predicting minimal coalitions, and yet we apparently have vast field evidence of the universalism tendency, how are we to reconcile the two?

MOpp argue that the experiments of MOW do not provide a clear universalistic alternative. This is difficult to evaluate since the preferences underlying the MOW experiments are impossible to ascertain. Nonetheless, MOpp construct the parameters in Table 1

which do address their question. Option D is clearly the "fair" and deliberately universalistic alternative, giving each player between $12.25 and $2.10. Note that each player had three alternatives better than D and two that were worse. The value of D was the same for all players; in one MOpp experiment all players received $12.25, in another they all received $8.50, and so on for values of $6.72, $4.20 and $2.10.

The value of the fair alternative D was varied in this manner to determine at what point subjects would eschew considerations of fairness and pull out the asymmetric knife required to implement the CS (viz., by proposing coalitions of only three players, excluding two players). Refer to the value of D as V(D). For each of the settings of V(D), MOpp conducted five experiments.

Each of their experiments used student subjects. In four of the five experiments for each value of V(D) undergraduates were used, and in the other experiment graduate economics students were used as in MOW. Each set of subjects only participated once. The procedures of the actual experimental session parallel those of MOW: there was a period of 15 minutes for free (ordinal) discussion, followed by an unlimited amount of time to arrive at a decision. Bargaining was again face-to-face.

The experiments of MOpp differed from those in MOW in two respects. First, unlike MOW the subjects did not receive any training in three-person experiments. Second, preferences were induced directly on a finite set of six alternatives, rather then being induced "spatially" by means of a Euclidean payoff function.

There is no necessary inconsistency between these two ways of representing preferences, since one can almost always find a Euclidean representation of any given preference ordering.[11] However, we will see that the form of representation of preferences can make a difference in behavior.

To understand the MOpp results we need to determine the expected value to a subject of the CS outcomes. Recall that there were five proposals and distinct coalitions in the CS. There is no prior as to which of these will form, so agents can be presumed to entertain a diffuse prior over each CS outcome. Thus the average outcome of the CS would be the simple average of the payoff each player receives over all five elements of the CS. Denote this expected value V(CS); V(CS) equals $8.40 for the parameters in Table 1.

Their results were astounding. When V(D) exceeded V(CS) MOpp observed 9 of 10 experiments selecting D, the universalistic outcome that is not in any of the CS predictions! When V(D) was equal to $6.72 and $4.20, slightly below V(CS), we observe 4 out of 5 experiments (for each value of V(D)) selecting D again. Finally the lure of D wanes as V(D) is lowered to a mere $2.10; in this series MOpp observed only 1 of 5 experiments selecting D. What is so surprising here is to see the strength of the attraction of D

[11] Caplin and Nalebuff [1988; Proposition 1] have a nice result on the classes of ordinal preferences that can be represented with spatial Euclidean functions. Let there be m distinct proposals defined over n policies. If m is no greater than n+1 then Euclidean preferences allow all m! ordinal preferences to be represented, whereas if m exceeds n+1 then some of the m! orderings are ruled out.

even as V(D) falls below V(CS) by as much as $4.20 per subject[12].

3.2 Eavey and Miller [1984]

A natural question, which is asked by Eavey and Miller [1984] in a nice follow-up to MOpp, is whether there was some other factor about the CS that might explain why it was so unpopular relative to the CS. They point out that the CS contains five proposals that generate a wide variance in outcomes for subjects. Simply comparing the expected value of the CS prediction with the expected value of the "fair" solution D misses the possible significance of the fact that the former has a non-zero variance whereas the latter has no variance. Thus one could argue that the subjects were being risk averse in avoiding the CS until the foregone expected payoff of not selecting an element of the CS became too great to ignore.

One way to circumvent this problem would be to come up with a set of preferences that would imply a CS containing just one coalition and proposal. A simple alternative procedure, which Eavey and Miller [1984] followed, was to consider environments in which a unique Core exists and to add a universalistic alternative to these games. This way the attraction of the fair alternative could not be due to its being less risky, and must instead be attributable to some "universalistic norm" as the political scientists would have it.

The basic design of the "majority rule" experiments of Eavey

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[12] This occurs when V(D)=$4.20, recalling that V(CS)=$8.40 in this design.

and Miller [1978] is adapted from the three-person experiments originally conducted by Isaac and Plott [1978]. The original preference structure used in those experiments is shown in panel (a) of Table 2. Alternative E is claimed by Isaac and Plott [1978] and Eavey and Miller [1984] to be the unique alternative in the Core of these games, and would be voted in by players 2 and 3 to the chagrin of player 1. However, option F is also in the Core as conventionally defined, since it is perfectly symmetric with option E in the preferences of the decisive coalition of 2 and 3. Neither option dominates the other, so we conclude that the Core in this game in fact consists of two options, E and F.

In the Isaac and Plott [1978] experiments student and non-student subjects were recruited from various campuses in Pasadena (including one hospital) and were inexperienced in experiments. The rules were similar to those of FP, apart from changes to reflect the use of non-spatial preferences and the absence of a requirement to make proposals in relation to the current motion on the floor. In other words, subjects could make any motion they pleased without reference to what had been proposed before. There is no mention of any time limit on discussions. Communication over ordinal preferences was allowed, although ordinal preferences were private information to begin with. Alternative D was the default option in the event that a majority could not agree. Finally, bargaining was again face-to-face.

The results of these experiments have been taken by many commentators, including Eavey and Miller [1984; p.574], to be

broadly supportive of the Core. Twelve repetitions were reported. Alternatives E and F were selected six times. Alternative H was selected four times, and options D and B were each selected once. A success rate of only 50% does not strike this reader as broad support, although one should note that these were just a subset of the complete results of Isaac and Plott [1978].

Eavey and Miller [1984] attempted to replicate the Isaac and Plott [1978] procedures with a vastly different preference structure. They recruited undergraduates from various disciplines at Michigan State University, as well as one group of economics graduate students.[13] In all other respects the procedures of Isaac and Plott [1978], such as they are documented, were followed.

The preference structure developed by Eavey and Miller [1984], reproduced in panel (b) of Table 2, is designed to see if the support for the Core is affected by the inclusion of a clearly "fair" alternative. In this setting option E is now the unique element in the Core, and would be voted in by a coalition of players 1 and 2. The fair alternative is G, and was carefully selected for these experiments.

Option G is designed to be "fair" in the sense of being in the top half of each player's rankings, and providing a payoff of $12.20 to each player (although note that cardinal payoffs were strictly private and could not be communicated). Moreover, the Core is now less obvious than in the original Isaac and Plott [1978]

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[13] It is not clear, however, that the graduates participated in the subset of experiments conducted by Eavey and Miller [1984] that we focus on here.

design, since it is no longer the best or second best alternative of all members of the relevant coalition. Players 1 and 2 would forego $13.40 and $0.25, respectively, in order to gain a "happy coalition of the whole" in selecting option D in which player 3 would gain $9.55 relative to E. Thus player 2 pays a relatively small price to gain whatever utility he may place on satisfying the "norm of universality" that allegedly lies deep in our social psyche.

Given these considerations, then, it may not be an overwhelming surprise to find that option G was selected in eight out of ten experiments, with the Core being selected in the other two cases. Eavey and Miller [1984] attribute the substantial increase in the popularity of the Core to the lower cost to one of the Core-coalition members of foregoing the Core, which certainly seems a plausible explanation.

4. THE ROLE OF PROCEDURES

A large number of studies have been concerned with the role that specific voting procedures may have on committee behavior. This concern arises naturally from the theoretical instability of simple majority rule that is unconstrained by any particular agenda or rules governing the succession of proposals that may be considered.

We have already found that this instability does not appear behaviorally in relatively institution-free settings such as the experiments of FP and MOW. However, with the notable exception of the CS and the existence of demonstrably "fair" alternatives, there is little guidance from theory as to what might happen in these circumstances. The present body of research attempts to define procedures that do theoretically constrain outcomes in well-defined ways. The experiments are then designed to see if these constraints operate behaviorally.

4.1 McKelvey and Ordeshook [1984]

One of the simple ways in which one can constrain voting behavior and outcomes in multi-dimensional majority rule setting is to require that amendments to the motion on the floor be made and voted on one dimension at a time. Kramer [1972] shows that the non-cooperative equilibrium with this procedure is the "issue-by-issue median preference". This outcome is found by looking at preferences on each issue one at a time and finding the median over all voters

on each issue taken individually. Thus we have a natural extension of the median voter theorem which is usually stated for just one dimension. This outcome is also unique.

McKelvey and Ordeshook [1984], hereafter MOrd [1984], examine three environments, each generated by Euclidean preferences in two dimensions. The first two are comparable to the experiments in MOW, although in this case they provide complete details of the preference configurations. Indeed, they employ an interesting way of scaling payoffs in terms of probabilities that the subject would receive a cash "stake". If the outcome was at this players ideal point the probability was 1.0 that he would receive his stake, if it was some specified (Euclidean) distance from the ideal point the probability dropped to (say) 0.76 that he would receive the stake, and so on.

With one further exception, these experiments paralleled those of FP. The exception concerns the use of issue-by-issue voting procedures in one experiment. Their Open procedure allowed any committee member to make any proposal at any time, and for discussion to cover any (ordinal) aspect of the problem. Their Closed procedure constrained the proposer to be a member selected by a neutral chairman[14], and constrained all debate to be about the motion on the floor. In each of the Open and Closed procedures there could be a motion to amend in only one dimension at a time.

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[14] The chairman was a hired research assistant that was not aware of any of the theoretical predictions being tested. This is an important feature of these experiments, since one might otherwise worry that an astute chairman could manipulate the selection of proposers to bring about a favored outcome.

The three spatial preference environments employed here are denoted PC, PH and PHR, with the first having a unique Core and the latter two not having a Core. The only difference between PH and PHR is that the latter has a "stable issue-by-issue equilibrium" which is different from the CS, whereas the former does not.

Undergraduates from Carnegie-Mellon were employed in these experiments. Some of the subjects were experienced, and some were not; the paper is not more specific than this. Nineteen committees were run in the PC environment, 33 in the PH environment, and 33 in the PHR environment. These were, in turn, divided roughly equally between Open and Closed procedure experiments.

The results in the PC environment all clustered around the Core, as might be expected. The use of Closed or Open procedures had no affect on this conclusion.

The results in the PH and PHR environment were surprising. The first notable result was that the strong support for the CS found in MOW evaporated in this environment. The only changes in procedures, at least in the Open procedure design, was the restriction to modify a floor motion and to only modify it in one dimension at a time. Nonetheless, almost all of the outcomes clustered well in the interior of the convex hull of the elements of the CS.[15] In this sense the authors conclude that the data does

[15] The CS may be found in these simple spatial environments by a simple geometric construction. Connect the ideal points of each pair of committee members with a straight line (think of these as bilateral contract curves between this pair). The elements of the CS are then points on the interior of the facets of the five-sided area in the interior of all of these lines. The convex hull we refer to in the text is simply this piecewise-linear area.

not support the CS.

One could argue, however, that the subjects were behaving "as if" they had decided to randomize over elements of the CS, rather than ending up at one or the other element. This would explain the clustering of outcomes in the interior of the convex hull of CS solutions, as well as being consistent with some subjects having to forego some payoff in order to arrive at a "universalistic" outcome (discussed in the previous section). A reasonable conjecture is that the foregone payoff of this interior outcome would be quite small for all members of CS solutions.

The second noteworthy result is that the data did not appear to cluster around the stable equilibrium predicted by Kramer [1972]. However, one can argue that the use of Closed and Open procedures had some effect in moving the average outcome closer to the stable equilibrium when the Closed procedure was in effect. Despite the statistical significance of this tendency of the mean to move towards the stable equilibrium, the size of the movement is not that great (evaluated either in terms of the policy space or, we conjecture, payoff space for the subjects).

4.2 Convener Experiments

(a) Isaac and Plott [1978]

One procedure which is expected to influence committee outcomes is the "closed rule procedure" in which one agent called a "convener" has the right to veto any motion. Alternatively, this person is the only one allowed to make motions (these two

interpretations are formally the same assuming a rational convener).

The first experimental study of convener games was by Isaac and Plott [1978]. They used three-person committees with ordinal preferences induced over ten alternatives, as shown in panel (a) of Table 2. Recall from our earlier discussion that these preferences lead, in the absence of a convener, to a Core outcome of either E or F, with players 2 and 3 voting this in to the chagrin of player 1. How does this game change if player 1 is the convener? He can easily avoid these two options, by simply refusing to allow them to be voted on as amendments to the status quo, point D. Specifically, he would rationally propose H as the alternative to D, and it would win unanimously on a vote.

Isaac and Plott [1978], followed later by Eavey and Miller [1984], claim that option H is the only element of the Core of this convener game. However it is apparent that option J is also in the Core, since it is preferred to H by player 1 and is preferred to D by player 3. Thus we will assume that the Core is defined in terms of options H and J.

Series I and II of Isaac and Plott [1978] employed exactly this design. The convener could make as many proposals as he or she liked in Series I, but was only allowed to make one proposal in Series II. All eight experiments in Series I, and all five in Series II, resulted in option H being selected. Thus the convener was able to bring about a Core outcome, but not the one that he preferred.

(b) Eavey and Miller [1984]

Eavey and Miller [1984] argue that this design does not effectively test the Core, since option H could also be said to be "fair". They argue as follows:

> But is support for the core really that overwhelming? Upon close examination of the incentive charts, it appears that the core in the convener game (option H) exhibits properties of fairness. "Option H was often cited as fair because it is the first option that all three individuals have in common when working from the top of their rankings downward. It is also frequently considered to be near the 'middle'" (Isaac and Plott [1978; p.25]). Option H is also the only alternative worth more than $10.00 for every player. What if the core was chosen not because of its unique position in the dominance relation, but because it appeared to be the fair alternative?

Eavey and Miller [1984] then propose a simple extension of the Isaac and Plott [1978] incentive structure to ensure that a demonstrably fair alternative is different from the Core outcome.

Specifically, consider the preferences in panel (c) of Table 2, which are the ones employed in Series I of Eavey and Miller [1984]. Options H and J remain in the Core, but now there is a fair alternative K which gives each player the same amount and is preferred by two players (1 and 3) to the status quo. Player 1 forgoes only $1.80 relative to the Core solution H, and player 2 gains $8.20, if the fair alternative K is adopted.

The results of five experiments were that K was selected three times and Core solution J was selected twice. Clearly there is a waning in support for the Core.

In their Series II Eavey and Miller [1984] made only one change to the preferences in panel (c) of Table 2: they deleted the

fair alternative K. Over five experiments they found that Core solution J was selected three times and the default option D was selected twice. They note that "according to the subjects, D was chosen because of its fairness properties" (p.577).

(c) Kormendi and Plott [1982]

Do the results of Isaac and Plott [1978] extend from three-person committees bargaining over ten alternatives to the normal environment encountered in committee experiments of five-person committees bargaining over infinite two-dimensional space? Kormendi and Plott [1982] evaluate this question. They also introduce an innovative "duplicate method" of rewards in which subjects are rewarded by their performance relative to similar "types" in different committees. A third feature of their design is the use of class grade points as a reward medium; this is of less immediate relevance than the other two features.

Let the ideal points of individuals 1 through 5 be (30,52), (39,68), (62,109), (165,32) and (25,72), respectively. The simple majority rule version of this game with no convener has the unique Core outcome (39,68) at the ideal point of player 2. Now let player 3 be the convener. The Core changes to be the line segment joining the ideal points of players 2 and 3. Similarly, if player 4 is the convener the Core becomes the line segment joining the ideal points of players 2 and 4. Refer to these three classes of games as MR, CR3 and CR4, respectively.

Apart from the convener features and the method of payment,

Kormendi and Plott [1982] used the same procedures as FP. The subjects were drawn from two economics classes taught by Kormendi in the MBA program at the University of Chicago, which clearly could bias behavior in favor of satisfying economic theory because of the recruitment context. Moreover, the actual committee meetings in 15 of the 21 experiments were not monitored, since they were conducted as a homework exercise away from class. However, there does not appear to be any observable difference in the behavior of those committees that were monitored from those that were not.

The results of the seven MR experiments were much like those in the high payoff FP experiments. The average outcome was (40,67), which is very close to the Core outcome of (39,68).

The results in the CR3 and CR4 experiments were also clustered around the Core outcome of (39,68), but were statistically different from the MR outcomes as well as each other as predicted. Moreover, a linear regression of the outcomes in each case is not significantly different from the elongated Cores predicted in each environment.[16]

4.3 McKelvey and Ordeshook [1981]

A "vote trading" game is one in which there are a finite

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[16] Kormendi and Plott [1982; p.188] note also that this latter test would not be all that interesting if the standard errors in the estimated regression were sufficiently large, since that would mean that no data in the positive orthant would reject their hypothesis. To this end, they indicate reasonable confidence bands for their estimated regression lines, showing that they do have a non-trivial area of the positive orthant that could allow them to reject the Core prediction.

number of bills to be passed by a given number of committee members. Each members has payoffs for each bill, and the committee is to decide which of the bills are to pass. More than one bill may pass, in which case the overall payoff of each agents is simply the sum of their payoffs over all of the bills that pass.

Table 3 presents the payoffs for one such experiment conducted by MOrd [1979] using procedures and instructions detailed in MOrd [1980]. If bills C and E are passed, agent 1 would receive 10, agent 2 would receive 1, and so forth. In this game agents did have some information about other player's payoffs, although it is not obvious exactly what was provided. MOrd [1981; p.710] note that "Subjects have ordinal information about each subjects' payoffs. Specifically, they know which bills each other player prefers to pass or fail." Unfortunately these two sentences do not mean the same thing: the first sentence implies that the players know the ordinal ranking of other players, and the second sentence implies that the players know whether other players payoffs are positive or negative.

There are $2^5 = 32$ possible outcomes, in the form of packages of bills that could be passed. Over all of these outcomes, CE is the unique Core outcome.[17] It is known that if the players consider the 5 alternative **bills** sequentially, rather than considering the 32 alternative **packages** sequentially, then non-Core and even Pareto-inferior outcomes can obtain (see Riker and Brams [1976]).

[17] "CE" refers to the passing of bills C and E, and the failure of all of the rest.

MOrd [1979] observed a very low success rate for the Core in these games: based on their Table 2 results, this rate seems to be 50% rather than the 45% they report.[18] In MOrd [1981] they devised a very simple test of the hypothesis that this low success rate may be due to the subjects approaching this problem bill by bill, rather than package by package. This test was implemented by presenting each subject with preferences defined directly over all packages[19] rather than doing this indirectly via preferences over the bills.

The result of this transformation of the problem was a dramatic improvement in the success rate of the Core. All of their eight experiments attained the Core outcome in these experiments!

Unfortunately for the Core, however, MOrd [1981] go on and suggest that this excellent performance may be attributable to the Core having some "fairness" properties. In roughly the same manner as MOpp and Eavey and Miller [1984], discussed earlier, they proceed to modify their design so as to distinguish between the Core outcome and some "fair" alternative. The success rate of the Core declines dramatically, ranging from 43% with "complete ordinal

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[18] Referring to MOrd [1981; Table 2], we have 2/7 successes for the core in their "Original Game" using the "Core=CE" prediction, and 5/7 successes in their "Indifference Modification" game using the "Core=0" prediction. This gives us 7/14, which is the 50% we report.

[19] Actually they induced preferences over only 26 of the 32 packages, probably so as to be able to use a simple mnemonic such as letters of the alphabet to represent each package. The packages that were eliminated were Pareto-inferior and highly undesirable for all parties. One would not expect this to make much difference to the outcomes.

information", to 67% with "incomplete ordinal information" and experienced subjects, and up to 86% with "incomplete ordinal information" and inexperienced subjects.[20]

[20] Presumably the term "complete ordinal information" means that all players know the rankings of every player for all alternatives, whereas the term "incomplete ordinal information" means that subjects just knew the sign of other players' payoffs.

5. CONCLUSIONS

The most attractive feature of the experiments in the political science literature has been that they are generally very simple. It is relatively easy to bring some theoretical light or other intuition to bear on the results, facilitating their interpretation and, in some cases, their critique. We draw two sets of conclusions from this review. The first set concerns several problems of experimental procedure,and the second set concerns substantive insights.

5.1 Experimental Qualifications

The problems with this literature are almost entirely due to loose or incomplete experimental procedures. At the risk of some simplification, which is corrected in specific references in the text, there is very little attempt to give subjects experience in committee games. Subjects are not brought back for repeat sessions, having had time to "sleep on it".[21] They are not allowed to play the game more than once in any given experimental session, which is quite unusual in relation to most other areas of experimental economics, especially the bargaining literature which is of most direct relevance. These are facets of experimental procedure that

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[21] One particularly noteworthy exception is McKelvey and Ordeshook [1981; Table 6], in which experience has the reverse effect that one might predict: it lowers the success rate of the Core in one set of experiments, and does not improve it in another. Nonetheless, there is ample evidence from experimental economics that "experience" can matter greatly.

are relatively easily remedied if needed.

A second general problem with the experiments reported here is that they may all be affected by allowing "face-to-face" negotiations. This can be particularly problematic when one is trying to test if subjects will forego "fair" outcomes for Core or CS outcomes, since there is greater pressure to be fair in a face-to-face setting. This point has been well documented in the psychology literature (see Milgram [1974] for example). Again, the remedy is apparent: see if the same bargaining problems have different outcomes when one conducts them face-to-face as compared to being on a computer system.

A third general problem, which is shared with many other experimental studies, is the lack of replication and appallingly small sample sizes. There are many studies here that draw strong conclusions from noisy samples of five or so. One naturally wonders how robust these results are. Again the remedy is simply to increase the sample size "sufficiently" and check.

A final problem, also common to most of the experimental literature, is the question of the adequacy of incentives. Salant and Goodstein [1990] address this problem, arguing that many anomalies in the political science literature concerning departures from the Core can be easily explained by the fact that subjects were only foregoing "trivial" amounts of expected payoff by tolerating such departures. The remedy for this problem is also well-known in the experimental literature (see Harrison [1990]).

5.2 What Has Been Learned?

Granting the above qualifications to any generalizations from this literature, there remain three major insights.

The first insight, clear from the early study by FP and reinforced in most subsequent studies, is that relatively unstructured institutions can do a surprisingly good job in "simple" spatial environments. The restriction here seems to be to environments for which there is a unique Core outcome.

The second insight is a natural corollary to the first: that it is easy to construct environments for which these unstructured institutions do poorly. One way is simply to represent preferences in a non-spatial manner. Another way is to introduce outcomes that satisfy "universalist" and "fairness" norms to some degree. A constructive implication of this insight is that any bargaining institution whose equilibrium outcomes happen to satisfy these norms to some degree will find that those outcomes might be focal for reasons other than the unbridled rationality of agents.

The third major insight provides a bound on the second: there are demonstrable limits to the extent to which agents will willingly forego an efficient outcome in order to satisfy "universalist" and "fairness" norms. It is not the case that outcomes are driven solely by these norms.

TABLE 1

Preferences and Predictions in the Miller-Oppenheimer Experiments

(a) Preferences of individuals for outcomes

Value	Player 1	Player 2	Player 3	Player 4	Player 5
$14.25	F	C	B	E	A
$13.30	C,B	E,F	A,F	A,C	E,B
$12.25 V(D) $2.10	D	D	D	D	D
$0.75	A	B	E	F	C
$0.00	E	A	C	B	F

(b) Proposals in the competitive solution

Proposal	Coalition	Alternative outcome
1	C_1 = 123	F
2	C_2 = 124	C
3	C_3 = 135	B
4	C_4 = 245	E
5	C_5 = 345	A

TABLE 2

Preferences in Some Three-Person Experiments

(a) Isaac and Plott [1978]

1		2		3	
B	26.00	F	33.00	E	22.20
G	22.60	E	26.40	F	18.00
J	19.40	I	20.60	H	15.20
A	16.40	D	15.60	J	12.40
H	13.60	H	11.40	D	9.75
C	11.00	C	8.00	I	7.40
D	8.60	G	5.40	B	5.15
I	6.40	A	3.60	G	3.15
E	4.40	J	2.60	A	2.80
F	2.60	B	2.40	C	1.00

(b) Eavey and Miller [1984; Table 4]

1		2		3	
E	19.60	I	22.00	B	23.50
F	15.40	H	17.10	C	16.10
G	12.20	J	13.15	A	13.20
D	6.20	E	12.45	G	12.20
A	5.40	G	12.20	D	5.70
I	4.60	D	5.30	F	4.10
J	3.35	F	3.70	E	2.65
C	2.70	B	2.95	H	2.45
H	2.10	A	1.20	J	1.50
B	1.20	C	0.85	I	0.75

(c) Eavey and Miller [1984; Table 2]

1		2		3	
B	26.00	F	33.00	E	22.20
G	22.60	E	26.40	F	18.00
J	19.40	I	20.60	H	15.20
A	16.40	D	15.60	J	12.40
H	13.60	K	11.80	K	11.80
K	11.80	H	3.60	D	9.75
C	11.00	C	3.40	I	7.40
D	8.60	G	3.20	B	5.15
I	6.40	A	3.00	G	3.15
E	4.40	J	2.80	A	2.80
F	2.60	B	2.60	C	1.00

TABLE 3

Payoffs in the McKelvey-Ordeshook Vote Trading Experiment

	Bills				
Player	A	B	C	D	E
1	10	-2	5	4	5
2	-2	10	5	-5	-4
3	4	-8	5	3	8
4	-8	4	-3	-5	8
5	-5	-5	-4	-10	-4

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