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Social interaction and negotiation outcomes: An experimental approach☆

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ABSTRACT

We study experimentally the impact of pre-play social interactions on negotiations. We isolate the impact of several common components of interactions: conversations, food, and alcoholic or non-alcoholic beverages. Participants perform a standardized (complex or simple) negotiation under six conditions: without interaction; interaction only; and interactions with water, wine, water and food, and wine and food. We find that none of the treatments improves the outcomes over the treatment without interactions. We also study trust and reciprocity, where we find the same lack of superiority of interaction.

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1. Introduction

Many transactions in economic life take place after social interactions. They are central at the start of business, government, and personal negotiations and other social, political, and economic processes. These interactions are important in the culture of organizations all over the globe.

As an example of their perceived importance, the following quote from Harvard's Program on Negotiation[1] is illustrative:

The reciprocal nature of trust reinforces the value of taking time to get to know the other party and build rapport before you begin to negotiate. Don't assume that you can form a bond simply by exchanging a few friendly e-mails before meeting in person. Rather, try to forge a personal connection by meeting for an informal lunch or two.

Policymakers have taken this kind of advice to heart. For example, business meals tend to be tax-deductible at least in part. The IRS considers that, in general, 50% of such expenses are deductible.[2] The HMRC allows deducting the part of the expense that is "wholly and exclusively" for the purpose of generating profits.[3] However, this is not only a matter for private businesses. Government offices and universities also subsidize business meals. Given this perceived importance, it is rather surprising that there has been very little research effort to ascertain the actual impact of this practice with a view to improving substantive economic outcomes.

Real negotiations often involve many issues over which participants usually have diverse preferences. Under incomplete information about those preferences, it is easy for negotiation outcomes to reach inefficient solutions. Our main goal is to investigate if social interactions with strangers improve the efficiency of negotiations through trust building. One initial difficulty to achieve our goal is that these interactions are complicated processes involving many components. The potential success of the complete process might not be able to tell us the role played by its different elements. For example, a business meal preceding a negotiation involves communication and other aspects, such as food and

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[2] https://www.irs.gov/newsroom/heres-what-businesses-need-to-know-about-the-enhanced-business-meal-deduction Retrieved on September 9, 2022
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beverage intake. Of course, negotiations preceded by communication are commonplace outside business as well, such as in ceasefire or peace talks, or in political negotiations. As stated in Seabright (2006):

"A telling piece of evidence in support of the signaling theory of laughter is the way in which, across all kinds of cultures in the world, people who have made a business deal with each other tend to seal the deal by having a drink together […]. At the same time as it disables people's capacity for exercising trust wisely, alcohol enables people to inspire trust by stimulating that excellent signal of positive affect, namely laughter, that is not under direct voluntary control."

For this reason, we designed an experiment that would allow us to distinguish the effect of different factors on the negotiations. Our main finding is straightforward. On the one hand, we find no significant effect of any of our treatments with social interaction over the baseline of no interaction at all. In the words of the HMRC, the part of the expense that is wholly and exclusively for the purpose of generating profits is, *on the basis of our experiment*, zero. The same result arises for trust and reciprocity. In the next section we use the previous literature to provide a framework to understand our results. For example, *in our particular setting*, they contradict the idea that cheap talk can solve coordination problems to enhance efficiency.

Our experiment has novel features compared to previous experiments reviewed later in our paper in three main respects: first, we carefully control for all possible effects taken in isolation. We construct a proper and controlled setup to study social interactions around a table and introduce wine and meals in that particular environment to measure whether the use of those additional activities may affect social interactions. Second, we do so with a relevant, very big yet controlled subject pool, and strong monetary incentives: more than 90% of subjects are meant to become managers or entrepreneurs and undertake negotiations in their future businesses. The game they play is a "usual" class exercise, which also minimizes both selection biases and experimenter demand effects. Finally, and very importantly, our experiment doesn't just measure inebriation and no matter which type of alcohol intake as the vast majority of the papers we cite hereafter (where participants are usually required to drink various types of alcohol, in most cases alone, or are already inebriated when recruited but what they drank is unknown). To inebriate subjects in our experiment, we use a sufficient quantity (the standard "three glasses") of good quality red wine, which is a cultural and social feature in negotiations in France, and create a natural social drinking environment by means of an innovative wine tasting that ensures inebriation without constraints.

The participants in our experiment were master's students at the Burgundy School of Business in Dijon.[4] This is an elite business school in the Bourgogne region of France. The participants were recruited for a wine tasting activity followed (or preceded) by some games. After gathering, they had 30 min to interact, except in the control treatment, where there was no interaction. Then, they read the experimental instructions. Note that because the participants read the instructions after the interaction phase, the interaction takes place without their knowledge of the games they will play later.

During the interaction phase, participants were assigned randomly to only one of five treatments, or to the control. The treatments differed depending on the availability of food and drinks. They were as follows (obviously all of them involve interaction): interaction only, water, wine, food and water, and food and wine. After the interaction, they participated in a four-player strategy-method trust game (Berg, Dickhaut & McCabe, 1995). We made an extra effort to ensure the setting was as natural as possible. For instance, we allocated desks and participants in circles of four facing each other. The unexpected high level of trust observed in the entire experiment indicated that our efforts were successful.

After the interaction phase, participants took part in an incentivized negotiation of a kind that is common in negotiation classes. They negotiate over a labor contract with many attributes, each of which carries a different number of points for each possible agreement. Participants' payments were a function of their total points, but they knew only their own points. This incomplete information about the others' points, and hence the possible beneficial trade-offs, was meant to create the opportunity for social interaction to increase trust and efficiency. Parties can find solutions that were not obvious if they exchange information.

We study two forms of negotiation (between subjects): Half of the subjects played a "hard" negotiation involving five issues; the other half a "simple" negotiation with two issues.[5] We administered a de-briefing questionnaire at the end of the experiment.

Regarding the form of pre-play interaction, we ran many treatments because our prior belief indicated that some form of pre-play interaction would indeed improve negotiations, and thus we wanted to find out the (possibly synergistic) impact of the different elements. As it turned out, nothing appears to work better than moving directly into negotiations (no interaction). This is true at the pair level, that is, there is no gain in the total number of points achieved in the negotiations. It is also true at the individual level, as there seems to be no higher dispersion, nor a particular side that gains through communication. We conjecture that the initial interactions serve a psychological need to lighten the load of an unpleasant task. We could say that pre-play social interaction is more of a consumption good than a production input. As such, its tax status might need a revision if future research on this topic confirms our results.

With respect to trust, no treatment significantly improved the level of trust from the baseline treatment: trust in the "no interaction" treatment was either equal or, in a couple of cases, superior to every other treatment.

In reciprocity we do not find any effect. Hence, there are no significant gains for more complex interaction settings (vs. no interaction at all).

Our results have relevant policy implications. There is a general belief both in the business world and in government that interactions benefit their organizational performance. We have cast doubt on that belief.

The structure of the paper is as follows. Section 2 reviews the related literature. Section 3 describes the experimental design. Section 4 establishes the results. Section 5 concludes.

2. Related literature and analytical framework

The discussion of the literature will help to provide a framework to understand the experiment and formulate the hypotheses for the work. Our departure point is that different models have different predictions about the impact of communication before negotiation on its efficiency. Our overarching aim is to test which of those theories are more likely to be predictive in the real world. We also want to do it in a diverse set of environments; and with negotiation simulations that are more realistic than in most of the previous literature.

A first group of papers in game theory (and related experimental economics) suggests that communication can improve outcomes in games with multiple equilibria. Kim and Sobel (1993) provide an

[4] See the discussion in the "Experimental design" section on why this specific subject pool was adequate for the purpose of our paper.

[5] It may not be clear ex-ante whether two or five issues are "more difficult" since the trade-offs, and thus agreement possibilities, generally multiply with a higher number of issues. But the cognitive load of finding those agreements is certainly higher. And our results show that efficiency is indeed higher in what we call "simple negotiations."

argument based on evolutionary games,[6] which is experimentally verified in Blume and Ortmann (2007).[7] An additional theoretical reason for an increase in efficiency through communication comes from the fact that asymmetric information yields reduction in efficiency in bargaining games (Samuelson, 1984), but individual preferences for truth-telling (or lying aversion) often yield improvements in efficiency with respect to a benchmark with standard preferences (Lundquist, Ellingsen, Gribbe & Johannessonl, 2009). As an experimental verification, Valley, Thompson, Gibbons and Bazerman (2002) studied a double oral auction with and without communication. They found that communication led to higher levels of efficiency than predicted by theory.[8] We summarize this literature's implication under the *efficiency-enhancing motivation* for pre-play communication in negotiation.

An alternative view of the attractiveness and popularity of pre-play communication in games arises from another body of literature that emphasizes the emotional costs of negotiation (Babcock, Gelfand, Small & Stayn, 2006; Brooks & Schweitzer, 2011, and Bowles, Babcock & Lai, 2007). For example, Gago (2019) shows experimentally that individuals are prepared to pay a cost to avoid bargaining even when it delivers higher payoffs. *Note that this literature also emphasizes that it is more difficult for females to negotiate with males*, which guides our decision to separate groups into all-male and all-female. We summarize this literature's implication under the *anxiety-reducing motivation* for pre-play communication in negotiation.

If the first strand of literature is correct, the popularity of pre-play communication in negotiations would tend to enhance negotiation efficiency, while the second literature predicts that pre-play communication would serve merely to assuage the anxiety that arises in negotiations.

Similarly, we can use the results from previous literature to guide our other design choices. One of them relates to the consumption of alcohol, since it is often consumed in pre-play negotiation meetings. Alcohol consumption has been shown to enhance trust (Attanasi, Bortolotti, Filippin, Pace & Urso, 2013).[9] Interestingly, from the latter paper it appears that it is not generalized trust but instantaneous trust related to the specific group sharing the substance.[10] In the laboratory, Bregu, Deck, Ham and Jahedi (2017) found more generous dictators. These results would point to a positive effect of alcohol on negotiation efficiency through the *positive impact on trust and altruism of alcohol*.

However, Corazzini, Antonio and Vanin (2015)) and Zak, Hayes, Paulson and Stringham (2021) found that alcohol intoxication increases impatience and makes subjects less altruistic and inhibits cooperation. This is consistent with the findings of Schweitzer and Gomberg (2001) who showed that alcohol lowers the efficiency of negotiation outcomes due to the use of more aggressive tactics, less integrative tactics (Thompson, 1991) and by making more mistakes. Moreover, Wang, Rao and Houser (2017) found that exercising willpower to limit alcohol consumption in cheating, impulse, and self-control games is moderated simply by alcohol intolerance and gender. Finally, Au et al. (2016) reported that mild alcohol use improves bargaining efficiency, and that this effect can be caused by impairment in information processing ability. In sender-receiver games, Au, Lim and Zhang (2021) also showed that alcohol consumption lowers both the lying cost and the degree of sophistication when interpreting received messages. These results point in the direction of a *negative impact on aggressiveness and cognitive ability of alcohol*.

Finally, alcohol may be able to relieve tension arising from the unpleasantness of negotiations. As Sher and Grekin (2007)) noted: "under conducive circumstances, alcohol can strongly reduce negative emotions and increase positive emotions."[11] In the same vein, Wang and Houser (2021) showed, by means of a lab-in-the field prisoner dilemma game with pre-play communication, an experiment, and a guilt aversion and alcohol myopia model, that intoxication increases promise-making but has no effect on promise-breaking. This points to a *stress reduction function of alcohol on negotiations*.

Food is often consumed together with alcohol in pre-play negotiations. We introduce it in the experiment mostly for the sake of realism. We should note, however, that there is a literature focusing on the effects of glucose in decision-making. Gailliot and Baumeister (2007) used a variety of sources to claim that lower glucose levels reduce self-control. However, a meta-analysis by Dang (2016) casts serious doubts on the view that glucose reduces the ability to self-control.

3. Experimental design

We invited the participants to our experiment to participate in wine tasting activities, followed (or preceded) by modified versions of the classic trust game (Berg et al., 1995) and of the negotiation game introduced by Schweitzer and Gomberg (2001).[12] Our experiment had a sequential structure intended to fulfill two requirements: constructing a proper setup to study social interactions around a table and introducing wine and meals in that particular environment to measure whether the use of these additional activities may affect social interactions.

We recruited 568 participants from among the students enrolled in the first year of the Master Grande École at the School of Wine and Spirits Business and in the Burgundy School of Business (BSB) in Dijon, France, in November 2015 and November 2016. We chose the timing (very soon after the start of classes) and participants (first-year students) so that, together with random matching, we minimized the chance that participants were in groups whose members had already established a relationship. The BSB has a good index of social diversity (it is ranked third in France), which means that participants are quite a good representation of the French population (30% of students at BSB are recipients of social scholarships; the highest percentage in France).[13] The sample is also externally valid since more than 90% of these students are expected to become managers or entrepreneurs and be in charge of negotiations in their future businesses.

Participants were invited to participate in a wine tasting event (something that occurs often at the BSB) and told they would also play some games. The invitations to such events (and more generally to paid experiments) are familiar and in accordance with the ethical standards

[6] See also Farrell (1987) and Wärneryd (1991).

[7] See also Cooper et al. (1992), Charness (2000) or Clark et al. (2001) for coordination games, Palfrey and Rosenthal (1991) for public good games, and Charness and Dufwenberg (2006) in trust games. Berkman et al. (2015) study the role of socialization on cooperation.

[8] This is not universal, Forsythe et al. (1991) showed that communication during a bargaining game did not improve the efficiency of negotiated outcomes.

[9] A caveat in this case is that this is a survey, not an experiment, and certainly not a game.

[10] Alcohol has also positively associated with risk taking (Proestakis et al. 2013; however, Burghart et al. 2013 found different results for men and women) and rejection of unfair offers (Morewedge et al. 2014).

[11] Of course, they also note that "Unfortunately, these benefits are often accompanied by considerable costs such as short-term negative emotional consequences. In addition, chronic, heavy alcohol use often leads to tonic changes in emotional state that may further motivate drinking. From this perspective, alcohol dependence may be considered, in part, a disorder of emotional regulation."

[12] No participant was deceived. All the participants in the experiment did eventually take part in a wine tasting session. Those for whom wine was not part of the treatment had the wine tasting after the experiment.

[13] However, note that BSB students enter the school after two years of intensive preparation in special schools called *préparatoire* to which they are admitted based on their grades and an exam. Also, at the end of the two years, they need to pass a highly selective entrance exam to be admitted to BSB (which has 4000 applicants for 450 places). This process means that BSB students are very good students and they come from good high schools, mostly from big cities.

of drinking alcohol inside the school (BSB has a main program in the School of Wine) and minimize both selection biases and experimenter demand effects. As is usual, we reminded participants not to consume alcohol before arriving at the study, not to eat for 2 h prior to the experiment, and to bring a valid form of identification to verify their age. Most experiments started at 11 am.[14] We chose the timing on the advice of wine tasting experts from the school, because the mouth is best prepared two to three hours after breakfast. The experiment lasted 1 hour 30 min on average, including reading instructions, answering comprehension questions, decisions, and payments. Participants earned, on average, €20.50 (min €5, max €38) in addition to the participation fee of €5, which are very strong incentives.[15] We assigned participants to a treatment or a session randomly upon arrival. One participant was involved in only one session and one treatment in a typical between-subjects design.

Participants arrived at the laboratory and waited in the corridor as they usually do for experiments. We assigned them anonymous numbers corresponding to the numbered places where they were to sit. However, the numbers were assigned (without specific emphasis) in such a way as to ensure that four same-gender individuals sat together in a group in the lab.[16] In some sessions at the beginning of the experiment, participants were seated in individual isolated cubicles, while in other sessions they were seated in four-person isolated cubicles according to the treatment. Because there were some no shows, some four same-gender groups were incomplete. These participants were seated in mixed groups of four people and allowed to participate in the experiment, but the data from the mixed groups (32 participants) are not considered for the analysis.[17] The final sample comprised 536 participants.

The interaction phase (see Fig. 1 below) consisted of a 30-minute period of (pre-play) communication. In other experiments on pre-play communication, (see, e.g., Bornstein & Rapoport, 1988; Bochet et al. 2009 for public good games, and Zultan, 2012 for ultimatum games) the period of communication is far shorter, just a few minutes. We thought that our more complicated game required a longer period to build the right amount of trust but also limited it at 30 min to prevent the effects of alcohol intake from vanishing. Moreover, business lunches usually take 1 hour 30 min in France, which is the total length of our experiment. Some of our participants did not participate in the interaction phase. They are the baseline.

Participants involved in the interaction sessions sat in four-person isolated cubicles and were allowed to talk. We divided this phase into two conditions:

- in one condition, the interaction phase was unstructured – participants simply engaged in pre-play verbal communication.
- in the other condition, the interaction phase was structured by its concomitance with the tasting phase, i.e., the introduction of liquids with or without food (nibbles). This could be interpreted as a team-building exercise requiring communication that participants do together.

Thus, pure interaction can be viewed as unstructured communication and tasting as structured communication. The reason for these variations is that we do not have a good theory to explain what exactly it is about communication that may help negotiation, so we needed to test different formats. In the interaction phase, our players do not know the games they will play. In real life (and in some of the cited papers), pre-negotiation interaction is often done knowing a negotiation phase comes later. We chose this format for two reasons. First, if we tell participants what they are going to do, they may start negotiating before the negotiation, and we lose some control over the activity they do. The second, and more important one, is that we conjectured (starting from our initial motivating quote) that communication is useful "to get to know the other party and build rapport before you begin to negotiate" and that does not require that participants know why they are getting to know each other. We therefore opted for a minimal setting for pre-play communication.

To avoid any deception, all our participants took part in the Tasting Phase, either at the beginning or at the end of the experimental session. Participants were presented with three standard INAO glasses containing the standard quantity of tasting liquids (100 ml each). This is the exact quantity allowing inebriation at the no-driving point in France (0.05) and is, in consequence, the quantity declared to be consumed during usual business meals usually taking place at noon and after which participants are supposed to drive after resting. The Tasting Phase could have four conditions: the liquid contained in the glasses was water, wine, and in some tasting exercises wine or water were accompanied by side nibbles. Glasses were presented in a "blind" tasting condition, i.e., without any indication of the label, price, or other identifying information about the liquid. Glasses were only identified with neutral numerical codes. Participants were instructed to indicate on an individual answer sheet which glass of liquid they preferred at three specific moments: after they took the first gulp from each of the glasses, after the 5th gulp, and after the 10th gulp. This procedure is standard in tasting exercises, as perception changes with time and sensorial familiarity. However, as to ensure alcohol intake without constraint, we did not force consumption, but motivated the intake of liquids as participants were not allowed to spit by default (spitting receptacles were not provided). This is a major point of difference with all previous studies cited here and conducted in the lab, in which participants are specifically forced to drink (and sometimes they also drink placebos), which is not consistent with an externally valid context meant to mimic real settings.

At the end of the tasting phase, participants were instructed to leave their glasses and the answer sheets on the table at the exact same places they were when they arrived. After finalizing the sessions and before the participants left the room, breathalyzer tests were conducted on everyone and each participant's results were recorded (this is the standard, valid, and non-invasive way to measure inebriation). Participants were not provided their scores; however, inebriated participants (with a score higher than 0.25) were asked to remain in the laboratory to watch a movie, as is the standard procedure in tasting sessions. The average consumption was 246 ml, with which most of the participants drank almost the entire content (the maximum was 300 ml). Despite the high consumption, only 15% reached the legal maximum score of 0.05 for driving, and just five (2.8%) had a score higher than 0.25. After the participants left the room, we collected the answer sheets and measured the remaining liquid in each glass to have another precise indication of the liquid intake.

The Trust Game/Risk Preferences elicitation phase consisted first in a variant of the traditional trust game. In a (sequential) Trust game, two players played the following roles: the sender (S) is endowed with certain money, P. S may send any fraction x of P (even nothing) to the other player, the receiver (R). Transferred money is tripled, R is entitled to return any amount (even 0). Rules are common knowledge. We interpret S's choices as a signal of trust (the higher the better) while R's choices indicate reciprocity (idem). In our variant, participants read the experimental instructions individually in their own cubicle. Every subject had an endowment of 10 euros. They played a double role: every participant is both S and R. Each of them played a Trust Game with the

[14] Two sessions needed to be scheduled after 11 am due to room availability issues.

[15] These incentives are considered high in France where students earn, on average 7 euros/hour in experiments.

[16] To be more precise, say the session consisted of X people, X/2 men and X/2 women. We paired them randomly in each group. Numbers from 1 to X/2 were distributed randomly to the men, and numbers between X/2 + 1 and X to the women (or vice versa). Participants were then called by numbers to go sit at a specific table.

[17] We conducted analyses with these omitted groups for robustness. Results, available upon request, are not affected by their inclusion.

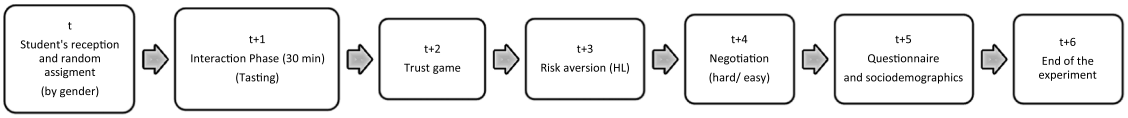


Fig. 1. Experimental phases ($t + 1$ is removed for Baseline - No Interaction).

other 3 players from his or her group (sending and receiving, instructions available in Appendix). One decision was paid randomly. Participants were also asked to reveal their expectations about the behavior of others. Participants then completed a Risk Preferences elicitation task (incentivized) following the standard procedure (multiple prices lists) proposed by Holt and Laury (2002). To avoid order effects, no feedback was revealed in this phase until the end of the game.

The negotiation phase consisted of a variant of the negotiating exercise of Schweitzer and Gomberg (2001). Participants were paired two by two in same-gender dyads. We separated the genders to avoid the complicated issues that arise from inter-gender negotiations (see, e.g., Babcock & Laschever, 2009, Bear & Babcock, 2012, Eckel, Oliveira & Grossman, 2008, Stuhlmacher & Walter, 1999). The exercise included two roles: an employer and a placement agent who negotiate over a compensation package for a prospective employee. The negotiation involved a Hard or an Easy negotiation condition, consisting of two or five issues (wage, bonuses, trips, etc.) and included opportunities to create joint gains. Participants were then randomly assigned to the role of either agent or employer. They were described their role and were allowed to make notes on their confidential information sheets. The exercise involved structuring a job offer and closing a deal for a previously interviewed candidate. The job description and candidate's resume are included in Appendix A (Experimental instructions – Hard negotiation, Employer). Both participants received private information describing their interests and how these interests converted to point values. The last page of the instructions was a table of point values including one of the two columns of values represented in the payoff table in the Appendix. Participants were informed that the points they earned in the negotiation would be converted to cash at an exchange rate of 10 points to 1 euro, and that they would earn nothing if they failed to reach an agreement. Once participants reached an agreement, we collected their agreement sheets.

In the Questionnaire Phase we collected data on the questions used in Schweitzer and Gomberg (2001): participants were asked about the negotiation process, their perceptions of how alcohol had affected their negotiation,[18] and general demographic information. Measurements of their height and weight were also taken. Finally, participants were asked demographic information, such as height, weight, age, and gender.

These different phases lead to several treatments (all these variations occur at $t + 1$ – see Fig. 1): no-interaction (*Baseline*), unstructured communication (*Comm* for short), and structured communication (*Comm+H2O, Comm+Wine, Comm+H2O+Food, Comm+Wine+Food*).

The *Comm+H2O* treatment mimics business meals with water as the base liquid. We ran three variations of this treatment: *Comm+Wine* (identical to the former *Comm+H2O* with wine instead of water); *Comm+H2O+Food* (identical to *Comm+H2O* plus a nibble) and *Comm+Wine+Food*, which combines wine and the nibble.

- In the *Baseline* treatment ($t + 1$ is absent), participants only participated in the Trust Game, Risk, Negotiation, and Questionnaire. To avoid deception, the tasting phase was done at the end of the experiment.
- In the *Comm* treatment, the sequence of phases consisted of Interaction, Trust Game, Risk, Negotiation Game, and Questionnaire. Again, the tasting phase was done at the end.
- In the four structured communication treatments (*Comm+H2O, Comm+Wine, Comm+H2O+Food, Comm+Wine+Food*), the phases were as reflected in Fig. 1: Interaction and tasting (simultaneous), Trust Game, Risk, Negotiation, and Questionnaire.

All in all, our setup was intended to put participants at ease and make them feel relaxed, and to make the situation as natural as possible. Although the sessions were conducted in the laboratory, the physical allocation of desks in circles, the position of participants facing each other and so on had the purpose of reducing the awkwardness of the setting. Photos of the session can be seen in the Appendix.

Table 1 describes the number of participants. The number of independent observations per treatment is indicated in parentheses.

Table 2 shows the summary statistics by treatment for the baseline characteristics. We focus on four observable characteristics of the participants: Gender, Size, Weight, and Risk aversion (MPL). The differences are computed with respect to the control treatment (*Baseline*) where negative values indicate that the characteristic in question is larger in the treatment than in the control.

Apart from participants' *height* in the *Comm* treatment, which is marginally larger ($p = 0.098$) than in the *Baseline*, overall, the balance tests indicate that assignment to different treatments can be considered random, that is, there are no observable differences between participants allocated to different treatments compared to the control.

Therefore, from Table 2 we conclude that the randomization of participants across treatments worked properly.

4. Results

This section explores four different outcomes from our experiments: hard negotiation (multidimensional), easy negotiation (bi-dimensional), trust, and reciprocity. Our hard negotiation treatment exposes the participants to a bargaining situation where negotiations take place over

Table 1
Sample by treatment.

	Trust	Recipr.	Hard	Easy
Baseline	84	84	36	48
	(21)	(21)	(18)	(24)
U-Comm	76	76	36	40
	(19)	(19)	(18)	(20)
Comm+H2O	108	108	68	40
	(27)	(27)	(34)	(20)
Comm+Wine	100	100	56	44
	(25)	(25)	(28)	(22)
Comm+H2O+Food	84	84	52	32
	(21)	(21)	(26)	(16)
Comm+Wine+Food	84	84	44	40
	(21)	(21)	(22)	(20)
Total	536	536	292	244
	(134)	(134)	(146)	(122)

[18] For instance, with respect to the influence of alcohol, in sessions involving alcohol, participants were asked, "How inebriated did you feel during your negotiation?" which was rated on a scale ranging from 1 (not at all inebriated) to 11 (very inebriated), "Do you think alcohol affected your negotiation?' which was rated on a scale ranging from 1 (not at all) to 11 (very much), and "Did alcohol consumption help or hurt your side of the negotiation?". With respect to the negotiation process, participants were asked, "To reach an agreement, both of you made some concessions. In your negotiation, who made most of the concessions?" which was rated on a scale ranging from 1 (I made all the concessions) to 6 (both about the same) to 11 (the other person made all the concessions).

Table 2
Balance tests.

	Mean	Std. Dev.	Difference	p-value (T-test)
Baseline				
Female	0.476190	0.502432	–	–
Height	1.717619	0.088407	–	–
Weight	65.14634	11.28325	–	–
Risk aversion	5.085366	1.664452	–	–
Comm vs. Baseline				
Female	0.421052	0.497008	−0.05513	0.4870
Height	1.740946	0.087510	0.02332	0.0983*
Weight	66.15278	12.82785	1.00644	0.6052
Risk aversion	5.226667	1.341372	0.14130	0.5612
Comm+H2O vs. Baseline				
Female	0.481481	0.501986	0.00529	0.9423
Height	1.729796	0.092591	0.01217	0.3677
Weight	65.18557	12.02283	0.03923	0.9822
Risk aversion	5.267327	1.377615	0.18196	0.4195
Comm+Wine vs. Baseline				
Female	0.480000	0.502116	0.00380	0.9592
Height	1.736869	0.094173	0.01925	0.1582
Weight	64.52020	11.54294	−0.62614	0.7141
Risk aversion	5.291667	1.541815	0.20630	0.3922
Comm+H2O+Food vs. Baseline				
Female	0.380952	0.488537	−0.09523	0.2147
Height	1.725542	0.084554	0.00792	0.5548
Weight	65.73494	11.82194	0.58860	0.7440
Risk aversion	4.792683	1.420640	−0.29268	0.2276
Comm+Wine+Food vs. Baseline				
Female	0.428571	0.497843	−0.04761	0.5381
Height	1.718571	0.095805	0.00095	0.9467
Weight	65.98049	12.38404	0.83415	0.6527
Risk aversion	5.025000	1.550623	−0.06036	0.8116

Note: * significant at 10%.

different variables (wage, bonuses, etc.). As explained before, we compare a series of environments:

- No pre-play interaction at all (*Baseline*)
- Pre-play interaction without any communication structure (*Comm*)
- Pre-play interaction with structured communication (water - *Comm+H2O*, wine - *Comm+ Wine*, water and nibbles - *Comm+H2O+Food,* or wine and nibbles *Comm+Wine+Food*).

A first observation is that all negotiations (100% of the groups) reached an agreement (even if often away from the Pareto frontier), so there is no variation in that outcome, and we can conclude with:

Result 0: Pre-play interactions – be they through free or structured communication, alcohol, or nibbles – do not change the propensity of either hard or easy negotiation to reach an agreement.

Table 3 presents the analysis for negotiation with all the treatments (including all the variants of *Comm* and the amount received in the trust game, hereafter trust received). Its main interest is to show in one shot the main conclusion, namely that none of the treatments make any difference in negotiations. One can also see with this analysis that the treatments explain well the variation observed. We can thus establish

Result 1: Pre-play interactions – be they through free or structured communication, alcohol, or nibbles – do not improve the efficiency of hard or easy negotiation.

Table 3 reports the coefficients of the regression of negotiation points on the different treatments and other control variables (Model 1a). The reference category is the *Baseline* in the *Easy* negotiation.

As can be see directly from the table, none of the *Easy* negotiation treatments makes any improvement in terms of the negotiation outcomes. In a couple of cases, communication worsens the outcome. The *Hard* negotiations are different from the baseline, and as we will test more formally later (see subSection 4.a and Table 4), they are not different from one another.

The analysis is repeated in Model 1b interacting with whether the group was all-male or all-female. In this case the baseline is *Baseline* in all-female groups in the *Easy* negotiation.

There are two significant differences with respect to the *Baseline* benchmark in Model 1b arising from Easy negotiation. One is negative and another positive, both at 5% (the latter can be seen in Table 4 which shows the Wald test to compare whether estimated differences are significant with respect to the relevant baseline). For hard negotiation we also find two significant coefficients: one negative at 10% and another positive at 5%.

All in all, we find only four exceptions, always at 5% or 10%. Two of them are negative, meaning that the treatments are worse than the *Baseline* benchmark, and two are positive, where the treatment outperforms the control. Remember that these four cases report interactions with *gender* treatments.

Another way to see that the treatments are not useful to explain negotiation points is by looking at a regression of negotiation points just on the categorical variables *Male* and *Hard* (regression 1c).

Indeed, we perform a TOST differences test (see Table 5) to check whether the total points obtained as the negotiation outcome in the *Baseline* is equivalent to the rest of the treatments assuming an effect size. Specifically, we test the null hypothesis that the effect is at least as small as the lower equivalence bound (Δ_L) or at least as large as the upper equivalence bound (Δ_U) The effect size in our case is inspired by the experiment of Schweitzer and Gomberg (2001). It is based on the total points (around 10) that authors obtained when analyzing the effect of alcohol consumption. In their study, Schweitzer and Gomberg (2001) found a negative impact of alcohol consumption out of the negotiation output. In our study, we estimate that Cohen's value ($-10 \leq d \leq 10$) is a proper measure to use as an effect size.

The results shown in Table 5 support the previous conclusion by which the total points under the negotiation process do not differ among treatments. Following the TOST analysis, we argue the equivalence between the baseline and any non-structured and structured communication.

a) Hard negotiation

The top of Fig. 2 shows the total number of points obtained in the complex bargaining across treatment conditions. The last three bars of the figure enrich the structured communication treatments combining wine and food to mimic business meals. The top of Fig. 2 top already suggests what the regression analysis will clearly show: none of the treatments are different from the baseline without communication (*Baseline*). That is, it does not facilitate negotiations. All in all, the use of wine or food or both does not appear to improve negotiations.

Table 4 shows the Wald test for differences between the coefficients of the regression shown in Table 3 for the *Hard* treatments with respect to the baseline in which the negotiation is Hard and there is no communication (*Baseline*). Column 1a focuses on hard negotiation without separating by gender. The comparisons are made with respect to *Comm, Comm+H2O, Comm+Wine, Comm+H2O+Food*, and *Comm+Wine+Food*. We do not find any single positive and significant effect across the treatments.

Column 1b repeats the same analysis but separated by gender groups. In this case, the *Comm+H2O+Food* treatment yields significant (at 5%) and positive effects in the female groups, while the opposite is true for males but only marginally ($p<0.1$). Given that there is only one significant coefficient at conventional levels, and only in the heterogeneity analysis, we think the most likely explanation is the multiple hypotheses we test. One way to see this formally is that none of the hypotheses has a p-value lower than 0.01. Even a very conservative application of Bonferroni's correction (considering there are $n = 5$ hypotheses to test) would imply that the "valid" conventional threshold is $\alpha/n = 0.01$, and none of the treatments would be significant.

Overall, none of our five treatments clearly outperforms the *Baseline* treatment of no communication. Both males and females are better off with a perfect stranger and the introduction of any sort of socializing

Table 3
Negotiation OLS regression – pooled sample. Clustered at 4-member group level.

Endogenous variable: Negotiation points per individual	1a		1b		1c	
	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)
Comm	−6.409***	(2.363)	−3.477	(3.427)		
Comm+H2O	−4.087**	(1.946)	−2.894	(2.736)		
Comm+Wine	−1.877	(1.624)	−1.127	(1.608)		
Comm+H2O+Food	−2.015	(1.341)	−5.772**	(2.721)		
Comm+Wine+Food	−3.568***	(1.308)	−2.892	(1.814)		
Comm * hard	4.673	(3.840)	2.501	(4.273)		
Comm+H20 * hard	1.232	(3.040)	3.583	(3.101)		
Comm+Wine * hard	−1.762	(2.874)	0.322	(2.436)		
Comm+H2O+Food * hard	1.701	(2.918)	6.499**	(3.283)		
Comm+Wine+Food * hard	−0.201	(3.020)	4.308	(3.649)		
Comm * male			−5.061	(4.398)		
Comm+H2O * male			−2.709	(3.696)		
Comm+Wine * male			−1.319	(2.928)		
Comm+H2O+Food * male			6.112**	(2.872)		
Comm+Wine+Food * male			−1.303	(2.372)		
Baseline * hard * male			7.918**	(3.769)		
Comm * hard * male			10.55*	(5.608)		
Comm+H2O * hard * male			3.450	(3.915)		
Comm+Wine * hard * male			3.515	(3.570)		
Comm+H2O+Food * hard * male			−1.265	(3.750)		
Comm+Wine+Food * hard * male			−1.004	(3.928)		
Employer	2.365**	(1.156)	2.349**	(1.169)		
Risk aversion	−0.411	(0.318)	−0.400	(0.343)		
Trust received	0.0547	(0.0619)	0.0325	(0.0572)		
Male	1.045	(0.860)	0.0442	(0.512)	1.063	(0.865)
Hard	24.62***	(2.202)	20.58***	(1.439)	25.09***	(0.906)
Constant	58.91***	(2.467)	60.02***	(2.364)	56.32***	(0.804)
Obs.	516		516		536	
R^2	0.605		0.617		0.571	
Adjusted R^2	0.593		0.597		0.569	

Note. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4
Wald tests: Comparisons to baseline.

Endogenous variable: Negotiation points	1a		1b	
	Diff. Coef.	(SE)	Diff Coef.	(SE)
Comm * hard – Baseline * hard	4.673	(3.840)	2.501	(4.272)
Comm+H2O * hard – Baseline * hard	1.232	(3.039)	3.582	(3.101)
Comm+Wine * hard – Baseline * hard	−1.762	(2.874)	0.322	(2.426)
Comm+H2O+Food * hard – Baseline * hard	1.701	(2.918)	6.499**	(3.282)
Comm+Wine+Food * hard – Baseline * hard	−0.200	(3.020)	4.307	(3.648)
Comm * male – Baseline * male			−5.060	(4.398)
Comm+H2O * male – Baseline * male			−2.709	(3.696)
Comm+Wine * male – Baseline * male			−1.318	(2.927)
Comm+H2O+Food * male – Baseline * male			6.111**	(2.872)
Comm+Wine+Food * male – Baseline * male			−1.302	(2.371)
Comm * hard * male – Baseline * hard * male			2.633	(6.778)
Comm+H2O * hard * male – Baseline * hard * male			−4.468	(5.353)
Comm+Wine * hard * male – Baseline * hard * male			−4.403	(5.160)
Comm+H2O+Food * hard * male – Baseline * hard * male			−9.182*	(5.492)
Comm+Wine+Food * hard * male – Baseline * hard * male			−8.922	(5.538)

Note. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5
Statistical equivalence TOST test.

H0: ($d \leq$ - 10 U $d \geq$ 10) vs H1: (- 10 < d < 10)			
Total points negotiation	Lower Bound	Upper Bound	Equivalence
Baseline = Comm	t1 = 2.657 (df=148.75) ***	t2 = 5.141 (df=148.75) ***	Yes
Baseline = Comm+H2O	t1 = 4.614 (df=191.01) ***	t2 = 3.999 (df=191.01) ***	Yes
Baseline = Comm+Wine	t1 = 4.656 (df=180.62) ***	t2 = 4.41 (df=180.62) ***	Yes
Baseline = Comm+H2O+Food	t1 = 5.902 (df=166.26) ***	t2 = 2.487 (df=166.26) ***	Yes
Baseline = Comm+Wine+Food	t1 = 3.548 (df=162.64) ***	t2 = 4.464 (df=162.64) ***	Yes

Note. Calculated considering a two-sided null hypothesis of null to equivalent points at the negotiation outcome (Cohen's -10 $\leq d \leq$ 10). Bonferroni corrected p-values: *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

does not help to increase efficiency. Summarizing,

Result 1a: Pre-play interactions – be they through free or structured communication, alcohol, or nibbles – do not improve the efficiency of hard negotiation.

Tables 3 and 4 show that most of the estimated coefficients are not statistically significant (or do not improve negotiation). This evidence suggests that interactions among participants do not improve the results of the negotiations. However, the lack of significance of positive coefficients might be caused by the lack of power due to the number of observations.

To check whether power is indeed a problem, we perform different

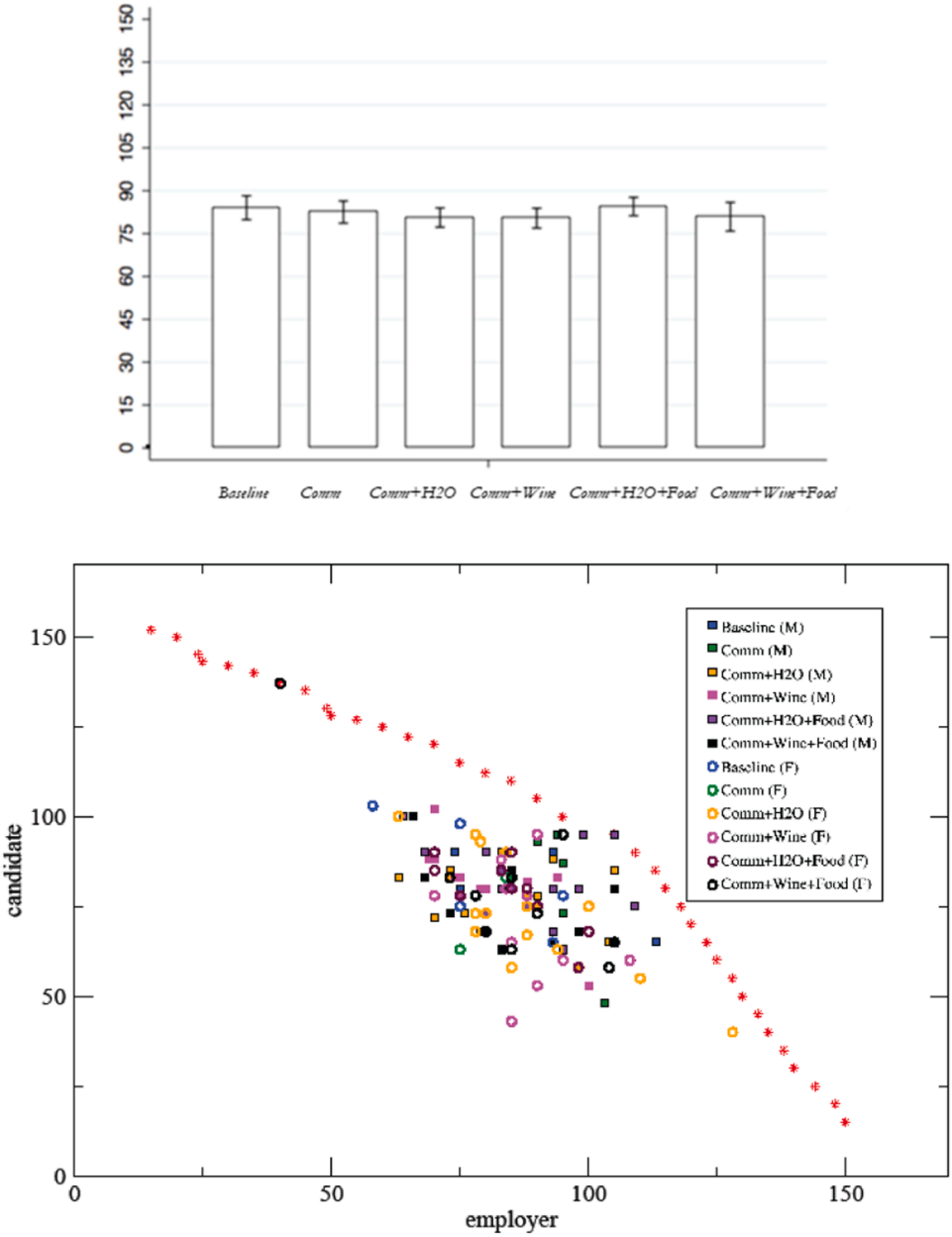


Fig. 2. Hard negotiation. Top: Results by treatment, Bottom: Results with respect to the Pareto frontier (in orange).

power calculations.[19] In model 1a from Table 3 we have a power of 70% to find an average treatment effect (ATE) higher than 4 negotiation points with respect to *Baseline*hard*.[20] However, effects lower than 4 points are economically irrelevant since they represent an increase of less than 5% of the average negotiation points in the *Baseline*hard*. In model 1b we have less power (60%) due to the triple interaction of treatments with hard and male. But again, positive coefficients are not particularly concerning. In the worst case scenario, they represent an increase of less than 9% of the mean with respect to the reference group.

Fig. A.1 (in Appendix A) suggests an equivalent result separating by agent and employer. Notice that even though the points achieved by agents and employers are quite similar on average, the distribution between them can vary considerably in the different pairs, as Fig. 2 (bottom) makes clear. Table A.1 (Appendix A) shows that no treatment provides significant effects for either agents or employers. Interacting with gender provides a single significant result at 5%, but it is negative (see also Table A.2. for the Wald test).

Result 1b: Pre-play interactions – be they through free or structured communication, alcohol, or nibbles – do not improve the number of points obtained by either agents or employers of hard negotiation

One reason communication is popular could be that it allows "clever" negotiators to obtain advantages. We would then not observe necessarily higher total points, but more "relative" benefit, and thus separate more from the equal division split. Table A.3 (columns 1a and 1b) confirm this observation by measuring the distance from the equal distribution for the different treatments.

Result 1c: Pre-play interactions – be they through free or structured

[19] We run the power analysis assuming a p-value of 10%, an R^2 of the covariates of 0.59 (coming from the regression of the outcome variable on all the covariates) and 12 arms (6 treatments and two types of negotiation). The standard deviation of the outcome variable (negotiation points) for the hard type is 12.39.

[20] We concentrate only in doing power calculations for positive coefficients, since they represent an improvement in negotiation.

communication, alcohol, or nibbles – do not increase the dispersion in the distribution of points in the hard negotiation.

Fig. 2 (bottom) shows graphically how the different outcomes are located with respect to the Pareto frontier of the game. The frontier is represented in orange and the outcomes for all the different pairs are represented in different colors according to the treatment.

Consistent with the results 1a 1b and 1c above, the colors/treatments are quite evenly spread inside the frontier and no color dominates in any part of the graph. This merely confirms results 1a and 1b above in a graphical summary way. It also shows that the outcomes are generally not efficient. This should not be surprising as there is a severe asymmetry of information and considerable cognitive complexity in negotiation, which would make obtaining an efficient solution quite challenging.

To understand if the lack of effects we observed were robust to the fact that we are only estimating an average treatment, we interacted the treatment with the quantity of liquid consumed, which is a proxy for alcohol inebriation in the treatments with alcohol. We found no qualitatively different results (details available upon request).

Overall, the results show that there is no effect of liquid consumption among wine treatments (no positive and significant treatment interaction terms). Note that the null effects we obtained regarding alcohol on negotiation could be driven by counteracting impacts on trust/altruism and aggressiveness/cognition that nullify one another, as we discussed in the literature section.

a) Easy negotiation

One possible explanation for why social interaction has no effect in the hard negotiation is that the problem may be too complex to solve. With five issues to discuss, there are too many trade-offs, even if participants are genuinely disposed toward reaching a more profitable agreement. To test if this explanation had merit, we ran treatments where we simplified the very same bargaining problem to two

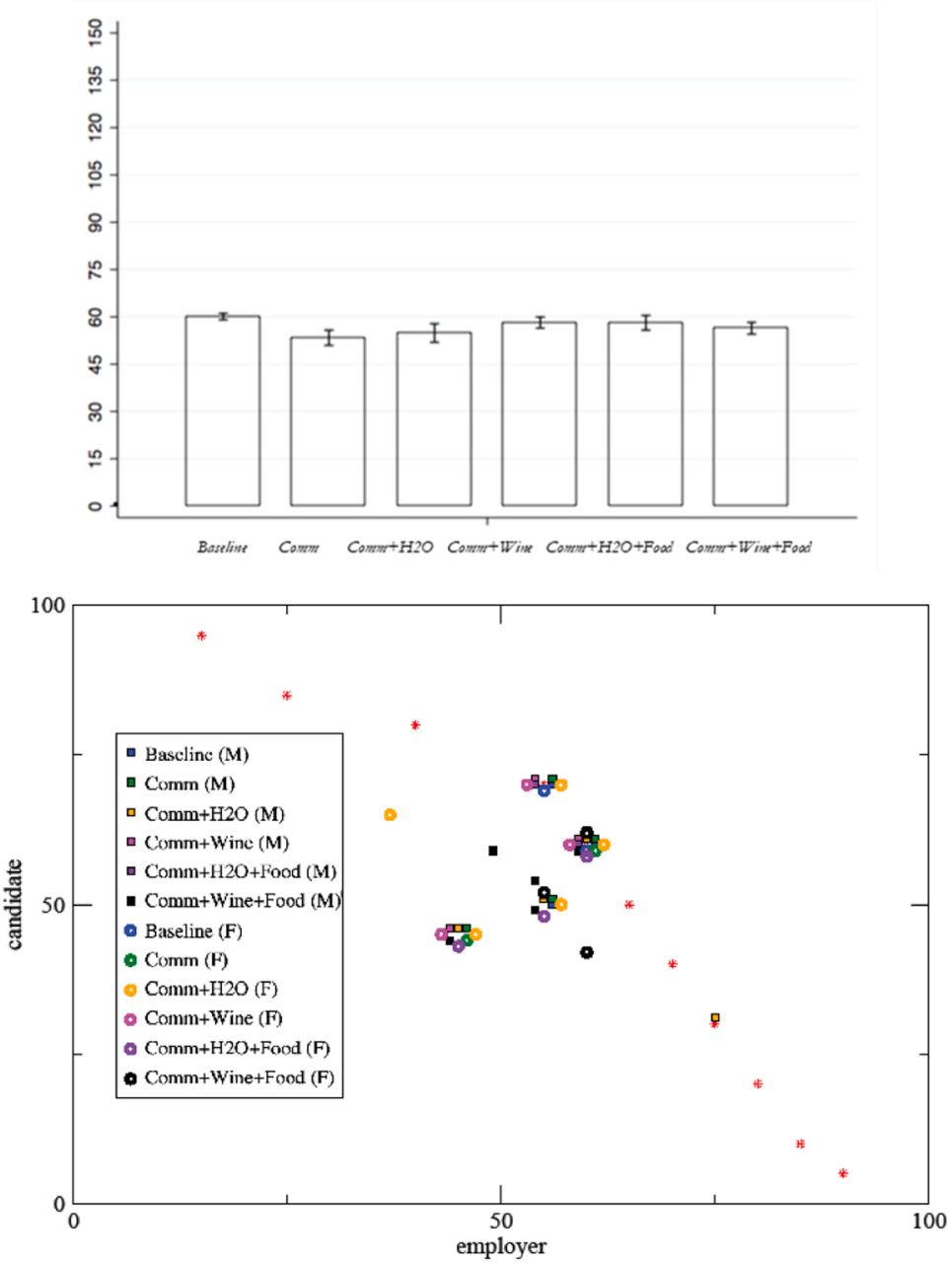


Fig. 3. Easy negotiation. Top: Results by treatment, Bottom: Results with respect to the Pareto frontier (in orange).

dimensions: wage and number of trips a month. Now participants only negotiate over these two dimensions. Fig. 3 shows the results descriptively. We do not find positive effects for pre-play interactions – structured or not –- on negotiation.

Column 1a in Table 3 shows the econometric analysis establishing these results formally for the sample of all groups. Indeed, the only significant treatments are negative. There are no treatments that are significantly better than *Baseline*. Column 1b shows that for females *Comm+H2O+Food* yields negative results. In Table 4 we find that the opposite is marginally true for males.

As in the case of hard negotiation, we have established the following result:

Result 2a: Pre-play interactions do not improve the outcome of easy negotiations. Result 1a is also replicated for easy negotiations.

In this way, we reject the hypothesis that the reason pre-play communication does not generate positive outcomes in negotiations is because they are complicated.

Similar to what happens in hard negotiations, we also do not find that pre-play negotiations make a positive difference for either employers or agents, as suggested in Fig. A.2 and confirmed using regression analysis in Table A.1: no significant and positive effect is found (see also Table A.2 for the Wald test).

Regarding power, all the estimated coefficients in model 1a (Table 3) are negative for easy negotiation treatments, therefore indicating that lack of power is indeed not a problem. In model 1b there is only one positive coefficient (*Comm+H2O+Food*) that is significant at 5% (see also the Wald test in Table 4). Therefore, our results are not driven by lack of power.

Result 2b: Pre-play interactions – be they through free or structured communication, alcohol, or nibbles – do not improve the number of points obtained by either agents or employers of easy negotiation.

In addition, as with the hard negotiations, we do not find that the outcomes separate more from the equal division. We show this in Table A.1, columns 2a and 2b. Thus we have the following result:

Result 2c: Pre-play interactions – be they through free or structured communication, alcohol, or nibbles – do not increase the dispersion in the distribution of points in the easy negotiation.

Let us see how the easy negotiation compares with respect to the Pareto frontier. Fig. 3 (bottom) shows the outcomes of the different environments. As for the Hard negotiation, it visually confirms Results 2a, 2b, and 2c above. But it also shows that in the Easy negotiation environment more points are concentrated in the Pareto frontier. They are also quite symmetric as both players get very similar points. It is tempting to conclude that the Easy environment makes the achievement of efficiency less daunting, thus showing the importance of information and cognitive constraints on negotiation. However, there was only one parametrization for each complexity level. To be sure that complexity is an important factor in negotiation efficiency, we would need more parametrizations. We leave this interesting conjecture for future research.[21]

This analysis confirms that the treatments do not improve the results of the negotiations.

a) Trust

The absence of positive results for negotiations shown in the previous section might be explained by the interplay of several factors. It might be the case that alcohol makes people less thoughtful and more aggressive (Schweitzer & Gomberg, 2001) or that pre-play interaction makes participants less focused on the task (Yuan, Head & Du, 2003). In this section, we will focus exclusively on trust among participants. Since trust might be a moderating factor in bargaining (we need trust to reach agreements) we will study now how our participants played the Trust Game using the same treatments as before.

Fig. 4 (top) shows the average trust behavior across treatments. On the left side we show the *Baseline* and moving to the right, treatments with an increasing number of characteristics. Table 6 (model 1a) analyzes the same problem using regressions clustered at group (of 4) level and several independent variables (the treatments), where the reference groups is the *Baseline*Female* treatment.

A first observation is that the trust level is higher than has been observed in many previous experiments. A likely cause for this difference is the fact that trust is higher because our participants interact face-to-face, something that is known to increase trust (see, e.g., Wilson, Straus & McEvily, 2006) and it is also quite realistic and appropriate given the situations we are trying to mimic. Apart from its intrinsic interest, this result indicates that our effort in making the experimental setting natural was successful.

Therefore, the trust resulting from the *Baseline* is no different than the trust resulting from the other treatments involving social interaction. The clustered regression (see Table 6, model 1a) shows that social interactions do not help to enhance trust among females. For men, we find basically the same result. We run a Wald test to check whether the estimated coefficients are significantly different from the control (*Baseline*) and observe that none of them are providing positive and significant results for trust.

Result 3 summarizes our findings about trust.

Result 3: For both females and males, pre-play interactions – either free or structured ones, alcohol, or nibbles – do not improve trust.

a) Reciprocity

Finally, we study whether reciprocity is sensitive to the different types of social conditions.[22] Recall that reciprocity reflects the amount of money an individual would like to return to another participant who previously sent him/her some money. This measure can be also interpreted as gratitude.

Our six treatments cover very different environments, ranging from pure strangers (no pre-play communication) to a situation akin to a business meal (spending time with the other partner, communicating with him or her, having wine and some food). The question is whether different levels of interaction may create different levels of reciprocity.

We do not see effects of different forms of social interactions on reciprocity. Fig. 4 (bottom) compares the average level of reciprocity among the six treatments. It is straightforward to check that the different forms of interaction we tried do not seem to outperform the "complete stranger" environment (i.e., no pre-play communication).

Table 6 (model 1b) shows the results using a clustered regression model. No single independent variable has a positive and significant impact on trust for either males or females. We may conclude as follows.

Result 4: Pre-play interactions do not improve reciprocity. This is true both for male and female participants.

a) Questionnaire outcomes

As we mentioned in Section 2, the participants responded to a questionnaire based on Schweitzer and Gomberg (2001) about the negotiation process. This could be useful in case there were significant results to understand the mechanisms through which the improved (or worsened) outcomes were reached.

The questions had to do with which person of the pair (on a scale from 1 to 11, with 1 being mostly the responder, and 11 mostly the other person of the pair) used specific negotiation tactics. These were: 1– Concessions, 2 – Asking questions, 3 – Giving information, 4 – Making

[21] We thank an anonymous referee for this suggestion.

[22] Twelve participants did not respond correctly to the Reciprocity questions. As a result, we lost some observations between the trust and reciprocity experiments.

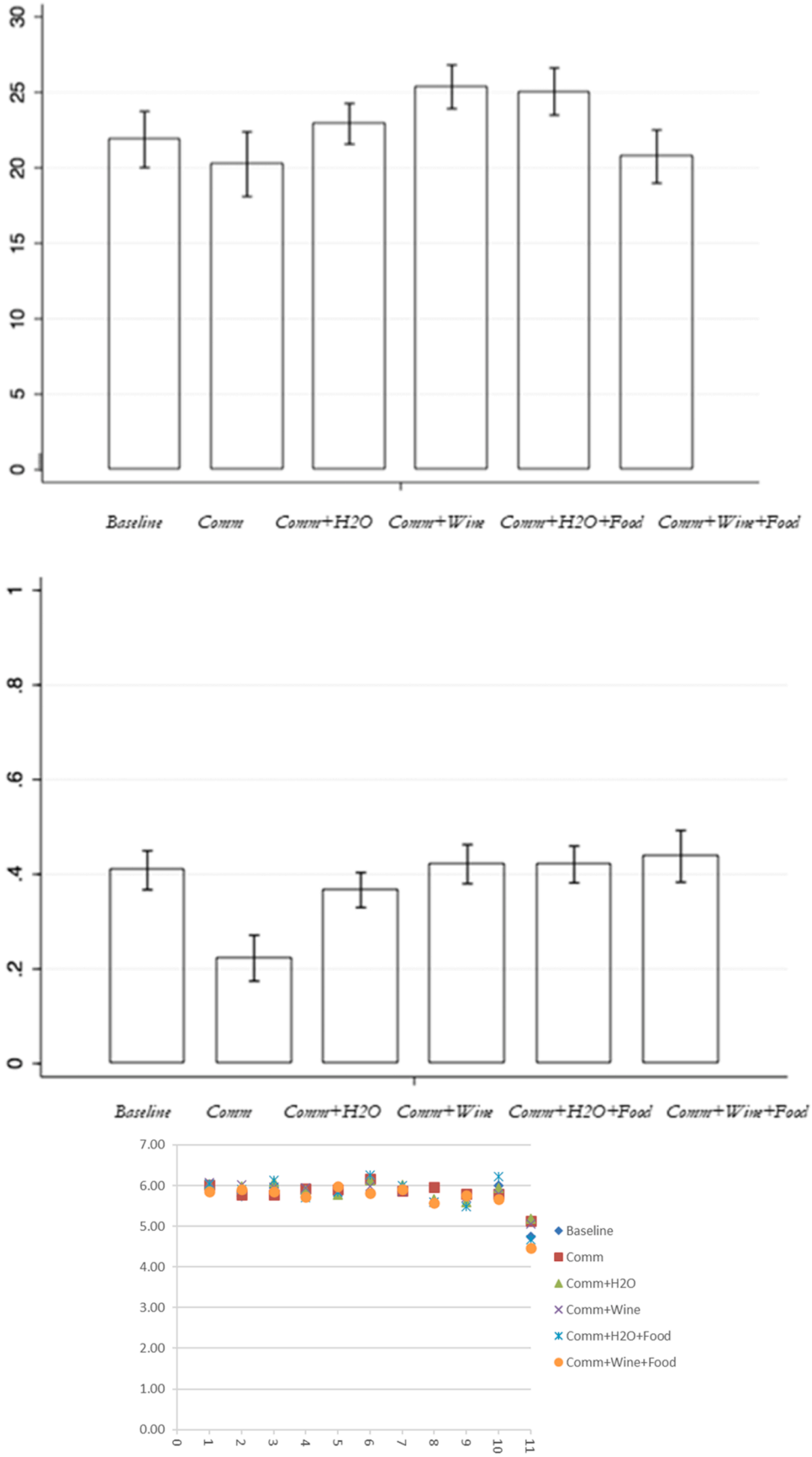


Fig. 4. Trust (top) and Reciprocity (bottom).
Figure 4. Questionnaire replies by treatment.

Table 6
Regression analysis (Clustered at 4-member group level): Trust and Reciprocity.

Endogenous variables	Trust received 1a		Reciprocity 1b	
	Coef.	(S.E.)	Coef.	(S.E.)
Comm	1.540	(1.316)	−0.147**	(0.0722)
Comm+H2O	0.206	(1.077)	−0.0852	(0.0519)
Comm+Wine	2.020*	(1.163)	0.0529	(0.0547)
Comm+H2O+Food	1.563	(1.265)	−0.0222	(0.0682)
Comm+Wine+Food	0.824	(1.150)	−0.00292	(0.0419)
Risk aversion	0.00551	(0.0875)	0.00382	(0.00671)
Baseline * male	2.380**	(1.133)	−0.0165	(0.0697)
Comm * male	−1.490	(1.400)	−0.0777	(0.0875)
Comm+H2O * male	2.682***	(0.726)	0.0557	(0.0613)
Comm+Wine * male	0.730	(0.969)	−0.107	(0.0753)
Comm+H2O+Food * male	1.223	(1.082)	0.0458	(0.0724)
Comm+Wine+Food * male	0.133	(1.116)	0.0355	(0.0844)
Constant	6.052***	(0.960)	0.397***	(0.0384)
Obs.	516		506	
R^2	0.158		0.124	
Adjusted R^2	0.137		0.102	

Note. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

offers, 5 – Synthesizing the other's preferences, 6 – Making insults, 7 – Pointing out that the other does not fit with the position/client's interests, 8 – Bluffing about the interests/position, 9 – Making ultimatums, 10 – Making threats, 11 – Comparing performance to the others. The exact questions are shown in the Appendix.

As one could expect, given that the negotiation outcomes themselves do not differ by treatment, the negotiation tactics also do not differ by treatment, as shown in Fig. 3.

5. Discussion and conclusions

In this paper, we have established that, in a specific environment, pre-play communication does not seem to help improve negotiation outcomes. This is true both at the aggregate session level, and in the regression analysis, when we analyze individual interactions more deeply. The lack of positive effects has been shown in a variety of conditions: where communication is more or less structured, accompanied or not by food and drink, and for both easy and hard negotiations. We have also shown that, in the same environment, communication does not enhance trust, which is a possible pathway to improve negotiation outcomes

As with any laboratory experiment, there are several limitations to our results, which as mentioned stem from a specific environment. We will now discuss them in turn.[23]

1 Our sample consists of students from a specific business school. This may be less of a problem than it looks at first sight. They come from an elite business school, with tough exam entrance requirements. At the same time, they are also rather diverse and representative of French society since, as we mentioned in the design section, many are recipients of social scholarships, and the diversity index is the third highest in France. They most likely understand they will be negotiators in the future and many of them have done internships prior to starting their studies and have an appreciation of the business world. They are clearly the kind of people that will engage in high level negotiations in the future. Moreover, the incentives we used are considered high (as explained earlier) and the alcohol stimuli was sufficient for inebriation (3 glasses of wine, motivated but, unlike other papers, not forced intake). Of course, it is entirely possible that with an even more diverse setting, perhaps with people from very different socio-cultural origins, or a more realistic setup, the results could be different. So, clearly, more research would be needed to establish stronger external validity.

2 Our setup is still somewhat artificial with participants being in a laboratory, receiving numbers, and being assigned at random to a table. In real situations, there would be a lot more context to the interaction, business partners may know about each other, and would know why they are meeting, and the setting would probably be a nice restaurant. These are all valid concerns, but many negotiations still take place among people who do not know each other, and the pre-negotiation meeting may not take place in a fancy, or familiar, environment.

3 The participants do not know they will play a negotiation game at the time of the social interaction. Business lunches often include negotiation, which makes our structure seem a bit artificial. We made this choice deliberately. Having people negotiate while they are socializing implies some loss of control. What is happening in that phase, socialization, or negotiation? Also, during the negotiation phase, there is still some socialization as our negotiation is free form, so we do not disallow it at the time. But naturally, this is a limitation that further research needs to investigate.

4 Similarly, it could be argued that it is often one party who invites the other to the informal lunch, and perhaps the act of taking the trouble to call up and invite the other party to the informal communication signals some important personality trait of that party. Like the previously discussed extensions, these are interesting conjectures that are worth exploring. But our research shows that it is not the act of communicating *per se* which improves negotiation, but rather something else: the signaling.

5 Communication, meals, and alcohol may affect different groups of people differently. There may be individual characteristics (unobserved to the experimenter) that interact with the intervention to generate asymmetrical results among different groups. This could be true, but at least we can say that the generic recommendation with which we start "The reciprocal nature of trust reinforces the value of taking time to get to know the other party and build rapport before you begin to negotiate" does not seem warranted now without some qualifications. It could be valid under some circumstances, but not universally.

6 In the real world, people self-select into socializing activities and may not consume alcohol or talk with anyone. Note that in our experiment people are not forced to either consume alcohol or to talk. Most people, however, ended up consuming alcohol and talking. One could argue that the stylized setup in the lab implicitly pressured people to do both things. But casual inspection of real-life professional interactions suggests that a large proportion of people also do both things in reality and perhaps for similar social cue reasons.

Also, we should emphasize again that our experiment made a lot of effort to replicate a real business meal. We fixed the starting time of all experimental sessions at the very same time (11 AM). This was done for two reasons: first, to prevent heterogeneity effects on participants due to glucose (see Danziger, Levav & Avnaim-Pesso, 2011); second, the timing of the trust and negotiation activities (which happened after the allocation of participants in the rooms and the interaction phase) occurred about noon, which is very close to real business lunchtime hours in France. Indeed, we made an extra effort so that the setting was a bit more casual. We allocated desks and participants in circles of four facing each other and they apparently enjoyed their experience. The unexpected high level of trust observed in the entire experiment might be explained by the relaxed atmosphere. The total length of our experiment mimics the real time business lunches take in France (and prevents the effects of alcohol from vanishing).

Finally, the policy implications of the paper, if its results prove robust to the removal of its limitations, could be very significant. While it would be premature to change the tax codes and practices of many

[23] We thank an anonymous referee for several of these important notes of caution.

countries based on a single study, we would recommend that tax authorities pay a lot more attention to the fact that business lunches, or dinners, could be a form of untaxed in-kind compensation to employees leaking out of badly stretched public finances.

Data availability

Data will be made available on request.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.socec.2022.101948.

Appendix

Experimental instructions – Negotiation game (*Hard, employer*)

Player 1

In this game, you will only interact with player 2. Player 3 interacts with player 4.

You will engage in a discussion process to potentially hire a candidate. The candidate's CV appears at the bottom of this page. In this process, you are the *employer* who received the CV for this candidate from a *human resources agent*, whose role is adopted by Player 2. You must discuss and come together on an agreement. The action takes place in 1996.

You have been offered the following position:

> We are looking for entry-level candidates for a position as a geologist. The candidate should be familiar with modern methods of construction and drilling. The project will consist of analyzing the foundations of large buildings and will involve a significant number of trips. We offer a competitive salary, and a mutual insurance package will probably be available.

You must reach an agreement on five different contract characteristics: salary, signing bonus received by the candidate, starting date, type of mutual insurance, and number of trips per month. To reach an agreement, both sides (you and the agent) must coincide on each of the five characteristics. Each line contains the number of points you earn by selecting it. **For the agent, the number of points on each line is different, so this information (conversion into points) must remain strictly private.**

Your total points will be calculated by adding up the points you have successfully obtained on each of the five characteristics.

At the end of the experiment, your points will be converted into euros at the rate of 10 points = 1 euro.

You have 15 min for the discussion and then you will have to answer a series of questions concerning the contract you have chosen. Here is the

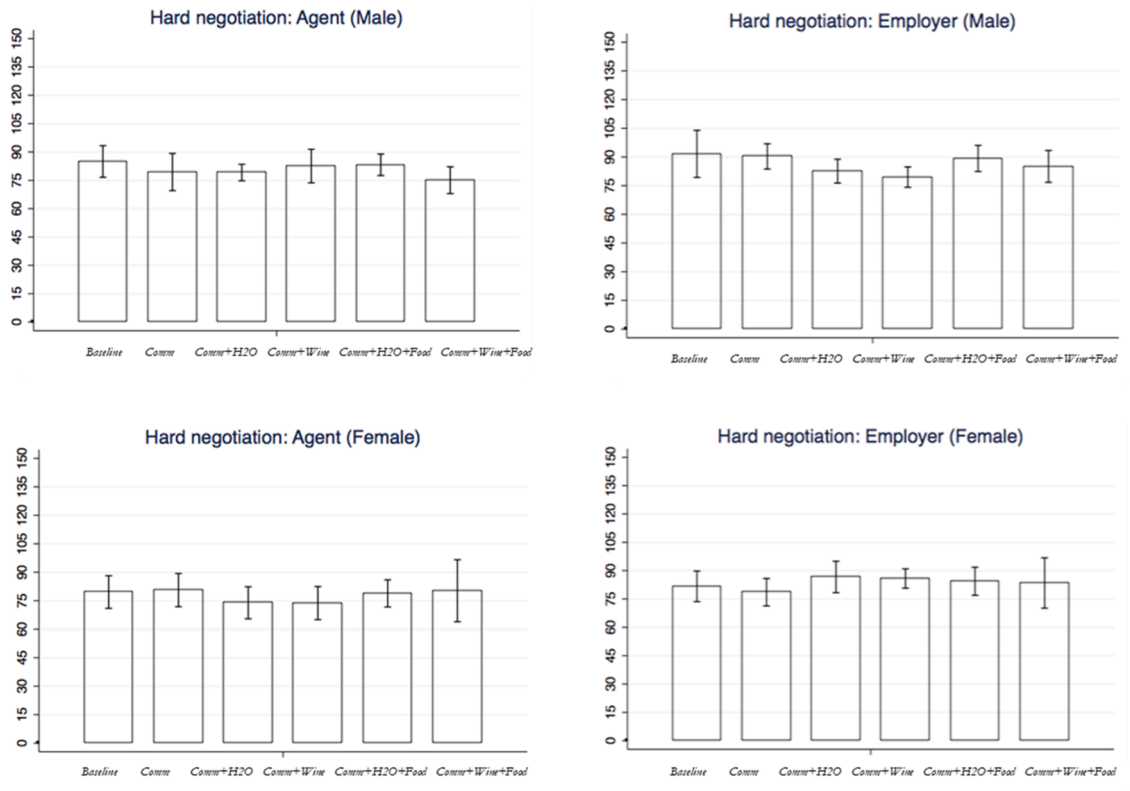


Fig. A.1. Results for Hard negotiation (males and females): agent (left) and employer (right).

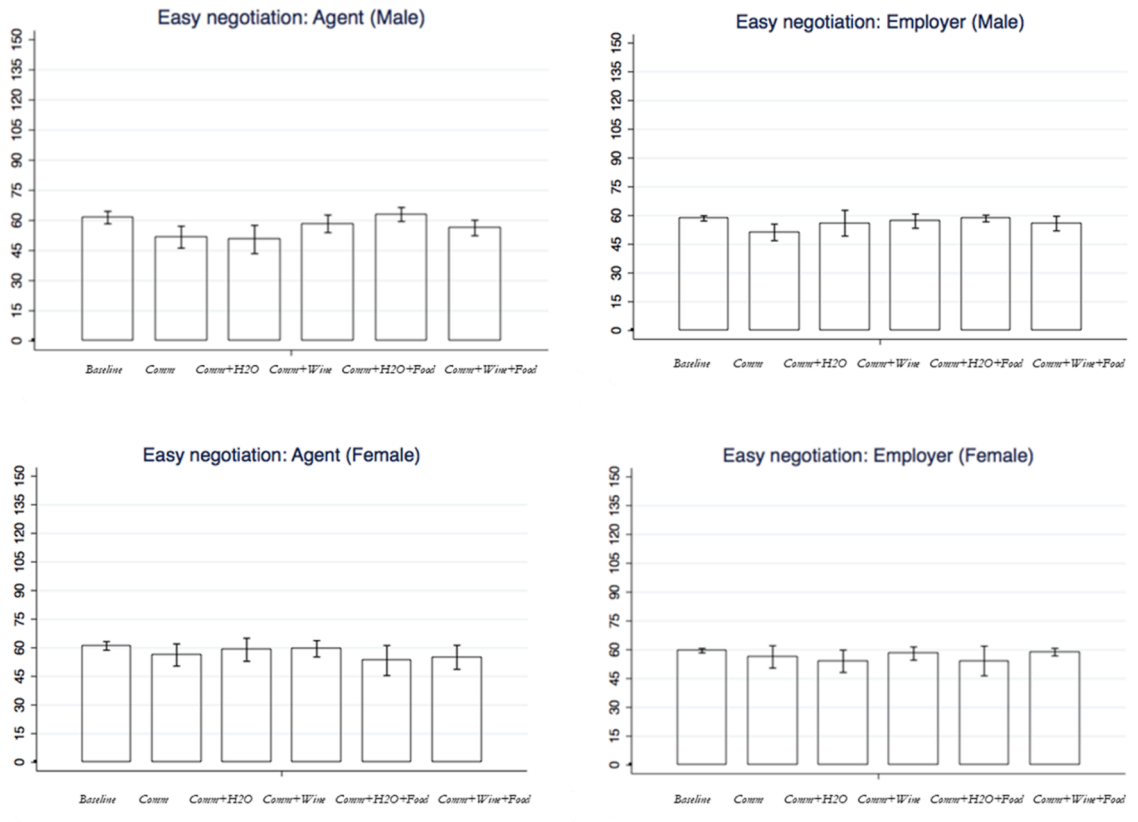


Fig. A.2. Results for Easy negotiation (males and females): agent (left) and employer (right).

candidate's CV:

Here is your **private** negotiation grid, with the conversion into points:

Table A.1
Regression OLS analysis by Agent and Employer: Clustered at group level.

	Agent				Employer			
	1a		1b		2a		2b	
	Coef.	(S.E.)	Coef.	(S.E.)	Coef.	(S.E.)	Coef.	(S.E.)
Comm	−6.438**	(2.566)	−2.866	(3.261)	−5.795**	(2.392)	−3.307	(3.543)
Comm+H2O	−3.616	(2.575)	0.299	(3.373)	−3.975*	(2.304)	−5.767	(3.892)
Comm+Wine	−1.581	(2.087)	0.555	(2.354)	−1.546	(1.553)	−1.882	(1.636)
Comm+H2O+Food	−1.620	(1.825)	−5.558**	(2.777)	−2.094	(1.420)	−5.590**	(2.697)
Comm+Wine+Food	−4.710***	(1.632)	−4.626	(3.084)	−2.045	(1.542)	−0.739	(1.526)
Comm * hard	4.717	(5.545)	6.032	(6.848)	3.934	(4.989)	−2.120	(5.610)
Comm+H20 * hard	−1.043	(4.673)	−3.548	(7.009)	2.954	(5.052)	11.38	(7.304)
Comm+Wine * hard	−1.551	(4.427)	−4.530	(5.739)	−2.713	(3.984)	4.874	(4.005)
Comm+H2O+Food * hard	0.425	(4.022)	6.052	(5.850)	2.635	(4.078)	6.964	(5.196)
Comm+Wine+Food * hard	1.386	(5.000)	9.056	(8.989)	−2.357	(5.235)	−0.645	(7.990)
Comm * male			−6.461	(4.478)			−3.725	(4.515)
Comm+H2O * male			−8.493*	(4.409)			3.372	(4.889)
Comm+Wine * male			−3.594	(3.673)			0.560	(2.996)
Comm+H2O+Food * male			6.552*	(3.432)			5.766*	(3.159)
Comm+Wine+Food * male			−0.732	(3.405)			−1.628	(2.475)
Baseline * hard * male			6.490	(5.276)			10.40*	(5.740)
Comm * hard * male			3.542	(8.463)			18.21***	(5.838)
Comm+H2O * hard * male			12.30*	(6.479)			−6.573	(7.510)
Comm+Wine * hard * male			12.78**	(5.896)			−5.318	(4.126)
Comm+H2O+Food * hard * male			−3.502	(5.435)			1.071	(4.873)
Comm+Wine+Food * hard * male			−7.123	(8.966)			5.025	(8.446)
Risk aversion	−0.936*	(0.488)	−1.035*	(0.549)	0.0313	(0.433)	0.236	(0.445)
Trust received	0.147	(0.116)	0.0753	(0.0974)	−0.0155	(0.0957)	0.00843	(0.0910)
Male	1.135	(1.655)	1.051	(1.132)	0.695	(1.390)	−1.168	(1.052)
Hard	21.91***	(2.909)	18.35***	(4.028)	27.73***	(3.320)	22.82***	(3.427)
Constant	61.14***	(3.526)	63.45***	(3.453)	58.79***	(3.458)	58.20***	(3.103)
Obs.	259	259	257	257				
R^2	0.540	0.572	0.681	0.706				
Adjusted R^2	0.514	0.526	0.663	0.674				

Note: (*** $p<0.01$, ** $p<0.05$, * $p<0.1$). Reference group: No-Interaction.

Table A.2
Wald tests - Comparisons to baseline:.

	Agent				Employer			
	1a		1b		1a		1b	
	Diff. Coef.	(Std. dev.)	Diff Coef.	(Std. dev.)	Diff. Coef.	(Std. dev.)	Diff Coef.	(Std. dev.)
Comm * hard – Baseline * hard	4.605	(5.521)	6.032	(6.847)	3.964	(5.035)	−2.120	(5.609)
Comm+H2O * hard – Baseline * hard	−1.094	(4.665)	−3.547	(7.008)	2.966	(5.059)	11.385	(7.304)
Comm+Wine * hard – Baseline * hard	−1.724	(4.485)	−4.529	(5.739)	−2.717	(3.988)	4.873	(4.004)
Comm+H2O+Food * hard – Baseline * hard	0.343	(4.104)	6.051	(5.849)	2.655	(4.132)	6.963	(5.195)
Comm+Wine+Food * hard – Baseline * hard	1.276	(5.007)	9.055	(8.989)	−2.360	(5.260)	−0.645	(7.989)
Comm * male – Baseline * male			−6.460	(4.478)			−3.725	(4.515)
Comm+H2O * male – Baseline * male			−8.493*	(4.408)			3.371	(4.888)
Comm+Wine * male – Baseline * male			−3.594	(3.672)			0.560	(0.852)
Comm+H2O+Food * male – Baseline * male			6.551*	(3.432)			5.766*	(3.159)
Comm+Wine+Food * male – Baseline * male			−0.732	(3.405)			−1.628	(2.474)
Comm * hard * male – Baseline * hard * male			−2.948	(9.787)			7.813	(8.262)
Comm+H2O * hard * male – Baseline * hard * male			5.809	(8.406)			−16.969*	(9.581)
Comm+Wine * hard * male – Baseline * hard * male			6.292	(7.782)			−15.714**	(7.091)
Comm+H2O+Food * hard * male – Baseline * hard * male			−9.991	(7.710)			−9.325	(7.678)
Comm+Wine+Food * hard * male – Baseline * hard * male			−13.613	(10.351)			−5.371	(10.114)

Note: (*** $p<0.01$, ** $p<0.05$, * $p<0.1$). Reference group: No-Interaction.

Table A.3
Distance from individual outcome to pair equally distributed payoffs.

	Hard		Easy	
	Male1a	Female1b	Male2a	Female2b
Baseline	0.5	0.5	0.5	0.5
	(0.05)	(0.06)	(0.02)	(0.01)
Comm	0.5	0.5	0.49	0.5
	(0.06)	(0.049)	(0.02)	(0)
Comm+H2O	0.5	0.5	0.5	0.5
	(0.05)	(0.097)	(0.07)	(0.05)
Comm+Wine	0.5	0.5	0.5	0.5
	(0.06)	(0.08)	(0.01)	(0.01)
Comm+H2O+Food	0.5	0.5	0.5	0.5
	(0.08)	(0.06)	(0.02)	(0.01)
Comm+Wine+Food	0.5	0.5	0.5	0.5
	(0.06)	(0.11)	(0.01)	(0.03)
Total	0.5	0.5	0.49	0.5
	(0.06)	(0.08)	(0.03)	(0.03)

Note. Average (standard deviation).

	Points
Salary (in euros)	
26,000	50
28,000	45
30,000	40
32,000	35
34,000	30
36,000	25
38,000	20
40,000	15
Signing bonus (in euros)	
0	0
1000	5
2000	10
4000	8
6000	4
8000	0
Starting date	
4 weeks	25
6 weeks	10
8 weeks	5
1é weeks	0
Mutual insurance decision	
Refuse mutual option	25
Mutual paid by the company at 50%	15
Mutual paid by the company at 100%	0
Number of trips per month	
1	0
2	10
3	25
4	40

Table A.4
Negotiation OLS regression, dyads - pooled sample. Clustered at group of 4 level. Note. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Endogenous variable: Negotiation points per dyad	1a		1b		1c	
	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)
Comm	−12.74***	(4.714)	−7.441	(6.748)		
Comm+H2O	−9.579**	(4.055)	−6.321	(5.281)		
Comm+Wine	−3.836	(3.180)	−2.648	(3.071)		
Comm+H2O+Food	−4.012	(2.870)	−12.18**	(5.566)		
Comm+Wine+Food	−7.326***	(2.560)	−6.346*	(3.546)		
Comm * hard	9.642	(7.695)	6.387	(8.479)		
Comm+H20 * hard	3.436	(6.287)	8.306	(5.910)		
Comm+Wine * hard	−3.976	(5.837)	1.270	(4.881)		
Comm+H2O+Food * hard	3.959	(5.976)	14.99**	(6.474)		
Comm+Wine+Food * hard	0.0810	(5.820)	9.020	(5.823)		
Comm * male			−9.149	(8.809)		
Comm+H2O * male			−7.130	(7.530)		
Comm+Wine * male			−2.103	(5.798)		
Comm+H2O+Food * male			13.25**	(4.641)		
Comm+Wine+Food * male			−1.940	(7.525)		
Baseline * hard * male			16.60**	(11.33)		
Comm * hard * male			19.79*	(8.127)		
Comm+H2O * hard * male			7.282	(7.048)		
Comm+Wine * hard * male			5.601	(7.559)		
Comm+H2O+Food * hard * male			−4.312	(6.817)		
Comm+Wine+Food * hard * male			−1.266	(0.238)		
Employer	−0.270	(0.231)	−0.286	(0.378)		
Risk aversion	−0.528	(0.357)	−0.495	(0.105)		
Trust received	0.0584	(0.118)	0.0188	(0.924)		
Male	1.803	(1.693)	−0.115	(2.697)	2.363	(1.724)
Hard	48.61***	(4.526)	40.21***	(3.317)	49.96***	(1.807)
Constant	120.4***	(3.402)	122.3***	(6.748)	112.6***	(1.607)
Obs.	516	516	536			
R^2	0.826	0.842	0.804			
Adjusted R^2	0.821	0.834	0.803			

Table A.5
Wald tests: Comparisons to baseline, dyads.

Endogenous variable: Negotiation points per dyad	1a		1b	
	Diff. Coef.	(SE)	Diff Coef.	(SE)
Comm * hard – Baseline * hard	9.642	(7.694)	6.386	(8.479)
Comm+H2O * hard – Baseline * hard	3.435	(6.287)	8.306	(5.909)
Comm+Wine * hard – Baseline * hard	−3.975	(5.836)	1.270	(4.880)
Comm+H2O+Food * hard – Baseline * hard	3.959	(5.976)	14.994**	(6.474)
Comm+Wine+Food * hard – Baseline * hard	0.081	(5.819)	9.019	(5.823)
Comm * male – Baseline * male			−9.148	(8.809)
Comm+H2O * male – Baseline * male			−7.130	(7.530)
Comm+Wine * male – Baseline * male			−2.102	(5.718)
Comm+H2O+Food * male – Baseline * male			13.254**	(5.798)
Comm+Wine+Food * male – Baseline * male			−1.940	(4.640)
Comm * hard * male – Baseline * hard * male			3.192	(13.687)
Comm+H2O * hard * male – Baseline * hard * male			−9.314	(10.858)
Comm+Wine * hard * male – Baseline * hard * male			−10.994	(10.210)
Comm+H2O+Food * hard * male – Baseline * hard * male			−20.908*	(10.957)
Comm+Wine+Food * hard * male – Baseline * hard * male			−17.862	(10.233)

Note. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

POST-NEGOTIATION QUESTIONS

1 To reach an agreement, each of you had to make concessions. Who made the most?

1	2	3	4	5	6	7	8	9	10	11
I made all the concessions					We made them equally					The other person made all the concessions

1 Did you ask questions:

1	2	3	4	5	6	7	8	9	10	11
I asked all the questions					We asked each other equally					The other person asked all the questions

1 Did you give information:

1	2	3	4	5	6	7	8	9	10	11
I gave all the information					We both provided the information equally					The other person gave all the information

1 Did you make offers:

1	2	3	4	5	6	7	8	9	10	11
I made all the offers					We made them equally					The other person made all the offers

1 Did you synthesize the preferences of the other person:

1	2	3	4	5	6	7	8	9	10	11
I did all the synthesis					We both did the synthesis equally					The other person did all the synthesis

1 Did you insult or belittle the other person (e.g., a candidate is young, she or he has no experience, it is zero):

1	2	3	4	5	6	7	8	9	10	11
It was me who made the insults					We both made them equally					It was the other person who made the insults

1 Did you point out that the job profile or offering does not match (e.g., we really need someone more experienced, my client needs a more reputable company):

1	2	3	4	5	6	7	8	9	10	11
I used this technique					We both used it equally					The other person used this technique

1 Did you bluff (e.g., there are plenty of candidates, my candidate had lots of offers):

1	2	3	4	5	6	7	8	9	10	11
It was me who bluffed					We bluffed equally					It was the other person who bluffed

1 Did you give any ultimatums (e.g., this is my final price/offer):

1	2	3	4	5	6	7	8	9	10	11
It was me who gave ultimatums					We gave ultimatums equally					It was the other person who gave the ultimatums

1 Did you make threats (e.g., we'll stop the negotiation immediately):

1	2	3	4	5	6	7	8	9	10	11
It was me who used threats					We used threats equally					It was the other person who used threats

1 Compared to other people who participated in this game, do you think you satisfactorily succeeded in finding a good contract?

1	2	3	4	5	6	7	8	9	10	11
It was me who found the best contract					Everyone found the same contract					The others found better contracts than me

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