Veto Power in Committees: An Experimental Study*

John H. Kagel
Department of Economics
Ohio State University

Hankyoung Sung
Department of Economics
Ohio State University

Eyal Winter
Department of Economics and Center for Rationality
Hebrew University

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Abstract

In a number of multilateral bargaining situations one or more players has veto power – the right to unilaterally block decisions but without the ability to unilaterally secure their preferred outcome. Our experimental outcomes show that committees with a veto player take longer to reach decisions (are less efficient) than without a veto player, that veto players proposals generate less consensus then non-veto players proposals, that veto power in conjunction with proposer power generates excessive power for the veto player, and that non-veto players show substantially more willingness to compromise than veto players, with players in the control game somewhere in between. We relate our results to the theoretical literature on the impact of veto power as well as concerns about the impact of veto power in real-life committees.

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1. Introduction

A large number of important voting bodies grant one or several of their members a veto right which allows its holder to block decisions even when a proposal has secured the necessary majority. Different voting bodies adopt the veto rule for different reasons. In the prominent case of the United Nations Security Council the rationale behind awarding permanent members a right of veto was to prevent the Council from reaching decisions that would then fail to be implemented. The US President's veto power over legislative actions was meant to allow the executive branch flexibility in conducting its policy and preserve it as a power separate from the legislature. There are a variety of institutions in which the veto power is formed rather than granted. Political parties may find themselves holding veto power because they comprise a significant number of seats in the legislative body and the legislation in question requires a supermajority to move forward (e.g., the United States Senate). Minority shareholders might have a veto position on the board of directors in a corporation as is the case with “golden shares,” sometimes used by governments who wish to maintain control over privatized companies. Whether granted exogenously or arising through the voting game, the existence of veto power often raises concerns among committee members.

The first concern is that the veto right grants its holder excessive power. The worry is that while the formal veto right only grants the power to block undesirable decisions, de facto it allows veto members to impose their ideal decision on the rest of the committee. The second concern is that the veto right inefficiently prolongs the process of decision making and stalls agreements. The two concerns above were at the core of a decades long debate within the UN General Assembly about veto power which has triggered numerous UN resolutions and various attempts to introduce procedural changes into the Council (see for example Russel and Muther (1958) and Bailey (1969)). In a less formal manner these concerns are often raised in other committees in which veto power exists.

Much of the theoretical literature about the effects of veto power in committees builds on models of the Baron and Ferejohn (1989) type used to study legislative bargaining. Winter (1996) summarizes some of the major comparative statics on committees with veto power. Among other things Winter shows that the share of power of veto members is increasing as the cost of delaying an agreement decreases, so that the share of power of non-veto members declines to zero as the cost of delay becomes negligible. Banks and Duggan (2000) derive a related result in a more general model of collective decision making. Other papers build on more specific environments, focusing primarily on the case of Presidential veto (see for example Diermeier and Myerson,1999, and McCarty, 2000).

The purpose of this paper is to offer an experimental framework by which we can analyze the effects and consequences of veto power in committees. Our objectives in this respect are twofold. First, we provide an experimental environment for testing some of the theoretical results on the effects of veto power in committees. But more importantly, we want to identify outcomes from the experimental results on which the theory is silent, and to identify implications of the outcomes reported on the debate about veto power in real-life committees.

Our experimental game is designed along the lines of Baron and Ferejohn's (1989) model of legislative bargaining and Winter's (1996) model of veto committees. Our veto committee involves three players (one of whom is a veto player) who vote on the allocation of a sum of money. To pass an agreement requires the acceptance of at least two players one of whom is the veto player. The voting game runs over a potentially unlimited number of stages. At each stage a proposer is designated randomly to propose an allocation followed by a voting phase. If the proposal passes the game terminates and the allocation is implemented. If it fails a new stage in the bargaining process begins with the selection of a new random proposer. We follow the theoretical literature by assuming that delay is costly using a common discount factor δ which represents the level of urgency that the committee faces. Our experimental design uses two levels of urgency: One with $\delta = .50$ (called urgent) and the other with $\delta = .95$ (non-urgent). (The level of urgency is a property of the issue under discussion rather than being a property of the committee.) In addition we conduct control treatments according to the same rules except that agreements are passed by a simple majority.

Our analysis focuses on four issues: (1) efficiency, (2) the extent of agreement on proposals, (3) the distribution of power (the distribution of benefits), and (4) voting patterns. In analyzing these issues we will compare results between veto committees and non-veto (control) committees holding the level of urgency constant. Hence there are four treatments altogether. For each treatment we have two inexperienced subject sessions and an experienced subject session. The main focus of the analysis is on inexperienced subject behavior for which the role played – veto player or non-veto player – was held constant, as switching roles between inexperienced and experienced subject sessions appears to affect some behaviors. And in real life committees, the role of the veto player tends to remain fixed.

We briefly summarize our main findings on the effect of veto power in committees:

1. Efficiency: Committees with veto power are less efficient (take longer to reach decisions) compared with ones with no veto power, with this difference most pronounced for non-urgent committees. It should be noted that this is a result on which the theory is completely silent, since regardless of the level of urgency, and independently of whether a veto player exists or not, the equilibrium predictions of the model are that agreement is reached with no delay.

2. Extent of Agreement on Proposals: There are significantly more minimum winning coalitions (MWCs) proposed by veto as compared to non-veto players for inexperienced subjects.[1] We believe this is a consequence of non-veto players attempting to collude against the veto player.

3. Distribution of Power: Although the point predictions of the theory fail to be satisfied regarding shares player types obtain, the comparative static implications of the theory regarding player's shares are largely satisfied; e.g., non-veto proposer's obtain larger shares than veto players in urgent committees but have smaller shares in non-urgent committees. Further, veto power in conjunction with proposer power increases the proposer's share proportionately more than the theory predicts, with proposer power increasing the veto player's share by more than 50% over their share as a coalition partner. This suggests that limiting veto players' proposer rights (e.g., limiting their ability to chair committees) would go a long way to curbing their power, a major concern in committees in which one or more players has veto power.

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[1] A MWC consists of the minimum number of players required to pass a proposal under majority rule while also accounting for the existing of a veto player in the veto games.

4. Voting Patterns: Discount rates push voting patterns in the predicted direction as there is a greater tendency to compromise in urgent committees than in non-urgent ones. Furthermore, non-veto players show substantially more willingness to compromise than veto players, with players in the control game somewhere in between.

Although the Baron-Ferejohn model is the leading formal legislative bargaining in the literature it has been subject to very limited experimental investigation until recently. McKelvey (1991) was the first person to investigate the Baron-Ferejohn model experimentally. He did so under closed amendment rule procedures with three voters choosing between three or four predetermined allocations (resulting in a mixed strategy equilibrium). His main result is that the proposer's share was substantially smaller than predicted under the stationary subgame perfect equilibrium (SSPE) for the game. Diermeier and Morton (2005) investigate the Baron-Ferejohn model focusing on varying recognition probabilities and on the share of votes that each elector controls under closed rule procedures, in an environment with a finite number of bargaining rounds and three voting blocks. They too find that coalition member shares are more equal than predicted under the SSPE, and that a majority of, but not all, allocations are for minimal winning coalitions. In a series of papers, Fréchette, Kagel and Morelli (2005a,b, in press) study the Baron-Ferejohn model and compare it with demand bargaining (Morelli,1999) and Gamson's Law (Gamson, 1961) using closed amendment rule procedures and an infinite time horizon. Their main findings are that there is support for the qualitative implications of the Baron-Ferejohn model, but serious deviations from the point predictions of the model, as proposer power is far less than predicted under the stationary subgame perfect equilibrium.[2] As a consequence, regressions using the experimental data, like those commonly employed to analyze field data, cannot clearly distinguish between the three models using the criteria commonly employed in evaluating the field data. The present paper is the first to add veto power into experimental studies of voting in committees.

The plan of the paper is as follows: Section 2 outlines the theoretical implications of adding veto power into the legislative bargaining process for our experimental games.

[2] Also see Fréchette, Kagel, and Lehrer (2003) who study the impact of closed versus open amendment rules within the framework of the Baron-Ferejohn model.

Section 3 characterizes our experimental procedures. Section 4 reports our experimental results. Section 5 concludes with a summary of our results and their broader implications.

2. The Theory

We model the process of decision making in a committee using the following version of Baron and Ferejohn's (1989) voting game. At the beginning of each bargaining round a player is selected with probability 1/3 to make a proposal. A proposal is an allocation (x_1, x_2, x_3) of the single unit of benefit among the three players, i.e., $x_i \geq 0$ and $\sum_i x_i=1$. Each proposal is voted up or down by the three members of a committee without any room for amendment. A proposal passes if it gets the support of a winning coalition. In the veto committee a winning coalition is any coalition containing at least two members one of whom is the veto player. In the non-veto committee any coalition containing at least two members is winning. If a proposal passes each player receives his proposed payoff and the game ends. If a proposal is rejected a second stage of bargaining begins with the process repeating itself, again with a random choice of proposer. Finally, if the agreement (x_1, x_2, x_3) is reached in stage t, then player i receives the payoff $x_i\delta^{t-1}$, where δ is the common discount factor.

Our theoretical benchmark is the stationary subgame perfect equilibrium (SSPE) of the game. For the veto committee, it can be shown that the (ex-ante) expected payoffs of the players in an SSPE must satisfy the following two equations:

$$u_v= (1/3)(1-\delta u_{nv})+(2/3)\delta u_v,$$

$$u_{nv}=(1/3)(1-\delta u_v)+(1/3)(1/2)\delta u_{nv},$$

where u_v is the payoff of the veto player, u_{nv} is the payoff of a non-veto player, and δ is the discount factor. The first equation asserts that the expected payoff of a veto player arises from two events. The first (with probability 1/3) involves the veto player making a proposal in which case he earns $1-\delta u_{nv}$ and the other (with probability 2/3) involves a proposal by a non-veto player under which the veto player earns δu_v. A similar equation applies to non-veto players. Here the second term refers to the event in which the proposer is the veto player, in which case each non-veto player will be selected to receive an offer with probability one half.

The ex-ante expected payoffs of the players also determine the ex-post payoffs when being selected to propose. For the veto player this is given by $u_v^*= 1- \delta u_{nv}$ and for the non veto player it's given by $u_{nv}* = 1- \delta u_v$. For our discount factors of δ = .95 and δ = .50 the equilibrium payoffs allocated within a formed coalition are given in Table 1.[3]

For our control committees where decisions are taken by a simple majority (without a veto player) the equilibrium payoffs are derived more easily. Since the three players are symmetric the *ex ante* expected payoff is a one third share for each player. In the SSPE the proposer offers this share and earns $1-\delta(1/3)$. This results in the payoffs shown in Table 2.

Two important properties of the equilibrium outcomes (for both the veto game and the control game) are the following:

1. The equilibrium outcomes are efficient in that the proposal is accepted in the first stage of the bargaining round (i.e., no delay). This results from the fact that the proposer offers his coalition member what the latter expects to earn when rejecting the proposal.
2. Only minimal winning coalitions (of two members) form in equilibrium. Put differently, the proposer should not offer positive shares to two coalition partners in equilibrium as he is better off allocating the share of one of these partners to himself and the other coalition member, thereby guaranteeing a higher share for himself with an accepted proposal.

As we will see both these properties fail to hold in our experimental results.

3. Experimental Procedures

Three subjects had to divide $30 among themselves in each bargaining round. Between 12 and 18 subjects were recruited for each experimental session, so that there would be between 4 and 6 groups bargaining simultaneously in each session. After each bargaining round, subjects were randomly re-matched in groups, with the restriction that in the veto sessions each group contained a single veto player. Subject numbers also changed randomly between bargaining rounds (but not between stages within a given bargaining round). In the veto sessions, veto players were selected randomly at the beginning of the session with their role as veto players remaining fixed throughout the session.

The procedures for each bargaining round were as follows: First all subjects entered a proposal on how to allocate the $30 among each of the three subjects in their bargaining

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PT[3] TPFor further details on the derivation of the SSPE of the game see Winter (1996) group. Then one proposal was picked randomly to be the standing proposal. This proposal was posted on subjects' screens giving the amounts allocated to each player, by subject number. If the proposal was accepted, the proposed payoff was implemented and the bargaining round ended. If the proposal was rejected, the process repeated itself (hence initiating a new stage for the same bargaining round), with the amount of money available reduced by the relevant discount factor. Complete voting results were posted on subjects' screens, giving the amount allocated by subject number, whether that subject voted for or against the proposal, and whether the proposal passed or not.[4] In veto sessions the veto player was clearly distinguished on everyone's computer screen throughout the entire bargaining process.

Subjects were recruited through e-mail solicitations from the set of students enrolled in undergraduate economics classes at the Ohio State University for the current and previous academic quarter.[5] For each treatment, there were two inexperienced subject sessions and one experienced subject session. Experienced subjects all had prior experience with exactly the same treatment for which they were recruited.[6] However, since not everyone either chose or was able to return, we did not attempt to hold type (veto or non-veto player) constant between inexperienced and experienced subject sessions. As we will see, past experience as a veto or non-veto player impacts on some behaviors. As such our analysis focuses on the behavior of inexperienced subjects, as the role of veto player tends to remain fixed in real world committees.

A total of 10 bargaining rounds were held in each experimental session with one of the rounds, selected at random, to be paid off on. In addition, each subject received a participation fee of $8. For sessions with inexperienced subjects, these cash bargaining rounds were preceded by a bargaining round in which subjects were "walked through" the contingencies resulting from either rejecting or accepting an offer. Inexperienced subject sessions lasted approximately 1.5 hours; experienced subject sessions approximately 1

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[4] Screens also displayed the proposed shares and votes for the last three bargaining rounds as well as the proposed shares and votes for up to the past three stages of the current bargaining round.

[5] This results in recruiting a broad cross-section of undergraduate students with a variety of majors. The demographic and ability characteristics of a typical experiment conducted with this recruiting method (including major, gender and SAT/ACT scores) are reported in Casari, Ham and Kagel (2005).

[6] All subjects were invited back for experienced subject sessions. In case an uneven number of subjects returned, we randomly determined who would be sent home.

hour as summary instructions were employed and subjects were familiar with the task. Although each bargaining round could potentially last indefinitely, there was never any need for intervention by the experimenters to insure completing a session within the maximum time frame (2 hours) for which subjects were recruited. Table 3 lists the number of sessions and the number of subjects in each treatment condition.

4. Experimental Results

4.1 Efficiency

Table 4 reports efficiencies and the percentage of bargaining rounds that end in stage one for both urgent (top panel) and non-urgent (bottom panel) decisions. Efficiency is calculated as the mean percentage of the maximum amount of money ($30) distributed for accepted proposals, summarizing the extent of delays along with their economic cost.

For both urgent and non-urgent committees efficiency is lower in the games with veto players than in the control treatment, regardless of experience levels. Although these differences are not statistically significant for urgent committees, they are for inexperienced subjects in the non-urgent case ($p < .01$ using a two tailed Mann-Whitney test with bargaining round as the unit of observation). Further, pooling the data for inexperienced and experienced subjects for non-urgent committees, mean efficiency is lower in the veto treatment for 15 out of the 18 bargaining rounds where the means differ ($p < .01$ using a two-tailed sign test; see Figure 1). The primary source of these efficiency differences for the inexperienced non-urgent case is that non-veto players stage-one proposals were accepted only 48.1% of the time compared to 71.4% of the time for veto players and 72.0% of the time for the controls.[7]

Finally, efficiency is lower in the $\delta = .50$ treatment than the $\delta = .95$ treatment for both veto and control sessions. This, however, is primarily the result of the much higher discount rate in the $\delta = .50$ treatment, as the average number of stages required to pass a proposal is uniformly lower in the $\delta = .50$ treatment.

Conclusion 1: Efficiency is lower in games with veto players than in the control treatment, with this effect most pronounced for non-urgent ($\delta = .95$) committees where it is significantly lower for inexperienced subjects. Underlying these efficiency differences

[7]For veto versus non-veto players $Z = 2.13$, $p < .05$, two-tailed binomial test statistic using bargaining round as the unit of observation.

is the tendency for non-veto players' proposals to be more likely to be rejected in stage-one of the bargaining process than veto players' proposals or proposals in the control treatment.

4.2 Extent of Agreements on Proposals

Table 5 shows the percentage of minimum winning coalitions (MWCs) for all proposals as well as all proposals that passed for both urgent (top panel) and non-urgent committees (bottom panel). For both the urgent and non-urgent treatments, inexperienced veto players are significantly more likely to propose and pass MWCs than non-veto players, with this tendency somewhat more pronounced for non-urgent committees.[8] However, in neither case do we find fewer MWCs in the veto games than in the control treatment. [9]

The differences in MWCs between veto and non-veto players does not extend to experienced subjects. The data is instead characterized by a sharp drop in MWCs for veto players in non-urgent committees. We suspect that this is a result of equity considerations resulting from the experienced veto players' time as inexperienced subjects. Recall that experienced subjects were not assigned the same role they played when inexperienced. For non-urgent committees four out of the five experienced veto players were non-veto players when inexperienced. The single player with past experience as a veto player always proposed MWCs in the last five bargaining rounds (just as he/she did as an inexperienced subject). None of the others even came close.[10] In contrast, ten of the eleven inexperienced veto players always proposed MWCs over the last five bargaining rounds. This suggests some consideration for the plight of non-veto players as a consequence of these veto players previous experience as non-veto players;

[8]For all proposals $Z = 1.76$, $p < .10$ ($Z = 2.30$, $P < .05$) two-tailed Mann-Whitney test using subject averages as the unit of observation for $\delta = .50$ ($\delta = .95$). For passed proposals $Z = 1.75$, $p < .10$ ($Z = 2.52$, $p < .05$) two-tailed binomial test using bargaining round as the unit of observation for $\delta = .50$ ($\delta = .95$).

[9] For inexperienced subjects in urgent committees we find significantly fewer MWCs for passed proposals in the control treatment than in the veto games ($Z = 3.13$, $p <.01$, two-tailed binomial test statistic using bargaining round as the unit of observation). This result, however, is misleading as there were relatively large numbers of non-MWCs in the control treatment in which one of the players was offered a 1/30th share or less, shares that were virtually never voted in favor of by the player in question (and which happened infrequently in all other treatments). If we consider offers that allocated a 1/30th share or less to the third player to be effectively MWCs, then 61.0% of all passed proposals were MWCs in the inexperienced control treatment, which is no longer significantly different from the corresponding veto games ($Z < 1.0$).

[10] Of the other four subjects with no experience as a veto player, in the last 5 bargaining rounds two proposed MWCs in 3 rounds, one in 1 round and the other in none of the rounds. All of the calculations in this subsection are for stage-one proposals.

i.e, the "golden rule" at work – do unto others as you would have them do unto you. As we will see below, this can be done at relatively low cost in this situation, as these proposers obtain almost as large a share of the money as their inexperienced counterparts, primarily splitting the remaining money more equally between the non-veto players than their inexperienced counterparts.

Figure 2 reports the frequency MWCs were proposed, by bargaining round, for the veto games. (We focus on all proposals as they give a better idea of players' intentions than do passed proposals.) MWCs grew substantially for inexperienced veto players: 60.6% (47.5%) averaged over the first two bargaining rounds versus 95.8% (93.6%) averaged over the last two bargaining rounds for urgent (non-urgent) committees. In contrast, there was much smaller growth in the frequency with which non-veto players proposed MWCs for urgent committees, and essentially no growth in their frequency for non-urgent committees. This, in conjunction with the significantly lower overall frequency of MWCs for inexperienced non-veto players versus veto players suggests the following story: A non-veto player offers a non-MWC frequently as a way to collude with the other non-veto player against the veto player, hoping that if the proposal fails the other non-veto player will reciprocate in the next stage of the bargaining round by proposing a non-MWC as well. The veto player doesn't need to offer a large coalition because he is assured of being a member of any proposed coalition. This argument also helps explain the growth in MWCs for non-veto players in the urgent committee case versus the absence of growth in the non-urgent case, as there were more likely to be multiple proposals for the $\delta = .95$ case, providing increased incentives for a non-veto player to propose large coalitions.

Conclusion 2: Inexperienced veto players tend to propose and pass significantly more minimal winning coalitions than non-veto proposers do for both urgent and non-urgent committees. There are substantial increases over time in the frequency with which inexperienced veto player propose MWCs, with much slower, or no growth, for non-veto players.

4.3 Distribution of Power

Table 6 shows the mean shares obtained by players as a function of who the proposer was for both urgent (top panel) and non-urgent (bottom panel) committees. Shown at the bottom of each panel are the shares predicted under the SSPE. We have included all final allocations in these calculations. Similar results are reported when restricting the analysis to MWCs (see the appendix for these results). For non-MWCs, partner's share in the case of veto proposers, and proposers in the control treatment, consists of the *largest* partner's share.

Although the quantitative predictions of the SSPE are off quite a bit – in all but one case proposers did *not* obtain nearly as large shares as predicted – the qualitative implications of the model are largely satisfied as:[11]

1. Consistent with the theory, veto players as proposers obtained larger average shares than they did as coalition partners for both urgent and non-urgent committees ($p < .01$ in all cases), and larger shares than non-veto players earned as proposers ($p < .01$ in all cases).

2. Consistent with the theory, veto players as coalition partners obtained *smaller* shares than non-veto proposers earned in urgent committees ($p < .05$ and $p = .14$ for inexperienced and experienced subjects, respectively), and obtained *larger* shares than non-veto proposers earned in non-urgent committees ($p < .01$ for both inexperienced and experienced subjects).[12] Thus, a naive argument that veto players earned larger shares strictly as a consequence of their holding veto power would be refuted by these results.[13]

3. Consistent with the theory, veto proposers earned larger shares than proposers did in the control treatment for both urgent committees ($p < .05$ and $p = .17$ for inexperienced and experienced subjects, respectively) and non-urgent committees ($p < .01$ and $p < .05$ for inexperienced and experienced subjects, respectively).

4. Shares for non-veto proposers were smaller than proposer shares in the control treatment for both urgent and non-urgent committees ($p < .01$ in all cases), consistent with the theory.

5. In urgent committees veto players as coalition partners obtained *smaller* shares than proposers did in the control treatment as the theory predicts ($p < .01$ for both

[11] Bargaining round is the unit of observation in all of these statistical tests. Unless otherwise noted all tests for statistical significance are one-tailed Mann-Whitney tests. One-tailed tests are used here as the theory makes definite predictions on all counts.

[12] A Wilcoxin signed rank test is used here.

[13] More generally, 46.0% (54.1%) of all inexperienced (experienced) non-veto players' proposals in non-urgent committees gave *more* money to the veto player than to themselves versus 13.7% (15.5%) in urgent committees.

inexperienced and experienced subjects). Further, in non-urgent committees veto players obtained *larger* shares than proposers in the control treatment for inexperienced subjects ($p < .01$), which is also consistent with the theory. This last result fails to hold for experienced subjects in non-urgent committees, which is *not* consistent with the theory. But the difference is small and not statistically significant at conventional levels.

The results reported above all involve within treatment comparisons of shares earned. There are also between treatment comparisons that are largely satisfied as a result of the sharp increase in the discount factor between urgent and non-urgent committees:

1. Proposers in the control treatment earned significantly larger shares in urgent compared to non-urgent committees ($p < .01$ for both inexperienced and experienced subjects), consistent with the theory.

2. Non-veto proposers earned significantly larger shares in urgent compared to non-urgent committees ($p < .10$ for inexperienced and $p < .01$ for experienced subjects, respectively), consistent with the theory.

3. Contrary to the theory's predictions veto proposers earned smaller shares in non-urgent committees compared to urgent committees. This difference is not statistically significant for inexperienced subjects ($Z < 1$), but is significant at the 5% level for experienced subjects.

That veto players in their role as proposers achieved substantially larger shares than non-veto proposers, or than proposers in the control treatments, is not terribly surprising given the large shares the theory predicts they will get. In what follows we compare how well veto proposers did conditional on the fact that the control treatment comes nowhere close to achieving the very large predicted differences in ex post shares between proposers and coalition partners. (As noted in the introduction, proposer power is typically substantially less than predicted in both the multilateral and bilateral bargaining literature.) That is, we calculate how well veto players are doing relative to the theory conditional on the fact that the control outcomes are not close to the point predictions of the theory (for reasons to be discussed below).

To do this we compute two complimentary measures: In both cases we use the difference between the share a veto proposer gets from the share a coalition partner gets in the control treatment as the base against which to evaluate the veto player's power.

The first measure calculates the percentage of this increased share due to the veto player's position as a proposer relative to the proposer's share in the control treatment. The second measure is designed to determine whether veto players have relatively more power as proposers as opposed to coalition partners. This has direct policy implications since, although the role of a veto player is typically fixed in committees, there is usually some flexibility in determining who is proposing (i.e., who is the committee's chair). To calculate this we take the difference between the veto player's share as a proposer versus their share as a coalition partner as a percentage of the difference between a veto player's share as a proposer and a coalition partner's share in the control treatment.

Table 7 reports these results, for both urgent (top panel) and non-urgent (bottom panel) committees. Shown at the bottom of each panel are the shares predicted under the SSPE. The first two columns show the increased share that veto power adds to proposer power. For inexperienced subjects, veto power adds to proposer power substantially more than the theory predicts for both urgent and non-urgent committees, for all coalitions and for MWCs alone. Although much smaller in magnitude, with the exception of the all coalitions category in non-urgent committees, veto power adds more to proposer power than the theory predicts for experienced subjects as well.

The last two columns of Table 7 show the increased share that proposer power adds to veto power. For non-urgent committees this is substantially more than the theory predicts for both inexperienced and experienced subjects. For urgent committees the percentage share is essentially the same as the theory predicts for inexperienced subjects, and is less than the theory predicts for experienced players. However, in all cases the increased share that proposer power adds to veto power is greater than 50%, and it is relatively larger in urgent compared to non-urgent committees. Thus, from a policy perspective (or a mechanism design perspective), to the extent that it is desirable to curb the veto player's power, there is much to be gained by limiting their proposal power; i.e., enact committee rules that either exclude, or at least rotate, the chair's position. In fact this policy implication is largely incorporated into the rules of the United Nation's Security Council where the provisional agenda for the Security Council is drawn up by the Secretary-General and approved by the President of the Security Council, with the presidency of the Council rotating among its members from month to month.

There is one final point worth making here. Recall that for non-urgent committees experienced veto players proposed and passed only about a third MWCs, which could be interpreted as a move towards more egalitarian payoffs. But average shares for these veto proposers is only slightly less than the average shares of their inexperienced counterparts who proposed MWCs much more often. Thus, the more egalitarian payoffs suggested by the reduced frequency of MWCs did not extend to very large reductions in the veto player's own payoffs, but were largely limited to sharing the coalition partners payment equally between both non-veto players.[14] This last result is reflected in the relatively large reduction in the non-veto partner's share going from inexperienced to experienced subjects in both types of committees.

Conclusion 3: As proposers, veto players' shares are substantially larger than those of non-veto players, while shares of players in the control game are somewhere in-between. Hence the qualitative implications of the model regarding proposer power are satisfied, although the point predictions fail as the model assigns an even larger share for the veto player. Furthermore, proposer power adds substantially to veto power (especially in urgent committees), adding proportionately more power than what the theory predicts.

4.4 Voting Patterns

Figure 3 pools the data over experience levels and plots the empirical cumulative density functions (cdf) for the frequency with which the different player types voted in favor of the shares they were offered. Votes of proposers are excluded from the analysis. For both urgent and no-urgent committees the data line up qualitatively the way the theory predicts except for a handful of observations involving very large shares that were rejected by non-veto players in non-urgent committees as (i) the empirical cdf for veto players stochastically dominates that of the controls (i.e., at every share level, the controls were more likely to vote in favor of a proposal than were the veto players) and (ii) the empirical cdf for the controls stochastically dominates that of the non-veto players.

However, the quantitative predictions of the SSPE were clearly not satisfied as both non-veto players and controls were unwilling to accept anything approaching the SSPE share as coalition partners, as were veto players in the case of urgent committees. In contrast, veto players were willing to accept much smaller shares than predicted in

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[14] For non-MWCs that passed, veto players as proposers averaged 53.9% of the pie (1.94 standard error of the mean) with shares for both non-veto players being split equally (or close to equally) for nine of these fourteen proposals.

non-urgent committees. For example, in non-urgent committees non-veto players should have accepted offers of 7.6% of the pie or more, and controls should have accepted offers of a 31.7% share or more according to the SSPE. But in both cases such offers had a little less than a 10% chance of being accepted. In contrast, for non-urgent committees veto players should have rejected all shares less than 79.8% of the pie. But this clearly did not happen, as shares just above the 50% level had better than a 50% chance of being accepted. As a result, coalition partner shares predicted under the SSPE would have generated much lower expected payoffs to proposers than the shares actually offered for both urgent and non-urgent committees.[15]

Figure 3 also reveals a number of focal points in voting. For veto players there is a focal point at the 50% split that generates large numbers of acceptances for both urgent and non-urgent committees, with a somewhat smaller focal point at the 33.3% split for urgent committees as well. For non-veto players there is a focal point at the 33.3% split for both urgent and non-urgent committees. For the control treatments there is a focal point at the 33.3% split for both urgent and non-urgent committees, and a focal point at the 50% split for the non-urgent controls, with a much smaller focal point at around 45% for the urgent controls. Note, these focal points are to a considerable extent sensitive to the conditions underlying the bargaining process. For example, the focal point of a 33.3% share for veto players in urgent committees is not found in non-urgent committees where veto players have much greater power because of the high discount factor. Similarly, there is a large equal split focal point for the controls in non-urgent committees which is not present in urgent committees where, because of the low discount factor, potential coalition partners are much weaker. Further, all player types are as likely, or more likely, to vote in favor of smaller shares in the presence of the lower discount factor associated with urgent versus non-urgent committees, as the theory predicts.

The voting patterns of non-veto players and coalition partners in the control treatment are quite similar to those reported in earlier experimental studies of the Baron-Ferejohn bargaining model – a minimum threshold for accepting offers that is typically well above the SSPE level. This has generally been attributed to “fairness” or “equity”

[15] This result is based on the detailed analysis of voting patterns reported in Sung (2005) based on probit estimates of voting.

considerations, and is characteristic of the bilateral bargaining literature as well (see Roth, 1995, for a review of this literature). However the veto players in the non-urgent committees show the opposite pattern, a minimum threshold for accepting offers that is *less than* the SSPE level. The closest counterpart to a veto player in these earlier infinite horizon multilateral bargaining games is that of an Apex player who controls more votes than other players but cannot, by herself, block a proposed allocation. Here too the "strong" player tends to accept a relatively smaller share than predicted under the SSPE as a coalition partner (Fréchette, Kagel and Morelli, in press). We believe that equity considerations could drive both the present results and the Apex results since in both cases the strong player as coalition partner averages more than 50% of the pie. This would seem to be more than "fair," particularly since the strong player as proposer is unable to get potential coalition partners to accept anything approaching the small share predicted under the SSPE.

Finally, given the focal points reported in Figure 3, and the role of equity considerations, or other regarding preferences, reported in the bargaining literature it is of some interest to determine how frequently subjects proposed equal splits of the $30, and whether these frequencies varied with player types. Figure 4 reports these results. We consider two types of equal splits: (i) when proposers divided the money equally between all three players and (ii) when proposers divided the money equally between two of the three players, with a zero allocation to third player. In both cases we classify outcomes in terms of "approximately" equal splits.[16] There are relatively few cases of equal splits all around reaching a maximum of 18.2% of all non-veto players' proposals in urgent committees and a low of 0.9% for veto players in the same committees. We find similar disagreements as a function of player power for the ½, ½, 0 case with 29.8% of non-veto players in non-urgent committees proposing splits of this sort (along with 33.3% of the controls) versus 4.6% of the veto players in these same committees. Thus, in general, non-veto players were much more "equality" minded than veto players, with control players' sense of equity changing substantially with changes in the discount factor.

[16] For the case of equal shares all around we permit a maximum difference of $0.30 between the largest and smallest share, and for splits of ½, ½, 0 we permit a maximum difference of $0.30 for the two players receiving the largest shares and a share of no more than $1.00 for the "zero" share player. Results reported are robust to using precise definitions of equality.

Conclusion 4: Voting patterns indicate a higher tendency to accept proposals in urgent committees (with low discount factor) than in non-urgent committees at almost all offer levels. Furthermore, veto players require a substantially larger share than what non-veto players require in order to vote in favor of a proposal, with coalition partners in the control treatment being somewhere in between.

5. Summary and Conclusions

Veto power has a substantial effect on the functioning of committees. First, it prolongs the process of decision making, especially in non-urgent committees which face no exogenous pressure to reach fast decisions. Second, it awards veto members excessive bargaining power particularly when combined with proposer power (though not as much as the theory predicts). Third, veto power can lead to less consensus (smaller winning coalitions) when veto members initiate proposals then when non-veto players initiate proposals. Our experimental results, which expose these side effects, on which concerns are often raised in real-life committees, also suggest some means to diminish them. First, limiting the proposal power of veto members by, for example, restricting the veto player's role as committee chair, can serve to reduce the second effect. Such a measure seems to be particularly effective in urgent committees. Limiting the proposer power of veto players would also have the beneficial side effect of increased consensus (fewer minimal winning coalitions). Second, to the extent that the proposal power of the veto player is restricted, the veto player's power can be further restricted by introducing deadlines on decisions. In contrast, when veto players have substantial proposal power introducing deadlines only adds to their power so that such deadlines should be resisted by non-veto players.

Our results also have some interesting general implications for bargaining behavior. First, they confirm previous findings suggesting that although fairness considerations are essential to explaining bargaining results these results are also responsive to strategic considerations. But more importantly, our results hint at the fact that subjects' perceptions about fairness are affected by strategic considerations. For urgent (non-urgent) committees inexperienced non-veto players propose ether equal shares for all, or an equal split between themselves and one other player, in 52.3% (38.5%) of all stage-one proposals versus 21.0% (51.3%) of the controls players' proposals for the same committees, and 20.9% (12.9%) of the veto players' proposals.[17] This suggests that the fairness reference point shifts as a function of the underlying rules of the game. Models of social preferences such as Fehr and Schmidt (1999) or Bolton and Ockenfels (2000), and even some of their extensions that incorporate issues of reciprocity and efficiency (Charness and Rabin, 2002, Dufwenberg and Kirchsteiger, in press) ignore this feature. Indeed, general models that take such features into account are likely to turn out to be exceedingly complex. But this suggests that modeling of social preferences should be tailored for specific strategic environments.

As is the case with many other experimental studies one might wonder whether the consistencies between the theory and behavior reported here result from the reasons the theory suggests or some other factors. Solving for the precise equilibrium of the game is probably beyond our subjects' capabilities with the training and tools at their disposal, and we are not suggesting that they do so. Nevertheless we believe that the fundamental economic forces and the basic intuition underlying the theory are sufficiently transparent for subjects to recognize and to respond to in their behavior. The key here is the effect of the change in the discount factor between urgent and non-urgent committees. In non-urgent committees it is reasonably transparent that veto players can more readily afford to reject offers that do not provide them with a relatively large share of the pie than in urgent committees, as it is much less expensive in terms of future expected income to reject such offers. Although our subjects probably cannot compute the precise continuation value of any offer between the two treatments, a difference of this sort should be reasonably transparent. And this is the fundamental economic force at play between the two treatments resulting in the equilibrium prediction that veto players will obtain smaller (larger) shares as coalition partners in urgent (non-urgent) committees than non-veto proposers. One unanticipated corollary of this fact that shows up in the data is that there are more frequent rejections of non-veto players' offers in the non-urgent compared to urgent committees for inexperienced players, as veto players are more resist to small shares in non-urgent committees. Thus, while we would not anticipate that the *point* predictions of the theory would be satisfied because (among other things) agents

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[17] Using the approximately equal split definitions developed earlier. Note that ½, ½, 0 splits by non-veto players almost always give the ½ share to the veto player.

cannot readily compute precise continuation values, the fundamental behavioral forces at play in the theory would appear to be reasonably transparent.

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δ= 0.95				δ = 0.50			
Veto Proposer		Non-Veto Proposer		Veto Proposer		Non-Veto Proposer	
Veto	Non-Veto	Veto	Non-Veto	Veto	Non-Veto	Veto	Non-Veto
92.4 %	7.6 %	79.8 %	20.2 %	85.7 %	14.3 %	21.5 %	78.6 %

Table 1: Equilibrium Payoffs for the Veto Game

δ = 0.95		δ = 0.50	
Proposer	Partner	Proposer	Partner
68.3 %	31.7 %	83.3 %	16.7 %

Table 2: Equilibrium Payoffs for the Control Game

Treatment		Experience Level	Number of Subjects
δ = .95	Veto game	Inexperienced	33
		Experienced	15
	Control game	Inexperienced	30
		Experienced	15
δ = .50	Veto game	Inexperienced	33
		Experienced	15
	Control game	Inexperienced	30
		Experienced	12

Table 3: Number of Subjects Per Treatment

			Efficiency[a]	Veto Proposer	Non-Veto Proposer	All Proposers
Urgent (δ= .50)	Veto Game	Inexp	92.8 % (1.84)	86.1 % {31/36}	87.8 % {65/74}	87.3 % (1.16) [4]{96/110}
		Exp	95.0 % (2.14)	100 % {14/14}	86.1 % {31/36}	90.0 % (1.09) [2]{45/50}
	Control Game	Inexp	93.6 % (1.88)	89.0 % (1.15) [4]{89/100}		
		Exp	96.9 % (2.23)	95.0 % (1.1) [3]{38/40}		
Non-Urgent (δ= .95)	Veto Game	Inexp	95.5% (0.69)	71.4 % {20/28}	48.1 % {39/81}	54.1 % (1.95) [12]{59/109}
		Exp	97.8 % (0.57)	66.7 % {12/18}	71.9 % {23/32}	70 % (1.46) [5]{39/50}
	Control Game	Inexp	98.1 % (0.33)	72 % (1.4) [4]{72/100}		
		Exp	98.3 % (0.58)	78 % (1.4) [5]{39/50}		

Table 4: Percentage of Bargaining Rounds that End in Stage 1 and Efficiency

Average number of stages in parenthesis. Maximum number of stages in brackets. Raw data in braces
[a] Standard error of the mean in parentheses.

			All Proposals			Passed Proposals		
			Veto Proposer	Non-Veto Proposer	Overall	Veto Proposer	Non-Veto Proposer	Overall
Urgent (δ= .50)	Veto	Inexp	79.7 % (102/128)	55.1 % (141/256)	63.3 % (243/384)	76.5 % (26/34)	59.2 % (45/76)	64.5 % (71/110)
		Exp	70.9 % (39/55)	76.4 % (84/110)	74.5 % (123/165)	64.7 % (11/17)	78.8 % (26/33)	74.0 % (37/50)
	Control	Inexp	42.6 % (147/345)			43.0 % (43/100)		
		Exp	76.7 % (99/129)			82.5 % (33/40)		
Non-Urgent (δ= .95)	Veto	Inexp	78.4 % (167/213)	46.2 % (197/426)	60.0 % (364/639)	72.3 % (34/47)	48.4 % (30/62)	58.7 % (64/109)
		Exp	39.7 % (29/73)	30.8 % (45/146)	33.8 % (74/219)	26.3 % (5/19)	35.5 % (11/31)	32.0 % (16/50)
	Control	Inexp	59.2 % (245/414)			61.0 % (61/100)		
		Exp	79.9 % (163/204)			90.0 % (45/50)		

Table 5: Percentage of Minimum Winning Coalitions
(raw data in parenthesis)

		Veto Treatment				Control Treatment	
		Veto Proposer	Non-Veto Partner[a]	Veto Partner	Non-Veto Proposer	Proposer	Partner[a]
Urgent (δ= .50)	Inexperienced	59.1 % (2.4)	36.5 % (2.2)	42.2 % (1.0)	44.6 % (1.7)	50.5 % (1.0)	40.6 % (1.0)
	Experienced	61.8 % (2.1)	30.7 % (1.9)	46.1 % (1.0)	50.2 % (1.4)	59.1 % (1.3)	38.6 % (0.9)
	Predicted Share	*85.7 %*	*14.3 %*	*21.5 %*	*78.6 %*	*83.3 %*	*16.7 %*
Non-Urgent (δ= .95)	Inexperienced	58.8 % (1.6)	37.0 % (1.0)	50.7 % (1.0)	41.2 % (1.6)	45.8 % (1.1)	44.0 % (0.7)
	Experienced	57.0 % (1.8)	30.0 % (1.7)	50.2 % (1.3)	37.4 % (1.8)	52.9 % (0.9)	45.6 % (0.8)
	Predicted Share	*92.4 %*	*7.6 %*	*79.8 %*	*20.2 %*	*68.3 %*	*31.7 %*

Table 6: Mean Shares Obtained by Players
(standard error of the mean in parenthesis)

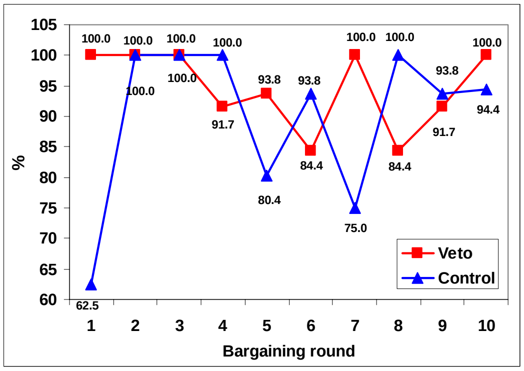
[a] In case of non-MWCs, partner's share consists of the largest share allocated to a coalition partner.

		Veto Power Adds to Proposer Power[b]		Proposer Power Adds to Veto Power[c]	
		Inexperienced	Experienced	Inexperienced	Experienced
Urgent (δ= .50)	All coalitions	46.5 %	11.4 %	91.4 %	67.7 %
	MWCs	41.3 %	11.2 %	92.2 %	67.2 %
	Predicted[a]	*3.5 %*	*3.5 %*	*93.0 %*	*93.0 %*
Non-Urgent (δ= .95)	All coalitions	87.8 %	36.1 %	54.6 %	59.6 %
	MWCs	69.4 %	49.4 %	68.6 %	55.7 %
	Predicted[a]	*39.7 %*	*39.7 %*	*20.8 %*	*20.8 %*

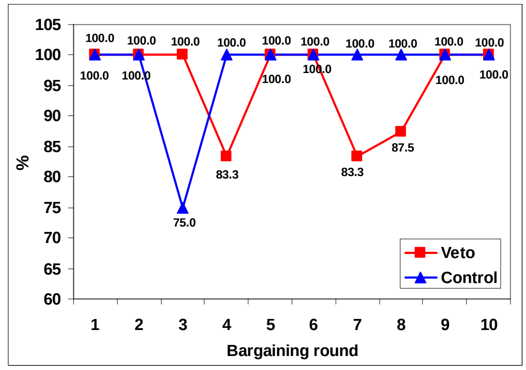
Table 7: Veto Player's Power: Predicted vs. Actual

[a]For MWC's
[b] [U(pr, v) – U(pr, c)]/[U(pr, v) – U(pa, c)] * 100
[c] [U(pr, v) – U(pa, c)]/[U(pr, v) – U(pa, c)] * 100
where U(pa, c) and U(pr, c) are payoffs of coalition partner proposer in control and U(pr, v) and U(pa, v) are payoffs of veto player as proposer and coalition partner.

Urgent (δ= .50)

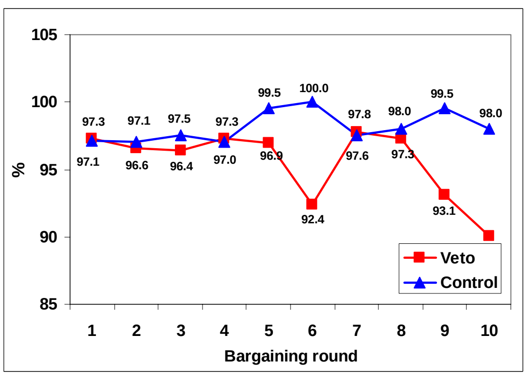


(a) Inexperienced subjects

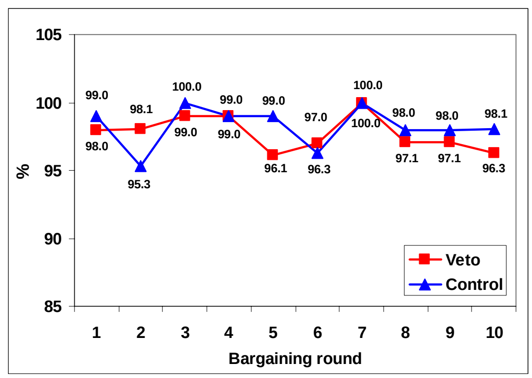


(b) Experienced subjects

Non-Urgent (δ= .95)



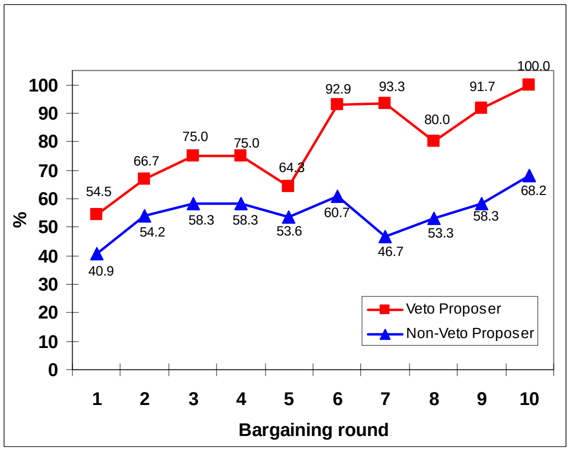
(c) Inexperienced subjects



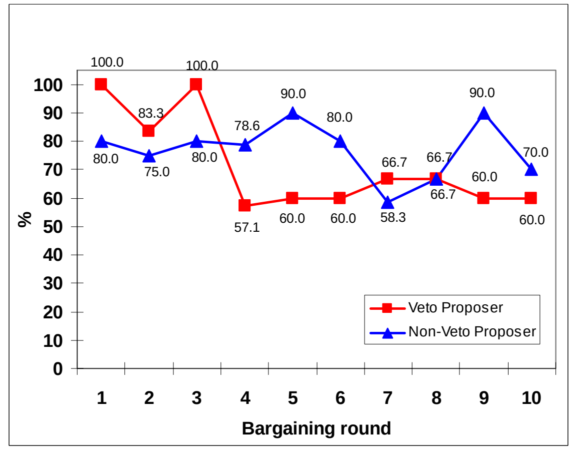
(d) Experienced subjects

Figure 1: Mean Efficiency Over Time

Urgent (δ= .50)

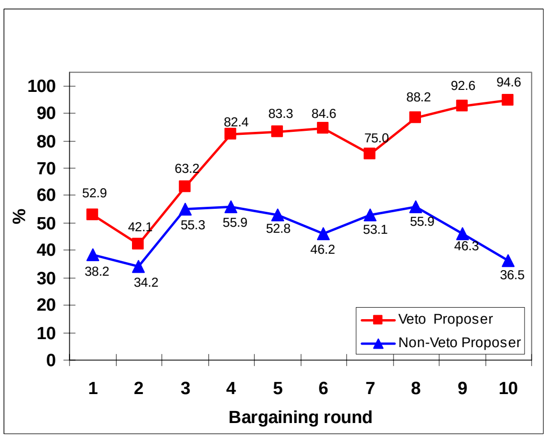


(a) Inexperienced subjects

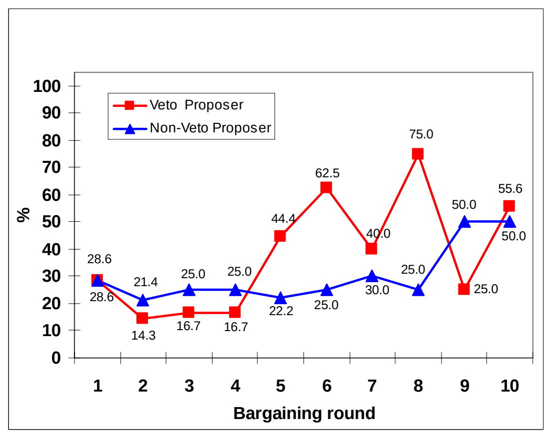


(b) Experienced subjects

Non-Urgent (δ= .95)



(c) Inexperienced subjects



(d) Experienced subjects

Figure 2: Percentage of MWCs: All Proposals

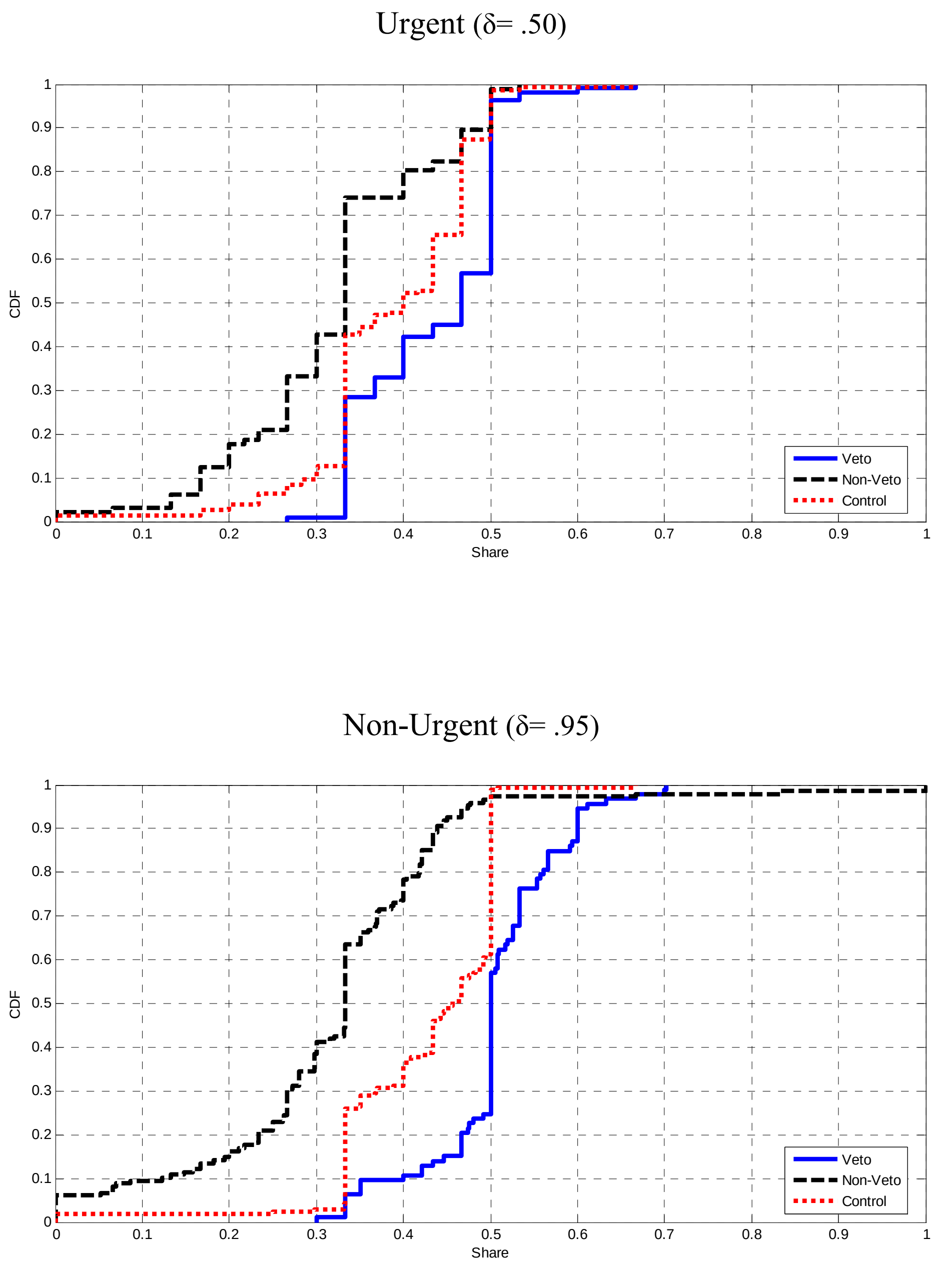


Figure 3: The Cumulative Density Function for Voting in Favor of a Proposal as a Function of Share Offered ($\delta = .95$)

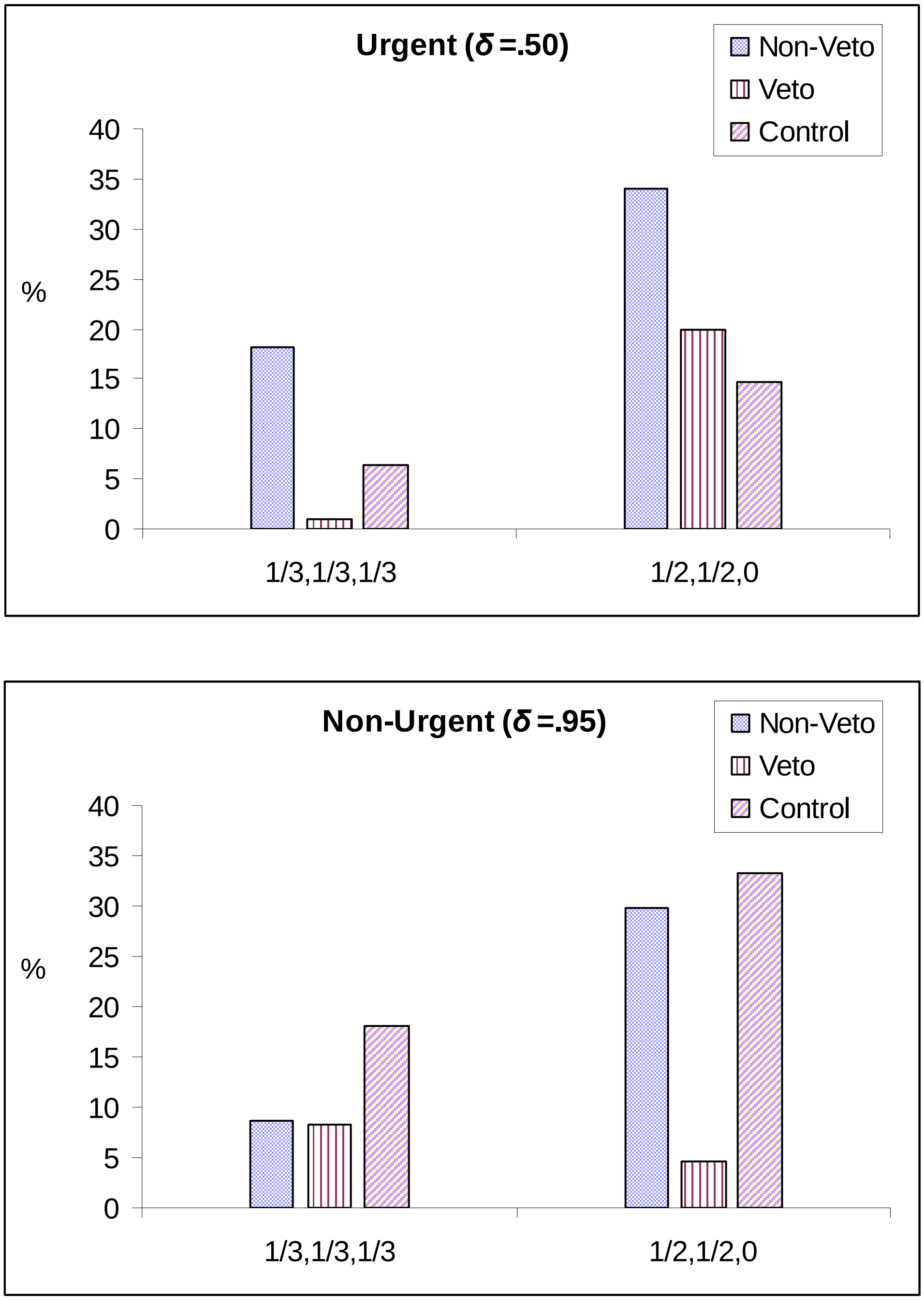


Figure 4: Equal Share Allocations by Player Type
(inexperienced subjects, stage-one only)

Appendix: Mean Shares Obtained for MWCs Only
(standard error of the mean in parentheses)

		Veto Treatment				Control Treatment	
		Veto Proposer	Non-Veto Partner	Veto Partner	Non-Veto Proposer	Proposer	Partner
Urgent (δ= .50)	Inexperienced	62.7 (2.5)	37.3 (2.5)	46.1 (1.1)	50.5 (2.3)	55.3 (1.1)	44.7 (1.1)
	Experienced	64.9 (1.6)	35.2 (1.6)	46.9 (1.0)	53.1 (1.0)	61.9 (0.8)	38.1 (0.8)
	Predicted Share	*85.7 %*	*14.3 %*	*21.5 %*	*78.6 %*	*83.3 %*	*16.7 %*
Non-Urgent (δ= .95)	Inexperienced	62.4 (1.3)	37.6 (1.3)	52.3 (0.6)	46.6 (1.3)	52.2 (0.5)	47.9 (0.5)
	Experienced	61.6 (3.1)	38.4 (3.1)	53.0 (1.6)	47.0 (1.6)	53.9 (0.8)	46.1 (0.8)
	Predicted Share	*92.4 %*	*7.6 %*	*79.8 %*	*20.2 %*	*68.3 %*	*31.7 %*