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Conciliation, Social Preferences, and Pre-Trial Settlement: a Laboratory Experiment.*

Matthieu Belarouci,[†] Vincent Lenglin,[‡] Rémi Suchon[§]

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Abstract

We experimentally study conciliation, an intervention aimed at improving bargaining efficiency. In conciliation, a neutral third party collaborates with the parties by suggesting resolutions to promote agreements. Unlike delegation or arbitration, conciliation fully preserves the autonomy of the parties. Unlike mediation, the conciliator cannot filter information. Whether conciliation can improve bargaining efficiency is an open question. In our laboratory experiment, two “litigants” bargain over the split of a loss in an unstructured protocol. In case of failure, a random split is implemented. In some conditions, a third party, the conciliator takes part in the bargaining by submitting non-binding suggestions to the litigants. We find that, on average, conciliation does not affect the likelihood of failure or the splits that are agreed upon by litigants. However, for bargaining pairs composed of selfish litigants, conciliation leads to more equal agreements. Conciliation also reduces bargaining delays: the time and the number of offers necessary to converge to an agreement are significantly reduced in the presence of a conciliator.

JEL codes: C78, C92, D91

Keywords: Bargaining game, Bargaining process, Conciliation, Social preferences, Experiment, Pre-trial settlement.

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1 Introduction

Bargaining failures are ubiquitous, carrying substantial economic costs. As underscored by Crawford (1982): “...the potential welfare gains from improving the efficiency of bargaining outcomes are enormous, perhaps even greater than those that would result from a better understanding of macroeconomic policy”.¹ For instance, the congestion within many judiciary systems partly arises from cases that, with improved pre-trial bargaining, could have been resolved prior to legal proceedings. This backlog not only delays access to justice but also introduces inefficiencies that might ultimately diminish the deterrent effect of legal sanctions.

In this paper, we study conciliation, an intervention aimed at improving bargaining efficiency. In conciliation, a neutral third party collaborates with the bargaining parties by making suggestions of resolution to help them reach an agreement. The third-party has no or very limited enforcement power and no access to superior information on the case at hand. Conciliation is distinct from other third-party interventions, such as arbitration, delegation, or mediation. In arbitration, the litigants delegate the resolution of their case to a neutral arbitrator and commit to respect his decision. As a consequence, the arbitrator acts as a privately contracted judge. In delegation, the parties delegate the bargaining to an agent. Both these mechanisms reduce the autonomy and the flexibility of the parties. It is also distinct from mediation because, unlike a mediator, a conciliator cannot filter the communication between the parties, but is allowed to suggest resolutions. However, it is precisely the ability to filter information that allows the mediator to improve bargaining efficiency: In mediation, the parties can confidentially disclose the outcome they would accept to the mediator. In turn, the mediator can commit to revealing this outcome to the other party only if it has the potential to lead to an agreement. This avoids bargaining strikes wherein parties can hold on to strong positions for an inefficiently long time.

We experimentally test the effect of conciliation on bargaining failure, the nature of agreements, and the bargaining processes. We also study whether the effectiveness of conciliation is affected by the social preferences of the litigants. This follows important contributions identifying how (the heterogeneity of) social preferences affect(s) the func-

¹We borrow this quote from Karagözoğlu (2019).

tioning of institutions (see e.g. Fehr and Charness, 2023) and bargaining outcomes (see e.g. Roth, 1995; Camerer, 2011; Bruyn and Bolton, 2008). Conciliation is a common practice in various contexts ranging from international conflict resolution, labor disputes or personal matters. Here, we embed our experiment in the context of pre-trial settlement conciliation, in which the conciliator is in charge of helping litigants reach an agreement out of the court, with the ultimate goal of filtering out the easier cases.

Conciliation is relatively cheap to implement, it is decentralized, and, most importantly, it preserves the autonomy of the parties. This explains why, along with other Alternative Dispute Resolution (ARD) mechanisms such as mediation and arbitration, pre-trial conciliation has attracted growing interest from policy-makers (See for instance the Directive 2013/11/EU in the European Union).² However, whether conciliation is effective is a largely unanswered question. Despite the limited power vested in the conciliator, conciliation may in principle help avoid bargaining impasses for at least two reasons. First, the intervention of a neutral conciliator may help cool down negotiations and avoid escalation. This can improve the outcome of negotiations since negative emotional reactions to unfair offers partly explain impasses (see e.g. Sanfey et al., 2003). Second, bargaining failures are often attributable to over-optimistic expectations and self-serving beliefs about deservingness (Babcock et al., 1995; Babcock and Loewenstein, 1997). Given his neutrality and relatively superior knowledge of the law, a conciliator may help align the beliefs of parties about what to expect and what they deserve in the negotiation.

To answer our research question, we developed a 3-parts lab experiment capturing the essence of pre-trial settlement. In the first part, participants work for a €15 endowment. In the second part, they bargain in pairs over the split of a €10 loss. Potential losses are deducted from the previously earned endowment. Bargaining over losses adds relevance to our experiment because most disputes are over damages.³ For instance, bankruptcy

²For instance, in France, conciliation has been mandatory for every civil dispute of moderate stake (less than €5000) since 2017. As a consequence 100000 were performed in France in 2015, a 30% increase compared to 2001 (Brunin and Pirot, 2017). Roughly half of the conciliation cases result in an agreement out of the court. Conciliation is also often used in labor disputes, family disputes and divorce. In Quebec, Article 54 of the Labor Code states that parties to a dispute may require the ministry to appoint a conciliator to help them find an agreement. In Australia, according to family law, the court has the power to require that the parties attend a dispute resolution event, which might comprise conciliation.

³A sizeable literature shows that social preferences are different in the loss domain (see e.g. Boun My et al., 2018; Fiedler and Hillenbrand, 2020; Benistant and Suchon, 2021), which translates into different

litigation, or the determination of responsibilities in an insurance claim are important situations in which losses are to be shared. We chose an unstructured bargaining game for two reasons. First, it is more natural for participants, it is closer to pre-trial bargaining than highly structured bargaining games such as the Ultimatum or Centipede game, and, as such, it improves the ecological validity of our experiment. Second, it allows us to collect interesting data on the bargaining process, such as the number of offers or the time needed to reach an agreement (Karagözoğlu, 2019; Camerer et al., 2019). If bargaining leads to an agreement, the split is implemented. Otherwise, a random device determines the split of the loss. In the course of the manuscript, we will refer to this random split as “the judgment”. In case of failure, both participants must pay an additional €1, meant to capture the uncertain nature of justice and the cost of waiting for the judgment. As a consequence, bargaining failures clearly harm efficiency. Before each round of the bargaining game, each participant has a 50% chance of being informed of the judgment. Draws are independent. This mimics *a priori* knowledge of the parties about the possible outcome of the judgment and the potential heterogeneity in this respect. In addition, because each party ignores whether the other party received the information, the asymmetry of information can lead to strategic bargaining behaviors, as can be the case in real life. Our main treatment is the presence of a *conciliator* in the bargaining game. Conciliators observe the judgment. This captures the fact that conciliators are knowledgeable of the law or have some form of experience with the type of conflicts in which they intervene.⁴ They also observe the offers by the parties and can make publicly observed suggestions on the split of the loss. The conciliator has no incentives to close deals and has no enforcement power. This closely replicates real-life conciliation. We also vary the distribution of judgment: in the Uniform treatments, the judgment can be any split of the loss, with equal probability. This mirrors many situations among which the repartition of losses between the stakeholders in bankruptcy, of assets (and custody) between former spouses in divorce, or of liability after a disaster between an insurance company and a client. In the Extreme treatment, one of the parties is selected to support

bargaining strategies in the gain and in the loss domain (e.g. De Dreu et al., 1994; Breitmoser and Tan, 2013; Neumann et al., 2017, 2018).

⁴For instance, in France, conciliators are often retired legal professionals.

the whole loss. The “Extreme” treatments emulate conflicts in which one of the parties will be liable for the whole loss. Such conflicts occur in real life: for instance, in the context of a car accident, the judge must determine who is at fault. This is also the case when, as a result of defects in a project that involves multiple subcontractors (e.g. in the construction sector), the need to determine who is responsible arises. These situations have interesting specific features: the bargaining power is extremely unbalanced and the downside risk in case of failed bargaining is maximal. Previous evidence from the ultimatum game and unstructured bargaining game suggests that such asymmetries impact both the likelihood and types of agreements reached (see e.g. Anbarci and Feltovich, 2013; Hennig-Schmidt et al., 2018).⁵ We conjecture that this might matter both for pre-trial settlement and the effectiveness of conciliation.

Lab experiments like ours present a number of attractive features to study pre-trial settlement and in particular, conciliation. First, lab experiments allow us to control the specifics of a dispute and the individual characteristics of the parties. More specifically, the introduction of conciliation is totally exogenous, which ensures that we are able to capture the *causal* effect of conciliation. In addition, it is hard to obtain detailed data on pre-trial settlement under conciliation, since the content is often held confidential by law. As a consequence, we cannot observe the bargaining outcome precisely, and we cannot observe the process at all. In the same vein, the social preferences of litigants are unobservable in field bargaining.

Our results are as follows. We find no effect of conciliation on the outcome of pre-trial settlement: failure is equally likely with and without a conciliator, and the agreements are not significantly different. This is the case both when judgments are uniformly distributed and when judgments are extreme. We also find that social preferences matter for pre-trial settlement: The presence of one or more inequity-averse individuals in a bargaining pair significantly reduces the likelihood of failure and leads to more equal agreements. More importantly, we find that when none of the bargaining parties is inequity averse, conciliation promotes more equal agreements. This suggests that conciliation has a heterogeneous

⁵Note that the extreme treatment increases the variance of the expected judgment for participants who are not informed of the judgment, which may also impact bargaining outcomes (see e.g. Cardella and Kitchens, 2017)

effect, depending on the social preferences of the parties. Importantly, we find that conciliation shortens the bargaining process: agreements are reached faster when there is a conciliator, and participants need to make fewer offers. Moreover, the share of agreements reached in the very last seconds before the deadline is also significantly reduced.⁶ This shows that conciliation has the potential to improve bargaining efficiency, mainly through facilitating the converging process towards an agreement, rather than drastically changing the occurrence or nature of agreements.

The remainder of the paper is organized as follows. Section 2 briefly reviews the related literature. Section 3 introduces the experimental design. Section 4 presents our main results and Section 5 concludes.

2 Related literature

In this section, we briefly review the related literature. We contribute to two streams of the literature: the economic literature studying how third-party interventions can improve bargaining efficiency, and the Law and Economics literature on pre-trial settlement.

2.1 (Economic) literature on third-party interventions to promote efficient bargaining.

There is a large literature on third-party interventions in bargaining in psychology and organization behavior (see e.g. Bazerman and Neale, 1993; Mnookin and Susskind, 1999). We focus on the economics literature, where mainly two types of third-party interventions have been studied: delegation to agents, and mediation. Delegation may obviously suffer from classical principal-agent problems, wherein the preferences of the agent and of the principal are not perfectly aligned (see Bolton and Dewatripont, 2004, for an overview). However, agents can still improve bargaining efficiency thanks to superior information and better bargaining skills. In addition, delegation can deflect the blame from the principal to the agent⁷: for instance, Fershtman and Gneezy (2001) show, in an ultimatum game,

⁶On the deadline effect in bargaining, see e.g. Roth et al. (1988).

⁷For instance, Hamman et al. (2010); Bartling and Fischbacher (2011) document that delegation allows principals to shift the blame onto the agents in social interactions.

that second movers tend to be more willing to accept moderate offers when they stem from agents, leading to higher bargaining efficiency. Sutan and Vranceanu (2016) present similar results. Agents can also be used as commitment devices, with uncertain effect: Schotter et al. (2000) show that principals may set inflexible objectives for the agent, which can ultimately lower agreement rates. In conciliation, the parties are actively involved in bargaining so they retain full autonomy and flexibility. As such, blame cannot be shifted onto the conciliator. Conciliation is thus fairly different from delegation to agents.

In mediation, the mediator filters and organizes communication, but the parties retain full autonomy. Recent theoretical contributions show that, when the mediator commits to suggest a resolution publicly only if both parties have accepted it in private, mediation can improve bargaining efficiency (see e.g. Goltsman et al., 2009; Hörner et al., 2015; Fanning, 2021). Such commitment erases the incentives for the parties to hide their bargaining strength. Clearly, this is an important difference with the setting of conciliation we study, in which no information censoring is possible. Using data from the wholesale market for used cars, Larsen et al. (2021) show that mediation can improve bargaining efficiency, but this crucially hinges on the skills of the mediator. Casella et al. (2020) study mediation in the lab. They find a limited effect of mediation on bargaining outcomes, but some effects on the bargaining strategies of the parties. This is broadly consistent with our results. In mediation, the mediator is neutral and has limited (or no) enforcement power. However, the capacity to filter and obfuscate information is what makes mediation potentially effective in improving bargaining efficiency. Given that a conciliator does not have the possibility to obfuscate, conciliation is thus fairly different from mediation.

2.2 Law and economics literature on pre-trial settlement.

Because we embed our study in the context of pre-trial settlement, we also contribute to the abundant law and economics literature on pre-trial settlement and Alternative Dispute Resolution mechanisms (ADR)(see e.g. Fisher, 2017, for a review). Shavell (1982); Spier (1992); Guha (2019), among others, offer theoretical accounts of pre-trial settlement,

while Kessler (1996); Deffains and Doriat (1999); Eisenberg and Lanvers (2009); Berleman and Christmann (2019) explore the determinants of pre-trial settlement empirically. Shavell (1995); Deffains et al. (2017) studies the choice between litigation and ADR. The bulk of the literature on ADR has focused on arbitration (see e.g. Ashenfelter and Bloom, 1984; Pecorino and Van Boening, 2001) and mediation (see e.g. Goltsman et al., 2009; Fanning, 2021).

Like ours, several papers have studied pre-trial settlement and tested the effectiveness of Alternative Dispute Resolution mechanisms in the lab (see e.g. Croson, 2009). Gabuthy and Lambert (2013) compare mandatory and voluntary pre-trial settlement and find that the former leads to more agreements. Lambert et al. (2019) refines the design of Gabuthy and Lambert (2013), to better isolate the pure effect of being forced to go through pre-trial settlement. They find that mandatory pre-trial settlement leads to more equal agreements. Our paper differs from these in two important ways: first, we use an unstructured bargaining game over losses while they use either a structured (Gabuthy and Lambert, 2013) or an unstructured bargaining game (Lambert et al., 2019) over gains. We believe that this better fits real-life disputes, which are mostly over losses. Second, and more importantly, we study conciliation which is not considered in these papers. We hold pre-trial negotiations mandatory and vary the presence of a conciliator across treatments.

Some experiments have studied arbitration as a substitute to trials (see e.g. Ashenfelter et al., 1992; Bolton and Katok, 1998; Dickinson, 2005; Deck et al., 2007). An important result from this literature is that the perspective of arbitration might have a “narcotic effect”: parties may exert less effort to find an agreement on their own because they rely on the arbitrator to impose a resolution.⁸ Arbitration is very different from conciliation: when litigants opt in an arbitration, direct bargaining stops, and a binding and definitive resolution is formulated by the arbitrator(s). Babcock and Landeo (2004) test the effect of escrow on pre-trial settlement. An escrow is a mechanism in which the litigants privately submit their limit claims to a neutral third party. If the claims overlap, the midpoint between the claims is used to determine the settlement value. They find that escrow can increase the rate of pre-trial settlement, and thus avoid trial, only if there is information

⁸Note however that the strength of the Narcotic effect depends on the details of the arbitration procedure.

asymmetry.

Finally, neither the literature on third-party intervention nor the law and economics literature on pre-trial settlement engage social preferences. We elicit participants' social preferences to link the (heterogeneity of) social preferences to pre-trial settlement, and more specifically to the effectiveness of the third-party intervention.

3 Experimental design.

Our experiment has three parts. In the first part, participants perform a real effort task that generates their endowment for the next part. In the second part, participants go through 12 rounds of an unstructured bargaining game over the sharing of a monetary loss, the potential loss being deducted from the endowment earned in the first part. In the third part, we elicit participants' risk and social preferences. The first and third parts of the experiment are identical across treatments. We introduce variations only in the second part.

3.1 Part 1: The real effort task.

In the first part, participants perform 100 encryption tasks to generate a €15 endowment (see e.g. Charness et al., 2014, for a similar task and see Figure G.4 in Appendix G for a screenshot). Participants have to successfully convert a number into a letter using a correspondence table. After each success, a new encryption task is generated by shuffling both rows. In case of failure, the encryption task remains the same and participants have to enter a new answer. We chose this task to generate an endowment effect because it is both simple and tedious.

3.2 Part 2: The unstructured bargaining game.

The second part consists of 12 periods of a two-person “unstructured bargaining game”. At the end of the experiment, one of the periods 3-12 is randomly selected to determine payment. The first two periods served as training periods for participants to familiarize themselves with the layout and the rules of the game. In an unstructured bargaining

game, only the time given to reach an agreement, the surplus, and the disagreement pay-off are imposed by the researcher. The sequence, number, timing, and value of proposals are endogenous. Parties are free to make as many proposals as they want, at any time or even not to make any proposal (see e.g. Karagözoğlu and Kocher, 2019). This allows for the collection of interesting data on the bargaining process in addition to those on bargaining outcomes (see e.g. Camerer et al., 2019). In addition, unstructured bargaining games are more natural to participants than highly structured ones, ensuring a fair deal of external validity.

In each period, participants are matched in pairs according to a “pseudo stranger matching” protocol to play the bargaining game.⁹ Interactions are anonymous. This prevents any reputation or reciprocity effect. Participants have 60 seconds to bargain over the split of a €10 loss. Bargaining over losses is both interesting theoretically (e.g. Breitmoser and Tan, 2013) and practically: many situations, ranging from bankruptcy to responsibility in an insurance claim, involve damages or liability. If the pair fails to reach an agreement in the allotted time, a random split is implemented. We call this random split the “judgment”. Participants are informed that each party has a 50% chance of being informed of the judgment before the start of the bargaining period, with independent draws. This means that none, one, or both parties can be informed. Note however that parties do not know whether their counterpart is informed. This introduces information asymmetries that are likely to occur in the field and which might impact the bargaining strategies. If the judgment is implemented (ie in case of failed bargaining) participants lose an additional €1 each, simulating the efficiency cost of delaying the decision and going to the court.

For the period determining payment, the share of the loss that is assigned to each participant (that results either from an agreement or the judgment) is deducted from their endowment earned in the first part.

Parties exchange offers via a customized live chat. An offer is a split of the €10 loss that would be implemented if it was accepted by the other party. Each party has continuous access to the history of offers made by both parties in the current round. Screenshots of

⁹Specifically, participants are informed that, although they can meet the same partner more than once during a session, they cannot meet the same partner in two consecutive periods.

the bargaining interface are presented in Figure G.7 in Appendix G.

3.2.1 Experimental conditions.

All participants in a given session are in the same condition (between-subjects design). The experimental conditions vary along two dimensions: (i) Conciliation: presence/absence of a conciliator during the bargaining game. (ii) Judgement: the distribution of the judgment.

Conciliation: In the “Conciliation” conditions, we introduce a third party, the conciliator, in the bargaining game. At the beginning of a “conciliation” session, 1/3 of the participants are informed that they will be conciliators for the whole experiment. Conciliators are assigned to bargaining pairs randomly and anonymously at each period. The role of the conciliator is to make suggestions on how to split the loss. Conciliators can make as many suggestions as they want, at any time, or not make any suggestion at all. Conciliators have no enforcement power, so they cannot impose a split. Conciliators are always informed about the judgment. This parallels real conciliation in which conciliators are generally more familiar with the law than the parties. The success or failure of the bargaining has no impact on the payment of the conciliator: conciliators always keep their earned endowment from the first part. This captures the fact that most of the time, conciliators are not paid based on their performance.

Distribution of judgment: We vary the distribution of the judgment. In the Uniform treatments, the judgment can be any split of the €10 loss (in step of €0.1), with equal probability. In the Extreme treatments, one party is selected to support all the losses. These situations are common in real life, for instance in cases where conciliation precedes a judgment that determines who is liable for a damage, or when it comes to determining whether a taxpayer is liable for a specific tax or a penalty. This might matter for bargaining outcomes for two reasons: (i) when none of the participants is informed about the judgment the variance of the judgment is greater in the extreme treatments, with maximum downside risk (see e.g. Cardella and Kitchens, 2017) (ii) When at least one participant is informed of the judgment, the extreme treatments can lead to extremely unbalanced bargaining power (Anbarci and Feltovich, 2013; Hennig-Schmidt et al., 2018).

We deem it interesting to explore the interactions between conciliation and the distribution of judgments. Participants are informed of the distribution of judgments in their sessions. Table 1 summarizes the experimental treatments.

		Distribution of judgement.	
Conciliation	No	Uniform	Extreme
	Yes	NoConcil_Uniform Concil_Uniform	NoConcil_Extreme Concil_Extreme

Table 1: Summary of the experimental treatments.

3.3 Part 3: Preference elicitation and demographic questionnaire.

In the third part of the experiment, we elicit participants’ risk and social preferences, and some demographic characteristics. Risk and social preferences elicitation are incentivized: participants are told that one of these two tasks will be selected randomly at the end of the experiment and paid for. Social preferences are one of the main interests of our paper, while risk preferences and demographics are collected to check the balance between treatments.

3.3.1 Social preferences

We adapt the method of Fehr et al. (2022, 2023) to elicit social preferences. According to Fehr et al. (2022, 2023), participants can be classified into three social-preference types: Inequality-averse, Selfish, and altruistic. Such parsimonious classification allows for a tractable analysis of the effects of social preferences, and crucially, Fehr et al. (2022, 2023) show that this classification has good predictive power.

Participants face 14 screens, presented in random order. On each screen, 7 alternative distributions between the decider and another anonymous, randomly selected participant in the session are presented. Across screens, the trade-off between the payment for oneself and the payment for the other varies. More specifically, some alternatives equalize payments, while others lead to a very unequal distribution that is very favorable to the individual who decides. In other situations, some alternatives are very favorable to the

individual designated as the recipient. We then use a classification algorithm to sort participants into three distinct social preference types based on their 14 decisions. The use of a classification algorithm allows to exploit the variation in the data without a priori assumptions about the social preference types present among participants. However, in line with Fehr et al. (2022, 2023), we identify 3 types with a natural interpretation.¹⁰ First, "selfish" participants are only interested in their personal payment, regardless of the situation: they systematically choose the alternative that maximizes their personal payment. "Inequality averse" participants mostly choose allocations that equalize payments between them and the receiver even if it reduces their own payment. Finally, "altruistic" participants agree to sacrifice a significant portion of their payment to increase the payment of the other participant, but, in contrast with the inequality-averse, they are not willing to sacrifice the payment of the better-off to enforce equality. More details on the elicitation of social preferences and classification are given in Appendix D.

3.3.2 Demographic questionnaire

At the end of the experiment, we collected some data on age, gender, status (student, employed, retired...), education, experience with experiments, and monthly expenses of the participants. We also elicited risk preferences in the gain and loss domain using the method proposed by Eckel and Grossman (2008) (more details are given in Appendix F).

3.4 Procedure.

The experiment was conducted at the Anthro- Lab in Lille, France. Participants were recruited via ORSEE (Greiner, 2015). The experiment was developed using Otree 3.3.11 (Chen et al., 2016). Instructions were distributed to participants at the beginning of each part and read aloud by the same experimenters. Instructions were neutrally framed: there was no mention of judgment or conciliation (See Appendix H for a copy of the instructions). We run 6 sessions in each treatment, with 10 to 21 participants in each; for a total of 414 participants. Table 2 summarizes the number of participants and observations

¹⁰The main difference with Fehr et al. (2022, 2023) is that we use a k-median algorithm in which we set the number of desired clusters to 3, while they use a more flexible algorithm that endogenously determines the number of clusters.

	Observations	Nb participants	Nb Sessions
NoConcil_Uniform	400	80	6
Concil_Uniform	410	123	6
NoConcil_Extreme	470	94	6
Concil_Extreme	390	117	6

Table 2: Distribution of observations, participants, and sessions per treatment.

per treatment. Sessions lasted about one hour and participants earned €16.9 (SD=4.64) on average. Table F.9 in Appendix F details the demographics of our participants. No systematic difference is observed between treatments.

4 Results

In the result section, we use a bargaining interaction as the level of observation. We drop the two first periods (training periods) which were not incentivized. When analyzing the nature of the agreements, we consider the distance from the equal split as our variable of interest. This variable ranges from 0 when participants agree to support €5 of loss each, enforcing equality, to 5 when one participant supports €10 of losses and the other €0, enforcing maximum possible inequality.¹¹

4.1 The effect of conciliation on bargaining outcomes

Table 3 reports descriptive statistics. The top panel displays the rate of failure and the distance from the equal split in case of agreements, by treatment. The rate of failure is very close to 30% in every treatment (session averages, Kruskal-wallis tests, KW hereafter: $p = 0.667$). Depending on the treatment, the distance from the equal split ranges from €0.98 in the Concil.Uniform treatment to €1.32 in the NoConcil_Extreme treatment (Session averages, KW: $p = 0.232$). Note that pooling Concil and NoConcil treatments, the distance from the equal split is on average greater in the Extreme condition (MW, $p = 0.052$).

In the middle panel, we report data related to the bargaining process. The number of

¹¹Formally, this variable is computed as follows: $Distance = |5 - loss\ supported\ by\ one\ of\ the\ player|$. Obviously, it does not matter which player is focal in computing the distance.

seconds needed to reach an agreement ranges from 38 to 44 seconds (Session averages, KW: $p = 0.049$). The number of offers needed ranges from 7.4 to 9.2 on average (Session averages, KW: $p = 0.149$), and between 34% and 42% of the agreements are concluded in the last 5 seconds (Session averages, KW: $p = 0.310$).

The bottom panel, reports the number of suggestions made by the conciliators in the two treatments with a conciliator. Conciliators made on average 3.2 ($SD = 2.56$) suggestions in the Concil_Uniform Treatment, and 2.19 ($SD = 1.86$) suggestions in the Concil_Extreme treatment (Session averages, Mann-Whitney: $p = 0.041$). It confirms that conciliators participated and made suggestions, despite the lack of monetary incentives.

Table 3: Descriptive statistics

	(1) NoConcil_Uniform mean/sd	(2) Concil_Uniform mean/sd	(3) NoConcil_Extreme mean/sd	(4) Concil_Extreme mean/sd
% of failed bargaining	0.31	0.31	0.27	0.29
	-	-	-	-
Distance from the equal split	1.05	0.98	1.32	1.16
	1.29	1.19	1.58	1.47
Timing of the last offer	43.93	43.85	42.81	38.31
	17.04	17.01	17.72	19.79
Agreements within the last 5 sec.	0.40	0.42	0.40	0.34
	0.49	0.49	0.49	0.47
Number of offers (parties only)	9.06	9.20	8.72	7.42
	4.34	5.33	5.53	4.33
Number of suggestions (conciliator)	-	3.21	-	2.19
	-	2.56	-	1.86
Observations	400	410	470	390

For the sake of concision, we present some data on the role of information and judgment on the rate and nature of agreements in Appendix B. We now turn to our main questions: the effect of conciliation and social preference on bargaining efficiency.

Result 1: On aggregate, conciliation has no effect on the failure rate or the nature of agreements. Table 3 provides strong evidence supporting Result 1. This result is further confirmed by the outcome of regressions presented in Table C.6 in Appendix C.

4.2 The effect of social preferences on bargaining outcomes

We now ask whether social preferences impact bargaining outcomes and the effectiveness of conciliation. Table 4 shows the distribution of social preference types by experimental treatments. Overall, roughly 40.7% of our participants are predominantly selfish, 36% are predominantly altruistic, and 23% are predominantly inequality-averse. There are some moderate differences between treatments, but overall these are not significant ($\chi^2(2) = 9.22$, $p = 0.161$).¹² Table 5 reports the rate of failure (upper panel) and

Table 4: The distribution of social preference types by treatment.

	NoConcil_Uniform	Concil_Uniform	NoConcil_Extreme	Concil_Extreme	Total
Altruist (A)	40.00	39.02	29.79	37.18	36.23
Inequality Averse (IA)	22.50	14.63	26.60	28.21	23.05
Selfish (S)	37.50	46.34	43.62	34.62	40.72

Notes: Percentage of participants belonging to each social preference type, by treatment. Conciliators are excluded.

the nature of agreements (bottom panel), separated by the composition of the pairs and experimental condition. First, it is apparent that the composition of the pairs in terms of social preference matters: failure rates range from 15.7% when both parties are inequality-averse to 36.6% when both parties are selfish. In addition, agreements between selfish parties are the most distant from equality (€1.34 on average), while agreements between inequality-averse individuals are the most equal (distance of €0.8 from equality).

To formally test the effect of social preferences on bargaining outcomes, we ran two sets of regressions reported in Table 6. Unless stated otherwise, all our regressions have standard errors clustered at the session level. In the first set, our variable of interest is a categorical variable indicating whether none, one or both parties are inequality-averse. The results are reported in the top panel of Table 6. Column (1) reports the marginal effect of the number of inequality-averse individuals on the probability of failure (Logit model). Column (2) reports the marginal effect of the number of inequality-averse individuals on the distance of the agreements from equality (Tobit model). The presence of 1 inequality-averse individual reduces failure by 3.8 percentage points on average ($p = 0.045$), while

¹²Note that we have a slightly different distribution of types than Fehr et al. (2022), who identify more inequity averse and altruist, and less selfish individuals. We can only speculate that it is due to a difference in the sample.

Table 5: Failure rates and the nature of agreements depending on social preferences and experimental condition.

	NoConcil_Uniform	Concil_Uniform	NoConcil_Extreme	ConcilExtreme	Total
% of failed bargaining					
A x A	.319	.298	.326	.263	.302
A x Ia	.322	.229	.222	.268	.262
A x S	.291	.304	.28	.319	.298
Ia x Ia	.083	.2	.217	.161	.157
S x Ia	.288	.355	.27	.303	.295
S x S	.455	.362	.292	.38	.366
Total	.31	.312	.272	.292	.296
Distance from the equal split					
A x A	1.17	1.21	1.11	1.25	1.19
A x Ia	1.05	1.07	.982	1.32	1.12
A x S	1.21	.992	1.58	1.41	1.27
Ia x Ia	.564	0	1.42	.696	.8
S x Ia	.77	.744	1.04	.898	.903
S x S	1.29	1	1.87	1.08	1.34
Total	1.05	.985	1.32	1.16	1.14

Notes: A: Altruist, Ia: Inequity Averse, S: Selfish. A x A corresponds to pairs in which both are altruists, A x Ia corresponds to pairs with one altruist and one Inequity averse.

when both parties are inequality-averse, failures drop by 15.8 percentage points ($p < 0.01$). Similarly, the presence of 1 or two inequality-averse parties leads to more equal agreements (resp. €0.52, $p = 0.001$ and €1, $p = 0.004$ closer to equality).

In the second set of regressions, we investigate the effect of conciliation depending on the social preferences of the bargaining parties. Thus, our variable of interest is an interaction term between the presence of a conciliator and the social preferences of the parties (Uniform and Extreme conditions are pooled). We are interested in the marginal effect of conciliation depending on the number of inequality-averse individuals in a pair. The results are reported in the bottom panel of Table 6. Column (3) reports the marginal effect of conciliation on the probability of failure when 0,1 or 2 parties are inequality-averse (Logit model). Column (4) reports the marginal effect of conciliation on the nature of agreements when 0,1 or 2 parties are inequality-averse (Tobit model). The main result is that conciliation has a heterogeneous effect on agreement, depending on the social preferences of the parties: conciliation promotes more equal agreements in pairs with no inequality-averse party. In such pairs, conciliation makes agreements €0.34 ($p = 0.029$) closer to the equal split. This analysis supports Results 2 and 3.

Table 6: The effect of social preferences on failures / nature of agreements

	(1) Failure=1	(2) Dist.	(3) Failure=1	(4) Dist.
Number of IA individuals=0	ref.	ref.		
Number of IA individuals=1	-0.035 ⁺ (0.018)	-0.522*** (0.133)		
Number of IA individuals=2	-0.160*** (0.039)	-1.002* (0.431)		
<i>The marginal effect of conciliation when:</i>				
Number of IA individuals=0			-0.003 (0.029)	-0.343* (0.157)
Number of IA individuals=1			0.008 (0.042)	0.196 (0.254)
Number of IA individuals=2			0.030 (0.084)	-0.772 (0.823)
Observations	1670	1184	1670	1184
Clusters (N sessions)	24	24	24	24
Controls	Yes	Yes	Yes	Yes

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Marginal effects of reported. Columns (1) and (3) are Logit models. Columns (2) and (4) are Tobit models. Standard errors clustered at the session level in parentheses. Controls include period, the value of the judgment, a categorical variable indicating the number of informed parties and the interactions of these last two variables.

Result 2: Social preferences matter for bargaining outcomes: The presence of inequality-averse individuals leads to less failure and more equal agreements.

Result 3: Conciliation promotes more equal agreements in pairs with no inequality-averse party.

4.3 The effect of conciliation on the bargaining process

We now ask whether conciliation affects *the bargaining process*. We run regressions explaining different dimensions of the bargaining process by the presence of a conciliator, namely the number of offers necessary to reach an agreement, the time needed to reach an agreement and the proportion of agreements that are reached within 5 seconds of the deadline. We focus on the subset of observations in which an agreement was reached. The outcomes are presented in Table 7.

In the top panel, we report the effect of conciliation pooling Extreme and Uniform judgments. Column (1) reports the effect of conciliation on the timing of the last offer (max: 60 seconds) estimated with a Tobit model to account for the potential censoring at 60 seconds (after which a bargaining phase is over). Column (2) reports the effect of conciliation on the number of offers from the parties estimated with an OLS model and Column (3) reports the effect of conciliation on the probability that conditional on reaching an agreement, the agreement is reached in the last 5 seconds. This effect is estimated using a Logit model and was performed in order to capture the deadline effect. Controls are introduced in every model, and standard errors are clustered at the session level. Column (1) shows that Conciliation reduces the time needed to reach an agreement by about 2.8 seconds. It is worth noting that this effect is found *despite* the intervention of a third party whose interventions might mechanically increase the bargaining time.¹³ Column (2) shows that conciliation does not affect the number of offers on average, while Column (3) shows that conciliation reduces the share of agreements that are reached in the last 5 seconds by roughly 10pp ($p = 0.004$).

¹³We could control for the number of suggestions made by the third party, but it is an *endogenous variable*, which might be problematic.

In the bottom panel, we separate the effect of conciliation by the distribution of the judgment (either Uniform or Extreme). Column (4) reveals that the effect of conciliation on bargaining time is driven mainly by the cases with Extreme judgment: conciliation reduces bargaining time by almost 7 seconds ($p = 0.002$) in this case, and only by about 2.3 seconds in the Uniform case ($p > 0.1$). Similarly, we find that conciliation significantly reduces the number of offers to reach an agreement in the Extreme case (-1.33 , $p = 0.016$) but not in the Uniform case. The effect of conciliation on the deadline effect is also mainly driven by the Extreme treatment. This supports Result 4.

Table 7: The effect of conciliation on the bargaining process.

	(1)	(2)	(3)	(4)	(5)	(6)
	Timing agr.	Nb offers	Last 5 sec.	Timing agr.	Nb offers	Last 5 sec.
<i>Marginal effect of:</i>						
Conciliation	-4.742** (1.660)	-0.551 (0.508)	-0.104** (0.036)			
<i>Marginal effect of conciliation for :</i>						
Uniform judgement				-2.260 (2.302)	0.317 (0.821)	-0.063 (0.053)
Extreme judgement				-6.969** (2.202)	-1.330* (0.513)	-0.140** (0.045)
Observations	1176	1176	1176	1176	1176	1176
Clusters (Nb sessions)	24	24	24	24	24	24
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Marginal effects of reported. Columns (1) and (4) are Tobit models. Columns (2) and (5) are OLS models. Columns (3) and (6) are Logit models. Standard errors clustered at the session level in parentheses. Controls include period, the value of the judgment, a categorical variable indicating the number of informed parties and the interactions of these last two variables.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Result 4: Conciliation shortens negotiation. The effect is mainly driven by situations with Extreme judgments.

5 Conclusion

We studied the effect of conciliation on bargaining, in the context of pre-trial settlement. Our results are mixed: On average, we found no effect of conciliation on the likelihood of agreements, and, conditional on reaching one, agreements are not different with and without conciliation. However, we found evidence that conciliation has a heterogeneous effect, depending on the social preferences of the parties: when none of the parties is averse to inequality, conciliation promotes more equal agreements. On the other hand, we found that conciliation significantly impacts the bargaining process: when a conciliator is present, the parties need less time to reach an agreement, they need to make fewer offers, and fewer agreements are concluded in the very last seconds before the deadline. The effect of conciliation on the bargaining process is mainly driven by situations in which the bargaining power is extremely uneven.

Our results show that conciliation can be an effective tool to improve bargaining efficiency, by reducing the time, effort, and costs required to reach an agreement, and by avoiding uncertainties associated with bargaining failures. As a consequence, our results provide support for the policies that promote conciliation to improve the efficiency of the judiciary system. Our results also show that the effectiveness of conciliation in promoting fair agreements depends on the individual characteristics of the litigants, this suggests that individual characteristics should be taken into account in designing efficient agreement-promoting institutions.

What could explain the effect of a powerless third party on the bargaining process? First, conciliation can partly alleviate information asymmetry. Information asymmetry can generate delays in bargaining because participants have an incentive to withhold concessions to signal that they have a strong bargaining position. Because the conciliator is neutral, his suggestions can be interpreted by the litigants as a credible signal of the outside option of the opponent. Second, the neutrality of the conciliator can also help reduce the gap between the litigants' subjective beliefs of deservingness, helping them to reach agreements faster. These mechanisms are consistent with suggestive evidence, presented in Appendix E, that conciliators generally make offers that lie in between equal sharing and the payoff that litigants would get if the bargaining fails. Last, it is also possible that

the mere observation of bargaining strategies by the third party makes the parties more reluctant to use aggressive bargaining strategies because they fear that it would make them look greedy (see e.g. Andreoni and Bernheim, 2009).¹⁴ Designing experiments that specifically test the aforementioned mechanisms would be an interesting extension of our work.

On a more applied note, it would also be interesting to study institutional variations to identify those that make conciliation more effective. In particular, testing whether allowing participants to choose third-party conciliation over regular conciliation or litigation would be informative to designing more effective institutions.

¹⁴Note however that merely being observed is not always sufficient to impact behavior (see e.g. van de Ven and Villeval, 2015; Suchon and Houser, 2022).

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A Power analysis and sample size determination.

To determine the necessary sample size, we adopted a simulation-based power calculation. We focus on the effect of conciliation on the failure rate between two treatments (for instance NoConcil_Uniform and Concil_Uniform). We simulated 100 samples for each possible combination of parameters. The parameters include the treatment effect (or effect size, ie a difference in failure rate expressed in percentage points), session effect, number of sessions, number of participants, and a variance parameter. We run regressions on each of these samples independently and counted how many times the coefficient estimated for the treatment effect was significantly different from 0. This gives us the simulation-based estimate of power for the given vector of parameters. Table A.1 reports the power for a subset of the tested parameters. Given our sample with 6 sessions per treatment, and between 60 and 70 observations per treatment, we have an estimated power of 80% to detect an effect of 15 percentage points, assuming a moderate session effect.

Table A.1: Power calculation

Power	Treatment effect	Session effect	Size Session	Sessions per treatment
.77	.1	.05	70	6
.78	.15	.1	60	6
.8	.15	.1	70	6
.95	.15	.05	60	6
.97	.15	.05	70	6

Note: The agreement rate in the baseline is set at 50%. Treatment effect is the difference from the baseline in percentage points, the session effect is measured in percentage point, Size Session is the number of observations in a session.

B Results on the effect of judgment and information on bargaining outcomes.

B.1 The effect of the judgment

We test whether the judgment impacts the likelihood and the nature of the agreement. Table B.2 reports the rate of failure and the distance from equality when an agreement is reached, by treatment. We separate the Judgement in 5 brackets, depending on their distance to equality. We exclude interactions in which none of the litigants is informed of the judgment because the judgment could not matter in these situations. Note however that the following results hold if we include these data.

Table B.2: Agreement across treatments and judgement bins.

	Judgement distance bracket				
	[0;1[[1;2[[2;3[[3;4[[4;5]
Treatment:					
NoConcil_Uniform					
% of failed bargaining	.232	.273	.362	.463	.343
Distance from the equal split	.636	.79	1.59	1.15	1.92
Concil_Uniform					
% of failed bargaining	.245	.266	.31	.421	.408
Distance from the equal split	.51	.715	.94	1.19	2.14
NoConcil_Extreme					
% of failed bargaining					.284
Distance from the equal split					1.45
Concil_Extreme					
% of failed bargaining					.329
Distance from the equal split					1.36

Notes: We dropped observations in which none of the parties is informed about the judgment, since in these situations, judgment cannot matter.

The frequency of failure is lowest when the judgment is closer to equality (in the first bracket) and highest when it is far from equality. This relationship is significant in a Logit model with session fixed-effects and standard errors clustered at the session level (marginal effect = 0.042, $p = 0.02$, Column (1) in Table B.3).

Similarly, agreements are closer to equality when judgments are more equal. This re-

relationship is significant in a Tobit model with session fixed-effects and standard errors clustered at the session level (marginal effect = 0.499, $p < 0.001$, see Column (2) in Table B.3). Thus, our data are consistent with the classical results that participants exploit their bargaining power, but not to the full extent (see e.g. Anbarci and Feltovich, 2018).

Table B.3: The effect of judgement on bargaining outcomes

	(1) Failure=1	(2) Dist.
Distance judgement	0.042** (0.001)	0.499*** (0.000)
Sessions FE	Yes	Yes
Controls	Yes	Yes
Sessions (Nb clusters)	24	24
Observations	1265	867

Notes: Model 1 is a Logit model. Model 2 is a Tobit model. Standard errors are clustered at the session level.

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B.2 The effect of information

We now turn to the effect of information on agreements. Table B.5 presents failure rates and the nature of agreements depending on whether 0,1 or both parties are informed of the judgment.

Overall, we find that the probability of failure increases with the number of parties informed in the Extreme treatments. In the Uniform treatments, the picture is more subtle: the failure rate is highest when one and only one participant is informed. On the other hand, having more parties informed leads to more unequal agreements.

In Table B.5, we report the outcome of some regressions in which the effect of information is estimated across treatments.

Table B.4: Agreement across treatments and information situation.

	Nb informed parties		
	0	1	2
Treatment:			
NoConcil_Uniform			
Distance from the equal split	.632	.931	1.62
% of failed bargaining	.24	.377	.248
Concil_Uniform			
Distance from the equal split	.663	.906	1.51
% of failed bargaining	.245	.367	.258
NoConcil_Extreme			
Distance from the equal split	.967	1.21	1.99
% of failed bargaining	.237	.26	.333
ConcilExtreme			
Distance from the equal split	.615	1.07	1.99
% of failed bargaining	.169	.281	.413

Notes : .

Table B.5: The effect of information on bargaining outcomes

	(1) Failure=1	(2) Dist.
<i>Effect of having 1 participant informed in:</i>		
NoConcil_Uniform	0.136 (0.123)	0.980* (0.011)
Concil_Uniform	0.132* (0.012)	1.034* (0.014)
NoConcil_Extreme	0.019 (0.675)	0.035 (0.875)
Concil_Extreme	0.098* (0.037)	0.503+ (0.068)
<i>Effect of having 2 participant informed in:</i>		
NoConcil_Uniform	0.027 (0.832)	2.627*** (0.000)
Concil_Uniform	0.041 (0.663)	2.294*** (0.000)
NoConcil_Extreme	0.074+ (0.090)	0.727* (0.043)
Concil_Extreme	0.203*** (0.000)	1.431*** (0.000)
Controls	Yes	Yes
Sessions (Nb clusters)	24	24
Observations	1670	1184

Notes: The reference category to compute the marginal effect is "none of the parties are informed". Model 1 is a Logit model. Model 2 is a Tobit model. Standard errors are clustered at the session level.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

C Regressions omitted from the main text.

C.1 Support for Result 1.

Table C.6 reports the outcome of regressions supporting Result 1. Columns (1) and (2) report the marginal effects of conciliation on the failure rate over the distribution of judgments. The underlying models are Logit models. Columns (3) and (4) report the marginal effects of conciliation on the nature of agreements (i.e. the distance to the equal split). The underlying models are Tobit models. None of the marginal effects of interest are statistically significant, which confirms the conclusion that conciliation has no effect on agreements on average.

Table C.6: The effect of conciliation on failures and the nature of agreements.

	(1)	(2)	(3)	(4)
	Failure=1	Failure=1	Dist.	Dist.
Effect of conciliation for :				
Uniform judgement	-0.007 (0.842)	-0.030 (0.329)	-0.067 (0.573)	-0.078 (0.470)
Extreme judgement	0.016 (0.723)	0.029 (0.514)	-0.160 (0.309)	-0.156 (0.294)
Observations	1670	1670	1184	1184
Cluster (Nb sessions)	24	24	24	24
Controls	No	Yes	No	Yes

Marginal effects of reported. Columns (1) and (2) are Logit models. Columns (3) and (4) are Tobit models. Standard errors clustered at the session level in parentheses. Controls include period, the value of the judgment, a categorical variable indicating the number of informed parties and the interactions of these last two variables.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D Elicitation of social preferences and classification.

To elicit social preference, we adapt the method of Fehr et al. (2022, 2023). In this method, participants make 56 allocation decisions between them and an anonymous recipient. Each allocation decision includes 7 interpersonal allocations, from which the decision maker has to pick one. Based on their decisions, participants are then classified into 3 meaningful social preference types thanks to a classification algorithm: Inequity-averse, Selfish, and Altruist.

We selected a subset of 14 allocation decisions among the 56 allocation decisions used in Fehr et al. (2022, 2023).¹⁵ We chose this subset of allocation decisions because we wanted to keep the experiment short and because Fehr et al. (2022, 2023) found that the choices made in these 14 allocation decisions were robust predictors of participant choices in the remaining decisions. Hence, we deemed that the inclusion of the other allocation decisions would be redundant.

Table D.7 summarizes the 14 different allocation decisions. Each row represents an allocation decision. For each allocation decision, there is one allocation that maximizes the decision maker’s payoff (Allocation Max), one Allocation that minimizes the decision maker’s payoff (Allocation Min), and one allocation that equalizes payoffs (Allocation Equ). Allocation Equ is always the fourth possible allocation, midway between the two extremes Min and Max. In addition, a Slope is reported for each allocation, which is the number of euros the decision maker has to sacrifice to increase the recipient’s payoff by €1. There are 2 intermediate allocations between Allocation Max and Allocation Equ., and two intermediate allocations between Allocation Equ. and Allocation Min, which can be deducted from the fact that, to go from one allocation to the next in a given allocation decision, the decision-maker has to give up a fixed amount.

As an illustration, Figure D.1 shows the Allocation Decision 1 as presented to the participants. In this scenario, at one extreme, the Allocation Min gives €4.5 to the decision-maker and €7.5 to the recipient. The opposite extreme allocation, the Allocation Max,

¹⁵These allocation decisions correspond to the "the center budget" in their paper

gives €7.5 to the decision maker and €4.5 The allocation Equ gives both the decision maker and the recipient €6. Given the slope of -1, the decision-maker sacrifices €1 for every €1 increase in the recipient's payoff.

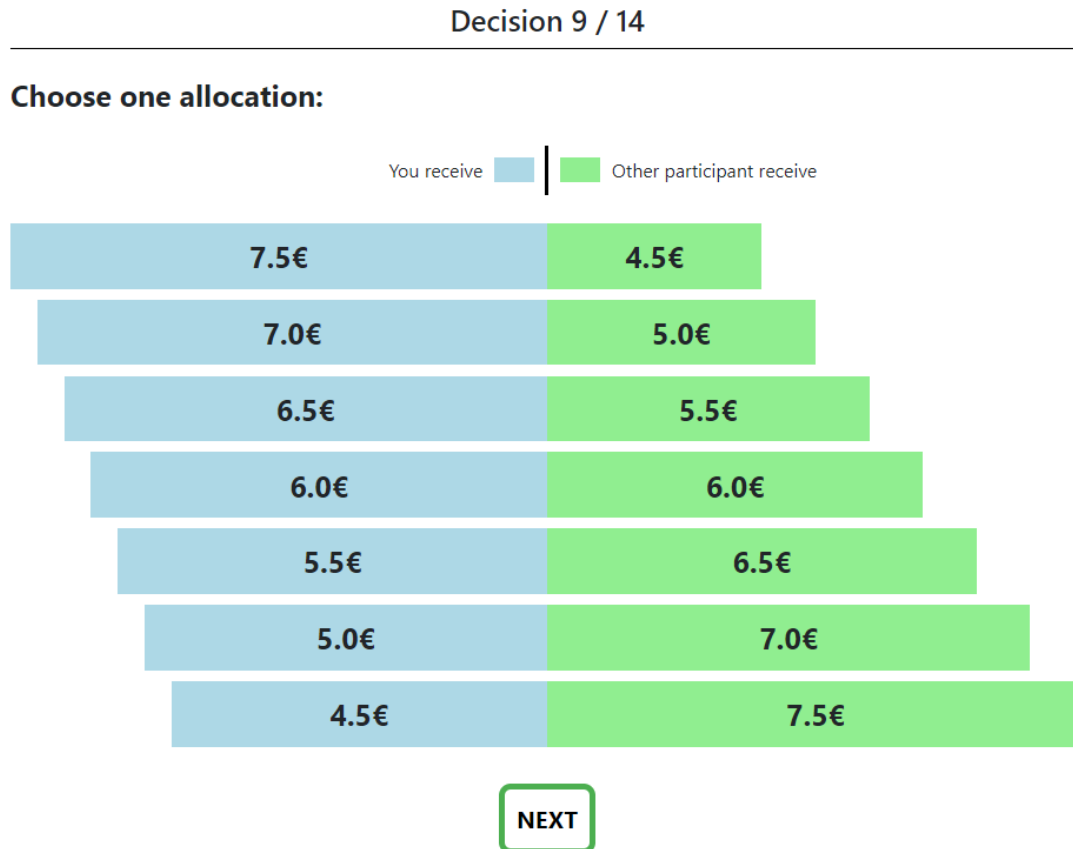


Figure D.1: An example of an allocation decision (Allocation decision 1).

Allocation decisions with negative slopes depict situations in which one has to sacrifice some of his earnings to increase the recipient's payoff. For instance, with a slope of -2, the decision maker has to sacrifice €2 to increase the recipient payoff by €1. A positive slope means that the decision-maker can increase both payoffs at the same time, and there is no tension between the decision-maker and the recipient's payoff. Note that a slope greater than one means that the recipient's payoff increases more than the decision maker's. A null (resp $-\infty$) slope means that the recipient (resp. decision maker) payoff is fixed and is not impacted by the choice of the decision maker.

Figure D.2 plots the median choices of participants in the allocation decisions with positive

Allocation decision	Allocation Min		Allocation Equ		Allocation Max		Slope
	DM	Recipient	DM	Recipient	DM	Recipient	
1	4.5	7.5	6	6	7.5	4.5	-1
2	7.5	10.5	9	9	10.5	7.5	-1
3	4.5	7.5	7.5	7.5	10.5	7.5	0
4	5	8	7.5	7.5	10	7	-0.2
5	5.5	8.5	7.5	7.5	9.5	6.5	-0.5
6	6	9	7.5	7.5	9	6	-1
7	6.5	9.5	7.5	7.5	8.5	5.5	-2
8	7	10	7.5	7.5	8	5	-5
9	7.5	10.5	7.5	7.5	7.5	4.5	$-\infty$
10	7	5	7.5	7.5	8	10	5
11	6.5	5.5	7.5	7.5	8.5	9.5	2
12	6	6	7.5	7.5	9	9	1
13	5.5	6.5	7.5	7.5	9.5	8.5	0.5
14	5	7	7.5	7.5	10	8	0.2

Table D.7: Allocation decisions in the dictator game

slopes and negative slopes. For the sake of readability, we normalize the choices in the following way to allow for representation: We set *choice* = 0 when the decision maker chooses Allocation Min, *choice* = 1 when the decision maker chooses Allocation Max, and *choice* = 0.5 when the decision maker chooses the payoff equalizing allocation.

Schematically, perfectly selfish decision-makers will always choose the allocation Max, which is located at (1,1), in the up-left corner of Figure D.2. Perfectly inequality-averse decision-makers will always choose the payoff equalizing allocation for every decision and would be found located at (0.5,0.5) of Figure D.2. Altruistic decision-makers choose the payoff equalizing allocation in allocation decisions with negative slopes, thus they sacrifice some of their endowment to increase the recipients' payoff. In contrast with purely inequality-averse individuals, they maximize their payoff for positive slope allocation, i.e. when doing so does not reduce the recipient's payoff. Thus, their concern for the recipient's payoff is not mainly driven by inequality aversion.

Of course, the choices of actual decision-makers are less clear-cut. To account for this and to avoid arbitrary classification, we use a K-median algorithm to determine the social preference type of each participant.¹⁶ K-median is a popular and widely used method for clustering, that is, partitioning a dataset into homogeneous groups. This technique attempts to divide a set of N observations into K clusters, where each observation belongs

¹⁶Results are very similar if we use a K-means algorithm.

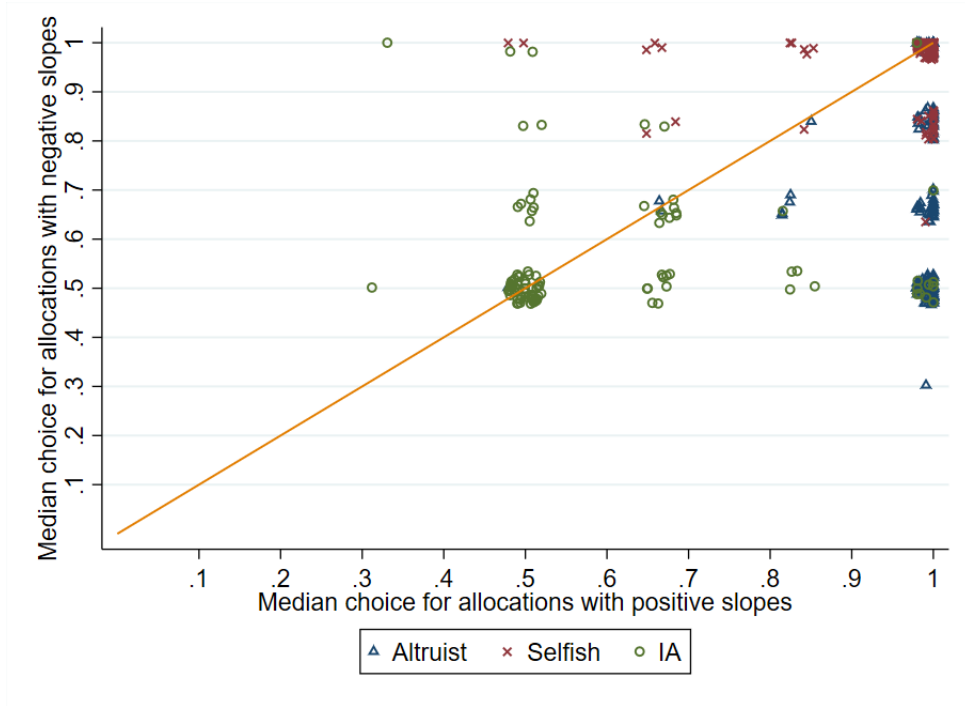


Figure D.2: Median Choice in the Allocation decision.

to the cluster with the closest median. We set 3 clusters corresponding to the 3 social preference types elicited in Fehr et al. (2022, 2023). We apply the clustering algorithm to the 14 decisions of the participants. In Figure D.2, the different social preference types are represented with different colors.

E First suggestion of the conciliators.

Figure E.3 plots the suggestions of the conciliator as a function of the judgment. Both variables are expressed as deviation from equality. We only use observations in which the suggestion of the conciliator is the first communication in the chat. We focus on these observations to avoid endogeneity: the suggestions made later by the conciliator may potentially be impacted by the offers made by the parties.

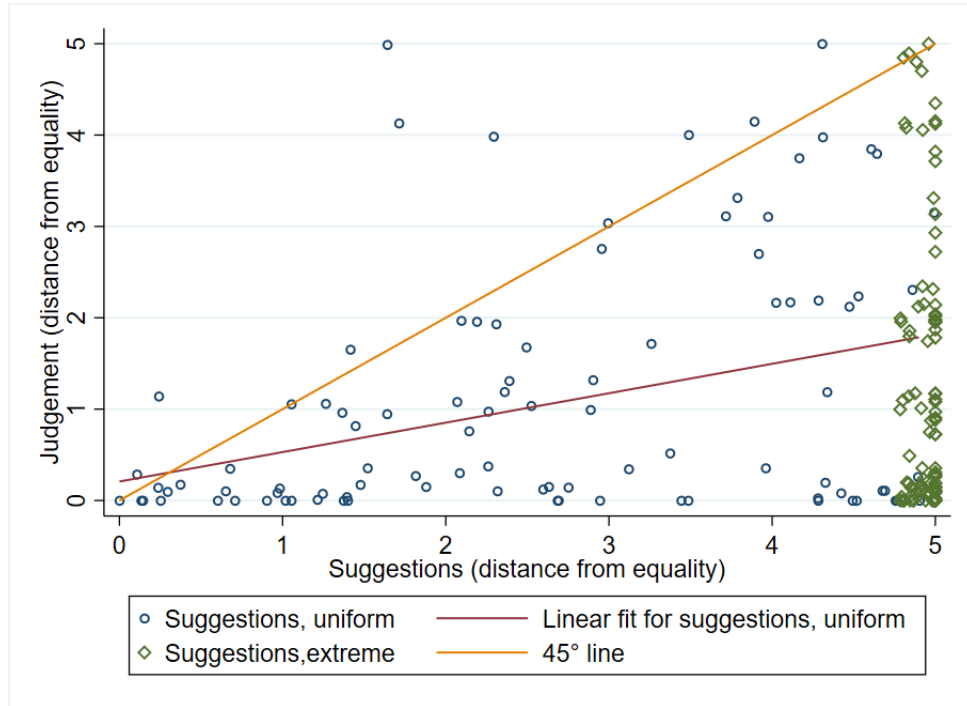


Figure E.3: First suggestions of the conciliator as a function of judgment.

Figure E.3 shows a clear positive relation between the judgment and the suggestions of the conciliators. Table E.8 reports the marginal effect of the distance from equality of the judgment on the distance from equality of the first suggestion of the conciliator. The marginal effect is positive (m.e. = 0.43, $p = 0.02$), significant, but also smaller than 1 ($p < 0.001$). This suggests that the first suggestions of the conciliators lie between equality and what would prevail in case of failed bargaining.

Table E.8: The effect of judgment on the first suggestion of the conciliator

	(1)
	Suggestion (distance from equality)
Judgment (distance from equality).	0.439** (0.002)
Controls	Yes
Sessions (Nb clusters)	12
Observations	201

Notes: Tobit model with standard errors clustered at the session level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

F Demographics and individual characteristics.

F.1 Demographics

Table F.9 reports the demographics of the participants, by experimental condition. The sample is mostly composed of a majority of student and of women. Importantly, no significant differences emerge between treatments, except for the number of participations.

Table F.9: Demographics by treatment

	% woman	% Student	Age	Monthly expenses (€)	Nb. Experiences
NoConcil_Uniform	.8	1	20	1582	1.7
Concil_Uniform	.78	.96	21	619	2.9
NoConcil_Extreme	.82	.93	21	765	1.9
Concil_Extreme	.71	.95	21	627	1.8
<i>p – value</i>	0.240	0.112	0.743	0.575	0.001

Notes: p-values are from chi-squared tests for Gender and Student status and from Kruskal-Wallis tests for age, monthly expenses and experiences.

F.2 Risk preferences

We use the method of Eckel and Grossman (2008) to elicit risk preferences. Participants had to choose one lottery in two consecutive sets of five binary lotteries displayed on their screen. The expected payoff of the lotteries ranged from €4 to €6, and variance increased with the expected payoff. Choosing a lottery with a higher expected payoff and variance reveals a higher risk tolerance. The first set is used to elicit risk attitudes in the gain

domain since expected payoffs are always positive. The second set elicits risk attitudes in the loss domains. In order to keep the expected payoff equal, for the second set of lotteries participants were endowed with €4. A screenshot of the lottery is presented in Appendix G.

Table F.10 reports the choices of participants in the risk preferences elicitation task. The upper panel reports choices in the gain domain. The bottom panel reports choices in the loss domain. No significant difference emerges between treatments (Chi-squared test: $p = 0.931$ in the gain domain, and 0.649 in the loss domain).

Table F.10: Loterie choice in the Eckel Grossman risk preference elicitation task.

	Gain domain.				
	0	1	2	3	4
NoConcil_Uniform	25	21	8	8	18
Concil_Uniform	49	29	13	14	18
NoConcil_Extreme	28	23	12	9	22
Concil_Extreme	43	27	14	10	23
	Loss domain.				
	0	1	2	3	4
NoConcil_Uniform	19	18	9	11	23
Concil_Uniform	37	18	13	15	40
NoConcil_Extreme	23	17	6	9	39
Concil_Extreme	25	21	11	20	40

G Screenshots from the experimental software.

Encryption task: 1/100

Convert the number 4 into a letter

K	J	I	C	L	V	R	W	G	Z	M	A	Q	F	B	X	T	S	N	U	Y	H	E	P	D	O
21	10	16	2	7	18	12	13	20	3	9	8	26	17	6	1	5	14	24	23	22	4	15	11	25	19

you answer:

SUBMIT

Figure G.4: Screenshot of the encryption task.

Time left to complete this page: 0:47

You must divide a loss of 10€ between yourself and the other participant A.

REMINDER: in the absence of an agreement

- **You will pay €2.0**
- **The other participant A will pay. €10.0**

Remember that the other participant A has a 50% chance of receiving information about the distribution in the absence of an agreement

Participant B always has this information.

If you accept the offer from the other participant A, you will have to pay **9€** and she/he will pay **1€**

N°	YOU OFFER TO PAY:	PARTICIPANT B SUGGESTS THAT YOU PAY:	THE OTHER PARTICIPANT A OFFER THAT YOU PAY:
3		6.0€ the other participant A will have to pay. 4.0€	
2			9€ She/he will have to pay 1€
1	3€ the other participant A will have to pay. 7€		

Make your offer:

SUBMIT MY OFFER

I ACCEPT THE AGREEMENT OF THE OTHER PARTICIPANT A.

Figure G.5: Screenshot of the bargaining interface (with a conciliator, called “participant B”).

For this choice situation, you receive an allocation of 8€.

- You must choose one of the 5 lotteries presented in the table below.
- Each lottery is characterized by 2 possible payoffs, each with a 50% chance of occurring. Note that some payoffs are losses.
- If this part is selected for payment, the program will randomly determine which payoffs you will receive from the 2 possible payoffs of the chosen lottery.
- This payment will be added to or deducted from the 8€ allocation.

Select you preferred option:

A	50% chance of losing 4€ and 50% chance of losing 4€	<input type="radio"/>
B	50% chance of losing 5€ and 50% chance of losing 2€	<input type="radio"/>
C	50% chance of losing 6€ and 50% chance of getting 0€	<input type="radio"/>
D	50% chance of losing 7€ and 50% chance of getting 2€	<input type="radio"/>
E	50% chance of losing 8€ and 50% chance of getting 4€	<input type="radio"/>

Figure G.6: Screenshot of the risk preferences elicitation in the loss domain.

- You must choose one of the 5 lotteries presented in the table below.
- Each lottery is characterized by 2 possible payoffs, each with a 50% chance of occurring.
- If this part is selected for payment, the program will randomly determine which payoffs you will receive from the 2 possible payoffs of the chosen lottery.

Select you preferred option:

A	50% chance of getting 4€ et 50% chance of getting 4€	<input type="radio"/>
B	50% chance of getting 6€ et 50% chance of getting 3€	<input type="radio"/>
C	50% chance of getting 8€ et 50% chance of getting 2€	<input type="radio"/>
D	50% chance of getting 10€ et 50% chance of getting 1€	<input type="radio"/>
E	50% chance of getting 12€ et 50% chance of getting 0€	<input type="radio"/>

Figure G.7: Screenshot of the risk preferences elicitation in the gain domain.

H Instructions sample

H.1 Part 1 (common to all experimental conditions).

Thank you for participating in this decision-making experiment. You are not allowed to communicate with other participants during the entire session. All your decisions are anonymous. Your decisions can earn you money. Your earnings will be paid to you via Lydia, according to the usual Anthro-po-Lab procedure. The experiment is composed of 3 parts. The instructions below describe the content of Part 1. The instructions for Part 2 and Part 3 will be distributed to you at the end of the previous parts. We will read these instructions together. If you have any questions, raise your hand and an experimentalist will come and answer you privately.

Part 1:

In this part, you must perform a conversion task 100 times. The task is to convert a number into a letter of the alphabet. The figure below¹⁷ represents the interface of the conversion task. To find out the letter corresponding to the number displayed, you must refer to a conversion table that will be presented on your screen. After entering the letter corresponding to the number displayed in the "your answer" field, you must validate your answer. If your answer is correct, you will move on to the next task, otherwise, you will have to enter a new letter. With each new number, the conversion table changes. For this game you receive a €15 prize, which is your prize for the next game.

H.2 Part 2.

H.2.1 NoConcilUniform

In this section, you will participate in 12 rounds of a game. In each round of the game, you will be randomly and anonymously paired with another participant in the session. You cannot be paired with the same participant in two consecutive rounds. The first two rounds are for training purposes and will have no impact on payment. One of the 10

¹⁷See Figure G.4

subsequent rounds (rounds 3-12) will be randomly selected by the program at the end of the experiment to determine your payment.

Game description:

In this game, you must allocate a loss of €10 between you and your co-participant. This loss will be deducted from the endowment that you acquired in the previous part, based on the allocation you agree upon. You have 60 seconds to reach an agreement. To do this, you use the interface described below.

In the absence of an agreement at the end of the 60 seconds, the program will select an allocation at random. All possible allocations (in steps of 10 cents) have an equal chance of being selected by the program. In the absence of an agreement, in addition to the loss involved in the allocation selected by the program, you and your co-participant will each lose an additional €1.

Before the start of each round, you have a 1 in 2 chance of being informed of the allocation selected by the program in the event of a disagreement. Your co-participant also has a 1 in 2 chance of being informed. You do not know if your co-participant is informed.

Interface description:

Using the interface shown on the second sheet¹⁸, you can propose a possible allocation of the loss to your co-participant. To do this, you enter the amount you propose to bear if this allocation is accepted by your co-participant in the dedicated field. You can enter any proposal from €0 to €10, in steps of 10 cents. This proposal is communicated to your co-participant through the table. For example, if you enter "3" in the field, your co-participant is informed that you propose to bear €3 of the loss and that they will bear €7. If you enter "8", your co-participant is informed that you propose to bear €8 of the loss and they will bear €2.

Symmetrically, your co-participant can also make allocation proposals. You are informed of their proposals through the right column of the table. Note that the sum of the losses borne by you and your co-participant always equals €10.

When you receive a proposal, you can either make an alternative proposal or accept it.

¹⁸See Figure G.7

To accept a proposal, you click on the "I accept the other participant's proposal" button. Note that clicking this button means you agree with your co-participant's last proposal, even if you made a proposal in the meantime. Clicking the agreement button leads to the application of your co-participant's last proposal and ends the round.

If, at the end of the 60 seconds, no proposal is accepted, then the allocation selected by the computer is applied. You and your co-participant will also have to pay an additional €1 each.

H.3 ConcilUniform

In this part, you will participate in 12 rounds of a game. Before the first round of the game, you will be assigned a role:

- 2 out of 3 participants will be "participant A."
- 1 out of 3 participants will be "participant B."

Roles are randomly determined for the entire Part 2. You will be informed of your role on the screen. In each round of this game, you will be randomly and anonymously grouped with two other participants from the session. In each group, there will be two participants A and one participant B. You cannot be grouped with the same participants in two consecutive rounds. The first two rounds serve as practice and will have no impact on payment. One of the next 10 rounds (rounds 3 to 12) will be randomly selected by the program at the end of the experiment to determine your payment.

Game description:

If you are one of the two "participants A": you must allocate a loss of €10 between you and the other participant A in your group. This loss will be deducted from the endowment you acquired in the previous part, depending on the allocation on which you agree. You have 60 seconds to reach an agreement. To do so, you use the interface described below. In the absence of an agreement after 60 seconds, the program will select an allocation at random. All possible allocations (with a step of 10 cents) have an equal chance of being drawn by the program. In the absence of an agreement, in addition to the loss involved in the allocation selected by the program, you and the other participant A in your group

each lose an additional €1. Before the start of each round, you have a 1 in 2 chance of being informed of the allocation selected by the program in the event of disagreement. The other participant A also has a 1 in 2 chance of being informed. You do not know if the other participant A is informed. If you are the "participant B": you can suggest allocations to the 2 participants A in your group. Note that the conclusion of an agreement or the final allocation has no impact on your earnings. You keep the entire €15 earned in the previous part. Participant B is always informed of the allocation randomly selected by the computer program, which is applied in the absence of an agreement between participants A.

Interface description:

You interact through the interface represented on the second sheet.¹⁹ If you are one of the two "participants A": you enter in the dedicated field the amount that you propose to bear if this distribution is accepted by the other participant A. You can enter any proposal from 0 to €10, in steps of 10 cents. This proposal is communicated to the other two participants in your group through the table. For example, if you enter "3" in the field provided for this purpose, the other two participants in your group are informed that you propose to bear €3 of losses and that the other participant A bears €7 of losses. If you enter "8", they are informed that you propose to bear €8 of losses and that the other participant A bears €2. Symmetrically, the other participant A can also make proposals for distribution. You are informed of their proposals via the right column of the table. Note that the sum of the losses borne by you and the other participant A is always equal to €10. When you receive a proposal from the other participant A, you can either make an alternative proposal or accept it. To accept a proposal, you click on the button "I accept the proposal of the other participant A". Note that clicking on this button means you agree with the last proposal of the other participant A, even if, in the meantime, you have made a proposal or the participant B has made a suggestion. Clicking on the agreement button leads to the application of the last proposal of the other participant A and ends the period. Note that it is not possible to accept a suggestion from participant

¹⁹See Figure G.7.

B.

If, at the end of the 60 seconds, no proposal is accepted, then the distribution selected by the computer is applied. You and the other participant A will have to pay an additional 1€ each.

At the end of the 12 periods, one of the periods from the 3rd to the 12th period will be randomly selected by the program to determine your payment. If you are one of the two "participants A", your payment will depend on the distribution you have agreed upon with the other participant A during this selected period. If you are "participant B", your payment will not depend on the agreements made by the two "Participants A" in your group during this selected period.