Year : 2020

OVERCONFIDENCE, DISHONESTY AND ECONOMIC BEHAVIOR

Colzani Paola

Colzani Paola, 2020, OVERCONFIDENCE, DISHONESTY AND ECONOMIC BEHAVIOR

Originally published at : Thesis, University of Lausanne

Posted at the University of Lausanne Open Archive http://serval.unil.ch
Document URN : urn:nbn:ch:serval-BIB_6A7D69931E5A5

UNIL | Université de Lausanne

FACULTÉ DES HAUTES ÉTUDES COMMERCIALES

DÉPARTEMENT D'ÉCONOMIE

OVERCONFIDENCE, DISHONESTY AND ECONOMIC BEHAVIOR

THÈSE DE DOCTORAT

présentée à la

Faculté des Hautes Études Commerciales
de l'Université de Lausanne

pour l'obtention du grade de
Docteure ès Sciences Économiques,
mention « Économie politique »

par

Paola COLZANI

Directeur de thèse
Prof. Luís Santos-Pinto

Jury

Prof. Felicitas Morhart, présidente
Prof. Rustamdjan Hakimov, expert interne
Prof. Simeon Schudy, expert externe

LAUSANNE
2020

UNIL | Université de Lausanne

FACULTÉ DES HAUTES ÉTUDES COMMERCIALES

DÉPARTEMENT D'ÉCONOMIE

OVERCONFIDENCE, DISHONESTY AND ECONOMIC BEHAVIOR

THÈSE DE DOCTORAT

présentée à la

Faculté des Hautes Études Commerciales
de l'Université de Lausanne

pour l'obtention du grade de
Docteure ès Sciences Économiques,
mention « Économie politique »

par

Paola COLZANI

Directeur de thèse
Prof. Luís Santos-Pinto

Jury

Prof. Felicitas Morhart, présidente
Prof. Rustamdjan Hakimov, expert interne
Prof. Simeon Schudy, expert externe

LAUSANNE
2020

UNIL | Université de Lausanne
HEC Lausanne
Le Décanat
Bâtiment Internef
CH-1015 Lausanne

IMPRIMATUR

Sans se prononcer sur les opinions de l'autrice, la Faculté des Hautes Etudes Commerciales de l'Université de Lausanne autorise l'impression de la thèse de Madame Paola COLZANI, titulaire d'un bachelor en Management et d'un master en Economie politique de l'Università Cattolica del Sacro Cuore di Milano, en vue de l'obtention du grade de docteure ès Sciences économiques, mention économie politique.

La thèse est intitulée :

OVERCONFIDENCE, DISHONESTY AND ECONOMIC BEHAVIOR

Lausanne, le 11 novembre 2020

Le doyen

Jean-Philippe Bonardi

Members of the thesis committee

Prof. Luís Santos-Pinto

University of Lausanne, Faculty of Business and Economics

Thesis supervisor

Prof. Rustamdjan Hakimov

University of Lausanne, Faculty of Business and Economics

Internal member of the thesis committee

Prof. Simeon Schudy

University of Munich, Department of Economics

External member of the thesis committee

University of Lausanne
Faculty of Business and Economics

PhD in Economics
Subject area Political Economy

I hereby certify that I have examined the doctoral thesis of

Paola COLZANI

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature: ____________ Date: 20.10.2020

Prof. Luís Pedro SANTOS-PINTO
Thesis supervisor

University of Lausanne
Faculty of Business and Economics

PhD in Economics
Subject area Political Economy

I hereby certify that I have examined the doctoral thesis of

Paola COLZANI

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature: ____________________ Date: 20. 10. 2020

Prof. Rustamdjan HAKIMOV
Internal member of the doctoral committee

University of Lausanne
Faculty of Business and Economics

PhD in Economics
Subject area Political Economy

I hereby certify that I have examined the doctoral thesis of

Paola COLZANI

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature: ______________________ Date: 04/11/2020

Prof. Simeon SCHUDY
External member of the doctoral committee

ACKNOWLEDGEMENTS

I would like to express my gratitude to my thesis supervisor, Professor Luìs Santos-Pinto, for providing guidance in the pursue of my PhD. I would like to thank him for his valuable comments and eagerness to help especially during my lab experiments.

I am also grateful to my co-author, Dr Georgia Michailidou, and to the members of my thesis committee, Professor Rustamdjan Hakimov and Professor Simeon Schudy, your suggestions were of great help for the improvement of my thesis.

Next, I would like to thank Professor Camille Terrier, Professor Joao Montez and Professor Jean-Paul Renne, members of the CPhD, as well as Professor Christian Zehnder, for their kind support and priceless advices.

I am also thankful to the LABEX team and the BEER Lab Meetings attendants, for their help and feedbacks. Running experiments was a challenging learning phase and yet the most interesting and motivating part of my PhD journey.

I am beyond grateful to my parents for the financial support they had provided for my former education and for the freedom I had in the choice of my own path. Without such elements all this would never have been possible.

Last but not least, I could not have completed my dissertation without the precious help of colleagues and friends: Valeria, Greta, Gema, Fabrizio, Valentina, Charles, Clemence, Vani, Giulia, Noemie, Costanza, Boris, Taka, Alberto, Moritz, Vaibhav, Mael, Jason, Matthieu, Sirio, Noëmi, Alice, Elio, Jeremy, David, Mark and Maha.

“ai miei pensieri

a com’ero ieri

e anche per me”

Dedicato - Loredana Bertè

Contents

Chapter 1

Does Overconfidence Lead to Bargaining Failures?

1.1 Introduction

One of the enduring puzzles in bargaining is why there is disagreement in cases where both parties would appear to be better off reaching an agreement. For example, the "gas wars" between Russia and Ukraine often lead to breakdowns in transit or supply that are inefficient (Tingle 2015). One prominent explanation for bargaining failures is asymmetric information (Fudenberg and Tirole 1981, Roth and Murnighan 1982, Sobel and Takahashi 1983, Bebchuk 1984, Kennan and Wilson 1993). Another prominent explanation is optimistic and self-serving biases (Bazerman and Neal 1982, Neal and Bazerman 1983, Neal and Bazerman 1985, Farber and Bazerman 1989, Thompson and Loewenstein 1992, Babcock et al. 1995, Babcock and Loewenstein 1997). If the parties involved in a legal dispute are mutually optimistic about their chances of winning in court they might fail to agree on a settlement (Priest and Klein 1984, Waldfogel 1995 and 1998, Farmer et al. 2004, Merlo and Tang 2019).[1] Yet, another potential explanation for bargaining failures is

[1]The parties' excessive optimism about their bargaining power in the future is a prominent explanation for costly delays before reaching an agreement (Hicks 1932, Farber and Katz 1979, Shavell 1982, Yildiz 2011). Yildiz (2003) shows that under bilateral bargaining over a deterministic surplus, optimism cannot cause bargaining delays. However, Ali (2006) finds that optimism can lead to costly delays when there are more than two bargainers, the surplus is deterministic, and bargainers are extremely optimistic More recently, Ortner (2013) shows that optimism can lead to costly delays under bilateral bargaining when the size of the surplus follows a stochastic process. Yildiz (2011) summarizes these

overconfidence about own performance (Bénabou and Tirole 2009).

Experimental evidence from Psychology and Economics shows that most people are overconfident, that is, they tend to overestimate their absolute skills, overplace themselves relative to others, and overestimate the precision of their private information, estimates, and forecasts.[2] Overconfidence affects behavior in goods markets (DellaVigna and Malmendier 2006, Grubb 2009) and in labor markets (Gervais and Goldstein 2007, DellaVigna 2009, Santos-Pinto 2010, Spinnewijn 2013, Kőszegi 2014, Spinnewijn 2015, Santos-Pinto and de la Rosa 2020). Overconfidence also plays a role in strategic decisions such as market entry (Camerer and Lovallo 1999) and mergers and acquisitions by CEOs (Malmendier and Tate 2005).

Most real-life bargaining situations involve the division of a surplus that is produced by the bargainers themselves (Karagözoğlu 2004). For example, business partners deciding how to share the profits of a partnership, labor negotiating with management over wages and states bargaining with each other over the ownership and sale of strategic resources (e.g., water, gas, oil). A large body of research shows that overconfidence leads people to overestimate their own contribution to joint tasks (Ross and Sicoly 1979, Van den Steen 2004). For example, Ross and Sicoly (1979) find that married couples overestimate their individual contributions to various household tasks they are responsible for such as making breakfast, cleaning house, shopping for groceries, and caring for children. Van den Steen (2004) shows that overconfident agents in cooperative venture tend to attribute success to their own choice of action and failure to the action choice of their associate. If overconfident bargainers overestimate their individual contributions to a joint project, this might lead to costly delays and even to disagreement.

This paper uses a laboratory experiment to investigate how overconfidence affects bargaining with joint production. Does overconfidence lead to bargaining failures? Does it lead to bargaining delays? If so, what are the mechanisms thought which this happens? For example, does overconfidence lead to disagreement in subjective entitlements and/or in opening proposals? Are overconfident bargainers less willing to make concessions?

results as follows "(...) optimism plays a subtle role in bargaining, and optimism alone cannot explain the bargaining delays."

[2]Moore and Healy (2008) distinguish between three types of overconfidence: (i) overestimation of one's absolute skills or performance, (ii) overestimation of one's relative skills or performance (overplacement or the "better-than-average" effect), and (iii) excessive confidence in the precision of one's private information, estimates, and forecasts (overprecision or miscalibration). We use the term overconfidence in the sense of overplacement.

In our experiment subjects start by performing a real-effort task which consists in answering a general knowledge quiz. We experimentally shift the subjects' self-confidence (self-placement) using a between-subjects design that randomly assigns half of the subjects to an easy quiz and the other half to a hard quiz. The easy and hard quizzes cause subjects to exhibit overconfidence and underconfidence, respectively. This manipulation uses the empirical finding that subjects tend to display overplacement in easy tasks and underplacement in hard tasks (Kruger 1999, Kruger and Dunning 1999, Krueger and Mueller 2002, Moore and Kim 2003, Moore and Healy 2008). Next, subjects are randomly matched into pairs. A pair's average rank in the quiz determines that pair's joint surplus which can be either low or high. A pair bargains over a low surplus when the pair's average rank in the quiz is higher than the average rank of their group, otherwise the pair bargains over a high surplus. Hence, the experiment has two quiz treatments – easy and hard – and two surplus size conditions – low and high.

After pairs are informed whether they will bargain over a low or a high surplus, subjects bargain anonymously with their partners over a computer network by sending proposals that consist of an amount for themselves and an amount for the partner. If an agreement is reached in 10 minutes the joint surplus is divided as agreed. If agreement is not reached by the end of the 10 minutes, bargaining ends with disagreement and subjects receive a zero payoff. This unstructured (or free-form) bargaining protocol follows Roth and Malouf (1979), Roth and Murnighan (1982), Gächter and Riedl (2005), and Karagözoğlu and Riedl (2014). Free-form bargaining allows us to analyze a rich set of layers of the bargaining process: agreements, opening proposals, concessions, and bargaining duration. Also, most bargaining in the real-life is unstructured, without the cut-and-dried rules of noncooperative models like Rubinstein (1982).

Our main experimental findings are as follows. First, the self-confidence manipulation is successful: subjects who take the easy quiz are, on average, overconfident while subjects who take the hard quiz are, on average, underconfident. Second, the percentage of bargaining failures when subjects take the easy quiz – 15% – is more than triple than when they take the hard quiz – 4%. Third, there is a remarkably high percentage of bargaining failures – 28% – when subjects take the easy quiz and bargain over a low surplus. Fourth, when subjects take the easy quiz and bargain over a high surplus, all pairs reach an agreement and most settle on an equal split. These results are in line with Bénabou and Tirole (2009)'s predictions about the effects of ego utility on bargaining with joint production. We discuss Bénabou and Tirole (2009) in Section 1.3.

Besides the main findings, our experiment provides five additional results on bargaining dynamics. First, there is more disagreement in subjective entitlements in the easy than in the hard quiz treatment. Second, we find no significant differences in disagreement in opening proposals across the two treatments. Third, there is more disagreement in opening proposals in the easy quiz and high surplus condition than in the hard quiz treatment. Fourth, concessions are the lowest in the easy quiz and low surplus condition. Fifth, we find no significant differences in bargaining duration across the two treatments and the two surplus size conditions.

Our experimental results shed light on the conditions and mechanisms under which overconfidence leads to bargaining failures. Overconfident bargainers feel entitled to a larger share of the surplus regardless of its size. When the surplus being negotiated is low, overconfident bargainers display a relatively low level of disagreement in opening proposals but are reluctant to make concessions and often fail to reach an agreement. Overconfident bargainers are reluctant to make concessions when the surplus is low because the utility gain from disagreement – maintaining a high self-confidence – is greater than monetary loss from disagreement – the share of the low surplus. In contrast, when the surplus being negotiated is high, overconfident bargainers display a high level of disagreement in opening proposals but are able to make concessions until an agreement is reached. Overconfident bargainers are able to make concessions when the surplus is high because the monetary gain from agreement – the share of the high surplus – is greater than the utility loss from agreement – the drop in self-confidence.

Our study contributes to the experimental literature on how judgement biases affect bargaining behavior. As we have seen, optimistic and self-serving biases are a prominent explanation for bargaining failures. This literature is composed of experimental studies (Bazerman and Neal 1982, Neal and Bazerman 1983, Thompson and Loewenstein 1992, Neal and Bazerman 1985, Farber and Bazerman 1989, Babcock et al. 1995, Babcock and Loewenstein 1997) and empirical studies (Waldfogel 1995 and 1998, Farmer et al. 2004, Merlo and Tang 2019). For example, Neal and Bazerman (1985) investigate the effects of optimism on labor-management negotiations and find that optimistic negotiators are less likely to display concessionary behaviors and reach an agreement than realistic negotiators. Thompson and Loewenstein (1992) find that self-serving, biased attention to available information in a conflict affected the parties' perceptions of fairness and the length of a strike in a simulated labor dispute. In Babcock and Loewenstein (1997), subjects are assigned the role of plaintiff or defendant in a tort case. Subjects need to reach a settlement and are asked to make a prediction regarding the judicial awards to the parties involved in the tort case. Babcock and Loewenstein (1997) find that self-serving judgements of fairness lead to a discrepancy in predictions regarding the judicial award which, in turn, lead to a settlement impediment. Walfogel (1998) attempts to distinguish one-sided asymmetric information from optimistic bias explanations using civil litigation data and finds support for the optimistic explanation. Farmer et al. (2004) analyze the causes of settlement failure using final-offer arbitration data from Major League Baseball and find that their results more consistent with an optimism model explanation than with one-sided asymmetric information models of arbitration.

Our study also contributes to the experimental literature on bargaining with joint production. In most of the experimental studies cited above subjects bargain over a surplus that is exogenously provided by the experimenter. Yet, bargaining commonly takes place between people who have produced the surplus they have to share. As Karagözoğlu (2004) points out "From a purely standard theoretical point of view, whether the surplus is produced by the bargaining parties or not should not make a difference since the costs incurred by the bargaining parties due to the production of the pie (e.g., cost of effort, investment, contribution) are sunk at the time when they sit at the bargaining table and thus should not affect bargaining behavior." However, experimental evidence shows that individuals' valuations and bidding behavior are heavily influenced by sunk costs they previously incur (Phillips et al. 1991, Hackett 1993). Hence, knowing if overconfidence affects bargaining with joint production is important for understanding real-life negotiations. Within the experimental literature on bargaining with joint production, the study that is closest to ours is Karagözoğlu and Riedl (2014). Karagözoğlu and Riedl (2014) analyze the impact of performance information and production uncertainties on bargaining over a jointly produced surplus. Karagözoğlu and Riedl (2014) manipulate performance information by telling subjects who is the best in a pair. The study finds that without performance information pairs tend to settle on the equal split while with performance information pairs reach asymmetric agreements in favor of the best in the pair and there are bargaining delays. Karagözoğlu and Riedl (2014) do not find bargaining failures. In stark contrast to Karagözoğlu and Riedl (2014), our study manipulates self-confidence and finds a remarkably high percentage of bargaining failures in the easy quiz and low surplus condition.

Finally, our study contributes to the growing literature on the impact of overconfidence on strategic interactions. Overconfidence has been shown to have both damaging (Camerer and Lovallo 1999; Malmendier and Tate 2005) and beneficial effects (Bénabou and Tirole 2002; Compte and Postlewaite 2004; Gervais and Goldstein 2007; Santos-Pinto 2010). Our study identifies conditions under which overconfidence can lead to bargaining failures.

The rest of the paper is organized as follows. Section 1.2 describes the experimental design. Section 1.3 presents the research hypotheses. Section 1.4 contains the results. Section 1.5 concludes the paper.

1.2 Experimental Design and Procedures

This section presents the experimental design and procedures.

The experiment is composed of two between subjects treatments which exogenously manipulate self-confidence using a general knowledge quiz. Half the subjects perform an easy quiz and the other half a hard quiz. On average, easy tasks induce overconfidence (overplacement) and hard tasks induce underconfidence (underplacement) due to the "hard-easy" effect (Kruger and Dunning 1999, Moore and Kim 2003, Moore and Small 2007, Moore and Healy 2008, Dargnies et al. 2019). Subjects believe to have lower (higher) ranks than their peers in easy (hard) tasks failing to realize that other subjects are facing the same level of difficulty. We label the two treatments EASY and HARD.

In each treatment subjects are randomly matched into pairs and each pair bargains over a joint surplus. The joint surplus is either low or high. If the pair's average rank in the quiz is smaller (greater) than the average rank of the group, then the pair bargains over a high (low) surplus. Hence, the experiment has two surplus size conditions which we label LOW and HIGH. Combining the two treatments with the surplus size conditions we have a total of four conditions: EASY-LOW, EASY-HIGH, HARD-LOW and HARD-HIGH.

The timing of the experiment is as follows. First, subjects perform a general knowledge quiz. Second, the joint surplus of each pair is determined. Third, subjects are informed about the size of the joint surplus. Fourth, subjects' beliefs about rank are elicited. Fifth, subjects in each pair are informed about who did best and worst on the quiz. Sixth, subjects' subjective entitlements are elicited. Seventh, subjects bargain over the joint surplus. Eighth, subjects' risk preferences are elicited. Ninth, subjects fill in a demographic questionnaire and are paid. We now explain each part of the experiment in detail.

1.2.1 Quiz

The quiz is composed of 46 questions which are divided into 6 different general knowledge topics: Science, Geography, Movies, Music, History, and Switzerland. We use questions from Moore and Healy (2008), update some of their questions, and add questions about Switzerland as we run the experiment at the University of Lausanne. Subjects have 20 minutes to complete the quiz. The number of correct answers in the quiz determines subjects' ranks. The average of subjects' ranks in turn determines a pair's surplus (and this is common knowledge). Groups are composed of 24 subjects.[3]

1.2.2 Joint Surplus

After completing the quiz, subjects are randomly and anonymously matched into pairs. The surplus of each pair is either high or low. The high surplus is worth CHF 39 and the low surplus is worth CHF 20.[4] The joint surplus of pair (i, j), denoted S_{ij}, is obtained as follows:

$$S_{ij} = \begin{cases} 39 & \text{if} \quad \frac{r_i + r_j}{2} \leq 12 \\ 20 & \text{if} \quad \frac{r_i + r_j}{2} > 12 \end{cases},$$

where r_i and r_j are the ranks of subjects i and j, respectively. Note that the average rank of a group of 24 subjects is equal to 12.5. Hence, if a pair's average rank in the quiz, $(r_i + r_j)/2$, is smaller (greater) than the average rank of the group, then the pair bargains over a high (low) surplus. This rule implies that the surplus is high when either both partners' ranks are above average or one partner's rank is above average and the other is not but the average of the partners' ranks is above average. The opposite is true for a low surplus. Importantly, the joint surplus is observable to the pair but exact individual contributions are not.

[3]In case of a tie, subjects who gave the same number of correct answers were assigned the same rank. Overall our data set, only two ties among members of the same pair appeared hence the impact of ties is negligible.

[4]The experimental points for the high and low surpluses, 2710 and 1370, respectively, are identical to those in Karagözoğlu and Riedl (2014) . However, payments in Karagözoğlu and Riedl (2014) are in Euros (€), while in our case, payments are in Swiss Francs (CHF). In Karagözoğlu and Riedl (2014) , 2710 and 1370 points correspond to 17.6 € and 9.0 €, respectively. This amounts are lower than the ones in Swiss Francs we pay. Hence, points being constant, we pay higher amounts to comply with laboratory payments norms at University of Lausanne.

1.2.3 Elicitation of Beliefs about Rank

After subjects are informed about the surplus size, we ask them to estimate their own rank and their partner's rank on the quiz. This allows us to measure self-confidence with respect to the group and with respect to the partner. Self-placement with respect to the group, b_{ii}, is the difference between a subject's rank and her estimate of own rank:

$$b_{ii} = r_i - E_i(R_i|S_{ij} = s),$$

where r_i is subject i's rank, $E_i(R_i|S_{ij} = s)$ is i's estimate of own rank conditional on the pair's surplus. Self-placement with respect to the group is zero when a subject correctly estimates her own rank, is positive (there is overplacement with respect to group) if the estimate is lower than her real rank, and negative (there is underplacement with respect to the group) if the estimate is higher than her real rank.

Since partners' ranks matter for the surplus assignment and for the bargaining stage we measure how subjects compare themselves with respect to their partners as well. Indeed, beliefs regarding relative performance in the pair and hence regarding relative contributions to the jointly produced surplus are likely to influence bargaining behavior. Following Moore and Healy (2008), self-placement with respect to the partner, Δb_{ij}, is measured by

$$\Delta b_{ij} = b_{ii} - b_{ij} = [r_i - E_i(R_i|S_{ij} = s)] - [r_j - E_i(R_j|S_{ij} = s)],$$

where r_j is subject j's rank (i's partner), and $E_i(R_j|S_{ij} = s)$ is i's estimate of j's rank conditional on the pair's surplus.

The estimates $E_i(R_i|S_{ij} = s)$ and $E_i(R_j|S_{ij} = s)$ are elicited with binarized scoring rules (Hossain and Okui 2013). The binarized scoring rule induces truth telling irrespective of subject's EU risk preference and even if subjects are non-EU maximizers. This is the main advantage of the binarized scoring rule over other scoring rules (e.g., the quadratic scoring rule) given the substantial evidence on heterogeneity in risk preferences (Hey and Orme 1994, Harless and Camerer 1994, Starmer 2000), which suggests that there is a majority of non-EU maximizers and a minority of EU maximizers (Bruhin et al. 2010, Conte et al. 2011, Bruhin et al. 2019).

Subject i's payoff for her estimate of her own rank is

$$\Pi_{ii} = \begin{cases} 2 & \text{if} \quad [E_i(R_i|S_{ij} = s) - r_i]^2 \leq k \\ 0 & \text{if} \quad [E_i(R_i|S_{ij} = s) - r_i]^2 > k \end{cases},$$

where $E_i(R_i|S_{ij} = s) - r_i^2$ is i's prediction error about her own rank, and k is a random number drawn from the uniform distribution with support on $[0, (n-1)^2]$, where n is the number of subjects in the group. Hence, subject i earns CHF 2 for her estimate of own rank if her prediction error squared is lower than k and otherwise earns CHF 0.

Subject i's payoff for her estimate of her partner's rank is:

$$\Pi_{ij} = \begin{cases} 2 & \text{if} \quad [E_i(R_j|S_{ij} = s) - r_j]^2 \leq k \\ 0 & \text{if} \quad [E_i(R_j|S_{ij} = s) - r_j]^2 > k \end{cases},$$

where $E_i(R_j|S_{ij} = s) - r_i^2$ is i's prediction error about her partner' rank. Hence, subject i earns CHF 2 for her estimate of her partner's rank if her prediction error squared is lower than k and otherwise earns CHF 0.

1.2.4 Information about Who is the Best in a Pair

We inform subjects about who was the best (and the worst) in the pair. Information about who is the best in a pair is a noisy signal about individual contributions to the joint surplus. We give this information to subjects for to two reasons. First, Karagözoğlu and Riedl (2014) show that without it most pairs agree on an equal-split. Second, we want to match the sequence of events in Karagözoğlu and Riedl (2014).

1.2.5 Elicitation of Subjective Entitlements

We define "subjective entitlement" as the fraction of joint surplus that a subject believes it is fair to keep for herself. For instance, if a subject believes that is it fair to equally split the joint surplus, her subjective entitlement is equal to 50%. To elicit subjects' subjective entitlements we use the formulation from Gächter and Riedl (2005) and ask subjects: "*According to your opinion, what would be a 'fair' distribution of the jointly produced surplus from the vantage point of a non-involved neutral arbitrator? (Please use exact amounts; no intervals! The amounts have to sum up to the jointly produced surplus!).*" Subjects are asked to enter on the computer screen which amount they think it is fair to keep for themselves and which amount they think it is fair to give to their partner. The two amounts need to sum up to the joint surplus. We divide the amount they think it is fair to keep for themselves by the surplus to have percentage measure and thus be able to compare subjects who receive high and low surpluses.

1.2.6 Bargaining

We implement an unstructured (or free-form) bargaining protocol following Roth and Malouf (1979), Roth and Murnighan (1982), Gächter and Riedl (2005), and Karagözoğlu and Riedl (2014). An unstructured bargaining protocol has three main advantages compared to a structured bargaining protocol (Nash 1953, Rubinstein 1982). First, it allows us to analyze a rich set of layers of the bargaining process: agreements, opening proposals, concessions, and bargaining duration. Second, most bargaining in the world is unstructured, without the cut-and-dried rules of noncooperative models like Rubinstein (1982). Third, it avoids exogenous first-mover effects. However, with free-form bargaining, there is no precise theoretical prediction for what the bargaining dynamics and outcomes will be. Still, classical cooperative bargaining solutions (e.g. Nash 1950) are often employed to predict agreements in free-form bargaining. In our free-form bargaining protocol the disagreement point is symmetric (both players get zero). If bargainers have the same utility function over money, $u(0) = 0$, then the bargaining problem is symmetric and the Nash (1950) bargaining solution predicts an equal split.

1.2.7 Elicitation of Risk Preferences

Risk preferences can play a large role in bargaining (Murnighan et al. 1988). If the randomization of subjects into treatments is successful there are no systematic differences in risk preferences across treatments. We believe that the random assignment of subjects to the two treatments was successful. Still, given the importance of risk preferences, we perform a randomization check. We use Crosetto and Filippin (2013)'s Bomb Risk Elicitation Task (BRET) to measure risk preferences. This task has five advantages compared to others. First, it allows to estimate both risk averse and risk seeking preferences very precisely. Second, it has a good trade-off between precision and comprehensibility. Third, it is defined on the gain domain and hence it does not suffer from loss aversion as a potential confound. Fourth, it does not provide endogenous reference points against which some outcomes could be perceived as losses. Fifth, it imposes a unique choice which prevents its results from being biased by violations of the Reduction Axiom. Subjects earn up to 2 CHF in the task depending on their choice. The BRET and the payoff allocation are accurately described in Appendix H.

1.2.8 Demographic Questionnaire and Payments

At the end of the experiment subjects fill in a demographic questionnaire. Thereafter, subjects are paid their earnings in cash individually and confidentially.

The experiment was computerized and programmed with the software z-Tree (Fischbacher 2007) and conducted at the LABEX (Laboratory for Behavioral Experiments) in the University of Lausanne. Subjects were recruited via ORSEE (Greiner 2015).

In total 190 subjects participated in 8 randomized experimental sessions. We had 7 sessions with 24 participants each and one session with 22 participants.[5] 94 subjects participated in the EASY treatment and 96 subjects in the HARD treatment. Sessions lasted on average 70 minutes. Most of the subjects were undergraduate students from different faculties at University of Lausanne and EPFL. The average earnings (including a show-up fee of CHF 10) were CHF 28. Experimental instructions can be found in Appendix J.

1.3 Research Hypotheses

This section presents our hypotheses on how self-confidence affects bargaining outcomes and dynamics.

1.3.1 Bargaining Outcomes

Neal and Bazerman (1985) and Babcock and Loewenstein (1997) show that overconfidence can lead to bargaining failures when subjects negotiate over an exogenous surplus. Similarly, we expect that overconfidence also leads to bargaining failures when subjects negotiate over a jointly produced surplus. This implies that we expect more bargaining failures when subjects take the easy quiz than when they take the hard quiz.

Hypothesis 1: *There are more bargaining failures when subjects take the easy quiz than when they take the hard quiz.*

An important feature of our experimental design is that the surplus to be negotiated is jointly produced and its size depends on the pair's average rank on the quiz. Our design resembles

[5] In one section only 22 participants showed up. We informed subjects that only 22 participants were in the room and explained that ranks would go from 1 to 22 and that neither the surplus assignment nor the payoff for belief elicitation would not be modified.

Bénabou and Tirole (2009)'s model of bargaining over a jointly produced surplus. In their model, two matched agents decide whether to keep or destroy their match. If the match is kept, they bargain over their joint surplus. Each agent has either low or high skill. The joint surplus is high if both agents have high skill and low otherwise. Importantly, agents are uncertain about their skills and individual contributions to the joint surplus are imperfectly recalled. Agents' utility is increasing with the share of the joint surplus and with ego utility (i.e. agents derive utility from having high beliefs about their skill). Bargaining is a standard Nash demand game (Nash 1953).

Our design is similar to Bénabou and Tirole (2009)'s model in three aspects. First, pairs are assigned a high or a low surplus depending on partners' performance in the quiz. Pairs whose average rank is smaller (greater) than the average rank of their group bargain over a high (low) surplus. If a pair is assigned a high surplus, it is more likely that both partners are high skilled; if a pair is assigned a low surplus, it is more likely that both partners are low skilled. Second, subjects are not informed about individual contributions to the joint surplus: similarly to Bénabou and Tirole (2009) in which subjects recall imperfectly their own contributions, here subjects only receive a noisy signal (the surplus size). Third, in Bénabou and Tirole (2009), agents derive utility not only from monetary gains but also from having high beliefs about their skills (ego utility). In our design, this translates into deriving utility not only from monetary gains but also from believing to be more skilled than others. In other words, subjects who are overconfident i.e. overplace themselves may derive positive utility by believing to be more skilled than others (and thus to have performed better than others in the quiz).

Bénabou and Tirole (2009) show that in a symmetric, pure-strategy Perfect Bayesian equilibrium, agents who bargain over a high surplus always reach an agreement and share the surplus equally. In contrast, agents who bargain over a low surplus only reach an agreement when ego utility concerns are low. If ego utility concerns are important, agents who bargain over a low surplus fail to reach an agreement. The intuition behind this result is as follows. Agreeing to inferior terms in a low surplus pair entails a loss in ego utility since it implies that at least one of the agents has low skill. When the surplus is low and ego utility concerns are important, the ego utility benefit from refusing to settle – maintaining a high self-confidence – is greater than the monetary cost of disagreement – the lost share of the low surplus.

In our design, in the easy treatment there is on average higher overplacement whereas in the hard treatment there is on average higher underplacement. Following Bénabou and Tirole (2009)'s predictions, we expect that bargaining failures are highest when subjects take the easy quiz and bargain over a low surplus. In addition, we expect that most pairs agree on the equal split when subjects take the easy quiz and bargain over a high surplus. Similarly to the intuition for the theoretical predictions in Bénabou and Tirole (2009), agreeing to inferior terms in a low surplus pair entails a loss in utility since it implies that at least one of the agents is less skilled than others (and has not performed as well as estimated in the quiz). Thus, when the surplus is low and overplacement is high, the utility of refusing to settle – thus maintain a high self-confidence – is greater than the monetary cost of disagreement – the lost share of the low surplus.

Hypothesis 2: *Bargaining failures are highest when subjects take the easy quiz and bargain over a low surplus.*

Hypothesis 3: *Most pairs agree on the equal split when subjects take the easy quiz and bargain over a high surplus.*

1.3.2 Bargaining Dynamics

At the beginning of the bargaining stage, when subjects exchange their first proposals, we can observe initial disagreement. We use two measures of initial disagreement: subjective entitlements and opening proposals. We expect subjects to be more likely to disagree at the start of the bargaining stage when they are overconfident. In other words, we expect more disagreement in subjective entitlements and opening proposals when subjects take the easy quiz than when they take the hard quiz.

Hypothesis 4: *Disagreement in subjective entitlements is higher when subjects take the easy quiz than when they take the hard quiz.*

Hypothesis 5: *Disagreement in opening proposals is higher when subjects take the easy quiz than in when they take the hard quiz.*

During the bargaining stage, subjects may overcome initial disagreement by making concessions. We expect overconfident subjects to concede less than underconfident ones. Hence, we expect to observe less concessionary behavior in the EASY than in the HARD treatment.

Hypothesis 6: *Concessions are lower when subjects take the easy quiz than when they take the hard quiz.*

As we have seen, Bénabou and Tirole (2009) predict that bargaining failures can arise when subjects bargain over a low surplus and ego utility concerns are important. We expect the interaction between overconfidence and surplus size to affect bargaining outcomes through concessions. Moreover, we expect overconfident subjects who bargain over a low surplus to be more reluctant to make concessions. In other words, we expect concessions to be the lowest in the EASY-LOW condition than in the other three conditions.

Hypothesis 7: *Concessions are the lowest when subjects take the easy quiz and bargain over a low surplus.*

Considering the existence of initial disagreement and the reluctance to concede part of the surplus in order to reach an agreement, we expect the bargaining duration to be longer when there is overconfidence.

Hypothesis 8: *Bargaining duration is longer when subjects take the easy quiz than when they take the hard quiz.*

We also expect the interaction between self-confidence and surplus size to affect bargaining duration. Moreover, we expect overconfident subjects who bargain over a low surplus to spend more time bargaining. In other words, we expect bargaining duration to be the longest in the EASY-LOW condition than in the other three conditions.

Hypothesis 9: *Bargaining duration is the longest when subjects take the easy quiz and bargain over a low surplus.*

1.4 Results

This section presents our results and is organized as follows. Section 1.4.1 reports the confidence manipulation. Section 1.4.2 presents results on bargaining outcomes: bargaining failures and surplus split. Section 1.4.3 presents results on bargaining dynamics: disagreement in subjective entitlements, disagreement in opening proposals, concessions, and bargaining duration.

1.4.1 Confidence Manipulation

As expected, the easy quiz resulted in higher scores (M $= 30.7$ out of 46, SD $= 7.15$) than did the hard quiz (M $= 8.4$ out of 46, SD $= 4.95$). The mean percentage of correct answers in the easy quiz, 64%, is highly significant greater than in the hard quiz, 17.5% (p-value < 0.01, 1-sided,

t-test).[6] As mentioned above, we measure overconfidence as self-placement with respect to the group and self-placement with respect to the partner.

Self-Placement with Respect to the Group

The top panel of Figure 1.1 depicts the means of self-placement with respect to the group in the two treatments. It shows that there is overplacement with respect to the group in the EASY treatment and underplacement in the HARD treatment.

The mean self-placement with respect to the group in the EASY treatment is equal to 2.04 and to -1.61 in the HARD treatment.[7]

The distributions of self-placement with respect to the group are highly significant different between the two treatments (p-value < 0.01, 1-sided, Kruskal-Wallis test).[8]

Result i: *There is overplacement with respect to the group when subjects take the easy quiz and underplacement when subjects take the hard quiz.*

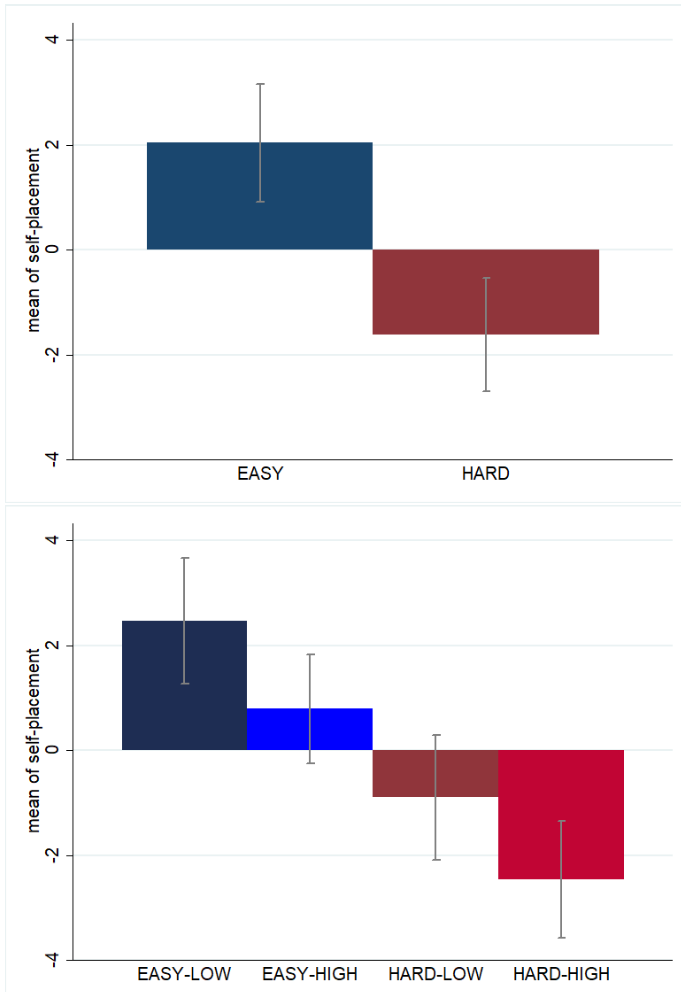
Being informed whether they will bargain over a high or a low surplus is a signal that subjects may use to update beliefs about their ranks. Knowing the surplus is high, a subject might revise her estimate of own rank towards higher relative performance on the quiz (towards the top ranks). Knowing the surplus is low, a subject might revise her estimate of own rank towards lower relative performance on the quiz (towards the bottom ranks). This leads us to expect higher overplacement with respect to the group in the EASY-HIGH condition than in the EASY-LOW condition and lower underplacement with respect to the group in the HARD-HIGH condition than in the HARD-LOW condition. However, low skill (bottom rank) subjects tend to overplace themselves whereas high

[6] From now on "highly significant" means with a p-value less than 1%, "significant" means with a p-value less than 5%, and "weakly significant" means with a p-value less than 10%.

[7] The mean estimated rank in the EASY treatment is equal to 10 and the mean estimated rank in the HARD treatment is equal to 14. The mean estimated rank in the EASY treatment is highly significant lower than the mean estimated rank in the HARD treatment (p-value < 0.01, 1-sided, t-test). We find a highly significant differences in the distributions of estimated ranks in the two treatments (p-value < 0.01, 1-sided Kruskal-Wallis test). The mean rank in the EASY treatment is equal to 12.04 and the mean rank in the HARD treatment is equal to 12.39. A 1-sided t-test and a 1-sided Kruskal-Wallist test highlight no differences in means nor in distributions in ranks.

[8] Note that self-placement with respect to the group can be compatible with Bayesian updating (Benoît and Dubra 2011). Appendix A shows that overplacement with respect to the group in the EASY treatment is incompatible with Bayesian updating whereas underplacement with respect to the group in the HARD treatment is compatible with Bayesian updating.

Figure 1.1: Mean self-placement with respect to the group



Notes: The top panel shows the means of self-placement with respect to the group in the EASY and HARD treatments; the bottom panel shows the means of self-placement with respect to the group in the EASY and HARD treatments and across LOW and HIGH surpluses. Spikes depict 95% confidence intervals.

skill (top rank) subjects tend to underplace themselves (Kruger and Dunning 1999). The way the joint surplus is determined implies that pairs that did worse on the quiz are more likely to bargain over a low surplus than pairs that did better on the quiz. Hence, a pair that bargains over a low surplus is more likely to be composed of partners who overplace themselves whereas a pair that bargains over a high surplus is more likely to be composed of partners who underplace themselves. Since this effect works in the opposite direction to the first it might well be that surplus size does not affect the self-confidence manipulation.

The bottom panel of Figure 1.1 depicts the means of self-placement with respect to the group in the four conditions. Self-placement with respect to the group is, on average, equal to 2.80 in the EASY-LOW condition, to 1.18 in the EASY-HIGH condition, to -0.67 in the HARD-LOW condition, and to -2.28 in the HARD-HIGH condition.[9,10] Hence, we observe overplacement with respect to the group in the EASY treatment and underplacement in the HARD treatment independently of the surplus size.

Self-Placement with Respect to the Partner

The top panel of Figure 1.2 depicts the means of self-placement with respect to the partner in the two treatments. It shows that there is overplacement with respect to the partner in the EASY treatment and underplacement in the HARD treatment. The mean self-placement with respect to the partner is equal to 2.01 in the EASY treatment and to -2.29 in the HARD treatment. The distributions of self-placement with respect to the partner are highly significant different between the two treatments (p-value < 0.01, 1-sided, Kruskal-Wallis test).

Result ii: *There is overplacement with respect to the partner when subjects take the easy quiz and*

[9]The mean estimated rank is equal to 12.48 in the EASY-LOW condition, to 7.18 in the EASY-HIGH condition, to 18.23 in the HARD-LOW condition, and to 10.98 in the HARD-HIGH condition. There are highly significant differences in means and distributions among all conditions (p-value < 0.01, 1-sided, t-test; p-value < 0.01, 1-sided, Kruskal-Wallis test in all cases besides the comparison among EASY-LOW and HARD-HIGH for which we find significant differences: p-value$= 0.029$, 1-sided, t-test; p-value $= 0.031$, 1-sided, Kruskal-Wallis test). The mean rank is equal to 15.28 in the EASY-LOW condition, to 8.36 in the EASY-HIGH condition, to 17.55 in the HARD-LOW condition, and to 8.69 in the HARD-HIGH condition. Further information on individual rank and pair composition can be found in Appendix F.

[10]There are highly significant differences among EASY-LOW and HARD-LOW, EASY-HIGH and HARD-HIGH, EASY-LOW and HARD-HIGH (p-value < 0.01, 1-sided, Kruskal-Wallis test). The difference among HARD-LOW and HARD-HIGH is weakly significant (p-value $= 0.09$, 1-sided, Kruskal-Wallis test).

underplacement with respect to the partner when subjects take the hard quiz.

Figure 1.2, bottom panel, depicts the means of self-placement with respect to the partner in the four conditions. Self-placement with respect to the partner is equal to 2.08 in the EASY-LOW condition, to 1.93 in the EASY-HIGH condition, to -3.7 in the HARD-LOW condition, and to -1.28 in the HARD-HIGH condition.[11]

Taken together, Results i and ii indicate that our manipulation worked: we observe overconfidence in the EASY treatment and underconfidence in the HARD treatment.

1.4.2 Bargaining Outcomes

This section reports results on how self-confidence and surplus size affect bargaining outcomes.

Bargaining Failures

To measure the percentage of pairs that failed to reach an agreement we use the variable "bargaining failures". The variable is a dummy equal to 1 if a pair failed to reached an agreement within 10 minutes and equal to 0 otherwise. We have one observation per pair and a total of 95 observations.

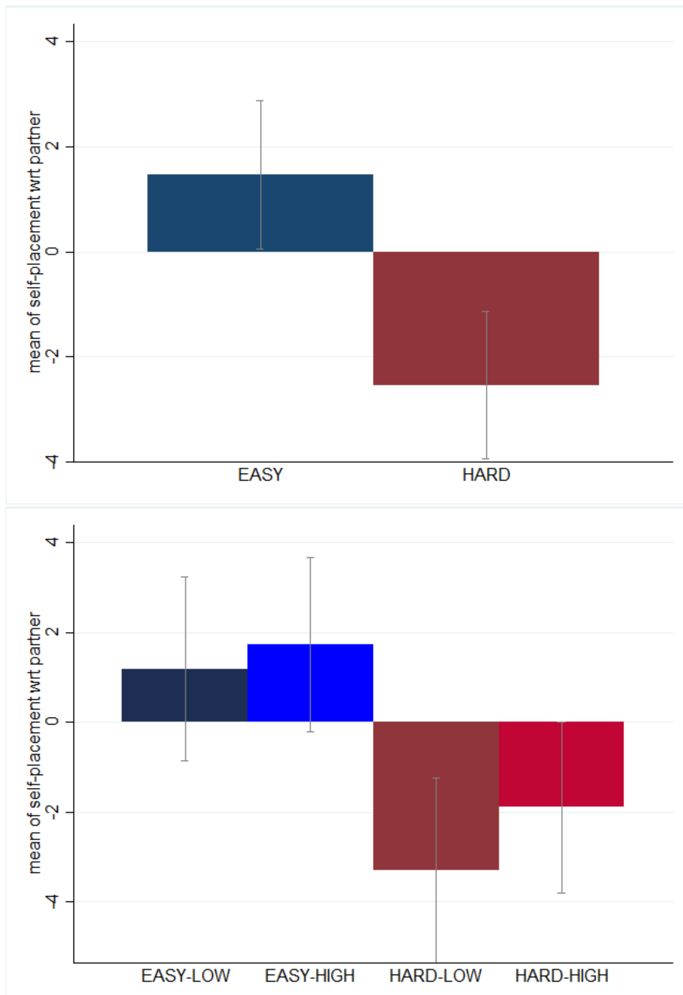
The top panel of Figure 1.3 displays the percentage of bargaining failures in the EASY and HARD treatments. In the EASY treatment 15% of pairs fails to reach an agreement. This is highly significant greater than zero (p-value < 0.01, 1-sided, t-test). In the HARD treatment only 4% of pairs fail to reach an agreement; this is weakly significant greater than zero (p-value $= 0.08$, 1-sided, t-test). Hence, the percentage of bargaining failures in the EASY treatment is more than the triple that in the HARD treatment. The percentage of bargaining failures in EASY treatment is highly significant greater than in the HARD treatment (p-value $= 0.01$, 1-sided, Fisher's exact test).

Result 1: *The percentage of bargaining failures when subjects take the easy quiz (15%) is more than the triple than when they take the hard quiz (4%).*

Result 1 suggests that overconfidence leads to bargaining failures when there is joint production. Result 1 is in line with our first hypothesis.

[11]There are highly significant differences in distributions among EASY-LOW and HARD-LOW (p-value < 0.01, 1-sided, Kruskal-Wallis test) and among EASY-HIGH and HARD-LOW (p-value < 0.01, 1-sided, Kruskal-Wallis test) while the difference among EASY-HIGH and HARD-LOW is weakly significant (p-value $= 0.08$, 1-sided, Kruskal-Wallis test).

Figure 1.2: Mean of self-placement with respect to the partner



Notes: The top panel shows the means of self-placement with respect to the partner in the EASY and HARD treatments; the bottom panel shows the means of self-placement with respect to the partner in the EASY and HARD treatments and across LOW and HIGH surpluses. Spikes depict 95% confidence intervals.

Let us now analyze how surplus size affects bargaining failures. The bottom panel of Figure 1.3 displays the percentage of bargaining failures in the four conditions. In line with our second hypothesis, the percentage of bargaining failures in the EASY-LOW condition (28%) is remarkably higher than the percentage of bargaining failures in the other three conditions. Indeed, there are no bargaining failures in the EASY-HIGH condition while the percentage of bargaining failures is equal to 5% in the HARD-LOW condition and to 3.6% in the HARD-HIGH condition. The percentage of bargaining failures in the EASY-LOW condition is highly significant greater than zero (p-value < 0.01, 1-sided, t-test) while this is not the case in the other three conditions (p-value > 0.3, 1-sided, t-test). In addition, the distribution of the percentage of bargaining failures in the EASY-LOW condition differs significantly from the other three conditions (p-value < 0.05, 1-sided, Kruskal-Wallis test).

Result 2: *There is a remarkably high percentage of bargaining failures (28%) when subjects take the easy quiz and bargain over a low surplus.*

Result 2 suggests that overconfident individuals who bargain over a low surplus often fail to agree. Result 2 is in line with our second hypothesis and the theory predictions in Bénabou and Tirole (2009). Appendix B shows that Result 2 is robust to demographic controls using an OLS regression of bargaining failures on condition dummies and demographic controls.

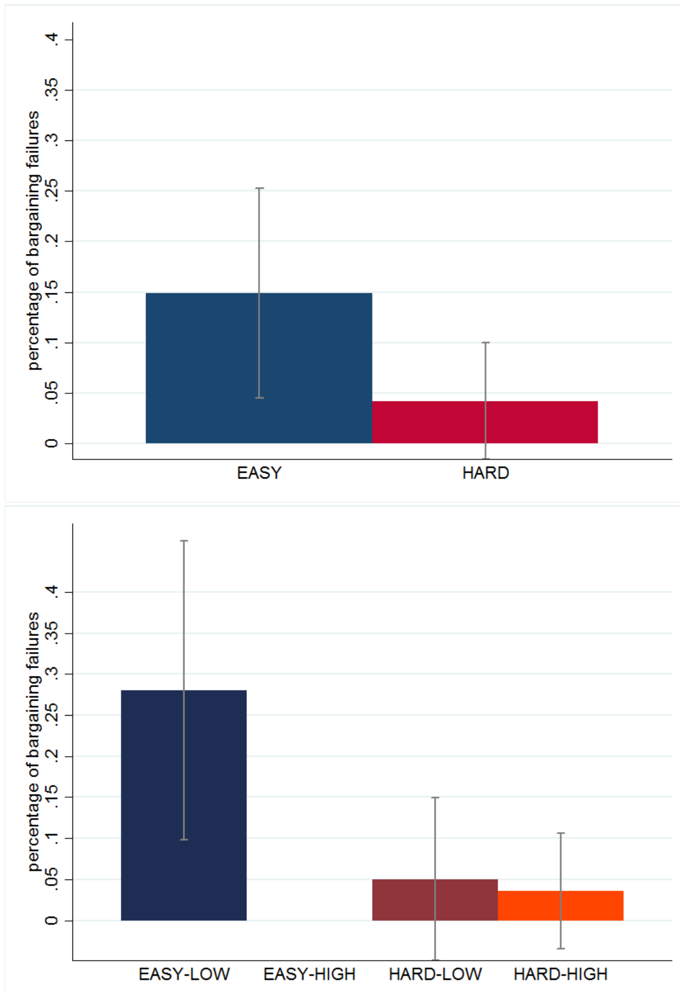
Surplus Splits

Here we analyze how pairs split the surplus across treatments. To avoid surplus size confounds we refer to percentages of the jointly produced surplus. Hence, the sum of the surplus splits of subject i and subject j in a pair is equal to 1. We have one observation per subject and a total of 190 observations.

Figure 1.4 shows the distributions of surplus splits in the four conditions. We observe more unequal splits in the EASY-LOW condition then in the other three conditions. In the EASY-LOW condition the distribution of surplus splits is bimodal with the 28% of pairs not being able to reach an agreement (as shown in section 1.4.2) and 28% of pairs settling on the equal split. The other three conditions show a unimodal distribution of surplus splits around the equal split.[12] The

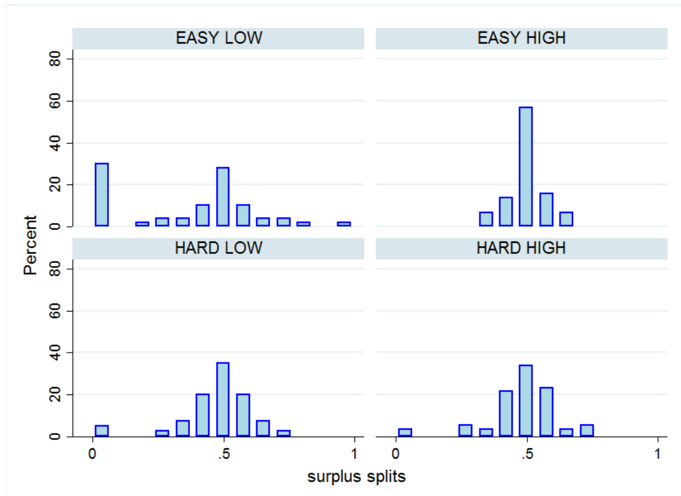
[12]There are significant differences in distributions among the EASY-LOW and the EASY-HIGH condition (p-value < 0.05, 1-sided, Kruskal-Wallis test) and among the EASY-HIGH and the HARD-HIGH condition (p-value < 0.05, 1-sided, Kruskal-Wallis test); the difference in distributions of surplus splits in the EASY-HIGH and the HARD-LOW

Figure 1.3: Percentage of bargaining failures



Notes: The top panel shows the percentage of bargaining failures in the EASY and HARD treatments; the bottom panel shows the percentage of bargaining failures in the EASY and HARD treatments and across LOW and HIGH surpluses. Spikes depict 95% confidence intervals with standard errors clustered at pair level.

Figure 1.4: Surplus splits



Notes: The graph shows the distribution of surplus splits in the EASY and HARD treatments across LOW and HIGH surpluses. Each band has 0.05 width.

percentage of pairs who settled for the equal split is equal to 54.5% in the EASY-HIGH condition, to 35% in the HARD-LOW condition and 28.5% in the HARD-HIGH condition.[13] Hence, in line with our third hypothesis, most of the agreements in the EASY-HIGH condition are on the equal split.

Result 3: *When subjects take the easy quiz and bargain over a high surplus, all pairs reach an agreement and most settle on an equal split.*

Result 3 suggests that overconfident individuals who bargain over a high surplus are always able to reach an agreement and most pairs settle on an equal split. Result 3 is in line with our third hypothesis and the theory predictions in Bénabou and Tirole (2009).

condition is weakly significant (p-value $= 0.074$, 1-sided, Kruskal-Wallis test).

[13] Since subjects were bargaining over values and not percentages, it is possible that many pairs did not precisely settle on the equal split but in a neighborhood of it. Hence we intend for equal split the 5% neighborhood around the 50-50 split. For instance, in the EASY-HIGH condition the 7% of pairs settles exactly on the equal split but almost the 30% of pairs settles on a 5% neighborhood of the equal split. To take this into consideration, in figure 1.4 each band has 0.05 width.

Recall that subjects are informed about who is the best and the worst in their pair. Conditional on agreement, partners who are the best in a pair get on average the 56% of the jointly produced surplus while subjects who are the worst in the pair get on average the 44% of the jointly produced surplus. There is a highly significant differences in the distribution of surplus splits for participants who are the best or the worst in a pair (p-value < 0.01, 1-sided, Kolmogorov-Smirnov test). This result is in line with Karagözoğlu and Riedl (2014). Appendix C reports an OLS regression with surplus splits as the dependent variable and condition dummies as well as demographic controls as explanatory variables. The regression show that subjects who are the best in a pair get on average higher surplus splits than subjects who are the worst in a pair.

Results 1, 2, and 3 could be due to differences in pair composition in terms of ranks. For example, there could have been more equal splits in pairs in which partners' ranks are closer together and more bargaining failures in pairs in which partners' ranks are further apart. Appendix F shows that this is not the case. In addition, Appendix G shows that Results 1, 2 and 3 are not driven by gender differences. However, we find two gender differences. First, males perform better in the quiz and are (marginally) more underconfident than females in the HARD quiz (underplacement of males but no underplacement of females). Second, females are more risk averse than males. Finally, Appendix H shows that there are no significant differences in attitude towards risk among conditions and Appendix I shows that the randomization of subjects across treatments was successful.

1.4.3 Bargaining Dynamics

This section reports results on how self-confidence and surplus size affect bargaining dynamics.

Disagreement in Subjective Entitlements

To measure disagreement in subjective entitlements we sum the fraction of the joint surplus each partner believes it is fair to keep for herself. The sum goes from 0, when each partner believes it is fair to give the full joint surplus to the partner, to 2, when each partner believes it is fair to keep the full joint surplus for herself. When the sum is greater than 1 there is disagreement in subjective entitlements. When the sum is equal to 1 there is agreement.[14] We have one observation per pair and a total of 95 observations.

[14]We sum fractions and not to amounts in order to avoid a surplus size confound.

Table 1.1: Disagreement in subjective entitlements

		LOW	HIGH	
	m	1.15	1.16	1.16
EASY	sd	(0.15)	(0.15)	(0.15)
	n	25	22	47
	m	1.06	1.09	1.08
HARD	sd	(0.13)	(0.12)	(0.12)
	n	20	28	48

Table 1.1 reports the means and standard deviations of disagreement in subjective entitlements in the EASY and HARD treatments and across HIGH and LOW surpluses as well as the number of observations in each condition. The mean disagreement in subjective entitlements is highly significant greater than 1 in the EASY and HARD treatments (p-value < 0.01, 1-sided, t-test). The mean disagreement in subjective entitlements is highly significant greater in the EASY than in the HARD treatment (p-value < 0.01, 1-sided, t-test) and the distribution in the EASY and HARD treatments are differ significantly (p-value $= 0.012$, 1-sided, Kruskal-Wallis test).

Result 4: *Disagreement in subjective entitlements is higher when subjects take the easy quiz than when they take the hard quiz.*

Result 4 suggests that overconfidence leads to disagreement in subjective entitlements. Overconfident bargainers feel entitled to a larger share of the surplus than underconfident bargainers. Result 4 is in line with our fourth hypothesis.

Next, we analyze whether surplus size affects disagreement in subjective entitlements. The mean disagreement in subjective entitlements is significantly greater than 1 in each of the four conditions (p-value < 0.05, 1-sided, t-test). The distributions of disagreement in subjective entitlements in the EASY-HIGH and EASY-LOW conditions are not significantly different (p-value $= 0.88$, 1-sided, Kruskal-Wallis test). The distributions of disagreement in subjective entitlements in the HARD-HIGH and HARD-LOW conditions are also not significantly different (p-value $= 0.19$, 1-sided, Kruskal-Wallis test). Hence, we conclude that surplus size has no effect on disagreements in subjective entitlements.

Table 1.2: Disagreement in opening proposals

		LOW	HIGH	
EASY	m	1.24	1.31	1.27
	sd	(.29)	(.25)	(.27)
	n	25	22	47
HARD	m	1.18	1.18	1.18
	sd	(.21)	(.15)	(.18)
	n	20	28	48

Appendix D performs additional analysis on subjective entitlements. It discusses how being the best or the worst in a pair affects subjective entitlements. It reports a Tobit regression with surplus splits as a dependent variable and condition dummies as explanatory variables (and demographic controls). The regression shows that subjects who are the best in a pair are more likely to have higher subjective entitlements than subjects who are the worst in a pair.

Disagreement in Opening Proposals

At the beginning of the bargaining stage, each subject sends an opening proposal indicating which part of the surplus she is willing to keep for herself and which part of the surplus she is willing to give to her partner. To measure disagreement in opening proposals we sum the fraction of the joint surplus each partner wants to keep for herself in her opening proposal. The sum goes from 0, when each partner wants to give the full joint surplus to her partner, to 2, when each partner wants to keep the full joint surplus for herself. When the sum is higher than 1, there is disagreement in opening proposals. When the sum is equal to 1 there is agreement.[15] We have one observation per pair and a total of 95 observations.

Table 1.2 reports the means and standard deviations of disagreement in opening proposals in the EASY and HARD treatments and across HIGH and LOW surpluses as well as the number of observations in each condition. The mean disagreement in opening proposals is statistically greater than 1 in the EASY and HARD treatments (p-value < 0.01, 1-sided, t-test). However, contrary

[15] Again, we refer to fractions and not to amounts in order to avoid surplus size confounds.

to our fifth hypothesis, the distributions of disagreement in opening proposals in the EASY and HARD treatments are not significantly different (p-value $= 0.13$, 1-sided, Kruskal-Wallis test).

Result 5: *There are no significant differences in disagreement in opening proposals across easy and hard quizzes.*

Next, we analyze whether surplus size affects disagreement in opening proposals. The mean disagreement in opening proposals is highly significant greater than 1 in each of the four conditions (p-value < 0.01, 1-sided, t-test). The distributions of disagreement in opening proposals in the EASY-HIGH and EASY-LOW conditions are not significantly different (p-value $= 0.14$, 1-sided, Kruskal-Wallis test). Similarly, the distributions of disagreement in opening proposals in the HARD-HIGH and HARD-LOW conditions are not significantly different (p-value $= 0.66$, 1-sided, Kruskal-Wallis test). However, the distributions of disagreement in opening proposals in the EASY-HIGH and HARD-LOW conditions are significantly different (p-value < 0.05, 1-sided, Kruskal-Wallis test) and weakly significant different in the EASY-HIGH and HARD-HIGH conditions (p-value $= 0.06$, 1-sided, Kruskal-Wallis test).

Result 6: *Disagreement in opening proposals is higher when subjects take the easy quiz and bargain over a high surplus then when they take the hard quiz and bargain over either a low or a high surplus.*

The differences among disagreement in subjective entitlements and disagreement in opening proposals may stem from the fact that subjective entitlements are non-binding verbal statements while opening proposals can be binding if the partners accept them.

Concessions

We define as concession the difference between the fraction of the surplus a subject demands for herself in her opening proposal (independently of who was the first mover in a pair) and the fraction of the surplus the subject obtains. A concession is positive if a subject has to give up on part of her opening proposal in order to reach an agreement. A concession is negative if a subject obtains a higher fraction of the surplus than her opening proposal in order to reach an agreement. A concession is equal to zero if a subject belongs to a pair that did not reach an agreement. We have one observation per subject and a total of 178 observations since 12 subjects accepted their partner's opening proposal (for their partners the concession is equal to zero since they obtained exactly what they asked for).

Table 1.3: Concessions

		LOW	HIGH	
EASY	m	6%	16%	11%
	sd	(16%)	(16%)	(17%)
	n	47	42	89
HARD	m	8%	9%	9%
	sd	(11%)	(11%)	(11%)
	n	38	51	89

Table 1.3 reports the means and standard deviations of concessions in the EASY and HARD treatments and across HIGH and LOW surpluses as well as the number of observations in each condition. Mean concessions are highly significant greater than zero in the EASY and HARD treatments (p-value < 0.01, 1-sided, t-test). However, contrary to our sixth hypothesis, the distributions of concessions in the EASY and HARD treatments are not significantly different (p-value $= 0.71$, 1-sided, Kruskal-Wallis test).

Result 7: *There are no significant differences in concessions across easy and hard quizzes.*

Next, we analyze whether surplus size affects concessions. Mean concessions are highly significant greater than zero in each of the four conditions (p-value < 0.01, 1-sided, t-test). Table 1.3 shows, in line with our seventh hypothesis, that concessions are the lowest in the EASY-LOW condition. There are significant differences in distributions among the EASY-LOW condition and the other three conditions (p-value < 0.05, 1-sided, Kruskal-Wallis test). There are significant differences in distributions among the EASY-HIGH condition and the other three conditions (p-value < 0.05, 1-sided, Kruskal-Wallis test).[16]

Result 8: *Concessions are the lowest (6%) when subjects take the easy quiz and bargain over a low surplus. Concessions are the highest (16%) when subjects take the easy quiz and bargain over a high surplus.*

Results 2, 6, and 8 can be explained as follows. When overconfident subjects bargain over a

[16]We do not find significant differences among the HARD-LOW and HARD-HIGH condition (p-value $= 0.89$, 1-sided, Kruskal-Wallis test).

Table 1.4: Mean bargaining duration

		LOW	HIGH	
EASY	m	304	347	325
	sd	(249)	(216)	(232)
	n	25	22	47
HARD	m	302	301	301
	sd	(230)	(227)	(226)
	n	20	28	48

low surplus, disagreement in opening proposals is not remarkable but many subjects do not display concessionary behavior which often leads to bargaining failures. When overconfident subjects bargain over a high surplus, disagreement in opening proposals is substantial but subjects display concessionary behavior which allows all pairs to reach an agreement.

Bargaining Duration

We define as bargaining duration the time that each pair takes to reach an agreement. One observation refers to one pair and we have 95 observations. Pairs bargain on average for 313 seconds, approximately 5 minutes. Table 1.4 reports the means and standard deviations of bargaining duration in the EASY and HARD treatments and across LOW and HIGH surpluses as well as the number of observations in each condition. Contrary to our eighth and ninth hypotheses, the mean bargaining duration is not significantly different across the four conditions (in all comparisons we observe p-value > 0.3, 1-sided, Kruskal-Wallis test).

Result 9: *There are no significant differences in bargaining duration across the four conditions.*

Result 9 is surprising given the remarkably high percentage of bargaining failures observed in the EASY-LOW condition (28%) and the absence of bargaining failures in the EASY-HIGH condition. To try to make sense of this result we analyze bargaining duration across three bargaining outcomes: bargaining failure, agreement on the equal split, and agreement on other splits. Table 1.5 reports the percentage of pairs and the mean bargaining duration for each of the three bargaining outcomes.

Table 1.5: Percentage of pairs and mean bargaining duration depending on bargaining outcome

	Mean	Barg. Failure		Equal Split		Other Split	
	t	%	t	%	t	%	t
EASY-LOW	304	28	600	28	190	44	188
EASY-HIGH	347	0	-	54.5	333	45.5	364
EASY	325	15	600	40	280	45	272
HARD-LOW	301	5	600	35	264	60	299
HARD-HIGH	302	3.6	600	28.5	262	68	301
HARD	301	4	600	31	263	65	300

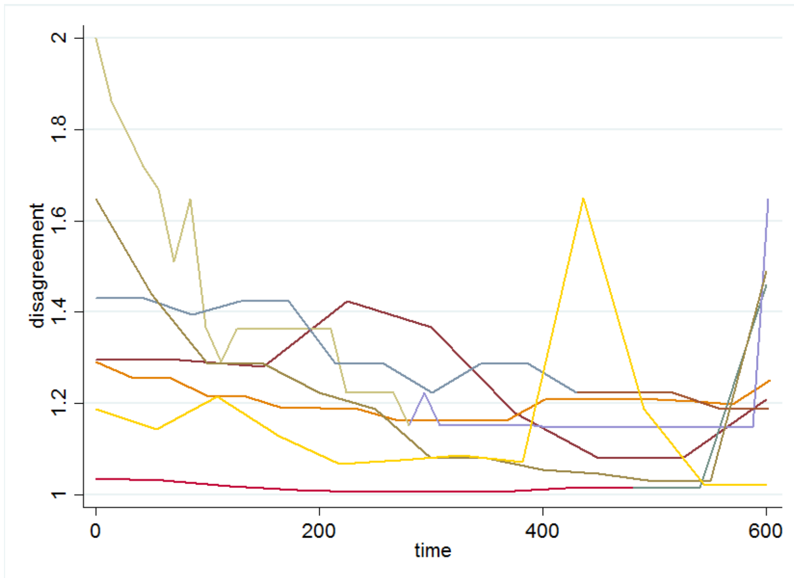
Table 1.5 shows that pairs in the EASY-LOW condition who settle on the equal split bargain on average 190 seconds whereas pairs in the EASY-HIGH condition who settle on the equal split bargain on average 333 seconds. Similarly, pairs in the EASY-LOW condition who settle on an unequal split bargain on average 188 seconds whereas pairs in the EASY-HIGH condition who settle on an unequal split bargain on average 364 seconds. Hence, in the EASY treatment, subjects are faster reaching an agreement when they bargain over a low surplus than when they bargain over a high surplus.[17] The shorter bargaining time of pairs in the EASY-LOW condition who reach an agreement compensates for the longer bargaining time of pairs in the EASY-LOW condition who fail to reach an agreement. This is the reason why the mean bargaining duration in the EASY-LOW and EASY-HIGH conditions is not significantly different.

For a better understanding of the dynamics that turned disagreement in opening proposals into bargaining failures in the EASY-LOW condition, we analyze how disagreement has evolved over time. Figure 1.5 reports disagreement dynamics for the pairs who failed to reach an agreement. In total, 7 pairs failed to reach an agreement in the EASY-LOW condition.

Similarly to disagreement in opening proposals, we measure disagreement at each point in time as the sum of partners' standing proposals at that time divided by the jointly produced surplus of the pair. We refer to fractions and not to amounts in order to avoid surplus size confounds. Disagreement is equal to one if partners agree on how to split the surplus and higher than one if partners disagree on how to split the surplus. The maximum value of disagreement is 2: in this

[17] This is not the case in the HARD treatment.

Figure 1.5: Disagreement over time



Notes: The graph shows the dynamics of disagreement for those pairs who failed to reach an agreement. Each line refers to one pair.

case both partners want to keep the whole surplus per se.[18]

As we can see from the graph, only one pair (in red) started with the maximum possible level of disagreement: this pair smoothed disagreement over time but was nevertheless not able to reach an agreement. Overall disagreement follows a decreasing pattern in the first half of the bargaining dynamics. However, none of the seven pairs was able to reach an agreement even when disagreement is reduced and some other pairs seem to strongly disagree at the end.

[18]Note that a few assumptions were needed to realize this graph. First, since we implement free-form bargaining, one partner can send to the other multiple proposals without waiting for a counter offer. Whenever this happened, we computed disagreement as the sum of the current standing proposal of one partner and the former proposal of the other one. In practice, we interpret the absence of a new proposal as standing by the very same proposal (while the other partner is sending new offers). Second, we failed to record the exact timing of each single proposal. However, we could precisely record bargaining duration and the number of proposals sent. Hence we approximate the timing of each proposal simply dividing the bargaining duration (600 seconds in case of disagreement) by the number of proposals sent by each partner. The second assumption also implies a third one: that proposals were sent at regular time intervals.

1.5 Concluding Remarks

In this paper we explore the effect of self-confidence on bargaining with joint production. To do that we exogenously manipulate self-confidence using easy and hard quizzes. The manipulation is successful: we find overconfidence in the easy quiz and underconfidence in the hard quiz (Results i and ii). Self-confidence affects bargaining outcomes. There are three times as many bargaining failures when subjects take the easy quiz than when they take the hard quiz (Result 1). There is a remarkably high number of bargaining failures when subjects take the easy quiz and bargain over a low surplus (Result 2). In contrast, when subjects take the easy quiz and bargain over a high surplus, all pairs reach an agreement and most pairs agree on the equal split (Result 3). These three results are in line with the theory predictions in Bénabou and Tirole (2009).

Self-confidence also affects bargaining dynamics. There is higher disagreement in subjective entitlements in the easy quiz than in the hard quiz (Result 4). This suggests that overconfident bargainers feel entitled to a larger share of the surplus than underconfident ones. In contrast, disagreement in opening proposal is similar across easy and hard quizzes (Result 5). However, disagreement in opening proposals is higher when subjects take the easy quiz and bargain over a high surplus then when they take the hard quiz and bargain over either a low or a high surplus (Result 6). There are no significant differences in concession across the easy and hard quizzes (Result 7), However, concessions are the lowest in the EASY-LOW condition and the highest in the EASY-HIGH condition (Result 8). These results show that overconfident subjects who bargain over a high surplus are willing to make concessions and reach an agreement. In contrast, overconfident subjects who bargain over a low surplus are reluctant to make concessions and, as a consequence, often fail to reach an agreement. Surprisingly, bargaining duration is not significantly different across the four conditions (Result 9).

We contribute to the literature on bargaining with joint production by showing that overconfidence can lead to inefficient bargaining outcomes when the surplus under negotiation is low. The remarkable high level of bargaining failures we find when subjects take the easy quiz and bargain over a low surplus stands in stark contrast with previous experiments on bargaining with joint production which do not find such high levels of bargaining failures.

Appendix

A. Self-Placement and Bayesian Updating

Self-placement with respect to the group can be compatible with Bayesian updating (Benoît and Dubra 2011). Burks et al.(2013) propose a simple rule to test whether overplacement (and underplacement) are compatible with Bayesian updating. This rule is called the "allocation function." According to the allocation function, when subjects are asked to estimate the skill level they most likely belong to, the largest (modal) group of subjects believing they belong to a certain skill level should be included in that skill level. When this allocation function is violated, Bayesian updating is rejected. In Burks et at. (2013) subjects are asked to indicate the most likely skill level (or the mode of their distribution of beliefs about skill). However, in our experiment, subjects are asked to estimate their mean rank not their modal rank. The allocation function is valid under the assumption that subjects have belief distributions where the mean and mode are not very far apart.

Tables 1.6 and 1.7 show how estimates of own ranks are related to the actual ranks in the EASY and HARD treatments, respectively (see also Figure 1.6). Subjects who display overplacement are below the diagonal since they estimate to have a higher rank than their actual rank. Subjects who display underplacement are above the diagonal since they estimate to have a lower rank than their actual rank. Table 1.6 shows that there is overplacement in the EASY treatment: 39 observation are on the diagonal, 41 observations are below the diagonal, and 14 observations are above the diagonal. Table 1.7 shows that there is underplacement in the HARD treatment: 44 observation are on the diagonal, 20 observations are below the diagonal, and 32 observations are above the diagonal.

Tables 1.6 and 1.7 allow us to investigate whether self-placement with respect to the group is compatible with Bayesian updating. If this is the case, then the largest number of subjects who estimate their rank to be in the first quartile should belong to the first quartile of actual ranks, the largest number of subjects who estimate their rank to be in the second quartile should belong to the second quartile of actual ranks, and so on. Table 1.6 shows that overplacement in the EASY treatment is incompatible with Bayesian updating. The allocation function in the EASY treatment is violated by subjects who believe their ranks belong to the third quartile. Among the 27 subjects who believe to be in the third quartile, only 8 belong to that quartile. This is less than the 14 subjects who belong to fourth quartile. In contrast, Table 1.7 shows that underplacement in the

Table 1.6: Relations among estimates and actual ranks, EASY treatment

EASY		estimates				
		BEST			WORST	
actual		$\{1;6\}$	$\{7;12\}$	$\{13;18\}$	$\{19;24\}$	n
BEST	$\{1;6\}$	**15**	8	1	0	24
	$\{7;12\}$	4	**16**	4	0	24
	$\{13;18\}$	2	13	**8**	1	24
WORST	$\{19;24\}$	1	7	14	**0**	22
	n	22	44	27	1	94

Table 1.7: Relations among estimates and actual ranks, HARD treatment

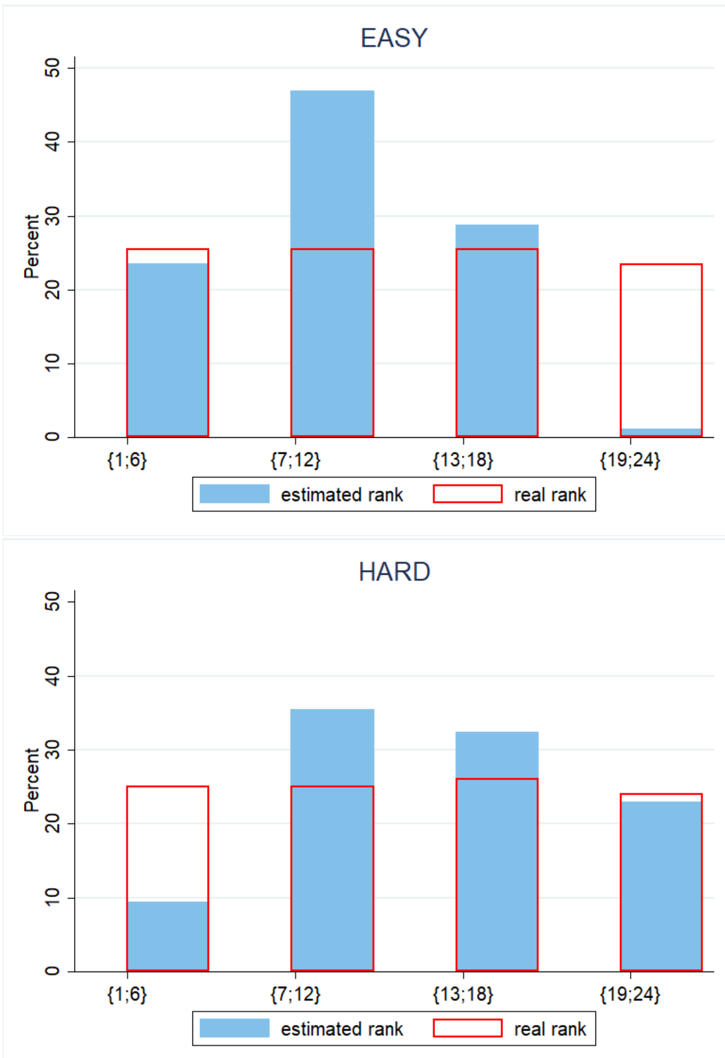
HARD		estimates				
		BEST			WORST	
actual		$\{1;6\}$	$\{7;12\}$	$\{13;18\}$	$\{19;24\}$	n
BEST	$\{1;6\}$	**6**	10	8	0	24
	$\{7;12\}$	3	**13**	6	2	24
	$\{13;18\}$	0	8	**11**	6	25
WORST	$\{19;24\}$	0	3	6	**14**	23
	n	9	34	31	22	96

HARD treatment is compatible with Bayesian updating. The allocation function in the HARD treatment is not violated by subjects who believe their ranks belong to the first, second, third, and fourth quartiles.

B. Bargaining Failures

Table 1.8 presents an OLS model regression with bargaining failures as dependent variable. Bargaining failures is a dummy variable which equals 1 if the pair did not reach an agreement, and 0 otherwise. The explanatory variables are three dummies, one for condition EASY-LOW, another one for condition HARD-LOW, and the third one for condition. The variable BEST equals 1 if a subject is the best in the pair, and 0 otherwise. We add demographic controls which are described

Figure 1.6: Real Rank vs Estimates - EASY and HARD



in Appendix I. Robust standard errors are clustered at pair level. We have 95 observations, one observation per pair.

Table 1.8 shows that the estimated coefficient for the EASY-LOW dummy is positive which provides support for Result 2: The percentage of bargaining failures is higher in the EASY-LOW condition than in the other conditions.[19]

C. Surplus Splits

Table 1.9 presents a Tobit regression with surplus splits as shares of the jointly produced surplus as dependent variable. The explanatory variables are three dummies, one for condition EASY-LOW, another one for condition HARD-LOW, and the third one for condition. The variable BEST equals 1 if a subject is the best in the pair, and 0 otherwise. We add demographic controls which are described in Appendix I. Robust standard errors are clustered at pair level. We exclude bargaining failures from the analysis and thus have 172 observations.

Table 1.9 shows that the dummy BEST plays a significant role: subjects who are the best in a pair obtain on average higher surplus splits than subjects who are the worst in a pair.[20] as mentioned in 1.4.2. Figure 1.7 reports the distributions of surplus splits for best and worst partner in the pair: the distribution of surplus splits is asymmetric in favor of the best in the pair.

This result is in line with Karagözoğlu and Riedl (2014) since they find that subjects who are the best in a pair display higher subjective entitlements than subjects who are the worst in a pair. This in turn implies that subjects who are the best in a pair get higher surplus splits than subjects who are the worst in the pair. We discuss subjective entitlements in Appendix D.

D. Subjective Entitlements

As mentioned, the paper closest to ours is Karagözoğlu and Riedl (2014). Karagözoğlu and Riedl (2014) study the impact of performance information and production uncertainties on bargaining with joint production. Karagözoğlu and Riedl (2014) show that performance information affects bargaining outcomes through subjective entitlements. When subjects are informed about their rel-

[19] A LOGIT regression would have been a suitable model for bargaining failures, however we are unable to implement it since the dummy variable for the EASY-HIGH condition only assumes the value zero given that all pairs reached an agreement.

[20] Equivalent results are found running OLS regressions instead of Tobit regressions.

Table 1.8: OLS Regression: Bargaining failures on condition dummies and controls

	Barg. Failures	
	Coeff.	Std. Error
EASY-LOW	0.265^{***}	(0.0895)
HARD-LOW	0.0349	(0.0752)
HARD-HIGH	0.0403	(0.0520)
BEST	−0.0331	(0.0654)
Risk Averse	0.0382	(0.0557)
Gender	−0.0278	(0.0563)
Swiss Nat.	−0.0463	(0.0686)
Unil	−0.0854	(0.0843)
Age	0.00141	(0.0233)
Grades	0.00452	(0.0611)
Bachelor	−0.0209	(0.0936)
Grad. Parents	0.0129	(0.0771)
onlychild	0.00117	(0.0940)
Big Town	0.0685	(0.0932)
People Known	−0.0189	(0.0235)
Constant	0.0393	(0.530)
R^2	0.216	
Adjusted R^2	0.067	
Observations	95	

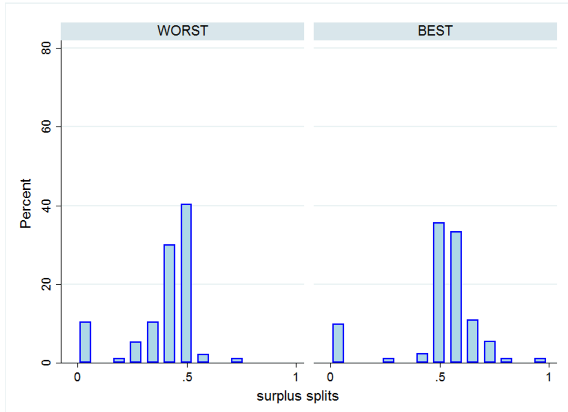
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9: Tobit Regression: Surplus splits on condition dummies and controls, conditional on agreement

	Surplus Splits	
	Coeff.	Std. Error
EASY-LOW	0.00275	(0.00848)
HARD-LOW	−0.00525	(0.00728)
HARD-HIGH	−0.00655	(0.00754)
Risk Averse	−0.0270	(0.0189)
BEST	0.125***	(0.0189)
Gender	−0.0163	(0.0126)
Swiss Nat.	−0.00511	(0.0137)
Unil	−0.0120	(0.0141)
Age	−0.00322	(0.00277)
Grades	0.00858	(0.00660)
Bachelor	0.00372	(0.0156)
Grad. Parents	−0.000654	(0.0188)
onlychild	−0.0182	(0.0259)
Big Town	0.0258	(0.0213)
People Known	−0.0180**	(0.00877)
Constant	0.506***	(0.0730)
sigma	0.0860***	(0.0134)
Pseudo R^2	−0.295	
Observations	172	

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

Figure 1.7: Surplus splits



Each band has 0.05 width.

ative contributions to the joint surplus, they feel entitled to split the surplus accordingly. Hence, subjects who are the best in a pair have higher subjective entitlements than subjects who are the worst in a pair. As a consequence, surplus splits are in favor of the best in a pair.

In line with Karagözoğlu and Riedl (2014), we find that the average subjective entitlement for subjects who are the best in a pair is higher and equal to 63% and the average subjective entitlement for subjects who are the worst in a pair is lower and equal to 49%. Figure 3.4.4 shows that the distribution of subjective entitlements is highly significant different across subjects who are the best and the worst in a pair (p-value < 0.01, 1-sided, Kruskal-Wallis test).

Table 1.10 presents a Tobit regression with subjective entitlements as shares of the jointly produced surplus as dependent variable. The explanatory variables are three dummies, one for condition EASY-LOW, another one for condition HARD-LOW, and the third one for condition. The variable BEST equals 1 if a subject is the best in the pair, and 0 otherwise. We add demographic controls which are described in Appendix I.Robust standard errors are clustered at pair level. We have 190 observations.

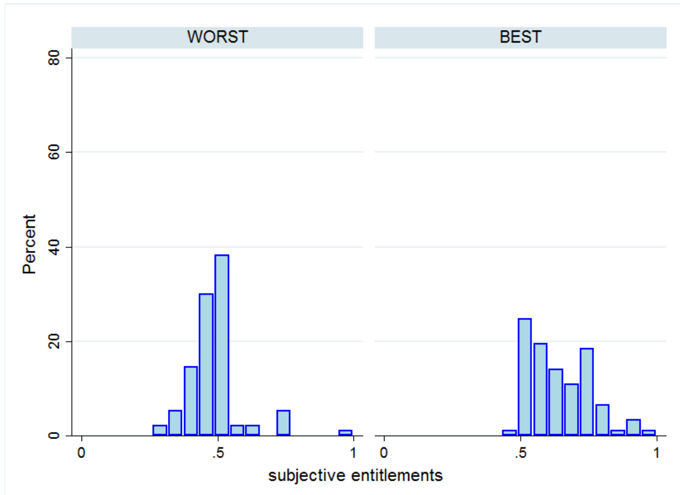
Table 1.10 shows that hat the dummy BEST plays a significant role: subjects who are the best in a pair have on average higher subjective entitlements than subjects who are the worst in a pair, confirming that information about who is the best in a pair has an impact on subjective entitlements. Neither the self-confidence manipulation nor the surplus size have an impact on subjective entitlements.

Table 1.10: Tobit Regression: Subjective entitlements on condition dummies and controls

	Subjective Entitlements	
	Coeff.	Std. Error
EASY-LOW	-0.000354	(0.0226)
HARD-LOW	-0.0450	(0.0230)
HARD-HIGH	-0.0295	(0.0213)
Risk Averse	0.00922	(0.0171)
BEST	0.150^{***}	(0.0173)
Gender	0.00597	(0.0167)
Swiss Nat.	0.00131	(0.0155)
Unil	-0.00786	(0.0165)
Age	0.000988	(0.00410)
Grades	-0.00637	(0.0124)
Bachelor	0.00516	(0.0240)
Grad. Parents	-0.0196	(0.0195)
onlychild	0.00557	(0.0235)
Big Town	0.0255	(0.0190)
People Known	0.000824	(0.00890)
Constant	0.504^{***}	(0.129)
sigma	0.103^{***}	(0.00809)
Pseudo R^2	-0.370	
Observations	190	

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

Figure 1.8: Subjective entitlements



Notes: the graph shows the distribution of subjective entitlements distinguishing among subjects who are the worst (left panel) and subjects who are the best (right panel) in a pair. Each band has 0.05 width.

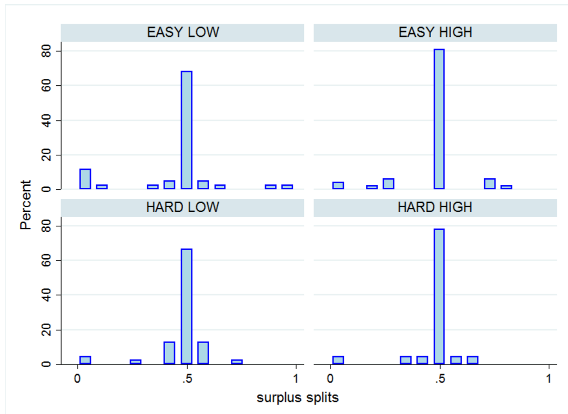
Karagözoğlu and Riedl (2014) run a treatment where subjects do not receive information about who is the best and worst in a pair. They find that in this case most subjective entitlements are equal to the 50%. As a consequence, bargaining often leads to equal splits. In Appendix E we report the results of introducing this performance information manipulation in our design.

E. Performance Information

To investigate whether the absence of performance information may have an impact in our design, we run an additional treatment in which we do not provide information about who is the best or the worst in a pair (without any further design modifications). We now discuss how performance information affects bargaining outcomes and bargaining dynamics.

We observe a lower percentage of bargaining failures without performance information. Similarly to Result 2, the highest number of bargaining failures is in the EASY-LOW condition, 9% , while in the other three conditions it is below 5%. The percentage of bargaining failures in the EASY-LOW condition is weakly significant greater than zero (p-value $= 0.08$, 1-sided, t-test).

Figure 1.9: Surplus splits



Notes: The graph shows the distribution of surplus splits in the EASY and HARD treatments when there is no information about who is the best in a pair across LOW and HIGH surpluses. Each band has 0.05 width.

Figure 1.9 shows the distribution of surplus splits without performance information across the four conditions.[21] Figure 1.9 shows clearly that without performance information 69.5% of pairs settle on the equal split whereas with performance information only 37% of pairs settles on the equal split. Hence, when there is no performance information, fairness concerns are predominant with respect to self-confidence.

Without performance information 60% of subjective entitlements are the equal split whereas with performance information only 30% of subjective entitlements coincide with the equal split. In line with Karagözoğlu and Riedl (2014), disagreement in subjective entitlements is highly significant greater with performance information than without (1.11 and 1.06 on average, respectively; p-value < 0.01, 1-sided, Kruskal-Wallis test). Without performance information, there are neither significant differences in disagreement in opening proposals among EASY and HARD treatment nor among the four conditions. In all treatments and conditions disagreement in subjective entitlements is significantly greater than 1 (p-value < 0.05, 1-sided, t-test). We do not find significant differences in disagreement in opening proposals, concessions, and bargaining duration across the

[21] As mentioned in 1.4.2, since subjects were bargaining over values and not percentages, it is possible that many pairs did not precisely settle on the equal split but in a neighborhood of it. In figure 1.9 each band has 0.05 width so that the equal split bin includes the 5% neighborhood around the equal split.

Table 1.11: Subjects ranks in the four conditions

		LOW	HIGH	
	m	15.28	8.36	12.04
EASY	sd	(5.92)	(6.10)	(6.91)
	n	50	44	94
	m	17.55	8.70	12.38
HARD	sd	(5.12)	(5.66)	(6.97)
	n	40	56	96
	m	16.29	8.55	
	sd	(5.67)	(5.83)	
	n	90	100	190

two performance information treatments. Moreover, results for disagreement in opening proposals, concessions, and bargaining duration are similar but hold without significance.

F. Relative Performance: Individual Ranks and Pair Composition

Differences in rank distributions may generate biases both at the individual and at the pair level. At the individual level, for instance, the presence of more ties in one condition than in another may generate confounds in the observed levels of overplacement. At the pair level, differences in rank variations across pairs may lead to different bargaining outcomes; for instance, we may observe more disagreement when there is high variation in partners' ranks and more equal split when there is low variation in partners' ranks.

In what follows, we investigate whether there are significant differences in ranks among the EASY and the HARD treatment and among conditions to exclude biases at the individual level; next we introduce a variation measure to exclude biases in ranks composition at the pair level. Table 1.11 reports means and standard deviations of ranks as well as the number of observations in the two treatments and in the four conditions. There are no significant differences in mean ranks (2-sided, t-test), standard deviations of ranks (2-sided sd-test), and ranks distributions (Kruskal-Wallis test) across the EASY and HARD treatments. There are no significant differences in ranks standard deviations across LOW and HIGH (2-sided sd-test). Since the surplus size depends on partners' mean relative performance, there must be differences in ranks means and distributions by construction. Indeed, we find highly significant differences in mean ranks and ranks distributions across surplus size (p-value < 0.01, 2-sided, t-test; p-value < 0.01, 1-sided, Kruskal-Wallis test). Finally, there are no significant differences in ranks standard deviations across the four conditions (2-sided, sd-test). There are highly significant differences in means and distribution across all the four conditions (p-value < 0.01, 2-sided, t-test; p-value < 0.01, 1-sided, Kruskal-Wallis test) with the exception of the difference among EASY-LOW and HARD-LOW that is weakly significant different (p-value $= 0.06$, 2-sided, t-test; p-value $= 0.07$, 1-sided, Kruskal-Wallis test) and of EASY-HIGH and HARD-HIGH condition that do not differ significantly (p-value $= 0.77$, 2-sided, t-test; p-value $= 0.63$, 1-sided, Kruskal-Wallis test). However, since there are differences among LOW and HIGH by construction, what matters to exclude a ranking bias across conditions is the absence of differences in variation (i.e. in standard deviations) that has been verified.

To make sure that there is no pair composition bias across treatments and conditions, it is important to exclude differences in ranks variation across pairs. To measure ranks variation we define the variable "pair variance", V, as follows:

$$V = \frac{\sum (r_i - r_j)^2}{N}, \tag{1.1}$$

where r_i is the rank of partner i, r_j is the rank of partner j, N is the number of pairs ij, $(r_i - r_j)^2$ is the difference of partners' ranks squared. There are in total 95 pairs out of which 48 took part to the EASY and 47 in the HARD treatment. In the EASY treatment V is equal to 112.5 and in the HARD treatment V is equal to 88. Given that V takes only one value per treatment, we test whether there are significant differences in the differences of partners' ranks squared, $(r_i - r_j)^2$, among the EASY and the HARD treatment. There are no significant differences in means nor in distributions among EASY and HARD (p-value $= 0.2$, 2-sided t-test; $p = 0.089$ 1-sided Kruskal-Wallis test).

Out of 95 pairs, 45 pairs were assigned a LOW jointly produced surplus and 50 pairs were assigned a HIGH jointly produced surplus. V is equal to 98 for pairs that were assigned a LOW surplus and to 104 for pairs that were assigned a HIGH surplus. Again, given that V takes only one value per condition, we test whether there are significant differences in the difference of ranks squared ($(r_i - r_j)^2$) among LOW and HIGH. There are no significant differences in means nor in distributions among LOW and HIGH (2-sided t-test, 1-sided Kruskal-Wallis test).

To complete, Graph 1.10 shows the distribution of the difference of partners' ranks squared in the EASY and in the HARD treatment (top panel) and the distribution of difference of partners' ranks squared in the four conditions (bottom panel).

The distribution of the difference of ranks squared ($(r_i - r_j)^2$) does not differ significantly across the four conditions (however note that the difference in distributions among EASY-HIGH and HARD-LOW is weakly significant (p-value $= 0.09$, 1-sided, Kruskal-Wallis test).

Regarding ties in pairs, we only had a tie in two pairs overall the experiment. Regarding ties among all participants in EASY and HARD, note that the distributions of ranks do not differ significantly hence ties should not play a role.

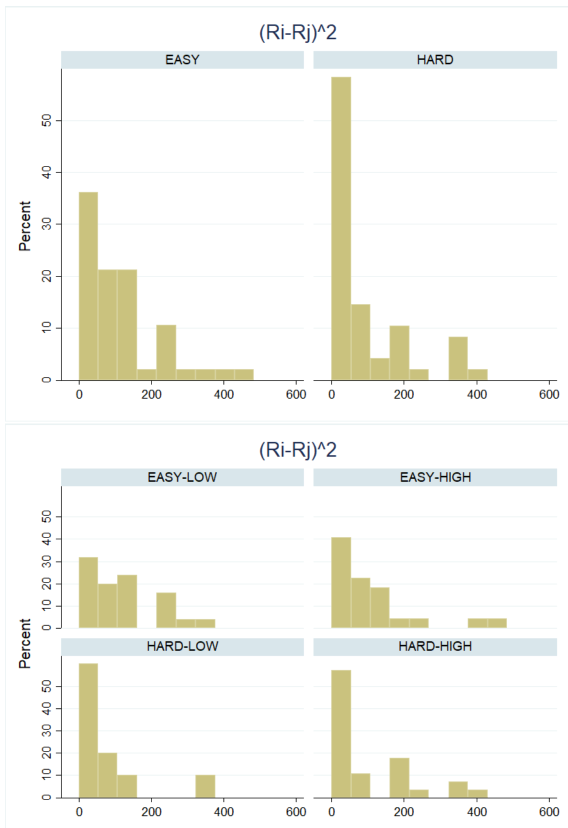
Finally, we want to exclude that bargaining failures are due to differences in sophistication among subjects. We investigate whether beliefs about own and partner ranks are consistent with the information about the surplus. A pair is assigned a high surplus if the average of partners' ranks is lower or equal to 12 (as shown in section 1.2.2). Beliefs about own rank and partner rank are consistent with the information about the surplus if the average of the estimations is lower or equal to than 12 and the surplus is high or if the average of the estimations is greater than 12 and the surplus is low. Out of 190 observations, 172 subjects hold beliefs that are consistent with the information about the surplus. In other words, the 90.5% of subjects make consistent estimations. Among 18 subjects who belong to a pair that failed to reach an agreement, only 3 subjects show inconsistency. Note that the 3 subjects belong to a pair who was assigned a low surplus. For 2 of the subjects the average of the estimations is equal to 12 (hence, the threshold) and for one subjects the average of estimations is equal to 11 (close to the threshold). Thus we can conclude that overall subjects make consistent estimations and bargaining failures do not stem from differences in sophistication among subjects.

G. Gender Differences

Out of 190 subjects, 100 males and 90 females participated to the experiment. Out of 95 pairs, 26 were composed of males, 21 of females, and in 48 pairs one partner was male and the other female. In what follows we analyze whether there are gender differences in bargaining outcomes, bargaining dynamics, overconfidence and in risk attitudes.

We find no significant differences in bargaining failures nor in surplus splits among males and females. We find no significant differences in disagreement and subjective entitlements, dis-

Figure 1.10: Difference of ranks squared



Notes: the top panel shows the distributions of the difference of partners' ranks squared in the EASY and HARD treatments; the bottom panel shows the distribution of the difference of partners' ranks squared in the EASY and HARD treatments and distinguishes among LOW and HIGH surplus.

agreement in opening proposals, and in concessions. Finally, there are no significant differences in bargaining duration among male and female. Thus we can conclude that there are no gender differences in either bargaining outcomes or dynamics.

The literature finds conflicting results on overconfidence and gender. Some paper find that men are more overconfident than women (Niederle and Vesterlund 2007, Pulford and Colman 1997, Soll and Klayman 2004) while other find that there are no gender differences in overconfidence (Moore and Healy 2008, Johansson Stenman and Nordblom 2010). We do not find significant differences in overconfidence when we refer to self-placement with respect to the partner (p-value $= 0.21$, 1-sided, Kruskal-Wallis test; overplacement with respect to a partner is equal to -0.97 for males and to 0.73 for females). However, when we refer to self-placement with respect to the group, we find that on average females are more overconfidence than males. Overplacement is on average equal to -0.47 for males and to 0.93 for females and the difference in distributions is weakly significant (p-value $= 0.062$, 1-sided, Kruskal-Wallis test).

A difference in performance among male and females may explain this result. Indeed, we find that males perform better than females both in the EASY and in the HARD quiz. In the EASY quiz males replied correctly to 32 questions on average while females replies correctly to 29 questions on average (the difference is weakly significant, p-value $= 0.054$, 1-sided, t-test). Both males and females are overconfident and females are slightly more overconfident (overplacement is equal to 2.3 for female and 1.8 for male and the difference is not significant, p-value $= 0.68$, 1-sided, t-test). In the HARD quiz males replied correctly to 10 questions on average while females replies correctly to 6.5 questions on average (the difference is highly significant, p-value <0.01, 1-sided, t-test). Both males and females are underconfident and males are more underconfident than females (overplacement is equal to -.46 for female and -2.6 for male, the difference is significant, p-value $= 0.024$, 1-sided, t-test). Hence if we distinguish across EASY and HARD, we do not find gender differences in overconfidence in the EASY treatment but we do find that males are more underconfident than females in the HARD treatment.

Even if Filippin and Crosetto (2016) show that the gender differences in risk aversion are task related, in many experimental studies women show more risk aversion than men (Eckel and Grossman 2008) and this is the case in our data. We find that the 77% of females and the 55% of males is risk averse and the difference is highly significant (p-value < 0.01, 1-sided, Kruskal-Wallis test). For the definition of risk aversion we follow Crosetto and Filippin (2013). Moreover, we introduce a continuous measure of risk preferences, “risk attitude” (described in details in Appendix G). The average risk attitude is equal to -13.2 for females and to -7.9 for males; the difference in risk attitude is highly significant (p-value < 0.01, 1-sided, Kruskal-Wallis test).

H. Risk Attitudes and the BRET

As mentioned in Section 1.2.7 we measure risk attitudes with the Bomb Risk Elicitation Task (BRET, Crosetto and Filippin 2013). The task is as follows. We display a 10x10 matrix (i.e. a square composed of 100 boxes) on the computer screen. Subjects can earn points for each box they decide to collect.The box collection process is automatic: for each second elapsed, a box is collected (during the experiment, boxes pass from dark gray to light gray). Subjects have the possibility to interrupt the collection process at any time pressing a STOP button. Behind one of the 100 boxes hides a “bomb” that destroys everything that has been collected. The bomb can be hidden in any box with the same probability (equal to 1/100). However, subjects do not know which box hides the bomb.

If a subject collects the box hiding the bomb her payoff for the task is equal to zero. If not, her payoff for the task is positive and proportional to the number of boxes collected. Participants learn where the bomb is (and hence if they collected it) only at the end of the task. To make sure that everyone has understood this task, we run a practice round that does not pay the points that participants may have accumulated.

The BRET is so that with 100 boxes subjects choosing to collect 50 boxes are risk neutral, subjects collecting less than 50 boxes are risk averse, and those collecting more than 50 boxes are risk seekers. The minimum possible number of boxes that one can collect is 0, the maximum is 100, and the probability of collecting the bomb increases with the number of boxes collected.

We find that 64% of subjects are risk averse in the EASY-LOW, 59% in the EASY-HIGH, 77% in the HARD-LOW, and 62.5% in the HARD-HIGH condition. We also find that 16% of subjects are risk seekers in the EASY-LOW, 20% in the EASY-HIGH, 12.5% in the HARD-LOW, and 16% in the HARD-HIGH condition. The remaining subjects in each condition are risk neutral. There are no significant differences in the proportions of risk adverse, risk seeker, and risk neutral subjects across the four conditions.

In addition to referring to discrete risk attitude measures suggested in Crosetto and Filippin (2013), we introduce a continuous measure, “risk attitude”, of risk attitudes. We define this variable as the difference among the number of boxes collected minus 50 (the variable is included among -49, absolute risk aversion, and $+49$, absolute risk seeking behavior). The average risk attitude is equal to -12 in the EASY-LOW condition, to -7 in the EASY-HIGH condition, -15 in the HARD-LOW condition and to -9 in the HARD-HIGH condition. There are weakly significant differences in means and distributions between EASY-HIGH and HARD-LOW (p-value $= 0.0516$, 2-sided, t-test; $p = 0.058$, 1-sided, Kruskal-Wallis test) and significant differences between HARD-LOW and HARD-HIGH (p-value < 0.05, 2-sided, t-test; p-value < 0.05, 1-sided, Kruskal-Wallis test). Further investigation regarding risk preferences is carried on with the randomization check in Appendix I.

I. Randomization Check

To make sure that the randomization over the EASY and HARD treatment was successful, we run a multinomial logit regression (table 1.12). The multinomial regression includes the variable "Risk averse " defined in G and the demographic controls. Demographic controls include: Gender (dummy equal to 1 if male, 0 if female), Swiss Nationality (dummy equal to 1 if Swiss, 0 otherwise), Age, Unil (dummy equal to 1 if the student is affiliated to UNIL, 0 otherwise), Grades, Bachelor (dummy equal to 1 if bachelor student, 0 otherwise), Grad. Parents (dummy equal to 1 if both parents have a degree, 0 otherwise), Only Child (dummy equal to 1 if only child, 0 otherwise), Big Town (dummy equal to 1 if resident in a big town, 0 otherwise), People Known (number of people known during the lab section). The EASY treatment is the base outcome in the model.

As table 1.12 shows, there are no significant differences EASY and HARD treatment and thus we can conclude that our randomization worked.

J. Experimental Instructions

In this session we report the English translation of experimental instructions that were distributed in French.

General Explanations of the Experiment

You are about to participate in an economic experiment. The experiment is conducted by the Departement d'Econometrie et Economie Politique (DEEP) of the University of Lausanne and funded

Table 1.12: Multinomial logit

	HARD
Risk Averse	0.245
	(0.320)
Gender	0.133
	(0.319)
Swiss Nat.	0.0162
	(0.343)
Unil	0.104
	(0.339)
Age	-0.116
	(0.0754)
Grades	0.264
	(0.267)
Bachelor	0.646
	(0.442)
Grad. Parents	0.254
	(0.335)
Only Child	0.144
	(0.494)
Big Town	0.109
	(0.387)
People Known	-0.285
	(0.192)
Constant	0.413
	(2.212)
r2	
chi2	14.11
p	0.227

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

by the Swiss National Science Foundation (SNSF). It aims at better understanding bargaining behavior.

For your participation in the experiment you will earn a lump sum payment of 10 CHF for sure. You can earn more during the experiment. The experiment consists of six parts. In some parts of the experiment you can earn points that depend on your decisions. During the experiment, we will consider points instead of CHF.

Thus it is to your own benefit to read these explanations carefully.

The total number of points you have earned during the experiment will be exchanged into CHF at the end of the experiment. The exchange rate is

70 points = 1 CHF

In other words, each point corresponds to approximately 1.43 cents.
You can take your decisions at your own speed.

It is prohibited to communicate with the other participants during the whole course of the experiment. It is also prohibited to use your mobile phone during the whole course of the experiment. If you do not abide by these rules you will be excluded from the experiment and all payments. However, if you have questions you can always ask one of the experimenters by raising your hand.

Your anonymity is guaranteed

At the end of the experiment, one of the experimenters will give you a payment sheet reporting the amount you will receive. You have to take it with you and bring it to the experimenter outside of the LABEX.

The experimenter outside of the LABEX is not informed about the decisions you have taken during the experiment. The experimenter will pay you in accordance to your payment sheet. After that, you will sign a payment receipt. Given that the receipt does not include your participant number, no experimenter will be able to determine your identity.

The backside of these explanations gives you an overview of the experiment. If you have any questions right now, please raise your hand. Otherwise, you can now proceed with the explanations on the first part of the experiment.

The backside of these explanations gives you an overview of the experiment. If you have any questions right now, please raise your hand. Otherwise, you can now proceed with the explanations on the first part of the experiment.

Overview of the Experiment

- **Part 1:**
  Answering General Knowledge Quiz
- **Part 2:**
  Receiving Information about the Joint Surplus
- **Part 3:**
  Making Two Estimations
- **Part 4:**
  Bargaining over the Joint Surplus
- **Part 5:**
  Making a Choice under Risk
- **Part 6:**
  Questionnaire and Payment

If you have questions please raise your hand.

Part 1: Answering a General Knowledge Quiz

In this part of the experiment you will have 20 minutes to answer a general knowledge quiz. There is a timer to the top right of the screen that indicates the remaining time (in seconds). Note that 20 minutes are equal to 1200 seconds.

The quiz is composed of 46 questions. **The greater the number of questions you answer correctly, the more likely is that you will earn more in the bargaining part of the experiment.** Hence, it is in your best interest to provide the correct answers to as many questions as you can.

Note that the correct answers to some questions involve providing the first and last names of famous people. You can only get full credit for your answers to these questions if you provide us

both with the first and the last name of that famous person. If you only provide us with the first name or the last name we will only give you half credit for your answer.
Unanswered questions count as wrong answers.

Part 2: Receiving Information about the Joint Surplus

During the rest of the experiment you will be randomly paired with another participant in this session but you will not know who that participant is. Neither during nor after the experiment will anybody be informed about who has been paired with whom.

In the experiment, you and the person you are paired with (your partner) will bargain over a joint surplus. Depending on your performance and the performance of your partner on the general knowledge quiz, the size of the joint surplus over which you will bargain in the fourth part of the experiment will be either small (**1390 points**) or large (**2710 points**).

We will now explain how the size of the joint surplus is determined. All 24 participants, including you and your partner, have completed the general knowledge quiz. According to the performance of all participants, each of them is attributed a rank. Rank 1 corresponds to the participant whose performance was the best (or, the participant who answered correctly the highest number of questions), rank 2 to the participant whose performance was the second best, and so on.
The size of the joint surplus is determined by your performance and your partner's performance on the general knowledge quiz. More precisely, the size of the joint surplus depends on the sum of your rank and your partner's rank in the general knowledge quiz, as follows:

1. If the sum of your rank and your partner's rank is from **2 to 24**, then the joint surplus will be **2710 points**.
2. If the sum of your rank and your partner's rank is from **25 to 47**, then the joint surplus will be **1390 points**.

The four examples that follow illustrate how the size of the joint surplus is determined.

Example 1: Suppose that your rank is 1 and that your partner's rank is 1 as well (you both performed the best). In this case, the sum of the ranks is 2. Since 2 is greater than 2 and less than 24, then the joint surplus would be 2710 points.

Example 2: Suppose that your rank is 5 and that your partner's rank is 10. In this case, the sum of the ranks is 15. Since 15 is greater than 2 and less than 24, then the joint surplus would be 2710 points.

Example 3: Suppose that your rank is 12 and that your partner's rank is 23. In this case, the sum of the ranks is 35. Since 35 is greater than 25 and less than 47, then the joint surplus would be1390 points.

Example 4: Suppose that your rank is 23 and that your partner's rank is 23 as well (you both performed the worst). In this case, the sum of the ranks is 46. Since 46 is greater than 25 and less than 47, then the joint surplus would be 1390 points.

Part 3: Making Two Estimations

In this part of the experiment you will be asked to provide us with two estimates. The first one is the estimate of your rank in the quiz. The second one is the estimate of your partner's rank in the quiz. We will now explain in detail how you should indicate your **estimate of your rank in the quiz** and how this estimate influences your earnings.

a) How to indicate your estimate of your rank?
   All 24 participants, including you and your partner, have completed the quiz. According to the performance of all participants, each of them is attributed a rank. Rank 1 corresponds to the participant whose performance was the best (or, the participant who answered correctly the highest number of questions), rank 2 to the participant whose performance was the second best, and so on.
   We want you to tell us your estimate of your rank as an integer between 1 and 24.

b) How does your estimate of your rank influence your earnings?
   The more precise your estimate of your rank is, the higher is the probability that you will earn 140 points. In other words, the likelihood of earning the 140 points is higher, the closer your estimate of your rank is to your true rank in the quiz.
   Your earnings are obtained as follows:

- First, the computer randomly draws a number between 0 and 529. Every number between 0 and 529 is equally likely.
- Second, the difference between your estimate of your rank and your true rank is the prediction error. If the prediction error, multiplied by itself, is not larger than the random number drawn by the computer, then you will earn **140 points**. Otherwise, you will earn **0 points**.

Important: You may wonder why we have chosen this payment rule. The reason is that this payment rule makes it optimal - for you - to state precisely your estimate of your rank.

Example: Your estimate of your rank is 13, however given your performance in the quiz, your true rank is 10. Thus in this case the prediction error is (13-10) = 3. The prediction error multiplied by itself is 9. If the random number drawn by the computer is greater than or equal to 9, for example 26, then you will earn 140 points. If the random number drawn by the computer is smaller than 9, for example 8, then you will earn 0 points.

We will now explain in detail how you should indicate your **estimate of your partner's rank in the quiz** and how this estimate influences your earnings.

a) How to indicate your estimate of your partner's rank?
All 24 participants, including you and your partner, have completed the quiz. According to the performance of all participants, each of them is attributed a rank. Rank 1 corresponds to the participant whose performance was the best (or, the participant who answered correctly the highest number of questions), rank 2 to the participant whose performance was the second best, and so on.
We want you to tell us your estimate of your partner's rank as an integer between 1 and 24.

b) How does your estimate of your partner's rank influence your earnings?
The more precise your estimate of your partner's rank is, the higher is the probability that you will earn 140 points. In other words, the likelihood of earning the 140 points is higher, the closer your estimate of your partner's rank is to your partner's true rank in the quiz.
Your earnings are obtained as follows:

- First, the computer randomly draws a number between 0 and 529. Every number between 0 and 529 is equally likely.
- Second, the difference between your estimate of your partner's rank and your partner's true rank is the prediction error. If the prediction error, multiplied by itself, is not larger than the random number drawn by the computer, then you will earn **140 points**. Otherwise, you will earn **0 points**.

Important: You may wonder why we have chosen this payment rule. The reason is that this payment rule makes it optimal - for you - to state precisely your estimate of your partner's rank.

Example: Your estimate of your partner's rank is 3, however, given his/her performance in the quiz your partner's true rank is 14. Thus, in this case, the prediction error is (3-14) = -11. The prediction error multiplied by itself is 121. If the random number drawn by the computer is greater than or equal to 121, for example 200, then you will earn 140 points. If the random number drawn by the computer is smaller than 121, for example 75, then you will earn 0 points.

Before providing us with your estimations, we will ask you to answer a few comprehension question. Your answers to these questions will have no consequences on the experiment nor on your final payment. The experiment will continue as soon as all the participants will have answered the questions correctly.

Part 4: Bargaining over the Joint Surplus

Information about relative performance in the quiz.

Remark: the following part is used only in the INFO treatments.

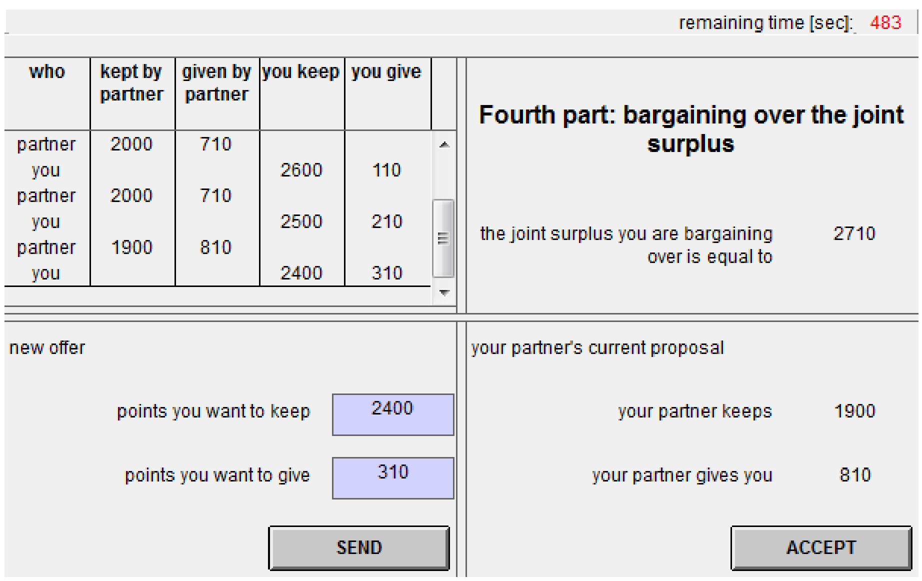
Next, you will receive information on your screen about who was the best performer in the general knowledge quiz among you and your partner.
If you have answered correctly more questions in the general knowledge quiz than your partner, then you are the best in the pair and your partner is the worst in the pair.
If you have answered correctly less questions in the general knowledge quiz than your partner, then you are the worst in the pair and your partner is the best in the pair.

If you and your partner have the same number of correct answers in the general knowledge quiz, then you and your partner are equally performing.

Bargaining

You will have a maximum of 10 minutes to reach an agreement on the distribution of the joint surplus. You do not have to use up all the bargaining time but must not exceed it. **If you do not agree on a distribution of the joint surplus within 10 minutes, then you will earn nothing from this bargaining stage! If you do agree on a distribution of the joint surplus then you will earn the points you and your partner agreed on.**

The bargaining takes place via computer. During bargaining you will work with a screen that consists of four parts, which we will explain in what follows. Hereby a screen-shot.



In the **upper-right part** of the screen the joint surplus you are bargaining over is displayed. The timer right on the top shows how much bargaining time (in seconds) is still remaining. Note that 10 minutes are equal to 600 seconds.

In the **lower-left part** of the screen you can enter a new proposal. You will need to enter the points you want to keep for yourself and the points you want to give to your partner. There is a SEND button to confirm and send proposals.

In the **upper-left part** of the screen a table shows all previous proposals and the identity of proposers (you or your partner). Each proposal is listed in the table in chronological order. Every time you make a proposal, the table will show that you have made the proposal and display the proposal you have made. Every time your partner makes a proposal, the table will show that he/she has made the proposal and display the proposal he/she has made.

In the **lower-right part** of the screen you see your partner's currently valid proposal. The ACCEPT button allows you to accept your partner's currently valid proposal. If your partner has not made any proposal to you yet, this part of the screen is empty. Similarly, if you have not made any proposal, this part of the screen in your partner's computer is empty. To make a first (or new) proposal you have to fill in two boxes in the lower-left part of the screen with corresponding points of the joint surplus for yourself and for your partner. The points you fill in have to add up to the joint surplus. Thereafter, you need to press the SEND button to send your proposal. The following rules apply:

1. The sum of points for yourself and for your partner cannot be exceed the joint surplus nor be lower than the joint surplus.
2. Only offers with integer points are allowed.
3. A sent offer is binding, that is, if your partner accepts your proposal, bargaining is finished and both of you earn the points on which you have agreed upon. The same holds if you accept a proposal of your partner. You can only accept the current proposal; earlier proposals are not valid any more.

Hence, as long as you have not pressed the SEND button you can still change the offer. A sent proposal is binding. You can always make a new proposal, provided that neither you nor your partner have accepted a proposal and provided that there is still time left.
If you want to accept a currently valid proposal, you have to press the ACCEPT button. Once you or your partner accept a proposal, bargaining is over and each of you will receive the agreed share of the joint surplus.

Part 5: Making a Choice under Risk

On your computer screen you will see a square composed of 100 boxes.
You earn 1.4 points for every box that you decide to collect. The box collecting process is automatic: for every second that elapses, a box changes color. The boxes start disappearing from the top-left corner of the screen and number of boxes collected is updated accordingly.

Behind one of these boxes hides a "bomb" that destroys everything that has been collected.

The bomb can be in any box with equal probability (the probability the bomb is in one particular box is equal to 1/100). However, **you do not know behind which box the bomb is.**

Your task for this stage is to choose when to stop the box collecting process. You can do it by hitting the STOP button at any time.

If you collect the box that contains the bomb, the bomb will explode and you will earn zero points. If you stop the box collecting process before collecting the box that contains the bomb, the bomb will not explode and you will earn the points accumulated that far.

Note that you will only know if one of the boxes you collected contains the bomb at the end of the task; indeed if you collect the box that contains the bomb, the bomb only explodes at the end of the task: this means that you can collect the box that contains the bomb without knowing it.

We will start this stage with a practice round. The goal of the practice round is to show you how this task works. After the practice round is over, the task starts. The practice round is just an example: you will not earn the points accumulated in this part.

Part 6: Questionnaire and Payment

At the end of the experience we will ask you to answer a questionnaire. Next, we will proceed with your payment.

Your final payment includes your gains for each part of the experiment in CHF and the lump sum payment[22]. The amount you will be paid will be shown to you at the end of the experiment.

[22] Show up fee of 10 CHF

Note that 70 points in the experiment correspond to 1 CHF.

If you have questions please raise your hand.

Chapter 2

Overconfidence and Effort Provision in an Asymmetric Tournament

2.1 Introduction

Firms and corporations make a wide use of tournaments as incentive schemes. Common examples are hierarchical promotions (Baker et al. 1994; Lazear and Rosen 1981), relative compensation of managers (Gibbons and Murphy 1990) or sales personnel rewards (Murphy et al. 2004). In a rank-order tournament, agents are rewarded based on relative and not on absolute performance: the best performer receives a higher, fixed, compensation compared to the worst performer. This is an advantage to incentivize effort provisions in situations in which effort is not contractible (Malcomson 1986) or to classify individuals when skills are not observable (Harbring and Lünser 2008).

While relative performance is fundamental to determine workers rewards in tournaments, behavioral biases such as overconfidence can affect the self-assessment of individual performance compared to others. The literature shows that most people believe to be better than average (Myers 1996): drivers (Svenson 1910), poker and chess players (Parker and Santos-Pinto 2010), currency traders (Oberlechner and Osler 2008), as well as fund managers (Brozynski et al. 2006) overestimate their performance. Moreover, people tend to overestimate their ranking when performing a skill test (Camerer and Lovallo 1999, Moore and Healy 2008).

This paper investigates theoretically the impact of overconfidence on effort provision in an asymmetric tournament with heterogeneous agents. The impact of overconfidence on effort provision in a symmetric tournament with homogeneous agents has been studied in Santos-Pinto (2010). In the latter, two workers compete in a rank-order tournament in which a firm sets a winner/loser prize to be rewarded to the best/worst performer. While the firm assesses workers' productivity correctly, workers assess their opponents' productivity correctly but are overconfident about their own. They overestimate their effort productivity and thus their probability of winning the tournament. Santos-Pinto (2010) shows that, under a set of circumstances, two overconfident workers who compete in a rank-order tournament exert more effort than the effort they would have exerted if they had a correct self-assessment of their own performance.[1] Yet, in the real world, agents are rarely homogeneous. To have a closer look at reality, I build on and extend the theoretical model in Santos-Pinto (2010) to include heterogeneity in workers' self-assessment. Similarly to Santos-Pinto (2010), a firm sets a winner/loser prize to be rewarded to the best/worst performer and correctly assesses workers' productivity. However, differently from Santos-Pinto (2010), while workers assess their opponents' productivity correctly, there is heterogeneity in workers' self-assessment biases: some workers are rational and correctly assess their own productivity while others are overconfident and overestimate their productivity and thus their probability of winning the tournament. For simplicity, the analysis is restricted to an asymmetric tournament with two workers, an overconfident and an unbiased worker. To be able to obtain tractable results, the model is specialized for quadratic costs of effort and uniform distribution of individual output. Finally, in the model, the relation among overconfidence and effort provision is non-monotonic. This feature allows for an inclusive model that studies whether overconfidence increases or decreases the effort provision of heterogeneous agents.

The main finding of the paper is that the overconfidence of worker 1 impacts the level of effort exerted by both workers shifting the Nash Equilibrium away from the symmetric equilibrium with two unbiased workers. Interestingly, different degrees of overconfidence lead to different outcomes. When overconfidence is small, the level of effort exerted by the overconfident worker is higher than the level of effort exerted by the unbiased worker. Indeed, while the level of effort exerted by the overconfident worker is higher than the level of effort that would have been exerted in a symmetric

[1] Overconfidence raises effort provision when workers are risk neutral and when workers are risk averse and overconfidence and effort are complements. When workers are risk averse and overconfidence and effort are substitutes or the relationship among overconfidence and effort is non monotonic, overconfidence may or may not increase effort provision.

tournament with two unbiased workers, the level of effort exerted by the unbiased worker is lower than the level of effort that would have been exerted in a symmetric tournament with two unbiased workers. For a given prize spread, the firm is better off when overconfidence is small since effort provision and thus expected profit increases on average. When overconfidence is large, the levels of effort exerted by an overconfident and an unbiased worker in an asymmetric tournament is lower than the level of effort that would have been exerted in a symmetric tournament with two unbiased workers. For a given prize spread, the firm is worse off when overconfidence is large, since effort provision and thus expected profit decreases.

This paper contributes to the literature on tournaments. Rank-order tournaments can be divided into symmetric tournaments in which homogeneous agents compete (Nalebuff and Stiglitz 1983, Santos-Pinto 2010) and asymmetric tournaments in which heterogeneous agents compete (Shotter and Weigelt 1992, Gürtler and Kräkel 2010, Harbrig and Lünser 2008). Heterogeneity is more suitable to capture the complexity of the environment that firms and workers inhabit. In the asymmetric tournament literature, heterogeneity of agents has been studied as a difference in workers' ability or effort costs. In this context Lazear and Rosen (1981) show theoretically that heterogeneity in effort costs leads to inefficient outcomes. Gürtler and Kräkel (2010) show that inefficiencies rise if firms set uniform prizes (i.e., prizes that are independent from workers' identity) while efficient effort levels are induced if firms set individual prizes. Shotter and Weigelt (1992) study the impact of equal opportunity laws and affirmative actions on effort provision. They find that policies that increase the probability of winning for disadvantaged (high cost) players reduce the effort they exert when heterogeneity is small but increase the effort exerted by both advantaged and disadvantaged players when heterogeneity is large. Finally, Harbring and Lünser (2008) show that an increase in the price spread increase effort provision in symmetric[2] as well as asymmetric tournaments. Moreover, for large prize spread, weaker players in an asymmetric tournament exert a higher effort than players in a symmetric tournament. I extend the symmetric settings of Santos Pinto (2010) to allow for heterogeneity in agents' self-assessment. Differently from the previous papers, in this paper workers have identical costs of effort, but different perceptions of their own productivity. The overconfident worker overestimates her productivity and thus her probability of winning the tournament while the rational worker has an unbiased self-assessment. Importantly, in

[2]It is a well know theoretical result that an increase in the price spread increases effort provision in tournaments (Nalebuff and Stiglitz 1983).

contrast with Shotter and Weigelt (1992), the difference in the probability of winning the tournament does not stem from the introduction of policies, but it is a behavioral bias of the overconfident worker. I find that, for a given prize spread, low degrees of overconfidence increase, on average, the effort exerted by both overconfident and unbiased workers leading to an increase in expected profits while high degrees of overconfidence decrease, on average, the effort exerted by both overconfident and unbiased workers leading to inefficiencies.

This paper also contributes to the literature on economic contests that include all situations in which agents' payoff depends on relative performance such as in the rank-order tournament modeled here. Tournaments are a particular subset of contests in which two or more players compete in one or more rounds until a winner is left (however, most contests can be considered as a tournament). Traditionally, authors have investigated theoretically the optimal design of contests to allow the principal to give agents appropriate incentives (Lazear and Rosen 1981, Nalebuff and Stiglitz 1983, O'Keeffe et al. 1984). More recently, Singh and Wittman (2001) have shown that when agents differ in marginal productivity of effort, output increases in ability, and effort provision decreases in effort costs. Krähmer (2007), Ludwig et al. (2011) and Ando (2004) introduce behavioral biases in economic contests. Krähmer (2007) shows that when ability and effort are complements, favorable beliefs about one's ability increase effort provision. Ludwig et al. (2011) interpret overconfidence as the underestimation of individual effort costs and demonstrate that effort provision is higher when some agents are overconfident with respect to the case with unbiased agents. Finally, Ando (2004) looks at overconfidence from two different points of view: the overestimation of one's type and the underestimation of a rival's type. Both cases induce an overestimation of the perceived probability of winning. However, when agents overestimate their own type, effort provision increases. When agents underestimate their rival's type, the impact of overconfidence is ambiguous. In this paper overconfidence is modeled as the overestimation of one's probability of winning, instead of the underestimation of one's effort costs (as in Ludwig et al. 2011). While it seems natural that the underestimation of effort costs enhances effort provision (Singh and Wittman (2001) show that effort and effort costs move in opposite directions in the absence of behavioral biases) the impact of the overestimation of the probability of winning may be ambiguous as shown in Ando (2004). When workers overestimate their probability of winning they may either increase effort (higher probability of winning increases the expected utility of winning) or decrease effort (workers believe that a lower level of effort is enough to win). Differently from Ando (2004), I find that overconfidence, as the overestimation of the probability of winning, increases or decreases effort provision depending on the magnitude of the bias. When overconfidence is small, overconfidence increases effort provision. When overconfidence is large, it decreases effort provision.

The main implication of this paper is that a small overconfidence bias can lead to better outcomes compared to large overconfidence. In the presence of heterogeneity in workers' self-assessment bias, firms should refrain from hiring extremely overconfident workers since their bias negatively affects the effort provision of all workers. Given that a lower effort provision implies a lower expected profit, the firm is on average worse off when overconfidence is large.

The paper is organized as follows: Section 2 introduces the generalized model; Section 3 presents a specialization the model for uniform distribution and quadratic effort costs; Section 4 discusses the implications of overconfidence for the firm and Section 5 concludes.

2.2 Generalized Model

I build on and modify Santos-Pinto's (2010) rank-order tournament model with overconfidence to introduce heterogeneity in the self-assessment bias of agents. For convenience, I restrict the analysis to two workers, worker 1 and worker 2, competing in the tournament. The winner receives a wage y_w while the loser receives a wage y_l, with $y_w > y_l > 0$. Winning or losing the tournament, and thus individual wages, depends on the relative ranks of workers and not on the absolute quantities they produce.

The two workers have the same preferences, the same cost of effort, the same outside option and the same productivity. However, they differ from one another in terms of the perception of their own productivity. Worker 1 is overconfident that is, she overestimates her productivity of effort while worker 2 is unbiased that is, she has an accurate assessment of her own productivity of effort. Since worker 1 overestimates her own productivity of effort, she also overestimates her probability of winning the tournament. Worker 2, on the contrary, can correctly estimate the probability of winning the tournament. Both workers observe each others biases or accuracy but worker 1 is not aware of being overconfident while worker 2 is aware of being accurate. Finally, both workers correctly assess their cost of effort and their outside options.

The firm correctly assesses workers' productivity and self-beliefs. The firm is a risk neutral monopolist that sets the tournament prizes in order to maximize profits, subject to the incentive and participation constraints of workers.

The timing of the model is the following. After the firm sets the optimal prizes, workers observe the realization of a common shock; after observing the prizes and the common shock, workers simultaneously choose the optimal effort level. The firm then observes worker rankings and finally workers are awarded prizes depending on their rank.

In what follows, I present the workers' problem. Both workers are weakly risk averse and are expected utility maximisers: they have identical von Neuman-Morgenstern utility functions that are separable in income (y_i) and effort (a_i).

$$U_i(y_i, a_i) = U(y_i) - C(a_i)$$

for i=1,2 and where u and c are twice differentiable with $U' > 0$, $U'' \leq 0$, $C_i' > 0$, $C_i'' > 0$, $C(0) = 0$ and $C'(0) = 0$. y_i is equal to y_w if the worker wins the tournament or y_l, $y_w > y_l > 0$, if the worker loses the tournament. Intuitively, the individual level of effort exerted cannot be negative i.e., $a_i \geq 0$.

Individual output (q_i) is a stochastic function of a worker's effort: each level of effort induces a distribution over output

$$G_i(q_i|e_i(a_i, \omega))$$

for i=1,2. $e_i(a_i, \omega)$ defines individual productivity as a function of individual effort a_i and a common shock ω. Individual productivity strictly increases in effort i.e., $e_i' > 0$, and marginal productivity is subject to diminishing returns to effort.

As mentioned, worker 1 is overconfident and overestimates her productivity while worker 2 has an accurate self-assessment of her productivity. Worker 1's perceived productivity of effort is

$$e_1 = e_1(a_1, \omega, \lambda)$$

where $\lambda > 1$ is a parameter that captures overconfidence.[3] Given the perceived productivity of effort, the perceived distribution over output is

$$G_1(q_1|e_1(a_1, \omega, \lambda))$$

[3]For $\lambda = 1$, worker 1 is unbiased and identical to worker 2.

In the case of worker 1, who is overconfident, $G_1(q_1|e_1(a_1, \omega, \lambda))$ first order stochastically dominates $G_1(q_1|e_1(a_1, \omega))$ for all levels of effort a_1: for each level of effort exerted, worker 1 believes that she is more likely to produce a higher level of output than he actually does.

Worker 2 has an accurate perception of his own productivity:

$$e_2 = e_2(a_2, \omega)$$

and thus the perceived and actual distribution over output coincide:

$$G_2 = G_2(q_2|e_2(a_2, \omega))$$

Given that perceived and actual distribution of over output of worker 2 coincide, it must be the case that $G_1(q_1|e_1(a_1, \omega, \lambda))$ first order stochastically dominates $G_2(q_2|e_2(a_2, \omega))$ for all levels of effort a_i: for each level of effort exerted, worker 1 believes that she is more likely to produce a higher level of output than worker 2.

In what follows I specify workers' stochastic production functions. I will now suppose that the common shock is additive. Moreover, there is a shock ε_i that can affect the individual productivity of workers:

$$Q_i = e_i(a_i) + \omega + \varepsilon_i$$

where G_i is the continuous distribution function of ε_i, $\varepsilon_i \sim g$, $E(\varepsilon_i) = 0$ and $g'() > 0$.

For worker 1 the stochastic production function is equal to

$$Q_1 = e_1(a_1) + \omega + \varepsilon_1$$

while the perceived stochastic production function is equal to

$$Q_1 = e_1(a_1, \lambda) + \omega + \varepsilon_1$$

For worker 2 the perceived and actual stochastic production function coincide:

$$Q_2 = e_2(a_2) + \omega + \varepsilon_2$$

The perceived probability of winning the tournament for worker 1 is

$$P_1(Q_1 \geq q_2) = 1 - P_1(Q_1 \leq q_2) = 1 - G_1(q_2|e_1)$$

the unconditional probability of winning the tournament for worker 1 is

$$P_1(a_1, a_2, \lambda) = \int [1 - G_1(q_2|e_1(a_1, \lambda))] g_2(q_2|e_2(a_2)) dq_2$$

For worker 2, the probability of winning the tournament is

$$P_2(Q_2 \geq q_1) = 1 - P_2(Q_2 \leq q_1) = 1 - G_2(q_1|e_2)$$

and the unconditional probability of winning the tournament for worker 2 is

$$P_2(a_1, a_2) = \int [1 - G_2(q_1|e_2(a_2))] g_1(q_1|e_1(a_1)) dq_1$$

Given the workers' perceived probabilities of winning the tournament, the perceived utilities for worker 1 and 2 are

$$V_1(a_1, a_2, \lambda, y_l, y_w) = u(y_l) + P_1(a_1, a_2, \lambda)\Delta u - C(a_1)$$

and

$$V_2(a_1, a_2, y_l, y_w) = u(y_l) + P_2(a_1, a_2)\Delta u - C(a_2)$$

where $\Delta u = u(y_w) - u(y_l)$.

Workers choose the optimal level of effort maximizing their perceived utilities. The first order conditions are

$$\frac{\partial P_1(a_1, a_2, \lambda)}{\partial a_1} \Delta u = C'(a_1) \tag{2.1}$$

and

$$\frac{\partial P_2(a_1, a_2)}{\partial a_2} \Delta u = C'(a_2) \tag{2.2}$$

From (2.1) and (2.2) I get

$$\frac{C'(a_1)}{C'(a_2)} = \frac{\frac{\partial P_1(a_1, a_2, \lambda)}{\partial a_1}}{\frac{\partial P_2(a_1, a_2)}{\partial a_2}} \tag{2.3}$$

Expression 2.3 indicates how the optimal effort choice of worker 1, the optimal effort choice of worker 2 and the self-image bias of worker 1 are related.

In Appendix A I derive a simplified version of the model for any distribution for a better understanding of the impact of overconfidence on effort provision. Appendix A shows that the impact of overconfidence on effort provision depends on the characteristics of the distribution of output. When the distribution of output is monotonic and non-decreasing, the overconfident worker exerts more effort than the unbiased worker; when the distribution of output is non-monotonic, the impact of overconfidence on effort provision is ambiguous.

2.3 Uniform Distribution and Quadratic Effort Costs

In what follows, I will specialize the model for the case in which individual output follows a uniform distribution and effort costs are quadratic. This specification allows to derive closed form solutions and tractable results. Moreover, the hypothesis that individual output follows a uniform distribution implies that the joint distribution[4] is non-monotonic, and allows to study the case in which the overconfident worker exerts more, and the case in which the overconfident worker exerts less, effort than the unbiased worker, making the model inclusive and suitable to describe reality.[5] Thus, the results found with uniform distribution and quadratic effort cost could be extended to more general cases.

I assume that the (actual) productivity function of workers is linear in effort

$$e_i = (e_i(a_i)) = a_i$$

Knowing that worker 2 has an unbiased self-assessment, her actual and perceived productivity will be identical and equal to

$$e_2 = e_2(a_2) = a_2$$

Worker 1 overestimates her productivity by λ and her perceived productivity is

$$e_1 = (e_1(a_1, \lambda)) = \lambda * a_1$$

with $\lambda > 1$. I assume that overconfidence enters the definition of perceived productivity in a multiplicative way as in Santos-Pinto (2010). Note that overconfidence could also enter productivity in an additive way ($e_1 = \lambda + a_1$). However in this case overconfidence may be identical to optimism. To avoid any confounds, I chose the multiplicative assumption.

[4]Given that individual shocks follow a uniform distribution, individual output follows a uniform distribution. However, what is relevant for the solution of the model is the distribution of the difference of workers' individual shocks $\varepsilon_i - \varepsilon_j$. Indeed, the difference of individual shocks enters the definition of perceived probabilities and expected utility of workers, as shown below. A similar specification is used in Shotter and Weigelt (1992).

[5]A common example of non-monotonic distribution is the normal distribution. However, when individual output distributes as a normal, it is extremely hard to derive tractable solutions. To show that when the distribution of output is non-monotonic, and in particular when individual output distributes as a normal, overconfidence can have an ambiguous impact on effort provision, Appendix A.1 investigates briefly the impact of overconfidence on effort provision when individual output follows a normal distribution and costs of effort are quadratic.

Each worker chooses the optimal effort level in order to maximize her (perceived) expected utility. To simplify the derivation, I will now assume that workers are risk neutral. Worker 1 maximizes

$$\begin{aligned}
V_1(a_1, a_2, \lambda, y_l, y_w) &= y_l + \Pr(Q_1 > q_2)\Delta y - c(a_1) \\
&= y_l + \Pr(a_1\lambda - a_2 - Y > 0)\Delta y - c(a_1) \\
&= y_l + \Pr(Y < \lambda a_1 - a_2)\Delta y - c(a_1) \\
&= y_l + G(\lambda a_1 - a_2)\Delta y - c(a_1).
\end{aligned}$$

where $\Delta y = y_w - y_l$. Worker 2 maximizes

$$\begin{aligned}
V_2(a_1, a_2, y_l, y_w) &= y_l + \Pr(Q_2 > q_1)\Delta y - c(a_2) \\
&= y_l + \Pr(a_2 - a_1 + Y > 0)\Delta y - c(a_1) \\
&= y_l + \Pr(Y > a_1 - a_2)\Delta y - c(a_2) \\
&= y_l + \left[1 - \Pr(Y < a_1 - a_2)\right]\Delta y - c(a_2) \\
&= y_l + [1 - G(a_1 - a_2)]\Delta y - c(a_2).
\end{aligned}$$

Costs of effort are quadratic i.e.,

$$C(a_i) = \frac{a_i^2}{2}, \text{ for } i = 1, 2. \tag{2.4}$$

The individual shocks follow a uniform distribution: $\varepsilon_1 \sim U[-u, u]$ and $\varepsilon_2 \sim U[-u, u]$, with $u > 0$. Thus, letting $Y = \varepsilon_2 - \varepsilon_1$[6], I have

$$G_Y(y) = \Pr(Y \leq y) = \begin{cases} \frac{1}{2} + \frac{y}{2u} + \frac{y^2}{8u^2} & -2u < y < 0 \\ \frac{1}{2} + \frac{y}{2u} - \frac{y^2}{8u^2} & 0 < y < 2u \end{cases},$$

and

$$g_Y(y) = \begin{cases} \frac{1}{2u} + \frac{y}{4u^2} & -2u < y < 0 \\ \frac{1}{2u} - \frac{y}{4u^2} & 0 < y < 2u \end{cases}.$$

For worker 1

$$G(\lambda a_1 - a_2) = \Pr(Y \leq y) = \begin{cases} \frac{1}{2} + \frac{\lambda a_1 - a_2}{2u} + \frac{(\lambda a_1 - a_2)^2}{8u^2} & -2u < \lambda a_1 - a_2 < 0 \\ \frac{1}{2} + \frac{\lambda a_1 - a_2}{2u} - \frac{(\lambda a_1 - a_2)^2}{8u^2} & 0 < \lambda a_1 - a_2 < 2u \end{cases}, \tag{2.5}$$

[6] $Y = \varepsilon_2 + \omega - \varepsilon_1 - \omega$

and

$$g_Y(\lambda a_1 - a_2) = \begin{cases} \frac{1}{2u} + \frac{\lambda a_1 - a_2}{4u^2} & -2u < \lambda a_1 - a_2 < 0 \\ \frac{1}{2u} - \frac{\lambda a_1 - a_2}{4u^2} & 0 < \lambda a_1 - a_2 < 2u \end{cases}.$$

For worker 2

$$G(a_1 - a_2) = \Pr(Y \leq y) = \begin{cases} \frac{1}{2} + \frac{a_1 - a_2}{2u} + \frac{(a_1 - a_2)^2}{8u^2} & -2u < a_1 - a_2 < 0 \\ \frac{1}{2} + \frac{a_1 - a_2}{2u} - \frac{(a_1 - a_2)^2}{8u^2} & 0 < a_1 - a_2 < 2u \end{cases}, \tag{2.6}$$

and

$$g_Y(a_1 - a_2) = \begin{cases} \frac{1}{2u} + \frac{a_1 - a_2}{4u^2} & -2u < a_1 - a_2 < 0 \\ \frac{1}{2u} - \frac{a_1 - a_2}{4u^2} & 0 < a_1 - a_2 < 2u \end{cases}.$$

Making use of (2.5) worker 1 maximizes

$$V_1(a_1, a_2, \lambda, y_l, y_w) = \begin{cases} y_l + \left[\frac{1}{2} + \frac{\lambda a_1 - a_2}{2u} + \frac{(\lambda a_1 - a_2)^2}{8u^2}\right] \Delta y - \frac{a_1^2}{2} & -2u < \lambda a_1 - a_2 < 0 \\ y_l + \left[\frac{1}{2} + \frac{\lambda a_1 - a_2}{2u} - \frac{(\lambda a_1 - a_2)^2}{8u^2}\right] \Delta y - \frac{a_1^2}{2} & 0 < \lambda a_1 - a_2 < 2u \end{cases}.$$

Making use of (2.6) worker 2 maximizes

$$V_2(a_1, a_2, y_l, y_w) = \begin{cases} y_l + \left[\frac{1}{2} - \frac{a_1 - a_2}{2u} - \frac{(a_1 - a_2)^2}{8u^2}\right] \Delta y - \frac{a_2^2}{2} & -2u < a_1 - a_2 < 0 \\ y_l + \left[\frac{1}{2} - \frac{a_1 - a_2}{2u} + \frac{(a_1 - a_2)^2}{8u^2}\right] \Delta y - \frac{a_2^2}{2} & 0 < a_1 - a_2 < 2u \end{cases}.$$

The first order condition (FOC) for worker 1 is

$$\begin{aligned} \lambda\left(\frac{1}{2u} + \frac{\lambda a_1 - a_2}{4u^2}\right)\Delta y &= a_1 \quad -2u < \lambda a_1 - a_2 < 0 \\ \lambda\left(\frac{1}{2u} - \frac{\lambda a_1 - a_2}{4u^2}\right)\Delta y &= a_1 \quad 0 < \lambda a_1 - a_2 < 2u \end{aligned}$$

The first order condition (FOC) for worker 2 is

$$\begin{aligned} \left(\frac{1}{2u} + \frac{a_1 - a_2}{4u^2}\right)\Delta y &= a_2 \quad -2u < a_1 - a_2 < 0 \\ \left(\frac{1}{2u} - \frac{a_1 - a_2}{4u^2}\right)\Delta y &= a_2 \quad 0 < a_1 - a_2 < 2u \end{aligned}$$

The best reply of worker 1 is given by

$$R_1(a_2) = \begin{cases} \frac{2u\lambda\Delta y}{4u^2 - \lambda^2\Delta y} - \frac{\lambda\Delta y}{4u^2 - \lambda^2\Delta y} a_2 & -2u < \lambda a_1 - a_2 < 0 \\ \frac{\lambda\Delta y}{2u} & \lambda a_1 = a_2 \\ \frac{2u\lambda\Delta y}{4u^2 + \lambda^2\Delta y} + \frac{\lambda\Delta y}{4u^2 + \lambda^2\Delta y} a_2 & 0 < \lambda a_1 - a_2 < 2u \end{cases}.$$

or equivalently

$$a_2 = \begin{cases} 2u + \frac{\lambda^2\Delta y - 4u^2}{\lambda\Delta y} a_1 & -2u < \lambda a_1 - a_2 < 0 \\ \lambda^2 \frac{\Delta y}{2u} & \lambda a_1 = a_2 \\ -2u + \frac{4u^2 + \lambda^2\Delta y}{\lambda\Delta y} a_1 & 0 < \lambda a_1 - a_2 < 2u \end{cases}.$$

The best reply of worker 2 is given by

$$R_2(a_1) = \begin{cases} \frac{2u\Delta y}{4u^2 + \Delta y} + \frac{\Delta y}{4u^2 + \Delta y} a_1 & -2u < a_1 - a_2 < 0 \\ \frac{\Delta y}{2u} & a_1 = a_2 \\ \frac{2u\Delta y}{4u^2 - \Delta y} - \frac{\Delta y}{4u^2 - \Delta y} a_1 & 0 < a_1 - a_2 < 2u \end{cases}.$$

The second order condition (SOC) for worker 1 is

$$\begin{aligned} \lambda^2 &< \frac{4u^2}{\Delta y} \quad -2u < \lambda a_1 - a_2 < 0 \\ \lambda^2 &> -\frac{4u^2}{\Delta y} \quad 0 < \lambda a_1 - a_2 < 2u \end{aligned}$$

The second order condition (SOC) for worker 2 is

$$\begin{aligned} \frac{4u^2}{\Delta y} &> -1 \quad -2u < a_1 - a_2 < 0 \\ \frac{4u^2}{\Delta y} &> 1 \quad 0 < a_1 - a_2 < 2u \end{aligned}$$

To solve the model, I analyze two cases: $a_1^* > a_2^*$ i.e., worker 1 who is overconfident exerts more effort than worker 2 who is unbiased, and $a_2^* > a_1^*$ i.e., worker 1 who is overconfident exerts less effort than worker 2 who is unbiased. I will also compare the effort level exerted in the asymmetric equilibrium to the effort levels that would be exerted in a symmetric equilibrium with two unbiased workers ($\lambda = 1$). From now on, I will refer to this case as “symmetric equilibrium ”for brevity. In the symmetric equilibrium, the effort levels exerted by both workers coincide and are equal to $a_R = \Delta y/2u$. Proposition 1 describes the impact of overconfidence on effort provision, the proof of Proposition 1 is reported in Appendix B.

Proposition 1 *Overconfidence affects the effort provision of workers 1 and 2 by shifting the Nash Equilibrium away from the symmetric equilibrium (a_R). The impact of overconfidence on effort provision depends on the degree of overconfidence of worker 1: (i) when overconfidence is small* *i.e., $\lambda < 4u^2/\Delta y$, the Nash Equilibrium effort levels are*

$$\begin{aligned} a_1^* &= \frac{8u^3\lambda\Delta y}{(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2} \\ a_2^* &= \frac{8u^3\Delta y+2u(\lambda-1)\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2} \end{aligned}$$

with $a_1^* > a_R > a_2^*$: the effort level exerted by the overconfident worker is higher and the effort level exerted by the unbiased worker is lower than the effort level exerted in the symmetric equilibrium; (ii) when overconfidence is large i.e., $\lambda > 4u^2/\Delta y$, the Nash Equilibrium effort levels are

$$\begin{aligned} a_1^* &= \frac{8u^3\lambda\Delta y+4u\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2} \\ a_2^* &= \frac{8u^3\Delta y+2u(\lambda^2+\lambda)(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2} \end{aligned}$$

with $a_R > a_2^* > a_1^*$: the effort level exerted by the overconfident worker is lower than the effort level exerted by the unbiased worker which in turn is lower than the effort level exerted in the symmetric equilibrium.

Compared to effort provision in the symmetric equilibrium, the effort exerted by the overconfident worker is the highest when overconfidence is small and the lowest when overconfidence is large. When overconfidence is small i.e., $\lambda < 4u^2/\Delta y$[7], worker 1 exerts more effort than worker 2 and the effort that would be exerted in the symmetric equilibrium. This may happen since the overestimation of one's productivity and thus probability of winning the tournament leads to an overestimation of the expected utility gain (compared to the disutility of exerting more effort). As a consequence, worker 1 to increases effort provision. When overconfidence is large i.e., $\lambda > 4u^2/\Delta y$[8], worker 1 exerts less effort than worker 2 and the effort that would be exerted in the symmetric equilibrium. In this case worker 1 overestimates her productivity and probability of winning so much that she believes she is able to win the tournament exerting little effort and thus decreases effort provision.

It is interesting to note that, no matter the degree of overconfidence, worker 1 overestimates her probability of winning the tournament. However, when lambda is small, the actual probability

[7]The Nash Equilibrium solution for small lambda satisfies workers' SOC, as shown in Appendix B.

[8]The Nash Equilibrium solution for large lambda satisfies workers' SOC, as shown in Appendix B.

of winning the tournament for worker 1 increases (the higher the effort, the higher the expected output). When lambda is large, the actual probability of winning the tournament for worker 1 decreases (the lower the effort, the lower the expected output). Appendix C shows analytically that this is the case.

As mentioned in Proposition 1, overconfidence shifts the effort provision of both workers away from the symmetric equilibrium. This happens since, while the overconfidence of worker 1 does not affect the best reply function of worker 2, it does shift the best reply function of worker 1. The shift in the best reply of worker 1 in turn implies that the equilibrium moves away from the bisector where the symmetric equilibrium lies.[9] Moreover, different degrees of overconfidence affect the equilibrium in different directions. Small overconfidence (lambda) shifts the equilibrium below the bisector while large overconfidence (lambda) shifts the equilibrium above the bisector. Hence, when overconfidence is small, worker 1 exerts more effort than worker 2 and when overconfidence is large worker 1 exerts less effort than worker 2. For a better understanding of these dynamics, in what follows I represent graphically the symmetric equilibrium (Figure 2.1) as well as the solutions for small and large lambda in the asymmetric equilibrium (Figures 2.2 and 2.3). The parametrization used and the mathematical expressions of the best reply functions represented are reported in Appendix D.

Figure (2.1) represents the symmetric equilibrium i.e., $\lambda = 1$. The effort level of worker 1 is represented on the x-axis and the effort level of worker 2 is represented on the y-axis. The symmetric equilibrium is indicated as "R", in black. The best reply function of worker 1 is indicated as "BR1,R"(the best reply function of worker 1 if worker 1 is rational i.e., $\lambda = 1$), dashed black line. The best reply function of worker 2 is indicated as "BR2", continuous black line. When both workers are unbiased, the symmetric equilibrium lies on the bisector and both workers exert the same level of effort ($a_1^* = a_2^* = a_R$).

Figure (2.2) represents the small lambda case i.e., $\lambda < \frac{4u^2}{\Delta y}$. The effort level of worker 1 is represented on the x-axis and the effort level of worker 2 is represented on the y-axis. The Nash Equilibrium is indicated as "NE", in red, while the symmetric equilibrium is indicated as "R", in black. The overconfidence of worker 1 shifts the best reply function from "BR1,R"(the best reply function of worker 1 if worker 1 was rational i.e., $\lambda = 1$), dashed black line, to "BR1"(the best reply

[9]Recall that at the symmetric equilibrium both workers exert the same effort level thus the equilibrium lies on the bisector.

function of worker 1 given that worker 1 is overconfident i.e., $\lambda > 1$), dashed red line. The best reply function of worker 2 is indicated as "BR2", continuous red line, and it is not affected from overconfidence since worker 2 is unbiased[10]. When overconfidence is small, the Nash Equilibrium lies below the bisector and the effort exerted by the overconfident worker is higher than the effort exerted by the unbiased worker. In this case, the overconfident worker exerts more effort than in the symmetric equilibrium ($a_1^* > a_R > a_2^*$) and the unbiased worker exerts less effort than in the symmetric equilibrium.

Figure 2.1: Symmetric Equilibrium, $\lambda = 1$

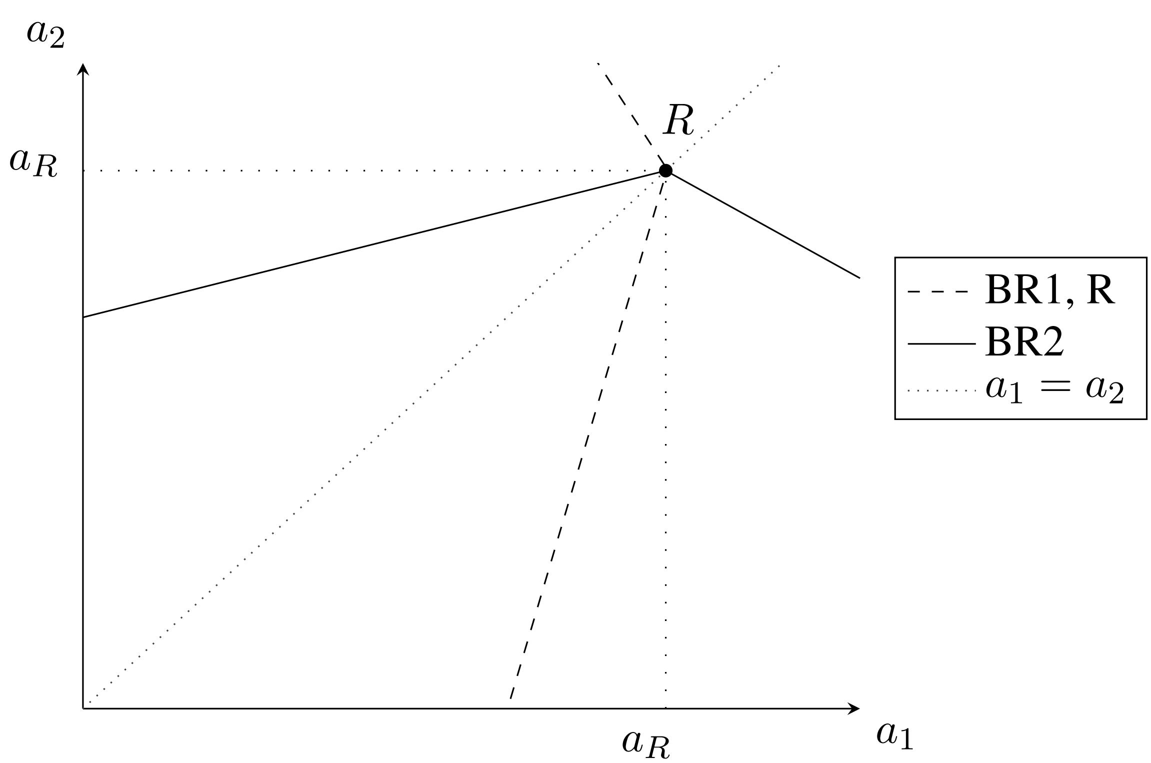
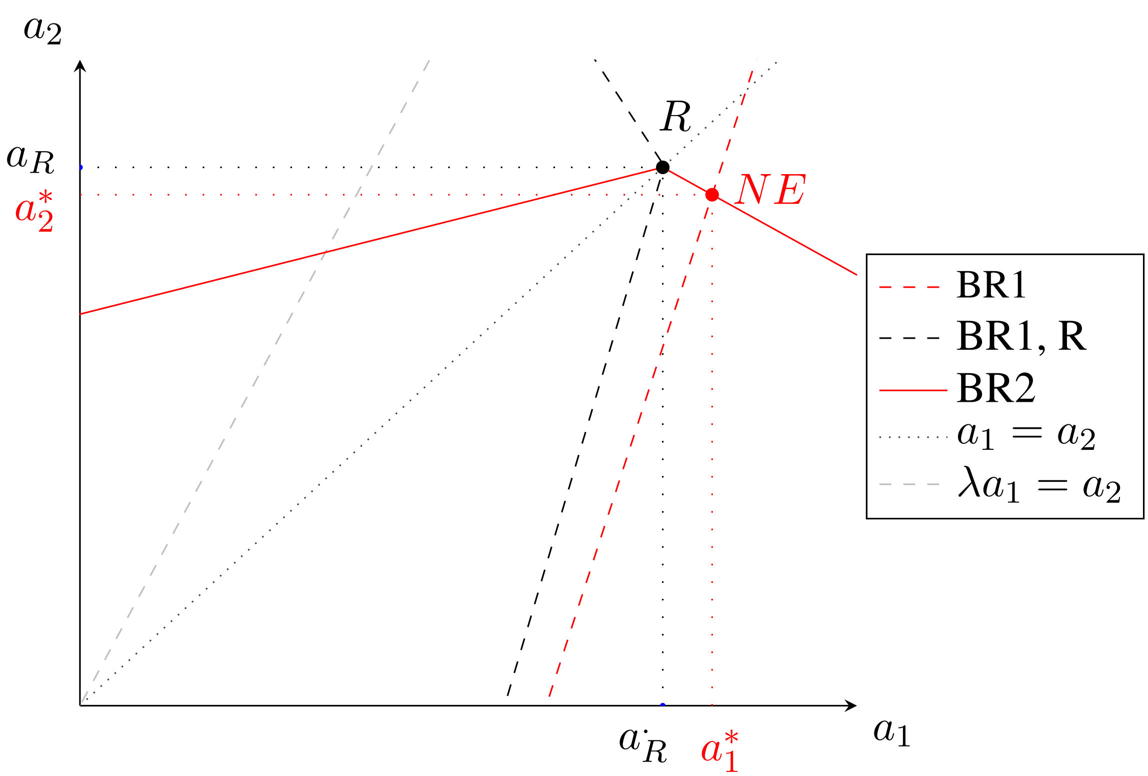


Figure (2.3) represents the large lambda case i.e., $\lambda > \frac{4u^2}{\Delta y}$. The effort level of worker 1 is

[10]Note that the best reply function of worker 2 remains unchanged from Figure (2.1) to Figure (2.2). However, in Figure (2.2), it is represented in red, and not in black, to underline that the equilibrium is now the Nash equilibrium and not the symmetric one.

represented on the x-axis and the effort level of worker 2 is represented on the y-axis. The Nash Equilibrium is indicated as "NE", in red, while the symmetric equilibrium is indicated as "R", in black. The overconfidence of worker 1 shifts the best reply function from "BR1,R"(the best reply function of worker 1 if worker 1 was rational i.e., $\lambda = 1$), dashed black line, to "BR1"(the best reply function of worker 1 given that worker 1 is overconfident i.e., $\lambda > 1$), dashed red line. The best reply function of worker 2 is indicated as "BR2", continuous red line, and it is not affected

Figure 2.2: Nash Equilibrium for Small Lambda, $\lambda < \frac{4u^2}{\Delta y}$

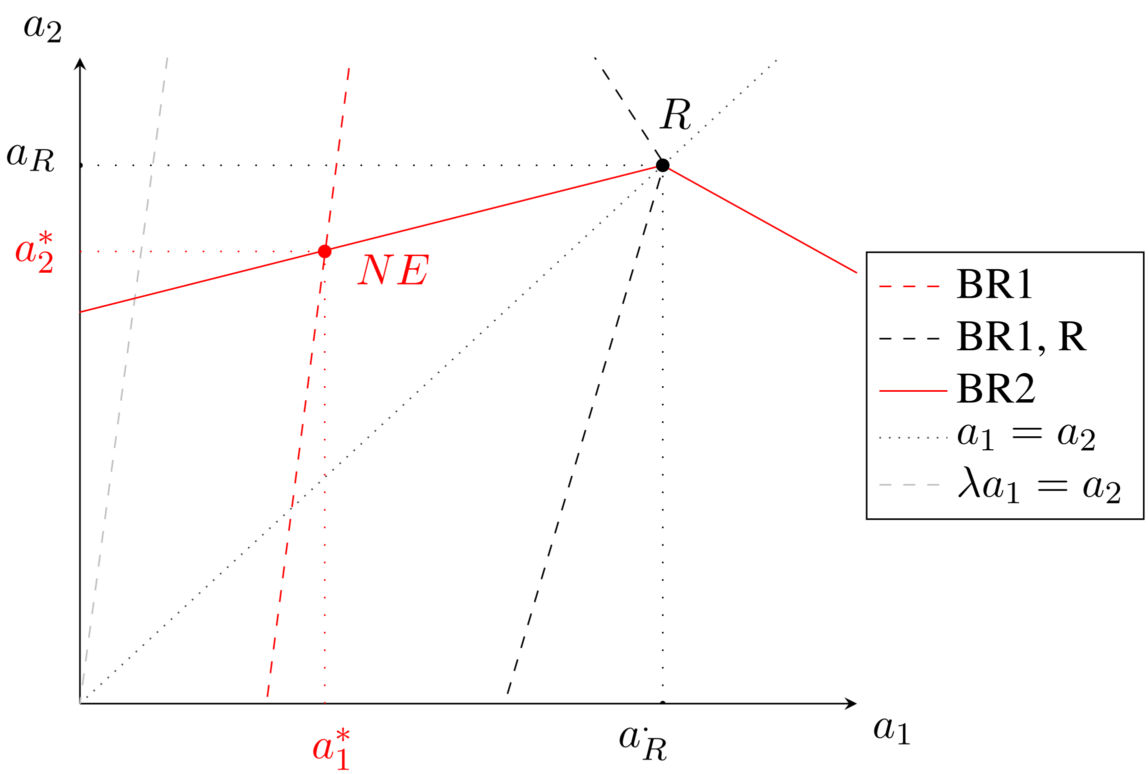


from overconfidence since worker 2 is unbiased[11]. When overconfidence is large, the Nash Equilibrium lies above the bisector and the effort exerted by the overconfident worker is lower than

[11]Note that the best reply function of worker 2 remains unchanged from Figure (2.1) to Figure (2.3). However, in Figure (2.3), it is represented in red, and not in black, to underline that the equilibrium is now the Nash equilibrium and not the symmetric one.

the effort exerted by the unbiased worker. In this case, both workers exert less effort than in the symmetric equilibrium ($a_R > a_2^* > a_1^*$).

Figure 2.3: Nash Equilibrium for Large Lambda, $\lambda > \frac{4u^2}{\Delta y}$



2.4 Overconfidence Implications for the Firm

This section discusses the implications of overconfidence for the firm. In what follows, I will compare the firm's expected profit for a given prize spread in the symmetric and asymmetric equilibrium.

As discussed above, the overconfidence of worker 1 has an impact on the effort provisions of both workers and this impact is different depending on the degree of overconfidence of worker 1 (small or large lambda). In both cases, as well as in the case of a symmetric equilibrium, individual effort at optimum increases in the prize spread[12]. However, when the degree of overconfidence of worker 1 is small, worker 1 exerts more effort than in the symmetric equilibrium while worker 2 exerts less effort than in the symmetric equilibrium. When the degree of overconfidence of worker 1 is large, both workers exert less effort than in the symmetric equilibrium. To disentangle whether the firm would be better off hiring an overconfident worker and an unbiased worker or two unbiased workers, I compare the expected profit in both cases.

The expected profit is equal to the expected total output minus the firm's cost or

$$E[\Pi] = \quad E[Q_1(a_1) + Q_2(a_2)] - (y_w + y_l)$$

When output distributes as a uniform and costs of effort are quadratic, the firm's expected profit is equal to

$$\begin{aligned}
E[\Pi] &= \quad E[Q_1(a_1)] + E[Q_2(a_2)] - (y_w + y_l) \\
E[\Pi] &= \quad E[a_1 + \omega + \varepsilon_1] + E[a_2 + \omega + \varepsilon_2] - (y_w + y_l) \\
E[\Pi] &= \quad E[a_1] + E[\omega] + E[\varepsilon_1] + E[a_2] + E[\omega] + E[\varepsilon_2] - (y_w + y_l) \\
E[\Pi] &= \quad a_1^* + a_2^* + 2E[\omega] - (y_w + y_l)
\end{aligned}$$

Given that, for a given prize spread Δy, the firm's cost, $(y_w + y_l)$, is the same in both the asymmetric and symmetric equilibrium, and that in both cases the expected value of the common shock $E[\omega]$ is the same, to compare the expected profits and disentangle in which case the firm is better off, it is sufficient to compare the expected total output i.e., $E[Q_1(a_1) + E[Q_2(a_2)]$. Following this reasoning, for a given prize spread, the higher the expected total output, the higher the expected profit and thus the better off the firm is. Proposition 2 describes the implications of overconfidence for the firm for a given prize spread. The proof of Proposition 2 is reported in Appendix E.

Proposition 2 *For a given prize spread $\Delta y = y_w - y_l$, (i) when lambda is small, the firm is better off in the asymmetric equilibrium compared to the symmetric equilibrium if and only if $2u^2 > \Delta y$. (ii) when lambda is large, the firm is always worse off in the asymmetric equilibrium compared to*

[12]This is a well known result in the tournament literature as incentive schemes (Nalebuff and Stiglitz 1983).

the symmetric equilibrium.

The reason behind Proposition 2 is as follows. When overconfidence is small effort provision is affected in opposite directions: the effort provision of worker 1 increases while the effort provision of worker 2 decreases. If the combined effort provision is higher than in the symmetric equilibrium, expected output and thus expected profit increases. The expected profit is higher when the increase in effort provision of worker 1 more than compensates the decrease in effort provision of worker 2. This the case if and only if $2u^2 > \Delta y$. The last condition links the price spread to output volatility.[13] If volatility is relatively high, for a given prize spread, the firm is better off hiring an overconfident and an unbiased worker since effort provision and thus expected output and profit increase. If volatility is relatively low, for a given prize spread, the firm is better off hiring two unbiased workers since effort provision and thus expected output and profit decrease. When overconfidence is large instead, effort provision always decreases. As a consequence, expected output and thus expected profit are on average lower compared to the symmetric equilibrium. Hence, the firm is always worse off when the overconfident worker is significantly overconfident.

2.5 Conclusion

This paper studies theoretically the impact of overconfidence on effort provision in an asymmetric tournament. The model extends Santos-Pinto (2010) to allow for workers' heterogeneity. In the model, the worker with the highest relative rank receives a winner wage and the worker with the lowest relative rank receives a loser wage, regardless of the absolute quantities they produce. Agents have identical preferences and cost of effort but are heterogeneous in the self-assessment of their own performance: worker 1 is overconfident and overestimates her own productivity and thus his probability of winning the tournament while worker 2 has an unbiased self assessment.

The main findings are as follows: the overconfidence of worker 1 shifts the Nash Equilibrium away from the symmetric tournament equilibrium, affecting the effort provision of both workers. However, different degrees of overconfidence impact effort provision in different directions. A small degree of overconfidence increases the effort provision of worker 1 and decreases the effort provision of worker 2 compared to the effort provision in a symmetric tournament in which both

[13] Note that the variance of the joint distribution of individual outputs, the triangular distribution, is equal to $1/6 * u^2$. Thus, $2u^2$ can be interpreted as a measure of output volatility.

workers are unbiased. A large degree of overconfidence decreases the effort provision of both workers compared to the effort provision in a symmetric tournament in which both workers are unbiased. The decrease in effort provision that follows from a large degree of overconfidence lowers the expected output and thus the expected profits for the firm. The firm is better off when the degree of overconfidence is small since, for a given prize spread, on average profits are higher.

Appendix

A. Simplified Model

In what follows I derive a simplified model. A monopolistic, risk neutral firm, rewards employees using a tournament incentive scheme. With two workers, the worker who produces the highest quantity is ranked first and receives a winner wage y_w and the worker who produces the lowest quantity is ranked last and receives the loser wage y_l,with $y_w > y_l > 0$. One worker, worker 1, is overconfident: she overestimates the probability of winning the tournament for a given level of effort exerted. The other worker, worker 2, has a correct self-assessment: she can correctly estimate the chances of winning the tournament for a given effort level. The bias of worker 1 is explained as follows: worker 1 believes she is more productive than what she is and thus she overestimates her probability of winning the tournament. Apart from the self-image bias of worker 1, both workers are identical.

The simplified model is as follows. Assume that the (actual) productivity function of workers is linear in effort

$$e_i = (e_i(a_i)) = a_i$$

Knowing that worker 2 has an unbiased self-assessment, her actual and perceived productivity will be identical and equal to

$$e_2 = e_2(a_2) = a_2$$

worker 1 overestimates her productivity by λ and her perceived productivity is

$$e_1 = (e_1(a_1, \lambda)) = \lambda * a_1$$

with $\lambda > 1$.

Each worker chooses the optimal effort level in order to maximize her (perceived) expected utility. To simplify the derivation, I will now assume that both workers are risk neutral. Worker 1 maximizes

$$\begin{aligned}
V_1(a_1, a_2, \lambda, y_l, y_w) &= y_l + \Pr(Q_1 > q_2)\Delta y - c(a_1) \\
&= y_l + \Pr(a_1\lambda - a_2 - Y > 0)\Delta y - c(a_1) \\
&= y_l + \Pr(Y < \lambda a_1 - a_2)\Delta y - c(a_1) \\
&= y_l + G(\lambda a_1 - a_2)\Delta y - c(a_1).
\end{aligned}$$

where $\Delta y = y_w - y_l$. Worker 2 maximizes

$$\begin{aligned} V_2(a_1, a_2, y_l, y_w) &= y_l + \Pr(Q_2 > q_1)\Delta y - c(a_2) \\ &= y_l + \Pr(a_2 - a_1 + Y > 0)\Delta y - c(a_1) \\ &= y_l + \Pr(Y > a_1 - a_2)\Delta y - c(a_2) \\ &= y_l + [1 - \Pr(Y < a_1 - a_2)]\,\Delta y - c(a_2) \\ &= y_l + [1 - G(a_1 - a_2)]\Delta y - c(a_2). \end{aligned}$$

The first order conditions for workers 1 and 2 are

$$\lambda g(\lambda a_1 - a_2)\Delta y = c'(a_1) \tag{2.7}$$

$$g(a_1 - a_2)\Delta y = c'(a_2) \tag{2.8}$$

The ratio is

$$\frac{\lambda g(\lambda a_1 - a_2)}{g(a_1 - a_2)} = \frac{c'(a_1)}{c'(a_2)} \tag{2.9}$$

Similarly to (2.3), expression (2.9) indicates how the optimal effort level of worker 1, the optimal level of effort of worker 2 and the overconfidence of worker 1 are related. The probability of winning the tournament increases in the level of effort exerted for both workers: $\frac{\partial G(a_1,a_2,\lambda)}{\partial a_1} = \lambda g(\lambda a_1 - a_2) > 0$ for worker 1 and $\frac{\partial G(a_1,a_2)}{\partial a_2} = g(a_1 - a_2) > 0$ for worker 2.[14] Intuitively, the higher the effort, the higher the output and thus the higher the probability of winning the tournament.

To study the impact of overconfidence on the perceived probability of winning the tournament for worker 1, I take the cross partial derivative of $G(a_1, a_2, \lambda)$

$$\frac{\partial G^2(a_1, a_2, \lambda)}{\partial a_1 \partial \lambda} = \frac{\partial \lambda g(a_1\lambda - a_2)}{\partial \lambda} = g(a_1\lambda - a_2) + \lambda^2 g'(a_1\lambda - a_2) \tag{2.10}$$

Knowing that $g(a_1\lambda - a_2) > 0$ and that $\lambda \in> 1$, the term $g'(a_1\lambda - a_2)$ determines the sign of (2.10). When (2.10) is positive, the perceived probability of winning the tournament increases in λ while when (2.10) is negative, the perceived probability of winning the tournament decreases in λ.

[14]In both cases the first derivative is positive given that $g()$ is a probability distribution function that is positively defined, and in the case of worker 1, $g()$ is multiplied by λ that is positive ($\lambda > 1$).

Moreover, when (2.10) is equal to zero, the perceived probability of winning the tournament does not depend on λ.

From condition (2.9), when both workers have an accurate self assessment ($\lambda = 1$), they exert the same effort[15]

$$\begin{aligned} \frac{g(a_1 - a_2)}{g(a_1 - a_2)} &= \frac{c'(a_1)}{c'(a_2)} \\ \Leftrightarrow c'(a_1) &= c'(a_2) \\ \Leftrightarrow a_1^* &= a_2^* \end{aligned}$$

However, when worker 1 is overconfident, this may not be the case. When $\lambda g(a_1\lambda - a_2) > g(a_1 - a_2)$ then $\frac{\lambda g(a_1\lambda - a_2)}{g(a_1 - a_2)} > 1$. Thus from (2.9) it follows that $c'(a_1) > c'(a_2)$ and so:

$$a_1^* > a_2^*$$

At optimum, worker 1 who is overconfident exerts more effort than worker 2 who is unbiased.

When $\lambda g(a_1\lambda - a_2) < g(a_1 - a_2)$, then $\frac{\lambda g(a_1\lambda - a_2)}{g(a_1 - a_2)} < 1$. Thus from (2.9) it follows that $c'(a_1) < c'(a_2)$ and so:

$$a_1^* < a_2^*$$

At optimum, worker 1 who is overconfident exerts less effort than worker 2 who is unbiased.

Whether $\lambda g(a_1\lambda - a_2)$ is higher or lower than $g(a_1 - a_2)$ depends on the characteristics of the probability distribution $g()$. When $g()$ is monotonic and non-decreasing, for instance if ε_i distributes as a uniform, then it is always the case that $\lambda^2 g(a_1\lambda - a_2) > g(a_1 - a_2)$. However, when $g()$ is non monotonic, for instance if ε_i distributes as a normal, then $\lambda g(a_1\lambda - a_2) > g(a_1 - a_2)$or $\lambda g(a_1\lambda - a_2) < g(a_1 - a_2)$ depending on λ, a_1 and a_2.

Now I will study the impact of λ on the optimal effort levels exerted by workers 1 and 2. Going back to the first order conditions I had:

$$\begin{aligned} \lambda g(\lambda a_1 - a_2)\Delta y &= c'(a_1) \\ g(a_1 - a_2)\Delta y &= c'(a_2) \end{aligned}$$

[15] Note that $g(0) > 0$.

The second order conditions are

$$\lambda^2 g'(\lambda a_1 - a_2)\Delta y - c''(a_1) \quad < \quad 0 \tag{2.11}$$

$$g'(a_1 - a_2)\Delta y + c''(a_2) \quad > \quad 0 \tag{2.12}$$

Differentiating the FOCs with respect to a_1, a_2, and λ I have

$$\lambda g'(\lambda a_1 - a_2)\Delta y(a_1 d\lambda + \lambda da_1 - da_2) + g(\lambda a_1 - a_2)\Delta y d\lambda = c''(a_1)da_1 \tag{2.13}$$

$$g'(a_1 - a_2)\Delta y(da_1 - da_2) = c''(a_2)da_2 \tag{2.14}$$

Solving (2.14) with respect to da_2

$$da_2 = \frac{g'(a_1 - a_2)\Delta y}{g'(a_1 - a_2)\Delta y + c''(a_2)} da_1. \tag{2.15}$$

And the effect of a_1 on a_2 is given by

$$\frac{da_2}{da_1} = \frac{g'(a_1 - a_2)\Delta y}{g'(a_1 - a_2)\Delta y + c''(a_2)}.$$

The sign of the denominator of $\frac{da_2}{da_1}$ is positive due to (2.12), the second order condition of worker 2. Hence, the sign of $\frac{da_2}{da_1}$ only depends on the sign of the numerator. Knowing that Δy is positive, the sign of the numerator is given by the sign of $g'()$ which in turn depends on the characteristics of the cumulative distribution G. Let us assume that G is such that $g'(x) > 0$ for $x < 0$, $g'(0) = 0$, and $g'(x) < 0$ for $x > 0$. This includes the normal distribution. For $a_1 < a_2$ the sign of the numerator is positive and therefore $\frac{da_2}{da_1} > 0$. This shows that if worker 1 exerts less effort than worker 2, then an increase in the effort of worker 1 increases the effort of worker 2. For $a_1 > a_2$ the sign of the numerator is negative and therefore $\frac{da_2}{da_1} < 0$. This shows that if worker 1 exerts more effort than worker 2, then an increase in the effort of worker 1 lowers the effort of worker 2.

Substituting (2.15) into (2.13) I obtain

$$\lambda g'(\lambda a_1 - a_2)\Delta y \left[a_1 d\lambda + \lambda da_1 - \frac{g'(a_1 - a_2)\Delta y}{g'(a_1 - a_2)\Delta y + c''(a_2)} da_1 \right] + g(\lambda a_1 - a_2)\Delta y d\lambda = c''(a_1)da_1.$$

Solving this equation with respect to $\frac{da_1}{da_\lambda}$ I obtain

$$\frac{da_1}{da_\lambda} = \frac{\lambda g'(\lambda a_1 - a_2)a_1 + g(\lambda a_1 - a_2)}{\frac{\lambda g'(\lambda a_1 - a_2)g'(a_1 - a_2)}{g'(a_1 - a_2)\Delta y + c''(a_2)} - [\lambda^2 g'(\lambda a_1 - a_2)\Delta y - c''(a_1)]} \Delta y. \tag{2.16}$$

From the SOCs (2.11) and (2.12) and the assumptions on the cumulative distribution G, it follows that the sign of the denominator of (2.16) is strictly positive. Hence, the sign of $\frac{da_1}{da_\lambda}$ only depends on the sign of the numerator. The sign of the first term in the numerator of (2.16) is non-negative when $\lambda a_1 - a_2 \leq 0$. The sign of the first term in the numerator of (2.16) is negative when $\lambda a_1 - a_2 > 0$. The sign of the second term in the numerator of (2.16) is always positive given that g is a density function. Hence, we know that:

$$\frac{da_1}{da_\lambda} > 0 \text{ when } \lambda a_1 - a_2 \leq 0$$

For the sign of $\frac{da_1}{da_\lambda}$ to be positive when $\lambda a_1 - a_2 > 0$ it must be that

$$\lambda g'(\lambda a_1 - a_2)a_1 + g(\lambda a_1 - a_2) > 0 \tag{2.17}$$

To conclude, if (2.17) holds, and $\lambda a_1 - a_2 > a_1 - a_2 > 0$, then

$$\frac{da_1}{da_\lambda} > 0 \text{ and } \frac{da_2}{d\lambda} = \frac{da_2}{da_1}\frac{da_1}{d\lambda} < 0.$$

In words, if (2.17) holds and worker 1 exerts more effort than worker 2, then an increase in the overconfidence of worker 1 raises the effort of worker 1 and lowers the effort of worker 2.

For completeness, Appendix A.1 studies effort provision when workers' output follows a normal distribution and effort costs are quadratic.

A.1 Normal Distribution

Hereafter I assume that workers' output follows a normal distribution and that individual costs of effort are quadratic:

$$C(a_i) = \frac{a_i^2}{2} \tag{2.18}$$

If $\varepsilon_i \sim N(0, \sigma^2)$, worker 1 maximizes

$$\begin{aligned} V_1(a_1, a_2, \lambda, y_l, y_w) &= y_l + P(Q_1 > q_2)\Delta y - c(a_1) \\ &= y_l + P(a_1\lambda - a_2 - \varepsilon > 0)\Delta y - c(a_1) \\ &= y_l + P(\varepsilon < \lambda a_1 - a_2)\Delta y - c(a_1) \\ &= y_l + \Phi(\lambda a_1 - a_2)\Delta y - \frac{a_1^2}{2} \end{aligned}$$

and worker 2 maximizes

$$\begin{aligned}V_2(a_1,a_2,y_l,y_w) &= y_l + P(Q_2 > q_1)\Delta y - c(a_2)\\ &= y_l + P(a_1 - a_2 - \varepsilon < 0)\Delta y - c(a_2)\\ &= y_l + P(\varepsilon > a_1 - a_2)\Delta y - c(a_2)\\ &= y_l + [1 - \Phi(a_1 - a_2)]\Delta y - \frac{a_2^2}{2}\end{aligned}$$

where $\Phi \sim N(0, 2\sigma^2)$.[16]

The first order conditions for workers 1 and 2 are

$$\lambda * \frac{1}{2\sigma\sqrt{\pi}} e^{-\frac{1}{4\sigma^2}(\lambda a_1 - a_2)^2}\Delta y = a_1$$

$$\frac{1}{2\sigma\sqrt{\pi}} e^{-\frac{1}{4\sigma^2}(a_1 - a_2)^2}\Delta y = a_2$$

The ratio is

$$\lambda * \frac{e^{-\frac{1}{4\sigma^2}(\lambda a_1 - a_2)^2}}{e^{-\frac{1}{4\sigma^2}(a_1 - a_2)^2}} = \frac{a_1}{a_2} \tag{2.19}$$

or

$$a_1 = \lambda * \frac{e^{-\frac{1}{4\sigma^2}(\lambda a_1 - a_2)^2}}{e^{-\frac{1}{4\sigma^2}(a_1 - a_2)^2}} * a_2 \tag{2.20}$$

Worker 1's overconfidence may increase or decrease the perceived probability of winning the tournament. To study the impact of overconfidence on the perceived probability of winning the tournament, I study the sign of (2.10) which in this case is equal to:

$$\frac{\partial \Phi^2(a_1, a_2, \lambda)}{\partial a_1 \partial \lambda} = \frac{1}{2\sigma\sqrt{\pi}} e^{-\frac{1}{4\sigma^2}(\lambda a_1 - a_2)^2}\left(1 - \frac{\lambda a_1 - a_2}{2\sigma^2}\lambda a_1\right)$$

Knowing that $\frac{1}{2\sigma\sqrt{\pi}}$ is positive and that e^x is positive for every value of x, the sign of this expression is determined by $\left(1 - \frac{\lambda a_1 - a_2}{2\sigma^2}\lambda a_1\right)$. This term is positive when $2\sigma^2 > (\lambda a_1 - a_2)\lambda a_1$ and negative

---

[16]

$$\Phi(\lambda a_1 - a_2) = \frac{1}{2\sigma\sqrt{\pi}} \int_{-\infty}^{\lambda a_1 - a_2} e^{-\frac{(x-0)^2}{4\sigma^2}} dx$$

and

$$\Phi(a_1 - a_2) = \frac{1}{2\sigma\sqrt{\pi}} \int_{-\infty}^{a_1 - a_2} e^{-\frac{(x-0)^2}{4\sigma^2}} dx$$

when $2\sigma^2 < (\lambda a_1 - a_2)\lambda a_1$. In the first case, the perceived probability of winning the tournament increases in λ (the higher the λ, the higher the perceived probability of winning the tournament). In the second case, the perceived probability of winning the tournament decreases in λ (the higher λ, the lower the perceived probability of winning the tournament).

Following the reasoning in Appendix A, I will study the impact of λ on the optimal effort level exerted by workers 1 and 2. I have shown that the effect of λ on the effort level exerted by worker 1 is given by expression (2.16). Knowing that $\lambda > 1$, $a_1 \geq 0$ and that $g(\lambda a_1 - a_2) > 0$ (because g is a probability distribution function), the sign of (2.16) is determined by the sign of $g'(\lambda a_1 - a_2)$. The sign of (2.16) is positive in two cases. First, when $\lambda a_1 - a_2 \leq 0$ for the characteristics of a normal distribution.[17] Second, when $\lambda a_1 - a_2 > 0$ (and thus $g'(\lambda a_1 - a_2) < 0$) and condition (2.17) is verified. With a normal distribution, condition (2.17) can be rewritten as

$$\frac{1}{2\sigma\sqrt{\Pi}} e^{-\frac{1}{4\sigma^2}(\lambda a_1 - a_2)^2} \left(1 - \frac{\lambda a_1 - a_2}{2\sigma^2}\lambda^2 a_1\right) > 0 \tag{2.21}$$

which is positive for $2\sigma^2 > (\lambda a_1 - a_2)\lambda^2 a_1$.

Similarly to section (2.5), if (2.21) holds, and $\lambda a_1 - a_2 > a_1 - a_2 > 0$, then

$$\frac{da_1}{da_\lambda} > 0 \text{ and } \frac{da_2}{d\lambda} = \frac{da_2}{da_1}\frac{da_1}{d\lambda} < 0.$$

In words, if (2.17) holds and worker 1 exerts more effort than worker 2, then an increase in the overconfidence of worker 1 raises the effort of worker 1 and lowers the effort of worker 2.

B. Proof of Proposition 1

Small Lambda

Assume the solution satisfies $a_1 > a_2$. If that is the case, then the relevant FOC of worker 2 is

$$\left(\frac{1}{2u} - \frac{a_1 - a_2}{4u^2}\right)\Delta y = a_2.$$

and the relevant FOC for worker 1 is

$$\lambda\left(\frac{1}{2u} - \frac{\lambda a_1 - a_2}{4u^2}\right)\Delta y = a_1.$$

[17] G is such that $g'(x) > 0$ for $x < 0$, $g'(0) = 0$, and $g'(x) < 0$ for $x > 0$.

The SOC of worker 2 is satisfied as long as

$$\frac{4u^2}{\Delta y} > 1.$$

The SOC of worker 1 is satisfied. Solving the FOC of worker 2 for a_2 I obtain the best response of worker 2

$$a_2 = \frac{2u\Delta y}{4u^2 - \Delta y} - \frac{\Delta y}{4u^2 - \Delta y} a_1.$$

Substituting this expression in the FOC of worker 1

$$\lambda \left(2u - \lambda a_1 + \frac{2u\Delta y}{4u^2 - \Delta y} - \frac{\Delta y}{4u^2 - \Delta y} a_1 \right) \Delta y = 4u^2 a_1,$$

or

$$2u\lambda\Delta y - \lambda^2 a_1 \Delta y + \lambda \frac{2u(\Delta y)^2}{4u^2 - \Delta y} - \lambda \frac{(\Delta y)^2}{4u^2 - \Delta y} a_1 = 4u^2 a_1,$$

or

$$\frac{8u^3\lambda\Delta y}{4u^2 - \Delta y} = \left[4u^2 + \lambda^2 \Delta y + \lambda \frac{(\Delta y)^2}{4u^2 - \Delta y} \right] a_1,$$

or

$$a_1^* = \frac{\frac{8u^3\lambda\Delta y}{4u^2 - \Delta y}}{4u^2 + \lambda^2\Delta y + \lambda\frac{(\Delta y)^2}{4u^2 - \Delta y}} = \frac{8u^3\lambda\Delta y}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2}.$$

Substituting in the best response of worker 2 I obtain

$$\begin{aligned}
a_2^* &= \frac{2u\Delta y}{4u^2 - \Delta y} - \frac{\Delta y}{4u^2 - \Delta y} \frac{8u^3\lambda\Delta y}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} \\
&= \frac{2u\Delta y}{4u^2 - \Delta y} \left[1 - \frac{4u^2\lambda\Delta y}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} \right] \\
&= \frac{2u\Delta y}{4u^2 - \Delta y} \left[\frac{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2 - 4u^2\lambda\Delta y}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} \right] \\
&= \frac{2u\Delta y}{4u^2 - \Delta y} \left[\frac{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) - \lambda\Delta y(4u^2 - \Delta y)}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} \right] \\
&= \frac{2u\Delta y(4u^2 + \lambda^2\Delta y - \lambda\Delta y)}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} \\
&= \frac{8u^3\Delta y + 2u(\lambda - 1)\lambda(\Delta y)^2}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2}.
\end{aligned}$$

I check if this solution satisfies $a_1^* > a_2^*$ or equivalently

$$\frac{8u^3\lambda\Delta y}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} > \frac{8u^3\Delta y + 2u(\lambda - 1)\lambda(\Delta y)^2}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2},$$

or

$$8u^3\lambda\Delta y > 8u^3\Delta y + 2u(\lambda - 1)\lambda(\Delta y)^2,$$

or

$$8u^3(\lambda - 1)\Delta y > 2u(\lambda - 1)\lambda(\Delta y)^2,$$

or

$$4u^2 > \lambda\Delta y.$$

Hence, this solution is feasible as long as $4u^2 > \lambda\Delta y$ (note that this condition also implies that the SOC of worker 2 is satisfied). The Nash Equilibrium effort levels when $4u^2 > \lambda\Delta y$ are given by

$$\begin{aligned} a_1^* &= \frac{8u^3\lambda\Delta y}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} \\ a_2^* &= \frac{8u^3\Delta y + 2u(\lambda - 1)\lambda(\Delta y)^2}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} \end{aligned}$$

What is the solution in the symmetric tournament ($\lambda = 1$)?

$$a_1^* = a_2^* = \frac{8u^3\Delta y}{(4u^2 + \Delta y)(4u^2 - \Delta y) + (\Delta y)^2} = \frac{8u^3\Delta y}{16u^4 + 4u^2\Delta y - 4u^2\Delta y - (\Delta y)^2 + (\Delta y)^2} = \frac{8u^3\Delta y}{16u^4} = \frac{\Delta y}{2u}.$$

I now show that worker 1 exerts more effort in the asymmetric tournament:

$$\frac{8u^3\lambda\Delta y}{(4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} > \frac{\Delta y}{2u}$$

or

$$16u^4\lambda > (4u^2 + \lambda^2\Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2,$$

or

$$16u^4\lambda > 16u^4 + 4u^2\lambda^2\Delta y - \lambda^2(\Delta y)^2 - 4u^2\Delta y + \lambda(\Delta y)^2,$$

or

$$16u^4(\lambda - 1) + \lambda(\lambda - 1)(\Delta y)^2 > 4u^2\Delta y(\lambda^2 - 1),$$

or

$$16u^4 + \lambda(\Delta y)^2 > 4u^2\Delta y(\lambda + 1),$$

or

$$4u^2(4u^2 - \Delta y) > \lambda\Delta y(4u^2 - \Delta y),$$

or

$$4u^2 > \lambda \Delta y,$$

which is the same condition found before. I now show that worker 2 exerts less effort in the asymmetric tournament:

$$\frac{8u^3 \Delta y + 2u(\lambda - 1)\lambda(\Delta y)^2}{(4u^2 + \lambda^2 \Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2} < \frac{\Delta y}{2u},$$

or

$$[8u^3 + 2u(\lambda - 1)\lambda \Delta y]2u < (4u^2 + \lambda^2 \Delta y)(4u^2 - \Delta y) + \lambda(\Delta y)^2,$$

or

$$16u^4 + 4u^2(\lambda - 1)\lambda \Delta y < 16u^4 + 4u^2 \lambda^2 \Delta y - \lambda^2(\Delta y)^2 - 4u^2 \Delta y + \lambda(\Delta y)^2,$$

or

$$4u^2 \lambda^2 \Delta y - 4u^2 \lambda \Delta y < 4u^2 \lambda^2 \Delta y - \lambda^2(\Delta y)^2 - 4u^2 \Delta y + \lambda(\Delta y)^2,$$

or

$$\lambda^2(\Delta y)^2 - \lambda(\Delta y)^2 < 4u^2 \lambda \Delta y - 4u^2 \Delta y,$$

or

$$\lambda(\lambda - 1)\Delta y < 4u^2(\lambda - 1),$$

or

$$\lambda \Delta y < 4u^2.$$

Large Lambda

Assume that the solution satisfies $a_2 > a_1$. If this is the case, then the relevant FOC of worker 2 is

$$\left(\frac{1}{2u} + \frac{a_1 - a_2}{4u^2} \right) \Delta y = a_2.$$

and the SOC of worker 2 is satisfied.

I need to consider two separate cases for the relevant FOC of worker 1: $\lambda a_1 > a_2 > a_1$ (large lambda) and $a_2 > \lambda a_1 > a_1$ (small lambda).

In the large lambda case the relevant FOC of worker 1 is

$$\lambda \left(\frac{1}{2u} - \frac{\lambda a_1 - a_2}{4u^2} \right) \Delta y = a_1$$

and the SOC for worker 1 is satisfied. Solving the FOC of worker 2 for a_2 I have

$$a_2 = \frac{2u\Delta y}{4u^2+\Delta y} + \frac{\Delta y}{4u^2+\Delta y} a_1.$$

Substituting this expression in the FOC of worker 1

$$\lambda\left(2u - \lambda a_1 + \frac{2u\Delta y}{4u^2+\Delta y} + \frac{\Delta y}{4u^2+\Delta y} a_1\right)\Delta y = 4u^2 a_1,$$

or

$$2u\lambda\Delta y - \lambda^2 a_1 \Delta y + \frac{2u\lambda(\Delta y)^2}{4u^2+\Delta y} + \frac{\lambda(\Delta y)^2}{4u^2+\Delta y} a_1 = 4u^2 a_1,$$

or

$$2u\lambda\Delta y + \frac{2u\lambda(\Delta y)^2}{4u^2+\Delta y} = \left[4u^2 + \lambda^2\Delta y - \frac{\lambda(\Delta y)^2}{4u^2+\Delta y}\right] a_1,$$

or

$$\begin{aligned}
a_1^* &= \frac{2u\lambda\Delta y + \frac{2u\lambda(\Delta y)^2}{4u^2+\Delta y}}{4u^2 + \lambda^2\Delta y - \frac{\lambda(\Delta y)^2}{4u^2+\Delta y}} \\
&= \frac{2u\lambda\Delta y(4u^2+\Delta y) + 2u\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2} \\
&= \frac{8u^3\lambda\Delta y + 4u\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2}.
\end{aligned}$$

Substituting in the best response of worker 2 I obtain

$$\begin{aligned}
a_2^* &= \frac{2u\Delta y}{4u^2+\Delta y} + \frac{\Delta y}{4u^2+\Delta y}\frac{8u^3\lambda\Delta y + 4u\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2} \\
&= \frac{2u\Delta y}{4u^2+\Delta y}\left[1 + \frac{4u^2\lambda\Delta y + 2\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2}\right] \\
&= \frac{2u\Delta y}{4u^2+\Delta y}\left[\frac{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2 + 4u^2\lambda\Delta y + 2\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2}\right] \\
&= \frac{2u\Delta y}{4u^2+\Delta y}\left[\frac{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) + 4u^2\lambda\Delta y + \lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2}\right] \\
&= \frac{2u\Delta y}{4u^2+\Delta y}\left[\frac{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) + \lambda\Delta y(4u^2+\Delta y)}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2}\right] \\
&= \frac{2u\Delta y(4u^2+\lambda^2\Delta y+\lambda\Delta y)}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2} \\
&= \frac{8u^3\Delta y + 2u(\lambda^2+\lambda)(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2}.
\end{aligned}$$

I check if this solution satisfies $a_2^* > a_1^*$ or equivalently

$$\frac{8u^3\Delta y + 2u(\lambda^2+\lambda)(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2} > \frac{8u^3\lambda\Delta y + 4u\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2},$$

or

$$8u^3\Delta y + 2u(\lambda^2+\lambda)(\Delta y)^2 > 8u^3\lambda\Delta y + 4u\lambda(\Delta y)^2,$$

or

$$(\lambda^2-\lambda)(\Delta y)^2 > 4u^2(\lambda-1)\Delta y,$$

or

$$\lambda\Delta y > 4u^2.$$

If this inequality holds, then $a_2^* > a_1^*$. I also check if this solution satisfies $\lambda a_1^* > a_2^*$. This inequality is equivalent to

$$\lambda\frac{8u^3\lambda\Delta y + 4u\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2} > \frac{8u^3\Delta y + 2u(\lambda^2+\lambda)(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2},$$

or

$$8u^3\lambda^2\Delta y + 4u\lambda^2(\Delta y)^2 > 8u^3\Delta y + 2u(\lambda^2+\lambda)(\Delta y)^2,$$

or

$$4u^2\lambda^2 + 2\lambda^2\Delta y > 4u^2 + (\lambda^2+\lambda)\Delta y,$$

or

$$4u^2(\lambda^2-1) > \lambda(-\lambda+1)\Delta y,$$

which is true since $\lambda > 1$. Hence this solution is feasible provided that $\lambda\Delta y > 4u^2$.

In the small lambda case the relevant FOC of worker 1 is

$$\lambda\left(\frac{1}{2u} + \frac{\lambda a_1 - a_2}{4u^2}\right)\Delta y = a_1$$

The SOC of worker 1 is satisfied as long as

$$\lambda^2 < \frac{4u^2}{\Delta y}.$$

Solving the FOC of worker 2 for a_2

$$a_2 = \frac{2u\Delta y}{4u^2+\Delta y} + \frac{\Delta y}{4u^2+\Delta y}a_1.$$

Substituting this expression in the FOC of worker 1

$$\lambda\left(2u+\lambda a_1-\frac{2u\Delta y}{4u^2+\Delta y}-\frac{\Delta y}{4u^2+\Delta y}a_1\right)\Delta y=4u^2a_1,$$

or

$$2u\lambda\Delta y+\lambda^2a_1\Delta y-\frac{2u\lambda(\Delta y)^2}{4u^2+\Delta y}-\frac{\lambda(\Delta y)^2}{4u^2+\Delta y}a_1=4u^2a_1,$$

or

$$\frac{8u^3\lambda\Delta y}{4u^2+\Delta y}=\left[4u^2-\lambda^2\Delta y+\frac{\lambda(\Delta y)^2}{4u^2+\Delta y}\right]a_1,$$

or

$$a_1^*=\frac{\frac{8u^3\lambda\Delta y}{4u^2+\Delta y}}{4u^2-\lambda^2\Delta y+\frac{\lambda(\Delta y)^2}{4u^2+\Delta y}}=\frac{8u^3\lambda\Delta y}{(4u^2-\lambda^2\Delta y)(4u^2+\Delta y)+\lambda(\Delta y)^2}.$$

Substituting in to the best response of worker 2 I obtain

$$\begin{aligned}
a_2^* &= \frac{2u\Delta y}{4u^2+\Delta y}+\frac{\Delta y}{4u^2+\Delta y}\frac{8u^3\lambda\Delta y}{(4u^2-\lambda^2\Delta y)(4u^2+\Delta y)+\lambda(\Delta y)^2}\\
&= \frac{2u\Delta y}{4u^2+\Delta y}\left[1+\frac{4u^2\lambda\Delta y}{(4u^2-\lambda^2\Delta y)(4u^2+\Delta y)+\lambda(\Delta y)^2}\right]\\
&= \frac{2u\Delta y}{4u^2+\Delta y}\frac{(4u^2-\lambda^2\Delta y)(4u^2+\Delta y)+\lambda(\Delta y)^2+4u^2\lambda\Delta y}{(4u^2-\lambda^2\Delta y)(4u^2+\Delta y)+\lambda(\Delta y)^2}\\
&= \frac{2u\Delta y}{4u^2+\Delta y}\frac{(4u^2-\lambda^2\Delta y)(4u^2+\Delta y)+\lambda\Delta y(4u^2+\Delta y)}{(4u^2-\lambda^2\Delta y)(4u^2+\Delta y)+\lambda(\Delta y)^2}\\
&= \frac{2u\Delta y(4u^2-\lambda^2\Delta y+\lambda\Delta y)}{(4u^2-\lambda^2\Delta y)(4u^2+\Delta y)+\lambda(\Delta y)^2}\\
&= \frac{8u^3\Delta y-(\lambda-1)2u\lambda(\Delta y)^2}{(4u^2-\lambda^2\Delta y)(4u^2+\Delta y)+\lambda(\Delta y)^2}
\end{aligned}$$

It is clear that for this solution it is not the case that $a_2^*>a_1^*$. This solution is not feasible.

From the above it follows that the Nash Equilibrium effort levels when $\lambda\Delta y>4u^2$ are given by

$$\begin{aligned}
a_1^* &= \frac{8u^3\lambda\Delta y+4u\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2}\\
a_2^* &= \frac{8u^3\Delta y+2u(\lambda^2+\lambda)(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2}
\end{aligned}$$

What is the solution in the symmetric tournament ($\lambda = 1$)?

$$\begin{aligned}
a_1^* &= a_2^* = \frac{8u^3\Delta y + 4u(\Delta y)^2}{(4u^2+\Delta y)^2 - (\Delta y)^2} = \frac{8u^3\Delta y + 4u(\Delta y)^2}{16u^4 + 8u^2\Delta y + (\Delta y)^2 - (\Delta y)^2} \\
&= \frac{8u^3 + 4u\Delta y}{16u^4 + 8u^2\Delta y}\Delta y = \frac{2u^2+\Delta y}{4u^3 + 2u\Delta y}\Delta y = \frac{\Delta y}{2u}.
\end{aligned}$$

I now show that worker 2 exerts less effort in the asymmetric tournament:

$$\frac{8u^3\Delta y + 2u(\lambda^2+\lambda)(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2} < \frac{\Delta y}{2u},$$

or

$$[8u^3 + 2u(\lambda^2+\lambda)\Delta y]2u < (4u^2+\lambda^2\Delta y)(4u^2+\Delta y) - \lambda(\Delta y)^2,$$

or

$$16u^4 + 4u^2(\lambda^2+\lambda)\Delta y < 16u^4 + 4u^2\lambda^2\Delta y + 4u^2\Delta y + \lambda^2(\Delta y)^2 - \lambda(\Delta y)^2,$$

or

$$4u^2\lambda\Delta y < 4u^2\Delta y + \lambda^2(\Delta y)^2 - \lambda(\Delta y)^2,$$

or

$$4u^2(\lambda - 1) < (\lambda^2 - \lambda)\Delta y,$$

or

$$4u^2 < \lambda\Delta y,$$

which is true.

C. Actual Probabilities of Winning the Tournament at the Nash Equilibrium

In this section I prove that when overconfidence is small, the actual probability of winning the tournament for worker 1 increases. When overconfidence is large the actual probability of winning the tournament for worker 1 decreases. To do so, I will compare the actual probability of winning the tournament for workers 1 and 2 in the small and large lambda case. Note that in the case of a symmetric equilibrium, the actual probability of winning for workers 1 and 2 is identical and equal to $1/2$, given that both workers exert the same level of effort a_R^*.

Small Lambda

When overconfidence (lambda) is small, the actual probability of winning the tournament for worker 1 is higher than the probability of winning the tournament for worker 2 if:

$$\begin{aligned}
\frac{1}{2}+\frac{a_1-a_2}{2u}-\frac{(a_1-a_2)^2}{8u^2} &> 1-\left[\frac{1}{2}+\frac{a_1-a_2}{2u}-\frac{(a_1-a_2)^2}{8u^2}\right] \\
\frac{1}{2}+\frac{a_1-a_2}{2u}-\frac{(a_1-a_2)^2}{8u^2} &> \frac{1}{2}-\frac{a_1-a_2}{2u}+\frac{(a_1-a_2)^2}{8u^2} \\
\frac{a_1-a_2}{2u}-\frac{(a_1-a_2)^2}{8u^2} &> -\frac{a_1-a_2}{2u}+\frac{(a_1-a_2)^2}{8u^2} \\
8u(a_1-a_2)-2(a_1-a_2)^2 &> 0 \\
4u(a_1-a_2)-(a_1-a_2)^2 &> 0 \\
(a_1-a_2)*(4u-a_1+a_2) &> 0
\end{aligned}$$

At the Nash Equilibrium, $a_1^* > a_2^*$ thus $(a_1^* - a_2^*)$ is positive. To make sure that the actual probability of winning of worker 1 is higher than the actual probability of winning for worker 2, I verify that $(4u - a_1^* + a_2^*) > 0$ or

$$\begin{aligned}
4u-\frac{8u^3\lambda\Delta y}{(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2}+\frac{8u^3\Delta y+2u(\lambda-1)\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2} &> 0 \\
4u\left[(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2\right]-8u^3\lambda\Delta y+8u^3\Delta y+2u(\lambda-1)\lambda(\Delta y)^2 &> 0 \\
4u\left[16u^4-4u^2\Delta y+4u^2\lambda^2\Delta y-\lambda^2\Delta y^2+\lambda\Delta y^2\right]-8u^3\lambda\Delta y+8u^3\Delta y+2u(\lambda-1)\lambda(\Delta y)^2 &> 0 \\
16u^4-4u^2\Delta y+4u^2\lambda^2\Delta y-\lambda^2\Delta y^2+\lambda\Delta y^2-2u^2\lambda\Delta y+2u^2\Delta y+\frac{(\lambda-1)\lambda(\Delta y)^2}{2} &> 0 \\
16u^4+4u^2\Delta y(\lambda^2-1)+\lambda\Delta y^2(1-\lambda)+2u^2\Delta y(1-\lambda)+\frac{(\lambda-1)\lambda(\Delta y)^2}{2} &> 0 \\
\frac{16u^4}{\lambda-1}+4u^2\Delta y(\lambda+1)-\lambda\Delta y^2-2u^2\Delta y+\frac{\lambda(\Delta y)^2}{2} &> 0 \\
\frac{16u^4}{\lambda-1}+2u^2\Delta y+4u^2\lambda\Delta y-\frac{\lambda(\Delta y)^2}{2} &> 0 \\
\frac{16u^4}{\lambda-1}+2u^2\Delta y+\lambda\Delta y\left(4u^2-\frac{\Delta y}{2}\right) &> 0
\end{aligned}$$

From the SOC of worker 2 $4u^2 > \Delta y$ hence this expression is positive. I can conclude that when lambda is small, the actual probability of winning the tournament for worker 1 is higher than the actual probability of winning for worker 2.

Large Lambda

When overconfidence (lambda) is large, the actual probability of winning the tournament for worker 1 is lower than the probability of winning the tournament for worker 2 if:

$$
\begin{aligned}
\frac{1}{2}+\frac{a_1-a_2}{2u}+\frac{(a_1-a_2)^2}{8u^2} &< 1-\left[\frac{1}{2}+\frac{a_1-a_2}{2u}+\frac{(a_1-a_2)^2}{8u^2}\right] \\
\frac{1}{2}+\frac{a_1+a_2}{2u}-\frac{(a_1-a_2)^2}{8u^2} &< \frac{1}{2}-\frac{a_1-a_2}{2u}-\frac{(a_1-a_2)^2}{8u^2} \\
8u(a_1-a_2)+2(a_1-a_2)^2 &< 0 \\
4u(a_1-a_2)+(a_1-a_2)^2 &< 0 \\
(a_1-a_2)*(4u+a_1-a_2) &< 0
\end{aligned}
$$

At the Nash Equilibrium, $a_1^* < a_2^*$ thus $(a_1^* - a_2^*)$ is negative. To make sure that the actual probability of winning of worker 1 is lower than the actual probability of winning for worker 2, I verify that $(4u + a_1^* - a_2^*) > 0$ or

$$
\begin{aligned}
4u+\frac{8u^3\lambda\Delta y+4u\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2}-\frac{8u^3\Delta y+2u(\lambda^2+\lambda)(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2} &> 0 \\
4u\left[(4u^2+\lambda^2\Delta y)(4u^2+\Delta y)-\lambda(\Delta y)^2\right]+8u^3\lambda\Delta y+4u\lambda(\Delta y)^2-8u^3\Delta y-2u(\lambda^2+\lambda)(\Delta y)^2 &> 0 \\
4u\left[16u^4+4u^2\Delta y+4u^2\lambda^2\Delta y+\lambda^2\Delta y^2-\lambda\Delta y^2\right]+8u^3\lambda\Delta y+4u\lambda(\Delta y)^2-8u^3\Delta y-2u(\lambda^2+\lambda)(\Delta y)^2 &> 0 \\
16u^4+4u^2\Delta y+4u^2\lambda^2\Delta y+\lambda^2\Delta y^2-\lambda\Delta y^2+2u^2\lambda\Delta y+\lambda(\Delta y)^2-2u^2\Delta y-\frac{(\lambda^2+\lambda)(\Delta y)^2}{2} &> 0 \\
16u^4+4u^2\Delta y+4u^2\lambda^2\Delta y+\lambda^2\Delta y^2+2u^2\lambda\Delta y-2u^2\Delta y-\frac{(\lambda^2+\lambda)(\Delta y)^2}{2} &> 0 \\
16u^4+2u^2\Delta y(1+\lambda)+4u^2\lambda^2\Delta y+\frac{\lambda^2\Delta y^2}{2}-\frac{\lambda\Delta y^2}{2} &> 0 \\
16u^4+2u^2\Delta y(1+\lambda)+4u^2\lambda^2\Delta y+\frac{\lambda(\lambda-1)\Delta y^2}{2} &> 0
\end{aligned}
$$

Given that $\lambda > 1$, I can conclude that this expression is positive. Hence, when lambda is large, the actual probability of winning the tournament for worker 1 is lower than the actual probability of winning for worker 2.

D. Mathematical Expressions for Figures (2.1), (2.2) and (2.3)

This section reports the mathematical expressions for the graphical representation of the symmetric and asymmetric (small and large lambda case) equilibrium in Figures (2.1), (2.2) and (2.3).

To represent the symmetric equilibrium, in Figure (2.1), I use the following set of parameters: $\lambda = 1$, $u = 2$, $\Delta y = 6$; the best reply function of worker 1 (if worker 1 was rational) is

$$a_2 = \begin{cases} -4 + 3.66a_1, & \text{if } a_1 > a_2 \\ 1.5, & \text{if } a_1 = a_2 \\ 4 - 1.66a_1, & \text{if } a_1 < a_2 \end{cases}$$

and the best reply function for worker 2 is

$$a_2 = \begin{cases} 2.4 - 0.6a_1, & \text{if } a_1 > a_2 \\ 1.5, & \text{if } a_1 = a_2 \\ 1.0909 + 0.2727a_1, & \text{if } a_1 < a_2 \end{cases}$$

In this case $a_1^* = a_2^* = a_R = 1.5$. Note that the best reply function of worker 1 (if worker 1 was rational) and the best reply function of worker 2 are the same in all figures.

To represent the small lambda case, in Figure (2.2), I use the following set of parameters: $\lambda = 2$, $u = 2$, $\Delta y = 6$. The best reply function of worker 1 is

$$a_2 = \begin{cases} -4 + 3.333a_1, & \text{if } \lambda a_1 > a_2 \\ 6, & \text{if } \lambda a_1 = a_2 \\ 4 + 0.67a_1, & \text{if } \lambda a_1 < a_2 \end{cases}$$

In this case $a_1^* = 1.63$, $a_2^* = 1.42$ and $a_R = 1.5$.

To represent the large lambda case, in Figure (2.3), I use the following set of parameters: $\lambda = 8$, $u = 2$, $\Delta y = 6$; the best reply function of worker 1 is

$$a_2 = \begin{cases} -4 + 8.33a_1, & \text{if } \lambda a_1 > a_2 \\ 216, & \text{if } \lambda a_1 = a_2 \\ 4 + 7.66a_1, & \text{if } \lambda a_1 < a_2 \end{cases}$$

In this case $a_1^* = 0.63$, $a_2^* = 1.26$ and $a_R = 1.5$ or $a_R > a_2^* > a_1^*$.

E. Proof of Proposition 2

To disentangle the effect of the overconfidence worker on the firm's problem for a given prize spread, first note that the expected total output in the symmetric equilibrium is simply $E[Q_1(a_1)] + E[Q_2(a_2)] = 2 * a_R = 2\frac{\Delta y}{2u} = \frac{\Delta y}{u}$.

Small Lambda

When overconfidence (lambda) is small, the expected total output is equal to $E[Q_1(a_1) + E[Q_2(a_2)] = a_1^* + a_2^*$ or, using the Nash Equilibrium expressions:

$$\frac{8u^3\lambda\Delta y}{(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2} + \frac{8u^3\Delta y + 2u(\lambda-1)\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2}$$

The expected total output when lambda is small is greater than the expected total output in the symmetric equilibrium when

$$\frac{8u^3\lambda\Delta y}{(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2} + \frac{8u^3\Delta y + 2u(\lambda-1)\lambda(\Delta y)^2}{(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2} > \frac{\Delta y}{u}$$

or

$$\begin{aligned}
8u^4\lambda\Delta y + 8u^4\Delta y + 2u^2(\lambda-1)\lambda(\Delta y)^2 &> \Delta y[(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2] \\
8u^4\lambda + 8u^4 + 2u^2(\lambda-1)\lambda\Delta y &> [(4u^2+\lambda^2\Delta y)(4u^2-\Delta y)+\lambda(\Delta y)^2] \\
8u^4\lambda + 8u^4 + 2u^2(\lambda-1)\lambda\Delta y &> 16u^4 - 4u^2\Delta y + 4u^2\lambda^2\Delta y - \lambda^2\Delta y^2 + \lambda(\Delta y)^2 \\
8u^4(\lambda+1-2) + 2u^2(\lambda-1)\lambda\Delta y &> 4u^2\Delta y(\lambda^2-1) + \lambda\Delta y^2(1-\lambda) \\
8u^4(\lambda-1) + 2u^2(\lambda-1)\lambda\Delta y &> 4u^2\Delta y(\lambda-1)(\lambda+1) - \lambda\Delta y^2(\lambda-1) \\
8u^4 + 2u^2\lambda\Delta y &> 4u^2\Delta y(\lambda+1) - \lambda\Delta y^2 \\
8u^4 + 2u^2\lambda\Delta y - 4u^2\lambda\Delta y - 4u^2\Delta y + \lambda\Delta y^2 &> 0 \\
8u^4 - 2u^2\lambda\Delta y - 4u^2\Delta y + \lambda\Delta y^2 &> 0 \\
(2u^2-\Delta y)(4u^2-\Delta y\lambda) &> 0
\end{aligned}$$

With small lambda $\Delta y\lambda < 4u^2$ so $4u^2 - \Delta y\lambda > 0$. Thus when $2u^2 - \Delta y$ is positive, or when $2u^2 > \Delta y$ the expected total output for the firm is higher in the asymmetric equilibrium compared to the symmetric equilibrium. When $2u^2 < \Delta y$ the expected total output for the firm is lower in the asymmetric equilibrium compared to the symmetric equilibrium. Finally for $2u^2 = \Delta y$ the output is the same in both cases. Hence the firm is better off when the degree of overconfidence of worker 1 is small and $2u^2 > \Delta y$ given that, for a fixed prize spread, the expected total output (and thus the expected profit) is higher.

Large Lambda

When overconfidence (lambda) is large, worker 1 and worker 2 always provide less effort than the effort provided in the symmetric equilibrium. This implies that the firm is worse off since, for any prize spread, the expected total output (profit) is lower in the asymmetric equilibrium than in the symmetric equilibrium.

Chapter 3

Are People Conditional Liars?

3.1 Introduction

The observation of peers' behavior in moral dilemmas can have an important impact on individual choices. The 2015 so-called Diesel gate, consisting on the violation of the Clear Air Act by the Volkswagen group, brought up awareness that diesel-powered cars produced not only by the Volkswagen group but also from other producers, may escape legal emission constraints. This suggests that being exposed to lying, misconduct and wrongdoing by colleagues or team members can contribute to the spreading of dishonesty within firms and across markets. Recent studies in Economics and Psychology have focused on the reasons which lead people to lie.

Lying behavior has been studied extensively using the die-roll paradigm introduced by Fischbacher and Föllmi-Heusi (2013). In their design, subjects privately observe the outcome of a die-roll and are asked to make a report that will determine individual monetary payoffs. Fischbacher and Föllmi-Heusi (2013), as well as Gächter and Schulz (2016) and Gneezy et al. (2018) find that people lie but often the do not lie maximally. Perfectly rational payoff maximizer individuals should not refrain from lying to the maximum extent. Hence, the existence of partial lying (i.e. lying without making the payoff maximizing report) suggests that lying gives rise to moral or psychological costs. Lying may also generate social image lying costs: Abeler et al. (2019) show that people lie less if the true state is observable (i.g. by the experimenter) compared to when it is not. They also investigate whether beliefs on others behavior affect lying and find no effect. On the contrary, Abeler et al. (2014) find that lying increases when people expect others to lie while

Rauhut (2013) shows that learning about lying increases dishonesty when people underestimate the extent of lying and decreases when they overestimate it.

Lying behavior has also been investigated under different types of strategic interactions, such as deception games and coordination games. In deception games, subjects strategically choose to lie taking into consideration not only their own gains but also the possibility to favor or damage others with their choice (Gneezy (2005), Erat and Gneezy (2012), Gneezy et al. (2013)). In coordination games, people strategically lie more if the monetary benefits of lying are equally shared with others i.e. there is payoff commonality and reports happen simultaneously (Barr and Michailidou (2017)) or sequentially (Weisel and Shalvi (2015)). In coordination games where the benefits of lying are not equally shared, Lauer and Untertrifaller (2019) find that one third of the subjects engage in strategic lying if and only if one or more group members lie. Also, Kocher et al. (2016) find that communication increases lying regardless of payoff commonality.

Finally, Diekmann et al. (2015) study the impact of observing lying in absence of strategic interactions by asking participants to report twice, once without information and a second time after observing the distribution of reports in the first round of the experiment or the distribution of reports from a similar Fischbacher and Föllmi-Heusi (2013) die-rolling experiment. They show that observing the distribution of reports, both in the first round or in a similar experiment, increases lying.

Related to lying behavior and dishonesty, a rich literature studies conditional social norm violation i.e the fact that subjects are more likely to break a rule when they observe others' misbehavior. Keizer et al. (2008) find empirical evidence for the fact that being aware of others misconduct triggers more misconduct (for instance, observing illegal parking causes illegal trespassing) while Cialdini et al. (1990) show that people are more prone to litter when they observe others littering. Finally, Gino et al. (2009) find that people are more likely to misbehave when they observe an in-group breaking a rule but are more likely to adhere to such a rule when it is an out-group that breaks the rule.

We are interested in understanding what determines lying behavior in absence of strategic lying confounds, communication and when group members can precisely observe each others lies. Such a framework allows us to isolate social image cost of lying from other lying motives. In particular, we investigate whether the presence of another group member who can also lie and/or whether observing a group member lying reduces social image costs of lying. We expect the reduction in social image costs of lying to induce, in turn, an increase in lying behavior. We also want to know if subjects who expect others to lie lie more than subjects who expect truth telling and whether being surprised by lying behavior reduces social image costs of lying and thus increases lying.

We use a laboratory experiment to answer these questions. We implement a variation of the die-roll paradigm in Fischbacher and Föllmi-Heusi (2013) in which subjects are randomly matched into pairs and each pair observes a single die-roll outcome. Subjects are then asked to make a report that will determine individual monetary payoffs. To minimize inequality aversion and social comparisons confounds, the two subjects in each pair observe the same die-roll outcome. We use a between-subjects design composed by three treatments, baseline, simultaneous and sequential. In the baseline treatment, one subject makes a report while the other subject is a passive observer. In the simultaneous treatment, subjects report simultaneously. In the sequential treatment, subjects report in a sequential order. Comparing the baseline to the simultaneous treatment allows us to investigate whether lying differs when there is a passive or an active partner who can also lie. Comparing the simultaneous to the sequential treatment allows us to investigate whether observing someone lying increases lying.

In order to rule out differences in social image costs of lying across treatments, our design makes the die-roll outcome and individual reports observable to the experimenter and to the partner in all treatments. In particular, while observability by the experimenter is constant across treatment, we introduce a passive observer who does not engage into reporting in the baseline treatment to avoid a social image cost confound among active reporters in the baseline treatment and subjects in the simultaneous and sequential treatment.

We measure lying as the choice to lie or not to lie (i.e. lying in the extensive margin) and as the size of the lie of subjects who lie (i.e. lying in the intensive margin). In addition to studying how being exposed to dishonesty affects lying, we investigate whether subjects anticipate dishonesty and how subjects' beliefs influence lying in the extensive and intensive margins. For this purpose, we elicit subjects beliefs regarding their partner's report.

Our main results are as follows. Lying in the baseline does not differ from lying in the simultaneous treatment, neither in the extensive nor in the intensive margins. In contrast, lying in the extensive margin is higher in the sequential treatment than in the simultaneous treatment. This is driven by second movers lying more often than subjects in the simultaneous treatment and suggests that second movers are conditional liars. We also find that observing lying affects lying in the intensive margin but not in the extensive margin. In other words, larger lies by first movers result in larger lies by second movers whereas the extensive margin of observed behavior (to lie or not to lie) is not predictive for the extensive margin.

Belief elicitation also allows us to show that subjects overestimate lying in the extensive margin. Similarly to lying behavior, beliefs of subjects in the baseline and simultaneous treatment, as well as beliefs of first movers, do not differ. Second movers, instead, are the ones who expect their partner to be the most likely to lie. Finally, we find that second movers shift their behavior towards lying when they expect first movers to tell the truth and observe first movers lying instead. However, second movers do not shift their behavior towards truth telling when they expect first movers to lie and observe first movers telling the truth instead.

We contribute to the literature on lying behavior. Researchers have shown that lying increases when there is payoff commonality (Barr and Michailidou (2017), Weisel and Shalvi (2015)) and when subjects can communicate (Kocher et al. (2016)). Moreover, Lauer and Untertrifaller (2019) find evidence for a category of subjects who lie when one or more group members lie. These studies include strategic interactions among subjects and/or the possibility to communicate. We find that observing lying increases lying even in absence of strategic interactions and communication.

The study closest to our is Diekmann et al. (2015). They show that subjects are more likely to lie when they are aware that others lied, in absence of strategic interactions. In their study, subjects are asked to make two reports. After making the first report, they are informed of the distribution of reports in the first round or in a similar Fischbacher and Föllmi-Heusi (2013) die-rolling experiment. Next, they proceed with the second report. Thus, in Diekmann et al. (2015), subjects can estimate the extent to which people lie but do not have information about individual behavior. In our design instead, not only we are able to exclude strategic interactions but also second movers know precisely whether and to which extent the first movers they are paired with lied. In this context, we find that the propensity to lie is higher when reports happen sequentially due to the high percentage of second movers lying and that the size of the lie increases when second movers observe first movers' lies. This implies that observing group members' lies diminishes the social image costs associated with lying and thus increases lying.

More generally, we contribute to the rich literature on conditional social norm violation i.e the fact that subjects are more likely to break a rule when they observe others' misbehavior. Empirical evidence shows that when being aware of others misconduct increases misconduct (Keizer et al.

(2008)). For instance, Cialdini et al. (1990) show that people are more prone to litter when they observe others littering and Carrell et al. (2008) find evidence for the contagiousness of cheating in academic environment. Gino et al. (2009) find that people are more likely to cheat when they observe an in-group cheating yet are reluctant to cheat if it is an out-group who cheats. In their design, subjects are asked to perform a real effort task. During the experiment, an in-group or an out-group would leave the room ostensibly early given the difficulty of the task, making it clear that misreporting own performance in the task was a possibility. Differently from Gino et al. (2009), in our design subjects do not perform a real effort task but perform a die-rolling task. We believe that misreporting one's performance in a real effort task may trigger different dynamics than misreporting the outcome of a die-roll.[1] Moreover, while in Gino et al. (2009) it is clear that one participant to the same experimental session lied, in our design subjects are informed about their each others behavior, whether they lied or not and to which extent. This is particularly relevant in the sequential treatment in which second movers observe first movers' reports and then proceed with reporting. In this context, we find that observing lying increases lying. Finally, Dimant (2019) shows that anti-social behavior is more contagious than pro-social behavior in a give-or-take donation game. In their experiment, participants can give money to a charity or keep it for themselves. Similarly, Brunner and Ostermaier (2019) show that peer influence on managerial behavior is more remarkable when managers over-report their expenses to obtain higher budgets compared to honest reporting. In our study, we exclude inequality aversion or altruism confounds and implement a more straight forward task (reporting the outcome of a die-roll). We show that lying is more contagious than truth telling: when subjects expect truth telling but observe lying, lying increases. However, when subjects expect lying but observe truth telling, lying does not decrease.

The reminder of the paper is organized as follows. Section 2 introduces our experimental design; section 3 presents the hypotheses we will test; section 4 discusses our results and section 5 concludes.

[1] Biases such as overconfidence or optimism may induce subjects to make biased reports about their own performance in a real effort task.

3.2 Experimental Design

We implement three treatments: baseline, simultaneous and sequential. In the baseline treatment, subjects are paired and are assigned the role of player A or B. Player Bs are passive observers i.e. they are not engaged in reporting. Although both Player A and Player B in each pair observe the single die-roll outcome, only Player A makes a report and his/her report will determine his/her payoff. Once Player A reports, Player B is asked to make a guess regarding A's report and next will observe Player A's report.

Player B's payoff is determined as follows. Before subjects are paired and learn about their tasks, we describe them the simultaneous treatment (in non-indicative language) which we call "Experiment 1" for convenience. We explain that some students, in the same laboratory as themselves, had already participated in a pilot ("Experiment 1") and that we have kept their reports. Then, we explain that Player B's payoff would be determined by picking one of the reports of the participants in "Experiment 1" who had observed the same die-roll outcome as the one observed by them and Player A in the current session. For example, if a pair in the baseline observes a 3, Player A receives a payoff equal to his/her report, and Player B receives a payoff randomly drawn among the reports of Player Bs who participated in "Experiment 1" and had observed a die-roll of 3. [2]

At the end of the experiment, Player As are asked to guess the randomly drawn report for Player B of "Experiment 1".

In the simultaneous treatment, subjects in a pair observe the outcome of a die-roll and are asked to make a report simultaneously. After subjects make their own reports, they are asked to guess each others report. After subjects have entered their guesses, they receive information about each others report.

In the sequential treatment, all conditions are identical to the simultaneous treatment except the

[2] The computer randomly draws from the data base one of the reports of the subjects in "Experiment 1" who observed the same die-roll as player B observes during the current session. Data were collected during a pilot session of the simultaneous treatment that is "Experiment 1". We do not inform subjects about each report draw probability for player B to avoid any confounds (such as hinting that subjects in "Experiment 1" lied). Moreover, the choice to assign a random draw from the distribution of reports in another session has the objective to render treatments comparable. Indeed, in the sequential and simultaneous treatments subjects' partners may lie. In the baseline, active player As make their choice knowing that passive player Bs may be assigned the report of someone who lied. If this was not the case, confounds may arise (e.g. inequality aversion).

sequence of reporting and the disclosure of players' reports to each other. In particular, after the pair observes the common die-roll, the first mover (Player A) goes first to report and then to guess the second mover (Player B)'s report while the second mover is waiting. Then, the second mover is asked to guess the first mover's report. Afterwards, the second mover learns the first mover's report and is asked to proceed with his/her own report. Then, the second mover's report is revealed to the first mover.

To investigate whether lying with a passive partner differs from lying with an active one, we compare subjects' behavior among the baseline treatment and the simultaneous treatment. To investigate whether reporting simultaneously or sequentially has an impact on lying, we compare the simultaneous treatment with the sequential treatment. Moreover, comparing the behavior of first and second movers in the sequential treatment allows us to investigate whether observing lying increases lying.

Our design builds on the standard die-rolling paradigm by Fischbacher and Föllmi-Heusi (2013). In Fischbacher and Föllmi-Heusi (2013), subjects privately observe the outcome of a random variable (a six sided die) and are asked to report the outcome they observed. Monetary payoffs are proportional to individual reports, independently from the die-roll outcome observed. More precisely, in their design, monetary payoff are equal to reports for any report from 1 to 5 while reporting a 6 yields a null payoff.

We differ from Fischbacher and Föllmi-Heusi (2013) in three respects. First, in our variant of the die-roll paradigm subjects are randomly matched into pairs. This allows us to study what determines lying behavior when individuals are in groups and lying is observable by group members. In order to minimize inequality aversion and social comparisons concerns, each subject in a pair observes the same die-roll outcome i.e. $x_i = x_j = x \in \{1, 2, 3, 4, 5, 6\}$ for a randomly matched pair (i, j). We inform subjects that the other person in their pair observes the same realization as they do. Each die realization is equally likely, i.e. $p_x = 1/6$, and this is common knowledge.

Second, individual monetary payoffs are equal to subjects' reports, for any report from 1 to 6: CHF 1 if $r = 1$, CHF 2 if $r = 2$ and so on. Importantly, each subject's payoff is independent from their partner's report and subjects cannot communicate. In this way we are able to study lying behavior in absence of strategic interactions (such as payoff commonality Barr and Michailidou (2017), Weisel and Shalvi (2015)) and communication (Kocher et al. (2016)).

Third, we implement random draws on computers and thus are able to recover the true state.

Thus the experimenter has full observability and we can measure lying at the individual level. It is is common knowledge that the experimenter can observe the die realization. Abeler et al. (2019) show that the possibility of being detected, for instance by the experimenter, reduces lying while Gneezy et al. (2018) suggest that verifiability of outcomes influences lying in the intensive margin (maximal lying increases and partial lying decreases). Importantly, in our design, observability from the experimenter is a common feature across all the experimental conditions hence we do not think it compromises in any way our treatment comparisons. However, in the simultaneous and sequential treatments subjects in a pair observe report. Being observed by group members may give rise to a different type of social image effects, or make the existing social image effects more salient. To control for social image effects across treatments, we introduced the presence of a passive observer in the baseline treatment.

A full anonymity protocol is applied in all treatments. When subjects enter the lab they are seated in individual, panel divided experimental booths. They individually read instructions and these are common knowledge. For clarity, Table 3.1 reports the timing of the experimental design. Experimental instructions are provided in Appendix C while screen shots and documents relative to experimental procedures are reported in Appendix C.4 . Section 3.2.1 describes belief elicitation.

3.2.1 Belief elicitation

We elicit beliefs about partners' reports to assess whether subjects anticipate dishonest behavior and whether this affects lying.

In the baseline, Player B is asked to guess Player A's report while Player A is asked to make a guess on the report that will be randomly drawn for Player B. In the simultaneous and sequential treatments subjects are asked to guess their partner's report before this is revealed. Subjects receive 1 CHF if their guess is correct and zero otherwise.

In the first set of experimental instructions we inform subjects that they will be asked to make a guess but we do not disclose in advance what this guess is about. We tell participants that the content of the guess will become clear during the experiment. We proceed in this manner since subjects may modify their behavior as a consequence of being asked to form expectations and we want to avoid confounds and strategic behavior.[3]

[3]For instance, subjects may try to coordinate knowing that they have to anticipate their partners behavior.

Table 3.1: Timing of the experiment

Instructions		
Random matching of subjects into pairs, role of A and B assignment		
A and B observe the die-roll outcome		
Baseline	**Simultaneous**	**Sequential**
A reports, B guesses A's report	A and B report	A reports, B guesses A's report
B observes A's report, A guesses the randomly drawn report for B	A and B guess each others report	B observes A's report
A and B observe the randomly drawn report for B	A and B observe each others report	B reports, A guesses B's report
		A observes B's report
Demographic Questionnaire		
Payment		

3.3 Hypotheses

In this section we discuss the hypotheses that we will test in our experiment.

The literature on lying has shown that subjects seem to face psychological costs when lying (Fischbacher and Föllmi-Heusi (2013), Gächter and Schulz (2016)). Gneezy et al. (2018) present a theory model in which subjects derive utility from monetary gains but face a cost when lying that also depends on the possibility of being detected. In other words, full observability induces a social image lying cost. The latter increases remarkably in the choice to lie or not while it does not depend on the extent to which a subject lies. Related to Gneezy et al. (2018), Abeler et al. (2019) find evidence for the existence of social image lying costs. When subjects derive utility from having a positive social image, being seen as a liar can generate a cost. We believe that when subjects derive utility from a positive social image, lying costs will not only increase when subjects decide to lie but also to with the extent to which a subject lies.

In our design, subjects are matched into pairs: they face social image costs when lying, not just because of their own behavior, but also depending on their partners'. Social image lying costs increase when the partner tells the truth and decrease when the partner lies. We expect subjects to lie more in the simultaneous treatment compared to the baseline treatment. The reason is as follows. In the baseline treatment, passive observers do not engage in reporting and thus cannot lie. The fact that their partner cannot lie increases the social image cost of lying of active reporters. In the simultaneous treatment instead, both partners can lie: if subjects anticipate that their partner is facing the same choice and social image costs of lying, the fact that each others partner can lie as well reduces social image costs of lying. This in turn implies that lying increases in the simultaneous treatment. One could argue that in the baseline treatment, as well as in the simultaneous treatment, subjects are aware that some participants to the experiment may lie. However, in the simultaneous treatment, partners' opportunity to lie is more salient and thus the impact on social image cost of lying is more remarkable.

Hypothesis 1: *Subjects lie more in the simultaneous treatment compared to the baseline treatment.*

In the simultaneous treatment, partners observe each other' reports after reporting simultaneously. In the sequential treatment, second movers observe first movers' reports before making their reports. We expect subjects to lie more in the sequential treatment compared to the simultaneous treatment for two reasons. First, the literature on conditional social norm violation shows that violations of a social norm trigger more violations. In particular, Diekmann et al. (2015) show that when subjects are asked to report twice and are informed about the distribution of reports in the first round or in a similar Fischbacher and Föllmi-Heusi (2013) die-rolling experiment, being aware that other people lied increases lying in the second round. Hence we expect second movers who observe first movers lying to lie as well. Second, if first movers anticipate that second movers will lie when they observe first movers who lie, first movers' social image cost of lying is reduced and this in turn increases lying. However, we expect the effect to be stronger for second movers compared to first movers, since second movers have the possibility to observe first movers' behavior before making their own report.

Hypothesis 2: *In the sequential treatment,*
(i) subjects lie more compared to the simultaneous treatment,
(ii) due to the fact that second movers lie more than subjects in the simultaneous treatment.

Related to the literature on conditional norm violation, Lauer and Untertrifaller (2019) find evidence for conditional dishonesty i.e. the fact that group members lie if they observe one or more group members lying. In particular, they identify a category of subjects that would not lie unless they are aware of the dishonesty of one or more group members. They call this subjects "conditional liars". Conditional liars are to be distinguished from subjects who would lie regardless of others' behavior ("always liars") since their behavior depends on the honesty or dishonesty of other group members. Moreover, conditional liars are to be distinguished from "never liars" i.e. subjects who never lie regardless of others' behavior. In Lauer and Untertrifaller (2019), individual monetary payoffs depend on the reports of all group members. We rule out this possibility to be able to isolate the effect of being in a group from payoff interdependence and expect second movers to be conditional liars. In other words, we expect second movers in the sequential treatment to lie more when they observe first movers lying.

Hypothesis 3: *In the sequential treatment, second movers who observe a first mover who lies, lie more than second movers who observe a first mover who tells the truth.*

In our experiment, we elicit subjects' beliefs about partners' reports. In this way, we are able to investigate whether subjects expect their partners to lie and to which extent, comparing actual and expected reports. This allows us to observe whether subject overestimate or underestimate lying. Similarly to the results in Abeler et al. (2014), we expect subjects to overestimate lying.

Hypothesis 4: *Subjects overestimate lying.*

Regarding the impact of beliefs on subjects' behavior, the literature finds mixed results. While Abeler et al. (2019) show that beliefs do not have an impact, Abeler et al. (2014) find that subjects who expect other people to lie, lie more and that, overall, subjects overestimate lying. Abeler et al. (2019) and Abeler et al. (2014) elicit subjects' beliefs after they have made their reports. Rauhut (2013) instead, analyses subjects' behavior before and after belief elicitation using an experiment in which subjects engage in a die-rolling task twice. After the first round, subjects are asked to estimate the frequency of reports of other participants; next, they are informed about the actual distribution of reports of other participants before the second round. Rauhut (2013) shows that lying increases when subjects underestimated lying while it decreases when subjects overestimated lying.

In the sequential treatment, second movers are asked to form beliefs about first movers, then they are informed about first movers' report and finally are asked to make their own report. This implies that second movers' beliefs can be confirmed or not when observing the report of first movers. Differently from Rauhut (2013), second movers are not informed about the distribution of reports but they are told exactly which report first movers made. Hence, second movers are aware, without the need to make any inference, whether their partner lied or not and to which extent.

We expect belief updating to be relevant in the case of second movers. Second movers holding different priors about others' lying behavior may suffer different lying costs. Those who anticipate lying but observe truth telling may face a higher cost than those who anticipate lying and observe lying; those who anticipate truth telling but observe lying may face a lower cost than those who anticipate and observe truth telling. As a consequence, we expect to find an effect when second movers are surprised by the behavior of first movers i.e. when they observe the contrary of what they anticipated. Precisely, we expect second movers who expect lying but observe truth telling to lie less than second movers who expect and observe lying. On the contrary, we expect second movers who expect truth telling and observe lying to lie more than second movers who expect and observe truth telling.

Hypothesis 5 *In the sequential treatment,*
(i) second movers who expect lying but observe truth telling lie less than second movers who expect and observe lying;
(ii) second movers who expect truth telling but observe lying lie more than second movers who

expect and observe truth telling.

3.4 Results

3.4.1 Subjects, Procedures and Payment

We conducted all experimental sessions at the LABEX (HEC, University of Lausanne) during November 2017 and September 2018. All subjects were recruited using ORSEE (Greiner (2015)) and the experiment was programmed in zTree (Fischbacher (2007)).

Instructions were presented in oral and in written form, in French language. Subjects' understanding was tested with a series questions prior to the experimental task, and their demographic characteristics were collected with a post-task questionnaire.

Each subject took part in one session only. We had on average 24 participants per session.[4] In total, 404 subjects participated to the experiment: 138 subjects in the baseline, 100 in the simultaneous and 166 in the sequential treatment.

Sessions lasted 30 minutes on average. Each subject received 10 CHF as a show up fee, and total earnings varied between 11 and 17 CHF. The average payment was equal to 15.09 CHF. Payments were carried via payment-sheets which subjects took outside the lab and were cashed by an experimenter who was not present during the experiment and could not identify individuals' experimental behavior. Payment receipts could not be linked to individuals' experimental behavior. Full anonymity applied throughout all sessions.

Finally, to exclude systematic differences in the allocation of subjects across treatments we conduct a demographics randomization check, reported in appendix A.

3.4.2 Lying across treatments

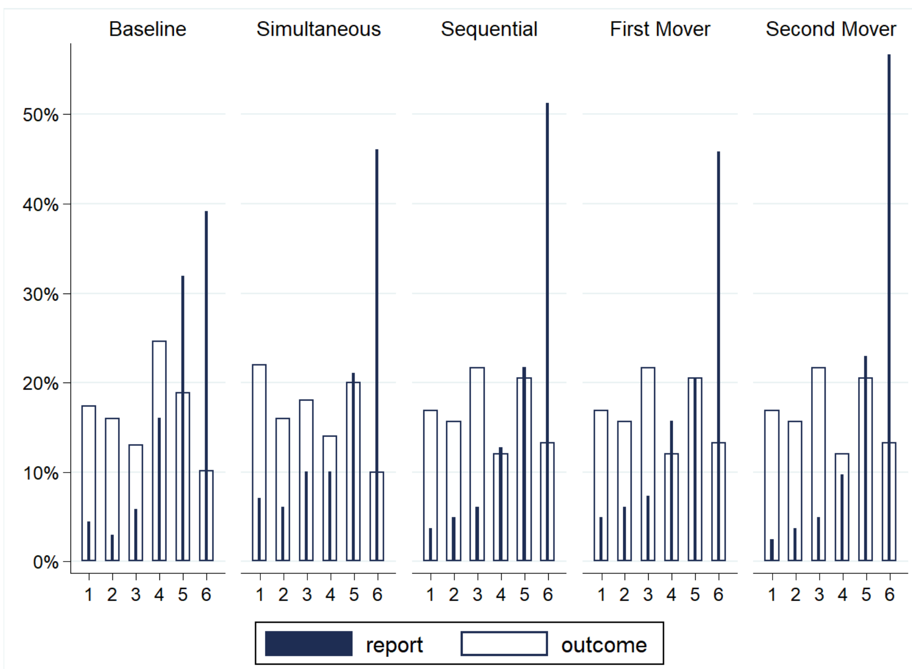
Figure 3.1 displays the distribution of die-roll outcomes and reports across treatments and distinguishing among first and second movers in the sequential treatment. The wider empty bars depict the frequency of occurrence of each outcome, while thin blue bars depict the frequency with which each of the die-roll outcomes was reported. Figure 3.1 shows that the frequency at which a 6 is reported is higher than the frequency at which a 6 is observed. The frequency at which a 5 is reported

[4]The minimum number of subjects per session was 22 and the maximum was 26.

is similar to the frequency at which a 5 is observed. On the contrary, the frequency at which 1, 2 or 3 are reported is lower than the frequency at which 1, 2 or 3 are observed. This indicates that subjects over-report in all treatments.

Figure 3.2 displays subjects' reports given the die-roll outcome observed across treatments and distinguishing among first and second movers in the sequential treatment. In Figure 3.2 each dot represents a subject. All dots that lay on the red line represent subjects who reported truthfully (intuitively, the red line corresponds to the bisector on which reports and die-roll outcomes coincide). All dots that lay above the red line represent subjects that lied i.e. subjects that reported a higher outcome with respect to the observed die-roll outcome.

Figure 3.1: Distribution of die-roll outcomes and subjects' reports



Notes: histograms of the frequency of die-roll outcomes (wider empty bars) and of reports (thin blue bars). Histograms, from left to right, refer to baseline, simultaneous and sequential treatment and to first and second mover in the sequential treatment. In the baseline, Player Bs are passive observers who are not asked to make a report while Player As are active and engage in reporting. Hence, we only consider observations relative to Player As in the Baseline.

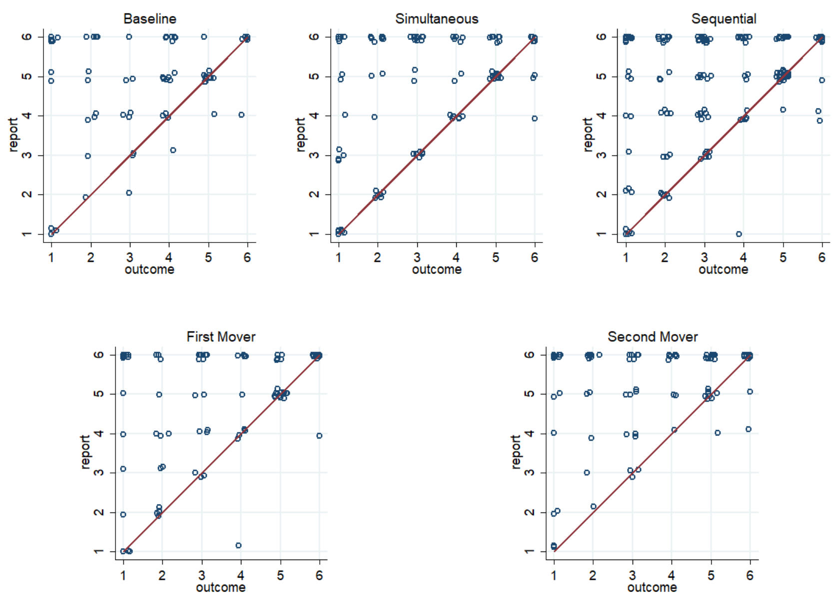
Similarly to Figure 3.1, Figure 3.2 shows that many of subjects' reports are higher than the die-roll outcome observed in all the treatments.

Among all subjects who lied we can distinguish two types of liars: (i) "maximal liars" and (ii) "partial liars". Maximal liars are subjects who lied and reported the payoff maximizing outcome i.e. subjects who reported a 6 and are on the top of each graph in Figure 3.2. Among the subjects who lied, 65% are maximal liars in the baseline, 74% in the simultaneous and 70% in the sequential treatment (71% of first movers and 69% of second movers are maximal liars). Partial liars, are subjects who lied and reported a die-roll outcome that is higher than the one observed but lower than the payoff maximizing outcome. Partial liars lay in the triangular area above the bisector but below a report of 6 in each graph of Figure 3.2. Among the subjects who lied, 35% are partial liars in the baseline, 26% in the simultaneous and 30% in the sequential treatment (29% of first movers and 31% of second movers are partial liars). There is a consistent pattern of more maximal liars and fewer partial liars across all treatments while the percentage of maximal and partial liars does not vary across treatments. A more exhaustive analysis of maximal liars and partial liars is reported in Appendix B.

A few subjects have lied down-wards, i.e. their reports are lower than the observed die-roll outcome; in Figure 3.2 they are represented from the dots that lay below the red line. When subjects lie downwards they implicitly choose to have a lower payoff lying, over having a higher payoff reporting truthfully. Overall, 6 subjects lied down-wards. In the following analysis, we exclude down-ward lies. Moreover, we exclude from the following analysis subjects that "could not lie" i.e. those subjects who observed a 6. Finally, since Player Bs in the baseline do not engage in reporting, we also exclude them from our analysis. We thus have 62 observations in the baseline treatment (Player As), 90 observations in the simultaneous treatment and 141 observations in the sequential treatment (divided into 70 first movers and 71 second movers). We have 293 observations in total.

In what follows we study lying in the extensive and intensive margins across treatments. To study lying in the extensive margin we use the dummy variable "liars", equal to 1 if a subject makes a report that is higher than the observed die-roll outcome and 0 otherwise. To study lying in the intensive margin, we define the variable "size of the lie", l_i, equal to the difference among the observed die-roll outcome and the report made (i.e. $l_i = r_i - x_i$) taking into considerations only subjects who lie.

Figure 3.2: Distribution of die-roll outcomes and subjects' reports



Notes: each dot refers to individual reports given a certain die-roll outcome. Histograms on the top panel, from left to right, refer to baseline, simultaneous and sequential treatment. Histogram on the bottom panel, refer to first (left) and second movers (right) in the sequential treatment. In the baseline, Player Bs are passive observers who are not asked to make a report while Player As are active and engage in reporting. Hence, we only consider observations relative to Player As in the Baseline. Dots are jittered to highlight the number of subjects making a certain report.

The first row of Table 3.2 reports the percentages of liars while the second row shows the mean of size of the lie across treatments.

The percentage of liars among the baseline and the simultaneous treatment is not significantly different (p= 0.39, one-sided test of proportions; p=0.78 one-sided Kruskal-Wallis test). In contrast, significantly more people lied in the sequential treatment compared to the baseline (p=0.04, one-sided tests of proportions; p-value=0.08, one-sided Kruskal-Wallis test) and to the simultaneous treatment (p=0.01, one-sided tests of proportions; p-value=0.02, one-sided Kruskal-Wallis test).

The average size of the lie is equal to 2.61 in the baseline, to 2.97 in the simultaneous and

Table 3.2: Lying in the extensive and intensive margins

	Baseline	Simultaneous	Sequential	First M.	Second M.
liars	50%	48%	63%	58%	69%
obs.	(62)	(90)	(141)	(71)	(70)
size of the lie	2.61	2.97	2.67	2.68	2.67
obs.	(31)	(43)	(89)	(41)	(48)

2.67 in the sequential treatment. One-sided T-tests of means and one-sided Kruskal-Wallis tests of distributions show that lying in the intensive margin does not differ across treatments.

We expected to observe more lying in the simultaneous treatment than in the baseline (Hypothesis 1). Nevertheless, these tests suggest that lying in the extensive and in the intensive margins does not vary among baseline and simultaneous treatment.

Result 1: *When lies are observable, having a passive or an active partner does not affect lying in either the extensive or intensive margins.*

However, we find evidence supporting Hypothesis 2: the sequential treatment differs from the simultaneous treatment in the extensive margin. Hypothesis 2 also conjectures that lying is higher in the sequential treatment due to lying of second movers. Hence, we now proceed to analyze whether there are differences in behavior among first and second movers in the sequential treatment. Moreover, we compare first and second movers with subjects in the baseline and in the simultaneous treatment.

The percentage of liars among first and second mover do not differ significantly (p= 0.09, one-sided tests of proportions; p=0.18 one-sided Kruskal-Wallis test). However, the percentage of liars among second movers in the sequential treatment is significantly higher than the percentage of liars in the baseline and in simultaneous treatments (p=0.015 and $p < 0.01$ respectively, one-sided tests of proportions; p=0.03 and $p < 0.01$ respectively, one-sided Kruskal-Wallis test). The same does not hold for the percentage of liars among first movers in the sequential treatment which does not differ significantly neither from the baseline nor from the simultaneous treatment (p=0.18

and p=0.10 respectively, one-sided tests of proportions; p=0.37 and p=0.21 respectively, one-sided Kruskal-Wallis test).

One-sided T-test of means and one-sided Kruskal-Wallis test suggest that there are no significant differences in the size of the lie among first and second movers. Moreover, we find no significant differences when we compare the first and second movers, respectively, with the baseline and the simultaneous treatment.

To summarize, we have found that subjects in the sequential treatment are more likely to lie than subjects in the simultaneous treatment. Comparing the behavior of first and second movers, we also find that the high percentage of liars in the sequential treatment is due to the high percentage of second movers who lie. This confirms our second hypothesis, that subjects lie more in the sequential treatment and that this is driven by the behavior of second movers.

Result 2: *When lies are observable and reports happen sequentially, lying in the extensive margin*
(i) is significantly higher in the sequential treatment than in the simultaneous treatment;
(ii) is significantly higher for second movers than subjects who report simultaneously.

Result 2 indicates that second movers in the sequential treatment are conditional liars.

In what follows, we test whether second movers lie more when they observe first movers lying (Hypothesis 3).

Table 3.3 reports the percentages of second movers who lie or tell the truth, conditional on the behavior of first movers, in the sequential treatment.

Table 3.3: Second movers' behavior conditional on first movers' behavior.

Sequential Treatment

		Second M.		
		lies	tells truth	obs
	lies	73%	27%	(41)
First M.	tells truth	62%	38%	(29)

Table 3.3 shows that being paired with a liar seems to increase the likelihood of lying. The 73% of second movers who are paired with a first mover who lies, lies; the 62% of second movers

who are paired with a first mover who tells the truth, lies. However, one-sided test of proportions and one-sided Kruskal-Wallis test of distributions show that there are no differences in subjects' behavior conditional on the behavior of their partners.[5] This suggests that lying of first movers does not affect lying of second movers in the extensive margin.

To complete the assessment of the impact of a partner's behavior on lying in the extensive margin, Table 3.4 reports a logit regression of Liars Second Movers (liars among second movers) on Liars First Movers (liars among first movers) in the sequential treatment treatment. Robust standard errors are clustered at the pair level and demographic controls are included. [6]

The coefficient for Liars of First Movers in the third column of Table 3.4 indicates a positive correlation among the behavior of second movers and the behavior of first movers. However, the coefficient is not significant. Hence, we can conclude that the behavior of partners does not impact lying in the extensive margin.

Table 3.5 reports the average size of the lie, conditional on the behavior of one's partner, in the sequential treatment.

Table 3.5: Size of the lie conditional on partner's behavior in the Sequential treatment.

Sequential		
		Second M. Size of the Lie
	lies	2.87
First M.	tells truth	2.33
	obs.	(48)

Table 3.5 shows that in the sequential treatment, being paired with a liar seems to increase the size of the lie while being paired with a truth-teller seems to decrease it. The average size of the lie for second movers who observed a first mover that lied is 2.87 while the average size of the lie for

[5] p=0.47 one-sided Kruskal-Wallis test; p=0.32, one-sided tests of proportions.

[6] Demographic controls are: Gender (dummy equal to 1 if male), Swiss Nationality (dummy equal to 1 if Swiss), Unil (dummy equal to 1 if student and affiliated to UNIL), Age, Grades (average grade, from 1 to 6), Bachelor (dummy equal to 1 if bachelor student, 0 if master or other), Grad. Parents (dummy equal to 1 if both parents hold a university degree), Only Child (dummy equal to 1 if only child), Big Town (dummy equal to 1 if resident in a big town), People Known (number of participants known during the lab section). A randomization check is presented in Appendix A.

Table 3.4: Liars in the Sequential treatment, logit regression.

	Sequential Treatment Liars Second M. (70)
Liars First M.	0.597
	(0.606)
Gender	-0.290
	(0.719)
Swiss Nat.	-2.072^{*}
	(1.020)
UNIL	-1.634
	(0.915)
Age	0.0136
	(0.0720)
Grades	-0.823
	(0.702)
Bachelor	-0.538
	(0.989)
Grad. Parents	-2.510^{**}
	(0.966)
Only Child	-1.543
	(1.261)
Big Town	-0.485
	(0.769)
People Known	-0.769
	(0.458)
Constant	8.270^{*}
	(4.110)
r2	
chi2	15.38
p	0.166

Standard errors in parentheses

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

second movers who observed a first mover who reported truthfully is 2.33. However, one-sided T-tests of means and one-sided Kruskal-Wallis tests of distributions show that there are no significant differences in the size of the lie conditional on the behavior of their partners.[7] This implies that the fact that a first mover lies or tells the truth does not affect lying of second movers in the intensive margin.

To further investigate whether observing lying increases lying, we analyze how the size of the lie of first movers influences the size of the lie of second movers.

Table 3.6 reports a linear regression of the size of the lie of second movers on the size of the lie of first movers. Robust standard errors are clustered at the pair level and demographic controls are included.[8]

As shown in Table 3.6, the coefficient for the size of the lie of first mover is positive and very significant. This implies that observing the size of the lie of first movers increases the size of the lie of second movers in the sequential treatment. In particular, out of the 48 second movers who lied, 30 lied given that the first mover they were matched with lied. Out of this 30, 14 second movers made exactly the same report as the first mover (and the report was equal to 6 in all cases), 9 made a higher report (8 reported a 6, 1 reported a 5) and finally 7 made a lower report (1 reported a 3 instead of a 5, 1 reported a 4 instead of a 6 and 5 a 5 instead of a 6).

Result 3: *When lies are observable and reports happen sequentially, observing lying increases lying in the intensive margin.*

Result 3 is in line with our third hypothesis, that second movers who observe first movers that lie, lie more than second movers who observe first movers that report truthfully.

[7] The size of the lie of first and second movers differs weakly in means (p=0.10, one-sided T-test of means) but not in distributions(p=0.24 one-sided Kruskal-Wallis test.)

[8] Demographic controls are: Gender (dummy equal to 1 if male), Swiss Nationality (dummy equal to 1 if Swiss), Unil (dummy equal to 1 if student and affiliated to UNIL), Age, Grades (average grade, from 1 to 6), Bachelor (dummy equal to 1 if bachelor student, 0 if master or other), Grad. Parents (dummy equal to 1 if both parents hold a university degree), Only Child (dummy equal to 1 if only child), Big Town (dummy equal to 1 if resident in a big town), People Known (number of participants known during the lab section). A randomization check is presented in Appendix A.

Table 3.6: Size of the lie in the Sequential treatment, linear regression

	Sequential Size of Lie Second M. (30)
Size of Lie First M.	0.732^{***}
	(0.141)
Gender	0.0183
	(0.402)
Swiss Nat.	-0.375
	(0.555)
UNIL	-0.286
	(0.462)
Age	0.130^{***}
	(0.0264)
Grades	-0.195
	(0.483)
Bachelor	1.035^{*}
	(0.429)
Grad. Parents	0.550
	(0.748)
Only Child	0.402
	(0.977)
Big Town	0.0661
	(0.502)
People Known	-0.256
	(0.215)
Constant	-1.645
	(2.962)
r2	0.651
chi2	
p	$3.89e-09$

Standard errors in parentheses

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

3.4.3 Expectations about Lying Behavior

As mentioned, we elicit individual beliefs regarding their partners' reports. In this section we analyze how accurately subjects anticipate lying behavior. [9]

To analyze subjects' beliefs in the extensive margin we introduce the dummy variable "expected liars", equal to 1 if subjects expect their partner to lie and equal to zero if they expect their partner to report truthfully. To analyze subjects' beliefs in the intensive margin we define the variable "expected size of the lie" as the difference among the die-roll outcome observed and the expected partner's report (i.e. $x_{i,j} - E_i[r_j]$) considering only subjects who expect their partner to lie.[10]

Table 3.7 shows the percentage of expected liars (first row), the percentage of actual liars (second row), the mean size of the lie (third row) and the mean expected size of the lie (last row) in the baseline, simultaneous and sequential treatments and distinguishing among first and second movers in the sequential treatment. [11]

[9] Similarly to the previous analysis, we exclude subjects whose partner could not lie, i.e. who observed a 6, since we are not able to disentangle whether a subject would expect their partner to report truthfully or not when they have the possibility to make the payoff maximising report truthfully. In addition, we drop subjects who expect their partner to lie downwards. In total, 7 subjects expect their partner to lie downwards. Subjects who expect their partner to lie downwards do not coincide with subjects who lie downwards. Differently from above instead, we do not take into consideration Player As in the baseline. Indeed Player As in the baseline are asked to guess the randomly drawn outcome for a passive Player B. This guess is not of interest and was implemented for consistency across treatments and subjects. Player Bs in the baseline instead are asked to guess the A's report. We are interested in knowing whether Player Bs anticipate lying behavior of Player As as passive observers.

[10] In the Baseline, expect lying is equal to the difference among the die-roll for Player A and the report that Player B expects Player A to make.

[11] Importantly, the percentage of liars and the actual size of the lie are marginally different from Table 3.2. Indeed, when analyzing beliefs, we not only drop observations of subject who observed a 6 or lied downwards, but also observations of subjects who expect their partner to lie downwards. Thus we refer to a reduced number of observations with respect to Table 3.2. Moreover, it is worth noting that, in order to be able to compare the expected and actual size of the lie, we only refer to subjects who expect their partner to lie and whose partner lied. Hence the number of the observations we have for the size of the lie may be lower than the percentage of the subjects who expected their partner to lie.

Table 3.7: Expected liars and expected size of the lie

	Baseline	Simultaneous	Sequential	First Mover	Second Mover
exp. liars	61%	59%	71%	63%	79%
liars	48%	48%	64%	59%	70%
obs.	(31)	(90)	(139)	(70)	(69)
exp. s. lie	2.45	3.10	2.69	2.61	2.76
size of lie	3.09	2.97	2.78	2.77	2.79
obs.	(11)	(39)	(78)	(36)	(42)

The first row of Table 3.7 shows that a very high percentage of subjects (71%) expected their partner to lie in the sequential treatment. This percentage is remarkably high for second movers in the sequential treatment (79%). The percentage of expected liars for second movers in the sequential treatment differs significantly in means and weakly in distributions from the baseline (p=0.03, one-sided test of proportions; p=0.06 one-sided Kruskal-Wallis test) and the simultaneous treatment (p=0.03, one-sided test of proportions; p=0.05 one-sided Kruskal-Wallis test) and differs significantly both in means and distributions from the percentage of expected liars for first movers (p=0.02, one-sided test of proportions; p=0.04 one-sided Kruskal-Wallis test). Thus, second movers in the sequential treatment are the subjects who expect their partner to be the most likely to lie.

The first and second rows of Table 3.7 show that subjects overestimate lying in the extensive margin i.e. the percentage of expected liars is significantly higher than the percentage of liars ($p < 0.01$, one-sided test of proportions; $p < 0.01$, one-sided Kolmogorov-Smirnov test). [12] This result confirms our fourth hypothesis.

The fourth row of Table 3.7 shows that the average expected size of the lie is higher in the simultaneous treatment. However, we only find weakly significant differences in means among the simultaneous treatment and the sequential treatment (p=0.06, one-sided T-test) and among the

[12] If we take into consideration each treatment and first and second movers in the sequential treatment separately, the differences in distributions highlighted in the whole data set hold (in all cases, $p < 0.01$, one-sided Kolmogorov-Smirnov test). However, this is not always the case when we consider the differences in proportions.We find a weakly significant difference in proportions among expected liars and actual liars only in the simultaneous and sequential treatment (p=0.07 and p=0.10, respectively, one-sided test of proportions).

simultaneous treatment and second movers in the sequential treatment (p=0.06, one-sided T-test) and these differences are not significant in distributions.

The fourth and fifth rows of Table 3.7 show that in all cases but in the simultaneous treatment, the expected size of the lie is lower than the actual size of the lie. However, overall, the difference among the expected and the actual size of the lie is significant in distributions ($p < 0.01$, one-sided Kolmogorov-Smirnov test) but not in means ($p = 0.44$, one-sided T-test). Hence, we cannot conclude that subjects underestimate lying in the intensive margin.[13]

To conclude, we find that subjects overestimate lying in the extensive margin and this in line with our forth hypothesis. Moreover, the expected percentage of liars is the highest for second movers in the sequential treatment.

Result 4: *In the extensive margin:*
(i) subjects overestimate the incidence of lying across all treatments;
(ii) second movers in the sequential treatment are the ones who expect their partners to be the most likely to lie compared to first movers in the sequential and to the baseline and simultaneous treatment.

3.4.4 Conditional lying and expectations about lying behavior

Belief elicitation allows us to investigate whether subjects anticipate lying and how this affects individual lying behavior.

In the sequential treatment, second movers form expectations about first movers' behavior, next they are informed about first movers' reports and then they make their own reports. This implies that second movers have the chance to have their beliefs proven right or wrong before reporting. In what follows, we investigate whether being surprised or not by first movers' behavior affects second movers' reports.

Table 3.8 compares the proportions of second movers who lie (i.e. percentage of liars), among

[13]If we take into consideration each treatment and first and second movers in the sequential treatment separately, we find a very significant difference in means among expected size of the lie and actual size for second movers in the sequential treatment ($p = 0.05$, one-sided T-test) and weakly significant differences in the baseline and in the simultaneous treatment (p=0.10 and p=0.08, respectively, one-sided T-test).

Table 3.8: % of second movers who lies, given first movers' behavior and expectations about it

		Second Mover	
		expects truth-telling	expects lying
First Mover	tells truth	12.5%	85%
	obs.	(8)	(20)
	lies	71 %	73.5%
	obs.	(7)	(34)

those second movers that (i) observe a first mover who tells the truth or lies and (ii) expected the first mover to tell the truth or lie.[14]

The first column of Table 3.8 shows the percentage of second movers who lies, conditional on expecting the first mover to tell the truth while the second column shows the percentage of second movers who lies, conditional on expecting the first movers to lie. Focusing on the first column, we find that the proportion of second movers who expect truth telling but observe lying and lies (71%) is remarkably higher than the proportion of second movers who expect and observe truth telling and lies (12.5%).[15] The difference is highly significant both in proportions and distributions ($p = 0.01$, one-sided test of proportions, $p = 0.02$ one-sided Kruskal-Wallis test). Focusing on the second column instead, a one-sided test of proportions and a one-sided Kruskal-Wallis test show no significant differences, that is, the behavior of second movers who expect truth telling but observe lying does not differ from the behavior of second movers who expect and observe lying.

The first row of Table 3.8 shows the percentage of second movers who lies, conditional on observing a first mover who tells the truth while the second row shows the percentage of second movers who lies, conditional on observing a first movers who lies. Focusing on the first row, we find that the proportion of second movers who expect lying but observe truth telling and lies

[14]For this part of the analysis we dropped subjects who observed 6, subjects who lied downwards and subjects who expect their partners to lie downwards. We thus have 69 observations.

[15]Importantly, percentages reported in cells do not sum to 100%. Cells have to be read as the percentage of second movers who lied, among all second movers who observe a certain behavior conditional on expecting that behavior or not. For instance, in the top-left cell we find the percentage of second movers that lied among all second movers who expect truth telling and observe truth telling: 8 second movers expect and observe truth telling, the 12.5% of them lies and the remaining tells the truth.

(85%) is remarkably higher than the proportion of second movers who expect and observe truth telling and lies (12.5%). The difference is highly significant both in proportions and distributions ($p < 0.01$, one-sided test of proportions, $p < 0.01$ one-sided Kruskal-Wallis test). Focusing on the second row instead, a one-sided test of proportions and a one-sided Kruskal-Wallis test show no significant differences, that is, the proportion of second movers who expect lying but observe truth telling and lie does not differ from the proportion of second movers who expect and observe lying and lie.

To summarize, we find that second movers who expect first movers to tell the truth but are surprised by first movers' lies are more likely to lie compared to second movers who expect and observe first movers who tell the truth. However, second movers who expect first movers to lie but are surprised by a first mover who tells the truth are more likely to lie than second movers who expect a first mover to tell the truth and observe a first mover who tells the truth.

Result 5*: When lies are observable and reports happen sequentially*
(i) second movers who expect truth telling but are surprised by lying are more likely to lie than second movers who expect and observe truth telling;
(ii) second movers who expect lying but are surprised by truth telling are more likely to lie than second movers who expect and observe truth telling.

Result 5 partially confirms our fifth hypothesis: on the one hand, second movers who expect truth telling but are surprised by lying are more likely to lie. On the other hand, we do not find evidence for the opposite surprise effect i.e. that second movers who expect lying but observe truth telling are less likely to lie than second movers who expect and observe lying. [16]

3.5 Conclusion

We study lying behavior when there are no strategic interactions and lies are observable. We are interested in investigating whether observing lying increases lying and whether expecting others to lie increases lying.

[16]We do not investigate lying in the intensive margin, conditional on what second movers expect and observe, due to the very little number of observation we have at disposal once we refer to the size of the lie.

We find that when there are no strategic interactions and lies are observable, lying in the baseline does not differ from lying in the simultaneous treatment. Subjects in the sequential treatment instead are more likely to lie than subjects in the simultaneous treatment. This is due to second movers who lie significantly more than subjects in the simultaneous treatment in the extensive margin. This suggests that second movers are conditional liars. Moreover, the size of the lie of second movers increases with the observed size of the lie of first movers. In other words, observing lying increases lying in the intensive margin.

Finally, we show that subjects overestimate lying in the extensive margin; among treatments, second movers are the ones who expect their partner to be the most likely to lie. Interestingly, we find that second movers shift their behavior towards lying when they expect truth telling and observe lying instead.

The literature on lying behavior has shown that lying increases in presence of strategic interactions and/or communication (Barr and Michailidou (2017), Kocher et al. (2016), Lauer and Untertrifaller (2019)). Diekmann et al. (2015) show that observing the distribution of reports increases lying even in absence of strategic interactions. However, in their design, subjects are only informed about aggregate reports and can only estimate individual lying behavior. Here, we study lying when subjects have full information about their partners' behavior. In our design, we not only exclude strategic lying but also allow second movers to precisely observe first movers' lies. We find that second movers in the sequential treatment lie more than subjects who report simultaneously. Moreover we find that observing the lies of first movers increases the size of the lie of second movers. We conclude that the motives of lying behavior when lies are observable are not only monetary but also related to social image costs of lying. The fact that second movers lie to a greater extent when they observe first movers lying, in absence of strategic interactions, suggests that observing other group members' lying decreases individual social image costs of lying.

Appendix

A. Demographics and Randomization of Subjects among Treatments

At the end of each experimental session, participants fill in a demographic questionnaire. Demographic variables include Gender (dummy equal to 1 if male), Swiss Nationality (dummy equal to 1 if Swiss), Unil (dummy equal to 1 if student and affiliated to UNIL), Age, Grades (average grade, from 1 to 6), Bachelor (dummy equal to 1 if bachelor student, 0 if master or other), Grad. Parents (dummy equal to 1 if both parents hold a university degree), Only Child (dummy equal to 1 if only child), Big Town (dummy equal to 1 if resident in a big town), People Known (number of participants known during the lab section).

To make sure that the randomization of subjects across treatments was successful, we run a multinomial logit regression. Consistently with the rest of our analysis, we drop observations of subject who lied downwards, subjects who observed a 6 and passive Player Bs in the baseline treatment. We compare active reporters in the baseline, subjects in the simultaneous treatment and first and second movers in the sequential treatment, choosing first movers in the sequential treatment as a benchmark. The multinomial logit regression is reported in Table 3.9.

Table 3.9 shows that the coefficients of Unil, Gender, Bachelor and People Known are significant. To make sure that our results are not due to a randomization fail, we not only use demographic controls in our regressions but also check if there are differences in extensive and intensive margins due to Unil affiliation, Gender, Bachelor and People Known in the room.

A.1 Unil

Table 3.10 reports the percentages of liars among Unil and non Unil subjects as well as the average size of the lie across treatments. The last two columns of the Table shows the p-value of one sided t-tests and one-sided Kruskal-Wallis tests.

As shown in Table 3.10, overall subjects who are not affiliated to Unil lie significantly more than subjects who are affiliated to Unil both to the intensive and extensive margin. On average the size of the lie is equal to 1.32 and 1.98 for Unil and non Unil respectively while on average the 50% of Unil and the 68% of non Unil subjects lies. A possible explanation is that Unil students are reluctant to lie since the lab we run the experiment at is the one of the University they are affiliated to.

Table 3.9: Mlogit of demographics, First M. in Sequential as benchmark

	Baseline	Simultaneous	Second M.
Gender	0.130	0.683*	0.141
	(0.356)	(0.338)	(0.353)
Swiss Nat.	0.324	-0.162	0.103
	(0.428)	(0.395)	(0.415)
Unil	-0.356	0.198	-0.953*
	(0.437)	(0.444)	(0.434)
Age	-0.0460	0.0570	0.0759
	(0.0721)	(0.0599)	(0.0628)
Grades	-0.101	0.127	-0.170
	(0.295)	(0.295)	(0.306)
Bachelor	0.402	1.080*	0.199
	(0.475)	(0.462)	(0.424)
Grad. Parents	-0.305	-0.253	-0.360
	(0.383)	(0.355)	(0.384)
Only Child	0.593	0.990	0.0653
	(0.728)	(0.622)	(0.787)
Big Town	-0.169	-0.126	-0.264
	(0.437)	(0.399)	(0.418)
People Known	-0.516	-0.562*	0.0800
	(0.329)	(0.264)	(0.209)
Constant	1.353	-2.504	-0.351
	(2.267)	(2.058)	(2.129)
r2			
chi2	32.66		
p	0.338		

Standard errors in parentheses

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

only those who could lie, no lie down, no B in baseline

The differences in intensive and extensive margins hold with significance in the baseline, sequential treatment and for second movers in the sequential treatment.

A.2 Gender

Table 3.11 reports the percentages of liars among males and females as well as the average size of the lie across treatments and distinguishing among first and second movers in the sequential treatment. The last two columns of the Table shows the p-value of one sided t-tests and one-sided Kruskal-Wallis tests.

In line with the finding of Abeler et al. (2019), on average male lie more than female both on the intensive (the average size of the lie is 1.70 for male and 1.36 for female) and extensive margin (61% of male lies and 51% of female lies). This difference holds in all treatments. However, it is significant only if we consider the overall data set and the baseline treatment, as well as for the percentage of liars among first movers in the sequential treatment.

A.3 Bachelor

Table 3.12 reports the percentages of liars among bachelor and non bachelor students as well as the average size of the lie across treatments and distinguishing among second movers in the sequential treatment. The last two columns of the Table shows the p-value of one sided t-tests and one-sided Kruskal-Wallis tests.

Although it seems that bachelor student lie more than non bachelor students both to the intensive and extensive margin (the size of the lie and the percentage of liars are equal to 1.49 and 53% and to 1.60 and 62% respectively), we do not find significant differences.

A.4 People Known

People known is a variable that indicates the number of participants a subject knows during a session. We introduced this variable to make sure that participating in the experiment with one or more colleagues or friends would not play a role, even if this should not be the case given the impossibility to communicate and the anonymity protocol we apply.

To make sure that this is not the case, we run linear regressions of lying and liars respectively on the variable "People Known" (in each regression we employ robust standard errors). Table 3.13

reports coefficients and p-values of such regressions across treatments and distinguishing among first and second movers in the sequential treatment.

Table 3.10: UNIL in Treatments

Unil	U	NU	P-value t-test	P-value KWallis
Overall	70%	30%		
Size of Lie	1.32	1.98	0.011	0.002
Liars	50%	68.5%	0.002	0.003
Baseline A	70%	30%		
Size of Lie	.86	2.3	0.001	0.009
Liars	42%	68%	0.026	0.056
Simultaneous	77%	23%		
Size of Lie	1.30	1.81	0.123	0.344
Liars	46%	52%	0.314	0.631
Sequential	65%	35%		
Size of Lie	1.55	1.94	0.101	0.112
Liars	56.5%	75.5%	0.013	0.0266
First M.	73%	27%		
Size of Lie	1.61	1.37	0.295	0.812
Liars	56%	63%	0.288	0.579
Second M.	57%	43%		
Size of Lie	1.47	2.3	0.022	0.030
Liars	57.5%	83.3%	0.010	0.022

Table 3.11: Gender in Treatments

Gender	M	F	P-value T-test	P-value KWallis
Overall	48%	51%		
Size of Lie	1.70	1.36	0.045	0.075
Liars	61%	51%	0.037	0.075
Baseline A	45%	55%		
Size of Lie	1.75	.94	0.033	0.069
Liars	61%	41%	0.062	0.130
Simultaneous	57%	43%		
Size of Lie	1.51	1.31	0.290	0.530
Liars	51%	43.5%	0.240	0.490
Sequential	44%	56%		
Size of Lie	1.83	1.57	0.176	0.285
Liars	69%	58%	0.087	0.175
First M.	41%	59%		
Size of Lie	1.79	1.38	0.159	0.233
Liars	69%	50%	0.055	0.114
Second M.	47%	53%		
Size of Lie	1.87	1.78	0.408	0.804
Liars	70%	67.5%	0.424	0.849

Table 3.12: Bachelor in Treatments

Bachelor	B	NB	P-value T-test	P-value KWallis
Overall	73%	27%		
Size of Lie	1.49	1.60	0.310	0.414
Liars	53%	62%	0.090	0.181
Baseline A	70%	23%		
Size of Lie	1.25	1.50	0.319	0.324
Liars	46%	64%	0.112	0.228
Simultaneous	80%	20%		
Size of Lie	1.37	1.61	0.304	0.555
Liars	46%	55%	0.230	0.462
Sequential	67%	33%		
Size of Lie	1.71	1.64	0.403	0.856
Liars	62.7%	63.8%	0.450	0.902
First M.	67%	33%		
Size of Lie	1.62	1.39	0.295	0.571
Liars	60%	52%	0.255	0.513
Second M.	66%	34%		
Size of Lie	1.80	1.87	0.435	0.795
Liars	65%	75%	0.402	0.406

Table 3.13: People Known: Regressions

Size of Lie	Coeff	P-value
Overall	0.25	0.032
Baseline A	0.56	0.049
Simultaneous	0.34	0.272
Sequential	0.16	0.421
Sequential A	0.36	0.201
Sequential B	-0.06	0.809
Liars	**Coeff**	**P-value**
Overall	0.038	0.216
Baseline A	0.055	0.485
Simultaneous	0.13	0.092
Sequential	.003	0.950
First M.	0.057	0.425
Second M.	-0.57	0.439

Table 3.13 shows that while coefficients for size of the lie are positive in all cases (with the exception of second movers for which the coefficient is very small). However, the coefficient is significant only if we consider the whole data set and not when we consider treatments separately.

Finally, coefficients for liars are positive in all cases (but for second movers) but not significant. Hence we can conclude that knowing more people does not affect lying in the extensive margin.

B. Lying to the extensive margin: type of liars

As mention in section 3.4.2, all dots lying above the red line in each graph of Figure 3.2 represent liars. In what follows, we distinguish two types of liars: (i) "maximal liars" and (ii) "partial liars".

Table 3.14 shows the percentage of liars, maximal liars and partial liars among those who lied in the three treatments and for second movers in the sequential treatment.

In all treatments, the percentage of maximal liars is significantly higher than the percentage of partial liars ($p<0.01$ one-sided test of proportions, $p<0.01$ one-sided Kruskal-Wallis test).

When comparing the proportion of partial liars or maximal liars across treatments (and dis-

Table 3.14: Extensive margin - liars and type of liars

	Baseline	Simultaneous	Sequential	First M.	Second M.
Obs.	(62)	(90)	(141)	(71)	(70)
liars	50%	48%	63%	58%	69%
			Among liars		
Obs.	(31)	(43)	(89)	(41)	(48)
partial liars	35%	26%	30%	29%	31%
max liars	65%	74%	70%	71%	69%

tinguishing among second movers in the sequential treatment), one-sided tests of proportions and one-sided Kruskal-Wallis test of distributions suggest no significant difference between these proportions (in all cases, p-values are above 0.178).

Hence we find that (i) the percentage of maximal liars is remarkably higher than the percentage of partial liars and that (ii) the percentage of maximal liars and partial liars does not differ across treatments.

The theory predictions from Gneezy et al. (2018) indicate that subjects lie maximally when lying can be detected. In our case we observe remarkably more maximal liars than partial liars, most likely due to the effect of die-roll verifiablity from the experimenter. However, Gneezy et al. (2018) theory also suggests that there should be no partial lying with full observability. This is not confirmed neither in their nor in our experimental data: a (small) percentage of liars lies partially even if most liars lie maximally.

For completeness, Table 3.15 reports the mentioned distinctions for the whole data set without excluding subjects who could not lie and subjects who lied downwards.

Table 3.15: Extensive margin - liars and type of liars, all observations included

	Baseline	Simultaneous	Sequential	First M.	Second M.
Obs	(69)	(100)	(166)	(83)	(83)
liars	44.93%	44%	56.63%	51.81%	61.45%
	(31)	(44)	(94)	(43)	(51)
partial-liars	15.94%	12%	19.28%	16.87%	21.96%
(among liars)	(11)	(12)	(32)	(14)	(18)
max-liars	28.99%	32%	37.35%	34.94%	39.76%
(among liars)	(20)	(32)	(62)	(29)	(33)

C. Experimental Instructions

Hereafter we report the English translation of the experimental instructions. The reader will first find the introduction to each section in (C.0) and then the part of experimental instructions that are peculiar to each treatment, baseline (C.1), simultaneous (C.2) and sequential (C.3). Finally, section C.4 explains in details the experimental procedures reporting screen-shots of a simulation of an experimental session.

C.0 Introduction

You are about to participate in an economic experiment. The experiment is conducted by the Department of Economics of the University of Lausanne.

For your participation in the experiment you will earn a payment of CHF 10 for sure. The experiment allows you to earn additional money. At the end of the experiment, you will be paid CHF 10 and any additional money you earned during the experiment. **It is to your own benefit to read these explanations carefully.**

You can perform the experiment at your own speed.

It is prohibited to communicate with the other participants during the whole course of the experiment. If you do not abide by this rule you will be excluded from the experiment and all payments. However, if you have questions you can always ask one of the experimenters by raising your hand.

You can abort the experiment anytime you wish without giving any reasons. To do so, please raise your hand and tell the experimenters that you wish to abort the experiment. One experimenter will then guide you outside the laboratory. You are not eligible to any payments in case you abort the experiment.

Your anonymity is guaranteed.

At the end of the experiment, one experimenter will give you a payment sheet with the amount you will be paid. You will need to carry the payment sheet with you and present it to an experimenter outside the LABEX. The experimenter outside the LABEX does not know about any of the decisions you made during the experiment. This experimenter will then pay you according to your payment sheet. After that you will sign a form stating that you received the payment. Since the form you sign does not contain your participant number, there is no way any experimenter can determine your identity.

If you have any questions right now, please raise your hand. Otherwise, you can now proceed with the detailed explanations of the experiment.

Thank you very much for your participation!

C.1 Baseline

We are now going to explain the task you will perform. For this task you are randomly and anonymously paired with another participant in this room. One participant is randomly assigned to the role of Person A and the other participant to the role of Person B. You will learn whether you have been assigned to the role of Person A or Person B in the end of these instructions.

During this experiment Person A will be asked to complete a task and Person B will be a passive observer.

Person A will observe the outcome of an electronic six sided die-roll. The experimenter will also observe the outcome of Person A's die-roll. Person A's die-roll has six possible outcomes: 1, 2, 3, 4, 5, and 6. Each outcome is realized with a probability of 1/6. The table below summarizes Person A's die-roll outcomes and their associated probabilities:

Outcome of the die-roll	1	2	3	4	5	6
Probability	1/6	1/6	1/6	1/6	1/6	1/6

The task of person A is to report the outcome of his/her die-roll. The monetary payment of Person A is determined by the number reported by Person A. If Person A reports number 1, then Person A is paid CHF 1, if Person A reports number 2, then Person A is paid CHF 2, etc. Here is a table of how the report of Person A is associated with the monetary payment of Person A:

Report of Person A	1	2	3	4	5	6
Monetary payment of Person A in CHF	1	2	3	4	5	6

Here is how Person B will get paid. A previous experiment took place here involving 24 participants recruited in the same way you were recruited. We call this experiment, "Experiment 1." The experiment you are participating now is not the same as "Experiment 1," however, you need to know about "Experiment 1" because what people did in that experiment is relevant for determining Person's B monetary payment today.

In "Experiment 1" participants were randomly and anonymously paired. In each pair there was a Person A and a Person B. The task of Person A and Person B was to observe the outcome of a die-roll and report this outcome. What Person A and Person B reported determined their monetary payments. For example, if Person A reported a 2, Person A received a monetary payment of CHF 2. If Person B, reported a 4, Person B received a monetary payment of CHF 4. Each of the 24 reports of "Experiment 1" has been recorded in a database.

Person B's monetary payment will be determined as follows. The computer will randomly draw from the database one of the reports of participants in "Experiment 1" **who observed the same die-roll as Person A has observed today**. It is this randomly drawn report that will determine Person B's monetary payment. For example, if Person A here today observed a die-roll of 2, the computer will randomly draw a report from all the participants in "Experiment 1" who observed a die-roll of 2. If the randomly drawn report is a 2, Person B will be paid CHF 2. If the randomly drawn report is a 3, Person B will be paid CHF 3, etc. Note that Person B does not make a report; the monetary payment of Person B depends only on the randomly drawn report from "Experiment 1." Here is a table of how the randomly drawn report from "Experiment 1" is associated with the monetary payment of Person B:

Randomly drawn report	1	2	3	4	5	6
Monetary payment of Person B in CHF	1	2	3	4	5	6

After Person A has observed the outcome of his/her die-roll and made his/her report, Person B observes the outcome of the die-roll of Person A and Person A's report, and Person A and B observe the outcome of randomly drawn report from "Experiment 1."

During the experiment Person A and Person B will also be asked to make a guess. This will become clear during the experiment. If Person A's guess is correct, Person A will be paid an additional CHF 1. If Person B's guess is correct, Person B will be paid an additional CHF 1. Therefore, the sequence of this experiment is as follows:

- Person A:

1. Person A observes the outcome of his/her die-roll
2. Person A makes a report
3. Person A makes a guess
4. Person A observes Person B's randomly drawn report from "Experiment 1"
5. Person A is paid his/her report

- Person B:

  1. Person B observes the outcome of the die-roll of Person A
  2. Person B makes a guess
  3. Person B observes the report of Person A
  4. Person B observes the randomly drawn report from "Experiment 1"
  5. Person B is paid the randomly drawn report from "Experiment 1"

The three examples that follow should make it clear how Person A's report and Person B's randomly drawn report from "Experiment 1" are related to the monetary payments in this experiment. **Example 1**: Assume the outcome of Person A's die-roll is 4, Person A reports 5, and Person B's randomly drawn report from "Experiment 1" is 4. In this example, Person A is paid CHF 5 and Person B is paid CHF 4. **Example 2**: Assume the outcome of Person A's die-roll is 2, Person A reports 4, and Person B's randomly drawn report from "Experiment 1" is 5. In this example, Person A is paid CHF 4 and Person B is paid CHF 5. **Example 3**: Assume the outcome of Person A's die-roll is 3, Person A reports 3, and Person B's randomly drawn report from "Experiment 1" is 3. In this example, Person A is paid CHF 3 and Person B is paid CHF 3.

It is important that you have a good understanding of the experimental instructions. To check that the instructions are clear to you we now ask you to answer a few questions. Your answers to these questions do not have any influence on the experiment itself or on the payment you will receive at the end of the experiment. The experiment will start once you and the person you are paired with have answered the questions correctly.

Questions to check your understanding:[17]

[17]Questions were shown on the experiment active screen before participants were assigned their roles of Person A or Person B

1. If Person A reports 5, how much is Person A paid?
2. If the outcome of the die-roll of Person A is 3 and Person A reports 2, how much is Person A paid?
3. If Person B's randomly drawn report from "Experiment 1" is 5, how much is Person B paid?
4. If the outcome of the die-roll of Person A is 4, Person B's randomly drawn report from "Experiment 1" is 3, and Person A reports 5, how much is Person B paid?
5. Does the report of Person A influence the monetary payment of Person B? Yes or No?

C.2 Simultaneous

We are now going to explain the task you will perform. For this task you are randomly and anonymously paired with another participant in this room. One participant is randomly assigned to the role of Person A and the other participant to the role of Person B. You will learn whether you have been assigned to the role of Person A or Person B in the end of these instructions.

Person A and Person B will observe the outcome of an electronic six sided die-roll. Both Person A and Person B will observe the same outcome of this die-roll. The experimenter will also observe the outcome of this die-roll. This die-roll has six possible outcomes: 1, 2, 3, 4, 5, and 6. Each outcome can be realized with a probability of 1/6. The table below summarizes the die-roll outcomes and their associated probabilities.

Outcome of the die-roll	1	2	3	4	5	6
Probability	1/6	1/6	1/6	1/6	1/6	1/6

Your task, and the other person's task, is the same: to report the outcome of the die-roll. Each person's monetary payment is only determined by his/her report. In other words, Person A's monetary payment is only determined by Person A's report and Person B's monetary payment is only determined by Person B's report.

If you report number 1, then you are paid CHF 1, if you report number 2, then you are paid CHF 2, etc. Here is a table of how your report is associated with your monetary payment:

Your report	1	2	3	4	5	6
Monetary payment in CHF	1	2	3	4	5	6

Once Person A and Person B have made their reports, each will be asked to make a guess. This will become clear during the experiment. If Person A's guess is correct, Person A will be paid an additional CHF 1. If Person B's guess is correct, Person B will be paid an additional CHF 1.

Finally, Person A observes the report of Person B and Person B observes the report of Person A.

Therefore, the sequence of this experiment is as follows:

1. Person A and Person B observe the outcome of the die-roll
2. Person A and Person B make their reports
3. Person A and Person B make their guesses
4. Person A and Person B observe each other's reports
5. Person A is paid his/her report and Person B is paid his/her report

The three examples that follow should make it clear how Person A's report and Person B's report are related to the monetary payments in this experiment.

Example 1: Assume the outcome of the die-roll is 4, Person A reports 5, and Person B reports 4. In this example, Person A is paid CHF 5 and Person B is paid CHF 4.

Example 2: Assume the outcome of the die-roll is 2, Person A reports 4, and Person B reports 5. In this example, Person A is paid CHF 4 and Person B is paid CHF 5.

Example 3: Assume the outcome of the die-roll is 3, Person A reports 3, and Person B reports 3. In this example, Person A is paid CHF 3 and Person B is paid CHF 3.

It is important that you have a good understanding of the experimental instructions. To check that the instructions are clear to you we now ask you to answer a few questions. Your answers to these questions do not have any influence on the experiment itself or on the payment you will receive at the end of the experiment. The experiment will start once you and the person you are paired with have answered the questions correctly.

Questions to check your understanding:[18]

1. If you report 5, how much are you paid?
2. If the outcome of the die-roll is 3 and you report 2, how much are you paid?
3. Does Person A observe a different outcome of the die-roll than Person B? Yes or No?
4. If the outcome of the die-roll is 2 and the person you are paired with reports 3, how much is the person you are paired with paid?
5. Does the report of one person influence the monetary payment of the other person? Yes or No?

C.3 Sequential

We are now going to explain the task you will perform. For this task you are randomly and anonymously paired with another participant in this room. One participant is randomly assigned to the role of Person A and the other participant to the role of Person B. You will learn whether you have been assigned to the role of Person A or Person B in the end of these instructions.

Person A and Person B will observe the outcome of an electronic six sided die-roll. Both Person A and Person B will observe the same outcome of this die-roll. The experimenter will also observe the outcome of this die-roll. This die-roll has six possible outcomes: 1, 2, 3, 4, 5, and 6. Each outcome can be realized with a probability of 1/6. The table below summarizes the die-roll outcomes and their associated probabilities.

Outcome of the die-roll	1	2	3	4	5	6
Probability	1/6	1/6	1/6	1/6	1/6	1/6

Your task, and the other person's task, is the same: to report the outcome of the die-roll. Each person's monetary payment is only determined by his/her report. In other words, Person A's monetary payment is only determined by Person A's report and Person B's monetary payment is only determined by Person B's report. If you report number 1, then you are paid CHF 1, if you

[18] Questions were shown on the experiment active screen before participants were assigned their roles of Person A or Person B

report number 2, then you are paid CHF 2, etc. Here is a table of how your report is associated with your monetary payment:

Your report	1	2	3	4	5	6
Monetary payment in CHF	1	2	3	4	5	6

Once Person A and Person B have observed the outcome of the die-roll, Person A makes his/her report. After this, Person B observes Person A's report. Then, Person B makes his/her report. Finally, Person A observes Person B's report.

During the experiment Person A and Person B will also be asked to make a guess. This will become clear during the experiment. If Person A's guess is correct, Person A will be paid an additional CHF 1. If Person B's guess is correct, Person B will be paid an additional CHF 1.

Therefore, the sequence of this experiment is as follows:

1. Person A and Person B observe the outcome of the die-roll
2. Person A makes his/her report
3. Person A and Person B make their guesses
4. Person B observes Person A's report
5. Person B makes his/her report
6. Person A observes Person B's report
7. Person A is paid his/her report and Person B is paid his/her report

The three examples that follow should make it clear how Person A's report and Person B's report are related to the monetary payments in this experiment.

Example 1: Assume the outcome of the die-roll is 4, Person A reports 5, and Person B, after having observed Person A's report, reports 4. In this example, Person A is paid CHF 5 and Person B is paid CHF 4.

Example 2: Assume the outcome of the die-roll is 2, Person A reports 4, and Person B, after having observed Person A's report, reports 5. In this example, Person A is paid CHF 4 and Person B is paid CHF 5.

Example 3: Assume the outcome of the die-roll is 3, Person A reports 3, and Person B, after having observed Person A's report, reports 3. In this example, Person A is paid CHF 3 and Person B is paid CHF 3.

It is important that you have a good understanding of the experimental instructions. To check that the instructions are clear to you we now ask you to answer a few questions. Your answers to these questions do not have any influence on the experiment itself or on the payment you will receive at the end of the experiment. The experiment will start once you and the person you are paired with have answered the questions correctly.

Questions to check your understanding:[19]

1. If you report 5, how much are you paid?
2. If the outcome of the die-roll is 3 and you report 2, how much are you paid?
3. Does Person A observe a different outcome of the die-roll than Person B? Yes or No?
4. Who is the first person to report the outcome of the die-roll? Person A or Person B?
5. Does the report of one person influence the monetary payment of the other person? Yes or No?

C.4 Experimental Procedure

In this section we take the reader throughout the core parts of the lab experiment. We report an example of the screenshots seen from player i[20] and player j in their French version (an English translation is available in each image caption).

In this example the die-roll is equal to 2, player i reports a 2 and guesses that j reported a 4 while player j reports a 4 and guesses that i reported a 6. Having guessed the partner's report, player i gets 1 CHF on top of the 10 CHF show up fee and the 2 CHF payment for her report. Player j instead did not guess: she will then receive 4 CHF for her report and the 10 CHF show up fee without any additional payment. This is shown in Figures from (3.3) to (3.15).

[19] Questions were shown on the experiment active screen before participants were assigned their roles of Person A or Person B

[20] Recall that during lab sessions we called "player i" "Person A" and "player j" "Person B".

Furthermore, we report the initial and final message of the questionnaire (Figures (3.16) and (3.17)) for a better understanding of the anonimous payment process. Finally we include the file - payment sheet for clarity.

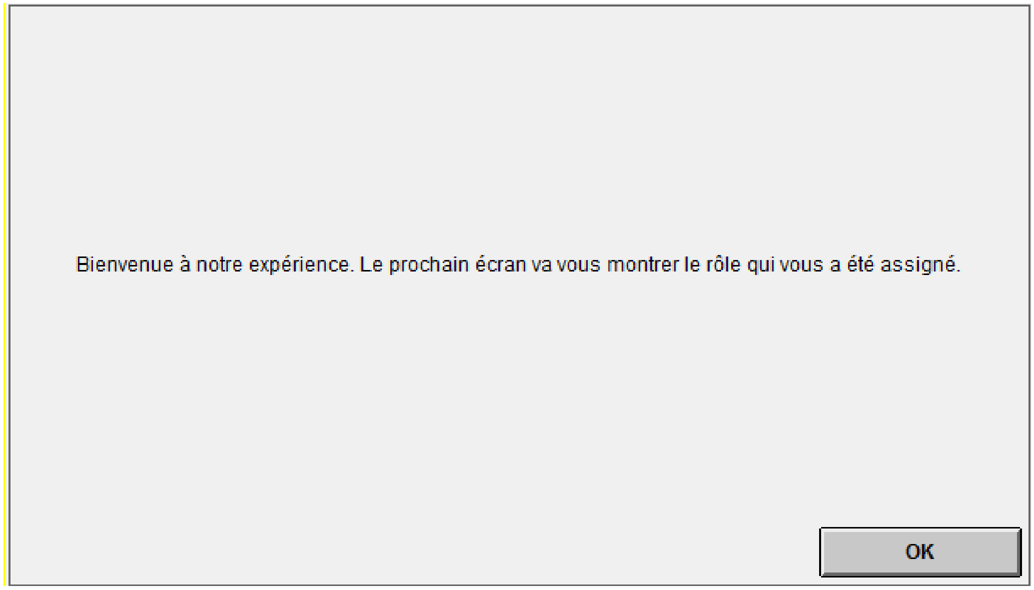


Figure 3.3: “Welcome to our experiment. The next screen will show you the role you have been assigned.”

Figure 3.4: “You have been assigned the role of Person A.”

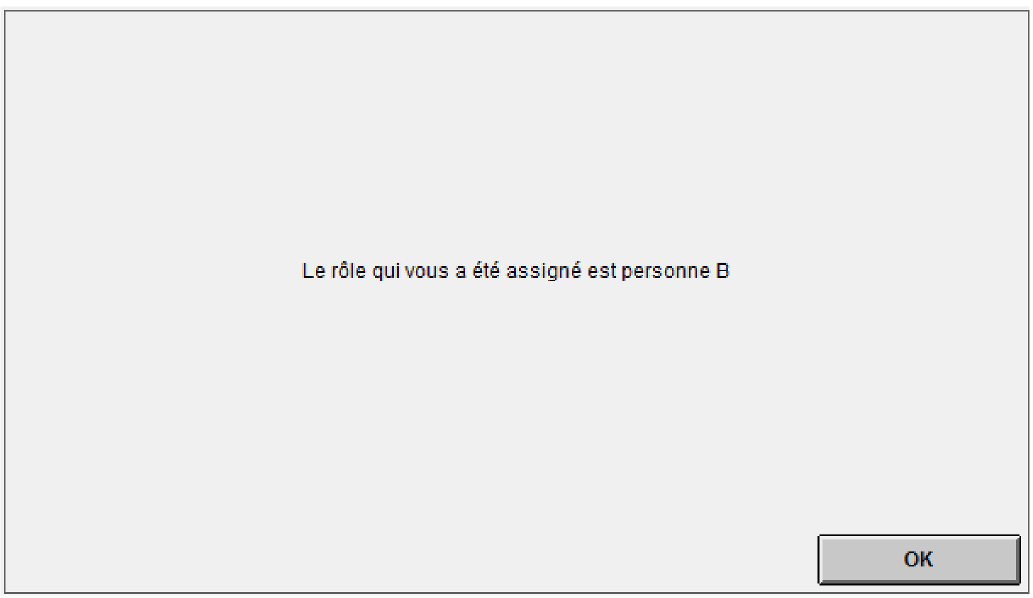


Figure 3.5: “You have been assigned the role of Person B.”

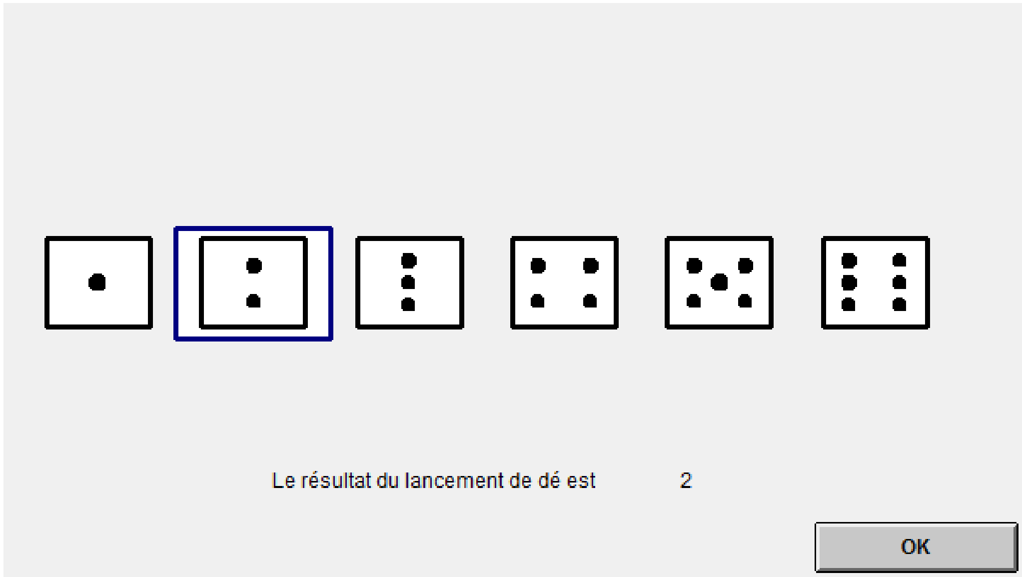


Figure 3.6: "The outcome of the die-roll is 2."

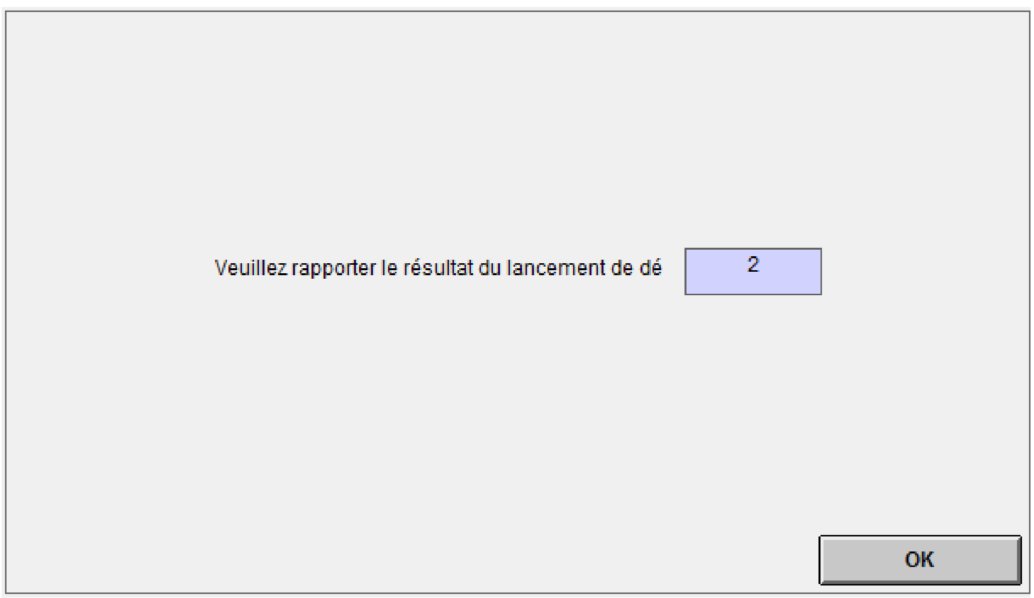


Figure 3.7: "Please report the outcome of the die-roll" - Person A reports 2.

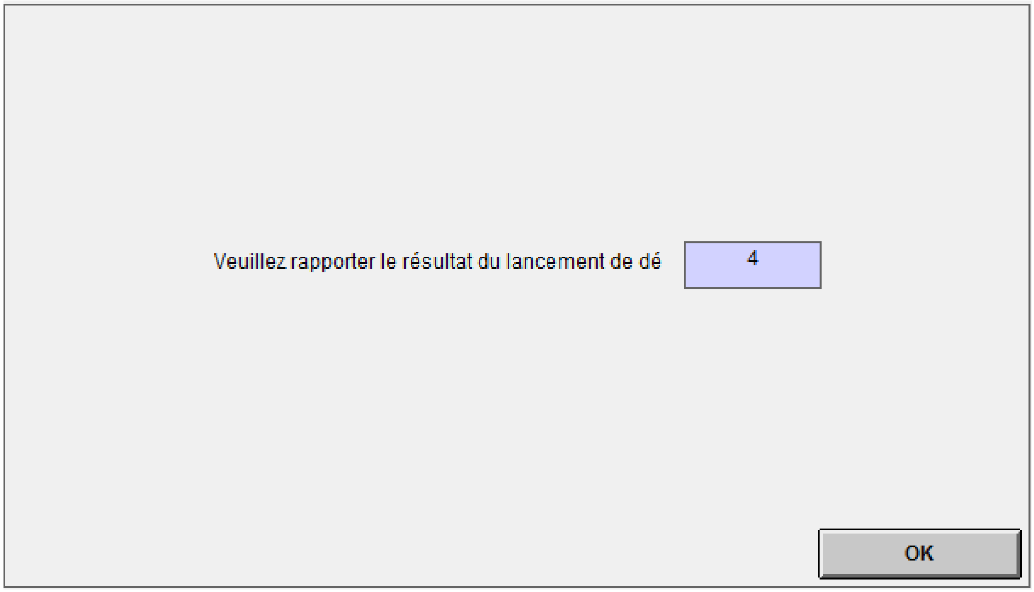


Figure 3.8: “Please report the outcome of the die-roll” - Person B reports 4.

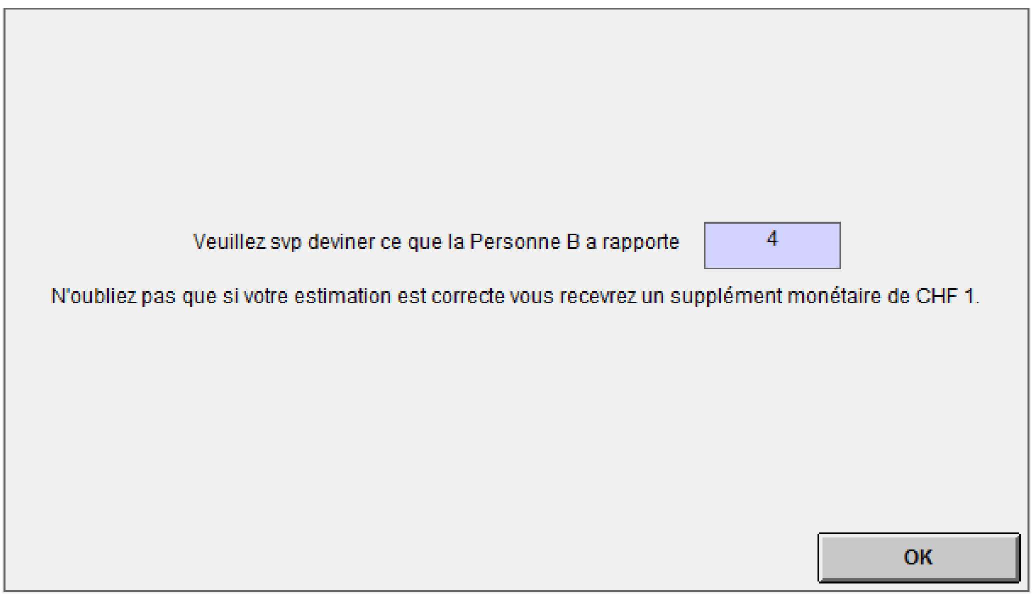


Figure 3.9: “Please guess the report of Person B” - Person A guesses that Person B reported a 4 -“Recall that if your guess is correct you will receive 1 CHF in addition.”

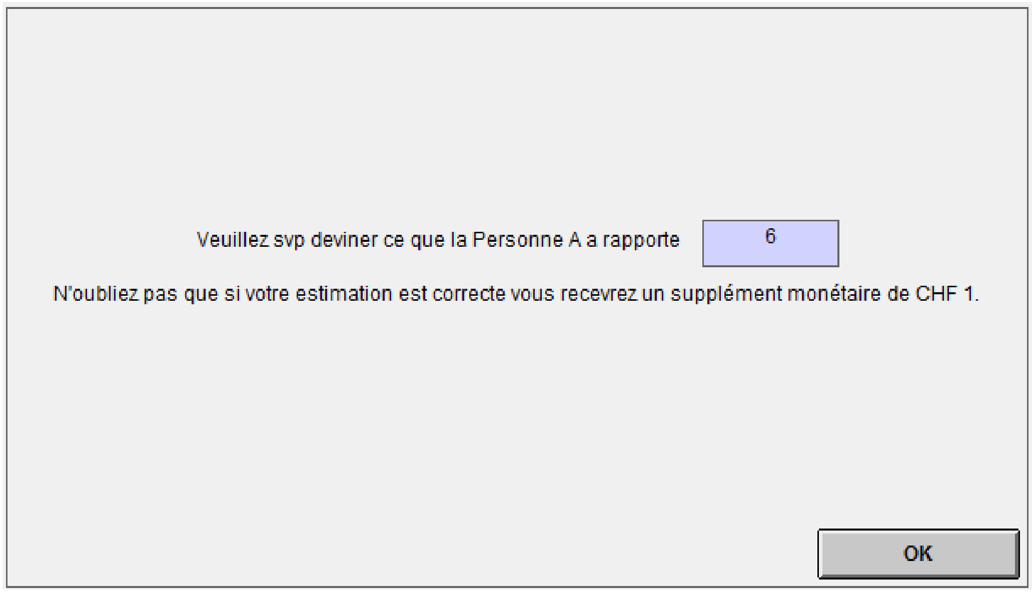


Figure 3.10: “Please guess the report of Person A” - Person B guesses that Person A reported a 6 - “Recall that if your guess is correct you will receive 1 CHF in addition.”

Figure 3.11: “Person B reported “ - 4 - “ You have guessed!”

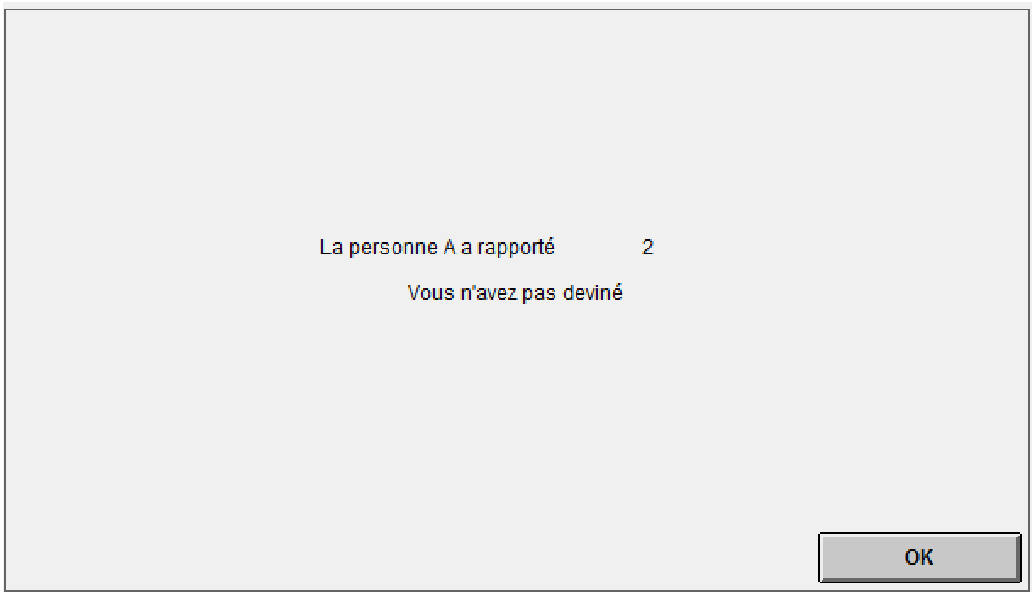


Figure 3.12: “Person A reported “ - 2 - “ You have not guessed!”

Figure 3.13: “You have gained “ - 2 - “Additional amount for your guess” -1- “Your payment is” - 13

Figure 3.14: “You have gained “ - 2- “Additional amount for your guess” -1 - “Your payment is” - 13

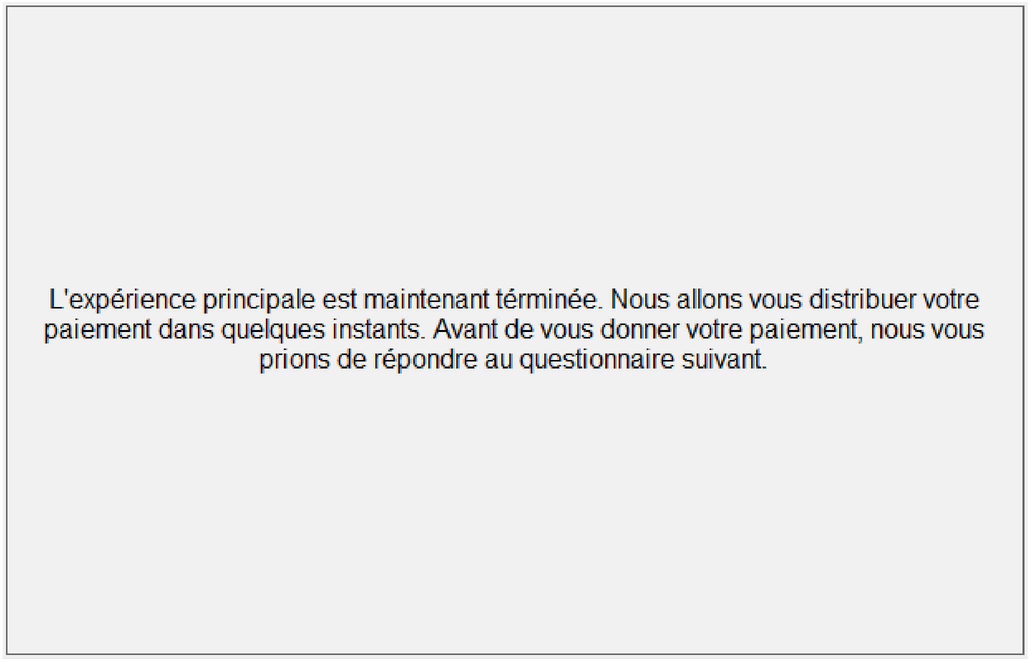


Figure 3.15: ”The experiment is now over. We will soon give you your payment. Before giving you your payment, we kindly ask you to fill in the following questionnaire.”

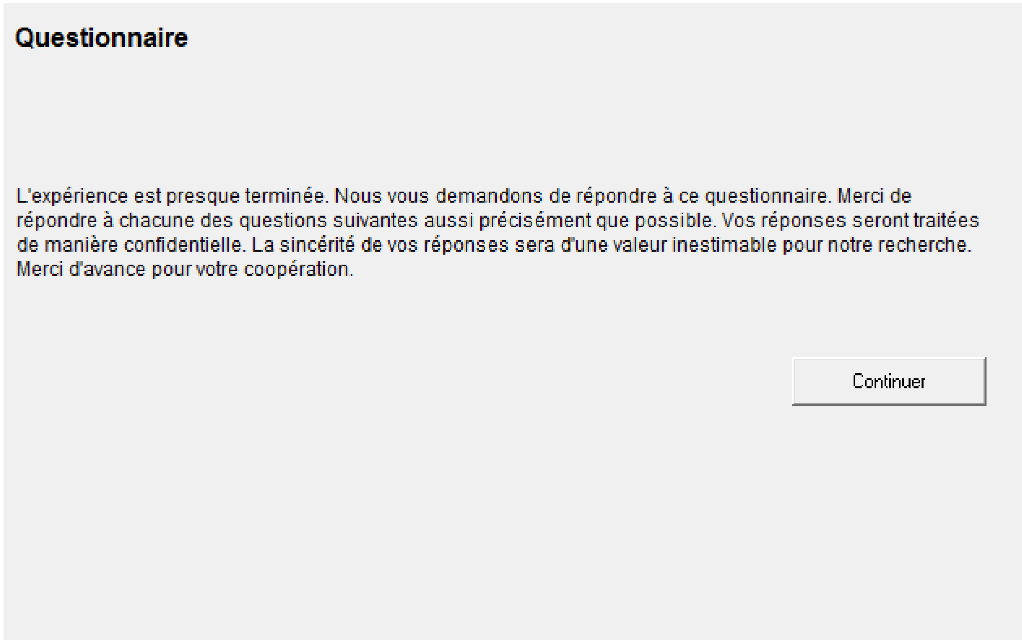


Figure 3.16: “Questionnaire - The experiment is almost over. We kindly ask you to answer the following questionnaire. Please answer each of the following questions as precisely as possible. Your answers will be kept confidential. A sincere answer is of great value for our research. Thank you in advance for your cooperation. - continue “

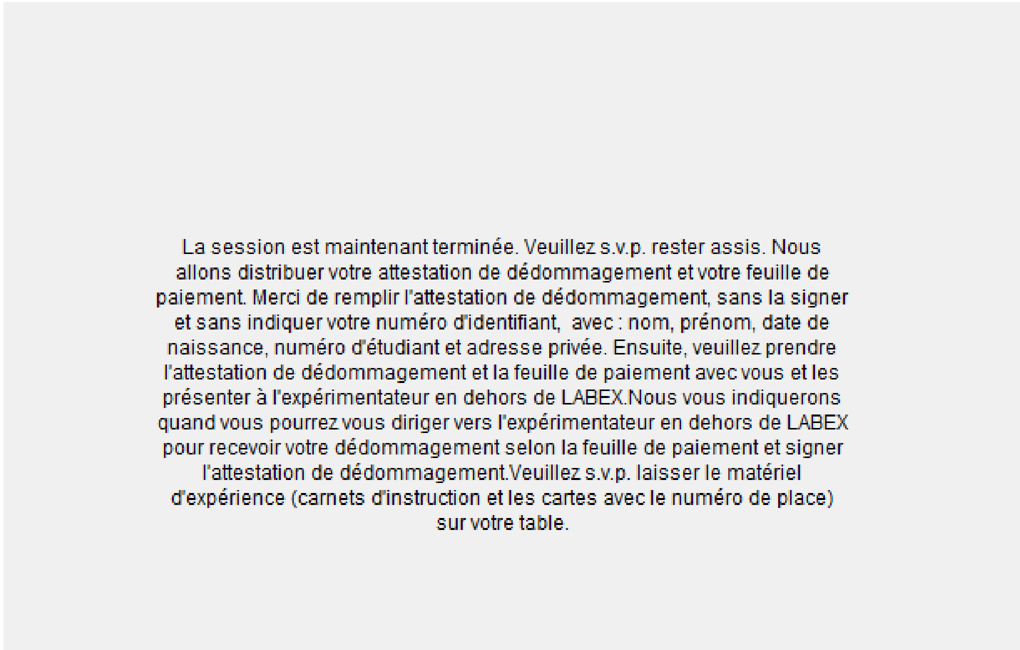


Figure 3.17: "The experiment is now over. Please remain seated. We are about to distribute you your payment sheet. Please fill the the payment receipt, without signing it nor indicating your subject number, with your name, surname, student ID and adress. Then please take the payment sheet and the payment receipt with you and show them to the experimenter outside of LABEX. We will tell you when to leave the room and reach the experimenter outside of LABEX to receive your compensation according to the payment sheet and sign the payment receipt. Please leave the experiment material (instuctions and cubicle number cards) on your desk."

LABEX_UNIL

Feuille de paiement

N° d'identifiant utilisé lors de cette expérience: «Subject»

Montant du dédommagement en CHF:«Profit»

Figure 3.18: “Idientification number used during this experiment”- subjects number- “Payment amount in CHF”-profit payoff

References

Abeler, J., Becker, A., and Falk, A. (2014).“Representative evidence on lying costs,” *journal of Public Economics* , 113, 96-104.

Abeler, J., Nosenzo, D., and Raymond, C. (2019). “Preferences for truth-telling,” *Econometrica*, Vol. 87, No. 4, 1115-1153.

Ali, S. Nageeb M. (2006). “Waiting to Settle: Multilateral Bargaining with Subjective Biases,” *Journal of Economic Theory*, Vol. 130, 109-137.

Ando, M. (2004). “Overconfidence in economic contests”, Available at SSRN 539902. 1, 306-318.

Babcock, Linda, George Loewenstein, Samuel Issacharoff, and Colin Camerer (1995). Biased Judgments of Fairness in Bargaining, *American Economic Review*, Vol. 85, No. 5, 1337-1343.

Babcock, Linda, and George Loewenstein (1997). “Explaining Bargaining Impasse: The Role of Self-serving Biases,” *Journal of Economic Perspectives*, Vol. 11, 109-126.

Baker, G.P., Gibbs, M. and Hollström, B. (1994). “The Wage Policy of a Firm,” *Quarterly Journal of Economics*, 109, 921-955.

Barr, A., and Michailidou, G. (2017).“Complicity without connection or communication,” *Journal of Economic Behavior and Organization*, Vol. 142, 1-10.

Bazerman, Max H., and M. A. Neal (1982). “Improving Negotiator Effectiveness under Final Offer Arbitration: The Role of Selection and Training,” *Journal of Applied Psychology*, Vol. 67, 543-548.

Bebchuk, Lucian A. (1984). “Litigation and Settlement Under Imperfect Information,” *The RAND Journal of Economics*, Vol. 15, No. 3, 404-415.

Bénabou, Roland, and Jean Tirole (2002). “Self confidence and personal motivation,” *The Quarterly Journal of Economics*, Vol. 99, No. 2, 459-65.

Bénabou, Roland, and Jean Tirole (2009). “Over my dead body: Bargaining and the price of dignity,” *American Economic Review*, Vol. 117, No. 3, 871-915.

Benoît, J. P., and Dubra, J. (2011). “Apparent overconfidence,” *Econometrica*, Vol. 79, No. 5, 1591-1625.

Brozynski, T.,Menkhoff, L. and Schmidt, U. (2006). “The Impact of Experience on Risk Taking, Overconfidence, and Herding of Fund Managers: Complementary Survey Evidence,” *European Economic Review*, 50, 1753-1766.

Bruhin, A., Fehr-Duda, H., and Epper, T. (2010). "Risk and Rationality: Uncovering Heterogeneity in Probability Distortion," *Econometrica*, Vol. 78, No. 4, 1375-1412.

Bruhin, A., Manai, M., and Santos-Pinto, L. (2019). "Risk and Rationality: The Relative Importance of Probability Weighting and Choice Set Dependence," Working Paper, University of Lausanne.

Brunner, M., and Ostermaier, A. (2019)."Peer influence on managerial honesty: The role of transparency and expectations," *Journal of Business Ethics* , Vol. 154, No. 1, 127-145.

Burks, S. V., Carpenter J. P, Goette L., and A. Rustichini (2013) "Overconfidence and social signalling," *The Review of Economic Studies* , Vol. 80, No. 3, 949â€"983.

Camerer, C., and Lovallo, D. (1999). "Overconfidence and excess entry: An experimental approach," *American Economic Review*, Vol. 89, No. 1, 306-318.

Carrell, S. E., Malmstrom, F. V., and West, J. E. (2008)."Peer effects in academic cheating," *Journal of human resources* , Vol. 43, No. 1, 173-207.

Cialdini, R. B., Reno, R. R., and Kallgren, C. A. (1990). "A focus theory of normative conduct: recycling the concept of norms to reduce littering in public places," *Journal of personality and social psychology* , Vol. 58, No. 6, 1015.

Compte, O., and Postlewaite, A. (2004). "Confidence-enhanced performance," *American Economic Review*, Vol. 94, No. 5, 1536-1557.

Conte, A., Hey, J. D., and Moffatt, P. G. (2011). "Mixture Models of Choice under Risk," *Journal of Econometrics*, Vol. 162, No. 1, 79-88.

Crosetto, P., and Filippin, A. (2013): "The "bomb" elicitation task," *Journal of Risk and Uncertainty*, Vol. 47, No. 1, 31-65.

Dargnies, M., Hakimov, R., and Kübler, D. (2019): "Self-confidence and unraveling in matching markets," *Management Science*, Vol. 65, No. 12, 5603-5618.

Della Vigna, S., and U. Malmendier (2006): "Paying Not to Go to the Gym," *American Economic Review*, Vol. 96, No. 3, 694-719.

Della Vigna, S. (2009). "Psychology and Economics: Evidence from the Field," *Journal of Economic Literature*, Vol. 47, No. 2, 315-372.

Diekmann, A., Przepiorka, W., and Rauhut, H. (2015)."Lifting the veil of ignorance: An experiment on the contagiousness of norm violations," *Rationality and Society*, Vol. 27, No. 3, 309-333.

Dimant, E. (2019).“Contagion of pro-and anti-social behavior among peers and the role of social proximity, ” *Journal of Economic Psychology*, Vol. 73, 66-88.

Eckel, C. C., and Grossman, P. J. (2008). “Men, women and risk aversion: Experimental evidence, ” *Handbook of experimental economic results*, Vol. 1, 1061-1073.

Erat, S., and Gneezy, U. (2012). “White lies ,” *Management Science*, Vol. 58, No. 4, 723-733.

Farber, Henry S., and Harry C. Katz (1979). “Interest Arbitration, Outcomes, and the Incentive to Bargain,” *Industrial and Labor Relations Review*, Vol. 33, No. l, 55-63.

Farber, Henry S., and Max H. Bazerman (1989). “Divergent Expectations as a Cause of Disagreement in Bargaining: Evidence From a Comparison of Arbitration Schemes,” *The Quarterly Journal of Economics*, Vol. 104, No. 1, 99-120.

Farmer, Amy, Paul Pecorino, and Victor Stango (2004). “The Causes of Bargaining Failure: Evidence from Major League Baseball,” *The Journal of Law & Economics*, Vol. 47, No. 2, 543-568.

Filippin, A., Crosetto, P. (2016). “A riconsideration of gender differences in risk attitudes,” *Management Science*, Vol. 62, No. 11, 3138-3160.

Fischbacher, U. (2007). “z-tree: Zurich toolbox for ready-made economic experiments,” *Experimental economics*, Vol. 10, No. 2, 171-178.

Fischbacher, U., and Föllmi-Heusi, F. (2013). “Lies in disguise—an experimental study on cheating,” *Journal of the European Economic Association*, Vol. 11, No. 3, 525-547.

Fudenberg, Drew, and Jean Tirole (1981). “Sequential Bargaining with Incomplete Information,” *The Review of Economic Studies*, Vol. 50, 221-248.

Gächter, Simon, and Arno Riedl (2005). “Moral Property Rights in Bargaining with Infeasible Claims,” *Management Science*, Vol. 51, No. 2, 249-263.

Gächter S., and Schulz, J. F. (2016). “Intrinsic honesty and the prevalence of rule violations across societies,” *Nature*, Vol. 531, No. 7595, 496-499.

Gervais, S., and Goldstein I. (2007). “The positive effect of biased self-perceptions in firms,” *Review of Finance*, Vol. 11, No. 3, 453-496.

Gibbons, R. and Murphy, K.L. (1990). “Relative Performance Evaluation for Chief Executive Officers,” *Industrial and Labor Relations Review*, 43, 30-S-51-S.

Gino, F., Ayal, S., and Ariely, D. (2009). “Contagion and differentiation in unethical behavior: The effect of one bad apple on the barrel,” *Psychological science*, Vol. 20, No. 3, 393-398.

Gino, F., Ayal, S., and Ariely, D. (2013).“Self-serving altruism? The lure of unethical actions that benefit others,” *Journal of economic behavior and organization*, Vol. 93, 285-292.

Gneezy, U., (2015).“Deception: The role of consequences,” *American Economic Review*, Vol. 95, 384-394.

Gneezy, U., Kajackaite, A., and Sobel, J. (2018). “Lying Aversion and the Size of the Lie,” *American Economic Review*, Vol. 108(2), 419-53.

Gneezy, U., Rockenbach, B., and Serra-Garcia, M. (2013). “Measuring lying aversion,” *Journal of Economic Behavior and Organization*, Vol. 93, 293-300..

Greiner, B. (2015). “Subjects pool procedures: Organizing experiments with ORSEE,” *Journal of the Economic Science Association*, Vol. 1, No. 1, 114-125.

Grubb, M. D. (2009). “Selling to Overconfident Consumers,” *American Economic Review*, Vol. 99, No. 5, 1770-1807.

Gürtler, O., and Kräkel, M. (2010). “Optimal Tournament Contracts for Heterogeneous Workers,” *Journal of Economic Behavior and Organization*, 75(2), 180-191.

Hackett, S. C. (1993). “Incomplete Contracting: A Laboratory Experimental Analysis,” *Economic Inquiry*, Vol. 31, No. 2, 274-297.

Harbring, C., and Lünser, G. K. (2008). “On the Competition of Asymmetric Agents,” *German Economic Review*, 9(3), 373-395..

Harless, D. W., and Colin F. Camerer (1994). “The Predictive Utility of Generalized Expected Utility Theories,” *Econometrica*, Vol. 62, No. 6, 1251-1289.Hey, John D., and C. Orme (1994).

“Investigating Generalizations of Expected Utility Theory Using Experimental Data,” *Econometrica*, Vol. 62, No. 6, 1291-1326.

Hicks, John (1932). *The Theory of Wages*. New York: Macmillan.

Hossain, T., and Okui, R. (2013). “The binarized scoring rule,” *Review of Economic studies*, Vol. 80, No. 3, 984-1001.

Hurkens, S., and Kartik, N. (2009). “Would I lie to you? On social preferences and lying aversion,” *Experimental Economics*, Vol.12 , No. 2, 180-192.

Johansson Stenman, O. and Nordblom, K. (2010). “Are men really more overconfindent than women? A natural field experiment on exam behavior,” Working Paper, University of Gothenburg.

Karagözoğlu, Emin (2004). "Bargaining Games with Joint Production," Oxford Handbook of Economic Conflict Resolution, (eds. R. Croson and G. Bolton), September 2012, Ch. 24, 359-372, Oxford University Press, USA.

Karagözoğlu, E., and Riedl, A. (2014). "Performance information,production uncertainty, and subjective entitlements in bargaining,"*Management Science*, Vol. 61, No. 11, 2611-2626.

Kennan, John, and Robert Wilson (1993). "Bargaining with Private Information," *Journal of Economic Literature*, Vol. 31, No. 1, 45-104.

Kőszegi, Botond (2014). "Behavioral Contract Theory," *Journal of Economic Literature*, Vol. 52, No. 4, 1075-1118.

Krähmer, D. (2007). "Equilibrium Learning in Simple Contests, " *Games and Economic Behavior*, 59(1), 105-131.

Keizer, K., Lindenberg, S., and Steg, L. (2008). "The spreading of disorder," *Science*, Vol. 322, No. 5908, 1681-1685.

Kocher, M. G., Schudy, S., and Spantig, L. (2018). "I lie? We lie! Why? Experimental evidence on a dishonesty shift in groups," *Management Science*, Vol. 64, No. 9, 3995-4008.

Kruger, Justin, and David Dunning (1999). "Unskilled and Unaware of It: How Difficulties in Recognizing One's Own Incompetence Lead to Inflated Self-assessments," *Journal of Personality and Social Psychology*, Vol. 77, No. 6, 1121-1134.

Krueger, Joachim, and Ross A. Mueller (2002). "Unskilled, Unaware, or Both? The Better-than-average Heuristic and Statistical Regression Predict Errors in Estimates of Own Performance," *Journal of Personality and Social Psychology*, Vol. 82, No. 2, 180-188.

Kruger, Justin (1999). "Lake Wobegon Be Gone! The 'Below-average effect' and the Egocentric Nature of Comparative Ability Judgments," *Journal of Personality and Social Psychology*, Vol. 77, No. 2, 221-232.

Lauer, T., and Untertrifaller, A. (2019). "Conditional dishonesty."

Lazear, E.P., and Rosen, S. (1981). "Rank-Order Tournaments as Optimum Labor Contracts," *Journal of Political Economy*, 89, 841-864.

Ludwig, S., Wichardt, P. C., and Wickhorst, H. (2011). "Overconfidence can improve an agent's relative and absolute performance in contests, " *Economics Letters*, 110(3), 193-196.

Malcomson, J. M. (1986). “Rank-Order Contracts for a Principal with Many Agents,” *Review of Economic Studies*, 53, 807-817.

Malmendier, U., and Tate, G. (2005). “Ceo confidence and corporate investment,” *The Journal of Finance*, Vol. 60, No. 6, 2661-2700.

Mazar, N., Amir, O.,and Ariely, D. (2008).“The dishonesty of honest people: A theory of self-concept maintenance,” *Journal of marketing research*, Vol. 45, No. 6, 633-644.

Merlo, Antonio, and Xun Tang (2019). “Bargaining with Optimism: Identification and Estimation of a Model of Medical Malpractice Litigation,” *International Economic Review*, Vol. 60, No. 3, 1029-1061.

Moore, Don A., and Small, D. A. (2007). “Error and bias in comparative judgment: on being both better and worse than we think we are,” *Journal of Personality and Social Psychology*, Vol. 85, No. 6, 1121-1135.

Moore, Don A., and Healy, P. J. (2008). “The trouble with overconfidence,” *Psychological review*, Vol. 115, No. 2, 502.

Moore, Don A., and T. G. Kim (2003). “Myopic Social Prediction and the Solo Comparison Effect,” *Journal of Personality and Social Psychology*, Vol. 85, No. 6, 1121-1135.

Murnigan, J. K., Roth, A. E., Schoumaker, F. (1988). “Risk aversion in bargaining: An experimental study,” *Journal of risk and uncertainty*, Vol. 1, No. 1, 101-124.

Murphy, W.H., Dacin, P.A. and Ford, N.M. (2004). “Sales Contests Effectiveness: an Examination of Sales Contest Design Preferences of Field Sales Forces,” *Journal of the Academy of Marketing Science*, 32, 127-143.

Myers, D. *Social Psychology*, (New York: McGraw Hill, 1996).

Nalebuff, B., and Stigltiz, J. (1983). “Prices and Incentives: Towards a General Theory of Compensation and Competition,” *The Bell Journal of Economics*, 14, 21-43.

Nash, John F. (1950). “The Bargaining Problem,” *Econometrica*, Vol. 18, 155-162.

Nash, John F. (1953). “Two-person Cooperative Games,” *Econometrica*, Vol. 21, 128-140.

Neal, M. A., and Max H. Bazerman (1983). “The Effect of Perspective Taking Ability under Alternate Forms of Arbitration on the Negotiation Process,” *Industrial and Labor Relations Review*, Vol. 36, 378-388.

Neal, M. A., and Max H. Bazerman (1985). "The Effects of Framing and Negotiator Overconfidence on Bargaining Behaviors and Outcomes," *The Academy of Management Journal*, Vol. 28, No. 1, 34-49.

Niederla, M. A., and Vesterlund, L. (2007). "Do women shy away from competition?," *The quarterly journal of economics*, Vol.122, No. 3, 1067-1101.

Oberlechner, T., and Osler, C. (2008). "Overconfidence in Currency Markets," Available at SSRN 1108787.

O'Keeffe, M., Viscusi, W. K., and Zeckhauser, R. J. (1984). "Economic contests: Comparative reward schemes," *Journal of Labor Economics*, 2(1), 27-56.

Ortner, Juan (2013). "Optimism, Delay and (In)efficiency in a Stochastic Model of Bargaining," *Games and Economic Behavior*, Vol. 77, 352-366.

Park, Y. J., and Santos-Pinto, L. (2010). "Overconfidence in tournaments: Evidence from the field," *Theory and Decision*, Vol. 69, No. 1, 144-166.

Pulford, B. D., and Colman, A. M. (1997). "Overconfidence: Feedback and item difficulty effects," *Personality and individual differences*, Vol. 23, No. 1, 125-133.

Phillips, O. R., R. C. Battalio, and C. A. Kogut (1991). "Sunk and Opportunity Costs in Valuation and Bidding," *Southern Economic Journal*, Vol. 58, No. 1, 835-856.

Priest, George L., and Benjamin Klein (1984). "The Selection of Disputes for Litigation," *Journal of Legal Studies*, Vol. 13, 1-56.

Rauhut, H. (2013). "Beliefs about lying and spreading of dishonesty: Undetected lies and their constructive and destructive social dynamics in dice experiments," *PloS one*, Vol. 8, No. 11, e77878.

Ross, Michael, and Fiore Sicoly (1979). "Egocentric Biases in Availability and Attribution," *Journal of Personality and Social Psychology*, Vol. 37, No. 3, 322-336.

Roth, Alvin E., and Michael W. K. Malouf (1979). "Game Theoretical Models and the Role of Information on Bargaining," *Psychological Review*, Vol 86, No. 6, 574-594.

Roth, Alvin E., and J. Keith Murnighan (1982). "The Role of Information in Bargaining: An Experimental Study," *Econometrica*, Vol. 50, No. 5, 1123-1142.

Rubinstein, A. (1982). "Perfect equilibrium in a bargaining model," *Econometrica: Journal of the econometric society*, 97-109.

Santos-Pinto, Luís (2010). "Positive Self-Image in Tournaments," *International Economic Review*, Vol. 51, No. 2, 475-496.

Santos-Pinto, Luís, and Enrique L. de la Rosa (2020). "Overconfidence in Labor Markets," K. Zimmermann (Ed), *Handbook of Labor, Human Resources and Population Economics*. Springer-Verlag.

Schotter, A., and Weigelt, K. (1992). "Asymmetric Tournaments, Equal Opportunity Laws, and Affirmative Action: Some Experimental Results.," *The Quarterly Journal of Economics*, 107(2), 511-539.

Shavell, S. (1982). "Suit, Settlement, and Trial: A Theoretical Analysis under Alternative Methods for the Allocation of Legal Costs," *Journal of Legal Studies*, Vol. 11, 55-81.

Singh, N., and Wittman, D. (2001). "Contests where there is variation in the marginal productivity of effort," *Economic Theory*, 18(3), 711-744.

Sobel, Joel, and Ichiro Takahashi (1983). "A Multi-Stage Model of Bargaining," *The Review of Economic Studies*, Vol. 50, 411-426.

Soll, J., and Klayman, J. (2004). "Overconfidence in interval estimates," *Journal of experimental psychology: learning, memory and cognition*, Vol. 30, No. 2, 299.

Spinnewijn, J. (2013). "Insurance and Perceptions: How to Screen Optimists and Pessimists," *The Economic Journal*, Vol. 123, 606-633.

Spinnewijn, J. (2015). "Unemployed but Optimistic: Optimal Insurance Design with Biased Beliefs," *Journal of the European Economic Association*, Vol. 13, 130-167.

Starmer, Chirs (2000). "Developments in Non-expected Utility Theory: The Hunt for a Descriptive Theory of Choice under Risk," *Journal of Economic Literature*, Vol. 38, No. 2, 332-382.

Svenson, O. (1981). "Are we all less risky and more skillful than our fellow drivers?," *Acta psychologica*, Vol. 47, No. 2, 143-148.

Thompson, L., and G. Loewenstein (1992). "Egocentric Interpretations of Fairness and Interpersonal Conflict," *Organizational Behavior and Human Decision Processes*, Vol. 51, 17-197.

Tingle, David (2015). "Bargaining Practice and Negotiation Failure in Russia-Ukraine Gas Relations," Cambridge Working Papers in Economics 1504, Faculty of Economics, University of Cambridge.

Van den Steen, Eric (2004). “Rational Overoptimism (And Other Biases),” *American Economic Review*, Vol. 94, No. 4, 1141-1151.

Yildiz, Muhamet (2003). “Bargaining without a Common Prior-An Immediate Agreement Theorem,” *Econometrica* Vol. 71, 793-811.

Yildiz, Muhamet (2011). “Bargaining with Optimism,” *Annual Review of Economics*,” Vol. 3, 451-478.

Waldfogel, Joel (1995). “The Selection Hypothesis and the Relationship between Trial and Plaintiff Victory,” *Journal of Political Economy*, Vol. 103, No. 2, 229-260.

Waldfogel, Joel (1998). “Reconciling Asymmetric Information and Divergent Expectations Theories of Litigation,” *The Journal of Law & Economics*, Vol. 41, No. 2, 451-476.

Weisel, O., and Shalvi, S. (2015). “The collaborative roots of corruption,” *Proceedings of the National Academy of Sciences*, Vol. 112, No. 34, 10651-10656.