CEPA DP No. 20

SEPTEMBER 2020

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CEPA Discussion Papers

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ISSN (online) 2628-653X

CEPA Discussion Papers can be downloaded from RePEc
https://ideas.repec.org/s/pot/cepadp.html

Published online at the Institutional Repository of the University of Potsdam
https://doi.org/10.25932/publishup-47550

CEPA Discussion Paper No. 20 September 2020

Inefficient Cooperation under Stochastic and Strategic Uncertainty*

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ABSTRACT

Stochastic uncertainty can cause difficult coordination problems that may hinder mutually beneficial cooperation. We propose a mechanism of ex-post voluntary transfers designed to circumvent these coordination problems and ask whether it can do so. To test this, we implement a controlled laboratory experiment based on a repeatedly played Ultimatum Game with a stochastic endowment. Contrary to our hypothesis, we find that allowing voluntary transfers does not entail an efficiency increase. We suggest and analyze two main reasons for this finding: First, the stochastic uncertainty forces proposers to accept high strategic uncertainty if they intend to cooperate by claiming a low amount (which many proposers do not). Second, many responders behave only incompletely conditionally cooperative by transferring too little (which hinders cooperation in future periods).

Keywords: stochastic uncertainty, strategic uncertainty, cooperation, Ultimatum Game, experiment
JEL Codes: C78, C92, D74

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* This research did not receive any specific grant from funding agencies in the public, commercial or not-for-profit sectors. Declaration of interest: None. This project was preregistered (https://osf.io/7tc5w/?view_ only=05889e2c6a904093bbba3eb632b2ab70). Upon acceptance, data and scripts will be made available at OSF. We thank Max Andres, John Duffy, Christin Hoffmann, Kai Konrad, and Christian Traxler for helpful comments and Luis Koch, Fenja Meinecke, Cosima Obst, Max Padubrin, and Sarah Skladny for excellent research assistance.

1 Introduction

Bargaining under uncertainty is difficult for many reasons. First of all, stochastic uncertainty—uncertainty about the realization of an environmental variable—creates coordination problems. These problems are solvable as long as the bargaining parties can condition bargaining outcomes on the realized state of nature (Riddell, 1981),[1] but efficient solutions get harder once such stochastic uncertainty is combined with other uncertainties. For example, Cramton (1984, 1992) shows theoretically that uncertainty about others' preferences leads to inefficiencies in bargaining outcomes. Furthermore, stochastic uncertainty often comes with strategic uncertainty—uncertainty about the behavior of others—because an increase in stochastic uncertainty for a given agent makes this agent's behavior less predictable or because it forces agents into a mutual dependency.[2] Finally, both stochastic and strategic uncertainty are often distributed asymmetrically among bargaining parties, for example, due to an informational advantage or due to a sequential order of decisions as, e.g., in Grossman and Perry (1986).

For illustration purposes, let us introduce one specific example of such bargaining under multiple forms of uncertainty. The difficulties described above arise almost always when a country considers committing resources to an international organization, but the World Health Organization's (WHO) Contingency Fund for Emergencies (CFE) is a particularly telling example.[3] The CFE was established in 2015 (after the Ebola outbreak in 2014) to allow the WHO to

[1] White (2008) additionally shows that risk aversion surprisingly is an advantage in similar contexts.

[2] Meirowitz et al. (2008) show that countries may even have a unilateral incentive to create strategic uncertainty about their own investment into military capacity, although it increases the risk of war.

[3] There are numerous other examples: In international relations, external shocks such as election outcomes can influence trade negotiations (v. Milner and Rosendorff, 1997), and uncertainty on a temperature threshold can impede successful climate negotiations (Barrett and Dannenberg, 2012). To deal with an increasingly globalized world—which is full of exogenous shocks—a country might consider surrendering some sovereignty to a supranational organization. In another domain, stochastic uncertainty may raise challenges for firms that intend to collude under demand fluctuations (Green and Porter, 1984). Wage negotiations, e.g. negotiations between firms and unions, can suffer from uncertainty on the firm's performance: Numerous external factors such as shocks to the business cycle can affect wage-increase leeway (Oderanti et al., 2012), which unions who start negotiations with a certain proposal may not know.

react quickly and flexibly to newly developing health crises (WHO, 2019, p. 4). In order to deal efficiently and effectively with upcoming crises of uncertain timing and impact (such as a new infectious disease), the WHO considered it appropriate to collect such funding in advance. A given country might want to contribute to the CFE to enable the WHO to react appropriately to such crises, not knowing what kind of crisis might come up and how strongly the given country would be affected. Thus, the country makes the funding decision under stochastic uncertainty. Furthermore, the country faces substantial strategic uncertainty because it commits resources and decision power to the WHO while it has no guarantee that the WHO will act in the country's interest when a crisis has emerged. Finally, stochastic uncertainty is considerably lower for the WHO after the emergence of a crisis than it was for the country when making the initial funding decision, enabling the WHO to make far better decisions, but also creating an asymmetry between both parties.

Although the CFE clearly deserves support, donations do not yet meet its full potential. Funding has increased in the last few years (WHO, 2019, p. 40), but large-scale events like the Ebola outbreak in the Democratic Republic of the Congo in 2018/2019 demand enormous allocations—\$67m in 2019, which is about half of the total sum donated to the CFE since its establishment (Brim and Wenham, 2019; WHO, 2020)—emphasizing the need for increased support for the program. Thus, it seems that there are substantial obstacles to cooperation under such complex uncertainty.

In this paper, we contribute to understanding the mechanisms hindering efficient cooperation. We replicate the main features of such bargaining under uncertainty in a model of an Ultimatum Game with a stochastic endowment and test in an experiment whether a mechanism of voluntary ex-post transfers can mitigate the problem. Given our result that the transfers cannot achieve that, we continue with an analysis of the reasons of this failure. In a nutshell, we find evidence for an interaction of two main forces: First, strategic uncertainty is too high to make the first-movers trust that second-movers will send appropriate compensation via an ex-post transfer. And second, we find that this lack of trust is justified because second-movers behave on average only incompletely conditionally cooperative.

The remainder of the paper is structured as follows: the next section discusses the adjacent literature, Section 3 introduces the model, and Section 4 explains the experiment. In Section 5, we present our hypotheses, and in Section 6, we show our results (divided into a part that tests the hypotheses and an exploratory part that presents detailed explanations of *why* we reject the hypotheses). The final section discusses our findings and concludes.

2 Related Literature

This study touches on different strands of literature, mainly the literature on cooperation in Public Goods experiments with punishments, rewards, and stochastic uncertainty, and the literature on conditional cooperation. The Ultimatum Game (Güth et al., 1982), a game of sequential bargaining to achieve mutually beneficial cooperation, serves as the design-basis of this study. Güth and Kocher (2014) provide a review of the Ultimatum Game literature.[4]

Efficiency-Enhancing Transfer Payments

We would expect that voluntary transfers have a similar effect on cooperation as rewards in Public Goods Game experiments in that they allow the responder to costly reward the proposer for cooperative behavior. In general, implementations of punishments or rewards in Public Goods Games decrease free-riding and increase contributions (Fehr and Gächter, 2000; Sefton et al., 2007; Choi and Ahn, 2013; Blanco et al., 2020). Bruttel and Güth (2018) analyze the effects of a voluntary transfer on contributions in a finitely repeated sequential two-player best-

[4]To some extent, uncertainty has been introduced to the analysis of ultimatum bargaining. Kagel et al. (1996) introduce information asymmetries, finding that responders accept unequal proposals more often if this is the result of a lack of information on the proposer's side. Srivastava et al. (2000) find that information asymmetries lead to inefficient bargaining results. Lee and Lau (2013) use unusual (meaning uneven) pie-sizes in their experiments and let these unknown to the participants to avoid a too rapid convergence to 50:50 splits.

shot experimental setting. They compare three treatments, two of which relate to our study. In one treatment, the lower contribution was refunded and the player who contributed less to the public good (and hence, did not contribute at all) was able to pay a voluntary transfer to the player who contributed more (and hence, contributed solely). In a second treatment, the lower contribution was refunded and no voluntary transfer was possible. The authors find that compensating through the use of the voluntary transfers is very frequent and increases efficiency, which is why we expected a similar effect in the present study.[5] Chatziathanasiou et al. (2020) experimentally test how redistribution affects the behavior of group members with different economic statuses. Participants are randomly assigned an economic status and are repeatedly matched with other participants of different (higher or lower) status to play a Battle-of-the-Sexes game. In a baseline treatment without redistribution, the authors find that lower-status participants concede and play the higher-status participants' preferred equilibrium (thus, increasing efficiency). In three treatments with different redistribution schemes (either voluntary transfers directly to the matched participant, voluntary transfers to a pool that is divided equally among all group members, or a randomly determined direct transfer to the matched participant), the efficiency in contrast to the baseline treatment further increases.

Uncertainty in Public Goods Games

Earlier research has introduced uncertainty in Public Goods Games in the form of threshold uncertainty (Suleiman, 1997; McBride, 2006, 2010), finding that the cooperation-facilitating or impeding effect of threshold uncertainty depends on the value of the public good (McBride, 2010) or, adapted from that, the value of the consequence of successful cooperation. The eventual pie-size in our design represents a threshold that players must undercut to reach the

[5]Hence, based on Bruttel and Güth (2018), we conducted a power-calculation (using G*Power by Faul et al. 2009) to estimate the sample size and according number of participants we need to test our main hypothesis H_1, which is the exact number of participants we invited. Please see the preregistration for more details (`https://osf.io/7tc5w/?view_only=05889e2c6a904093bbba3eb632b2ab70`).

desired outcome: Contributing in the Ultimatum Game is claiming less. Similarly, Common Pool Resource Games under stochastic uncertainty also relate to the present study (Rapoport and Au, 2001; Aflaki, 2013). If the size of the common pool is uncertain, both automated sanctions for asking too much and rewards for asking little are effective in preventing over-exploitation of the common pool resource (Rapoport and Au, 2001). Our aim differs in that the Ultimatum Game may describe goods of various natures, and sequential decision making with asymmetry in the agents' roles, knowledge, and possible actions. By emulating negotiations where agents can handle rewards and punishments endogenously, the Ultimatum Game also describes the actual process of facilitating cooperation (or failing to do so) more closely.

Conditional Cooperation

The literature on conditional cooperation, the finding that people "are willing to contribute more to a public good the more others contribute" (Fischbacher et al., 2001, p. 397), is vast. See Chaudhuri (2011) and Thöni and Volk (2018) for reviews. Although it usually applies to Public Goods Games (Croson et al., 2005) and to issues such as charitable giving (Frey and Meier, 2004) or tax moral (Frey and Torgler, 2007), we believe it helps to predict behavior or explain failed cooperation in our study, especially because we implement a repeated interaction design, where incomplete conditional cooperation can be the driving force of declining cooperation (Neugebauer et al., 2009).[6]

[6]The literature has also discussed reasons for the behavioral pattern of conditional cooperation. See the review by Frey and Torgler (2007), who point to reciprocity and fairness (see, e.g., Falk and Fischbacher 2006) and conformity to social norms (see, e.g., Henrich 2004; Fehr and Fischbacher 2004). Cubitt et al. (2017) link conditional cooperation to an individuals' betrayal aversion, which is the "greater reluctance of people to take social risks associated with trusting another person, compared to a benchmark of corresponding natural risks" (Cubitt et al., 2017, p. 111).

3 Model

We base this study on a modified stochastic Ultimatum Game and make two changes with respect to the standard Ultimatum Game: First, the amount of money players can distribute among each other (the pie) is not fixed but drawn randomly from a uniform distribution. Second, we extend the game by an additional stage, in which the responder can make a money transfer to the proposer.

Thus, the game follows this sequence:

1. The proposer chooses a claim $x \in [0;1]$ she wants to keep from the pie $\pi \in [0;1]$ but without knowledge of the eventual size of the pie. (x and π are on normalized intervals.)

2. The size of the pie is determined randomly from a uniform distribution, yielding the rest $y = \pi - x$.

3. Now, the responder is informed about π and x and is offered y, that the responder accepts or declines.

4. If the responder declines, both profits are zero: $\pi_{\text{Proposer}} = \pi_{\text{Responder}} = 0$. If the responder accepts, the proposer's profit is $\pi_{\text{Proposer}} = x$ and the responder's profit is $\pi_{\text{Responder}} = \pi - x = y$.

5. If the responder accepts and the transfer stage is included in the game, then the responder also has the option to make a transfer $z \in [0; y]$ to the proposer, so that $\pi_{\text{Proposer}} = x + z$ and $\pi_{\text{Responder}} = y - z$.

The proposer faces a trade-off between increasing her expected profit by choosing a large x and increasing the probability of acceptance $P(\text{acceptance})$ by choosing a small x, since the probability of acceptance is $P(\text{acceptance}) = 1 - x$ if we assume that the responder accepts every offer $y > 0$. The proposer's profit is zero if the responder declines the offer. Thus, in expectation, the risk-neutral payoff-maximizing proposer earns

$$\mathbb{E}\left[\pi_{\text{Proposer}}\right] = x \int_x^1 d\pi = x[\pi + C]_x^1 = x(1-x) \tag{1}$$

which yields an optimal claim of $x^* = 0.5$ and an expected profit of $\mathbb{E}\left[\pi_{\text{Proposer}}\right] = 0.25$.

In expectation, the risk-neutral payoff-maximizing responder earns

$$\mathbb{E}\left[\pi_{\text{Responder}}\right] = \int_x^1 (\pi - x) d\pi = [0.5\pi^2 - x\pi + C]_x^1 = 0.5 - x + 0.5x^2 \tag{2}$$

which yields an expected profit of $\mathbb{E}\left[\pi_{\text{Responder}}\right] = 0.125$ for $x^* = 0.5$. In expectation, both players earn the same if $x = \frac{1}{3}$ which yields $\mathbb{E}\left[\pi_{\text{Proposer}}\right] = \mathbb{E}\left[\pi_{\text{Responder}}\right] = \frac{2}{9} \approx 0.22$. Figure 1 illustrates the expected profit functions.

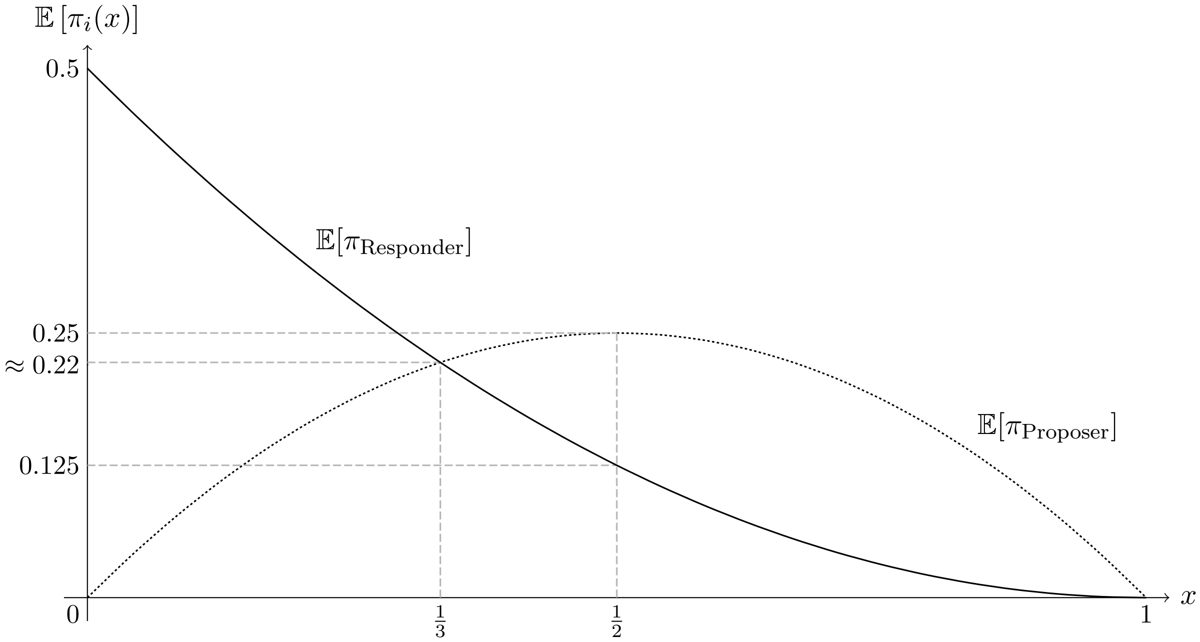


Figure 1: Expected Profits as Functions of the claim

4 Experimental Design and Procedures

The experimental design is based on a finitely repeated version of the modified stochastic Ultimatum Game described in the previous section.[7] The between-subject treatment-difference is whether the transfer stage is included or not. In the Base treatment, it was not and the participants played the basic stochastic Ultimatum Game. In the Transfer treatment, the transfer stage was included.

At the beginning of a session, we randomly assigned all participants to either the role of proposer or responder, which they kept throughout the experiment.[8] In the main part of the experiment, we randomly matched each participant to a participant of the other role with whom they interacted repeatedly for ten periods (i.e., the first phase). They were then rematched using perfect stranger-matching and interacted for another ten periods (i.e., the second phase). We matched participants into groups of four to keep as many independent observations as possible.

One period represents one repetition of the game. First, the proposer states a claim $\in$ [0 euro; 20 euro] (in 10-cent increments), ignorant of the eventual size of the pie but knowing the interval of the pie: [0 euro; 20 euro] and its discrete uniform distribution (again, in 10-cent increments). Then the responder is informed about the randomly drawn size of the pie $\in$ [0 euro; 20 euro] and the remaining rest = pie $-$ claim. The responder can then accept or reject the proposal.[9] If the responder accepts the proposal, the proposer's profit in this period is the claim and the responders's profit in this period is the rest. In the Transfer treatment, if the responder accepted a positive proposal, she can make a transfer $\in$ [0 euro; rest euro] (in

[7]From now on, in the paper as in the experiment, we use descriptive names for the variables and call x the claim, y the rest, π the pie, and z the transfer.

[8]In the experiment, we called the proposer Person A, and the responder Person B.

[9]The responder is allowed to accept a negative proposal, but is prompted to confirm this decision.

10-cent increments) to the proposer (thus, the proposer's profit is the claim plus the transfer, and the responder's profit is the rest minus the transfer).

We determined 19 sequences of pie-sizes randomly ahead of the sessions. One matching group in each of the two treatment went through the same sequence of pie-sizes. This ensures that the pie-sizes do not vary between the treatments.

At the end of each period, participants received feedback on their decision(s), their matching-partner's decision(s), the pie-size in this period, and both profits. At the end of each phase, they received the full history of the respective phase. At the end of the first phase, the history was accompanied by a question that asked participants to explain their strategy in a text box (we intended to increase between-supergames learning effects by letting the participants reflect what they did in the first phase). There was no time limit to write down the answer. However, the participants could not leave this stage before at least two minutes had passed.

After the main part of the experiment, a short second part followed (the participants were made aware of this part but not of its content). For this part, participants were rematched (stranger-matching) again, according to their roles in the main part. To test whether the participants in the role of proposers have the ability to look at the stochastic Ultimatum Game from their counterpart's perspective (which could be important for the ability to initialize cooperation), we designed a simple 2×2 one-shot game (the "Empathy Game", see Table 1). The Empathy Game requires the proposer to recognize the responder's dominant strategy in order to maximize her payoff. "Left" is the dominant strategy for the responder. If the proposer recognizes this, she should choose "Up." However, the higher payoff of 5 in (Down, Right) could tempt her. If the proposer chooses "Down," she does not recognize the responder's dominant strategy since her payoff from (Down, Left) is substantially smaller than her payoff from (Up, Left). In the experiment, the game was presented as a 4×4 matrix (see Table A.1 in the Appendix).

Table 1: The Normal Form of the Empathy Game

		Responder	
		Left	Right
Proposer	Up	4 euro, 2 euro	1 euro, 1 euro
	Down	2 euro, 3 euro	5 euro, 2 euro

Before the participants received their eventual payoff information, they were asked to fill in a questionnaire that included questions on four variables to control for in our analyses: gender, whether a participant knows game theory, whether a participant knows people who have previously taken part in this experiment, and the number of other participants in the session whom the participant knows personally.[10]

All profits were expressed in euro throughout the experiment. Each participant's final payoff was determined by one randomly chosen period from each of the two phases of the main part of the experiment (which periods was only revealed at the very end of the experiment)[11] plus the profit in the Empathy Game plus a show-up fee of 5 euro. We ensured that a participant's final payoff could not fall below zero (which was only possible by accepting a negative proposal in the role of the responder, these negative proposals would have been deducted from the other profits and the show-up fee).

At the beginning of a session, all participants received the same set of detailed on-screen instructions covering the main part of the experiment. We also informed them that there would be a second part and that neither the second part nor the main part would have any influence on the other part's profits. Before starting the experiment, all participants had to

[10]In Table A.2 in the Appendix, we supply summary statistics of the control variables and compare them across treatments. Only the share of participants who know game theory is higher in Base than in Transfer (significant at the 10%-level).

[11]We used this mechanism instead of paying every period to make cooperation by alternating ("I take a lot in this period, then you get a lot in the next and so on.") impractical; see Prisbrey (1992) and Duffy et al. (2017) for experimental evidence on alternation. See Charness et al. (2016) for a discussion of paying one period or all.

answer control questions correctly to ensure comprehension.[12] Participants were prompted with an additional explanation if they answered a question incorrectly.

Except for one pretest of the Transfer treatment with eight participants on June 4, 2019, we conducted all sessions between June 19, 2019, and July 4, 2019, in the Potsdam Laboratory for Economic Experiments (`https://www.uni-potsdam.de/plex/public/`). We recruited 152 participants, 76 in each of the two treatments, from an existing subject pool based on ORSEE (Greiner, 2015).[13] This subject pool exclusively consists of students of different disciplines at the University of Potsdam, FU Berlin, Film University Babelsberg, and the University of Applied Sciences Potsdam. We programmed the experiment in z-Tree (Fischbacher, 2007). At the beginning of each session, we asked participants to sign an informed consent form. Sessions lasted up to 90 minutes (depending on the number of participants) including welcome and payoff procedures. Participants earned 16.11 euro on average ($\approx$ 18.16 US-dollars at the time of the experiment), with a minimum of 6 euro and a maximum of 32.30 euro (SD = 5.61 euro). We paid the participants privately in cash immediately after the experiment. Each participant took part in one session only.

5 Hypotheses

We want to know whether the institution of voluntary transfers increases cooperation, and thus increases efficiency. To achieve cooperation, both players need to coordinate their behavior. We explain the three conditions that form the coordination mechanism in Transfer, starting with the responder:

1. The responder's first condition, the sufficient intra-period condition for acceptance, is

[12] Experimental instructions and quiz questions can be found in Section A.3 in the Appendix.

[13] These numbers were previously determined in a power calculation which can be found in the preregistration.

that she makes a positive profit. In expectation, the proposer can meet this condition by choosing a claim that is smaller than the expected size of the pie, i.e., 50% or 10 euro in our design. By lowering the claim, the proposer can increase expected acceptance. With certainty, the proposer can meet the condition by choosing to claim nothing, i.e., 0 euro.

2. The responder's second condition, the necessary intra-period condition for paying a transfer to the proposer, is that she earns more than the proposer.[14] In expectation, the proposer needs to claim less than 25% or 5 euro to meet this condition, or 0 euro to meet it with certainty.

3. The proposer's inter-period condition for cooperation is that she expects to receive a sufficiently large transfer if she meets the responder's second condition. (In the first period, the proposer cannot condition her expectation on responder's past behavior.)

If the players cooperate by coordinating their behavior in this way, they increase overall efficiency by increasing the acceptance rate (which means that fewer proposals are rejected and thus fewer pies are lost).

Hence, our main, primary hypothesis is:

H_1*: Compared to* Base*, the average total profit in* Transfer *is higher. This is equivalent to an increase in efficiency.*

We have two secondary hypotheses that form the mechanism for H_1:

H_2*: Compared to* Base*, the average claim in* Transfer *is smaller.*

H_3*: Compared to* Base*, the average acceptance rate in* Transfer *is higher.*

[14]We assume that she would not reduce her own profit to increase disadvantageous inequality without an efficiency increase (see Fehr and Schmidt 1999).

We are also interested in the distribution of profits between proposers and responders. We analyze this difference both unconditional and conditioned on acceptance (as a high rejection rate could lead to equal but low average profits). Given that the responder accepted a proposal, the Transfer treatment could lead to a more equal distribution if the transfer is used to split the pie equally. Thus, we test our first exploratory[15] hypothesis:

E_1*: Given acceptance, profits are more equally distributed in* Transfer*.*

We also want to consider gender differences in the willingness to facilitate cooperation. Gender differences in risk aversion (see Croson and Gneezy 2009) could translate into different attitudes towards the risk of choosing a claim that is larger than the pie. Gender differences in the willingness to volunteer for low-promotability tasks (reported by Babcock et al. 2017) could result directly in choosing different claims, as choosing a smaller claim increases the probability of successful cooperation but decreases the proposer's profit. We would interpret this as volunteering to cooperate. Hence, our second exploratory hypothesis is:

E_2*: On average, invariant of the treatment, women's claims are smaller than men's.*

6 Results

In this section, we present our main results and conduct further analysis of our experimental data. We base the analysis only on data from the second phase of the experiment because we expected learning effects after rematching and tried to foster these effects by asking participants about their strategy after they had completed the first phase.[16] These restrictions were included in the preregistration of our study.

[15] We regard E_1 and E_2 as exploratory hypotheses because we have no clear background in theory that directly implies these hypotheses and because we based the power-calculation on H_1.

[16] Such learning effects seem to have taken place early in the first phase and not between supergames (compare Figure A.1 in the Appendix which shows average claims in the two treatments over time).

6.1 Main Results

We base our main results on treatment comparisons of matching-group averages. Table 2 provides summary statistics of our main outcome variables, effect sizes between the treatments (measured by Cohen's d)[17], as well as tests of statistical significance of the difference of the variables between treatments. Figure A.3 in the Appendix displays all individual decisions over the course of the second period.

Table 2: Summary Statistics of Main Variables in the Second Phase

Variable		Base	Transfer	Cohen's d	Difference
H_1:	Total Profit	€ 7.85 (1.73)	€ 8.14 (2.39)	0.14	$p = 0.2895$
	Proposer's Profit	€ 3.83 (0.96)	€ 3.97 (1.19)	0.13	$p = 0.3202$
	Responder's Profit	€ 4.02 (1.17)	€ 4.17 (1.45)	0.12	$p = 0.3852$
H_2:	claim	€ 6.73 (1.28)	€ 5.09 (2.10)	0.94	$p = 0.0028$
	transfer	-	€ 1.17 (1.08)	-	-
H_3:	Acceptance Rate	0.63 (0.11)	0.69 (0.21)	0.38	$p = 0.0815$

Matching group averages are treated as statistically independent observations. Data from 730 periods in the second phase played by 152 participants in 38 matching groups ($N = 38$, 19 per treatment). Standard deviations in parentheses. p-values based on one-sided Wilcoxon rank-sum tests with continuity correction.

We reject our main hypothesis H_1. There is no substantial or statistically significant treatment effect neither on the average total profit nor on the average of the individual profits. However, there is a large ($d = 0.94$)[18] and statistically significant ($p = 0.0028$) treatment effect on the average claim (H_2) and a small effect on the average acceptance rate (H_3, $p = 0.0815$). In Transfer, proposers claim on average 1.64 euro less than in Base, and responders accept

[17] Cohen (1988, p. 25) and Cohen (1992) give a rule of thumb for assessing the value of d: $|d| < 0.2$ is a negligible effect, $|d| < 0.5$ is a small effect, $|d| < 0.8$ is a medium effect, and $|d| \geq 0.8$ is a large effect.

[18] Consider that Cohen's d was calculated based on matching group averages. Because we take the average of averages, the standard deviation is naturally lower, which somewhat artificially inflates the effect size.

about 6 percentage points more offers.[19] But this does not translate into an overall effect. The availability of voluntary transfers does not increase overall efficiency.

In our hypothesis E_1, we stated our expectation that the transfer allows the two players to reduce inequality between them. We test this with the help of a random-effects panel regression[20] where we regress the absolute difference of the players' profits on a treatment-dummy and a constant (just comparing the means of the profits in Table 2 is not sufficient as we cannot see the inequality in the profit division within a period). Table 3 shows the results. Model (1) uses all observations from the second phase, model (2) further conditions on acceptance. Both models confirm our hypothesis: the transfer is used to reduce inequality by about 1.05 to 1.77 euro (between 10% and 18% of the expected pie-size).

To check the robustness of our findings regarding H_2, to test hypothesis E_2, and for some additional tests, we conduct random-effects panel regressions where we regress the claims (expressed as percentages of the maximum pie-size) in different specifications on a treatment-dummy, control variables, lagged outcomes, and a constant. Table 4 shows the results of these regressions, Table A.3 in the Appendix shows the results from the same regressions as in Table 4 with additional controls from the post-experimental questionnaire (knowledge of game theory, whether a participant knows other people who have previously participated in this experiment and the number of other people in this session whom the participant knows personally). In model (1) in Table 4, we can see that the claims in Transfer are about 8 percentage points lower than in Base (this is confirmed in Table A.3 in the Appendix, where

[19]Proposers in Base claim 6.73 euro (or 33.7% of the maximum pie-size) on average, which is close to the point where proposers' and responders' expected profits are equal (see Figure 1). This extends a standard result of the Ultimatum Game to its stochastic case.

[20]Because we use time-constant covariates in most of the models (such as the treatment-dummy), we can neither estimate fixed-effects nor correlated-random-effects models. Random-effects models can be used if the group-specific, time-constant error is uncorrelated with the covariates. This is probably the case for the randomly assigned treatment-dummy, but not necessarily for, e.g., the gender-dummy if we think of, for example, baseline fairness preferences as part of the group-specific error. All random-effects regressions were conducted using the `plm`-package for R (Croissant and Millo, 2008). Tables were exported using the `texreg`-package for R (Leifeld, 2013).

Table 3: Panel Regression: Profit Distribution

| Proposer's profit − responder's profit |	(1)	(2)
Transfer	-1.054^{***}	-1.771^{***}
	(0.302)	(0.368)
Intercept	2.722^{***}	4.218^{***}
	(0.213)	(0.260)
Restrictions	A	A, B
Observations	760	500
Number of groups	76	76
Obs per group	10	6.6
Within R^2	-	-
Between R^2	0.142	0.238

Random-effects panel regressions, treating pairs as groups, with the absolute value of the difference between the profits of both players as dependent variable. Standard errors in parentheses. ***,**,* indicate significance at the 1%, 5%, 10% levels. Obs per group in (2) is the average number of observations. Restrictions on included observations: (A) only second phase (preregistered), (B) offer was accepted. The effects in these regressions cannot be attributed to just one of the two players, which is why we did not estimate these regressions with controls for the individual participants' characteristics.

we include additional controls.).

We test hypothesis E_2 with the help of model (2) in Table 4 and use all data from both phases in this regression, as we assume no interaction between learning effects and possible baseline gender effects. Do women claim less than men? We do not find the expected gender effect. But we have to be cautious with this result: If we consider the extended model (2) in Table A.3 in the Appendix, we find that the female-coefficient and the female-Transfer interaction term have a statistically significant effect: On average, women in Base claim 6.6 percentage points less ($p < 0.05$). However, in Transfer this effect is nullified by the interaction's effect of 7.9 percentage points more ($p < 0.1$). We consider our findings regarding a gender effect on claims as inconclusive.

In our experimental design, we additionally implemented the "Empathy Game" (see Table 1) to test whether a proposer's ability to change perspective can explain her behavior. The results are striking: while 81.6% of responders in Transfer chose their dominant strategy (indicating that the game was well understood by responders), only 26.3% of proposers chose the strategy that would have been the best answer to the responder's dominant strategy. This means that most proposers in TRANSFER were unable to play optimally and thus, we believe, were unable to follow a simple change of perspective. Contrary to our expectation, a "Down"-dummy for this inability to change perspective cannot explain the proposer's behavior (see model (3) in Table 4). The coefficient is small and not statistically significant: the Empathy Game cannot identify different groups of proposers.

Table 4: Panel Regression: Proposer's Decision

claim in % of max. pie-size	(1)	(2)	(3)	(4)	(5)
TRANSFER	-8.213^{***}	-9.526^{***}			
	(2.489)	(2.998)			
Female		-4.678			
		(3.045)			
TRANSFER $\times$ Female		6.429			
		(4.297)			
Strategy "Down"			-2.950		
			(4.593)		
Accept_{t-1}				0.503	
				(1.801)	
Transfer_{t-1}				-1.993^{***}	
				(0.434)	
Proposer's $\text{share}_{t-1} < 50\%$					10.369^{***}
					(2.325)
Intercept	33.645^{***}	35.842^{***}	27.605^{***}	27.741^{***}	16.379^{***}
	(1.760)	(2.036)	(3.943)	(1.681)	(2.363)
Restrictions	A		A, C	A, C, D	A, C, D, E
Number of observations	760	1520	380	342	168
Number of groups	76	76	38	38	35
Obs per group	10	20	10	9	4.8
Within R^2	–	–	–	0.009	0.044
Between R^2	0.128	0.142	0.011	0.747	0.424

Random-effects panel regressions, treating pairs as groups, with the proposer's claim in percent of 20 euro as dependent variable. Standard errors in parentheses. ***,**,* indicate significance at the 1%, 5%, 10% levels. Female refers to the proposer's gender. Obs per group (4) is the average number of observations. Restrictions on included observations: (A) only second phase (preregistered), (C) only TRANSFER, (D) first period excluded, (E) claim in $t-1$ was smaller than or equal to half of the pie in $t-1$.

6.2 Exploratory Analysis

To summarize: As we had expected, average claims in Transfer are lower than in Base and the average acceptance rate is higher. However, these effects are too small to cause a substantial efficiency gain. In this section, we analyze *why* our proposed mechanism failed.

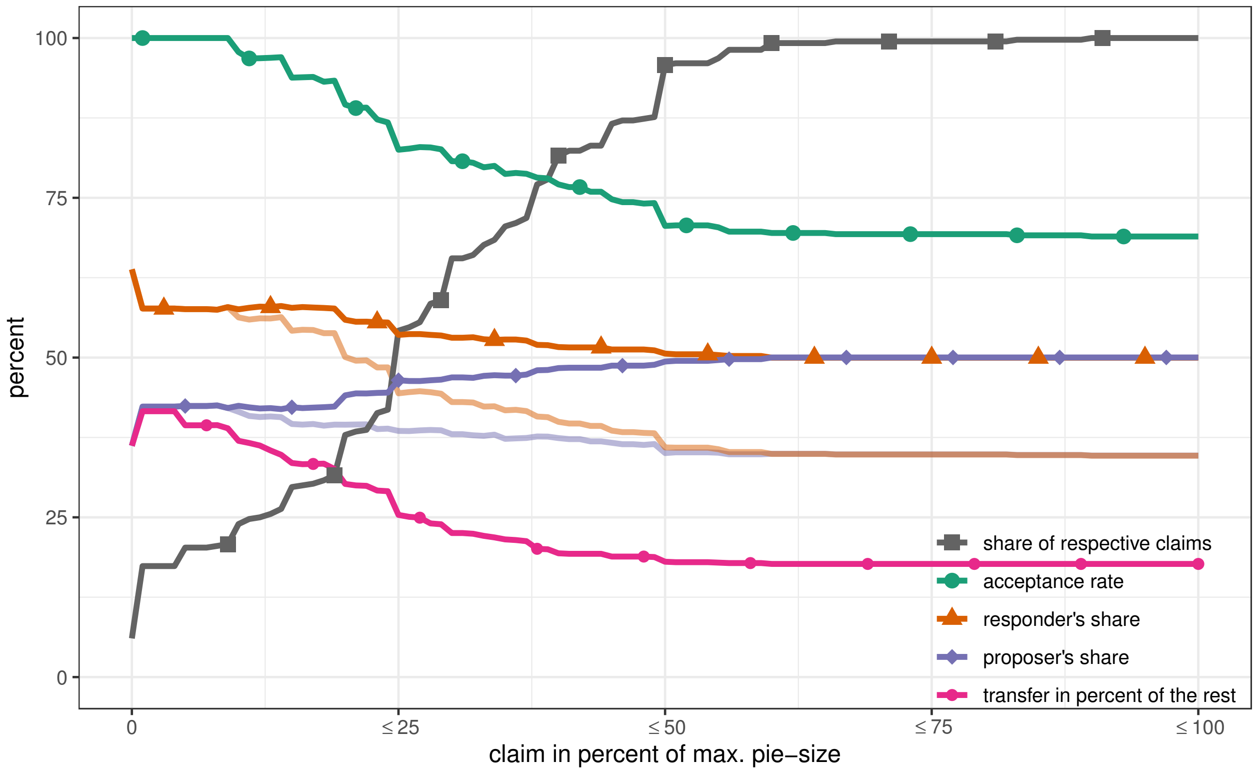
Strategic Uncertainty

The proposer's problem in Base is characterized by the following trade-off between the expected profit and the probability of acceptance: Claiming more has a direct and positive effect on the expected profit through the claim and an indirect and negative effect through the probability of acceptance, see Equation 1 and Figure 1. In Transfer, the proposer can now avoid this trade-off, given that she assumes that the responder will use the transfer to divide profits equally. She can both maximize the acceptance rate (and thus, efficiency) to 100% and her expected profit to 50% of the pie by claiming nothing.[21] However, this is a very restrictive definition of cooperation, since it leaves the proposer with no profit in case the responder does not make a transfer. A less restrictive definition would include claims that were made with the clear intention of receiving *less* than the responder. This would meet the responder's necessary condition for paying a transfer. Thus, we will also define an attempt to initialize cooperation as claiming ≤ 5 euro (or $\leq 25\%$ of the maximum pie-size) as this leads to proposer's profit $<$ responder's profit if the expected value of the pie is realized.

In order to study the proposers' claims and their consequences on acceptance and profit division in Transfer, we show the cumulative averages of key variables conditioned on the claim-size relative to the maximum pie-size (thus, taking an ex-ante perspective) in Figure 2. These cumulative averages give the average of a variable on the y-axis for *all* observations where the

[21] In fact, all claims $< 10\%$ are accepted (all rests in this range were positive). The acceptance rate for non-negative rests offered to the responders is 93.4% in Base and 94.9% in Transfer. Thus, rejecting positive rests to (costly) punish or educate proposers does not seem to play an important role.

Figure 2: Cumulative Averages in TRANSFER



Averages (y-axis) of selected variables of all observations that were smaller than the respective claim (x-axis) in percent of the max. possible pie-size in the second phase. Responder's share and proposer's share are conditional on accepted offers, the respective pale lines without markers are unconditional.

claim is *smaller than or equal* to the respective relative claim on the x-axis.[22] First of all, we observe that the overwhelming majority of claims is smaller than the point prediction of 50% (95.8% of claims are 50% or less), but only 6.1% of claims are 0% and 54.2% of claims are 25% or less.[23] Thus, by the widest definition of cooperation by the proposer, only about half make a cooperative offer.

[22]For example: The average acceptance rate for claims that are smaller than or equal to 25% of the max. possible pie-size is 82.5%. The grey line is the cumulative distribution function (CDF) for the respective observations. We also show the acceptance rate for context, as well as average profits for both players and the average transfer for a discussion of the profit distribution. In Figure A.2 in the Appendix, we also show the cumulative averages for BASE.

[23]As behavior in the first period could be used to show good intentions for the remaining second phase (there is no previous experience with the participant after rematching), we report the claims separately. In the first round of the second phase, 52.6% of proposers chose to claim 25% or less, and 7.9% chose to claim 0%. First-period claims are not different from the claims in the rest of the phase.

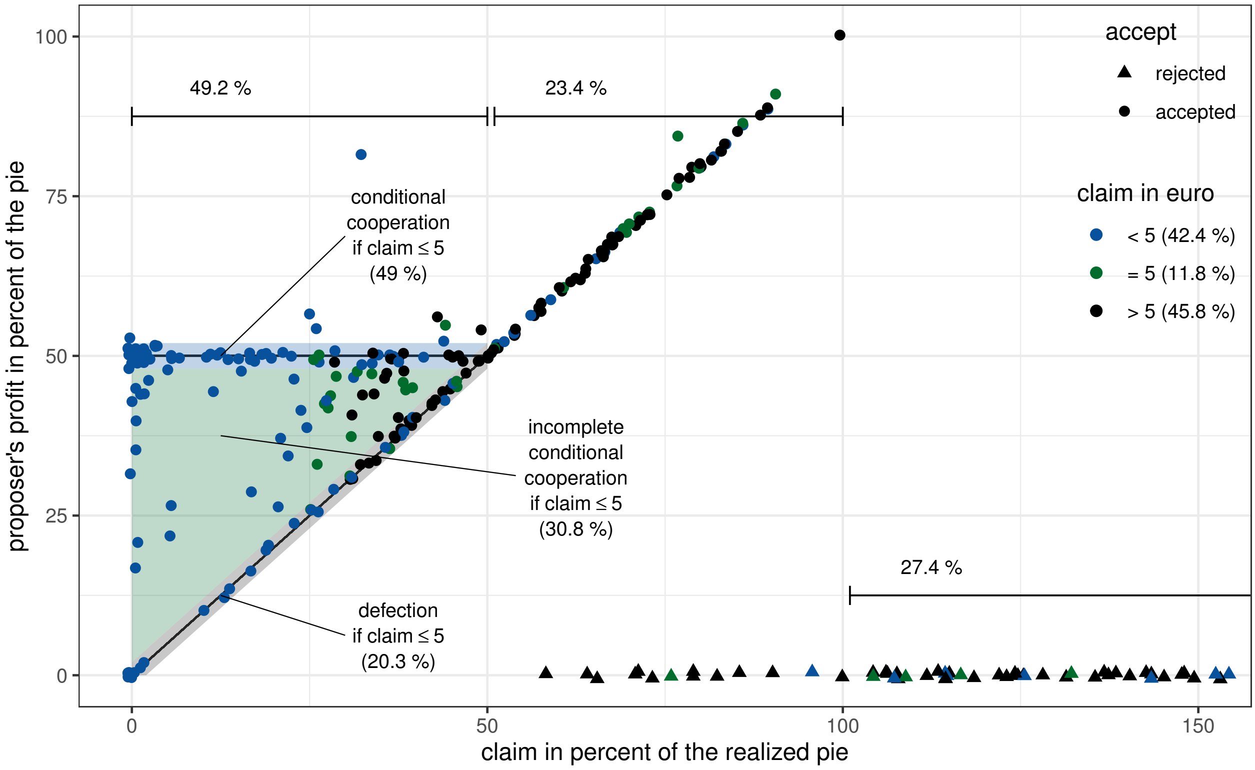
We suggest that the main reason for the reluctance to initialize cooperation is the high level of strategic uncertainty that the proposer is subject to. By reducing her claim, she can minimize the stochastic uncertainty but only at the risk of depending on the responder's behavior, and thus increasing the strategic uncertainty. Compare this to the findings in related experiments: In standard Ultimatum Game experiments, the proposer can claim 50% and keep it with near certainty, in standard Public Goods Game experiments, participants can free-ride and keep at least their endowment with certainty. This is not possible in the stochastic Ultimatum Game: the stochastic uncertainty amplifies the strategic uncertainty since it induces this trade-off.

This explanation is consistent with the past-dependent behavior we observe. In two models in Table 4, we regress the claim on responders' decisions in $t-1$: In model (4), we can see that proposers lowered their claim significantly if they received a higher transfer in the previous period. Learning that a responder cooperated in the last period reduces strategic uncertainty, and, thus, leads to a higher willingness to cooperate by the proposer. On the other hand, if their share of the pie was strictly smaller than 50% given that they made a cooperative offer, they increase their claim on average by 10 percentage points (see model (5) where we regress the claim on a dummy-variable and a constant). The proposers learn and adjust their behavior by claiming more in order to have a buffer (which lowers, as seen in Figure 2, the acceptance rate). These effects make stable and efficient cooperation very difficult to maintain.

Incomplete Conditional Cooperation

Cooperative behavior by the responder is defined by how much of the amount that the responder can divide between herself and the proposer, the rest, is transferred, given that the proposer made a cooperative offer. In Figure 2, we see that the responders on average send back 36.2% if the proposer claimed nothing, which then jumps to 41.6% when the proposers claimed less than 5%, and then gradually transfer less and less (25.4% of the rest are transferred if 25% or less were claimed). This observation indicates that, on average, responders

Figure 3: Anatomy of Failure



Summary graph with raw data from the second phase in TRANSFER. Data points are jittered randomly with a maximum displacement of 0.6 units on both axes. The x-axis is cut off at 150% to improve readability but the cut off data points are included in the percentages.

cooperate only incompletely conditionally cooperative, because they transfer substantially less than the amount that would be necessary to even out profits. This results in an unequal division of the pie between the two parties, as can be seen in the unequal shares of the pie (conditioned on acceptance of the pie as bold lines with markers, unconditional as pale lines without markers).[24]

Cooperation and Defection by both Players

We are now at a point where we can classify the data into categories of cooperation and defection by both proposers and responders, making it possible to quantify the weight of the

[24]Still, claiming almost nothing is profit-maximizing for the proposer because higher claims lead to a higher rejection rate.

respective reasons for the failure discussed above. Figure 3 displays the proposer share on the claim-size, both relative to the *realized* pie. Thus, we take an ex-post perspective (in contrast to the previous figures where we conditioned on the maximum pie-size).

We can distinguish three different categories: (i) the proposers earn as much as they have claimed (the points on the 45-degree line), (ii) they earn nothing if the claim has been rejected (the triangles on the x-axis), (iii) or they earn more than they have claimed if the transfer is used (the points above the 45-degree line).

The criterion for the proposer's behavior is the absolute size of her claim (see color coding of the data points and data points lying on the y-axis). We consider a claim of $\leq$ 5 euro (25% of the max. possible pie-size) as cooperative (54.2% of claims), and a claim above 5 euro as defective (45.8% of claims).

The criterion for the responder's cooperation behavior is the transfer, given that the proposer made a cooperative offer and the claim was smaller than 50% of the realized pie. We consider a transfer of zero as defective (gray shading on the 45-degree line, 20.3%). A non-zero transfer is incompletely conditional cooperative if it is not used to completely even out profits (green shading, 30.8%); and, finally, conditionally cooperative if the transfer leads to an equal profit split (blue shading on the horizontal line, 49%).

Which party can we blame for the failure of the transfer mechanism? On the one hand, about half of the proposers claim too much, resulting in the rejection of many pies. On the other hand, about half of the responders only behaved incompletely conditionally cooperative or defected fully. Thus, their behavior validates the proposers' hesitance.

7 Conclusions

In this paper, we examined cooperation under exogenous stochastic uncertainty. We adapted the Ultimatum Game to a stochastic version, where the pie-size was determined randomly and conducted an experimental comparison of the repeated game with and without voluntary ex-post transfers. Our main result is that, on average, participants failed to coordinate their behavior in an efficient and profit-maximizing manner.

Previous research has shown that voluntary transfers can increase efficiency in different games without stochastic uncertainty, such as a best-shot Public Goods Game (Bruttel and Güth, 2018), a linear Public Goods Game (Blanco et al., 2020) and a Battle of the Sexes Game (Chatziathanasiou et al., 2020). Our contribution to this literature is that we show that this result seems to be highly dependent on the environment: With uncertain surroundings, the availability of voluntary transfers does not increase efficiency.

We further show that the cause of this failure can be attributed to both sides—proposer and responder—via two distinct mechanisms. First, the transfer option can only unfold its cooperation-enhancing effect if the proposer is willing to trust that the responder will react with a fair transfer when the proposer demanded a small share of the original pie. The stochastic uncertainty in our setup increases this strategic uncertainty faced by the proposer because the size of the strategic incentives of the responder is unknown to the proposer. Only when stochastic uncertainty has been reduced by the responder's cooperative behavior in past rounds, we observe that proposers are willing to make cooperative offers in the future. This is consistent with findings in the Public Goods literature, where Wit and Wilke (1998) and Gangadharan and Nemes (2009) report that stochastic uncertainty decreases contributions to a public good if *strategic* uncertainty is high. Second, responders in our experiment behave on average only incompletely conditionally cooperative, which is consistent with Fehr and Fischbacher (2004) and Neugebauer et al. (2009) who concluded that incomplete conditional cooperation is the main reason for inefficient contributions in Public Goods Games. With our setup, we extend this finding to an asymmetric bargaining situation, which can apply to goods of public and private nature. Our results show that efficient cooperation under stochastic uncertainty is extremely hard to achieve.

The present experiment was designed to mimic real situations where stochastic uncertainty is present and asymmetrically distributed due to sequential decision-making, and to test whether a voluntary transfer option can help to reach an efficient outcome in such situations. Our finding that the transfer option does not have such an effect replicates the apparent real-world difficulties in achieving efficient cooperation under uncertainty, such as the relatively low contributions to the WHO's Contingency Fund for Emergencies. Giving these strong similarities between behavior in the experiment and in the real world outside the laboratory, we believe that our design can be used as a testbed for further studies on the effect of different institutions such as binding agreements, communication, or third-party impartial arbitrators on cooperation in such uncertain environments. We leave this for future research.

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A Appendix

A.1 Additional Tables

Table A.1: The Empathy Game in the Instructions

Decision of Person A	Decision of Person B	Earnings of Person A	Earnings of Person B
X	X	4 euro	2 euro
X	Y	1 euro	1 euro
Y	X	2 euro	3 euro
Y	Y	5 euro	2 euro

Style, in which the Empathy Game (see Table 1) was presented in the experimental instructions. Please see the preregistration for screenshots.

Table A.2: Summary Statistics of Control Variables

Variable	Base	Transfer	Cohen's d	Difference
Female (dummy)	0.47 (0.50)	0.59 (0.49)	0.24	$p = 0.1453$
Knows game theory (dummy)	0.59 (0.49)	0.43 (0.50)	0.32	$p = 0.0525$
Number of known participants	0.43 (0.82)	0.24 (0.56)	0.28	$p = 0.1129$
Knows previous participants (dummy)	0.12 (0.33)	0.12 (0.33)	0.00	$p = 1.0000$

Summary statistics of control variables with treatment-comparison. Standard deviations in parentheses. p-values based on two-sided Wilcoxon rank-sum tests with continuity correction.

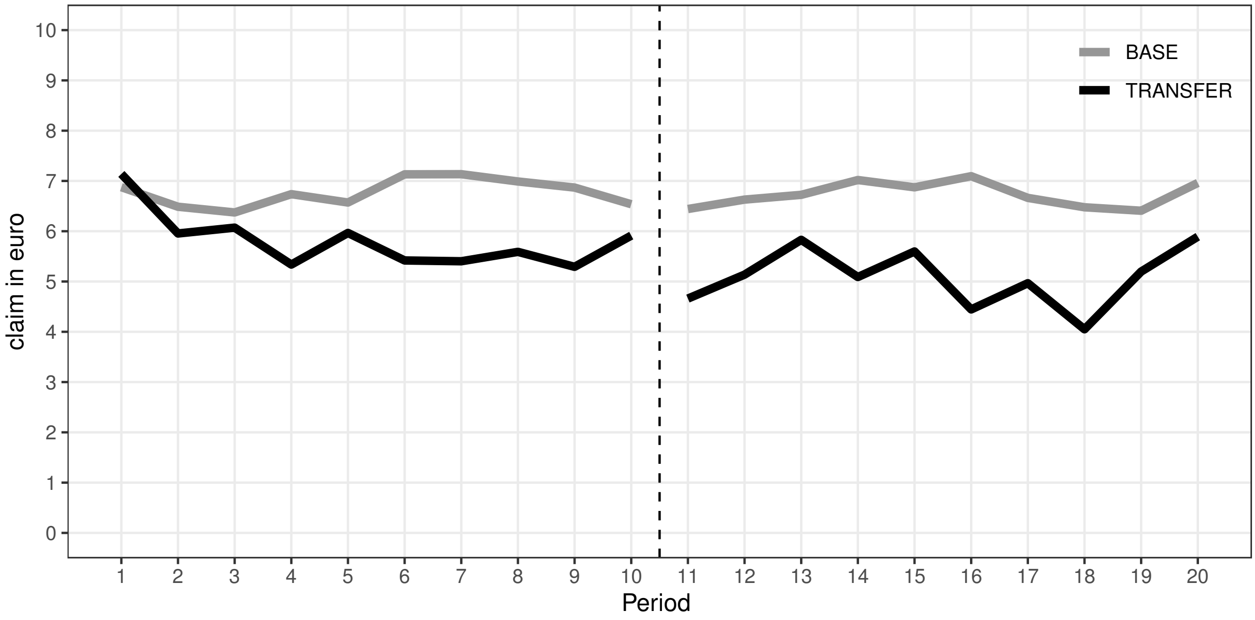
Table A.3: Panel Regression: Proposer's Decision (with additional controls)

claim in % of max. pie-size	(1)	(2)	(3)	(4)	(5)
TRANSFER	-9.245^{***}	-11.936^{***}			
	(2.671)	(3.096)			
Female		-6.550^{**}			
		(3.098)			
TRANSFER × Female		7.864^{*}			
		(4.259)			
Strategy "Down"			-2.978		
			(4.709)		
Accept_{t-1}				0.935	
				(1.806)	
Transfer_{t-1}				-1.904^{***}	
				(0.434)	
Proposer's $\text{share}_{t-1} < 50\%$					11.235^{***}
					(2.333)
Knows game theory	-1.569	-4.113^{*}	-6.654	-5.502^{**}	-3.677
	(2.559)	(2.202)	(4.264)	(2.584)	(3.518)
Number of known participants	-2.471	-3.717^{**}	-3.921	-2.344	2.805
	(2.288)	(1.917)	(8.084)	(4.881)	(6.499)
Knows previous participants	-0.779	-0.382	2.497	2.673	8.697
	(4.441)	(3.707)	(6.932)	(4.136)	(5.602)
Intercept	35.696^{***}	40.763^{***}	30.825^{***}	29.847^{***}	16.413^{***}
	(2.607)	(2.817)	(4.788)	(2.154)	(3.073)
Restrictions	A		A, C	A, C, D	A, C, D, E
Number of observations	760	1520	380	342	168
Number of groups	76	76	38	38	35
Obs per group	10	20	10	9	4.8
Within R^2	–	–	–	0.009	0.044
Between R^2	0.147	0.218	0.081	0.760	0.539

Random-effects panel regressions, treating pairs as groups, with the proposer's claim in percent of 20 euro as dependent variable. Standard errors in parentheses. ***,**,* indicate significance at the 1%, 5%, 10% levels. Female refers to the proposer's gender. Obs per group (4) is the average number of observations. Restrictions on included observations: (A) only second phase (preregistered), (C) only TRANSFER, (D) first period excluded, (E) claim in $t-1$ was smaller than or equal to half of the pie in $t-1$.

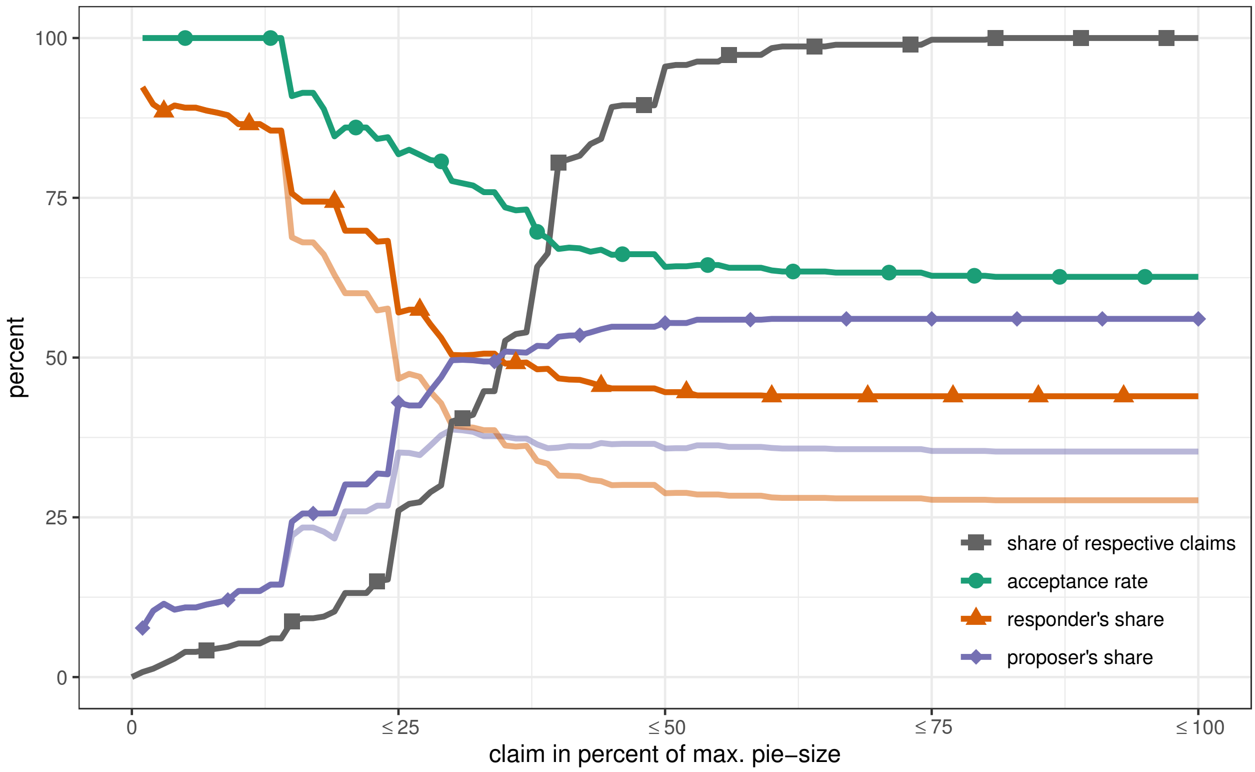
A.2 Additional Figures

Figure A.1: Average claim Over Time



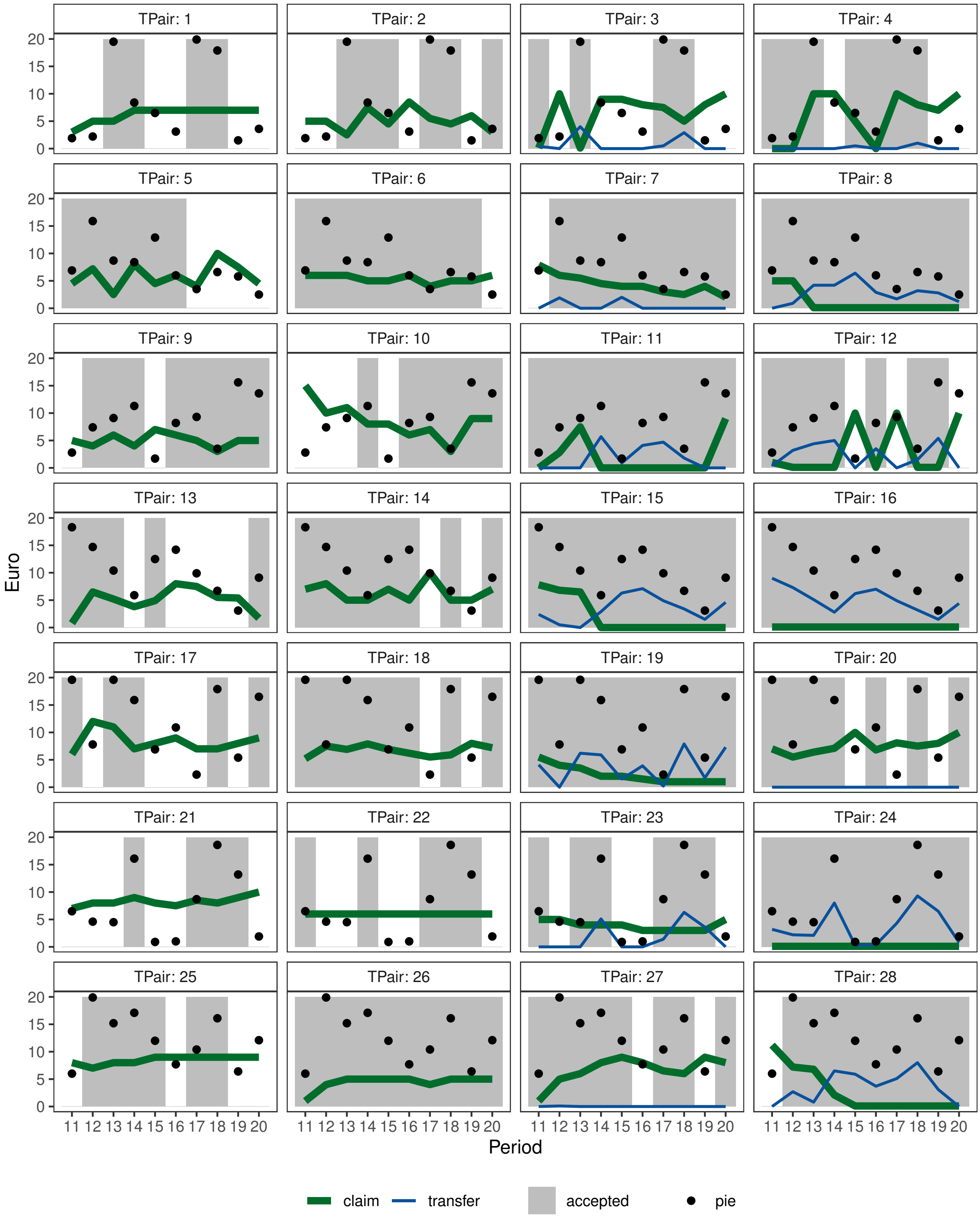
Period averages of the proposers' claims, plotted over time, separated by treatment. Participants were re-matched after the 10th period, i.e., after the first phase.

Figure A.2: Cumulative Averages in Base



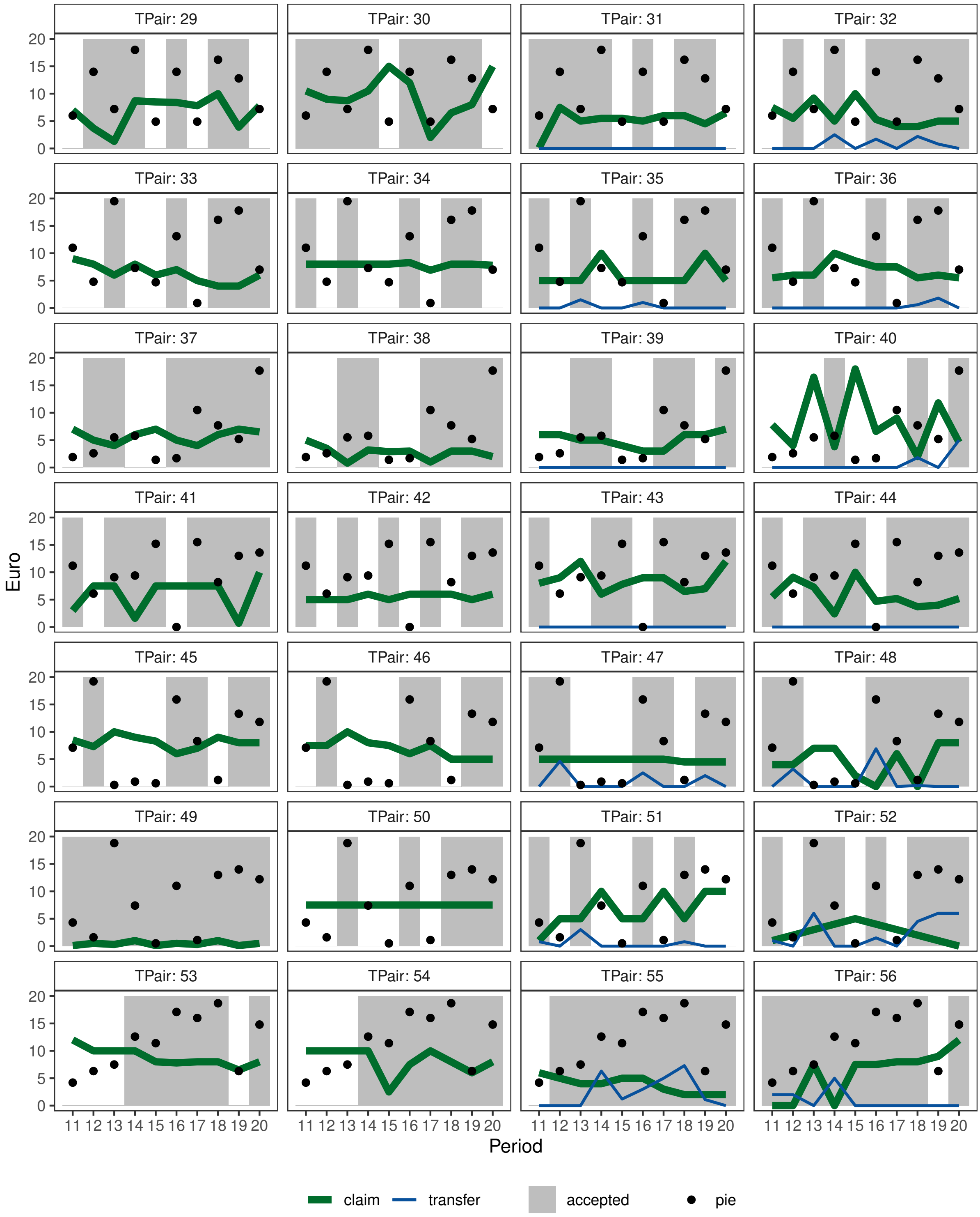
Averages (y-axis) of selected variables of all observations that were smaller than the respective claim (x-axis) in percent of the max. possible pie-size in the second phase. Responder's share and proposer's share are conditional on accepted offers, the respective pale lines without markers are unconditional.

Figure A.3: All Pairs' Individual Decisions in the Second Phase



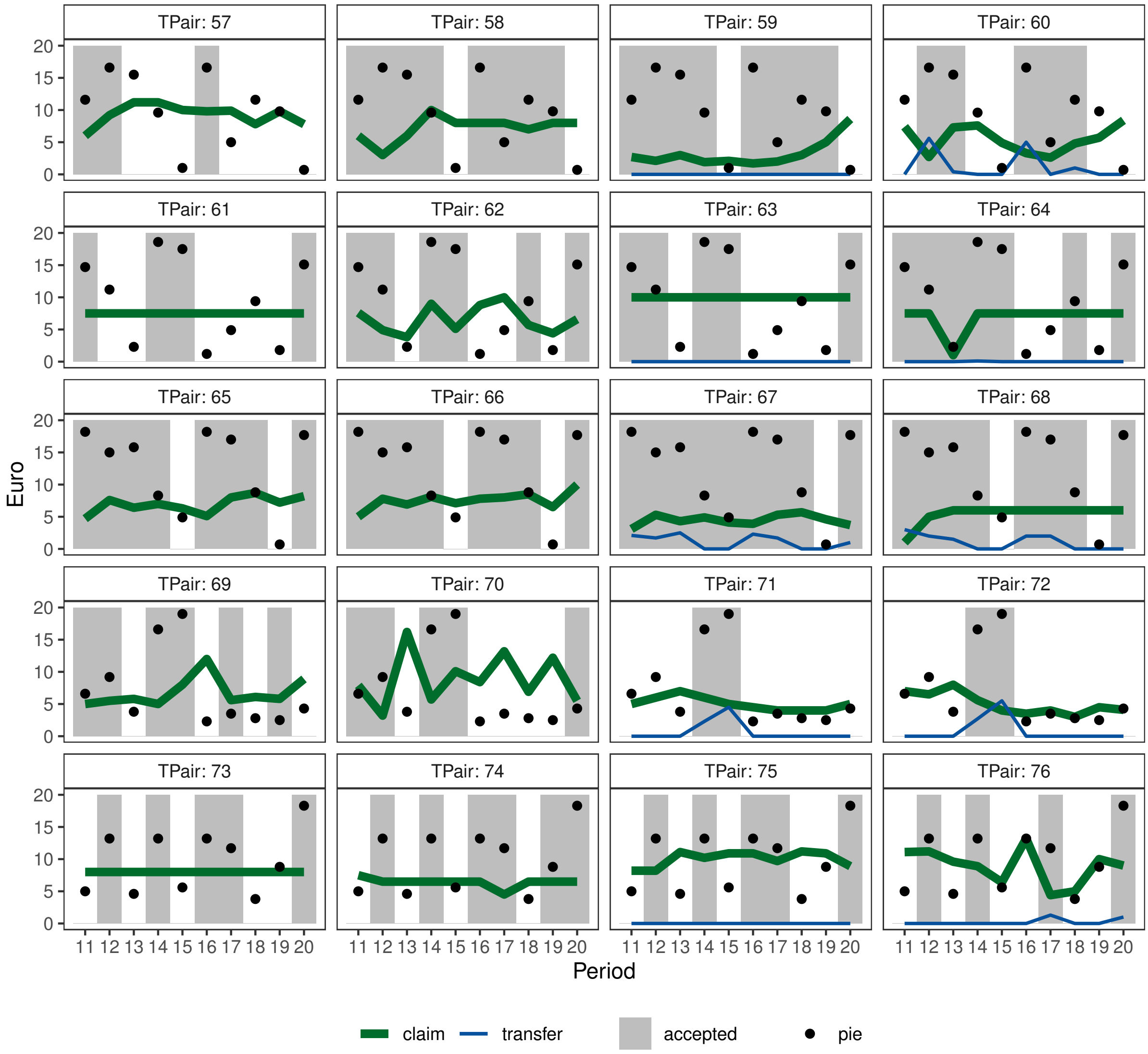
Individual decisions of pairs 1-28 in euro (y-axis), plotted over time (x-axis). Four consecutive pairs face the same pie sizes. Two consecutive pairs are in the same matching group. Pairs in columns 1 and 2 are in the Base treatment.

Figure A.3: All Pairs' Individual Decisions in the Second Phase (continued)



Individual decisions of pairs 29-56 in euro (y-axis), plotted over time (x-axis). Four consecutive pairs face the same pie sizes. Two consecutive pairs are in the same matching group. Pairs in columns 1 and 2 are in the Base treatment.

Figure A.3: All Pairs' Individual Decisions in the Second Phase (continued)



Individual decisions of pairs 57-76 in euro (y-axis), plotted over time (x-axis). Four consecutive pairs face the same pie sizes. Two consecutive pairs are in the same matching group. Pairs in columns 1 and 2 are in the BASE treatment.

A.3 Experimental Instructions

This section provides the translated experimental instructions, that were shown to the participants onscreen. Treatment differences are in *gray italics*. For the original (German) instructions, as well as screenshots and simplified screenshots, please see the preregistration.

General information

Welcome to this experiment. Thank you for your participation! From now on, please do not talk to other participants. Please turn off your mobile devices as well. We will show you the instructions for this experiment on the following screens. Please read the instructions carefully and raise your hand if you have any questions. We will then come to your seat and answer your questions in private. You can take notes at any time. These will be collected after the experiment. Following these instructions, we will test your understanding with a small quiz.

Your payoff

The payoff you can earn in this experiment depends on your decisions and those of other participants. Your earnings will be expressed in Euro during the experiment and paid to you in cash at the end of the experiment. Additionally, all participants will receive an endowment of 5 Euro. If you incur losses as a result of your decisions, these will be deducted from the endowment. At the end of these instructions, you will find more detailed information on how your payoff is determined.

Your role in this experiment

In this experiment you will interact with other participants. At any time, neither you nor the other participants will be informed of with whom they are interacting. Two roles are assigned randomly: The role "A" and the role "B." From now on we call the participants with these roles Person A and Person B. Person A and Person B receive the same instructions. Throughout the entire experiment you are in the role of Person A. [B.]

Experimental procedure

The experiment consists of two parts. The first part consists of two phases. Both phases follow the same structure. Within a phase, you will interact repeatedly with the same participant. In the second phase you will interact with another participant than in the first phase. In both phases, this other participant will be assigned to you randomly. Every phase consists of 10 rounds that all follow the same structure. After the first part there will be a very short second part, which we will explain to you then. Both parts are completely independent of each other and work completely differently. The first part of the experiment has no influence on the second part. The second part has no influence on the first part. Then we would like to ask you to fill out a short questionnaire. In the following we will explain the procedure of the first part of the experiment.

Procedures of one round: Overview

Now we will explain the procedures of one round in more detail. In each round, one Person A interacts with one Person B. Person A and Person B can make different decisions. Person A always makes her or his decision first, then it is Person B's turn.

Procedures of one round: Decision of Person A

First, an amount of money is determined. From now on we call this amount of money the "pie." The size of the pie is a random number between 0 Euro and 20 Euro. The size of the pie is given in increments of 10 cents. At the beginning of a round, Person A decides what amount she wants to take from the pie. This amount is now called the "claim." At this point, Person A does not yet know the size of the pie. The claim can also be between 0 Euro and 20 Euro, and can be determined in 10 cent increments.

Procedures of one round: Size of the pie

The random size of the pie is determined in 10 cent increments. Each of the possible values between 0 Euro and 20 Euro has the same probability of being determined. This means: Suppose one would let the computer randomly determine this pie very often: Then, every possible value (0.00 Euro; 0.10 Euro; 0.20 Euro ... 19.90 Euro; 20.00 Euro) would occur the same number of times.

Procedures of one round: Decision of Person B

Person B is now offered the remaining part of the pie. From now on we call this part the "rest." So rest = pie − claim. Person B is completely informed about the size of the pie as well as the amounts of claim and rest. The rest is positive (greater than zero) if the pie is greater than the claim. The rest is negative (less than zero) if the pie is less than the claim. Person B now decides whether she wants to accept this offer. If Person B accepts a negative offer, then Person B's earnings in this round are also negative. If Person B rejects the offer, then nobody gets anything in this round and the pie expires. If Person B accepts the offer, then Person A gets the claim in this round and Person B gets the rest. *If the rest is positive, then Person B additionally has the possibility to pay a positive amount of money to Person A. If Person B accepts the offer, Person A receives the rest in this round. From now on we call this amount the payment.*

Procedures of one round: 2. decision of Person B

If Person B has accepted the offer and the rest is positive, Person B can make a payment to Person A. The payment can be at most as large as the rest that Person B has accepted in this round. The payment cannot be less than zero. If Person B sets the payment to zero, then this means that Person B pays nothing to Person A. The payment can be specified in 10 cent increments.

Procedures of one round: The earnings of Person A and Person B

This table summarizes the earnings in one round.

How does Person B decide?	Earnings of Person A	Earnings of Person B
Person B rejects the offer.	0	0
Person B accepts the offer.	claim	rest = pie − claim
Person B accepts the offer and pays a payment to Person A.	*claim + payment*	*rest − payment = pie − claim − payment*

Procedures of one round: Feedback on the decisions

At the end of a round, Person A and Person B receive the same feedback on the course of that round. In this feedback you will be informed about all decisions made, the size of the pie and the earnings in this round. After the feedback the round is finished. The randomly determined size of the pie in one round does not affect the size of the pie in another round. After one phase, you will receive feedback on all 10 rounds of that phase.

More details on your payoff

Your payoff, which you receive in cash at the end of the experiment, is determined as follows: From each of the two phases of the first part, a round is randomly selected and paid to you. Your payoff corresponds to the sum of your earnings in these two rounds. Your earnings in one round are 0 if B has rejected the offer. If B has accepted the offer, A's earnings are the claim and B's earnings are the rest, respectively, or claim + payment or rest − payment. Your earnings from the second part of the experiment will be added to this. Additionally, you get 5 Euro as endowment. If the sum of your earnings is negative, this amount will be deducted from your endowment. Your payoff cannot fall below 0 Euro. You will receive your payoff in private. No other participant will know the amount of your payoff.

Summary of the first part

The first part of the experiment consists of 2 phases. Each of these phases consists of 10 rounds. For the duration of one phase, the same two participants interact with each other in roles A and B. One round proceeds like this: Person A determines a claim between 0 Euro and 20 Euro that Person A wants to get from a pie. At this point, Person A does not yet know the size of the pie. The size of the pie is a randomly determined number between 0 Euro and 20 Euro. Then Person B gets the offer to accept or reject the remaining part rest (rest = pie − claim). If Person B accepts the offer, Person A gets the claim and Person B gets the rest. If Person B rejects the offer, nobody gets anything in that round. *If Person B has accepted the offer and the rest is positive, Person B makes another decision: Person B can make a payment to Person A. The payment must be greater than or equal to zero and can be at most as large as the rest.*

Quiz

We would now like to ask you to answer a little quiz about the instructions and the procedures of the experiment. Please do not hesitate to raise your hand if you have any questions. We will then come to your seat and answer your questions in private. The experiment starts as soon as all participants have answered the questions correctly. After the two phases we will ask you to fill in a short questionnaire. As a reminder: You assume the role of Person A [B] for the entire course of the experiment.

Quiz

Please indicate whether the following statements are true or false. The experiment starts as soon as all participants have answered the questions correctly. Your answers to these questions do not affect your payoff. [Correct answers in **bold** font.]

You will interact with the same other participant in both phases.	true	**false**
Within a phase you interact with the same other participant.	**true**	false
Each phase consists of 10 equal rounds.	**true**	false
At the time of Person A's decision, Person A knows the size of the pie.	true	**false**
Person B can also make a loss if Person A has set a claim that is larger than the pie.	**true**	false
Person B can make a payment to Person A if she or he has accepted the offer.	***true***	*false*
A participant's payoff is the sum of her earnings in all 20 rounds.	true	**false**