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Durable coalitions and communication: Public versus private negotiations☆

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ABSTRACT

We present a laboratory experiment to study the effect of communication on durable coalitions — coalitions that support the same allocation from one period to the next. We study a bargaining setting where the status quo policy is determined by the policy implemented in the previous period. Our main experimental treatment is the opportunity for subjects to negotiate with one another through unrestricted cheap-talk communication before a proposal is made and comes to a vote. We compare committees with no communication, committees where communication is public and messages are observed by all committee members, and committees where communication is private and any committee member can send a private message to any other committee member. We find that the opportunity to communicate has a significant impact on outcomes and coalitions. When communication is public, there are more universal coalitions and fewer majoritarian coalitions. With private communication, there are more majoritarian coalitions and fewer universal coalitions. With either type of communication coalitions occur more frequently and last longer than with no communication. The volume and content of communication is correlated with coalition type. These findings suggest a coordination role for communication that varies with the mode of communication.

1. Introduction

In this paper, we present a laboratory experiment to study durable coalitions in a dynamic legislative bargaining setting where the current status quo is the policy implemented in the previous period. An *endogenous status quo* is a feature of many policy domains—for instance, tax rates, regulations, and entitlements—where policies can be changed by the legislature but continue in effect in the absence of a new agreement. This makes the policy-making process an intrinsically dynamic game that cannot be studied as a static competition among different constituencies, or even as a sequence of independent competitions as in a repeated game. In choosing a policy proposal and coalition partners, a policy-maker must not only consider the direct effect of the agreement but also the indirect effect of the agreement on future policy decisions. This creates incentives for a coalition to continue from one period to the next. We refer to this as a *durable coalition*.

Recent theoretical research on dynamic divide-the-dollar bargaining (Epple and Riordan, 1987; Kalandrakis, 2004, 2010; Diermeier and Fong, 2011; Bowen and Zahran, 2012; Battaglini and Palfrey, 2012; Richter, 2014; Anesi and Seidmann, 2015; Baron and Bowen, 2015) has produced a rich assortment of predictions. Baron and Bowen (2015) identify a large class of Markov Perfect Equilibria (MPE), and Anesi and Seidmann (2015) show that almost all allocations can be supported as stationary MPE. Anesi and Duggan (2016) show that there is a continuum of equilibria and a form of indeterminacy in more general dynamic legislative bargaining theory, even with restrictions to Markovian, stationary and weakly undominated strategies. Reviewing this literature, Binmore and Eguia (2016), referring to Anesi and Seidmann (2015), note that this "powerful result leaves us with shattered predictive power. [...] it is not the case that in real world applications anything happens in totally unpredictable fashion (budgets could be burned but are not; and

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in most assemblies some subsets of members are known to more frequently cooperate and coalesce with each other) and we still wish to explain and predict these coalitional patterns and bargaining outcomes in real world applications. One might conjecture that we need to include other elements [...] into the choice set in order to obtain more realistic predictions."

The experiments in this paper include such "other elements" by focusing on an aspect of bargaining processes that has received little attention in models of dynamic bargaining, that of communication. Social interaction among individuals is an integral part of such processes, and it is difficult to find examples in which democratic decisions are made without people engaging in negotiations beforehand. Committee members are allowed to—and do—engage in sometimes intense communication over both proposal-making and voting. In the complete information, dynamic models studied to date there is no role for communication, and models with incomplete information are typically complex to study.[1] Communication can play a role in complete information models with multiple equilibria by coordinating the strategies of players.

In particular, we explore experimentally how free-form communication affects bargaining outcomes and the formation and durability of coalitions in a dynamic environment. Laboratory experiments provide a direct and powerful tool for investigating the effect of communication on dynamic bargaining processes. We study the behavior of laboratory committees in a simple dynamic bargaining game with an endogenous status quo. In each period of an infinite horizon, one of three committee members is randomly selected to make a proposal for the allocation of a divisible resource. The proposed allocation is implemented if it receives at least two affirmative votes (that is, a simple majority). Otherwise, the status quo policy prevails, and the resources are allocated according to that policy. The status quo policy, thus, evolves endogenously. Our main experimental manipulation is allowing players to negotiate with one another through unrestricted cheap-talk communication before a proposal is made and brought to a vote. We compare committees with no communication, committees where communication is public and all messages are observed by all committee members, and committees where communication is private and any committee member can send private messages to any other committee member.

Our main goal is to answer the following questions: Does communication affect the distribution of resources? More interestingly, does it increase the formation and duration of coalitions? How are resources allocated among the members of a durable coalition? To what extent do these answers depend on whether communication is public or private?

We find that the opportunity to communicate has a significant impact on outcomes and coalitions. In our experimental committees, the incidence of dictatorial outcomes—where one committee member gets the lion's share of resources—is negligible. In our no communication treatment, we find 27% of majoritarian outcomes—where two committee members share most of the resources—and 71% of universal outcomes—where everyone receives a significant share of the resources. Private communication results in more majoritarian outcomes (51%) relative to no communication and fewer universal outcomes (45%). In contrast, public communication results in fewer majoritarian outcomes (4%) relative to no communication and more universal outcomes (95%). We show that inequality (as measured by the Gini coefficient) is higher with private communication relative to the baseline, and lower with public communication compared to the baseline. Durable coalitions emerge more frequently and last longer when communication is allowed.

We analyze the content of the messages to help understand the association of particular words, such as those related to fairness or to forming a coalition, with coalition type. Private communication by proposers and public communication by non-proposers are correlated with universal allocations. Advocating for fairness increases the likelihood of a universal allocation, and advocating for minimal winning coalitions and one's own allocation decreases it. Words associated with punishment, deviation, or retaliation are infrequently used, are not correlated with more equal allocations with private communication, and are negatively correlated with more equal allocations with public communication, suggesting that punishment strategies are not a principal explanation for universal coalitions.

This paper is related to two strands of literature. First, this paper contributes to the literature on laboratory experiments evaluating models of legislative bargaining (McKelvey, 1991; Diermeier and Morton, 2005; Diermeier and Gailmard, 2006; Fréchette et al., 2003; Fréchette et al., 2005a,b,c, 2012; Fréchette, 2009; Baranski, 2016). These works focus on static environments where a given amount of resources is allocated only once.[2] The only exceptions are Battaglini and Palfrey (2012) and Nunnari (2016) who investigate experimentally dynamic models of committee bargaining with an endogenous status quo in the absence of communication. A subset of the experiments presented in Battaglini and Palfrey (2012) is analogous to our treatment without communication. They find a similar distribution of outcomes, but a lower incidence of universal allocations and a higher incidence of dictatorial and majoritarian outcomes than we find.[3] They do not study the effect of communication. The experiments in Nunnari (2016) are less comparable to ours, as the bargaining protocol includes players who can veto a proposal.

This paper also contributes to a growing experimental literature on the impact of unrestricted communication (Charness and Dufwenberg, 2006, 2011; Brandts and Cooper, 2007; Goeree and Yariv, 2011; Oprea et al., 2014). These studies show that communication facilitates greater coordination on Pareto superior outcomes. The policies in our experiment are all Pareto efficient, so communication can focus on coordination on coalitions and their allocations.

Three recent experiments have allowed subjects to communicate in a multilateral bargaining setting (Agranov and Tergiman, 2014a,b; Baranski and Kagel, 2015). [4] These papers study sequential (one-period) games that end once the resources are allocated rather than dynamic games with an endogenous status quo and thus cannot address coalition durability or the stability of allocations. They find that, when communication is allowed, outcomes in these sequential bargaining games are closer to the unique stationary subgame perfect equilibrium in which the proposer captures a disproportionate share of the resources. In contrast to sequential legislative bargaining, in our dynamic game the prediction is equal allocations for the coalition partners, and, in our experiment, relative to no communication proposers sacrifice current benefits to gain from greater coalition durability in the future and the current sacrifice is greater with public than private communication.

[1] The literature on sequential legislative bargaining where a committee is disbanded once it reaches a decision has considered communication. For instance, Austen-Smith (1990) and Chen and Eraslan (2014) study theoretically the effects of cheap talk in the presence of asymmetric information between committee members.

[2] Roth (1995) surveys the earlier experimental literature in bargaining. These experiments are less related as they are predominantly bilateral, static, and do not allow communication.

[3] There are many small differences between the two experimental designs (in the discount factor, the bargaining protocol, the sample size, and the subject pool) which may account for these differences in outcomes.

[4] The experiments in Murnighan and Roth (1977), Roth and Murnighan (1982), Rapoport and Kahan (1984), Rapoport et al. (1995), Valley et al. (2002), Croson et al. (2003), and Andreoni and Rao (2011) also explore the effect of communication on bargaining outcomes but are less related to ours, as they are bilateral and static.

The remainder of the paper is organized as follows. In Section 2, we describe the dynamic divide-the-dollar game subjects play in the laboratory. In Section 3, we identify the equilibria in the existing theoretical literature for the game with no communication and state testable hypotheses regarding the introduction of communication. In Section 4, we present the experimental design. Section 5 discusses the results of the experiment, and Section 6 concludes.

2. The dynamic divide-the-dollar game

We consider a committee of 3 players who bargain over how to divide a dollar among its members in each period of an infinite horizon.[5] In each period, the committee chooses an allocation $x^t = (x_1^t, x_2^t, x_3^t)$, where $x_i^t \in \mathbb{R}^+$ for any $i = \{1, 2, 3\}$, and $\sum_{i=1}^{3} x_i^t = 1$; that is, only efficient allocations that do not waste resources are allowed. Player i derives utility $u(x_i^t)$ from the allocation he receives in period t, where u is strictly increasing. Players are assumed to maximize the expectation of their discounted, infinite stream of utilities, where $\delta \in [0, 1)$ is the common discount factor.

The bargaining protocol with which an allocation x^t of the current period dollar is chosen is as follows. At the beginning of each period, a player is chosen at random to be the proposer and proposes an allocation y^t. The committee then votes between this allocation and the status quo. If a simple majority votes in favor of the proposal, it is accepted and $x^t = y^t$ is the implemented allocation in period t and the status quo for period $t + 1$. If the proposal is supported by less than a simple majority, it is rejected and the status quo allocation $x^t = x^{t-1}$ is implemented and remains the status quo for the following period. The initial status quo x^0 is exogenously selected at random.

3. Predictions from the theory

The theories developed for dynamic divide-the-dollar games characterize different classes of Markov Perfect Equilibria (MPE) with various assumptions about committee size, payoffs, discount factors, selection probabilities, rules for breaking indifference, and the space of possible agreements. An MPE is a subgame perfect equilibrium in which strategies depend only on the payoff-relevant history (Maskin and Tirole, 2001) , which in this game is the status quo policy. Committees are composed of three players with equal agenda setting powers and do not allow for waste — that is, the sum of allocations to the three players exhausts the dollar. Given these parameters, the theories that can inform behavior in our experiment are Kalandrakis (2004), Battaglini and Palfrey (2012), Anesi and Seidmann (2015), and Baron and Bowen (2015).[6]

In the MPE characterized by Kalandrakis (2004), outcomes quickly converge to a rotating dictatorship with an ergodic distribution where in each period the randomly selected proposer extracts all the resources. Panel (a) of Fig. 1 shows the transitions where a player receiving all the resources in the status quo proposes the status quo if (randomly) selected as the proposer, and the other players, if selected, propose taking all the resources for themselves. Along the convergence path to this distribution, coalitions are majoritarian and unstable, with the proposer giving a positive allocation only to one other player, either to the cheaper or to a randomly chosen one. This MPE exists for any degree of players' patience.

In the Markov Logit Quantal Response Equilibria (MLE) numerically computed by Battaglini and Palfrey (2012)[7] outcomes converge to a rotating dictatorship if players' utilities are linear. If players' utilities are strictly concave, players are averse to sequences of outcomes in which the status quo changes and the incentives for more symmetric allocations among players are stronger. Battaglini and Palfrey (2012) present numerical results for highly risk averse players: starting from a dictatorial allocation, the committee moves to a minimal-winning allocation where two players divide the dollar equally or, less frequently, to a universal allocation. These minimal-winning allocations are highly persistent but not absorbing, as the committees transition to a universal allocation around 20% of the time. Once a universal allocation is reached, it is essentially an absorbing state (the probability of leaving this state is approximately 2%). The risk aversion assumed by Battaglini and Palfrey (2012) to obtain convergence to a universal allocation is too extreme to be plausible in the experiment, so their relevant predictions are rotating dictatorships or rotating minimal winning coalitions.[8]

The equilibria in Anesi and Seidmann (2015) and Baron and Bowen (2015) are driven by what Diermeier et al. (2008) refer to as the *fear of exclusion*: a player accepts a proposal in the current period because of fear that if he rejects the proposal he may be excluded from the coalition formed in the next period. Anesi and Seidmann (2015) show that, as players become increasingly patient, almost any allocation—the exceptions being dictatorial and universal allocations—is possible with stationary MPE proposals, what they call *simple solutions*, that depend on the identity of players and include a punishment for each of the other players. The result that almost all outcomes can be supported requires that the values of the game differ among players and that allocations include waste. Baron and Bowen (2015) characterize stationary MPEs that support allocations with an equal opportunity property that implies that the values of the game are the same for all players. With three players, no waste and for a sufficiently high δ, these MPEs support minimal-winning coalitions with allocations of the form $(c, c, 1 - 2c)$ for all $c \in \left(\frac{1}{3}, \frac{1}{2}\right]$. Allocations of this form are also the only simple solutions in Anesi and Seidmann (2015) and in Anesi and Duggan (2016) with equal values of the game and efficient allocations, as in the experiment. The equilibrium outcomes in Anesi and Seidmann (2015) and Baron and Bowen (2015) are reached in one bargaining period and are persistent.[9]

In the experiment, we have 60 tokens to be allocated each period with $\delta = 0.8$, so a feasible allocation is a triplet of integers between 0 and 60 that sum to 60. With this discount factor the outcomes predicted in Anesi and Seidmann and Baron and Bowen

[5] We describe how we implement an infinite horizon game in the laboratory in Section 4.

[6] Epple and Riordan (1987) assume that players deterministically alternate in making proposals. Diermeier and Fong (2011) assume a persistent agenda setter. The MPE in Richter (2014) crucially depends on the possibility of waste. The MPEs in Kalandrakis (2010) and Bowen and Zahran (2012) exist only for, respectively, five or more and seven or more players. Therefore, the predictions from these five papers cannot inform behavior in our experiment.

[7] In Battaglini and Palfrey (2012) equilibria are computed as the limit of MLEs by gradually reducing noise in the players reaction functions. In the logit version of quantal response equilibrium, each player at each information set uses a behavioral strategy where the log probability of choosing each available action is proportional to its continuation payoff.

[8] They specify a utility function $u(x_i) = \frac{1}{1-\gamma} x_i^{1-\gamma}$ with $\gamma = 0.95$. The certainty equivalent for a 50-50 lottery with payoffs 0 and 3, the range of payoffs in our experiment, is 0.0000028.

[9] The equilibria in these theories depend importantly on how indifference in voting is broken. Kalandrakis (2004) assumes that a player votes for a proposal when indifferent between it and the status quo, and Battaglini and Palfrey (2012) assume that a player votes for the proposal with probability 0.5 when indifferent. Anesi and Seidmann (2015) assume that when indifferent the players vote for the status quo when it is in a simple solution, and for the proposal when the status quo is not in a simple solution. Baron and Bowen (2015) assume that players vote for the status quo when indifferent. The indifference rules in the latter two theories yield stability rather than rotation. For comparability the predictions here for the Baron and Bowen model are based on the indifference rule in Anesi and Seidmann (2015).

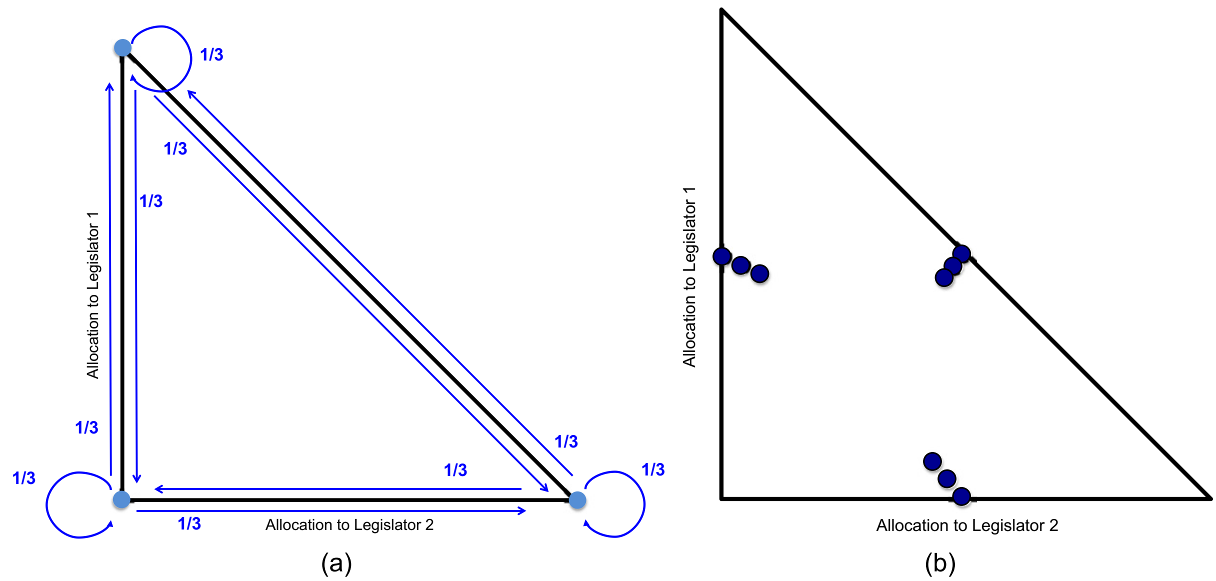

Fig. 1. Panel (a): Stationary distribution induced by MPE in Kalandrakis (2004) with $\delta = 0.8$ and Battaglini and Palfrey (2012) with $\gamma = 0, \delta = 0.8$. Panel (b): Absorbing outcomes according to different MPEs with $\delta = 0.8$ in Anesi and Seidmann (2015) and Baron and Bowen (2015).

are $(30, 30, 0)$, $(29, 29, 2)$, $(28, 28, 4)$ and their permutations.[10] Panel (b) of Fig. 1 shows these outcomes, with each point representing a possible equilibrium allocation. For example, the point on the horizontal boundary of the feasible set represents an allocation of 30 to each coalition member and 0 to the third player. The adjacent points represent allocations (29, 29, 2) and (28, 28, 4). Fig. 1, thus, shows the predicted outcomes for the theories relevant to our experimental setting.

Neither Anesi and Seidmann (2015) nor Baron and Bowen (2015) predict that the universal allocation $(20, 20, 20)$ is supported in an efficient MPE because there is no exclusion risk.[11] This allocation can be supported in a subgame perfect equilibrium using player-specific punishments in which a player who deviates from the universal allocation receives 0 thereafter and the other two players receive 30 each and hence have an incentive to punish. The universal allocation can also be supported in an MPE if players have sufficiently altruistic preferences.[12] Analysis of the communication among experimental subjects reveals the frequent use of words related to fairness, which is consistent with altruism or a norm of fairness being present among the subjects. The presence of universal allocations is not correlated with discussions of the history of play, including punishment, deviation, or retaliation, which is inconsistent with universal allocations supported by punishment strategies.

The dynamic bargaining games studied in the literature all assume complete information with no role for communication. In the theories there is a set of subgame perfect equilibria, including rotating dictatorships, universal, minimal winning, and surplus coalitions, and symmetric and asymmetric allocations among coalition members. Moreover, players can have differing utility functions, which allows for additional equilibria. This leaves the questions of which, if any, of these equilibria would be played in a setting in which players may not know the equilibrium strategies or which strategies the other players will use. The experiment provides evidence on what players do as a function of communication that could support coordination on particular equilibria. Our main experimental treatment is the ability of players to negotiate with one another through unrestricted cheap-talk communication. Our hypothesis is that communication serves as a coordination device and can make some outcomes focal. For example, the MPE in Anesi and Seidmann (2015) with equal values of the game and Baron and Bowen (2015) are particularly simple, are identity free, exhibit outcome and coalition stability, provide equal allocations to coalition members, and could be coordinated on through straightforward communication between the proposer of the coalition and potential coalition partners.

If subjects have altruistic preferences or follow a norm, such as a norm of fairness, the effect of communication on outcomes could depend on whether that communication is private or public. For example, suggesting a minimal winning coalition with public communication could be viewed as an expression of selfish preferences or a violation of a norm of fairness, resulting in rejection of the suggestion; when communication is private, only two subjects know of the suggestion and, hence, whether preferences might be selfish or a norm of fairness violated. This reasoning suggests a hypothesis that private communication facilitates coordination on minimal winning coalitions and public communication inhibits coordination on minimal winning coalitions. The frequency of minimal winning coalitions then should increase with private communication and decrease with public communication, relative to no communication. This is consistent with a hypothesis that public communication facilitates coordination on universal coalitions. This hypothesis is consistent with the findings of Diermeier et al. (2008) that in face-to-face, one-shot, protocol-free negotiations, "proto-coalition members appear to be embarrassed to 'shut the door' on the third party permanently,

[10] If the values of the game can differ among players, additional allocations can be supported in an MPE. A simple solution, SS, is a triple of policies, and equilibrium allocations to individual players can be as different as in $SS = \{(34, 26, 0), (8, 26, 26), (34, 0, 26)\}$, for which the values of the game are $\left(\frac{78}{3}, \frac{52}{3}, \frac{52}{32}\right)$ for the three players. There is little reason to believe that subjects in the experiment view themselves or others as advantaged or disadvantaged in the game, which is required if the values of the game implemented in the experiment are different. Note that in the simple solution above player 3 accepts proposals $(8, 26, 26)$ and $(34, 0, 26)$ but not proposals $(0, 34, 26)$ or $(26, 8, 26)$. There is also little reason to believe that a player views the other anonymous and randomly selected players as different.

[11] Richter (2014) shows that the universal allocation can be supported as an MPE if there is a threat created by the possibility of waste off the equilibrium path.

[12] If preferences of player i are $U_i(x) = x_i + \theta \sum_{j=1, j \neq i}^{n} x_j, \theta \geq 1$, the universal allocation is supported in an MPE, as shown in Baron (2017).

as this may be considered socially unacceptable behavior"(p. 498). Our experiment finds that this may also be the case in anonymous, computer-mediated, dynamic, structured bargaining.[13]

4. Experimental design

The experiments were conducted at the Columbia Experimental Laboratory for Social Sciences (CELSS) in March 2014 using students from Columbia University. Subjects were recruited from a database of volunteer subjects. Six sessions were run with a total of 120 subjects, and no subject participated in more than one session. All the interactions between subjects were performed through computers.[14] The 60 tokens available to each committee in each period correspond to US$3.

In all committees the discount factor was $\delta = 0.8$. Discounting was induced by a random termination rule: after each period of the same game, a random number between 0 and 100 was drawn by the computer with the outcome determining whether the game continued to another period (with probability δ) or was terminated (with probability $1 - \delta$). This is a standard technique used in the experimental literature to preserve the incentives of infinite horizon games in the laboratory (Roth and Murnighan, 1978; Palfrey and Rosenthal, 1994; Dal Bo, 2005; Duffy and Ochs, 2009).

We use a novel implementation of this methodology introduced by Fréchette and Yuksel (2013), the block random termination rule: subjects play as in the standard random termination rule but in blocks of four periods. Within a block subjects receive no feedback about whether the game has continued to that period, and they make choices that will be payoff-relevant contingent on the game actually having reached that point. After each block of four periods, subjects are told whether the game ended within that block and, if so, in what period; otherwise, they are told that the game has not ended yet, and they start a new block of four periods. Subjects are paid for periods only up to the end of their game, and all decisions for subsequent periods within that block are void with respect to payment. As shown by Fréchette and Yuksel (2013), this alternative implementation of an infinitely repeated game results in the same theoretical properties and in similar laboratory behavior as the standard random termination rule. This implementation is appealing for studying the formation and duration of coalitions, because it allows us to observe subjects' behavior for a greater number of periods. In the empirical analysis, we use all available data, including data from periods that, *ex post*, were not used in determining payments to subjects. The methodology ensures that every period played (including periods that, ex post, are not used to determine payments) have the same incentives.

Sessions were conducted with a minimum of 15 subjects and a maximum of 24 subjects, divided into committees of 3 members each. Committees stayed the same throughout the periods of a given game, and subjects were randomly rematched into committees between games. Each game corresponded to one play of the dynamic game, using the block termination rule.[15]

Our main experimental manipulation is the opportunity for subjects to negotiate with one another through unrestricted cheap-talk communication before a proposal is made and comes to a vote. We compare committees with no communication, committees where communication is public and messages are observed by all committee members, and committees where communication is private and any committee member can send private messages to any other committee member. We conduct two sessions where communication is not allowed, two where only public communication is allowed, and two where only private communication between two committee members is allowed.

At the beginning of each game, subjects are randomly divided into committees of three members each. In each committee, subjects are assigned to be Committee Member 1, Committee Member 2, or Committee Member 3. This member assignment remains the same for all periods of a game. For each committee, an initial status quo is randomly chosen by the computer, using a uniform distribution on the set of feasible allocations. The drawing of an initial status quo is independent across games and across committees.[16] At the beginning of each period, one of the three members is randomly selected to be the proposer, and his committee member number is revealed to the entire committee. When communication is not allowed, the proposer proposes an allocation that is observed by all members of the committee with the shares to each member clearly indicated. Then, all members of the committee simultaneously vote to accept or reject the proposed allocation. If the allocation is supported by a simple majority of members, it passes, determines the distribution of the 60 tokens in that period, and becomes the new status quo allocation for the next period. If the allocation is rejected, the shares in this period are determined by the status quo, which becomes the status quo for the next period. After each period, subjects observe how each member in the committee voted and how this determines committee members' payoffs for this period and the status quo allocation for the following period. After each game, subjects are randomly rematched to form new committees and assigned new committee member numbers. Each subject plays four games.

In the Baseline treatment, no communication was allowed. The Public Communication and Private Communication treatments are similar to the Baseline treatment except for one feature. After the proposer is selected and his committee member number revealed but before the proposer submits his proposal, members of the committee can communicate with each other using a chat tool. In the Private Communication treatment, subjects can send private messages that are delivered only to a particular member. When a committee member sends a message to another, the third committee member does not know the content of this communication nor the fact that a message was sent.[17] In the Public Communication treatment, subjects were only allowed to send messages that were received by all the other members of their committee. The duration of the communication was in the hands of the proposer, and the chat tool was disabled when the proposer submitted his proposal for a vote or after 120 seconds had passed. The software recorded all the messages sent by subjects during the communication stage. Table 1 summarizes the details of all the treatments.

5. Experiment results

In this section, we first compare the allocation of resources among committee members for the three treatments. Second, we identify durable coalitions and study whether their emergence and duration are affected by communication. Third, we present an analysis of

[13] Beyond these differences in the game and in the treatment implementation, the participants to the experiments presented by Diermeier et al. (2008) were confronted with a hypothetical-choice situation: they "were not paid and did not receive special course credit contingent on negotiation success" (p. 490).

[14] Sample instructions are provided in Online Appendix E. The computer program used in the experiment was an extension to the open source software Multistage.

[15] The length of the games ranged from 4 to 24 periods in blocks of 4.

[16] The initial status quo is given exogenously to preserve symmetry of the bargaining protocol with the other periods of the game. Note that the theories discussed in the previous section do not depend on the initial status quo. Moreover, in Online Appendix C, we show that the exogenous status quo at the beginning of the game does not anchor outcomes beyond the first period, in particular, it does not affect the committee allocation in the second period of the same game.

[17] In the private communication treatment, a committee member could send the same message to each other committee member, but one other member would not know the other had received the message.

Table 1
Experimental design.

Session	Treatment	n	δ	Games	Subjects	Committees
1	Public communication	3	0.8	4	21	28
2	Public communication	3	0.8	4	21	28
3	Private communication	3	0.8	4	21	28
4	Private communication	3	0.8	4	24	32
5	Baseline (no communication)	3	0.8	4	18	24
6	Baseline (no communication)	3	0.8	4	15	20

the determinants of voting. Finally, we explore the content of conversations to shed light on the mechanism behind the differences in allocations and coalitions. To compare outcomes and behavior between different treatments, we use random effects panel regressions with standard errors clustered at the session level. Clustering at the session level accounts for potential interdependencies between observations that come from random re-matching of subjects between matches in a session.[18] To allow for learning in the initial repetition of our infinite horizon divide-the-dollar game, we present and analyze the data from games 2 through 4 in each session. Unless stated explicitly, our statements about statistical significance are robust to considering the whole sample (that is, including data from the first game).

5.1. Distribution of resources

We begin the analysis of the experimental results by examining the bargaining outcomes, that is, the allocation of resources (60 tokens) at the end of a period. We define as "Dictatorial" those allocations that give at least 50 tokens (that is, 83% of the 60 tokens) to a single committee member and define as "Universal" allocations that give at least 10 tokens (that is, 17% of the 60 tokens) to every member of the committee. All other allocations give a significant fraction of the tokens to exactly two committee members (that is, to a minimal winning coalition), which we label as "Majoritarian."[19] In the latter two categories, we highlight allocations that give coalition members an even number of tokens. For the Universal allocations this correspond to the allocation (20, 20, 20); for the Majoritarian allocations this includes all allocations of the form $(b, b, 60-2b), b \in \{26, \ldots, 30\}$. Table 2 presents the distribution of allocation types for the three treatments.

Finding 1. *Private communication makes Majoritarian allocations—in particular, those with even sharing between two committee members—more likely and Universal allocations less likely. Public communication makes Majoritarian allocations less likely, and Universal allocations—in particular, those with even sharing among all committee members—more likely.*

Table 2 shows that the frequency of Majoritarian allocations is 27% when communication is not allowed, 51% when only private communication is allowed, and 4% when public communication is allowed. The frequency of Universal allocations is 71% with no communication, 45% with private communication, and 95% with public communication.[20] These differences are statistically significant.[21,22] The difference in the incidence of Majoritarian allocations between the treatment with no communication and the treatment with private communication is due to the difference in Majoritarian allocations with equal sharing between two committee members, which triples (from 11% to 34%). Strikingly, subjects who communicate publicly divide resources evenly (that is, 20 tokens each) 90% of the time.

Private communication increases the frequency of allocations with even distribution among the members of a Majoritarian coalition, which is consistent with the prediction of Baron and Bowen (2015) and supportive of the even distribution allocations in Anesi and Seidmann (2015). While Dictatorial allocations are more frequent when subjects can communicate privately, the findings for all treatments are inconsistent with a rotating dictator equilibrium as in Kalandrakis (2004) and Battaglini and Palfrey (2012) (for a linear utility function), where allocations belong to the Dictatorial region in almost every period.

The results from Table 2 suggest that the opportunity to communicate supports coordination on coalitions in which members share the resources differently from the baseline treatment where no communication is allowed.

Finding 2. *The degree of inequality of allocations (both in a period and in a game) is significantly higher in Private Communication and significantly lower in Public Communication, when compared to the Baseline.*

For each committee and each period, we compute a Gini coefficient as $G = \frac{1}{3}\left(4 - 2\frac{\sum(4-i)x_{it}}{60}\right)$, where x_{it} is the allocation to committee member i in period t, and committee members are indexed in non-decreasing order of allocation. This Gini coefficient ranges from 0 — when the allocation is (20, 20, 20) — to 0.67 — when the allocation is (60, 0, 0). The average Gini coefficients are 0.14 for Baseline, 0.22 for Private Communication and 0.02 for Public Communication. The differences between treatments are statistically significant. Similarly, we compute Gini coefficients for the distribution of earnings in a full infinite horizon bargaining game, summing the resources allocated to each committee member in all periods of a game. The average Gini coefficients for these cumulative allocations are 0.07 for Baseline, 0.16 for Private Communication and 0.02

[18] See Fréchette (2012) for a discussion.

[19] These are the definitions used by Battaglini and Palfrey (2012).

[20] In Online Appendix A we show that our results are robust to a different definition of Majoritarian and Universal allocations: at least 15 tokens (that is, 25% of the tokens) to every member as Universal; at least 20 tokens (that is, 33% of the tokens) to two members and less than 15 to the other as Majoritarian. The distribution is similar: pooling together all treatments, Majoritarian and Universal allocations are, respectively, 32% and 65% with the original definition; 39% and 55% with this alternative definition.

[21] When using the whole sample, the fractions of Majoritarian (pooling together Even and Uneven) and Universal (pooling together Even and Uneven) allocations in Private Communication are different from the Baseline at the 10% level (p-values 0.077 and 0.099, respectively). All other comparisons are unchanged.

[22] In Table 2, an observation is a committee in a period. Given the random termination rule, different committees interact for different numbers of periods. As a robustness check, we use as a unit of analysis a committee in a match and compare the fraction of outcomes belonging to each allocation type. We cannot use panel regressions because we have a single observation for each committee. The p-values of OLS regressions clustering standard errors by session are consistent with the results presented in Table 2: the fraction of Majoritarian (pooling together Even and Uneven) and Universal (pooling together Even and Uneven) allocations in Private Communication are different from the Baseline at the 10% level (p-values 0.056 and 0.077, respectively); the fraction of Universal Uneven allocations in Private Communication is indistinguishable from the Baseline; all other comparisons are unchanged.

Table 2
Frequency of allocation types by treatment. †: significant difference between treatment and baseline ($p < 0.05$) according to a regression where the independent variable is a treatment dummy. We use random effects panel regressions clustering standard errors by session. An observation is a committee in a period.

Allocation type	Baseline	Private	Public
Dictatorial	1%	5%	1%
Majoritarian	27%	51%†	4%†
Even	11%	34%†	2%†
Uneven	16%	17%	2%†
Universal	71%	45%†	95%†
Even	49%	31%	90%†
Uneven	22%	14%†	5%†
Observations	328	472	252

for Public Communication. The differences between treatments are statistically significant.[23]

What does communication mean for the allocation to the proposer relative to a non-proposer, or the *proposer's advantage*? To properly assess the proposer's advantage in this infinite horizon game where a committee bargains over the division of multiple dollars, it is useful to define two related concepts: let the *static proposer's advantage* be the additional amount allocated to the current proposer in the current period (with respect to current non-proposers); and let the *dynamic proposer's advantage* be the additional amount allocated to the current proposer in the following period (with respect to current non-proposers).

Finding 3. *Compared to the Baseline, the static proposer's advantage is lower in Public Communication and undistinguishable in Private Communication; the dynamic proposer's advantage is higher in both Public and Private Communication.*

Table 3 presents random effects panel regressions. In the first two columns, the dependent variable is the allocation to a committee member at the end of a period. This allocation depends on the role a committee member has in that period, with proposers receiving more than non-proposers. Interestingly, this static proposer's advantage is unchanged with private communication and sharply dampened by public communication. In the third and fourth columns, the dependent variable is the allocation to a committee member at the end of the following period. In the Baseline treatment, this amount is negatively correlated with being the proposer in the current period. Introducing either private or public communication significantly reduces this dynamic proposer's disadvantage, increasing the amount allocated in the following period to the current proposer compared to the Baseline. The interpretation of these findings in the context of the theory is that proposers take more in the current period t but moderate the increase with communication. The moderation is to attract and retain the support of coalition partners in future periods, resulting in significant gains in period $t+1$.

We can compare these results with the experiments on the static bargaining game à la Baron and Ferejohn (1989) studied by Baranski and Kagel (2015). In these experiments, a committee divides a single dollar and the introduction of private communication increases the share of this dollar allocated to the current proposer, that is, the static proposer's advantage. The authors argue that this is due to the reduction in uncertainty and to the competition among potential coalition partners for inclusion in the coalition.[24]

In contrast, in our experiments a committee divides multiple dollars and private communication does not increase the preferential treatment to the proposer in the current period but it increases the chance the proposer will remain a member of the future ruling coalition (that is, that he will be allocated a significant fraction of resources in the following period). This is consistent with a different mechanism that might be at play in the dynamic game studied here. In the dynamic game, a proposer has an incentive to propose an allocation that the coalition partner(s) will support in future periods, which requires the proposer to allocate evenly among coalition members. In the static game, there are no future periods, and the proposer can capture 44 tokens, allocating 16 to one other subject.

In both the static legislative bargaining game and our dynamic game, the introduction of communication results in observed allocations being closer to the allocations predicted by the corresponding theory. In the Baron and Ferejohn (1989) game, there is a unique stationary SPE, and the question is whether play is according to that equilibrium. In our dynamic legislative bargaining game, multiple equilibria are supported by stationary MPEs. Among these are equilibria in which proposers can sacrifice proposal power for the durability of their coalition, as predicted by Anesi and Seidmann (2015) and Baron and Bowen (2015). In the next section, we explore the extent to which coalitions form, their duration, and how they are affected by the two modes of communication. This analysis may help refine the theory for dynamic legislative bargaining games, and, specifically, help to understand which coalitions are focal with communication.

5.2. Coalition formation and duration

In Table 2, we classified outcomes using categories based on the allocation and not by which subjects received the tokens. For example, both $(30, 30, 0)$ and $(0, 30, 30)$ were classified as majoritarian allocations, but a majoritarian coalition is not present unless the same subjects receive the 30 tokens in two consecutive periods. We define a *durable coalition* as a committee that continues from one period to the next with the same allocation.[25] To investigate the emergence and persistence of durable coalitions, we study the evolution of allocations over time. Since the status quo in the first period of a game is exogenous, we exclude the first period from the analysis in this subsection (that is, we only include periods whose status quo is endogenous).[26]

Finding 4. *Private communication increases the persistence of Majoritarian allocations. Public communication decreases the persistence of Majoritarian allocations and increases the persistence of Universal allocations.*

For each of the three treatments, Table 4 presents the probability that an allocation persists from one period to the next, i.e., a durable

[23] Results are robust to alternative measures of inequality, for example, $\sqrt{\sum\left(\frac{x_i}{60}-\frac{1}{3}\right)^2}$.

[24] Baranski and Kagel (2015) use two communication treatments: the "closed door bargaining" treatment is analogous to our Private Communication treatment; in the "open door bargaining" treatment—as well as in the communication treatments of Agranov and Tergiman (2014a,b)—subjects can send a message to any subset of members in their committee (that is, they can send messages both privately and publicly). These papers do not have communication treatments analogous to our Public Communication treatment. The experiments in Baranski and Kagel (2015) are with committees composed of three members, while the experiments in Agranov and Tergiman (2014a,b) are with committees composed of five members.

[25] In Online Appendix B, we show results are stronger and robust to a less stringent definition of durable coalitions.

[26] The results presented in this subsection are unchanged if we include the first period in the analysis and if we allow a durable coalition to start in the first period of a game.

Table 3
Random effects panel regressions. Standard errors clustered by session in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. An observation is a committee member in a period. Other regressors: treatment dummies.

Dependent variable:	Allocation to i in t		Allocation to i in $t+1$	
Allocation to i in status quo	0.44***	0.09	0.48***	0.09
	(0.11)	(0.09)	(0.13)	(0.12)
i Proposer in t	6.77***	6.59***	−4.20***	−1.72***
	(0.32)	(0.40)	(1.17)	(0.16)
i Proposer in $t*$ Private	−1.28		3.34**	
	(1.97)		(1.57)	
i Proposer in $t*$ Public		−5.83***		1.38**
		(0.63)		(0.56)
Constant	9.02***	15.94***	11.80***	18.75***
	(2.21)	(1.76)	(2.37)	(2.50)
Treatments	Base & private	Base & public	Base & private	Base & public
Observations	2400	1740	2166	1515
R^2	0.2671	0.1292	0.2204	0.0116

coalition is present. A Majoritarian allocation persists 18% of the time without communication, 58% with private communication and 0% with public communication. The difference between each communication treatment and the baseline is statistically significant. The same Universal allocation persists 67% of the time without communication, 69% with private communication and 93% with public communication. The difference between public communication and the baseline is statistically significant.[27,28] Private communication, thus, is associated with coordinating on Majoritarian allocations and sustaining a two-subjects coalition over time. Public communication is associated with coordinating on Universal allocations and sustaining a three-subjects coalition over time. The latter finding suggests that subjects are less willing to participate in coalitions that disadvantage one subject when communication is public and exposed to all subjects than when communication is private. This is consistent with the hypothesis that private communication facilitates coordination on Majoritarian coalitions and public communication facilitates coordination on Universal coalitions when a norm of fairness is present among subjects. This interpretation is supported by the subjects' use of words corresponding to fairness as discussed in Section 5.3.

Finally, we investigate how the duration of a coalition is affected by the opportunity to communicate. Duration is defined as the number of periods that a coalition continues with the same allocation, so a coalition that begins in period 2 and ends after period 4 has a duration of 2. In the baseline treatment there are 44 coalitions in 33 committees; in the private communication treatment there are 78 coalitions in 45 committees; and with public communication there are 41 coalitions in 42 committees. Note that in each of the treatments there can be more than one coalition formed in a single committee. To investigate duration, we focus on Majoritarian and Universal coalitions.[29] In the 78 committees from the baseline and private communication treatments, there are 56 Majoritarian coalitions and 66 Universal coalitions. In the 75 committees from the baseline and public communication treatments, there are 72 Universal coalitions. Since with Public Communication there are no durable Majoritarian coalitions, we do not estimate the effect of this treatment on the duration of Majoritarian coalitions.

Finding 5. *Private communication increases the duration of Majoritarian coalitions; public communication increases the duration of Universal coalitions.*

With the block random termination design a coalition exogenously ends when the block during which the game ends is terminated. Since the length of a game is stochastic, it is important to control for the number of periods in a game when assessing whether the duration of coalitions is statistically different across treatments. To do so, we run an OLS regression on pairs of treatments where the dependent variable is the number of periods a durable coalition persists. The independent variables are the treatment (public communication or private communication) and the length of a game. The results in Table 5 indicate that introducing private communication increases the duration of a Majoritarian coalition by about 2 periods (with respect to the baseline with no communication at the average game length).[30] Introducing private communication does not affect the duration of a Universal coalition, but introducing public communication increases the duration of Universal coalitions by about 3 periods (with respect to the baseline with no communication at the average game length).[31]

5.3. Voting behavior

Table 6 presents the results from Probit regressions where the dependent variable is the likelihood of supporting the proposal. An observation is a single committee member's vote on a single proposal. We cluster standard errors by session to take into account possible correlations among decisions taken by individuals participating in the same experimental session. The proposer's vote is excluded. The data are presented separately for different treatments (Baseline, Private Communication or Public Communication). The independent variables are Premium to Oneself, that is, the amount proposed to the voter minus the amount to the same voter in the status quo; Premium to Proposer, that is, the amount proposed to the proposer minus the amount to the proposer in the status quo;

[27] When using the whole sample, the persistence of Majoritarian allocations in Public Communication is indistinguishable from the Baseline. The other comparisons are unchanged.

[28] The probability that a Majoritarian allocation persists from one period to the next does not depend on whether the proposer is a member of the Majoritarian coalition in any treatment. To test for this hypothesis, we run two random effects panel regressions (one for Baseline versus Private Communication and one for Baseline versus Public Communication) where the dependent variable is the probability an allocation persists and the independent variables are a treatment dummy, a dummy for the proposer being a member of the Majoritarian coalition, and an interaction term. The coefficients of these two regressors are indistinguishable from zero.

[29] We exclude persistent dictatorial allocations, which are approximately 1% of the data. A dictatorial allocation can persist with probability one-third, because when the dictator is selected as the proposer he can propose the status quo and the vote is then immaterial.

[30] The average game lengths (including the first period of the games) are 9 periods, 11 periods, and 8 periods for Baseline, Private Communication, and Public Communication, respectively.

[31] There are no durable Majoritarian coalitions with public communication. This can also be seen from Table 4: there are 9 Majoritarian status quo allocations and none of them persists. When using the whole sample, public communication does not affect the duration of durable coalitions.

Table 4
Persistence of allocation by treatment and allocation type. Number of observations for each allocation type and treatment in parentheses. †: significant difference between treatment and baseline ($p < 0.05$) according to a regression where the independent variable is a treatment dummy. We use random effects panel regressions clustering standard errors by session. The results are unchanged if we use a Probit regression with standard errors clustered at the session level. An observation is a committee in a period.

	Pr [Allocation$_{(t)}$ = Allocation$_{(t-1)}$]		
Allocation type$_{(t-1)}$	Baseline	Private	Public
Dictatorial	0.33 (3)	0.38† (21)	0.00† (1)
Majoritarian	0.17 (83)	0.58† (218)	0.00† (7)
Universal	0.68 (209)	0.69 (188)	0.93† (202)
Observations	295	427	210

Table 5
OLS regressions. Standard errors clustered by committees are in parentheses. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. An observation is a coalition that lasts for at least one period.

	Dependent variable: coalition duration		
Private communication	1.94***	−1.16	
	(0.45)	(1.02)	
Public communication			2.91***
			(0.68)
Game length	−0.03	0.22*	0.65***
	(0.06)	(0.13)	(0.14)
Constant	1.43*	2.32*	−2.31**
	(0.74)	(1.38)	(1.13)
Treatments	Base & private	Base & private	Base & public
Coalition type	Majoritarian	Universal	Universal
Observations	56	66	72
R^2	0.0908	0.0983	0.3822

and the Inequality Difference, that is, the Gini coefficient of the proposed allocation minus the Gini coefficient of the status quo allocation. In all treatments, the probability of supporting the proposal is increasing in the amount allocated to oneself and decreasing in the inequality of the proposal. In the Baseline and Private Communication treatments, it is also decreasing in the amount allocated to the proposer.

5.4. Analysis of conversations

The analysis presented above shows marked differences in allocations and coalitions between the baseline treatment with no communication and the treatments with private or public communication. To shed light on the mechanism underlying these differences, we analyze the messages exchanged in the communication treatments.[32] We answer the following questions: How does private communication differ from public communication? What aspects of communication (volume, participants, direction, content) matter for the allocation of resources and the persistence of a durable coalition?

Subjects took advantage of the opportunity to communicate with other committee members. Communication preceded most proposals, especially at the beginning of a game: the fraction of periods with some conversation is 62%, and 92% of committees communicated in the first period of a game. Moreover, 99% of committees have conversations in at least one period of the game. Table 7 gives the breakdown of communication by treatment and presents the average number of messages (overall as well as by sender and by recipient) for each treatment. There is more communication with private than with public communication (at least one message sent in 66% versus 55% of periods) and this difference is statistically significant.

[32] Chat logs were analyzed by three research assistants, who were unaware of the research questions.

Table 6
Probit regressions. Standard errors clustered by session in parentheses. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. An observation is a committee member in a period.

Dependent variable: Pr {vote for proposal}			
Premium to oneself	0.14***	0.04***	0.05***
	(0.01)	(0.01)	(0.01)
Premium to proposer	−0.01***	−0.01	−0.03**
	(0.01)	(0.01)	(0.01)
Gini proposal − Gini status quo	−5.81***	−2.54***	−1.97***
	(0.59)	(0.02)	(0.01)
Constant	0.84***	1.04***	0.66***
	(0.17)	(0.39)	(0.11)
Treatment	Baseline	Public	Private
Observations	656	504	944
R^2	0.440	0.121	0.250

The average number of messages exchanged in a period is higher with private than with public communication (4.22 versus 3.36 in all periods; 6.00 vs. 3.86 in the first period of a game). These differences, however, are not statistically significant.

Table 7 includes two other interesting results: a proposer does not send a different number of messages than a non-proposer in either treatment.[33] Also, when communication is private, non-proposers are more likely to contact the proposer than the other non-proposer.[34] The natural interpretation is that non-proposers believe that communicating with the member who has the power to set the agenda is more fruitful in terms of steering the decision of the committee towards what they prefer. This is consistent with the experimental literature on unrestricted communication in bilateral bargaining, which has highlighted the positive effect of asking for more resources (Andreoni and Rao, 2011). We next investigate whether this is also true in our dynamic multilateral bargaining setting.

Finding 6. *With private communication, the volume of messages between two committee members is positively correlated with the resources they obtain and the equality of their relative allocation. With public communication, the volume of messages is positively correlated with the equality of the allocation.*

Table 8 reports the results of random effects panel regressions. Columns 1 and 2 investigate the relationship between communication volume and outcomes in Private Communication. In the first column, the dependent variable is the total allocation to a pair of committee members at the end of a period and the independent variables are the allocation to this pair in the status quo at the beginning of that period and the number of messages exchanged by the pair in the period. Thus, there are 3 observations for each committee in each period. In the second column, the dependent variable is the Gini coefficient of the distribution of resources within each pair of committee members at the end of a period and the independent variables are the Gini coefficient of the distribution of resources within each pair in the status quo at the beginning of that period and the number of messages exchanged by the pair in the period. The number of messages exchanged between any two committee

[33] There are two non-proposers and only one proposer in each period and in each committee, and Table 7 reports the average number of messages sent by the two non-proposing members in a period. The average number of messages sent by a single non-proposing member in a period is 1.35 with private communication and 1.08 with public communication. These averages are not statistically distinguishable from the average number of messages sent by a proposer in the respective treatments.

[34] With private communication, the difference (0.64) between the average number of messages sent by a non-proposer to a proposer and the average number of messages sent by a non-proposer to a non-proposer is statistically significant at the 1% confidence level.

Table 7
Use of communication.'Any message sent' refers to the fraction of committees where at least one message was sent; 'Mean number of messages' refers to the average number of messages sent by the members of a committee in a period. †: significant difference with public communication ($p < 0.05$) according to a regression where the independent variable is a treatment dummy. For comparisons on All Periods data, we use random effects panel regressions clustering standard errors by session. For comparisons on Period 1 data, we use OLS regressions clustering standard errors by session.

	Private comm.	Public comm.
Any message sent (All Periods)	66%†	55%
Any message sent (Period 1)	91%	93%
Mean number of messages (All Periods)	4.22	3.36
From proposers	1.51	1.20
From non-proposers	2.70	2.15
Non-proposer → Proposer	1.67	–
Non-proposer → Non-proposer	1.03	–
Mean number of messages (Period 1)	6.00	3.86

members is associated with a greater appropriation by those same committee members (at the expense of the third committee member) and with a lower degree of relative inequality between the communicators. These findings are consistent with the hypothesis that when one member of the pair is receiving a low allocation, e.g., the out member when there is a Majoritarian coalition, that member argues for forming a new coalition to replace the present one. Then, if successful, the allocation to the pair increases and the inequality decreases.

For column 3 with Public Communication, where messages cannot be targeted, the dependent variable is the Gini coefficient of the allocation implemented at the end of a period and the independent variables are the Gini coefficient of the status quo allocation at the beginning of a period and the number of public messages broadcasted. In all three columns the dependent variable is positively correlated with the status quo, reflecting the stability of policies and the durability of coalitions.

The more a committee communicates in a period the less unequal is the distribution of resources among members of the committee at the end of that period. As noted above, exchanging messages might increase the resources to the communicating pair because subjects use private messages with the goal of forming or maintaining a coalition, which would decrease the inequality between the allocations of the communicators. Alternatively, subjects could use private or public messages to advocate for fairness or for a

Table 8
Random effects panel regressions. Standard errors clustered by session are in parentheses. One observation in columns (1) and (2) is a pair of two committee member in a committee in a period. One observation in column (3) is a committee in a period. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	(1)	(2)	(3)
Dependent variable:	Allocation to $\{i, j\}$	Inequality in $\{i, j\}$	Overall inequality
Status quo allocation to $\{i, j\}$	0.53^{***}		
	(0.08)		
Status quo inequality in $\{i, j\}$		0.71^{***}	
		(0.02)	
Messages between i and j	0.79^{***}	-0.01^{***}	
	(0.08)	(0.01)	
Status quo overall inequality			0.18^{**}
			(0.08)
Messages in committee			-0.01^{***}
			(0.01)
Constant	17.56^{***}	0.10^{***}	0.03^{**}
	(3.37)	(0.02)	(0.01)
Treatment	Private	Private	Public
Observations	1416	1409	231
R^2	0.3399	0.5140	0.0808

Table 9
Average number of messages belonging to semantic domain. One observation is a committee in a period. ALL is for all members; P is for proposers; NP is for non-proposers. †: significant difference from public communication ($p < 0.05$) according to a regression where the independent variable is a treatment dummy. We use random effects panel regressions clustering standard errors by session.

	Private communication			Public communication		
	ALL	P	NP	ALL	P	NP
Lobby oneself	0.10†	0.03	0.07†	0.03	0.02	0.02
Lobby fairness	0.25	0.12	0.13	0.21	0.05	0.15
Form coalition	0.55†	0.21†	0.34†	0.07	0.02	0.05
History	0.11	0.04	0.07	0.06	0.02	0.04
Suggest 20-20-20	0.25	0.10	0.15	0.17	0.03	0.14
Suggest 30-30-0	0.19†	0.09	0.10†	0.02	0.01	0.01
Suggest other allocation	0.14†	0.07†	0.07	0.01	0.00	0.01

universal allocation. In what follows, we explore these possibilities by investigating the content of messages.

In analyzing the content of communication, we search for expressions or keywords and assign messages containing them to one or more semantic domain. First, we identify all messages that include a numerical suggestion on how to divide the 60 tokens among the three committee members. In particular, we classify messages that contain '30-30' as 'Suggest 30-30-0'; messages that contain '20-20-20' as 'Suggest 20-20-20'; and messages that contain any other triplet of numbers summing to 60 as 'Suggest other allocation '.[35] Second, we identify the messages that contain at least one word or expression about fairness ('Lobby for fairness '), about obtaining resources for oneself ('Lobby for oneself '), about forming a coalition ('Form a coalition ') and about the history of play ('History').[36] Table 9 shows the frequency of messages related to each of these semantic domains by treatment and by sender.[37]

Finding 7. *The content of messages differs across treatments.*

Private messages are more likely than public messages to be about lobbying for oneself, forming a coalition, or proposing an allocation of resources other than 20-20-20. Messages advocating

35 In identifying messages that belong to the domains 'Suggest 30-30-0' and 'Suggest 20-20-20', we consider all possible variations of these numbers in a message, as '20-20-20', '20/20/20', '202020', '20 20 20', and so on.

36 We classify a word or expression as belonging to a semantic domain if at least two out of the three research assistants who analyzed the chat logs assign that word or expression to the same domain. Examples of words or expressions that lead a message to be classified as 'Lobby for fairness' are 'equal', 'fair', 'give him'. Examples of words or expressions that lead a message to be classified as 'Lobby for oneself' are 'give me', 'help me', 'I want'. Examples of words or expressions that lead a message to be classified as 'Form a coalition' are 'alliance', 'trust', 'team', 'loyal', and 'deal'. Examples of words or expressions that lead a message to be classified as 'History' are 'betrayal', 'deviation', 'punish', 'cheated', and 'lied'. Some messages were classified as belonging to multiple semantic domains, for example: 'any chance you could make it a little more fair? help me out!'. The messages that do not belong to any of these semantic domains have mostly to do with timing, wanting the game to end, discussing the rules of the experiment, and interpreting the incentives. The complete list of words and expressions for each of these semantic domains is provided in Online Appendix D. We searched both algorithmically and manually. The full transcripts of the communication and the classifications are available from the authors upon request.

37 When using the whole sample, the difference between private and public communication in Suggest 30-30-0 (0.16 versus 0.02) is not statistically significant. The other comparisons are unchanged. Comparing conversations in Game 1 with conversations in Games 2–4 for the same treatment, Lobby for oneself, Form a coalition, History, and Suggest other are less frequent in later games with public communication; Lobby for fairness and Suggest 20-20-20 are more frequent in later games with public communication; Form a coalition is more frequent in later games with private communication at the 10% level (p-value 0.051).

Table 10
Penalized maximum likelihood regressions. NP stands for messages sent by non-proposers; P stands for messages sent by proposers. *p < 0.10, **p < 0.05, ***p < 0.01. An observations is a committee in a period. Observations do not include periods with Dictatorial allocations.

	Pr {universal allocation}			
Any message	−0.72***	−0.82***	−0.37	−1.12
	(0.20)	(0.30)	(0.24)	(0.68)
NP: Lobby for oneself	−0.13	−1.79**	0.20	−1.76
	(0.37)	(0.79)	(0.44)	(1.10)
NP: Lobby for fairness	0.29	1.97**	0.15	−0.01
	(0.31)	(0.84)	(0.37)	(0.97)
NP: Form a coalition	−0.12	−0.15	−0.39	0.61
	(0.25)	(0.69)	(0.29)	(1.81)
NP: History	−0.06	−1.58**	0.27	−1.75
	(0.35)	(0.64)	(0.44)	(1.09)
NP: Suggest 20-20-20	0.83***	1.07	0.97**	1.47
	(0.31)	(0.67)	(0.38)	(1.47)
NP: Suggest 30-30-0	−0.49	−-0.63	−0.77*	−0.77
	(0.38)	(1.20)	(0.44)	(1.57)
P: Lobby for oneself	0.70	1.94	0.16	0.11
	(0.59)	(1.65)	(0.67)	(1.83)
P: Lobby for fairness	0.97**	0.76	1.03**	0.76
	(0.39)	(0.77)	(0.41)	(1.72)
P: Form a coalition	−0.60**	0.19	−0.39	0.21
	(0.29)	(1.03)	(0.32)	(1.52)
P: History	−0.51	0.30	0.15	−0.69
	(0.55)	(1.09)	(0.64)	(1.55)
P: Suggest 20-20-20	1.71***	0.44	1.78***	−0.73
	(0.43)	(0.89)	(0.49)	(1.48)
P: Suggest 30-30-0	−1.32***	−0.15	−1.17**	−0.46
	(0.43)	(1.62)	(0.46)	(2.16)
Constant	0.49***	1.98***	0.04	3.45***
	(0.14)	(0.23)	(0.16)	(0.54)
Treatment	Private	Public	Private	Public
Subjects	All	All	Experienced	Experienced
Observations	638	433	450	251
Pseudo R^2	0.1084	0.0854	0.1047	0.0335

fairness and proposing an even allocation of resources have similar frequencies in both communication treatments.

Finally, we investigate whether we can relate the content of communication to actual allocations and coalitions. Table 10 shows how the probability of observing Universal allocations is correlated with the use of the communication channel and the presence of messages belonging to each semantic domain in a period. The corresponding table in Online Appendix A is consistent with Table 10, indicating that the findings have a degree of robustness to the categorization of allocations. Our experimental design does not allow us to exogenously manipulate the amount and content of conversations. For this reason, this table does not identify whether the messages help in attaining a particular allocation or whether the messages are used to explain an action taken on other grounds.

Finding 8. *With private communication, universal allocations are less likely when proposers suggest 30-30-0 or to form a coalition and more likely when proposers lobby for fairness or subjects suggest 20-20-20. With public communication, universal allocations are more (less) likely when non-proposers lobby for fairness (lobby for themselves or discuss history).*

Table 10 shows penalized maximum likelihood estimates for the probability a committee chooses a Universal, rather than Majoritarian, allocation in a period.[38] The variable 'Any messages ' is a dummy variable that takes value 1 if at least one message is exchanged in a period. It is included in the regressions to distinguish between periods in which no message was sent and periods with messages. The other independent variables are dummies which take value 1 if at least one message in each of the semantic domains is sent by one non-proposer (the first set of variables) or by the proposer (the second set of variables).

With private communication, the estimates for the whole sample (in column 1) and for experienced subjects (in column 3) are quite similar: messages advocating fairness sent by the proposer and messages suggesting a 20-20-20 allocation sent by any committee member are positively correlated with universal allocations, whereas messages suggesting a 30-30-0 allocation sent by the proposer (and by non-proposers, in the whole sample) are negatively correlated with universal allocations. The latter is consistent with the messages supporting Majoritarian allocations. With private communication and experienced subjects messages aimed at forming a coalition sent by the proposer are negatively correlated with the presence of a Universal allocation, which is consistent with the word 'coalition' and the semantic domain interpreted as pertaining to a coalition of size two.

For public communication, the estimates for the whole sample (in column 2) and for experienced subjects (in column 4) are different. With experienced subjects (that is, in games 2–4), the content of public communication is uncorrelated with the probability of observing a Universal allocation, whereas, for the entire sample, the coefficient of Lobby for fairness sent by non-proposers and is positive and statistically significant and the coefficient of Lobby for oneself sent by non-proposers is negative and statistically significant. This is consistent with our interpretation of the results from the previous sections: subjects learn to coordinate on and maintain Universal coalitions in the first game and, thereafter, do not need substantive communication, as if following a norm, perhaps established in the first game. Words related to history, on which player-specific punishments could be based, are uncorrelated or negatively correlated with the presence of a Universal allocation, which is consistent with a norm based on altruism, rather than the threat of punishment.

In Table 10, talking about past behavior (History) in private communication is not significantly correlated with universal allocations, and it is *negatively* correlated with the formation of universal coalitions in public communication. As detailed in Online Appendix D, we included in the 'History' domain keywords and expressions that can be related to history-dependent strategies: 'betray', 'deviate', 'retaliate', 'cheat', 'lie', 'revenge', 'threat', 'promise', and 'punish' (and words sharing the same root). The findings in Tables 10 are inconsistent with the emergence and duration of Universal allocations or coalitions based on the threat of punishment using history-dependent strategies. These findings are consistent with universal outcomes being supported by a norm of fairness. However, the extent to which the effect of these terms is causal cannot be gauged by our experimental design because the content of messages is endogenously chosen by the subjects.

With private communication, the prediction is minimal winning coalitions and, hence, proposer messages are required to make universal allocations more likely. With public communication, non-proposers advocate fairness and avoid lobbying for themselves to increase the likelihood of a universal allocation in which they benefit. One interpretation of this is that non-proposers are selfish and say so with public communication. With private communication, proposers who are altruistic or follow a norm of fairness must be the ones that advocate for universal allocations.

6. Conclusions

In this paper, we present a laboratory experiment to study durable coalitions in a dynamic legislative bargaining setting. Our

[38] Observations in Table 10 do not include periods with Dictatorial allocations. We use penalized maximum likelihood (Firth, 1993) rather than logistic estimates to deal with the small sample bias arising from the reduced number of periods (less than 5% of the total) with non-universal allocations when communication is public and subjects are experienced.

experimental treatment is the opportunity of subjects to communicate with one another. The three treatments are: committees that cannot communicate, committees that can engage in private conversations before a proposal is made, and committees that can engage in public conversations before a proposal is made. Communication is viewed as a mechanism for coordinating on particular allocations and coalitions.

With regard to the research questions posed in the Introduction, we find that the opportunity to communicate has a significant impact on the formation of coalitions and, consequently, on how resources are allocated. When communication is possible, durable coalitions emerge more frequently and last longer. Compared to the treatment with no communication, private communication increases coordination on majoritarian coalitions and decreases coordination on universal coalitions, whereas public communication has the opposite effect.

To shed light on the mechanism underlying these differences, we analyze the messages exchanged in the communication treatments. Subjects who can only communicate privately engage in more lobbying for themselves and are more likely to discuss the formation of a coalition or to suggest a non-universal allocation of the resources than do subjects who can only communicate publicly. Messages related to fairness and suggesting an equal division are positively correlated with Universal allocations in both communication treatments but, with private communication, it is due to the proposers' messages and, with public communication, it is due to non-proposers' messages. Messages associated with history-dependent strategies do not increase the likelihood of universal allocations.

The experimental evidence suggests a role for social interaction and communication in theories of dynamic legislative bargaining and points theorists to a fruitful direction for future research. This role may be in affecting a norm of fairness, in reducing strategic uncertainty or the fear of exclusion from a less-than-universal coalition, and in coordinating players' strategies to attain particular types of equilibria. Our results suggest that not only the existence of a communication channel but also the mode of communication matters in coordinating on equilibria. Coordination could be reinforced by experience. With private communication, a higher frequency of majoritarian coalitions results as subjects become more experienced, and with public communication, universal coalitions become more frequent.

Because of the absence of theories encompassing communication, this experiment is not a test of the existing theories for a dynamic divide-the-dollar game with communication. Nonetheless, existing theories for the dynamic game with no communication help us understand the incentives present in the experiment. The gap in that understanding pertains to universal coalitions. The modal allocation in the no-communication and public communication treatments is universal, and most of the universal allocations have even sharing among the coalition members. Three explanations are consistent with theory.[39] The first is extreme risk aversion as in the Battaglini and Palfrey (2012) model, but the required risk aversion is so extreme as to be unbelievable in the experimental setting. The second is player-specific punishments that deter individual subjects from deviating from the equilibrium path to take short-term gains. This takes fairly sophisticated strategies and implicit threats of punishment. As discussed in Section 5.3, messages related to such punishment strategies are uncorrelated with the presence and duration of universal coalitions The third possible explanation is preferences that are not selfish but instead exhibit a degree of altruism that could generate a norm of fairness that supports universal allocations. This explanation is consistent with the overall experimental findings, but experiments designed to identify the foundations of and measure the strength of a norm of fairness are needed before concluding that such a norm explains bargaining outcomes. As Cooper and Kagel (2016) note in their survey of the empirical finding in sequential (static) legislative bargaining, "more is going on in games of this sort than we currently understand."

[39] The universal allocation is also not explained, for example, by the von Neuman-Morgenstern solution concept in cooperative game theory, which selects minimal winning coalitions with allocations of the form 30-30-0.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2017.09.002.

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