

When Parity Promotes Peace: Resolving Conflict Between Asymmetric Agents

Erik O. Kimbrough ^a
Roman M. Sheremeta ^b
Timothy W. Shields ^b

^a Department of Economics, Simon Fraser University,
8888 University Drive, Burnaby, BC V5A 1S6, Canada

^b Argyros School of Business and Economics, Chapman University,
One University Drive, Orange, CA 92866, USA

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Abstract

Due to the high costs of conflict both in theory and practice, we examine and experimentally test the conditions under which conflict between asymmetric agents can be resolved. We model conflict as a two-agent rent-seeking contest for an indivisible prize. Before conflict arises, both agents may agree to allocate the prize by fair coin flip to avoid the costs of conflict. In equilibrium, risk-neutral agents with relatively symmetric conflict capabilities agree to resolve the conflict by randomization. However, with sufficiently asymmetric capabilities, conflicts are unavoidable because the stronger agent prefers to fight. The results of the experiment confirm that the availability of the random device partially eliminates conflicts when agents are relatively symmetric; however, the device also reduces conflict between substantially asymmetric agents.

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Corresponding author: Roman M. Sheremeta, sheremet@chapman.edu

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1. Introduction

"Justice originates among those who are approximately equally powerful (...) where there is no clearly recognizable predominance and a fight would mean inconclusive mutual damage (...)"

(Friedrich Nietzsche, *Human, All Too Human*, Section 92)

In the opening quote, Nietzsche contends that peace and justice are most easily negotiated when the costs of conflict are highest, or when opponents are evenly matched. In this paper, we develop a model formalizing Nietzsche's intuition, and we test the predictions of the model in a laboratory experiment in which two individuals bargain to avoid conflict over a valuable resource. We model conflict as a variation of the classic Tullock (1980) rent-seeking contest between two agents. In our setup, two agents face the prospect of conflict over an indivisible prize. Before conflict arises both participants may agree to settle the dispute via a simple conflict resolution mechanism (a fair coin flip) and thereby avoid the costs of the contest. We assume that individuals can credibly commit to the outcome of the random device,¹ and we show analytically that if agents are relatively symmetric, then conflicts can be avoided through appeals to the random device (consistent with Nietzsche's conjecture). However, when agents are substantially asymmetric then conflicts are unavoidable because the stronger agent has no incentive to consent to randomization.

Equipped with theoretical predictions, we conduct a laboratory experiment to examine the conditions under which human participants avoid conflict. In the experiment, we alter both the availability of a conflict resolution mechanism (random device versus no random device) and the relative strength of agents (weak asymmetry versus strong asymmetry). The results of the

¹ When faced with the threat of conflict, individuals seeking a peaceful resolution usually face two problems: (1) a coordination problem, in which agents must assent to mediation, and (2) a commitment problem, in which individuals must agree to bind themselves to the mediated outcome. Here, we limit our attention to problem (1) and assume that there is no commitment issues. For interested readers, the issue of commitment is addressed in studies by Kimbrough and Sheremeta (2012) and Kimbrough et al. (2013).

experiment indicate that participants' behavior is largely consistent with the predictions: the introduction of a random device reduces conflict, and the reduction is significantly greater (though not complete) when participants are more symmetric. However, although the costs of conflict are not always avoided when the theory predicts that they will be, they are nevertheless frequently avoided when theory predicts that they will not.

We chose to use a random device (a fair coin flip) as a conflict resolution mechanism for a number of reasons. First, using a coin flip is transparent and easy to understand for participants. Second, it provides an ex-ante unbiased allocation of an indivisible resource (consistent with egalitarian norms). Finally, there are many historical as well as modern examples suggesting that such conflict resolution mechanisms are common in practice.

Historically conflict resolution mechanisms using a random device took many forms, including divining rods, the casting of lots, and the inspired interpretation of cracks in fire-heated bones (Bernstein, 1996; Bowden, 2005).² All of these mechanisms were used to settle private disputes, determine guilt or innocence, and make decisions related to war and peace, but perhaps the most famous example of conflict resolution via random device is the Delphic Oracle (Bowden, 2005; Iannaccone et al., 2011).

In archaic Greece, representatives from powerful city-states would travel to remote Delphi to consult the Oracle housed at a temple dedicated to the god Apollo. The Oracle, a priestess believed to be possessed by Apollo, provided advice to visitors about controversial political matters by providing a randomized response to questions that typically could be

² Another well-known historical example of conflict resolution by what might be considered a random device is the battlefield agreement to settle a conflict by single combat between two renowned warriors. Such combat is attested in myth (e.g., combat described in the Iliad between Menelaus and Paris and between Achilles and Hector) and numerous historical wars (e.g., the Battle of Clastidium 222 BCE, the Battle of Badr 624 CE, the Battle of the Trench 627 CE, to name a few). Similarly, Leeson (2010) describes the medieval mechanism of "trial by battle" through which uncertain property rights were frequently assigned.

answered with a “yes” or “no”, and owing partly to the Oracle’s divine connection, the city-states would abide by its counsel. The Oracle served its purpose successfully for many years, but its success was tied to the relative balance of power among those who sought its counsel.

Consider a (hypothetical) dispute between Athens and Sparta over a piece of territory, where the alternative to abiding by the Oracle’s proposed allocation is armed conflict. In this context, appealing to the Oracle offers a surplus-preserving solution to a multi-player contest. With evenly matched opponents, the potential cost of conflict may overwhelm the gains to deviation from the Oracle’s decision. Hence, the small and autonomous city-states have a strong interest in supporting a randomizing Oracle. However, if Sparta, for example, is sufficiently stronger than Athens, then Sparta may no longer benefit from using a random device. If the gains to conflict are sufficient, then random decisions by the Oracle can only handcuff Sparta’s ambition. Indeed, as Greece became a unified empire it no longer had need of the Delphic Oracle for political purposes. As such by the third century BCE the Oracle was used mainly for religious and personal inquiries (Bowden, 2005).

While the most striking examples are found in history, more familiar modern examples abound. Rock-paper-scissors, drawing straws, and throwing dice, are all regularly used to settle friendly disputes. Recently, a judge in Florida required lawyers on opposing sides of a case to settle a disagreement by rock-paper-scissors. Similarly, the auction houses of Sotheby’s and Christie’s played the game to determine who would receive a contract to sell \$17.8 million worth of art.³ In some political jurisdictions, a coin flip decides the outcome of elections in which two candidates receive equal numbers of votes, and in other jurisdictions coin flips are employed to determine the recipient of a government contract when two companies tender equal offers

³ See the following news article from the AP: http://www.msnbc.msn.com/id/13221673/ns/us_news-weird_news/

(Lissau, 2011).⁴ Regardless of the mechanism, willingness to submit to the outcome of a random device can be viewed as a strategic choice to avoid potentially costly conflict.

2. Background

A rich literature on conflict resolution in economics extends back to Schelling's (1960) *Strategy of Conflict* in which he applied the tools of game theory to identify the necessity of credible commitment to avoiding conflict. Since Schelling, economists and others have modeled conflict extensively, emphasizing the resources wasted when conflict occurs (Konrad, 2009). One of the best-known conflict models is the rent-seeking contest, first developed by Tullock (1980) and elaborated by Skaperdas (1996), in which agents expend resources to increase their probabilities of winning a valuable prize. An agent's probability of winning equals his expenditure divided by the sum of all contestants' expenditures, so an agent who expends more resources has a better chance of receiving the prize. In equilibrium, all agents make positive expenditures; however, from the point of view of society, all such expenditures are wasteful. Moreover, laboratory experiments testing this model indicate that in practice the costs of conflict are even higher than predicted by the theory (Dechenaux et al., 2012; Sheremeta, 2013).⁵

Given the high costs of conflict both in theory and practice, a number of mechanisms for eliminating conflict have been proposed, ranging from deterrence via extensive armament

⁴ Third-party arbitration can also be viewed as a conflict resolution mechanism employing a random device (Ashenfelter et al. 1992; Burgess et al., 1996). There is a substantial experimental literature on arbitration. Deck and Farmer (2007, 2009), Dickinson (2004, 2005), Famer and Pecorino (2004), and Pecorino and Van Boening (2001, 2004, 2010) experimentally examine various arbitration mechanisms and their impact on the probability and costs of conflict. However, in all these studies the arbitration process is not random and depends on the arbitrator's notion of fairness. Moreover, the outcome of arbitration also depends on endogenously chosen offers of conflicting parties. In contrast, the outcome of a random draw does not depend on the preferences of the third party and contestants cannot influence the outcome.

⁵ Sheremeta (2013) reviews 30 conflict experiments and finds that in 28 of those experiments individuals exert significantly higher conflict expenditures than predicted. In some cases the expenditures are so high that individuals earn negative expected payoffs (Sheremeta, 2010; Deck and Sheremeta, 2012).

(Schelling, 1960) and hostage exchange (Williamson, 1985) to contractually binding side-payments (Charness et al., 2007; Schoonbeek, 2009; Kimbrough and Sheremeta, 2013). The common property of all such conflict resolution mechanisms is that they seek to alter payoffs in a way that eliminates the incentive for one party to unilaterally initiate conflict. In general, *any* mechanism that provides all potential combatants with an expected payoff at least as large as their expected payoff from conflict is preferable to outright conflict. Hence, in a dispute over an indivisible resource, random assignment, or assignment by means of an “uncorrelated asymmetry”, that is, some asymmetry between players that is uncorrelated with their probabilities of winning a conflict (e.g., property assignment via first possession) can be supported as a Nash equilibrium (e.g., Maynard-Smith, 1982).

Here we explore the decision to employ a random device to avoid costly conflict in a Tullock contest. Prior to the conflict, both parties can choose whether to use the random device. If both parties elect to use the random device, then the contest prize is awarded randomly and no conflict ensues. Otherwise, if one or both of the parties elect *not* to use the random device, then the two parties engage in a Tullock contest to assign the prize.

One critical assumption underlying both our model and our experiment is that there is no commitment problem. Specifically, we assume that once both parties elect to appeal to the random device, they are bound by its outcome. In our model the random device addresses the strategic aspect of conflict resolution. Its purpose is to allocate the prize to one party without costly conflict expenditures. Restricting our attention only to the strategic aspects of this conflict resolution mechanism allows us to examine causal effects of power differences on the likelihood of conflict resolution (the crucial role of which is noted by Nietzsche in our opening quote).

3. The Conflict Resolution Model

We model a game of potential conflict between two risk-neutral players for a prize valued at v by both players. The game takes place in two sequential stages, the conflict resolution stage and the conflict stage. In the conflict resolution stage, players 1 and 2 simultaneously decide whether to resolve the conflict by flipping a fair coin *or* to enter the conflict stage.⁶ If both players agree to the random device, then the game ends with neither player advancing to the conflict stage. The prize is allocated to each player with probability $p_1^{Random} = p_2^{Random} = 0.5$, and players 1 and 2 receive the expected payoffs of $E(\pi_1^{Random}) = E(\pi_2^{Random}) = v/2$. However, if either player refuses to use the random device, then both players advance to the conflict stage.

In the conflict stage, both players make irreversible effort expenditures e_1 and e_2 to increase their probabilities of receiving the prize. Players have different conflict capabilities (strengths) a_i , so that the stronger player 1 ($a_1 > a_2$) can expend the same effort, yet have a higher chance of winning the prize. Specifically, player i 's probability of winning is defined by the following contest success function (Skaperdas, 1996):

$$p_i(e_1, e_2) = a_i e_i / (a_1 e_1 + a_2 e_2). \quad (1)$$

The expected payoff in a conflict for player i is equal to the probability of player i winning, $p_i(e_1, e_2)$, times the prize valuation, v , minus contest expenditure, e_i :

$$E(\pi_i^{Conflict}) = p_i(e_1, e_2)v - e_i. \quad (2)$$

By differentiating (2) and solving the best response functions simultaneously, we obtain unique Nash equilibrium effort expenditure levels for players 1 and 2:

$$e_1^* = va_1 a_2 / (a_1 + a_2)^2 \text{ and } e_2^* = va_1 a_2 / (a_1 + a_2)^2. \quad (3)$$

Given the equilibrium effort expenditures (3), the probabilities of winning the contest by players 1 and 2 are:

⁶ The model can be easily extended to an unfair coin, i.e. $p_1^{Random} \neq p_2^{Random}$.

$$p_1^* = a_1/(a_1+a_2) \text{ and } p_2^* = a_2/(a_1+a_2). \quad (4)$$

The expected payoffs of players 1 and 2 in the equilibrium are:

$$E(\pi_1^*) = va_1^2/(a_1+a_2)^2 \text{ and } E(\pi_2^*) = va_2^2/(a_1+a_2)^2. \quad (5)$$

Hence, the contest is both individually costly to the loser and socially costly because all conflict stage expenditures are wasted. As a result, if both players possess complete knowledge of the game's structure and incentives, they may prefer to avoid the conflict stage by flipping the coin.⁷ In fact, the weaker player 2 will always choose the random device over the conflict, since his expected payoff in the case of conflict is lower than in the case of a coin flip, i.e., $E(\pi_2^{Conflict}) = va_2^2/(a_1+a_2)^2 < va_2^2/(a_2+a_2)^2 = v/4 < v/2 = E(\pi_2^{Random})$. However, the choice of player 1 crucially depends on how much stronger player 1 is than player 2. In particular, player 1 will choose the coin flip only if $E(\pi_1^{Conflict}) = va_1^2/(a_1+a_2)^2 \leq v/2 = E(\pi_1^{Random})$, or when $a_2 \geq (\sqrt{2}-1)a_1$.

Hence, our theory predicts that if players' capabilities are relatively symmetric, then conflicts will be avoided via a random device.⁸ However, when the asymmetry is substantial then conflicts are unavoidable because the stronger party will always choose to enter the conflict. In what follows we explore the case of a fair coin ($p_1^{Random} = p_2^{Random} = 0.5$) and there are no costs of using the coin ($c_1 = c_2 = 0$). Note that if we increase the probability that the random device assigns the prize to the stronger player, i.e., $p_1^{Random} > p_2^{Random}$, we can again create a random device that both players would prefer over conflict. Indeed, because a random device is always surplus-preserving, there exists a random device (a coin) that would be preferred by both players for any costly conflict situation. Even if the random device itself is costly (i.e., $c_1 > 0$ and $c_2 > 0$),

⁷ One can interpret the conflict resolution stage as players bargaining to determine the form of the contest success function, i.e. either completely noisy or lottery contest success function.

⁸ In the relatively symmetric case, i.e. $a_2 \geq (\sqrt{2}-1)a_1$, there is also a subgame perfect Nash equilibrium in which both players simultaneously choose not to use the random device. Since the use of random device requires multilateral agreement, if one player chooses not to use the random device then the other player might as well choose not to use it. However, it is easy to see that such subgame perfect Nash equilibrium is not strategically stable (Kohlberg and Mertens, 1986), and both players can benefit from unilaterally deviating from it. In fact, we do not find any support for this equilibrium in our data, as weak players almost always choose to use the random device.

it is preferable by both players as long as individual (expected) costs of using the random device are less than individual (expected) benefits of avoiding conflicts through the random device, i.e., $c_1 \leq E(\pi_1^{Random}) - E(\pi_1^{Conflict})$ and $c_2 \leq E(\pi_2^{Random}) - E(\pi_2^{Conflict})$. In the experiment we examine the simplest case of a fair coin (i.e., unbiased random device) and no explicit costs of using a coin flip (nonetheless, participants had to explicitly choose to see the outcome of the random device each round in the repeated interaction). This allows us to focus on the strategic aspects of the random device as a conflict resolution mechanism and to examine causal effects of power differences (i.e., $a_1 > a_2$) on the likelihood of conflict resolution.

4. Experimental Design and Procedures

We employ four treatments to test the predictions of our theory: *Baseline-Unbalanced*, *Baseline-Balanced*, *Random-Unbalanced*, and *Random-Balanced*, where *Unbalanced* and *Balanced* refer to the relative strengths of players 1 and 2. The outline of the experimental design and the theoretical predictions for each treatment are shown in Table 1. In all treatments participants compete for a prize with value $v = 100$. In the *Baseline-Unbalanced* treatment, players 1 and 2 have substantially different strengths, i.e., $a_1 = 4$ and $a_2 = 1$. In equilibrium, the expenditures of players 1 and 2 are $e_1^* = 16$ and $e_2^* = 16$, the probabilities of winning are $p_1^* = 0.8$ and $p_2^* = 0.2$, and the expected payoffs are $E(\pi_1^{Conflict}) = 64$ and $E(\pi_2^{Conflict}) = 4$. In the *Baseline-Balanced* treatment, players 1 and 2 have relatively symmetric strengths, i.e., $a_1 = 3/2$ and $a_2 = 1$. In equilibrium players 1 and 2 expend $e_1^* = 24$ and $e_2^* = 24$, win the conflict with probabilities $p_1^* = 0.6$ and $p_2^* = 0.4$, and earn expected payoffs of $E(\pi_1^{Conflict}) = 36$ and $E(\pi_2^{Conflict}) = 16$. Thus, the costs of conflict are increasing in the relative symmetry of the players.

The other two treatments, *Random-Unbalanced* and *Random-Balanced* introduce the conflict resolution stage to the game, prior to the conflict stage. In the conflict resolution stage, players simultaneously decide whether to flip a coin in order to determine the winner of the prize. In case of disagreement or joint refusal to use the random device, both treatments are equivalent to *Baseline-Unbalanced* and *Baseline-Balanced* treatments. The equilibrium prediction in the *Random-Unbalanced* treatment is that conflict always occurs, because the stronger player 1 always chooses to enter the conflict since $E(\pi_1^{Conflict}) = 64 > 50 = E(\pi_1^{Random})$. On the other hand, in the *Random-Balanced* treatment, conflict never occurs, because both players always agree to use the random device since $E(\pi_1^{Conflict}) = 36 < 50 = E(\pi_1^{Random})$.

180 participants were recruited at random from the subject pool consisting of graduate and undergraduate students at a private university in the United States. Participants were randomly assigned into 8 experimental sessions (2 per treatment) with between 20 and 24 participants each. Participants sat at, and interacted via, visually isolated computer terminals, and instructions were read aloud by the experimenter as participants followed along on paper.⁹ The experiments were programmed using z-Tree (Fischbacher, 2007). At the beginning of each session participants took a quiz (non-incentivized) to confirm their understanding of the experimental procedures.

Each experimental session consisted of 30 periods of a single treatment. Participants were randomly assigned the initial role of either player 1 or player 2, and each player 1 was matched with a player 2. Participants remained in this role and matched pair for the first 15 periods of the session. Then all players 1 became players 2 and vice versa, and each participant was randomly re-matched with another participant in the session (who had previously been in the opposite

⁹ Instructions for the *Random-Unbalanced* treatment are included in an appendix. Instructions for the other treatments are available upon request.

initial role) to form a new pair for the final 15 periods. Each period, in the *Baseline-Unbalanced* and *Baseline-Balanced* treatments participants chose their expenditures, any integer between 0 and 100, which determined their probabilities of winning the prize of 100 francs. At the end of each period, the computer displayed these expenditures, as well as corresponding payoffs. In the *Random-Unbalanced* and *Random-Balanced* treatments, participants first made their decisions whether to flip a computerized coin in order to determine who would receive the prize, or to enter the second stage conflict. The computer displayed participants' first stage decisions. Then, if at least one of the paired players decided not to flip the coin, participants proceeded to the second stage where they chose their expenditures. At the end of each period, the computer displayed individual decisions, as well as corresponding payoffs, to each participant.

At the end of the session, we randomly selected 4 of the 30 periods for payment. Participants' total earnings from these 4 periods were added to or subtracted from an initial endowment of 500 francs (which we described as a participation fee). We converted francs to USD at a rate of 25 = \$1, and participants were paid privately in cash and dismissed from the experiment. The average experimental earnings, including the \$20 participation fee, were \$24.9, ranging from a low of \$11.6 to a high of \$35.6. Sessions lasted approximately an hour each.

Before reporting the results, it is important to comment on some elements of our design. First, we elected to induce strength asymmetries ($a_1 \neq a_2$) in all treatments so that differences in behavior can be directly attributed to the degree of strength asymmetry, and not simply to whether players were symmetric or not. Second, our role-switching protocol helps mitigate concerns about expected-payoff inequity across participants. Furthermore, we chose to use fixed matching to reduce the potential for contamination of participants' behavior in one session by outliers (i.e., in a random matching environment the presence of several outliers could

substantially impact the behavior of all participants in the session) and to mirror the repeated interactions parties in our historical examples faced. Finally, we chose to pay participants based on a random subset of periods to increase the per-chosen-period payoff (and thus the salience of the decision), without increasing the cost of running the experiment, and to reduce wealth effects and other confounding path-dependencies that are not part of the economic system being modeled.

5. Results

Table 2 reports the aggregate experimental results across the four treatments, including the average expenditure, payoff, probability of entering the conflict and economic surplus. Most of our statistical analysis is based on panel regressions. To control for repeated measures, our regressions (both linear and probabilistic) employ random-effects models. We also control for learning by including a period trend and for role-switching by including a dummy-variable for the last 15 periods after participants switch roles. Finally, treatment effects are captured by dummy-variables.

5.1. Surplus

One of the main research questions of our study is whether the availability of the random device reduces the cost of conflict and increases economic surplus. Comparing surpluses in Table 2, we find that surplus is 39% higher in the *Random-Unbalanced* than the *Baseline-Unbalanced* treatment (76.1 versus 54.8), and it is 69% higher in the *Random-Balanced* than the *Baseline-Balanced* treatment (75.8 versus 44.9). Figure 1, displaying time series of mean pairwise surplus for each treatment, indicates that the difference between treatments persists

throughout the experiment (although it declines over time). To support these findings statistically, we estimate panel regressions separately for the *Unbalanced* treatments and for the *Balanced* treatments where the dependent variable is pairwise surplus, defined as $v - (e_1 + e_2)$ for each pair in each period, and the independent variables are a period trend, a dummy-variable controlling for the last 15 periods after participants switch roles, and a dummy-variable for the presence of the random device. We include a random effects error structure for the individual participants to account for repeated measures, which is equivalent to group random effects since surplus is measured at the level of the pair.

The estimation results are shown in Table 3. Mixed evidence on the significance of the period trend indicates increasing surplus over time in the *Unbalanced* treatments, though not in the *Balanced* treatments.¹⁰ The insignificant estimated effect of switching roles reveals the absence of notable re-matching effects. More important for our purposes, regressions (1) and (2) indicate that the effect of the random device on surplus is positive and significant for both treatments (p-values < .01). However, relative to theoretical benchmarks, surplus in the *Random-Unbalanced* treatment is significantly higher than predicted (76.1 versus 68); while in the *Random-Balanced* treatment, it is significantly lower than predicted (75.8 versus 100).¹¹ We summarize these findings in Result 1.

Result 1: The random device increases mean surplus in both treatments. However, the surplus is significantly higher than predicted in the *Random-Unbalanced* treatment and significantly lower in the *Random-Balanced* treatment.

¹⁰ As can be seen in Figure 3, discussed in Section 5.3, this is largely due to relatively high early-period conflict expenditures in the *Unbalanced* treatments.

¹¹ Separately for the *Random-Unbalanced* and *Random-Balanced* treatments, we estimate random effects panel regressions in which the dependent variable is surplus and the independent variables are a constant, a period trend, and a dummy-variable controlling for switched roles. A Wald test indicates that the estimated constant, which captures surplus given an insignificant coefficient on the period trend, is significantly higher than predicted in the *Random-Unbalanced* treatment (p-value = .02) and significantly lower than predicted in the *Random-Balanced* treatment (p-value < .01).

Does the deviation from the theory derive from the entry decisions in the conflict resolution stage or from expenditures in the conflict stage? The estimations of regressions (3) and (4) in Table 3 provide the first insight. These regressions use only those observations that resulted in conflict. The estimated effect of the random device is insignificant for both the *Unbalanced* and *Balanced* treatments, indicating that higher surplus in the *Random-Unbalanced* and *Random-Balanced* treatments mainly comes from decisions not to enter the conflict. When conflict occurs in the *Random-Unbalanced* and *Random-Balanced* treatments, it is neither more nor less intense than conflict in the *Baseline-Unbalanced* and *Baseline-Balanced* treatments. In the next section we further analyze how entry decisions differ from the theoretical predictions, therefore implicitly shaping Result 1.

5.2. Probability of a Conflict

The primary source of deviations between observed and predicted surplus in Result 1 is deviation from the predicted frequency of using the random device to settle conflicts. In particular, Table 2 reports that participants in the *Random-Balanced* treatment employ the random device with probability 0.63, although the theory predicts that no conflict will occur. On the other hand, the *Random-Unbalanced* participants use the random device with probability 0.40, though theory predicts that they will fight with probability 1. Figure 2 displays time series of conflict entry probability by participant type. Although observed probabilities of entry clearly differ from the Nash equilibrium, each player type moves in the direction of the predictions over time. Moreover, when conflict occurs, the stronger player usually instigates, and the probability of entry by strong players is higher in the *Random-Unbalanced* than the *Random-Balanced*

treatment. Finally, weaker players typically choose not to enter conflict, regardless of their relative strength.

To support these observations, we estimate panel probit models with random effects for each participant, where the dependent variable takes a value of 1 if a player chose to engage in conflict and 0 otherwise. The independent variables are a constant term, a dummy-variable for the weaker player 2, a period trend, a weaker-period interaction term to account for separate learning trends by player type, and a dummy-variable controlling for switched roles after period 15. Table 4 reports estimation results. An insignificant estimated coefficient of the role-switching dummy indicates no notable changes as a result of re-matching. However, for the *Random-Unbalanced* treatment, stronger players significantly increase and weaker players significantly decrease their probabilities of entering conflict over time. In the *Random-Balanced* treatment, both period trends have signs matching that in the *Random-Unbalanced* treatment, but the effects are insignificant (period p-value = .43 and period-weaker p-value = .14).

Furthermore, our results are consistent with the comparative statics predictions of the model. The weaker player 2 chooses to engage in conflict significantly less often than the stronger player 1. However, as noted above, the overall probability of a conflict is lower than the predicted value of 1 in the *Random-Unbalanced* treatment and higher than 0 in the *Random-Balanced* treatment. Table 4 reports an additional estimation combining data from the *Random-Unbalanced* and *Random-Balanced* treatments (column 3). Again consistent with the comparative statics predictions, the *Unbalanced* coefficient is positive, indicating that the probability of a conflict increases in players' asymmetry. We summarize these findings in Result 2.

Result 2: The probability of a conflict is higher when players are more asymmetric, and players' choices are increasingly aligned with the theory over time. There are significantly fewer conflicts than predicted in the *Random-Unbalanced* treatment and significantly more conflicts in the *Random-Balanced* treatment.

These findings support our claim that the divergence between the observed and predicted impact of the random device on surplus in both treatments (Result 1) can largely be traced to differences between the observed and predicted probabilities of conflict (Result 2). What can explain why we observe “too little” conflict in the *Random-Unbalanced* and “too much” conflict in the *Random-Balanced* treatment?

First, the predicted probabilities of conflict are at the boundaries, so that in the *Random-Unbalanced* treatment even one non-equilibrium choice of non-entry implies an average probability of conflict less than one.¹² Similarly, in the *Random-Balanced* treatment even one non-equilibrium choice of entry implies an average probability of conflict more than zero. Thus, deviations from the predictions can only be observed in one direction, consistent with our findings.

Second, the deviation from theory can also be explained by the relatively weak incentives faced by the stronger player. Note that when comparing individual probability of choosing conflict (see Table 2), the weaker player 2 hews to the predictions of the theory rather well while the stronger player 1 does not. Theoretically, in the *Random-Unbalanced* treatment, by engaging in a conflict instead of using a coin flip, player 1 gains 14 in expectation ($64-50 = 14$) while player 2 loses 46 ($4-50 = -46$). In the *Random-Balanced* treatment, by engaging in a conflict, player 1 loses 14 ($36-50 = -14$) while player 2 loses 34 ($16-50 = -36$). Thus, the weaker player

¹² The problem of boundary equilibrium predictions has been well recognized in linear public good experiments, where the Nash equilibrium is to contribute nothing. For a review see Laury and Holt (2008).

always has a stronger incentive to conform to the theory, which is exactly what we observe in our experiment.

Whether the relatively weak incentives faced by the stronger player or the end point predictions produce deviations of individual entry decisions from point predictions, the comparative statics of the conflict resolution model hold in our experiment, suggesting that a random device can serve as an effective conflict resolution mechanism. This is encouraging, given that many earlier experiments have documented very high costs of conflict (Davis and Reilly, 1998; Potters et al., 1998; Fonseca, 2009; Sheremeta, 2010, 2011; Morgan et al., 2012). Indeed, the same pattern emerges in our experiment. As we show in the next section, *when conflict occurs*, the costs exceed the predictions of the theory.

5.3. Conflict Expenditures

Table 2 reports mean expenditures in excess of the Nash equilibrium predictions for both player types in all treatments, and Figure 3, which displays time series of mean individual expenditure by role and treatment, also shows that the excess expenditures persist throughout the experiment, though there is some early decline, particularly in the *Baseline-Unbalanced* treatment.

To examine the determinants of expenditures, we combine the data across treatments and estimate a single panel regression in which the dependent variable is expenditure and the independent variables are a dummy-variable for the weaker player 2, a period trend, a dummy-variable for the last 15 periods indicating the periods after roles have switched, *Unbalanced* and *Random* treatment dummies, and a constant term. For the *Random-Balanced* and *Random-*

Unbalanced treatments we restrict analysis to instances of conflict. As before, we include a random effects error structure for the individual participants to account for repeated measures.

Table 5 displays the output of this regression. As in the other regressions, the estimated coefficient of the role-switching dummy is insignificant. Furthermore, the period trend, the period-weaker interaction, and the weaker dummy variable are all insignificant. However, when allowing for separate trends for each player type by treatment, there was a negative and significant trend for both types in the *Baseline-Unbalanced* treatment (p-values < .05; also see Figure 3). Most importantly, consistent with the theory, expenditures decrease as players become more asymmetric (the coefficient on *Unbalanced* is negative and significant).

One might expect that the forgone opportunity to avoid the costs of conflict would induce retaliatory behavior by those participants that chose *not* to enter but were dragged into conflict by their counterpart. The insignificant effect of the random device on conflict expenditures, conditional on entry, indicates that conflict intensity remains unchanged in the presence of the random device.¹³ Nevertheless, Wald tests reject the hypotheses that the constant term is equal to 24 (the predicted expenditure in the *Balanced* treatments) and that the sum of the constant and the *Unbalanced* dummy-variable is equal to 16 (the predicted expenditure in the *Unbalanced* treatments) (p-values < .01). We summarize these findings in Result 3.

Result 3: Although participants overinvest in conflict on average, expenditures of both types are lower when players are more asymmetric. Furthermore, for a given asymmetry level, there is no difference in expenditures between stronger and weaker players.

While our experiment was not designed to test this hypothesis in particular, the data suggest that, *ceteris paribus*, conflict expenditures decline with relative strength asymmetry in

¹³ As a robustness check in the *Random-Balanced* and *Random-Unbalanced* treatments, we have also estimated the regression in Table 5 controlling for the individual entry decisions in the conflict resolution stage. Estimation results are virtually the same and are available from authors upon request.

repeated Tullock contests, and this finding is consistent with a wider literature. For example, in animal behavior studies, one common explanation for conflict behavior is based on “fighting assessment” (Parker, 1974). So-called “mutual assessment” models hold that in resource contests, animals assess the relative strength of their opponents, and upon recognizing an unfavorable asymmetry they are less likely to expend resources in conflict, thereby reducing the total costs of conflict.¹⁴ Empirical evidence from a broad range of species shows that the average length of resource contests is decreasing in the relative asymmetry of the competitors (Arnott and Elwood, 2009). Finally, several recent experimental studies have also documented that increasing players’ asymmetry reduces effort expenditures in diverse competitive environments (Fonseca, 2009; Anderson and Freeborn, 2010; DeScioli and Wilson, 2011).

6. Conclusion

Our model of conflict resolution between asymmetric contestants predicts that when players are sufficiently symmetric, they will choose to settle their disputes by using a random device in order to avoid a costly conflict. On the other hand, if players are sufficiently asymmetric, the stronger player will always choose outright conflict because the expected benefits of conflict exceed the expected value of the random allocation. We test these theoretical predictions in the laboratory with two treatments, one in which the players are relatively *Balanced* and one in which they are *Unbalanced*. In keeping with previous studies of conflict we find evidence that conflicts are generally more costly than expected, as conflict expenditures are higher than predicted in all treatments. Also, consistent with previous animal behavior and human participant research on the intensity of conflict with respect to the relative strength asymmetry of the contestants, we find that expenditures are greater in the *Balanced* treatments

¹⁴ As the Kenny Rogers lyric famously reads, “You gotta know when to hold ‘em, know when to fold ‘em...”

than in the *Unbalanced* treatments. Most importantly, we find compelling evidence that the random device reduces the costs of conflict in both treatments: when agents are relatively symmetric, the random device increases average surplus by 69%, and when they are relatively asymmetric, it still increases average surplus by 39%.

Our results highlight the power of randomized allocation as a surplus-preserving conflict resolution mechanism and explain both historical and modern implementations of such mechanisms ranging from the Delphic Oracle in archaic Greece to rock-paper-scissors. Many superficially mystifying practices that appear absurd to modern eyes can be revealed to serve similar purposes, and the history of economic and political institutions may benefit from studying such mechanisms in this light. It is also important to emphasize that the random device is most successful in resolving conflicts between relatively symmetric agents, which according to prior research tend to engage in more costly conflicts than more asymmetric agents (Davis and Reilly, 1998; Fonseca, 2009; Anderson and Freeborn, 2010; DeScioli and Wilson, 2011).

Both the data and the theory suggest a number of interesting extensions. Although it is clear why the random device may work as a surplus-preserving conflict resolution mechanism, it is not obvious why agents would commit to a proposed resolution after observing an ex post unfavorable decision by the random device. In this paper we ensure that agents abide by the outcome of the random device by enforcing binding contracts. However, in practice, agents cannot always enter binding contracts. As in our introductory example, it is unclear what would prevent Athens from rejecting the Oracle's counsel and fighting Sparta anyway if Sparta was

avored by the decision.¹⁵ Future research should examine commitment problems in conflict resolution.¹⁶

Another interesting question is how much participants would be willing to pay in order to use a random device; the theoretical willingness to pay is noted in Section 3, but if there exists a threshold for provision of the random device as a public good, then the collective action problem may reduce the likelihood of surplus preserving solutions. On the other hand, one could ask whether various side-payment schemes would increase the willingness of strong players to consent to randomization, and if participants can bargain over the assigned probabilities, the observed surplus gains may be extended to even more asymmetric environments.

¹⁵ Two reasons why agents committed to the Oracle's decisions in ancient times were historical context and the perceived 'mystical' nature of such decisions (Iannaccone et al., 2011), and reputational concerns resulting from repeated interaction with one another and with the Oracle (Dellarocas, 2006).

¹⁶ See Powell (2006) for a discussion of commitment problems and armed conflict between nations.

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Tables and Figures

Table 1: Experimental Design and Theoretical Predictions

	Player	Treatment			
		<i>Baseline- Unbalanced</i>	<i>Baseline- Balanced</i>	<i>Random- Unbalanced</i>	<i>Random- Balanced</i>
Strength, a_i	1	4	3/2	4	3/2
	2	1	1	1	1
Expenditure, e_i^*	1	16	24	16	24
	2	16	24	16	24
Probability, p_i^*	1	0.80	0.60	0.80	0.60
	2	0.20	0.40	0.20	0.40
Expected Payoff, $E(\pi_i^*)$	1	64	36	64	36
	2	4	16	4	16
Individual Probability of Choosing Conflict	1	N/A	N/A	1.00	0.00
	2	N/A	N/A	0.00	0.00
Probability of a Conflict		1.00	1.00	1.00	0.00
Surplus		68	52	68	100

Table 2: Aggregate Experimental Results

	Player	Treatment			
		<i>Baseline- Unbalanced</i>	<i>Baseline- Balanced</i>	<i>Random- Unbalanced</i>	<i>Random- Balanced</i>
Expenditure, e_i	1	21.4 (0.8)	27.9 (0.8)	17.8 (0.7)	31.8 (1.7)
	2	23.7 (1.1)	27.2 (0.9)	19.7 (1.2)	28.4 (1.8)
Probability, p_i	1	0.77 (0.02)	0.61 (0.02)	0.80 (0.02)	0.64 (0.03)
	2	0.23 (0.02)	0.39 (0.02)	0.20 (0.02)	0.36 (0.03)
Expected Payoff, $E(\pi_i)$	1	55.2 (1.7)	33.1 (1.8)	61.7 (2.1)	32.4 (3.1)
	2	-0.4 (1.6)	11.8 (1.8)	0.8 (1.8)	7.4 (2.9)
Individual Probability of Choosing Conflict	1	N/A	N/A	0.60 (0.02)	0.33 (0.02)
	2	N/A	N/A	0.06 (0.01)	0.13 (0.01)
Probability of a Conflict		1.00	1.00	0.63 (0.02)	0.40 (0.02)
Surplus		54.8 (1.5)	44.9 (1.4)	76.1 (1.2)	75.8 (1.6)

Mean (standard errors) reported. The “Individual Probability of Choosing Conflict” and “Probability of a Conflict” are the empirical probabilities of conflict.

Table 3: The Impact of the Random Device on Group Surplus

Regression	(1)	(2)	(3)	(4)
Dependent Variable	Surplus			
	<i>Unbalanced</i>	<i>Balanced</i>	<i>Unbalanced</i>	<i>Balanced</i>
<i>Period</i>	0.502**	-0.082	0.713***	-0.243
[period trend]	(0.163)	(0.175)	(0.179)	(0.182)
<i>Switch</i>	-3.722	5.549	-3.954	4.918
[1 if period > 15]	(5.935)	(6.543)	(6.071)	(8.476)
<i>Random</i>	21.31***	30.96***	6.150	-10.41
[1 if Random]	(5.478)	(5.939)	(5.461)	(8.495)
<i>Constant</i>	48.91***	43.36***	45.76***	46.18***
	(5.231)	(4.815)	(5.467)	(6.739)
Observations	1320	1380	1090	967
Number of groups	88	92	86	80

*** p<.001, ** p<.01, * p<.05. Robust standard errors in parentheses. Panel linear models (1)-(4) include individual participant random effects. Regressions (3) and (4) use only those observations which resulted in conflict.

Table 4: Individual Probability of Choosing Conflict

Regression	(1)	(2)	(3)
Dependent Variable	Probability of a Conflict		
	<i>Random- Unbalanced</i>	<i>Random- Balanced</i>	<i>Random</i>
<i>Weaker</i>	-1.345***	-0.57^	-0.993***
[1 if player 2]	(0.288)	(0.315)	(0.216)
<i>Period</i>	0.025*	-0.011	0.017*
[period trend]	(0.012)	(0.013)	(0.0088)
<i>Weaker × Period</i>	-0.043*	-0.029	-0.035*
[weaker times period]	(0.018)	(0.019)	(0.014)
<i>Switch</i>	0.0003	-0.312	-0.168
[1 if period > 15]	(0.192)	(0.190)	(0.134)
<i>Unbalanced</i>			0.595**
[1 if Unbalanced]			(0.207)
<i>Constant</i>	0.0997	-0.734**	-0.682***
	(0.180)	(0.246)	(0.184)
Observations	1260	1380	2640
Number of groups	42	46	88

*** p<.001, ** p<.01, * p<.05, ^ p<.01. Standard errors in parentheses. Panel probit models (1)-(3) include individual participant random effects.

Table 5: Determinants of Expenditure

Regression (1)	
Dependent Variable	Expenditure
<i>All Treatments</i>	
<i>Weaker</i>	0.422
[1 if player 2]	(2.602)
<i>Period</i>	-0.158
[period trend]	(0.09)
<i>Weaker × Period</i>	0.011
[weaker times period]	(0.152)
<i>Switch</i>	-0.682
[1 if period > 15]	(1.879)
<i>Unbalanced</i>	-7.752**
[1 if Unbalanced]	(2.486)
<i>Random</i>	-0.568
[1 if Random]	(2.503)
<i>Constant</i>	31.439***
	(2.566)
Observations	4114
Number of groups	177

*** p<.001, ** p<.01, * p<.05. Robust standard errors in parentheses. Panel linear model (1) includes individual participant random effects.

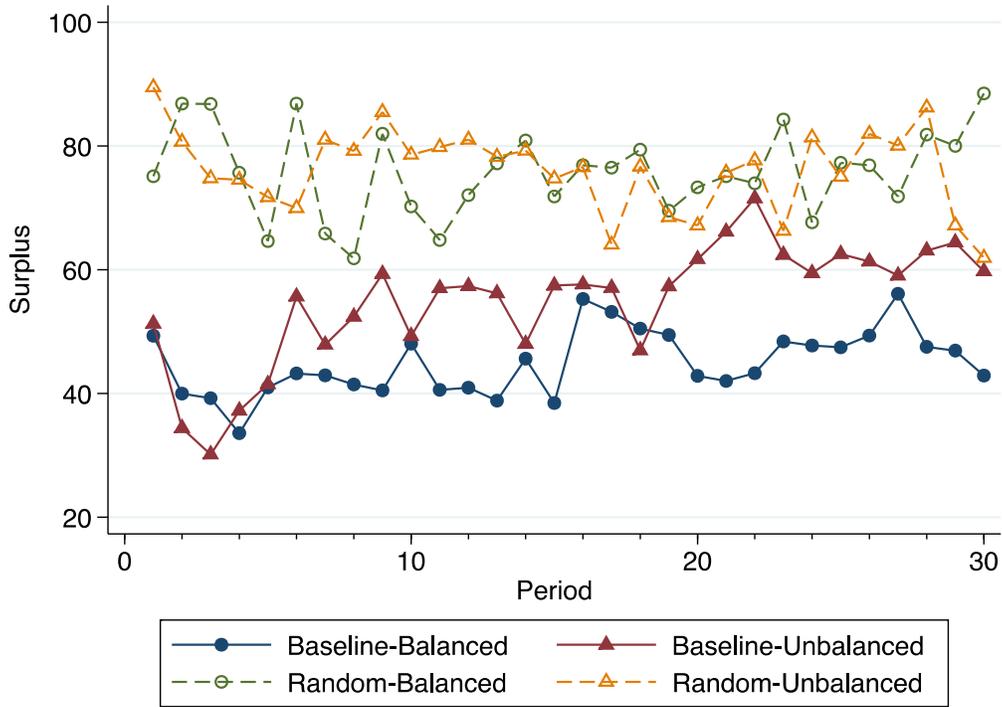


Figure 1: Time Series of Mean Surplus, by Treatment

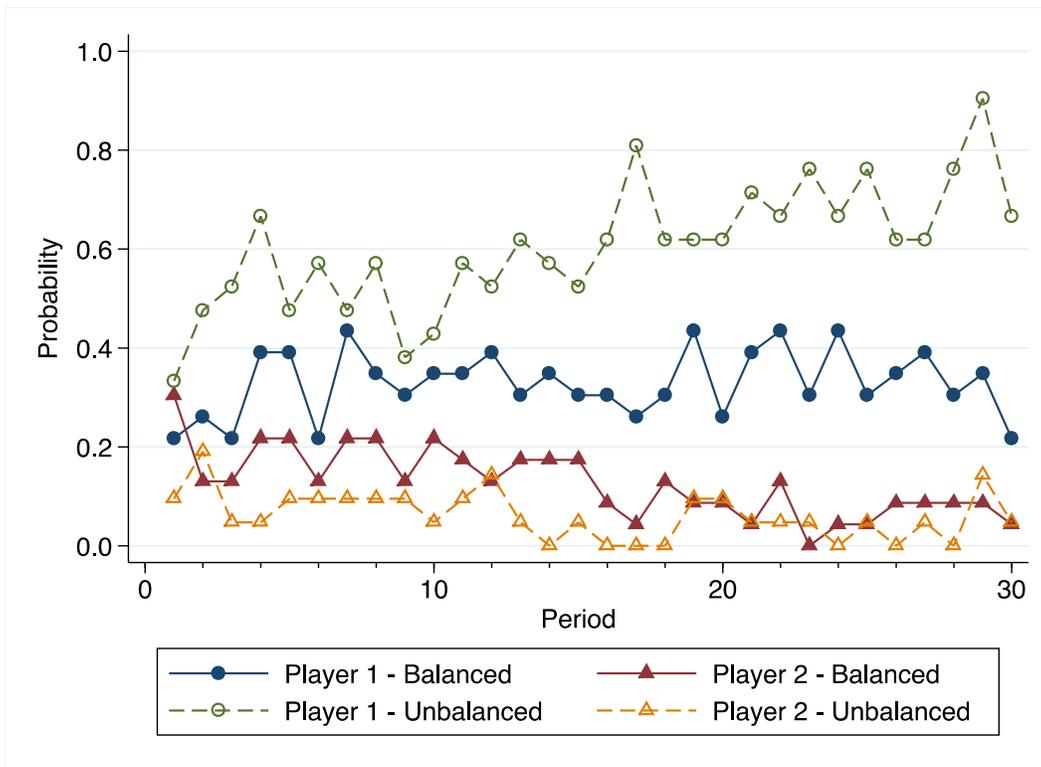


Figure 2: Time Series of Mean Probability of Choosing Conflict by Role, Random Treatments

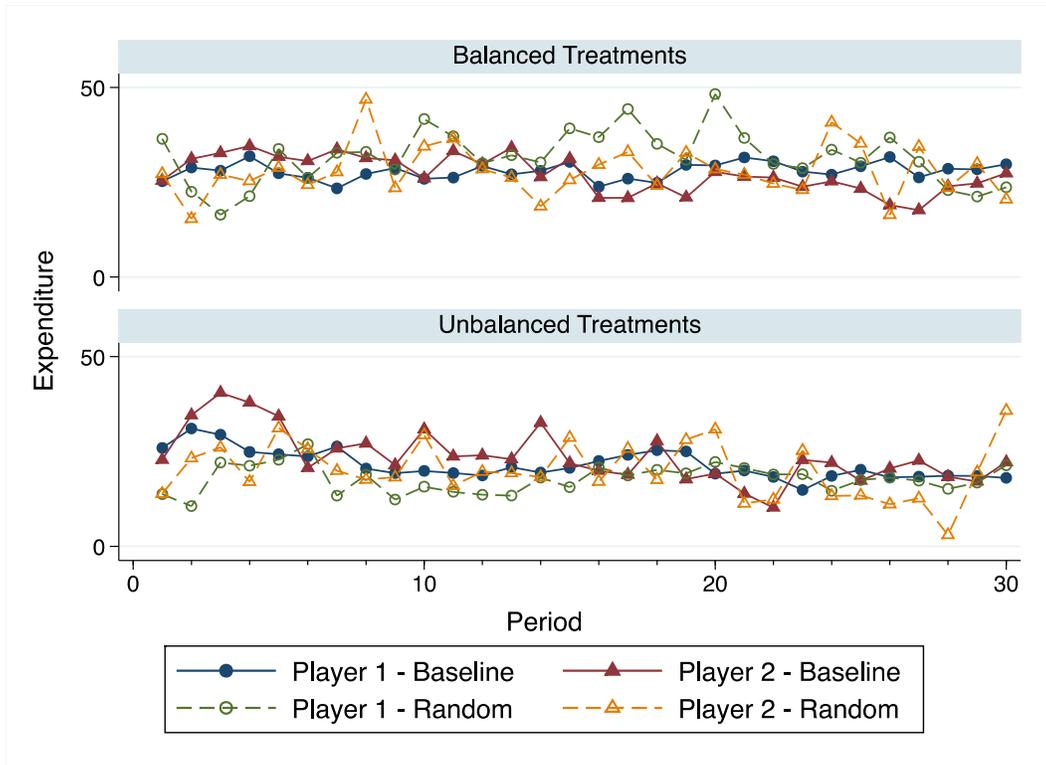
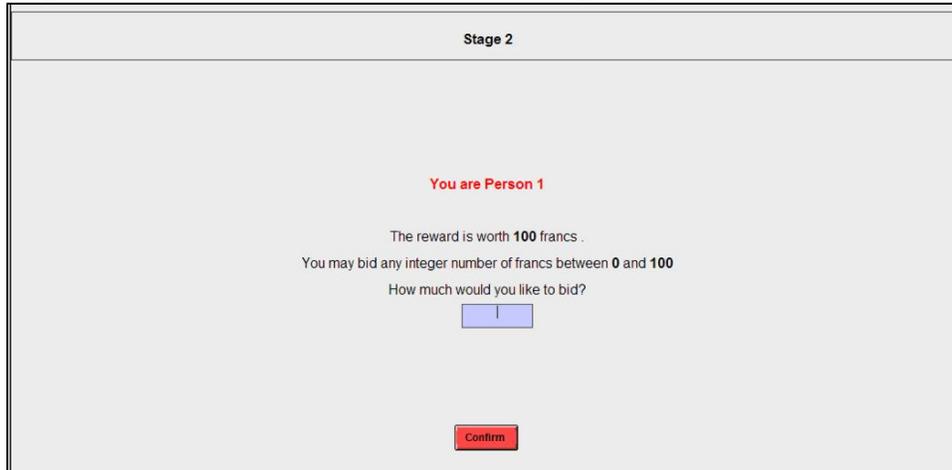


Figure 3: Time Series of Mean Expenditure by Role and Treatment

If you want to flip the coin, check 'Yes'. However, if *either* Person 1 or Person 2 chooses to **not flip**, by checking 'No', then both participants will enter Stage 2. At the end of the experiment we will randomly select four out of 30 periods for payment--two from the first 15 periods and two from the last 15 periods.

YOUR DECISION IN STAGE 2

If either person chooses to enter **stage 2** by checking 'No', each person may **bid for the 100 franc reward**. You may bid any integer number of francs between **0** and **100**. An example of your decision screen is shown below.



EARNINGS IN STAGE 2

After both participants make their bids, your earnings for the period are calculated. Regardless of who receives the reward, *both* participants will have to pay their bids. So your earnings will be calculated in the following way:

- If you receive the reward:
Earnings in Stage 2 = 100 – Your Bid
- If you do not receive the reward:
Earnings in Stage 2 = 0 – Your Bid

Remember you have already earned a \$20.00 participation fee (equivalent to 500 francs). In any period, you may receive either positive or negative earnings. At the end of the experiment we will randomly select four out of 30 periods for payment--two from the first 15 periods and two from the last 15 periods. We will sum the total earnings for these four periods and convert them to a U.S. dollar payment. If the summed earnings are negative, we will subtract them from your participation fee. If the summed earnings are positive, we will add them to your participation fee.

What Does my Bid Mean?

The more you bid, the more likely you are to receive the reward. The more the other person bids, the less likely you are to receive the reward. Specifically:

- For **each franc** Person 1 bids Person 1 receives **8 lottery tickets**
- For **each franc** Person 2 bids Person 2 receives **2 lottery tickets**

At the end of each period the computer **draws randomly** one ticket among all the tickets purchased by **you and the other person**. The owner of the winning ticket receives the reward of 100 francs. Each ticket has an equal chance of winning. So your chance of receiving the reward is given by the number of tickets you buy divided by the total number of tickets bought by you and the other person.

$$\text{Chance of Receiving the Reward} = \frac{\text{Your Total Lottery Tickets}}{\text{Sum of Your and Other Person's Lottery Tickets}}$$

If both participants bid *zero* the reward is randomly assigned to one of the two participants.

Determining Who Wins The Reward

This is a hypothetical example used to illustrate how the computer determines who wins the reward of 100 francs. If Person 1 bids 20 francs and Person 2 bids 20 francs, then Person 1 receives 160 lottery tickets and Person 2 receives 40 lottery tickets. Then the computer randomly draws one lottery ticket out of 200 (160 + 40). As you can see, Person 1 has a higher chance of receiving the reward, $0.80 = 160/200$. Person 2 has a $0.20 = 40/200$ chance of receiving the reward.

After both participants bid, the computer will make a random draw that will determine who receives the reward. Then the computer will calculate your period earnings based on your bid and whether you received the reward or not. At the end of each period, you will see:

1. Whether you chose to flip a coin and whether the other person chose to flip a coin,
2. If applicable, how much you bid and how much the other person bid,
3. Whether you received the reward or not, and
4. Your earnings for the period.

Once the outcome screen is displayed you should record your results for the period on your **Personal Record Sheet** under the appropriate heading. An example of the outcome screen is shown below.

Stage 1

You wanted to use Coin Flip: Yes No

The other participant wanted to use Coin Flip: Yes No

Your bid:

Your number of tickets:

The other participant's bid:

The other participant's number of tickets:

Did you receive the reward:

Total earnings for this period:

OK

IMPORTANT NOTES

You will not be told which of the participants in this room are assigned to which two-person group. The group stays intact for 15 periods. At the end of 15 periods you will be randomly re-grouped with a different participant to form a new two-person group for another 15 periods. At the beginning of each period you will be told if you are Person 1 or Person 2. You will be Person 1 in half of the periods and Person 2 in half of the periods. You can never guarantee yourself the reward. If both participants choose to flip a computer coin in stage 1, then the reward is assigned randomly. If either person chooses not to flip a computer coin, then both participants proceed to stage 2. In stage 2, by increasing your bid you can increase your chance of receiving the reward.

Quiz

Before starting, we want you to answer some questions regarding the experiment to be sure you understand what will follow. After five minutes an experimenter will return to privately review your answers.

You will be Person 1 in how many of the periods?

You always have to bid more than zero (true or false)?

Your bid will be subtracted from your earnings (true or false)?

If both participants agree to the coin flip, what is the chance of Person 1 winning the reward?

Imagine Person 1 bids 1 franc and Person 2 bids 1 franc. What is the chance of Person 1 winning the reward?

Imagine Person 1 bids 2 francs and Person 2 bids 8 francs. What is the chance of Person 1 winning the reward?

Imagine Person 1 bids 8 francs and Person 2 bids 16 francs. What is the chance of Person 1 winning the reward?

Imagine Person 1 bids 18 francs and Person 2 bids 24 francs. What is the chance of Person 1 winning the reward?

Imagine Person 1 bids 18 francs and Person 2 bids 72 francs. What is the chance of Person 1 winning the reward?

Imagine Person 1 bids 0 francs and Person 2 bids 1 franc. What is the chance of Person 1 winning the reward?

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12-22 Dechenaux, E., Kovenock, D. and Sheremeta, R. “A Survey of Experimental Research on Contests, All-Pay Auctions and Tournaments.”

12-21 Rubin, J. and Sheremeta, R. “Principal-Agent Settings with Random Shocks.”

12-20 Gómez-Miñambres, J. and Schniter, E. “Menu-Dependent Emotions and Self-Control.”

12-19 Schniter, E., Sheremeta, R., and Sznycer, D. “Building and Rebuilding Trust with Promises and Apologies.”

- 12-18** Shields, T. and Xin, B. “Higher-order Beliefs in Simple Trading Models.”
- 12-17** Pfeiffer, G. and Shields, T. “Performance-Based Compensation and Firm Value: Experimental evidence.”
- 12-16** Kimbrough, E. and Sheremeta, R. “Why Can’t We Be Friends? Entitlements, bargaining, and conflict.”
- 12-15** Mago, S., Savikhin, A., and Sheremeta, R. “Facing Your Opponents: Social identification and information feedback in contests.”
- 12-14** McCarter, M., Kopelman, S., Turk, T. and Ybarra, C. “Too Many Cooks Spoil the Broth: How the tragedy of the anticommons emerges in organizations.”
- 12-13** Chowdhury, S., Sheremeta, R. and Turocy, T. “Overdissipation and Convergence in Rent-seeking Experiments: Cost structure and prize allocation rules.”
- 12-12** Bodsky, R., Donato, D., James, K. and Porter, D. “Experimental Evidence on the Properties of the California’s Cap and Trade Price Containment Reserve.”
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- 12-10** Klose, B. and Kovenock, D. “Extremism Drives Out Moderation.”
- 12-09** Buchanan, J. and Wilson, B. “An Experiment on Protecting Intellectual Property.”
- 12-08** Buchanan, J., Gjerstad, S. and Porter, D. “Information Effects in Multi-Unit Dutch Auctions.”
- 12-07** Price, C. and Sheremeta, R. “Endowment Origin, Demographic Effects and Individual Preferences in Contests.”
- 12-06** Mago, S. and Sheremeta, R. “Multi-Battle Contests: An experimental study.”
- 12-05** Sheremeta, R. and Shields, T. “Do Liars Believe? Beliefs and Other-Regarding Preferences in Sender-Receiver Games.”
- 12-04** Sheremeta, R., Masters, W. and Cason, T. “Winner-Take-All and Proportional-Prize Contests: Theory and experimental results.”
- 12-03** Buchanan, J., Gjerstad, S. and Smith, V. “There’s No Place Like Home.”
- 12-02** Corgnet, B. and Rodriguez-Lara, I. “Are you a Good Employee or Simply a Good Guy? Influence Costs and Contract Design.”
- 12-01** Kimbrough, E. and Sheremeta, R. “Side-Payments and the Costs of Conflict.”

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- 11-20** Cason, T., Savikhin, A. and Sheremeta, R. “Behavioral Spillovers in Coordination Games.”
- 11-19** Munro, D. and Rassenti, S. “Combinatorial Clock Auctions: Price direction and performance.”
- 11-18** Schniter, E., Sheremeta, R., and Sznycer, D. “Restoring Damaged Trust with Promises, Atonement and Apology.”
- 11-17** Brañas-Garza, P., and Proestakis, A. “Self-discrimination: A field experiment on obesity.”
- 11-16** Brañas-Garza, P., Bucheli, M., Paz Espinosa, M., and García-Muñoz, T. “Moral Cleansing and Moral Licenses: Experimental evidence.”
- 11-15** Caginalp, G., Porter, D., and Hao, L. “Asset Market Reactions to News: An experimental study.”
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- 11-13** Chui, M., Porter, D., Rassenti, S. and Smith, V. “The Effect of Bidding Information in Ascending Auctions.”
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10-23 Sheremeta, R. “Perfect-Substitutes, Best-Shot, and Weakest-Link Contests between Groups.”

10-22 Mago, S., Sheremeta, R., and Yates, A. “Best-of-Three Contests: Experimental evidence.”

10-21 Kimbrough, E. and Sheremeta, R. “Make Him an Offer He Can't Refuse: Avoiding conflicts through side payments.”

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10-19 Sheremeta, R. and Shields, T. “Do Investors Trust or Simply Gamble?”

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09-08 Sheremeta, R. and Zhang, J. “Multi-Level Trust Game with "Insider" Communication.”

09-07 Price, C. and Sheremeta, R. “Endowment Effects in Contests.”

09-06 Cason, T., Savikhin, A. and Sheremeta, R. “Cooperation Spillovers in Coordination Games.”

09-05 Sheremeta, R. “Contest Design: An experimental investigation.”

09-04 Sheremeta, R. “Experimental Comparison of Multi-Stage and One-Stage Contests.”

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08-09 Dickhaut, J., Houser, D., Aimone, J., Tila, D. and Johnson, C. “High Stakes Behavior with Low Payoffs: Inducing preferences with Holt-Laury gambles.”

08-08 Stecher, J., Shields, T. and Dickhaut, J. “Generating Ambiguity in the Laboratory.”

08-07 Stecher, J., Lunawat, R., Pronin, K. and Dickhaut, J. “Decision Making and Trade without Probabilities.”

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08-05 Anctil, R., Dickhaut, J., Johnson, K., and Kanodia, C. “Does Information Transparency Decrease Coordination Failure?”

08-04 Tila, D. and Porter, D. “Group Prediction in Information Markets With and Without Trading Information and Price Manipulation Incentives.”

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08-01 Oprea, R., Porter, D., Hibbert, C., Hanson, R. and Tila, D. “Can Manipulators Mislead Prediction Market Observers?”