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Veto power in committees: an experimental study

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Abstract Veto power consists of the right of one or more players to unilaterally block decisions but without the ability to unilaterally secure their preferred outcome. Our experiment shows that (i) committees with a veto player take longer to reach decisions (are less efficient) and generate less consensus than without a veto player, (ii) veto power substantially enhances proposer's power, and (iii) non-veto players are substantially more willing to compromise than veto players. We relate our results to the theoretical literature on the impact of veto power as well as to concerns about the impact of veto power in real-life committees.

Keywords Veto power · Bargaining · 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 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 although 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. These concerns 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, Russell 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. He shows that the veto player's share of power is increasing as the cost of delaying an agreement decreases, so that non-veto members' shares decline 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 for analyzing 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, as well as implications of the results for the debate about veto power in real-life committees.

Our experimental game is designed along the lines of the Baron and Ferejohn (1989) model of legislative bargaining and Winter (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. Passing a proposal requires the acceptance of at least two players, one of which 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 the process repeats itself, beginning 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 cost of delay that the committee faces along with the ability to convene frequent meetings to consider proposals.[1] Our experimental design employs two values for δ: $\delta = .50$ (the high delay cost case) and $\delta = .95$ (the low delay cost case).[2] In addition, we conduct control treatments without a veto player using the same rules (and computer interface) except that agreements are passed by a simple majority.

Our analysis focuses on four issues: (1) efficiency, (2) the distribution of power/benefits, (3) the extent of agreement on proposals, and (4) voting patterns. In analyzing these issues we compare results between veto committees and non-veto (control) committees holding the cost of delay constant, as well as between veto and non-veto players. Our main findings are summarized below:

1. *Efficiency*: Committees with veto power are less efficient (take longer to reach decisions) than ones with no veto power, with this difference most pronounced in the case of low delay costs. This is a result on which the theory is completely silent, since regardless of the cost of delay, and independently of whether a veto player exists or not, the model predicts that agreements are reached without delay in equilibrium.
2. *Distribution of Power*: The existing literature on legislative bargaining games focuses on the strong power that proposers have with respect to the command of the available resources. However, both the theory and experimental results support the idea that veto players as coalition partners obtain significantly larger shares that non-veto proposers with low delay costs. This indicates that in many legislative actions where the delay between proposals is typically quite short, veto power may be a substantially more important issue than proposer power. Further, previous experimental work on games of this sort show that proposers get larger shares than coalition partners, but these shares fall well short of predicted levels (see the brief review of previous research reported on below). Our experiment shows that veto power substantially enhances proposer power, well above what the theory predicts.

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[1] That is to say, high delay costs can be offset by more frequent meetings and low delay costs increased by less frequent meetings.

[2] One might argue that $\delta = .50$ is too high a cost of delay to be realistic. However, for experimental purposes it is excellent for establishing strongly contrasting predictions relative to the $\delta = .95$ case.

3. *Extent of Agreement on Proposals*: There are significantly more minimal winning coalitions (MWCs) proposed by veto as compared to non-veto players.[3] Our data suggests that this is a consequence of tacit collusion between non-veto players attempting to offset the power of the veto player.
4. *Voting Patterns*: Discount rates push voting patterns in the predicted direction as there is a greater tendency to compromise in high than in low delay cost cases. Further, non-veto players show substantially more willingness to compromise than veto players, with players in the control games falling somewhere in between.

Although the Baron–Ferejohn model is the leading formal legislative bargaining in the literature, it has been subject to 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 et al. (2005a, 2005b, 2005c) 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.[4] The present paper was the first to explore veto power in experimental studies of voting within the context of the Baron–Ferejohn legislative bargaining model.[5] The most important result of the present paper in terms of these earlier findings is the large increase in proposer power that results from adding veto power to proposer power.

There is also a substantial body of experimental work on committee decision making using spatial voting that emerged during the late 70s and 80s. Unlike our framework, in this literature money is not assumed to be transferable. Instead, proposals involve two-dimensional vectors representing policies on two issues and voter payoffs are measured in terms of the distance between a player's ideal point and the implemented policy (see McKelvey and Ordeshook 1990, for a detailed survey of this literature). In the experiments that grant the power of agenda setting to one of

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

[4] See also Fréchette et al. (2003) who study the impact of closed versus open amendment rules within the framework of the Baron–Ferejohn model.

[5] A subsequent experiment by Drouvelis et al. (2007) also looks at a veto power treatment within the context of Winter's extension of the Baron–Ferejohn model. They report very similar results to ours, which we briefly discuss in the concluding section of the paper.

the players, with either simple majority voting or qualified majority voting (without a formal veto player), the theoretical literature predicts that the agenda setter can implement her ideal point as the outcome of the voting game. However, the experimental results do not support this prediction (see, for example, Berl et al. 1976; Fiorina and Plott 1978; Hoffman and Plott 1983; Eavey and Miller 1984). The reduced power of the agenda setter in these experiments, as compared with the theory, is related to our experimental observation that proposers, whether they are veto players or have no veto power as in our control treatments, do not earn as much as the theory predicts.

We are aware of two spatial voting experiments that provide players with the power to block proposals, but no individual, including the "veto" player, has agenda setting power (Wilson and Herzberg 1987; Haney et al. 1992). In the control treatment, absent a veto player and absent a core outcome, with a simple majority voting rule outcomes can, in theory, potentially end up anywhere in the outcome space. Introducing a veto player yields the theoretical prediction that outcomes will coincide with the veto player's ideal point, as the veto player will simply exercise his veto power until this outcome is achieved. The experiment shows that although outcomes do not fall precisely at the veto player's ideal point, they systematically favor the veto player, in contrast to the control treatment where no single member appears to be advantaged (Wilson and Herzberg 1987).[6] The closest analogue to this in our design is when a non-veto player is the proposer, in which case the veto player can also exercise "negative proposer power," and is predicted to get a larger share than coalition partners in the control treatment.[7] Our data confirms this prediction, but once again payoffs are far more egalitarian than predicted. Further, beyond the fact that these two papers deal with a spatial environment while ours deals with a distributive environment, they do not deal with comparative static results, which are at the center of our analysis.

The plan of the paper is as follows: Section 2 outlines the theoretical implications of introducing veto power into the legislative bargaining process for our experimental games. 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, including the large and growing body of literature on other-regarding preferences.

2 The theory

We model the process of decision making in a committee using the following version of Baron and Ferejohn (1989) voting game. Under our main experimental treatments, 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.

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[6] Haney et al. (1992) employ a somewhat different setup but obtain very similar results.

[7] Although not statistically significant, it is nevertheless similar to our result that the time it takes to get a proposal passed is longer with the veto player than without.

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 which 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 of one half.

The ex-ante expected payoffs of the players also determine the ex-post payoffs of a player when acting as a proposer. 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 $\delta = .95$ and $\delta = .50$ the equilibrium payoffs allocated within a formed coalition are given in Table 1.[8]

Note that for low delay costs the predicted ex-post payoff for the veto player as coalition partner is greater than that of the non-veto proposer. This outcome is essentially supported by the large share the veto player gets as proposer in conjunction with the small shrinkage in the amount of money to be allocated; i.e., the veto player can afford to wait her turn as proposer if the share allocated is too small. In contrast, with high delay costs the share of the veto player as coalition partner is less than that of the non-veto proposer as a consequence of the high cost of delay. We view this

Table 1 Equilibrium payoffs of the veto game

High delay cost ($\delta = .50$)				Low delay cost ($\delta = .95$)			
Veto proposer		Non-veto proposer		Veto proposer		Non-veto proposer	
Veto	Non-veto	Veto	Non-veto	Veto	Non-veto	Veto	Non-veto
85.7%	14.3%	21.5%	78.6%	92.4%	7.6%	79.8%	20.2%

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[8] For further details on the derivation of the SSPE of the game, see Winter (1996).

Table 2 Equilibrium payoffs of the control game

High delay cost ($\delta = .50$)		Low delay cost ($\delta = .95$)	
Proposer	Partner	Proposer	Partner
83.3%	16.7%	68.3%	31.7%

contrasting prediction as one of the key comparative static implications of the model as to whether the behavioral forces underlying the theory are actually at play in the experiment.

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 $1 - \delta(1/3)$ (see Table 2).[9]

Two important properties of the equilibrium outcomes for both veto and control games are the following:

1. The equilibrium outcomes are efficient as proposals are accepted in the first stage of any given bargaining round (i.e., no delay). This is a consequence of proposers offering a coalition member what she 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 a positive share to one of the two other players in equilibrium as any money allocated to the redundant player can be better allocated to one's own payoff and to the non-redundant player, thereby increasing the likelihood of the proposal passing.

After completing our main experimental treatments, we conducted an additional low delay cost treatment in which we reduced the recognition probability of the veto player to 1%. This treatment was motivated by the fact that although the emphasis in the literature on legislative bargaining games is on proposer power, with equal recognition probabilities, the presence of a veto player blunts this proposer power, both in theory and in our experimental results. Within the theory, it takes recognition probabilities well below 10% for the veto player's share to drop well below 50%. Reducing the recognition probability of the veto player to 1% reduces her predicted share to 9.1% as a coalition partner.[10] This provides a convenient "stress test" of the theory: does veto power still trump proposer power in this extreme case?

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[9] There is an interesting, and somewhat counter-intuitive, contrast between the effect of the high delay cost on proposer power as the veto player's power shrinks a bit with $\delta = .50$ but it increases substantially for the control treatment and for non-veto proposers. The latter is the proximate cause for the reduction in the veto player's power. More generally, the veto player's share as proposer does not change monotonically with changes in δ, and reaches a minimum of 83.2% when $\delta = .71$.

[10] All other predictions of the theory continue to hold in this case. We chose the 1% recognition probability over a zero recognition probability as the equilibrium analysis for the latter is based on a corner solution.

3 Experimental procedures[11]

Three subjects had to divide \$30 in each bargaining round. Between 12 and 18 subjects were recruited for each experimental session, so that there were between 4 and 6 groups bargaining simultaneously in each session. After each bargaining round, subjects were randomly re-matched, with the restriction that in the veto sessions each group contained a single veto player. Subject identification numbers also changed randomly between bargaining rounds (but not between stages within a given bargaining round) to preserve anonymity. 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 for their group. Then one proposal was picked randomly to be the standing proposal. This proposal was posted on subjects' screens, displaying 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 (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, displaying the amount allocated by subject number, whether that subject voted for or against the proposal, and whether the proposal passed or not.[12] 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. There were two inexperienced subject sessions for each treatment.[13]

A total of 10 bargaining rounds were held in each experimental session with one of the rounds, selected at random, to be paid off. In addition, each subject received a participation fee of \$8. 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. Although each bargaining round could potentially last indefinitely, there was never any need for intervention by the experimenters to ensure completing a session, with sessions lasting approximately 1.5 hours. Table 3 lists the number of subjects in each treatment condition.

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[11] For the experimental instructions, see http://www.econ.ohio-state.edu/kagel/veto_insts.pdf.

[12] 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.

[13] We also ran one experienced subject session for each veto treatment. (Inexperienced subjects did not know that they would be asked to return for a second session.) However, we were unable to hold player type (veto or non-veto) constant across sessions, which resulted in a number of spurious results reflective of subjects' past roles; primarily in terms of veto players with past experience as non-veto players providing substantially fewer MWCs. Since we believe these results to be spurious (while also being rather tedious to report), and the role of veto player tends to remain fixed in real-world committees, we only report results for inexperienced subject sessions. Results for these experienced subject sessions are available from the authors on request.

Table 3 Number of subjects per treatment

Treatment		Recognition probabilities	Number of subjects[b]
$\delta = .95$	Veto game	Equal	33
		One percent for veto[a]	24
	Control game	Equal	30
$\delta = .50$	Veto game	Equal	33
	Control game	Equal	30

[a] Non-veto players' recognition probabilities 49.5% for both players

[b] Two inexperienced subject sessions in each treatment

Table 4 Percentage of bargaining rounds that end in stage 1 and efficiency

		Efficiency[a]	Percentage of bargaining rounds that end in stage-one[b]		
			Veto proposer	Non-veto proposer	All proposers
High delay cost ($\delta = .50$)	Veto game	92.8% (1.84)	86.1% {31/36}	87.8% {65/74}	87.3% (1.16) [4]{96/110}
	Control game	93.6% (1.88)	89.0% (1.15) [4]{89/100}		
Low delay cost ($\delta = .95$)	Veto game	95.5% (0.69)	71.4% {20/28}	48.1% {39/81}	54.1% (1.95) [12]{59/109}
	Control game	98.1% (0.33)	72% (1.4) [4]{72/100}		

[a] Standard error of the mean in parentheses

[b] Average number of stages in parentheses. Maximum number of stages in brackets. Raw data in braces

4 Experimental results

In what follows we first cover results concerning efficiency, market power, and the extent to which MWCs are formed for our main experimental treatments—the $\delta = .5$ and .95 treatments with equal recognition probabilities. We then report the effects of the 1% recognition probability treatment on these outcomes. We conclude with an analysis of voting patterns.

4.1 Efficiency

Table 4 reports efficiencies and the percentage of bargaining rounds that end in stage one for both high (top panel) and low (bottom panel) delay cost cases. Efficiency is calculated as the mean percentage of the maximum amount of money ($30) distributed for accepted proposals, which serves to summarize the extent of delays along with their economic cost.

For both high delay and low delay cost cases efficiency is lower in the games with veto players than in the control treatment. Although these differences are not statistically significant for $\delta = .50$, they are for $\delta = .95$ ($p < 0.10$).[14] As the data reported in the remainder of Table 4 show, the primary source of these efficiency differences for the low delay cost 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 ($p < 0.01$) and 72.0% of the time for the controls ($p < 0.01$), resulting in a large overall difference in the frequency with which stage 1 proposals were accepted between the veto and non-veto treatments.[15]

Efficiency is lower in the $\delta = .50$ treatment than in 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, and significantly lower when there are low delay costs ($\delta = .95$). The efficiency differences with low delay costs reflect a substantially smaller probability of proposals being accepted in stage one for games with veto players, as well as a handful of bargaining rounds with a veto player that take four or more stages to reach completion.

4.2 Distribution of power

Table 5 shows the mean shares obtained by players as a function of who the proposer was for both high (top panel) and low (bottom panel) delay cost cases. 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). In the case of non-MWCs, for both the control and veto treatments (with a veto proposer) a partner's share consists of the *largest* share given to any other player.[16]

Looking at the results as a whole, there are a total of 66 possible pairwise comparisons that can be made between shares in Table 5.[17] Although virtually all of these pairwise comparisons fail to satisfy the quantitative predictions of the SSPE (and in a number of cases are off by quite a bit), the qualitative implications of the model are

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[14] All statistical results, unless stated otherwise, are two-tailed tests based on regressions with robust standard errors (clustering) at the session level.

[15] In those bargaining rounds that failed to reach closure in stage one, 17.9% of the time players in the low delay cost veto treatment wound up with a smaller share than would have been the case had they accepted their stage 1 offer (14.3% for veto players; 21.4% for non-veto players; 8.9% for the controls). This was *not* the result of disadvantageous counter-offers as there were very few of these (3.6% and 1.8% for veto and control treatments in stage two, respectively), where by "disadvantageous counter-offer" we mean players proposing less money for themselves in the next stage than they had rejected in the previous stage.

[16] As a result of non-MWCs the shares sum to less than one in all cases.

[17] Here we are comparing any pair, including, for example, a veto proposer with high delay costs with a control partner with low delay costs.

Table 5 Mean shares obtained by proposers and coalition partners (standard error of the mean in parentheses)

		Veto proposer		Non-veto proposer		Control treatment	
		Proposer	Partner[a]	Partner	Proposer	Proposer	Partner[a]
High delay cost ($\delta = .50$)	Obtained share	59.1% (2.4)	36.5% (2.2)	42.2% (1.0)	44.6% (1.7)	50.5% (1.0)	40.6% (1.0)
	Predicted share	*85.7%*	*14.3%*	*21.5%*	*78.6%*	*83.3%*	*16.7%*
Low delay cost ($\delta = .95$)	Obtained share	58.8% (1.6)	37.0% (1.0)	50.7% (1.0)	41.2% (1.6)	45.8% (1.1)	44.0% (0.7)
	Predicted share	*92.4%*	*7.6%*	*79.8%*	*20.2%*	*68.3%*	*31.7%*

[a] Largest share other than for the proposer

satisfied in all but four cases, and in none of these cases are the differences statistically significant at conventional levels.[18]

Table 6 summarizes results for the primary comparative static predictions of the model. Note in particular result 2 for the within treatment comparisons: veto players as coalition partners obtained *larger* shares than non-veto proposers earned in the low delay cost case and *smaller* shares than non-veto proposers earned in the high delay cost case. This rules out a naive argument that veto players earned larger shares strictly as a consequence of their holding veto power. Further, the fact that veto players as coalition partners in the low delay cost case obtained larger shares than non-veto proposers, although anticipated in the theory, goes against the emphasis in the literature on proposer power.

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. What is striking in the control data, as well as in previous legislative bargaining experiments, is that proposers fail to achieve anything like the large shares predicted in the theory.[19] Given this limited (actual) proposer power, our data show that veto power adds substantially to proposer power. In addition, veto power adds substantially more to proposer power than it adds to the share a player can expect as a coalition partner. The latter has important policy implications for controlling veto power, namely, limiting the veto player's proposal power by, for example, enacting committee rules that either exclude, or at least rotate, the chair's position.

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[18] The four cases where the differences have the incorrect sign relative to the predicted outcome are: (1, 2) veto proposers and non-veto partners with high delay costs versus their counterparts with low delay costs, (3) veto partners with low delay costs versus proposers with high delay costs in the control treatment, and (4) non-veto proposers with high delay costs versus proposers with low delay costs in the control treatment.

[19] The reasons behind this will be discussed in some detail in Sect. 4.4 below, where we review how players voted conditional on the shares allocated to them.

Table 6 Summary of share outcomes relative to the theoretical predictions

Within treatment comparisons:

Results consistent with the theory:

1. Veto players as proposers obtained larger average shares as coalition partners and larger shares than non-veto players as proposers for both high delay ($\delta = .50$) and low delay ($\delta = .95$) costs ($p < 0.01$ for all cases).
2. Veto players as coalition partners obtained *larger* shares than non-veto proposers with low delay costs and *smaller* shares than non-veto proposers with high delay costs ($p < 0.01$ in both cases).
3. Veto proposers earned larger shares than proposers did in the control treatment ($p < 0.1$ in both cases).
4. Shares for non-veto proposers were smaller than proposer shares in the control treatment ($p < 0.01$ in both cases).
5. Veto players as coalition partners obtained smaller shares than proposers did in the control treatment with high delay costs and larger shares than proposers in the control treatment with the low delay costs ($p < 0.01$ in both cases).

Results inconsistent with the theory: None

Between treatment comparisons:

Results consistent with the theory:

1. Proposers in the control treatment earned significantly larger shares with high delay costs compared to low delay costs ($p < 0.10$).
2. Non-veto proposers earned larger shares with high delay costs compared to low delay costs ($p < 0.05$).

Results inconsistent with the theory:

Veto proposers earned smaller shares with low delay costs compared to high delay costs ($p > 0.10$).

To measure the increase that veto power adds to proposer power we take the difference between the veto player's share as proposer and the proposer's share in the control treatment and divide it by the difference between the veto player's share as proposer and the partner's share in the control treatment. These calculations are reported in the next to last column of Table 7 along with the shares predicted under the SSPE. Veto power adds substantially more to proposer power than the theory predicts for both high and low delay cost cases: 46.5% versus 3.5% predicted for $\delta = .50$ and 87.8% versus 39.7% for $\delta = .95$. Thus, veto power and proposer power are strong complements, so that from a policy perspective (or a mechanism design perspective), so that 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.

The last two columns of Table 7 contrasts the increased shares veto players get as proposers to the increased share they get as coalition partners. To calculate this we take the difference between the veto player's share as proposer and her share as coalition partner divided by the difference between the veto player's share as proposer and the partner's share in the control treatment. For the low delay cost case this is substantially more than the theory predicts—over 50% achieved in practice versus just under the 21% predicted. For $\delta = .50$ the percentage share is essentially the same as the theory predicts.

Conclusion 2 The qualitative implications of the model regarding player shares are largely satisfied (62 out of 66 cases), although the point predictions typically fail. In particular, proposer power is not nearly as large as predicted in the control treatments.

Table 7 Veto player's power: predicted versus actual

		Veto power adds to proposer power[b]	Proposer power adds to veto power[c]
High delay cost	All coalitions	46.5%	91.4%
($\delta = .50$)	MWCs	41.3%	92.2%
	Predicted[a]	*3.5%*	*93.0%*
Low delay cost	All coalitions	87.8%	54.6%
($\delta = .95$)	MWCs	69.4%	68.6%
	Predicted[a]	*39.7 %*	*20.8%*

[a] For MWC's

[b] $[U(pr, v) - U(pr, c)]/[U(pr, v) - U(pa, c)] * 100$

[c] $[U(pr, v) - U(pa, v)]/[U(pr, v) - U(pa, c)] * 100$ where $U(pa, c)$ and $U(pr, c)$ are payoffs of coalition partner and proposer in the control treatment and $U(pr, v)$ and $U(pa, v)$ are payoffs of veto players as proposer and coalition partner

However, veto power adds substantially to actual proposer power. Further, veto power adds substantially more to proposer power than to one's share as a coalition partner, at least when there are equal recognition probabilities.

4.3 Extent of agreements on proposals

Table 8 shows the percentage of minimum winning coalitions (MWCs) for all proposals as well as all proposals that passed for both high (top panel) and low (bottom panel) delay costs. In both cases veto players are significantly more likely to propose and pass MWCs than non-veto players, with this tendency somewhat more pronounced with low delay costs ($p < 0.01$ for all cases). Further, veto players are significantly more likely to propose, but not to have passed, MWCs than in their respective control treatments.[20]

Figure 1 shows the frequency with which 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 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 high (low) costs. In contrast, there was much smaller growth in the frequency with which non-veto players

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[20] For all proposals it would be more appropriate to account for subject-specific effects as opposed to session-level effects as it is clear from data in games of this sort that there are subject-specific tendencies. To deal with this, we ran random-effect probits, treating subject as the random component, while ignoring any possible session-level effects as (i) these have been consistently shown to be nonexistent for this data set (standard errors are essentially the same or smaller when clustering at the session level in all of our regressions) and (ii) we are not aware of any statistical package that permits clustering at the session level and at the individual subject level nested within sessions. The results are robust for the high delay cost case whether we consider all periods or just the last five periods given the obvious learning in the data ($p < 0.10$ for two tailed tests in all cases). For the low delay cost case results start to approach statistical significance at conventional levels for the last five periods only ($p = 0.14$ for veto versus non-veto players; $p = 0.15$ for veto versus control players).

Table 8 Percentage of minimum winning coalitions (raw data is in parentheses)

		All proposals			Passed proposals		
		Veto proposer	Non-veto proposer	Overall	Veto proposer	Non-veto proposer	Overall
High delay cost	Veto game	79.7%	55.1%	63.3%	76.5%	59.2%	64.5%
($\delta = .50$)		(102/128)	(141/256)	(243/384)	(26/34)	(45/76)	(71/110)
	Control game		42.6%				43.0%
			(147/345)				(43/100)
Low delay cost	Veto game	78.4%	46.2%	60.0%	72.3%	48.4%	58.7%
($\delta = .95$)		(167/213)	(197/426)	(364/639)	(34/47)	(30/62)	(64/109)
	Control game		59.2%			61.0%	
			(245/414)			(61/100)	

proposed MWCs for the high delay cost case, and essentially no growth in their frequency for the low delay cost case. This, in conjunction with the significantly lower overall frequency of MWCs for non-veto 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 with high delay costs versus the absence of growth with low delay costs, as there are more likely to be multiple proposals for the $\delta = .95$ case, providing increased incentives for a non-veto player to propose large coalitions.

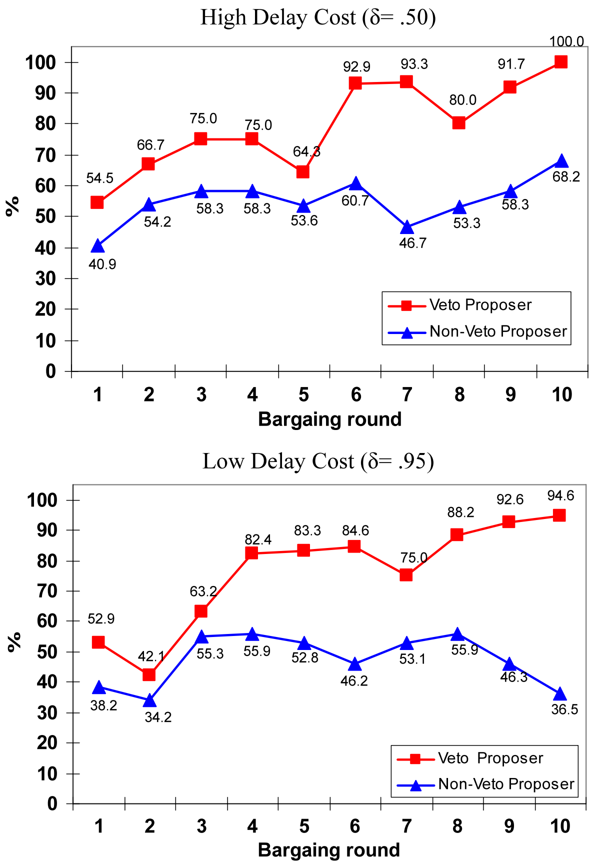
Conclusion 3 Veto players tend to propose and pass more minimal winning coalitions than non-veto proposers with both high and low delay costs. There are substantial increases over time in the frequency with which veto players propose MWCs, with much slower, or no growth, for non-veto players.

4.4 Veto games with one percent recognition probability

This section briefly reviews and compares efficiency levels and the frequency of MWCs in the 1% recognition case compared to the equal recognition probability case with $\delta = .95$. We then report the effect of the virtual elimination of proposer power for the veto player on her share of the pie as coalition partner, the issue of primary interest.

Efficiency averaged 97.8% (0.50) under the 1% recognition treatment compared to 95.5% (0.69) in the equal recognition case (standard errors of the mean in parentheses; $p < 0.10$). Bargaining rounds ended in stage one in 70.9% of all cases here compared to 48.1% of the time with non-veto players as proposers in the equal probability recognition case. So that much of the gain in efficiency in the 1% recognition

Fig. 1 Percentage of MWCs: all proposals



probability case resulted from a sharp decline in the frequency with which players exercised their veto power.

Table 9 compares shares obtained by non-veto proposers for proposals that passed in the 1% recognition rule case with the shares of their veto partners. Also reported, for comparative purposes, are average shares veto players obtained as coalition partners for the equal recognition rule cases. In the 1% recognition rule treatment, shares of veto players (as coalition partners) are slightly higher than shares of non-veto proposers: 45.8% (0.8) versus 43.7% (1.2) (with standard errors in parentheses). This is in striking contrast to the much smaller shares veto players are predicted to get in this treatment. However, consistent with the comparative static prediction of the model, the virtual elimination of the veto player's proposer power inherent in the 1% recognition rule treatment reduces the veto player's average share as a coalition partner compared to the equal recognition rule treatment from 50.7% (1.0) to 45.8% (0.8) ($p < 0.01$). These lower shares, in conjunction with the higher efficiency in the 1% recognition rule case, suggests that veto players were more accommodating than in the corresponding equal recognition rule treatment, and/or that non-veto players in the 1% recognition rule treatment were satisfied with getting approximately half the pie and chose not to push the issue. Finally, shares of veto players

Table 9 Mean shares: one-percent recognition rule versus equal recognition rule (standard errors of the mean in parentheses)

	One-percent recognition ($\delta = .95$)		Equal recognition: veto player's share as coalition partner	
	Veto player	Non-veto player	$\delta = .95$	$\delta = .50$
Actual	45.8% (0.8)	43.7% (1.2)	50.7% (1.0)	42.2% (1.0)
Predicted	9.1%	90.0%	79.8%	21.5%

as coalition partners in the 1% treatment are slightly higher than with equal recognition probabilities and high delay costs: 45.8% (0.8) versus 42.2% (1.0) ($p < .05$). Thus, halving the discount factor had a slightly greater impact on reducing the veto player's power as coalition partner compared to a drastic reduction in their proposer power.

The 1% recognition rule treatment might be likened to an infinite horizon bilateral bargaining game where one player, the veto player, never gets to make an offer. How can veto players maintain such high shares contrary to the theory's prediction in this case? Non-veto proposers know that if their proposal is rejected there is a 50% chance that they will not be the proposer in the next round, with a resulting drastic drop in their budget share (to zero in the case of a MWC). As a result they may opt for the safe strategy of giving a relatively large share of the pie to the veto player in the hope that their proposal will be accepted.[21] Of course, non-veto proposers face this same issue in the equal recognition probability treatment, plus the fact that the veto player could be chosen as the proposer and take an even larger share—hence the even larger share for veto players as coalition partners in the equal recognition case.

Proposer power has been reported in shrinking-pie bilateral bargaining games where it does not exist in theory (Ochs and Roth 1989), as well as in multilateral bargaining games (Fréchette et al. 2005c) where theory implies it should not exist. The lack of proposer power for non-veto players in the 1% probability recognition treatment, compared to the share they are predicted to get, provides a notable counterexample to these other cases, and a tribute to the veto player's power.

Conclusion 4 Reducing the ability of the veto player to make a proposal to a negligible level still results in veto players obtaining larger average shares than non-veto proposers in the low delay cost case, contrary to the theory's prediction. As a result veto power trumps proposer power with low delay costs even when the theory predicts the opposite result. However, the comparative static prediction of the model is satisfied as the reduction in the veto player's ability to make proposals reduces the share she receives as a coalition partner.

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[21] For accepted proposals, the average share of a non-veto player when they were not the proposer, conditional on shares being allocated to everyone in the 1% treatment, was 22.8%. Non-veto players proposed MWCs 61.7% of the time in the 1% treatment.

4.5 Voting patterns

We ran probit regressions for voting with own share as the explanatory variable, but with dummies accounting for differences in the likelihood of voting in favor of a given share as a function of a player's type—whether they were a veto or non-veto player or in the control treatment. We only looked at votes for potential coalition partners, not votes of proposers.

For the $\delta = .95$ case non-veto players are significantly more likely to vote in favor of a proposal for any given own share than veto players are ($p < 0.01$), with control players more likely to vote in favor of such proposals as well ($p < 0.05$). These results are robust to the more demanding random effect probits with subject as the random effect ($p < 0.05$ in both cases). Further, veto players in the $\delta = .50$ treatment are significantly more likely to vote in favor of a proposal for any given share than are veto players in the $\delta = .95$ treatment, with this difference even larger than the within treatment ($\delta = .95$) differences noted above.[22] For the $\delta = .50$ case, we are unable to reject a null hypothesis of any differences in the likelihood of voting in favor of proposals between veto and non-veto players and between veto players and the control treatment. However, for the random effect probits, the difference is positive (as predicted) but not significant ($p = .19$) for non-veto compared to veto players, with essentially no difference in the likelihood for control versus veto players.

Although qualitative predictions regarding how different types will vote in response to offers are largely consistent with the SSPE, as with other voting experiments the quantitative predictions concerning the offers that different player types are predicted to accept are clearly not satisfied: Both non-veto players and controls were unwilling to accept anything approaching the SSPE share as coalition partners, as were veto players in the high delay cost case. In contrast, veto players were willing to accept much smaller shares than predicted with low delay costs. Focusing on the high delay cost ($\delta = .95$) treatment: Non-veto players as coalition partners within a MWC should have accepted all shares of 7.6% or more. However, they never received an offer of 15% or less, and of the three offers of 20% or less that were voted on, only one passed. Thus, offers in the neighborhood of the SSPE were considered too small to be seriously considered by veto players, and stood little chance of being accepted. Similarly controls should have accepted all shares of 31.7% or more according to the SSPE. But within MWCs there were a total of eleven offers between 30 and 39% that were voted on, of which three were accepted. In contrast, with $\delta = .95$ veto players should have *rejected* all shares of 79.8% or less as coalition partners within a MWC. But this clearly did not happen, as (i) the maximum share offered within a MWC was 62.0%, and (ii) 63.3% of all offers in the interval 50–62% that were voted on were accepted.

The voting patterns of non-veto players and control players are quite similar to those reported in earlier experimental studies of the Baron–Ferejohn bargaining

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[22] However, the standard errors (of the random effect probits) are somewhat larger for veto players with $\delta = .50$ than for either veto, non-veto, or control players with $\delta = .95$. This indicates greater diversity in veto players responses for $\delta = .50$ than in the $\delta = .95$ treatment.

model: a minimum threshold for accepting offers that is typically well above the SSPE. This has generally been attributed to "fairness" or "equity" considerations, and is characteristic of the bilateral bargaining literature as well (see Roth 1995, for a review). However, veto players in the low delay cost case show the opposite pattern, a minimum threshold for accepting offers that is *less than* the SSPE. The closest counterpart to a veto player in earlier infinite horizon multilateral bargaining games is that of an Apex player who controls more votes than other players but cannot, unilaterally, block or pass a proposed allocation. Here too the "strong" player (the Apex player) tends to accept a relatively smaller share than predicted under the SSPE when invited in as a coalition partner (Fréchette et al. 2005c). We believe that similar equity and strategic considerations drive the strong players to accept smaller shares than their SSPE allocation in both cases as (i) they average more than 50% of the pie and (ii) as a proposer the strong player is unable to get potential coalition partners to accept anything approaching the small share predicted under the SSPE.

Shares offered to potential coalition partners exhibit a number of focal points in the data. For example, veto players often receive a share offer of 50% in both the high and low delay cost cases, and for the control treatments there is a focal point at the 33.3% split for both $\delta = .95$ and .50. Given these focal points and the role of 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 players bargaining strength. 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 the third player. In both cases we classify outcomes in terms of "approximately" equal splits.[23] There are relatively few cases of equal splits for all players reaching a maximum of 18.2% for non-veto players' proposals in the high delay cost case and a low of 0.9% for veto players on the same committees. We find similar differences as a function of players bargaining power for the $\frac{1}{2}$, $\frac{1}{2}$, 0 case with 29.8% of non-veto players in the low delay cost case proposing these splits (along with 33.3% of the controls) versus 4.6% of the veto players. Thus, in general, non-veto players were much more "equality-minded" than veto players, with the control players' sense of equity changing substantially with changes in the discount factor.

Conclusion 5 Voting patterns show that all player types were significantly more willing to vote in favor of any given share of the pie in the high compared to the low delay cost treatments. As the theory predicts, veto players required a substantially larger share than non-veto players, particularly in the high delay cost case, to vote in favor of a proposal. However, consistent with results from both the multilateral and bilateral bargaining literatures, the quantitative predicts of the SSPE are far from

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[23] 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 $\frac{1}{2}$, $\frac{1}{2}$, 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.

being satisfied, as players predicted to accept very small shares typically require substantially larger shares to vote in favor of a proposal. More interesting yet is the opposite pattern for veto players in the high delay cost cases who are predicted to only accept shares of 79.8% of the pie or more as they (i) never even get shares that are that large and (ii) vote in favor of shares of 50% or more over 60% of the time.

4.6 A methodological note

There is a methodological note worth adding here. Our experimental design involves randomly rematching subjects into voting groups, with five or more such groups within a given experimental session, with two sessions per treatment. One of the issues in a design such as this, in using all of the data for these voting groups, is whether session level effects severely compromise the statistical analysis. We have dealt with this by basing all of our statistical analysis on regressions with clustering at the session level. In the course of doing so we have looked at the standard errors of our estimates with and without clustering in order to determine the extent to which session-level effects impact the results reported, at least in terms of what the voting groups do. In all cases we observe that the standard errors of the estimates are essentially the same, or in some cases somewhat smaller, without clustering compared with clustering, indicating essentially no session-level effects. Of course *individual* behavior, such as how a given subject votes in response to a given share offered, shows strong serial correlation, which can (and was) controlled for by random-effect regressions with subject as the random factor. But issues like the average share veto players get as proposers or coalition partners for passed proposals, which are based on committee (group) actions, at least in this case exhibit essentially no session-level effects.

5 Summary and conclusions

Veto power has a substantial effect on the functioning of committees. First, it prolongs the process of decision making, especially with low delay costs when committees face no exogenous pressure to reach fast decisions or are able to meet frequently and easily make proposals. 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 than when non-veto players initiate proposals. Our experimental results expose these side effects that often raise concerns in real-life committees. In particular our results indicate that when a veto power is combined with a proposal power a veto player is granted excessive power that leaves the rest of the committee members rather weak.

In a paper that studies the effect of adding new members to a committee, Drouvelis et al. (2007) report results comparing a veto treatment with a control treatment within the context of the Baron–Ferejohn bargaining model. Their results are qualitatively consistent with ours, in spite of a wide range of procedural differences between the two experiments. In particular, they find that veto games take longer to reach agreement (i.e., are more inefficient) than the symmetric power control treatment, and that these differences tend to grow over time. Also similar to our low delay cost treatment, veto players get substantially larger shares than their coalition partners, but nowhere near the very large shares predicted.

Our results also have some interesting general implications for bargaining behavior. First, they confirm previous findings 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 example, with high cost (low cost) of delays non-veto players propose either 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 20.9% (12.9%) for veto proposers, and 21.0% (51.3%) for control players when they are proposers.[24] These differences as a function of a player's role in the bargaining game are quite dramatic, which suggests that agents fairness reference point shifts as a function of the underlying rules of the game in conjunction with their bargaining power (consistent with results others have reported; e.g., Hennig-Schmidt 2002). This feature is ignored in models of social preferences (Fehr and Schmidt 1999; Bolton and Ockenfels 2000), as well as some of the extensions that incorporate issues of reciprocity and efficiency (Charness and Rabin 2002; Dufwenberg and Kirchsteiger 2004). Indeed, general models that take such features into account are likely to turn out to be exceedingly complex. 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 are generated by the strategic forces that the theory suggests or other factors. Solving for the precise equilibrium of the game is no doubt well beyond our subjects' capabilities, nor are we suggesting that they do so. Nevertheless we believe that the fundamental strategic forces, and the basic intuition underlying the theory, are sufficiently transparent for subjects to recognize and to respond to. A key result supporting this is the effect of the change in the discount factor between high and low delay costs treatments. With low delay costs 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 they can with high delay costs, as it is much less expensive in terms of future expected income to reject such offers. This is the fundamental strategic force at play between the two treatments. It results in the equilibrium prediction that with low delay costs veto players will obtain larger shares as coalition partners than non-veto proposers, but will obtain smaller shares than non-veto proposers with high delay costs, results which are satisfied in the data.

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[24] Using the approximately equal split definitions developed earlier. Note that $\frac{1}{2}$, $\frac{1}{2}$, 0 splits by non-veto players almost always give the $\frac{1}{2}$ share to the veto player.

Appendix

Table 10 Mean shares obtained for MWCs only (standard error of the mean in parentheses)

		Veto proposer		Non-veto proposer		Control treatment	
		Proposer	Partner	Partner	Proposer	Proposer	Partner
High delay cost ($\delta = .50$)	Obtained share	62.7	37.3	46.1	50.5	55.3	44.7
		(2.5)	(2.5)	(1.1)	(2.3)	(1.1)	(1.1)
	Predicted share	*85.7%*	*14.3%*	*21.5%*	*78.6%*	*83.3%*	*16.7%*
Low delay cost ($\delta = .95$)	Obtained share	62.4	37.6	52.3	46.6	52.2	47.9
		(1.3)	(1.3)	(0.6)	(1.3)	(0.5)	(0.5)
	Predicted share	*92.4%*	*7.6%*	*79.8%*	*20.2%*	*68.3%*	*31.7%*

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