Essays on behavioural and organizational economics

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To my loving family...

Abstract

This thesis consists of three self-contained experimental studies focusing on conformity behavior in the leader appointment process, self-group risk preferences of elected leaders and performance feedback mechanisms.

In Chapter 1, I investigate discrimination against women in election settings and whether group dynamics undermine women's chances to become leaders. I conduct a voting experiment which tests the effect of the candidate's gender on voting behavior, and the role of conformity. Consistent with the predictions of a simple model, subjects tend to vote for candidates who exhibit similar (risk) preferences. Information on the gender of the candidates mitigates proximity concerns of the voter especially in favor of the male candidate. Yet, there is no conclusive result for the gender bias. The results also confirm that conformity is a significant factor in group decision-making.

In Chapter 2, I analyze the mechanism which induces the difference between self and group risk attitudes of elected leaders. I focus on two motivations: a "leadership effect", that is created by the competition and the sense of responsibility of the leadership status, and a "group concern" of the leader. The results show that elected leaders significantly become more risk-seeking when deciding on behalf of a group compared to their individual decisions. Meeting the expectations of group members seems the main driver of this observed behavioral change.

In Chapter 3, in a setting where feedback is given strategically by a supervisor, we theoretically and experimentally analyze how employees interpret the received feedback in forming beliefs of themselves and whether feedback communicates the actual performance information truthfully. We found that information transmission occurs only in verifiable feedback mechanisms and private-verifiable is the most informative mechanism. We observed lying-aversion among principles: the results indicate a lying cost, and there is a tendency to send the true information where lying is profitable.

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Declarations

No part of this thesis has been submitted for another degree.

Chapter 3 is a joint work with Associate Professor Seda Ertac and Associate Professor Levent Kockesen. Duygu Ozdemir's contribution in the paper includes: (1) Design of the experiment; (2) Literature review; (3) Programming the experiment in z-tree experimental software; (4) Conducting the experimental sessions in Koc University and Bilgi University experimental labs; (5) Data management for making it ready for the analysis; (6) Data analysis; (7) Drafting the article. The rest of the paper including the theoretical model has been done with contribution of Associate Professor Seda Ertac and Associate Professor Levent Kockesen.

The other chapters in this thesis are exclusively mine.

Chapter 3 has been previously published in Games and Economic Behavior (Volume 100, November 2016).

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Chapter 1

Absence of Female Leaders: Do group dynamics play a role?

Abstract

It is well-known that the proportion of women in executive positions is substantially lower relative to men. I investigate whether group dynamics undermine women's chances to become leaders since leader positions are generally appointed by group decision. This experimental study aims to shed light on determinants of voting behavior and the impact of candidate gender on the voting decision in a group setting, with a focus on conformity behavior. Consistent with the predictions of a simple model based on spatial proximity, subjects tend to vote for candidates who exhibit similar (risk) preferences. Information on the gender of the candidates mitigates proximity concerns of the voter especially in favor of the male candidate. Yet, there is no conclusive result for the gender bias. The results also confirm that conformity behavior is one of the leading determinants of group decision making. Although not conclusive, there is evidence for women being more prone to conform. Conformity is more likely to be motivated by taste than social learning.

Keywords: Conformity, Voting, Discrimination, Lab experiments.

JEL Classification: C91, C92, D71, D72, D81, J16.

1.1 Introduction

It's no secret that there are fewer women in executive positions than men. A census by Catalyst (2012) documents that women held only 14.3% of the executive officer positions and 16.6% of the board seats in Fortune 500 firms. Moreover, more than 25% of those firms have no woman in an executive officer position.

A popular supply-side explanation is the gender-specific differences in individual preferences, such as differences in the willingness to occupy leadership positions, differences in risk preferences, women's distaste for competition and men's overconfidence as nominees for leadership positions (Ertac and Gurdal (2012), Niederle and Vesterlund (2007), Gneezy et al. (2003) and Reuben et al. (2012)). Yet, despite being fewer in number, there are many women who are willing to take on leadership positions. A recent Catalyst (2013) survey among the alumni of top MBA programs shows that those high potential women and men "in the pipeline" are equally likely to aspire to an executive position. This suggests that individual preferences may not be the sole factor involved.

To investigate the demand side for the lack of women in top positions, one needs to consider appointment procedures. The strategic decisions in companies and organizations about who should take on certain leadership roles such as executive positions are often made in small committees such as executive boards. In the committee decision-making process, there may be group dynamics which undermine women's chances to be appointed.

In this paper, I examine this conjecture. In particular, I focus on conformity behavior as a potential factor hindering women to be selected by the group. In a group decision-making process, conformity may cause contagion of individual bias over the group. Even though a group member has no gender bias on her own preferences over candidates, the choice of a biased majority might affect her both psychologically and strategically, which in turn leads her to change the prior decision in favor of the popular candidate. Therefore, conformity may amplify the effect of gender bias on a group's decision.

My experiment is designed to proxy the committee decision setting in order to test this effect. Clearly, the committee decisions are made after a deliberation process. I, rather, use a simple voting model to proxy this setting for simplification. I conduct a lab experiment that emulates a voting environment with two candidates, where voters cast a vote for the candidate who will make a risky decision on behalf of the group. The voting decision is based on the information about individual risky decisions of both candidates. I also collect data on the perceptions of voters about risk preferences of candidates. Across treatments, I manipulate the revelation of (1) candidate gender, (2) polling information.

I propose a simple voting model based on the proximity theory (Downs (1957)). The model predicts subjects voting for the spatially closer candidate on the spectrum of (risk) preferences. The model is tested via historical and perceptional approaches that claim voters care about the proximity of the candidate in the voting decision. Consistent with the predictions of this stylized model, subjects tend to vote for the candidate who exhibits similar risk preferences.

An important result from the paper is that revealing the candidate's gender shifts the vote distribution in favor of male candidates. The analysis of the same-sex and opposite-sex votes also shows that both female and male voters favor the male candidate only. Information on the gender of the candidates mitigates proximity concerns of the voter especially in favor of the male candidate. Yet, there is no conclusive result for the gender bias.

I further find support for the conformity hypothesis. Minority status, defined as voting for the less popular candidate, increases the propensity to conform. Subjects are willing to give up some proximity to their ideal point on the risk spectrum in order to conform with the ideal point of others in the committee. While there is some support for women being more prone to conform, the evidence is not conclusive.

I also conduct additional treatments to explore the motivation behind the conformity behavior. In particular, I aim to discriminate between the taste-based conformity as opposed to the informational conformity. In the former motivation, one may converge to the majority's decision in the committee since noncompliance may incur disutility. The latter motivation is based on the social learning within the committee. Under an information asymmetry across voters, the convergence to the majority may occur due to a voter updating her belief about candidates based on the majority's choice. To test this conjecture, I compare the propensity to conform under symmetric and asymmetric information cases. The results indicate conformity behavior being more likely to be motivated by taste than learning from the committee.

The rest of the paper proceeds as follows: section 1.2 reviews the related literature, 1.3 describes the design and the procedures, section 1.4 reviews the theoretical model of the voting behavior, section 1.5 provides the hypotheses of the study, section 1.6 explains the empirical strategy followed in the analysis, section 1.7 presents the results and checks the robustness of the provided evidence and section 1.8 provides a discussion and conclusion.

1.2 Related Literature

The glass ceiling for women in both politics and the business world has been considered in the literature from various dimensions. I focus specifically on impediments to women breaking the glass-ceiling. There is substantial evidence of a bias[1] against female leaders/politicians both at the individual and group level (see Fox and Smith (1998), Casas-Arce and Saiz (2015)). According to the World Values Survey[2], around 28% of European respondents believe men make better political leaders and 26% of them perceive men to be better business executives than women. Economics studies explain the bias by two reasons: the taste-based

[1]Note that the term *gender bias* is defined as an unfair treatment to men or women because of their sex. Here, the bias refers to the case whereby the appointment to certain positions in workplaces and politics is affected by the applicant's or the candidate's gender.

[2]World Values Survey Wave 6 2010-2014. www.worldvaluessurvey.org Note that the data is aggregated over all European countries (Germany, Netherlands, Spain, Sweden, Slovenia, Poland, Romania, Cyprus and Turkey) which are included in Wave 6.

discrimination and the statistical discrimination. In the former, individuals may have a taste for discrimination whereby hiring a female executive incurs a disutility (Becker (1957)). In the latter, individuals may not prefer a female executive due to the limited information about women's executive ability which is a result of the fewer number of women in such positions (Aigner and Cain (1977)). The focus of this paper is not to investigate the motivation behind the bias, but rather identify the bias.

To address this issue, affirmative action in politics, business and academia has been introduced in several countries[3]. Although evidence on the effectiveness of these policies is mixed (Ford and Pande (2011), Bagues and Esteve-Volart (2010), Jones (2004)), exposure to women in such positions reduces the effect of stereotypes (Beaman et al. (2009), Finseraas et al. (2016) and Gangadharan et al. (2016)).

This paper contributes first and foremost to the literature trying to identify the demand-side reasons behind the lack of female leaders[4]. This strand of the experimental literature predominantly explains the phenomenon with the prejudice against female leaders which is formed due to the conflict of gender role in the society and the perceived role of leaders (Eagly and Karau (2002) and Garcia-Retamero and López-Zafra (2006)). With a focus on discrimination in STEM fields, in a lab experiment, Reuben et al. (2014) show that women are perceived as less talented for an arithmetic task and therefore are half as likely to be hired as their men counterparts. This paper adds to the demand-side of leadership literature by investigating the potential mechanisms that cause discrimination in the leader appointment process.

As shown in several empirical studies, group dynamics seem to play an impor-

[3]For more information, please refer to Jones (2004) for parity law in politics, Ford and Pande (2011) for an overview of gender quotas for corporate boards and Bagues and Esteve-Volart (2010) for gender quotas in scientific committees.

[4]In the literature, leadership has been extensively used in the leader-follower contexts. Note that this paper does not contribute to that strand of the literature. As in Ertac and Gurdal (2012), I, rather, focus on leadership as an executive role in workplaces where leader acts as the decision-maker for the company.

tant role in the phenomenon. An election study by Casas-Arce and Saiz (2015) shows that despite their higher vote potential, women are less likely to be nominated by the party, which suggests a party bias in the candidate appointment. Similarly, Jones (2004) documents that the legislated gender quota on political parties became effective only when the placement of women in electable positions is mandated in Costa Rica. This indicates that the impact of the positive action depends on the willingness of the political party to include female candidates in their list. While existing studies suggest that group dynamics is an important factor, there is no study up to date attempting to identify them. This study aims to fill this gap.

Group dynamics cannot be considered separately from the formation of norms within groups (see Bettenhausen and Murnighan (1985), Friedkin (2001) and Sherif (1936)). In a recent study, Bagues et al. (2015) find that unanimous decisions are prevalent in scientific committees in which there is an interaction between committee members before voting. This indicates that the deliberation process may induce convergence. Consequently, I investigate conformity as a social multiplier of a bias in a group decision-making process.

Conformity behavior refers to the act of changing one's behavior to match with others' responses (Cialdini and Goldstein (2004)). It has been explored in different contexts both in psychology and economics. Neuroscientists point out a natural instinct to conform under group pressure since any conflict with group opinion is perceived as a punishment by the brain (Berns (2008), Klucharev et al. (2009)). Besides, economics demonstrates it as a normative behavior such as herding, the threat against social status and ingratiation (Banerjee (1992), Akerlof (1980), Robin et al. (2014)). Yet, there is no study up-to-date focusing conformity in committee decisions. This study contributes to the literature by opening the black box of group decision-making with a focus on conformity.

This paper also adds to recent experimental literature focusing on the relative importance of conformity-inducing mechanisms. In economics and psychology, some conformity studies motivate the behavior with the preference mechanism in which taste drives the behavioral convergence (Festinger (1954)), and others explain it with the belief mechanism in which social learning constructs a shared norm (Zafar (2011), Krupka and Weber (2009), Carpenter (2004)). Interestingly, the relative importance of the two mechanisms has been investigated by very few studies. Robin et al. (2014) and Bernheim and Exley (2015) attempt to disentangle these two mechanisms. Although they cannot significantly differentiate the motive, Robin et al. (2014) find the tendency to conform being as more strategic in ingratiation context of employer-employee relations. Bernheim and Exley (2015), on the other hand, find equal importance of both mechanisms in a broader setting. Their group treatment provides a somewhat similar setting to my design. Participants choose an effort level in advance of being selected as a leader, while the effort level of each group member is publicly observable. They find more support for the preference mechanism than the belief mechanism and they attribute the behavior to an identity concern due to the public announcement. Although the social image is not a motive in my design, my results are consistent with their findings.

Finally, this paper also contributes to the experimental literature on the political economy by exploring voting behavior. There are two leading theories on voting behavior in political sciences. The proximity theory (Downs (1957)) describes the voting decision as casting the vote for the spatially closer candidate on the political spectrum, whereas the directional theory (Rabinowitz and Macdonald (1989)) proposes voting for the extreme candidate on the same 'side' with the voter. Lacy and Paolino (2010) attempt to discriminate between these theories with an experiment. Although the results are not conclusive, they find more support for the proximity theory. To understand the effect of group dynamics in voting behavior, I investigate a proximity-type voting. My study provides evidence for proximity voting in a two-candidate election, thereby contributing to the voting literature with experimental evidence.

1.3 Experimental Design

The experimental design aims to shed light on the role of candidate gender in the voting decision and the impact of conformity behavior on the magnitude of gender bias in group voting. Information about candidate gender and the polling information about votes of other group members are the treatment variables which are applied between-subject and within-subject respectively.

In order to clearly investigate the proposed questions, I focus on only one leadership characteristic in my design. Since leadership positions generally involve decisions with risk and uncertainty, I employ a risk elicitation task by Gneezy and Potters (1997). Each participant is initially endowed with 100 ECU[5] to allocate between two options: a riskless option, i.e. bond option and a risky option, i.e. stock option. The risky multiplier, k_t, is randomly chosen from [1.5,3] for each round t and is applied as a common multiplier for all subjects. The amount allocated to the risky option, X_2, yields $k_t X_2$ with the probability of good state 0.5 and 0 otherwise, whereas riskless option secures the allocated amount. So the expected payoff is:

$$E[U_t|k_t] = (100 - X_2) + [0.5(k_t X_2) + 0.5(0)],$$

where k_t denotes the risky multiplier, at round t, $k_t \, \epsilon \, [1.5, 3]$.

The experiment consists of two stages: the individual stage and the group stage. The aim of the individual stage is to collect data for risk preferences of participants which is utilized as the "candidate information" in the group stage election. The participants individually perform the investment task for a randomly selected risky multiplier in each 9 round[6]. I additionally use a strategy method in round 10 of the individual stage, *the strategy method round*, where subjects are expected to state their own investment decisions for a list of risky multipliers that

[5]ECU refers to Experimental Currency Unit. The exchange rate is set as 1 ECU= £0.04, i.e. 4p.

[6]The investment task is repeated for design concerns. The reasons are explained with the corresponding parts of the design.

include 1.5, 2, 2.5 and 3. (see Figure A.6 in Appendix A). This is used in the perception analysis which is explained in detail in Section 1.6.2.

The group stage is the main part of the experiment in which all within-subject treatments are implemented. At the beginning of each round, 7-person groups are randomly formed. A stranger matching protocol is used in group formation in order to prevent reciprocity. In each round, 2 candidates, 1 female and 1 male, are exogenously appointed. In order to guarantee that each group has at least one female and one male member for candidacy, stratified randomization by gender is applied in group formation. The appointed candidates are represented anonymously as candidate A and B. Also one of the individual stage rounds is randomly selected as *the information round*. On the voting screen, the risky option allocations of both candidates for the corresponding risky multiplier of chosen information round is displayed (refer to Figure A.7 for the voting screen). With the given information, voters cast their votes for the desired candidate. The leader is elected with majority rule and she performs the investment task on behalf of the group. The payoff of all group members is determined by the leader's decision.

Additionally, voters are asked to guess the risky option allocations of both candidates in strategy method round with a reference to the given information (refer to Figure A.8 for the guess screen). Since the candidate information is a signal of their risk preferences, the guesses are used as a tool to observe the perception of voters about candidates' risk behavior. Note that guesses are monetarily incentivized[7].

Due to the scope of the paper, I focus on the following treatments (for other treatments, see Appendix A.2): :

- **Between-subject:**
  - **Gender treatment:** In order to identify a potential gender bias, i.e.

[7]The guess is regarded as correct when it is in the ∓ 3 interval of the actual allocation of the candidate. Each correct guess for each candidate is rewarded with 5 ECU bonus.

whether a candidate's gender affects the voting decision, revelation of the gender of the candidates is altered across sessions[8]. Unlike in the control group, gender treatment group candidates are additionally represented with icons that indicate their gender (see Figure A.7 in Appendix A.3.).

- **Within-subject:**
  - **Baseline treatment:** The explained group stage procedure is applied.
  - **Conformity treatment:** A pre-election stage, i.e. a poll, is employed. The poll result represented in terms of vote share of each candidate. After observing the poll result in their group, the participants are allowed to vote again in the second stage of the election (see Figure A.9 in Appendix A.3). Note that all group members observe the poll results. In order to incentivize the decisions in both stages of the election, the final vote is randomly selected from the poll and second stage votes of the subject.

Note that to collect more data from participants, each within-session treatment is repeated for 3 rounds. To eliminate potential wealth effects, one of the individual and group stage rounds is randomly chosen for payment and subjects are paid according to their payoffs in the chosen round.

1.3.1 Procedures

The experiment was programmed using z-Tree experimental software (Fischbacher (2007)) and implemented at the ESSEXLAB, University of Essex, in June 2014. The data is collected from 196 subjects in total (112 subjects were in gender treatment and 84 subjects were in gender control group) who are recruited via hroot (Bock et al. (2014)). The experiment is conducted in 11 sessions, with

[8]In the gender treatment sessions, the gender information of the candidates is provided in all group stage rounds of the session. In gender control, the information is withheld throughout the session.

6 gender treatment and 5 gender control sessions. The sample consists of 99 female (55 in treatment, 44 in control group) and 97 male (57 in treatment, 40 in control) participants, who are mostly undergraduate students. Since the student population is diverse in various terms at the University of Essex, a survey is conducted at the end of each session to collect demographic data such as age, ethnicity, major, siblings and annual personal income. The duration of the sessions were 50 minutes, and subjects earned £9.91 on average, including a show-up fee of £2.50.

An extension experiment for checking the robustness of findings is implemented at the ESSEXLAB in June 2016. 140 subjects (69 female, 71 male) participated in the extension experiment and they earned £7.1 on average[9]. The extension experiment is conducted in 7 sessions and each session took 50 minutes. Note that the participants of the main experiment are excluded from the recruited sample of extension experiment sessions.

1.4 Theoretical Model

In the design of the experiment, voter's utility depends not only on the self risk preference but also on the risk preference of the leader, $u(r_{self}, r_{leader})$, since the allocation decision is made by the leader in the group stage. Similar to the Downsian proximity theory (Downs (1957)), a voter's decision is based on the minimization of the distance between the self and leader's risk aversion rates

$$\underset{C_A,C_B}{min} \mid r_{self} - E[r_{C_i}] \mid,$$

where i = A, B,

C_i: candidate i,

r_{self}: risk aversion index of the voter,

r_{C_i}: risk aversion index of candidate i.

[9] Earnings ranged between £4.10 and £17.40 in the main and £3.50 and £14.10 in the extension experiment, including the show-up fee.

Note that the minimization of distances depends on the expected risk preference of candidates $E[r_{C_i}]$ since the risk indices of candidates are unobservable.

The design of the experiment allows to validate this model in two ways:

- **Historical proximity:** Based on the observed information from the chosen information round, voter minimizes the difference between the self and candidate's allocation to the risky option in the information round for the given risky multiplier.

$$\underset{C_A,C_B}{min} ||X_t^{self} - X_t^{Ci} \mid k_t||$$

where k_t: the risky multiplier in the information round t,

X_t^{self}: voter's allocation to risky option in the information round t,

X_t^{Ci}: Candidate i's allocation to risky option in the information round t.

- **Perceptional proximity:** The observed information from the information round is regarded as a signal of the candidate's risk behavior. Therefore, the voting decision depends on the minimization of the distance between the self risk preference and the perceived risk preference of the candidate.

$$\underset{C_A,C_B}{min} |r^{self} - \mu(X_t^{C_i}) \mid,$$

where $\mu(X_t^{C_i})$ is the signal about the risk behavior of the candidate i, based on the risky allocation of the candidate in information round for a given risky multiplier.

1.5 Hypotheses

Hypothesis 1.1. Individuals vote for the candidate whom they believe to have closer risk behavior to their own risk preferences.

Following the proximity theory of Downs (1957), I expect voters vote for the spatially closer candidate. Consider two candidates C_A and C_B with corresponding risk aversion rates r_{C_A} and r_{C_B}, such that

$$\mid r^j_{self} - E[r_{C_A}] \mid < \mid r^j_{self} - E[r_{C_B}] \mid .$$

Then $C_A \succeq C_B$ for the voter j since $u_j(r^j_{self}, r_{C_A}) \geq u_j(r^j_{self}, r_{C_B})$. Therefore I expect voter j vote for C_A with a higher probability,

$$Prob(V_j = C_A) > Prob(V_j = C_B),$$

where V_j: the vote of the voter j.

Hypothesis 1.2. Gender bias prevails in voting decisions of individuals.

Considering the evidence presented by Casas-Arce and Saiz (2015), the voting decision is expected to be affected by the gender of the candidate. Comparison of the baseline stage voting pattern in gender-blind (gender control group) and gender revealed (gender treatment group) elections provides information about the effect of the candidate's gender on voting decisions. Any difference in the voting behavior between gender treatment and control groups is regarded as "bias".

Following the discrimination literature, the change in the voting pattern between gender treatment and control group may occur due to the following reasons:

- Taste-based bias: Choosing a woman as a leader may give a disutility which in turn creates a tendency not to vote for the female candidate when gender is observable.

- Statistical bias: Several studies provide evidence for women being more risk-averse (see Croson and Gneezy (2009) for an extensive review of gender differences in risk attitudes). Also, other studies indicate that women are predicted as being more risk-averse (see Eckel and Grossman (2002) and Daruvala (2007)). Therefore, the gender of the candidates may provide more information about the risk attitudes which may cause a difference in gender treatment and control voting patterns.

Note that the purpose of this study is not distinguishing the type of the bias. Rather, I aim to identify the bias.

Hypothesis 1.3. There is conformity behavior under group pressure, i.e. revelation of the preferences of the group majority.

Following the literature on conformity, I expect to observe a high propensity to conform in a group decision-making process. Consider that the voter j voted for candidate A which indicates $C_A \succeq C_B$ according to the preference of voter j. Under the expected voting behavior by the first hypothesis, the presented share of votes has nothing to do with voter's own preference. Therefore any switch from the poll stage vote indicates a conformity behavior. The analysis of the direction of conformity provides information whether conformity causes a contagion of gender bias over the group.

Hypothesis 1.4. Women are more likely to conform.

In light of the results of Eagly et al. (1981), the propensity to conform among women is expected to be higher relative to men.

1.6 Empirical Strategy

With the proposed hypotheses, I am interested in estimating the impact of distance to both candidates on the voting decision. The hypothesis about the voting behavior is analyzed by the following model.

1.6.1 Historical proximity

Consider that d_i is the absolute distance between the risky allocations of the voter and the candidate i in the information round t, for a given risky multiplier k_t.

$$d_i^{historical} = ||X_t^{voter} - X_t^{Ci} \mid k_t||,$$

where i: A, B. The historical difference in distances variable, $DD_{historical}$, is formed as:

$$DD_{historical} = d_A^{historical} - d_B^{historical}.$$

1.6.2 Perceptional proximity

The perceptional distance is formed by averaging the absolute distances between the risky allocation of the voter in the strategy method round (SMR) and the voter's guess about candidate's allocation for each risky multiplier in the same round.

$$d_i^{k_{SMR}=1.5} = ||X_{SMR}^{voter} - guess^{Ci} \mid k_{SMR} = 1.5||$$

$$d_i^{k_{SMR}=2} = ||X_{SMR}^{voter} - guess^{Ci} \mid k_{SMR} = 2||$$

$$d_i^{k_{SMR}=2.5} = ||X_{SMR}^{voter} - guess^{Ci} \mid k_{SMR} = 2.5||$$

$$d_i^{k_{SMR}=3} = ||X_{SMR}^{voter} - guess^{Ci} \mid k_{SMR} = 3||$$

The perceptional distance is

$$d_i^{perceptional} = \frac{1}{4}\left(d_i^{k_{SMR}=1.5} + d_i^{k_{SMR}=2} + d_i^{k_{SMR}=2.5} + d_i^{k_{SMR}=3}\right),$$

where i: A, B. The perceptional difference in distances variable, $DD_{perceptional}$, is formed as:

$$DD_{perceptional} = d_A^{perceptional} - d_B^{perceptional}.$$

1.6.3 Empirical Model

With the formed difference in distances variables, DD, the following model is applied.

$$Pr(V=1) = \beta_0 + \beta_1 \mathbb{1}(DD>0) + \beta_2 DD{*}\mathbb{1}(DD>0) + \beta_3 DD{*}\mathbb{1}(DD<0) + \epsilon, \tag{1.1}$$

where

V : vote dummy; V =1 if the voter casts his/her vote for the candidate B, 0 otherwise,

$\mathbb{1}(DD>0)$: the indicator for positive difference in distances, $1(DD>0)$= 1 if $d_A > d_B$, i.e. candidate B has closer risky allocation, 0 otherwise,

$DD{*}\mathbb{1}(DD>0)$: the interaction term of difference in distances and the positive DD indicator,

$DD{*}\mathbb{1}(DD<0)$: the interaction term of difference in distances and the negative DD indicator.

Note that $DD{*}\mathbb{1}(DD>0)$ (and $DD{*}\mathbb{1}(DD<0)$) captures the impact of DD, i.e. relative absolute distance with reference to candidate B, when candidate B has the closer risky allocation (and when candidate A has closer risky allocation). Besides accounting for the impact of the proximity of the candidates independently, this model incorporates the relative proximity of the candidates into the voting analysis.

In order to account for the individual specific correlation due to the multiple observations within each treatment, random effects model[10] is employed through-

[10] Note that random effects model is used due to its suitability. With the fixed effects model, a great part of the variation is lost. Since Hausman test failed to reject the null for each regression, random effects model is a better option for the analysis.

out the analysis.

1.7 Results

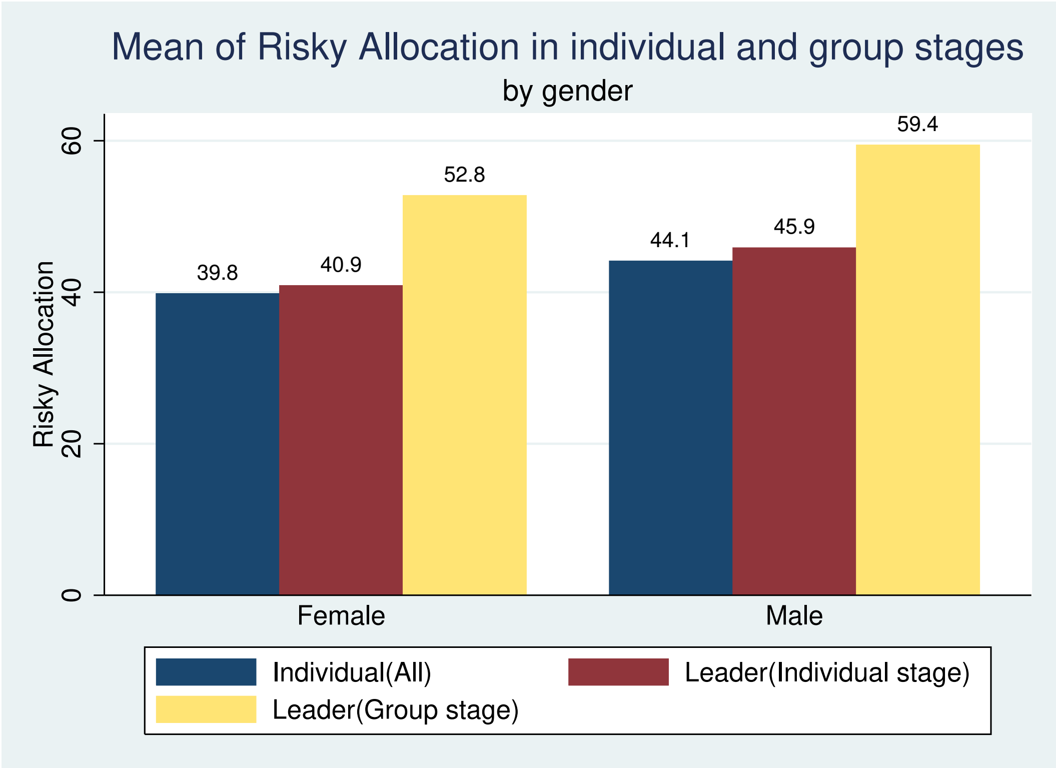
1.7.1 Investment decision

First I provide some summary statistics on the risk behavior of the participants. On average, female participants allocated 41.8 ECU, while male participants put 46.3 ECU to the risky option where the average risky multiplier is 2.26. Consistent with the literature, male participants are generally less risk-averse than their women counterparts ($p = 0.0006$ in a Mann-Whitney test). Yet, the sample is generally risk-averse since even male subjects allocate less than half of their endowment to the risky option for a high risky multiplier.

Figure 1.1 displays the allocations to the risky option in individual and group stages regarding the gender. Considering the impact of the candidates' risk behavior in the voting process, the elected male leaders are more risk-loving than their female counterparts both in the individual and group stages. More interestingly, leaders are more risk-seeking when making decisions on behalf of the group, regardless of their gender[11]. There is evidence for the difference between self-group risk preferences in the literature (see Charness and Jackson (2009), Harrison et al. (2013), Ertac and Gurdal (2012)). This result is consistent with their findings.

[11]The average risky multiplier is higher in the group stage, 2.32, where it is 2.18 in the individual stage. Although an increase is expected, the change in risky allocation between the group and individual stages is slightly higher regarding the proportions of increase in the risky multiplier and the risky allocations.

Figure 1.1: Risky Allocation in Individual and Group Stages by Gender



1.7.2 Voting behavior and the Gender Bias

One of the major goals of this paper is to understand the voting behavior. I first analyze[12] the distributions of the votes in the baseline treatment with respect to the distance and absolute distance to the voted candidate. As it is displayed in Figure 1.2a and 1.2b, the votes are accumulated in the closer distances whereas the frequency of votes diminishes as the distance increases.

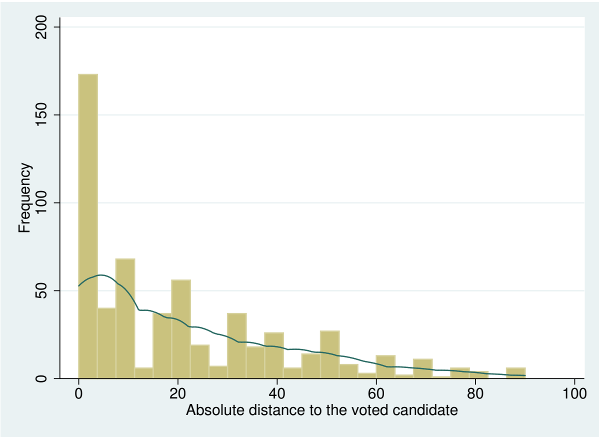
In order to identify a potential gender treatment effect on voting patterns, the distributions are compared between treatment and control groups (see Figure 1.3a and 1.3b). The distributions both in absolute distance and distance cases are significantly different between gender treatment and control groups ($p = 0.026$ and $p = 0.016$ respectively in a Mann-Whitney test). According to the results of the Mann-Whitney test, the average distance and average absolute distance to the voted candidate in the treatment group is higher. This means the voting decision

[12]Note that the complete analysis is also executed by excluding the candidates from the sample. The results are robust to the exclusion of the candidates. Please refer to Appendix A.1 for the corresponding tables and graphs.

is less sensitive to the "proximity" of the candidates when the candidates' gender information is revealed.

Figure 1.2: Vote Distribution with the Absolute Distance/Distance to the Voted Candidate

(a) Absolute distance to the Voted Candidate



(b) Distance to the Voted Candidate

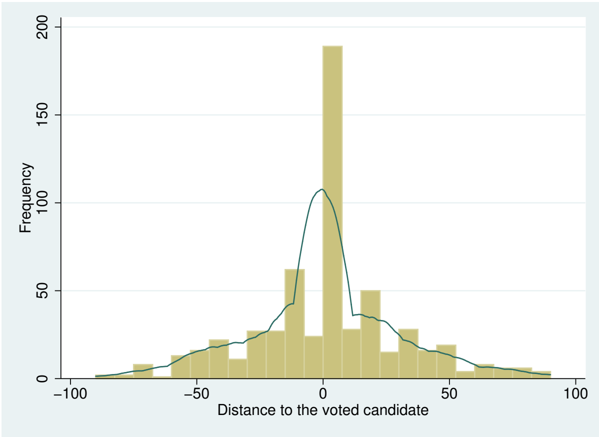
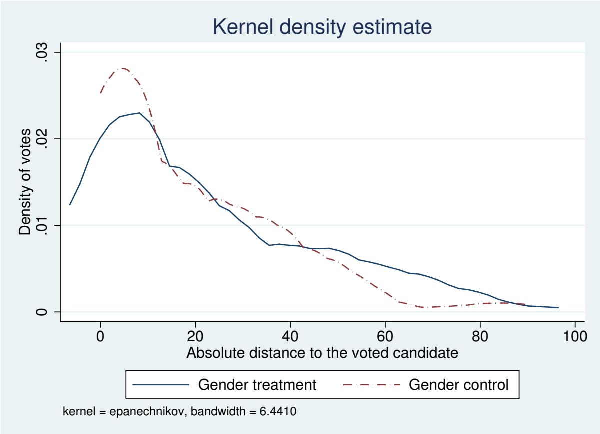
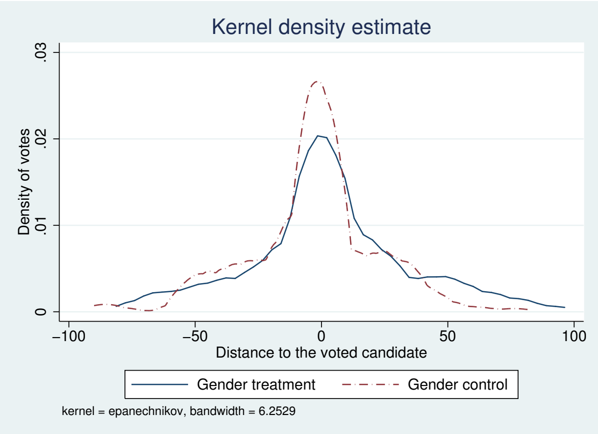


Figure 1.3: Comparison of Vote Distributions over Gender Treatment and Control Groups

(a) Absolute distance to the Voted Candidate



(b) Distance to the Voted Candidate

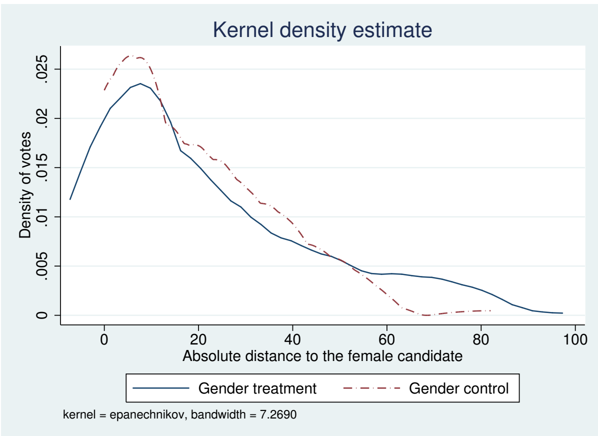


Delving deeper, the distribution analysis is decomposed as votes for female and male candidates in order to observe whether the shift in distribution is biased towards a specific gender (see Figure 1.4a and 1.4b for votes for female candidate and Figure 1.4c and 1.4d for votes for male candidate). In the votes for the female candidate, there is no significant difference in distributions across gender treatment and control groups in either absolute distance or distance cases (p= 0.427

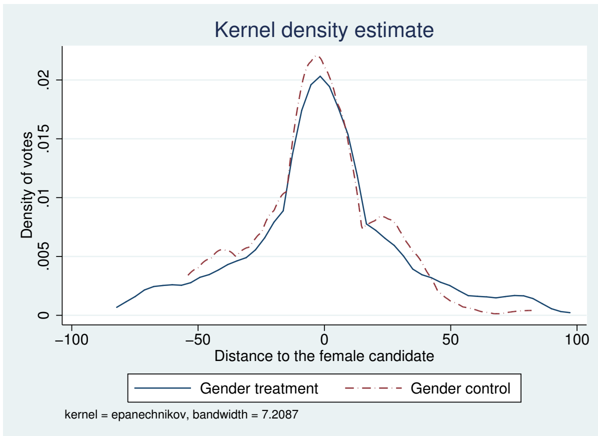
and p= 0.576 respectively in a Mann-Whitney test). However, a Mann-Whitney test confirms that the distribution of votes for the male candidate significantly differs between the treatment and control groups both in the absolute distance and the distance cases (p= 0.025 and p= 0.004 respectively). Moreover, the average distances, both in absolute and net terms, are higher in the gender treatment group. Hence the revelation of the candidate's gender shifts the vote distribution favorably for the male candidate.

Figure 1.4: Comparison of Female and Male Vote Distributions over Gender Treatment and Control Groups

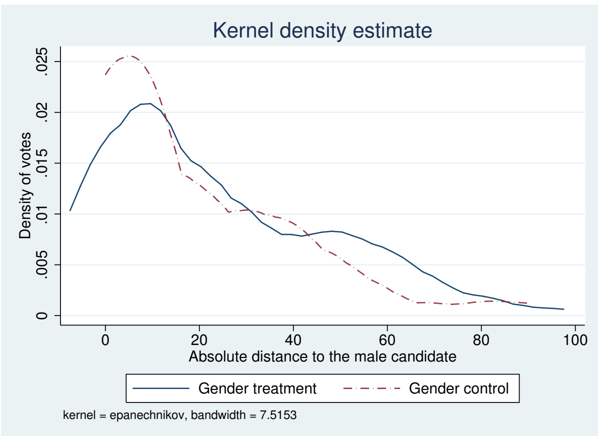
(a) Absolute distance- Votes for Female candidate



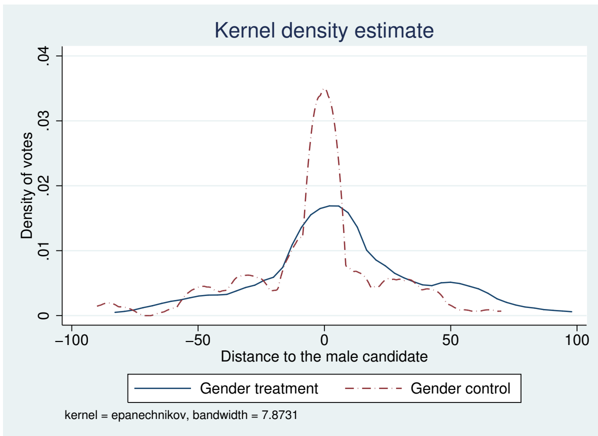
(b) Distance- Votes for Female candidate



(c) Absolute distance- Votes for Male candidate



(d) Distance- Votes for Male candidate



The same analysis is conducted for the same-sex and the opposite-sex votes. Both in the same-sex votes (the cases where a female voter votes for a female candidate and a male voter votes for a male candidate) and in the opposite-sex votes (the cases where a female voter votes for a male candidate and a male voter votes for a female candidate), the revelation of the candidates' gender creates an impact only on the male votes[13, 14]. Therefore, both female and male voters favor the male candidate when they observe the candidates' gender.

In order to check the theoretical model for voting behavior, the vote proportions are analyzed by the proximity of the candidates both historically and perceptionally. In the historical analysis, the column for the overall results in Table 1.1 shows that 74% of the votes for the male candidate are used when he is closer to the voter than the female candidate, while 70% of the votes for the female candidate are used when she has closer risky allocation.

The perceptional analysis results are similar to the historical ones. The overall results in Table 1.2 point out that voters consider the proximity of the perceived risk behavior since 70% of the votes for the male candidate and 67% of the votes for the female candidate are cast in favor of the corresponding candidate when he or she is believed to have a closer risk behavior. The results indicate that both historical and perceptional proximity of the candidate is important in the voting decision.

The proportions of votes in both analyses are compared over the gender treatment and control group in order to identify a potential treatment effect on voting pattern. According to the test of proportions, they do not differ across the gender treatment [15] ($p = 0.354$ when the male candidate and $p = 0.646$ when the female candidate is historically closer; $p = 0.357$ when the male candidate and $p = 0.369$ when the female candidate is perceptionally closer).

[13] For the graphs, please see Figure A.1 and Figure A.2 in Appendix A.1.

[14] According to a Mann-Whitney test, the results are as following: $p = 0.354$ in absolute distance and $p = 0.267$ in distance case for female to female votes; $p = 0.050$ in absolute distance and $p = 0.013$ in distance case for male to male votes; $p = 0.847$ in absolute distance and $p = 0.891$ in distance case for male to female votes; $p = 0.048$ in absolute distance and $p = 0.136$ in distance case for female to male votes.

[15] The proportions in male and female columns of Table 1.1 and 1.2 are tested by a test of proportions across gender treatment.

Table 1.1: Voting decision in the baseline with respect to Historical Proximity of the candidates

	Closer Candidate								
	Overall			Treatment			Control		
Vote	Male	Female	Equal distanced	Male	Female	Equal distanced	Male	Female	Equal distanced
Male Candidate	206	84	10	117	46	8	89	38	2
	(73.84)	(29.89)	(35.71)	(71.78)	(31.08)	(32.00)	(76.72)	(28.57)	(66.67)
Female Candidate	73	197	18	46	102	17	27	95	1
	(26.16)	(70.11)	(64.29)	(28.22)	(68.92)	(68.00)	(23.28)	(71.43)	(33.33)

Number of votes for each candidate, percentages in parentheses

Table 1.2: Voting decision in the baseline with respect to Perceptional Proximity of the candidates

	Closer Candidate								
	Overall			Treatment			Control		
Vote	Male	Female	Equal distanced	Male	Female	Equal distanced	Male	Female	Equal distanced
Male Candidate	134	64	14	75	32	12	59	32	2
	(70.16)	(32.65)	(42.42)	(67.57)	(29.91)	(54.55)	(73.75)	(35.96)	(18.18)
Female Candidate	57	132	19	36	75	10	21	57	9
	(29.84)	(67.35)	(57.58)	(32.43)	(70.09)	(45.45)	(26.25)	(64.04)	(81.82)

Number of votes for each candidate, percentages in parentheses

Table 1.3: Model (1.1)- Historical proximity

Logit (robust std. errors)			
Dependent var:	(1)	(2)	(3)
Vote for Candidate B (Male candidate)	Control	Treatment	Overall
$\mathbb{1}(DD_{historical} > 0)$	0.345***	0.461***	0.373***
	(0.083)	(0.070)	(0.078)
$DD_{historical}*\mathbb{1}(DD_{historical} > 0)$	0.002	-0.003	0.002
	(0.002)	(0.002)	(0.002)
$DD_{historical}*\mathbb{1}(DD_{historical} < 0)$	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)
Gender Treatment X $\mathbb{1}(DD_{historical} > 0)$			0.059
			(0.096)
Gender Treatment X $(DD_{historical}*\mathbb{1}(DD_{historical} > 0))$			-0.004
			(0.003)
Gender Treatment X $(DD_{historical}*\mathbb{1}(DD_{historical} < 0))$			-0.0004
			(0.002)
N	252	336	588
χ^2	35.795	39.055	75.247

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
GLS Regressions for the impact of distances to the voting behavior
Gender Treatment X $\mathbb{1}(DD_{historical} > 0)$, *Gender Treatment X* $(DD * \mathbb{1}(DD_{historical} > 0))$ and *Gender Treatment X* $(DD * \mathbb{1}(DD_{historical} < 0))$ are the interaction terms of variables with the gender treatment.

To understand the impact of proximity on the voting decision, Model (1.1) is applied both in historical and perceptional proximity contexts. Note that throughout the analysis, candidate A and B are set as the female and male candidates, respectively. The results in Table 1.3 show that in the gender control group, when candidate B (male candidate) has a closer risky option allocation historically, the voter casts a vote for him 35 percentage points more (see the estimate of $\mathbb{1}(DD_{historical} > 0)$ in column 1). When the gender of the candidates are revealed, the male candidate is voted 46 percentage points more under the same condition (see the estimate of the same variable in column 2). Consistent with the vote proportions analysis in Table 1.1, this shows that historical proximity has a significant effect on the voting decision. In order to check the effect of the gender treatment, the interaction of *Gender Treatment* with the main variables is analyzed. Even though the tendency to vote for the male candidate is 6 percentage points higher in the treatment group, it is not significantly different between gender treatment and control groups (see the estimate of $Gender\ Treatment\ X\ \mathbb{1}(DD_{historical} > 0)$ in column 3). Therefore, the gender treatment does not seem to create a significant difference in the voting decision. Both in gender treatment and control groups, the relative distance does not have a significant effect on the voting decision in the historical proximity context (see estimates of $DD{*}\mathbb{1}(DD > 0)$, $DD{*}\mathbb{1}(DD < 0)$ and their interaction with *Gender Treatment* in column 3).

Table 1.4: Model (1.1)- Perceptional proximity

Logit (robust std. errors)			
Dependent var:	(1)	(2)	(3)
Vote for Candidate B (Male candidate)	Control	Treatment	Overall
$\mathbb{1}(DD_{perceptional} > 0)$	0.246**	0.173*	0.207**
	(0.104)	(0.090)	(0.098)
$DD_{perceptional} * \mathbb{1}(DD_{perceptional} > 0)$	0.007	0.002	0.008
	(0.006)	(0.004)	(0.006)
$DD_{perceptional} * \mathbb{1}(DD_{perceptional} < 0)$	0.003	0.014***	0.005
	(0.004)	(0.005)	(0.003)
Gender Treatment X $\mathbb{1}(DD_{perceptional} > 0)$			-0.003
			(0.110)
Gender Treatment X $(DD_{perceptional} * \mathbb{1}(DD_{perceptional} > 0))$			-0.006
			(0.007)
Gender Treatment X $(DD_{perceptional} * \mathbb{1}(DD_{perceptional} < 0))$			0.007
			(0.005)
N	180	240	420
χ^2	20.995	23.444	44.199

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

GLS Regressions for the impact of distances to the voting behavior

Gender Treatment X $\mathbb{1}(DD_{perceptional} > 0)$, *Gender Treatment X* $(DD_{perceptional} * \mathbb{1}(DD_{perceptional} > 0))$ and *Gender Treatment X* $(DD_{perceptional} * \mathbb{1}(DD_{perceptional} < 0))$ are the interaction terms of variables with the gender treatment.

The results of the perceptional proximity analysis are similar to the historical proximity case. When candidate B (male candidate) is perceived as having closer risk preferences, the voter prefers to vote for him 25 percentage points more in the control and 17 percentage points more in the treatment group (see Table 1.4, column 1 and 2). In line with the results of the vote proportions analysis in Table 1.2, perceptional proximity significantly affects the voting decision. As in the historical proximity case, the tendency to vote for the male candidate is not significantly different between the gender treatment and control groups (see the estimate of *Gender Treatment X* $\mathbb{1}(DD_{perceptional} > 0)$ in column 3). Differing from the historical results, the relative distance has a significant impact, especially in the treatment group. When the female candidate is perceived as having a closer risk behavior, as DD increases, i.e. the perceived risk preferences of both candidates are getting similar, the voter casts a vote for the male candidate 1.4 percentage points higher (see the estimate of $DD_{perceptional} * \mathbb{1}(DD_{perceptional} < 0)$ in column 2). In other words, as the voter becomes indifferent between the candidates, even though the female candidate is perceived with a closer risk behavior, voters prefer the male candidate.

The results of distribution analysis indicate that the provision of the candidates' gender relaxes the proximity principle especially in favor of the male candidate. However, the proportion and regression analyses do not show any significant effect of the gender treatment. Therefore, there is no conclusive result for the gender bias. The findings can be summarized as:

Result 1.1. Voters cast a vote for the candidate who exhibits similar risk preferences. This result confirms the prediction of Hypothesis 1.1.

Result 1.2. Information on the gender of the candidates mitigates the proximity concerns on the voting decision. Yet, there is no conclusive evidence for the gender bias.

1.7.3 Conformity behavior

In the conformity treatment, the poll vote represents the true preference of the voter over candidates. Therefore, one would expect a voter not to deviate from her prior choice upon receiving the information on poll results. Yet, the results on Table 1.5 present that being in the minority according to the poll results, i.e. voted for the candidate who has a lower vote share, increases the probability to change the prior vote by 17 percentage points.

As a deeper analysis of conformity behavior, the frequency of switches is evaluated by focusing on the proximity of the candidates. The results show that 70.37% of the switches occur from the closer candidate to the further one, where 71.05% of those switching their vote are in the minority according to the poll results. Thus, the minority notion has a significant influence on the voting pattern where proximity principle becomes redundant.

In order to investigate the role of conformity on the impact of gender bias, the direction of switches among minority voters is analyzed. 49% (20 out of 41) of the "conformists" deviate to the male candidate. Due to the small number of observations and the close margin, the direction of the conformity behavior is inconclusive.

A gender-specific tendency to the conformity behavior is also analyzed. Female voters are 5.5 percentage points more likely to change their prior votes compared to their male counterparts (see second column of Table 1.5). The joint effect of *Female* and *Minority X Female* (in the third column of the same table) gives the relative tendency of female voters to conform with respect to their male counterparts. Female voters who are in the minority switch their votes 8.5 percentage points more and the difference is significant at 1% level. The results suggest women are more likely to conform.

Table 1.5: Switching behavior in Conformity Stage

Logit (robust std. errors)			
	(1)	(2)	(3)
Dependent var:	Switch	Switch	Switch
Gender treatment	0.006	0.004	0.001
	(0.023)	(0.022)	(0.022)
Minority	0.171***	0.166***	0.107***
	(0.024)	(0.023)	(0.036)
Female		0.055**	-0.008
		(0.023)	(0.039)
Minority X Female			0.093*
			(0.048)
Period	YES	YES	YES
N	588	588	588
χ^2	52.053	54.969	61.902

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
GLS Regressions for the analysis of the switch behavior
Average marginal effects are presented in the table.

Table 1.6: The factors influencing switching behavior in Conformity Stage

Logit (robust std. errors)			
	(1)	(2)	(3)
Dependent var:	Switch	Switch	Switch
Difference on Poll Shares (Δ_{share})	0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
Minority	0.176***	0.068	0.073
	(0.025)	(0.047)	(0.046)
Minority X Δ_{share}		0.003**	0.003**
		(0.001)	(0.001)
Absolute distance to the voted candidate			0.001***
			(0.000)
Period	YES	YES	YES
N	588	588	588
χ^2	52.309	55.124	58.865

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
GLS Regressions for the analysis of the switch behavior
Average marginal effects are presented on the table

Additionally, the factors which potentially affect the conformity behavior are investigated. According to the results presented in the second column of Table 1.6, any increase in the difference between the poll shares of candidates leads to 0.3 percentage points increase in probability to switch only when the subject is in minority according to the poll results. Hence, the gap on the poll shares has an impact only among minorities since it potentially increases the strength of the "minority" feeling. The third column of the same table shows that a rise in the absolute distance to the originally voted candidate causes a 0.1 percentage point increase in the probability to switch. In other words, as the candidate satisfies the proximity condition to a lesser extent, the voter is less willing to commit to the prior decision.

1.7.4 Robustness checks

1.7.4.1 Robustness of the conformity behavior

Since majority rule is applied in the election, poll results might make minority voters feel as not pivotal on the final outcome. This casts doubt on the evidence of the conformity behavior. In order to remove this confounder, an extension experiment with a change in the election mechanism is designed. Rather than majority rule, a random voter system, i.e. a randomly selected voter's final vote, is implemented for the leader appointment[16].

Before the conformity analysis, the voting behavior in the new election mechanism is checked. As in the old version, the proportion of votes is higher for the spatially closer candidate both in historical and perceptional proximity analysis (see Table 1.7). This ensures the comparability of both versions.

Although the magnitude is smaller, conformity behavior is still significant in the new version. Being in the minority according to poll results significantly increases the probability to change the prior vote by 6 percentage points (see Table 1.8). In comparison to male voters in the minority, minority female voters are 4 percentage points more likely to change their votes. However, the difference is not significant as in the results of the main experiment.. Therefore, the evidence for gender difference on the tendency to conform is not clear. The findings can be summarized as follows:

Result 1.3. Conformity behavior is a significant factor in the group voting setting. Subjects are willing to give up some proximity in order to conform with others in the group. This result confirms the prediction of Hypothesis 1.3.

Result 1.4. Female participants are more prone to conform. Yet, the result depends on the election mechanism. This result partially confirms the prediction of Hypothesis 1.4.

[16]Except election mechanism, the design of the main experiment is kept same for the comparability reasons. Note that only gender treatment version of the design is implemented in the extension experiment. For more information, please refer to Appendix.

Table 1.7: Voting behavior in Random Voter System

	Closer Candidate					
	Historical Proximity			Perceptional Proximity		
	Treatment only			Treatment only		
Vote	Male	Female	Equal	Male	Female	Equal
Male Candidate	187	74	22	118	58	20
	(80.60)	(26.06)	(50.00)	(75.16)	(28.57)	(50.00)
Female Candidate	45	210	22	39	145	20
	(19.40)	(73.94)	(50.00)	(24.84)	(71.43)	(50.00)

Number of votes for each candidate, percentages in parentheses

Table 1.8: Conformity behavior with Random Voter System

Logit (robust std. errors)			
	(1)	(2)	(3)
Dependent var:	Switch	Switch	Switch
Minority	0.059***	0.059***	0.022
	(0.023)	(0.023)	(0.025)
Female		-0.002	-0.037
		(0.022)	(0.029)
Minority X Female			0.078*
			(0.041)
Period	YES	YES	YES
N	560	560	560
χ^2	13.891	13.902	16.617

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
GLS Regressions for the analysis of the switch behavior
Average marginal effects are presented in the table.

1.7.4.2 Inconsistent behavior

In the investment task, some of the participants exhibit some non-standard behavior. As expected, the subjects are supposed to respond the increasing risky multiplier with either constant or increasing risky allocations. The participants who do not follow this pattern in the strategy method round are classified as "inconsistent". As a robustness check, the same analyses are held with dropping those inconsistent observations.

The results of the analyses, in general, are robust to the exclusion of inconsistent observations. Since those inconsistent responses are effective especially on the perceptional proximity analysis, I focus on those findings. Considering the vote proportions, voters prefer the male candidate 72% when they perceive him as having closer risk behavior, while the female candidate is voted 68% when she is perceived to have a closer risk behavior (see Table 1.9). The gender treatment does not significantly affect the final vote: the vote proportions in the gender treatment and control groups are not significantly different (p= 0.785 when the male candidate and p= 0.225 when the female candidate is perceptionally closer, in a two-sample test of proportions).

Table 1.9: Vote proportions with the Perceptional Proximity (without inconsistent observations)

	Closer Candidate								
	Overall			Treatment			Control		
Vote	Male	Female	Equal distanced	Male	Female	Equal distanced	Male	Female	Equal distanced
Male Candidate	95	46	11	54	22	9	41	24	2
	(71.97)	(31.72)	(45.83)	(71.05)	(27.50)	(56.25)	(73.21)	(36.92)	(25.00)
Female Candidate	37	99	13	22	58	7	15	41	6
	(28.03)	(68.28)	(54.17)	(28.95)	(72.50)	(43.75)	(26.79)	(63.08)	(75.00)

Number of votes for each candidate, percentages in parentheses

Unlike the results with the full sample, the perceptional proximity of the candidate has a significant impact only on the treatment group when the inconsistent observations are excluded (see Table 1.10). When voters perceive the male candidate to have a closer risk preference, they are 19 percentage points more likely to vote for him (see column 2). In the gender treatment group, when the female candidate is perceived to be closer, as the voter becomes more indifferent between the candidates, she chooses the male candidate 12 percentage points more (see the estimate of $DD_{perceptional} * \mathbb{1}(DD_{perceptional} < 0)$ in column 2). As in the general findings, the gender treatment has no significant impact on the voting decision (see the estimate of $Gender\ Treatment\ X\ \mathbb{1}(DD_{perceptional} > 0)$ in column 3).

Table 1.10: Perceptional Proximity- Model (1.1) (without inconsistent observations)

Logit (robust std. errors)			
Dependent var:	(1)	(2)	(3)
Vote for Candidate B (Male candidate)	Control	Treatment	Overall
$\mathbb{1}(DD_{perceptional} > 0)$	0.200	0.192**	0.164
	(0.135)	(0.098)	(0.118)
$DD_{perceptional}*\mathbb{1}(DD_{perceptional} > 0)$	0.009	0.003	0.008
	(0.007)	(0.004)	(0.007)
$DD_{perceptional}*\mathbb{1}(DD_{perceptional} < 0)$	0.003	0.012**	0.004
	(0.004)	(0.005)	(0.003)
Gender Treatment X $\mathbb{1}(DD_{perceptional} > 0)$			0.054
			(0.131)
Gender Treatment X $(DD_{perceptional}*\mathbb{1}(DD_{perceptional} > 0))$			-0.006
			(0.008)
Gender Treatment X $(DD_{perceptional}*\mathbb{1}(DD_{perceptional} < 0))$			0.008
			(0.005)
N	129	172	301
χ^2	14.158	20.689	34.915

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
GLS Regressions for the impact of distances to the voting behavior
Gender Treatment X $\mathbb{1}(DD_{perceptional} > 0)$, *Gender Treatment X* $(DD_{perceptional}*\mathbb{1}(DD_{perceptional} > 0))$ and *Gender Treatment X* $(DD_{perceptional}*\mathbb{1}(DD_{perceptional} < 0))$ are the interaction terms of variables with the gender treatment.

1.7.5 Motivation of Conformity Behavior: Taste-based or Social Learning

With the evidence, conformity behavior can be regarded as an important component of the group decision-making. In the current design, this behavior can only be motivated by a taste for conformity. However, social learning that is induced by an election result may also lead to the conformity behavior. Under an information asymmetry across voters, convergence to the majority may occur due to a voter updating her belief about candidates with the poll result. For example, the voter who has less information about the candidates (compared to the information that other committee members have) may regard the poll result as an information of candidates. As a result, she may change her prior vote simply due to updating her belief of the candidates.

To investigate the phenomenon, *Imprecision treatment*, which creates information asymmetry across voters, is introduced in the extension experiment. Rather than a single round, voters observe multiple information rounds from the individual stage. The observed number of rounds, Precision of Information [17], is varied across voters and subjects are informed accordingly (see Figure B.12 and Figure B.13 for the screens of subjects who receive different precision of information).

The propensity to conform is greater among the voters who receive information with low precision: switch rates are 22% for low and 15% for high type minority voters (see Table 1.11). However, the difference is not significant (p=0.228 according to the test of proportions). The motivation of observed conformity behavior in imprecision treatment might be both taste and learning. The sole effect of learning is analyzed by comparison of conformity behavior in the conformity and the imprecision treatments. As it is displayed in Table 1.12, the social learning is not a significant factor on the observed conformity. Therefore, rather than belief, preference mechanism is more likely to be dominant in conformity behavior. In summary, the findings are:

Result 1.5. Conformity behavior is more likely to be motivated by taste than social learning.

Table 1.11: Switch Rates among Minority with the Precision of Information

	Precision of Information							
Switch	20%	40%	60%	80%	TOTAL	Low (20% &40%)	High (60% & 80%)	TOTAL
NO	36	33	48	33	150	69	81	150
	(76.60)	(80.49)	(84.21)	(86.34)	(81.97)	(78.41)	(85.26)	(81.97)
YES	11	8	9	5	33	19	14	33
	(23.40)	(19.51)	(15.79)	(13.16)	(18.03)	(21.59)	(14.74)	(18.03)
TOTAL	47	41	57	38	183	88	95	183

Number of votes for each candidate, percentages in parentheses

[17] Precision level refers to the number of information rounds observed by the voter. i.e. If the precision of information is 80%, corresponding voter observes 8 out of 10 individual stage rounds. Higher precision level means more information about both candidates. Note that four precision levels, 20%, 40%, 60% and 80%, are employed in the design. For more information, please refer to Appendix.

Table 1.12: Comparison of Switching Behavior among Minority Voters in Conformity and Imprecision Stages

Logit (robust std. errors)	
	(1)
Dependent var:	Switch
Imprecision Treatment	0.023
	(0.053)
Period	YES
N	360
χ^2	2.124

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
GLS Regressions for the analysis of the switch behavior
Average marginal effects are presented in the table.
Imprecision Treatment = 1 if Imprecision treatment
0 if Conformity Treatment

1.8 Conclusion

The under-representation of women in top positions is a well-known problem. The literature focuses more on the supply-side explanations such as gender-based differences on individual preferences in explaining the phenomenon. However, the leader appointment process is an important component of the issue in investigating the demand-side of the phenomenon.

This study provides a comprehensive explanation how people vote and whether candidate's gender affects it, and how and with what motive the group influences the voting decision of individuals. The design of the paper offers a novel approach to the literature in two ways. First, the factors involved in the voting decision are identified in a two-candidate election setting. Second, conformity behavior is regarded as an influencing group dynamic on the voting decision.

The results suggest that voters cast their votes for the candidate who presents similar risk preferences to their own. The information on the candidates' gender relaxes the proximity concerns in the voting decision. Yet, there is no conclusive evidence for the gender bias. Regarding the findings, conformity behavior is an important determinant of group voting in which "minority" feeling is detected as one of the driving factors in convergence. Although it depends on election mechanism, there is evidence for the female being more prone to conform. As the mechanisms that induce the conformity behavior, taste-based and social learning motivations are investigated. Rather than learning, taste for conformity is more likely to be dominant on conformity behavior.

Although the results do not provide conclusive evidence for the gender bias, the role of gender information seems to influence the voting decision. Regarding the evidence for the conformity behavior in group decision-making, the multiplier effect of conformity is likely to be relevant in the workplaces. Policy makers and political parties should be more careful to ensure gender bias does not affect group decision making. A gender-blind voting with a restriction on the information exchange in the committee would allow women to compete against men in an unbiased environment.

Several studies highlight risk-taking as a predominantly male characteristic. In my design, there should not be any disadvantaged group in leader appointment since the voting decision is based on the subjective risk preferences and proximity of the candidate on the risk domain. Yet, I cannot rule out a potential perception of participants about risk-taking regarded as a leader characteristic. This study clearly provides evidence for the risk-based settings.

Chapter 2

Self and Group Risk Preferences of Elected Leaders: An Experimental Approach

Abstract

Existing studies that compare the risk behavior for oneself and for others indicate a shift in risk preferences, but the evidence is mixed on the direction of this shift. Moreover, the mechanism driving this change in preferences remains unexplored. This experimental study aims to uncover this mechanism by investigating individual and (on behalf of) group risk behavior of an elected leader. I focus on two motivations that may cause the risk in leader's risk preference: a "leadership effect", that is created by the competition, i.e. election, and the sense of responsibility of the leadership status, and a "group concern" of the leader. Elected leaders significantly increase their allocation to risky option when acting on behalf of a group compared to their individual allocations. The results provide support for both motivations, yet the concern for the group's preference induces a stronger effect. The perceptions of elected leaders on their competitors indicate that leaders who believe being more risk-seeking than their unsuccessful competitor take more risk when acting for the group. The leadership effect creates a small change in risk behavior. The motivation for the shift in risk attitude seems driven by the leadership effect among male leaders, whereas it is due to concerns about the group's preference among female leaders.

Keywords: Risk, Group decision-making, Leadership, Emotions, Altruism, Gender, Lab experiments.

JEL Classification: C91, C92, D71, D81, D91.

2.1 Introduction

A large group of friends is trying to pick a restaurant for dinner. Everyone states their own preference according to his or her taste. In most groups, one friend either volunteers or is delegated to take the action on behalf of the group. Does she choose her favorite restaurant in town or favor the preferences of other group members?

In modern society, there are various contexts similar to this case which require individuals to take risky decisions for the well-being of others. The question of whether the decision-maker (DM) deviates from her individual preference in such settings has been investigated by several studies in economics and psychology. In different experimental contexts, Chakravarty et al. (2011), Agranov et al. (2014) and Sutter (2009b) find that subjects tend to take more risk on behalf of others than themselves, while Charness and Jackson (2009), Reynolds et al. (2009), Bolton and Ockenfels (2010) and Ertac and Gurdal (2012) find evidence for more risk-aversion. Moreover, there is limited evidence for the mechanism behind this risk shift.

The present experimental study aims to uncover this mechanism by examining elected leaders'[1] risk preferences in individual and group settings. In particular, I focus on two potential motives which may cause a difference between self and group risk preferences: a "leadership effect", the effect of being an elected leader, and a "group concern" stemming from the group's expectation from the leader. In the former motivation, leadership effect contains the effect of two elements: competition, i.e. election, and a sense of responsibility. Competition may trigger a risk-seeking behavior which causes a risky-shift on the DM's risk preference. In contrast, leadership naturally brings a sense of responsibility which may lead to a cautious behavior. In the latter motivation, the DM may take into account her prediction of the group's expectation. She may evaluate the election outcome as a signal about the risk behavior that the group expects from the DM. As a

[1]Throughout the paper, the term *leader* refers to the decision-maker of the group.

result, when deciding on behalf of the group, the DM may behave based on her perception of the group's desire.

In a setting where a group member is appointed as the DM, Ertac and Gurdal (2012) find that subjects tend to take less risk on behalf of the group than themselves. If the delegation is based on a competition, would it affect the DM's risk behavior in a different way? Although being in a setting where the DM does not bear the risk, Agranov et al. (2014) show that competition increases risk-taking. Based on their finding, if the DM is delegated as a result of an election, the DM may exhibit more risk-seeking behavior.

Similar to Ertac and Gurdal (2012), Charness and Jackson (2009) also find a cautious shift in the DM's risk attitude. They motivate the cautious shift with a sense of responsibility. Therefore, the sense of responsibility coming along with leadership position may cause DM to be more risk-averse on behalf of the group.

Following the argument of Charness and Jackson (2009), a sense of responsibility may also lead to a more risk-seeking behavior if the DM believes that the group prefers a more risk-loving leader. Daruvala (2007) and Chakravarty et al. (2011) show that DM uses a combination of her own risk preferences with her predicted risk preference of the person or the group that is affected by DM's decision. This indeed indicates that the expectation of the group may be a motivation for the DM on the group risk context.

To test these two motivations behind a potential risk shift, I conduct a lab experiment that involves making a risky decision for the subject and on behalf of the group which affects the payoff of all group members including herself. Across treatments, at the election stage, I manipulate the revelation of information about the risk preferences of candidates. The information-blind election is designed to investigate the effect of being the "winner" of the election. In the information-revealed election setting, the candidate information accompanied with the election outcome provides the signal for the socially preferred risk preference in the group. Therefore, the information-revealed election analyzes whether leaders respond to this information. I collect data on leaders' perceptions of the risk preferences of their unsuccessful competitors. The perception data is used to create a proxy for leaders' belief about the desired risk attitude in the group. Moreover, I check the gender difference in the shift of risk preferences and whether motivations differ between female and male leaders.

Consistent with the evidence provided in the literature, leaders tend to exhibit different risk attitudes when making choices for themselves and on behalf of the group. They are more risk-seeking when deciding on behalf of the group in comparison to deciding only for themselves. The results provide support for both motivations, yet the concern for the group's preference induces a stronger effect. Meeting the expectations of group members seems the main driver of the observed behavioral change. The perceptions of elected leaders about their competitors indicate that leaders who believe being more risk-seeking than their unsuccessful competitor take more risk when acting for the group. Therefore, the change of risk attitude seems to occur due to the other-regarding concerns of the leader wanting to comply with the group's desire. The leadership effect caused by competition creates a small change in risk preference. Moreover, the motivation for the shift in risk attitude is based on leadership effect among male leaders, whereas it is driven more by other-regarding concerns for the group among female leaders. The qualitative evidence also supports the aforementioned findings.

The rest of the paper proceeds as follows: section 2.2 provides a review of the related literature, section 2.3 explains the design and procedures of the experiment and states the hypotheses of the study, section 2.4 presents the results and section 2.5 concludes the paper.

2.2 Related Literature

In daily life, some settings allow or oblige a DM to reach a decision for someone else or for a group where the DM is not a member such as policymakers, CEOs and portfolio managers. In other contexts, the DM is part of the group and bears the risk with other group members such as when the DM is a head of household or a politician representing voters. Studies in both contexts consistently provide evidence for a shift in risk behavior. However, the evidence for the direction of the shift is mixed. In the context where the DM decides for other(s),Chakravarty et al. (2011), Pollmann et al. (2014) and Agranov et al. (2014) find that DM taking more risk on behalf of other(s) than herself, while Kvaløy et al. (2014) and Reynolds et al. (2009) find evidence for more risk-aversion. In the latter context where DM decides on behalf of other(s) as well as herself, Sutter (2009b) finds a risky shift, whereas, Charness and Jackson (2009), Bolton and Ockenfels (2010), Ertac and Gurdal (2012) and Pahlke et al. (2015) observe a cautious-shift. Yet, there are very few studies exploring the mechanism behind the shift in risk behavior. This study contributes specifically to the self-group risk behavior literature by focusing on the mechanism behind the shift.

While Ertac and Gurdal (2012) find a cautious shift in a similar setting, the leaders tend to become more risk-seeking when they decide on behalf of the group in Ozdemir (2017). What may have created this difference in the direction of the risk change? There are two main differences: the appointment mechanism of the leader and the timing of the risk decision for the group. Ertac and Gurdal (2012) use a setting where the leader is appointed (not elected) among the candidates who prefer to be leaders and the leader makes the risky decision for the group before the actual appointment. In Ozdemir (2017), on the other hand, the leader is elected by the group from two exogenously selected candidates and makes the decision on behalf of the group after the election outcome is announced. Therefore, being an elected leader or an appointed leader may potentially affect the preferences for the group in a different way. A "leadership effect" as a result of being elected may be a potential motive behind the observed change in a leader's risk preference.

In an experimental study based on portfolio manager-investor setting, Agranov et al. (2014) find that competition increases risk-taking of the portfolio manager. They also investigate the case where portfolio manager shares the risk with the investor. Although risk sharing substantially decreases risk-taking, it does not reduce it completely. The evidence indicates that competition induces risk-seeking behavior. The present study adds to this strand of the literature by studying the effect of the election on risk behavior of the DM when she is part of the group.

Why does the competition trigger a more risk-seeking behavior? One explanation may be the effect of power and emotions created by competition on the risk behavior. In the psychology literature for power, it is presented that emotions influence judgments and decisions which in turn affect the risk-taking behavior. Lerner and Keltner (2001) show that emotions like happiness and anger affect perceptions of risk and raise subjective probabilities of positive outcomes. Following their finding, Anderson and Galinsky (2006) find that people with a higher sense of power have more optimistic risk perceptions and powerful individuals prefer risky behavior due to these optimistic perceptions. Anderson and Galinsky also find that a sense of responsibility reduces the effect of power on risky behavior. Therefore, a psychological effect created by being an elected leader may influence the risk perceptions and result in leaders deviating from their own risk attitude. Yet, these studies are based on subjects' general happiness and perceptions for risk, sense of power and responsibility related to their occupations in their daily life. Although they are good proxies for the behavior of powerful individuals, they do not provide clear evidence. Emotions, rather, seem to influence risk perceptions when there is an external shock altering the emotional state of the individual. Also, a significant correlation between general sense of power and risk attitudes cannot provide a clear causal inference. This paper aims to fill this gap by satisfying these terms in the analysis of power/leadership effect on risk behavior.

While the psychology literature predominantly argues that power causes more risk-seeking behavior, Ertac and Gurdal (2012) and Charness and Jackson (2009) find a cautious shift in leader's risk attitude. In a stag-hunt game where DM decides on behalf of himself as well as his partner, Charness and Jackson (2009) observe DM playing more cautiously in the pair setting than in an individual baseline. They explain the cautious shift as a sense of the DM's responsibility. Similarly, Pahlke et al. (2015) provide evidence for a cautious shift under the responsibility of deciding on behalf someone else. One of the main contributions of this paper is the investigation of responsibility effect in the presence of competition.

In the setting of Charness and Jackson (2009) where the DM does not have information on risk preference of the person who is affected by the decision, they define responsibility as the leader/DM making cautious decisions on behalf of someone/a group of people in comparison to self-decision. Yet, a sense of responsibility may also induce a leader to have more risk-seeking behavior if she believes that the group expects her to do so. In a team decision-making context, Sutter (2009b) shows a risky shift between self-team decisions where each member of the team sequentially becomes the DM of the team. In the treatment where DM receives non-binding messages from other team members, the DM becomes even more risk-seeking than in the no-message case. In a similar respect, Harrison et al. (2013) find a cautious shift when the DM has information about the group's risk preference, while they found no difference in self-group risk preferences in the no information case. These findings indicate the importance of the group's preference in the group decision-making process. Therefore, when evaluating the difference in self-group risk behavior, it is essential to understand the leader's perception of the risk preference of the group.

In the context where the DM decides for others, Daruvala (2007) and Chakravarty et al. (2011) investigate the role of the leader's beliefs in her choice on behalf of the group. The results of both studies indicate that the leader/DM uses a combination of her own risk preference and her predicted risk preference of the targeted group. Although the choice of DM who is part of the group involves a risk for the DM herself as well as a social risk, the factors influencing the DM's decision may be similar.

Moreover, economics and psychology literature consistently show women being more risk averse in many contexts (see Croson and Gneezy (2009), Eckel and Grossman (2008) and Charness and Gneezy (2012) for an extensive review of gender and risk preference studies). However, the literature focusing on the gender difference in self-other and self-group risk preferences is scarce. Ertac and Gurdal (2012) focus on this aspect, finding that women are more risk averse both in the individual and in the group context.

Arch (1993) argues that the gender difference in risk behavior is formed due to different motivations between genders rather than differences in ability or eagerness to perform the task well. A similar argument is valid for the differences in self-group risk preferences: if women and men have different motivations causing a difference in their risk behavior, then their motivations when making risk-involving decisions for others/group may differ, too. One of the main explanations of Croson and Gneezy (2009) for the gender difference in risk behavior is emotions. Departing from Loewenstein et al. (2001) "risk as feelings" theory, they argue that the differences in emotional reactions to risky situations may be one of the reasons creating the gender differences. Following their argument, if women are more emotionally responsive to risk conditions, their emotional reaction to risk in social contexts may be stronger as well. Additionally, a vast majority of studies agree on women being more other-regarding (Eckel and Grossman (1998), Andreoni and Vesterlund (2001) and Croson and Gneezy (2009)). If this is the case, then women may also respond more to group's preferences. In consequence, different motivations may lead to a difference between female and male risk behavior for the group. The present study aims to disentangle this conjecture.

2.3 Experimental Design

The experimental design aims to investigate the motivations that lead to a change in risk preferences of the elected leaders. The group decision making as an elected leader and the information availability to candidates are the treatment variables which are applied within-subject. The control treatment in which each subject decides for herself is used as a benchmark for detecting the behavioral change when elected.

I employ the Gneezy and Potters (1997) risk elicitation task. Each participant is initially endowed with 100 tokens[2] to allocate between two options: a riskless option (bond) and a risky option (stock). The risky multiplier, k_t, is randomly chosen from [1.5,3] for each round t and is applied as a common multiplier for all subjects. The amount allocated to the risky option, X_2, yields $k_t X_2$ with probability 0.5 and 0 otherwise, whereas the riskless option secures the allocated amount. So the expected payoff is:

$$E[U_t|k_t] = (100 - X_2) + [0.5(k_t X_2) + 0.5(0)]\,,$$

where k_t denotes the risky multiplier, at round t, $k_t \, \epsilon \, [1.5, 3]$.

The experiment consists of two stages: the individual stage and the group stage. The individual stage is the baseline treatment which serves as a benchmark for detecting the behavioral change when elected. In the individual stage, participants perform the risk task for themselves: for randomly appointed risky multipliers in the first 9 rounds and for a list of risky multipliers composed of 1.5, 2, 2.5 and 3 in round 10. The single risky-multiplier decision case is repeated in order to collect data on individual risk preferences which is utilized as the "candidate information" in the group stage election. The strategy method is additionally used in round 10 of the individual stage, which is referred to as *the strategy method round*, for direct comparison of individual decisions of the leaders with their group

[2]The exchange rate is set as 1 token= £0.04.

decisions and their guesses for their unsuccessful competitors.

The group stage is the part of the experiment in which all within-subject treatments are applied. 7-person groups are randomly formed at the beginning of each round. Stranger matching protocol is used in grouping in order to prevent reciprocity motivations. In each round, 2 candidates, 1 Female and 1 Male, are exogenously appointed [3]. In order to guarantee that each group has at least one female and one male member for candidacy, stratified randomization by gender is applied in the group formation. The appointed candidates are represented anonymously as candidate A and B. Also one of the individual stage rounds is randomly selected as the information round. On the voting screen, the risky option allocations of both candidates (for the corresponding risky multiplier) in the information round is displayed (see Figure B.4 for the voting screen). With the given information, voters cast their votes for the desired candidate. The elected leader performs the investment task on behalf of the group. The payoff of all group members is determined by the leader's decision.

Additionally, all participants are asked to guess the risky option allocations of both candidates in the strategy method round. The guess of the leader about her unsuccessful competitor provides information about how the leader associates risk preferences with her role and how she evaluates the election outcome in terms of expected risk preference (see Figure B.7, B.8 and B.10 in Appendix B.3 for guess screens of non-candidates and candidates in different treatments). Note that guesses are monetarily incentivized. The guess is regarded as correct when it is in the ∓ 3 interval of the actual allocation of the candidate. Each correct guess for each candidate is rewarded with a 5-token bonus.

The group stage contains the main treatment manipulation: [4]

- **NO INFO treatment:** Two candidates are exogenously appointed. While

[3] Note that the appointment procedure is random.

[4] There are five treatments in total implemented during the experiment. Due to the scope of the paper, I only focus on aforementioned treatments. The instructions for each treatment is explained at the beginning of the corresponding period. Also, the excluded treatments were always applied after the focused ones. Therefore, the results are not affected due to this exclusion.

non-candidates observe the information about both candidates, candidates observe their candidacy status and whether they are elected as a leader without receiving any information about their competitors (see Figure B.4 and B.5 in Appendix B.3 for Non-candidate and Candidate screens in the NO INFO treatment). The leader makes the investment decision on behalf of the group.

- **INFO treatment:** The setting is the same as in the NO INFO. Unlike in the NO INFO treatment, candidates observe the information about their competitors' risk preference and gender (see Figure B.4 and B.9 in Appendix B.3 for Non-candidate and Candidate screens in the INFO treatment).

Note that to collect more data from participants, each treatment is played for 3 rounds. The payment was done based on one of the randomly chosen individual and group stage rounds. The payment scheme has implemented in order to eliminate wealth effects.

2.3.1 Procedures

The experiment was programmed using z-Tree experimental software (Fischbacher (2007)) and implemented at the ESSEXLAB, University of Essex, in June 2015. The data collected from 189 subjects in total who were recruited via hroot (Bock et al. (2014)). The participants of the study described in Ozdemir (2017) are excluded from the recruited sample. The experiment was conducted in 9 sessions. The sample consists of 95 female and 94 male participants, who are mostly undergraduate students. Since the student population is diverse in various terms at the University of Essex, a survey is conducted at the end of each session to collect demographic data such as age, ethnicity, major, siblings and annual personal income. Also, qualitative data is collected about the motivations of a potential risk attitude change (refer to Appendix B.3.3 for the survey) The duration of the sessions was 70 minutes, and subjects earned £8.10 on average, including a show-up fee of £2.50[5]. For the purpose of increasing the comprehension level of the task, a quiz was conducted before the experiment[6] (see Appendix B.3.2).

2.3.2 Hypotheses

With the evidence provided in Ozdemir (2017), I expect participants to vote for the candidate who exhibits similar risk behavior. For the other treatments, my hypotheses are as following:

Hypothesis 2.1. There is a shift in risk behavior between individual and (on behalf of) group decisions.

Based on the findings in the literature, I expect a shift in the risk behavior of leaders between their individual and group decisions. Any change in the investment decisions of leaders between their individual stage and group stage allocations will indicate a risk shift and its direction.

Hypothesis 2.2. The risk shift is motivated by the leadership effect.

If there is any shift between individual and group decisions, the motivation is observable with the design. Any difference between the individual stage and the NO INFO stage allocations indicate the effect of being an elected leader. The shift may occur in different directions by the effect of election and a sense of responsibility. As Agranov et al. (2014) found in the self-other risk behavior context, competition may trigger a more risk-seeking behavior which would cause a risky-shift. On the other hand, regarding the evidence presented by Charness and Jackson (2009) and Pahlke et al. (2015), a sense of responsibility may lead to a cautious shift.

Hypothesis 2.3. The risk shift occurs due to the leader's concern on meeting the expectations of the group members.

[5] Earnings ranged between £4.10 and £17.40, including the show-up fee.

[6] The responses of subjects to quiz questions do not affect subjects' payoffs. After their response, subjects observed the detailed solution of each question.

Based on the findings of Daruvala (2007) and Chakravarty et al. (2011), the prediction about the group's expectation may cause the shift in risk preference of the DM. In the INFO treatment, the observed information and the election outcome will provide the signal to the leader about the preferred risk behavior in the group. Therefore, the change in risk attitude of the leader in the INFO stage may arise due to a leadership effect as well as a concern about group's expectations from the leader. The comparison of the change in risk behavior of the leader in the INFO and the NO INFO stages provide the sole effect of the revealed candidate information and, therefore, the concerns of the leader about group members' expectations.

2.4 Results

The main focus of this paper is to determine whether the risk preferences in a decision for self and for a group of people differ and to identify potential mechanisms creating this difference. In what follows, I first analyze a potential change in the risk attitudes of leaders and then focus on two motivations behind this preferential change. Following the quantitative analysis, Section 2.4.3 provides a supporting qualitative evidence for understanding the mechanism.

2.4.1 Analysis of Leaders' Risk Attitude in Individual and Group Stages

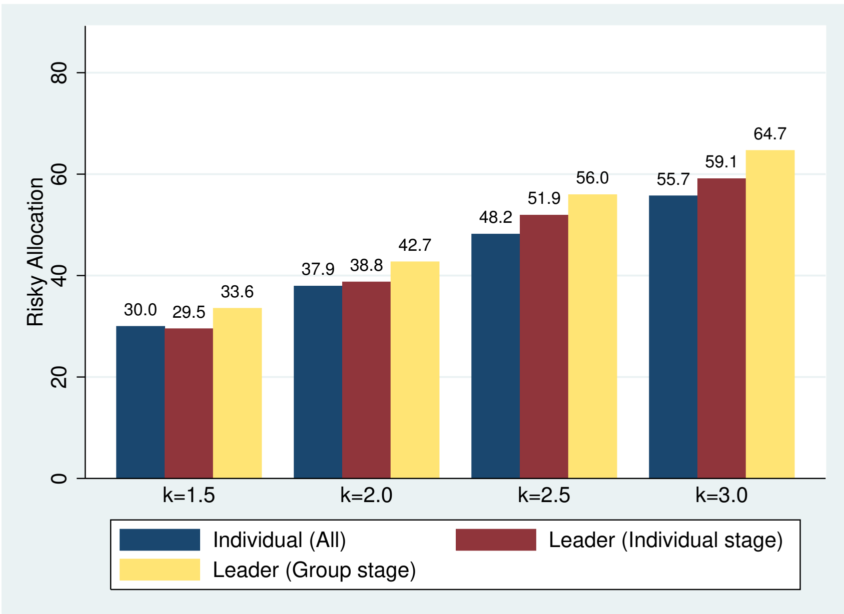
In order to observe a change in the risk behavior of the elected leaders, the risky allocations are analyzed over individual and group stages. Figure 2.1 displays the allocations to the risky option for different return levels in the individual and the group stages. As expected, participants respond to the risky multiplier (k) both in the individual stage and in the group stage: the amount invested in the risky option is higher for the high multipliers. Elected leaders allocate significantly higher amounts to the risky option in the group stage with respect to their individual stage allocations for each return level (p= 0.046 in k=1.5, p= 0.075 in k=2.0, p=0.114 in k=2.5 and p= 0.055 in k=3.0 in a Mann-Whitney test). The regressions also confirm that the leaders allocate 3 tokens higher in the group stage for each return level (see Table 2.1)[7]. These results are consistent with the findings of Ozdemir (2017). The findings can be summarized as:

Result 2.1.

(a) There is a shift in risk attitudes of elected leaders. This result confirms the prediction of Hypothesis 2.1.

(b) The elected leaders become more risk-seeking when deciding on behalf of the group.

Figure 2.1: Risky Allocation in Individual and Group Stages (All Treatments)



[7]The regressions are also conducted with the control whether the leader has lost in a previous election. The results are robust to the inclusion of controls.

Table 2.1: Comparison of Risky Allocation of Leaders in Individual and Group Stages for Different Return Levels (All Treatments)

Dependent var:	(1)	(2)	(3)	(4)
Risky allocation	k=1.5	k=2.0	k=2.5	k=3.0
Group stage	3.443**	3.173**	2.364*	3.885**
	(1.532)	(1.370)	(1.211)	(1.589)
Being leader more than once	3.713	4.138	6.328	6.762
	(3.686)	(3.519)	(3.928)	(4.307)
Constant	37.653***	46.842***	57.375***	64.578***
	(5.823)	(5.551)	(6.174)	(6.788)
Session	YES	YES	YES	YES
N	345	345	345	345
χ^2	20.096	24.320	15.518	23.070

Standard errors in parentheses
GLS Regressions for the impact of group period on riskiness
* $p < .1$, ** $p < .05$, *** $p < .01$

2.4.2 Motivation Behind the Risk Attitude Change: A Leadership Effect or A Group Concern

In the current design, the observed change in risk behavior may be induced by two factors: the leadership effect and the concern to satisfy the group's expectation. In order to investigate these two motives, the change in the risk behavior of leaders is analyzed for NO INFO and INFO treatments separately.

In the NO INFO treatment analysis (see Figure 2.2), the results show that there is no distributional shift on risky allocations at any of the return levels (p= 0.190 in k=1.5, p= 0.275 in k=2.0, p=0.671 in k=2.5 and p= 0.435 in k=3.0 in a Mann-Whitney test). As it is displayed in Table 2.2, there is a small significant increase in the means of the group stage allocations at k= 1.5 and k= 2.0 cases. Therefore, the leadership effect seems to induce a small change only at low-return levels. Also, as the results show a risky shift in leader's risk behavior, the competition seems to have a significant effect on the risk behavior of the leader.

Result 2.2.

(a) The leadership effect creates a small change in the magnitude of the leaders' risky allocations. This result confirms the prediction of Hypothesis 2.2.

(b) Competition seems to have a significant effect on the risk behavior of the leader.

Figure 2.2: Risky Allocation in Individual and Group Stages (NO INFO Treatment)

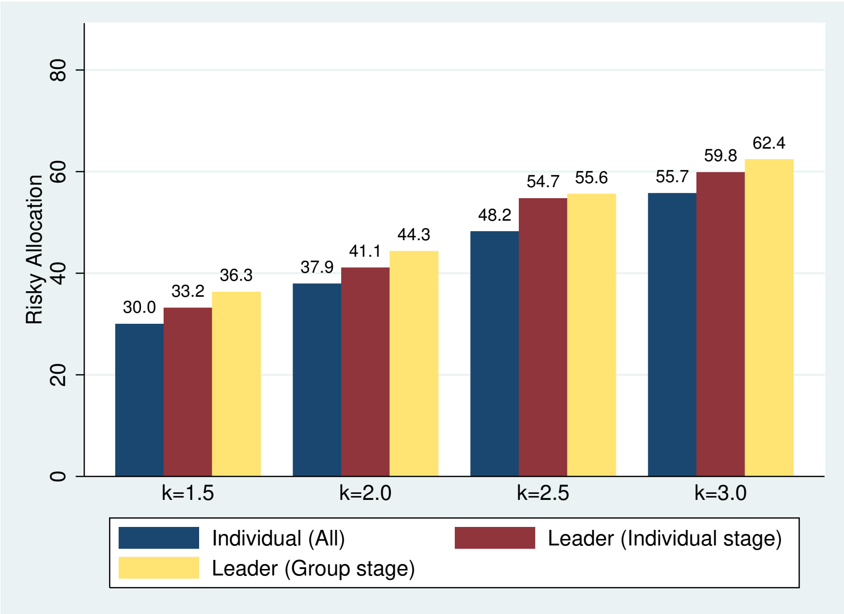


Table 2.2: Risky Allocation of Leaders in Individual and NO INFO Stages for Different Return Levels

Dependent var:	(1)	(2)	(3)	(4)
Risky allocation	k=1.5	k=2.0	k=2.5	k=3.0
Group stage	2.710*	2.555**	0.330	1.335
	(1.479)	(1.261)	(1.042)	(1.527)
Being leader more than once	3.902	6.429	5.429	9.427
	(6.023)	(5.348)	(6.023)	(6.537)
Constant	42.101***	49.397***	60.470***	63.221***
	(7.949)	(7.051)	(7.903)	(8.618)
Session	YES	YES	YES	YES
N	193	193	193	193
χ^2	13.312	16.863	4.669	13.381

Standard errors in parentheses

GLS Regressions for the impact of group period on riskiness

* $p < .1$, ** $p < .05$, *** $p < .01$

Moreover, the INFO treatment analysis points out an increase in risky allocations for high-return levels (see Table 2.3 and Figure 2.3). A Mann-Whitney test, additionally, confirms that the distributions of risky allocations are significantly different at high-return levels (p= 0.115 in k=1.5, p= 0.135 in k=2.0, p=0.098 in k=2.5 and p= 0.061 in k=3.0).

Figure 2.3: Risky Allocation in Individual and Group Stages (INFO Treatment)

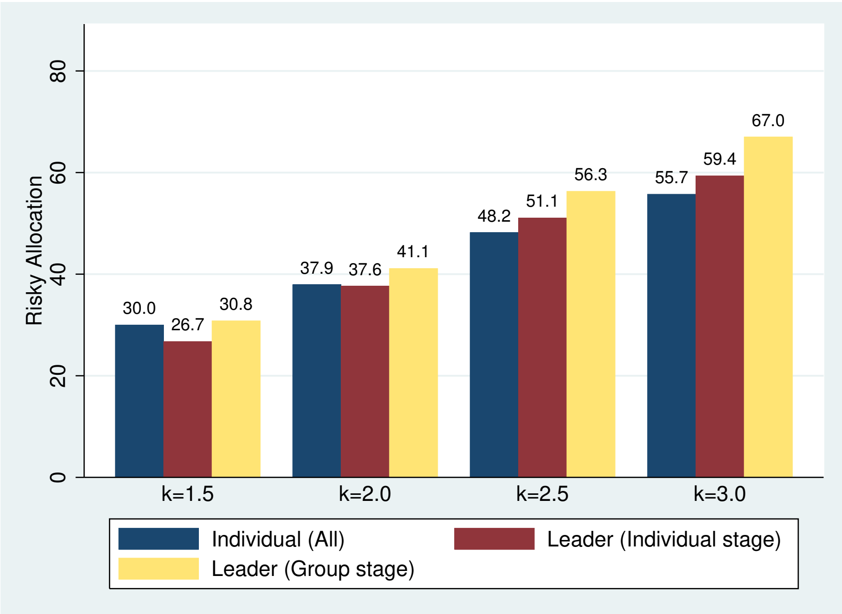


Table 2.3: Risky Allocation of Leaders in Individual and INFO Stages for Different Return Levels

Dependent var:	(1)	(2)	(3)	(4)
Risky allocation	k=1.5	k=2.0	k=2.5	k=3.0
Group stage	3.571	3.033	3.796**	6.128**
	(2.331)	(2.084)	(1.888)	(2.415)
Being leader more than once	2.625	4.390	7.476	9.378
	(4.699)	(4.998)	(5.547)	(5.953)
Constant	38.585***	48.649***	60.677***	70.946***
	(7.031)	(7.414)	(8.169)	(8.820)
Session	YES	YES	YES	YES
N	190	190	190	190
χ^2	19.431	17.726	15.117	21.039

Standard errors in parentheses

GLS Regressions for the impact of group period on riskiness

* $p < .1$, ** $p < .05$, *** $p < .01$

Besides the concern for the group, the leadership effect also plays a role in the impact created by the INFO treatment. To isolate the marginal effect of the revealed information in the INFO treatment, the relative effect of INFO treatment (with respect to the NO INFO treatment) on the difference between leaders' group and individual stage allocations is analyzed. As it is displayed in Table 2.4, the marginal effect of the INFO treatment is significant only at k=2.5 and k=3.0 cases. Therefore, the results imply that the observed information significantly influences the leader's risk attitude when the return is high.

Table 2.4: The Marginal Effect of INFO treatment on Risky Allocations of Leaders for Different Return Levels

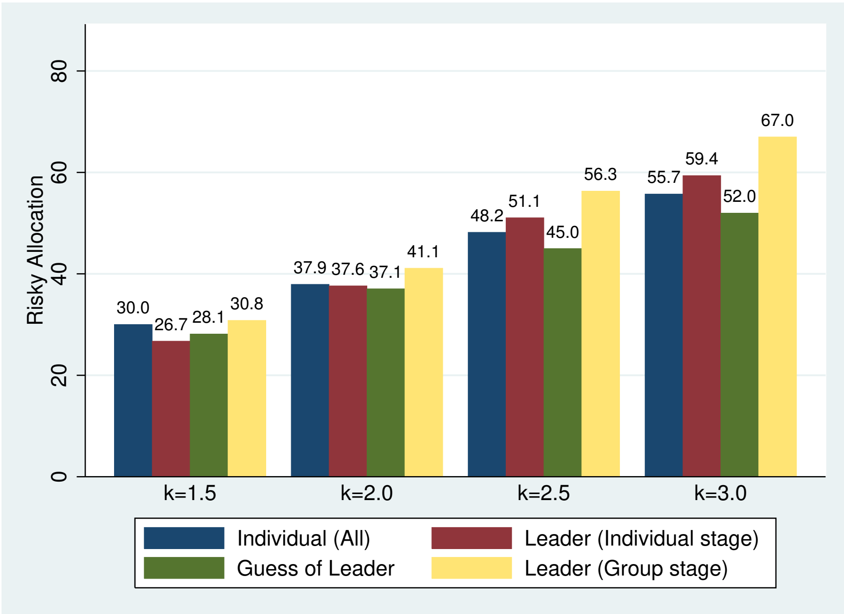
Dependent var:	(1)	(2)	(3)	(4)
$I_{group} - I_{individual}$	k=1.5	k=2.0	k=2.5	k=3.0
INFO	0.129	0.227	3.412*	4.752*
	(2.502)	(2.286)	(1.964)	(2.543)
Being leader more than once	-2.961	-3.531	-3.488	-2.173
	(2.698)	(2.465)	(2.117)	(2.742)
Constant	-3.217	-2.571	-4.069	-3.378
	(4.388)	(4.009)	(3.443)	(4.460)
Session	YES	YES	YES	YES
N	216	216	216	216
R^2	0.079	0.075	0.104	0.092

Standard errors in parentheses
OLS Regressions for the marginal effect of INFO on risk behavior change
* $p < .1$, ** $p < .05$, *** $p < .01$
INFO is a dummy where, *INFO*= 1 if INFO treatment
0 if NO INFO treatment

2.4.2.1 The Role of Leader's Perception on the Risk Attitude Change

In order to identify the channel through which the observed information affects the risk attitude, one needs to examine the perception the leader has about the group's expectation. Here, the belief that leader has on her unsuccessful competitor's risk attitude serves as a good proxy. It indicates the leader's belief in the type of risk behavior that the group prefers from the leader. So the leader's perception of her competitor's risk attitude is included in the INFO stage analysis (see Figure 2.4). A Wilcoxon signed-rank test indicates that leaders perceive their unsuccessful competitors as having the same risk attitudes as themselves at the low-return levels, and as being more risk-averse than leaders in the high-return levels (p= 0.672 in k=1.5, p= 0.760 in k=2.0, p=0.002 in k=2.5 and p= 0.007 in k=3.0).

Figure 2.4: Risky Allocation in Individual and Group Stages with Belief of Leaders (INFO Treatment)



In order to understand the role of leader's belief on his/her risk attitude with the impact of each treatment, the following model is employed.

$$\begin{aligned}\Delta_i(group, individual) = \beta_0 + \beta_1 \mathbb{1}(Prra_i \geq 0) + \beta_2 Prra_i \text{x} \mathbb{1}(Prra_i \geq 0) + \beta_3 Prra_i \text{x} \mathbb{1}(Prra_i < 0) \\ + \beta_4 INFO \text{x} \mathbb{1}(Prra_i \geq 0)) + \beta_5 INFO \text{x}(Prra_i \text{x} \mathbb{1}(Prra_i \geq 0)) \\ + \beta_6 INFO \text{x}(Prra_i \text{x} \mathbb{1}(Prra_i < 0)) + \epsilon_i,\end{aligned} \tag{2.1}$$

where

$\Delta_i(group, individual)$: the change in risky allocation between group stage and individual stage for leader i where $\Delta_i(group, individual) = I_{group,i} - I_{individual,i}$,

$Prra_i$: the perceived relative risk aversion of leader i with respect to her competitor $-i$ where $Prra_i = I_{individual,i} - Guess_{-i,i}$,

$\mathbb{1}(Prra_i \geq 0)$: the indicator for positive Prra, i.e. the leader i who believes having similar risk behavior or being more risk-seeking than her competitor,

$Prra_i$x$\mathbb{1}(Prra_i \geq 0)$: the interaction term of perceived gap in risk attitude and the positive Prra indicator,

$Prra_i$x$\mathbb{1}(Prra_i < 0)$: the interaction term of perceived gap in risk attitude and the negative Prra indicator,

$INFO$x$(\mathbb{1}(Prra_i \geq 0))$: the interaction term of INFO treatment with the positive Prra indicator,

$INFO$x$(Prra_i$x$\mathbb{1}(Prra_i \geq 0))$&$INFOx(Prra_ix\mathbb{1}(Prra_i < 0))$: the interaction terms of INFO treatment with $Prra_i$x$\mathbb{1}(Prra_i \geq 0)$ & $Prra_i$x$\mathbb{1}(Prra_i < 0)$.

Note that $Prra_i$x$\mathbb{1}(Prra_i \geq 0)$ (and $Prra_i$x$\mathbb{1}(Prra_i < 0)$) captures the impact of the perceived risk gap with the competitor among leaders who believe being more risk-seeking or having similar risk preference with their competitors (and among leaders who believe being more risk-averse than their competitors). The interaction of all variables with INFO treatment displays the marginal impact of the observed information on the corresponding variables. Therefore besides incorporating the leaders' beliefs into the risk change analysis, this model also distinguishes the marginal impact of "leadership" and the information about the unsuccessful competitor.

The results of Model (2.1) indicate that the belief of the leader is important on the risk shift both in information-blind and in information-revealed settings, yet its impact on the information-revealed case is stronger (see Table 2.5). The estimate of $\mathbb{1}(Prra \geq 0)$ shows that, in the NO INFO case, belief type of the leader does not have a significant effect on the change in risk behavior of the leader. Rather, the leader's perception of the risk gap with the unsuccessful candidate creates a significant impact: the more risk-seeking (the more risk-averse) the leader perceives herself compared to her competitor, the lower (the higher) the deviation from her individual allocation (see estimates of $Prra$x$\mathbb{1}(Prra \geq 0)$ and $Prra$x$\mathbb{1}(Prra < 0)$)). In the INFO case, on the other hand, both belief types and the perceived risk gap significantly affect the difference between group and individual allocations of leaders. Consistent with the findings provided in Section

2.4.2, the sole effect of observed information is significant only in high-return levels. The joint effect of $INFO$x$\mathbb{1}(Prra_i{\geq}0)$ and $INFO$x$(Prra$x$\mathbb{1}(Prra_i{\geq}0))$ in the high-return level case shows that the difference between the group and individual risky allocations of leaders who believe being more risk-seeking than their unsuccessful competitor or having same risk attitude is 4.532 tokens higher than the leaders who perceive themselves as more risk-averse in the INFO stage[8]. The effect of the leader's perceived risk gap with the unsuccessful candidate in the INFO stage is similar to its impact in the NO INFO stage. Therefore, leaders seem to follow a strategy in response to their 'informed' prediction about the group's preferences on a leader: they exhibit their individual risk preferences in the lower returns, while they become more risk-seeking in the higher returns since they believe their unsuccessful competitor is more risk-averse than themselves in the higher returns.

Table 2.5: Model (2.1)

Dependent var: $\Delta(group, individual)$	(1) Overall	(2) k_{low}	(3) k_{high}
$\mathbb{1}(Prra{\geq}0)$	-1.749	0.167	-2.853
	(1.708)	(2.273)	(2.353)
$Prra$X$\mathbb{1}(Prra \geq 0)$	-0.214***	-0.361***	-0.153**
	(0.060)	(0.107)	(0.069)
$Prra$X$\mathbb{1}(Prra < 0)$	0.237***	0.291***	0.214**
	(0.067)	(0.080)	(0.107)
$INFO$X$(\mathbb{1}(Prra{\geq}0))$	2.702	1.417	4.752**
	(1.729)	(2.379)	(2.181)
$INFO$X$(Prra$X$\mathbb{1}(Prra \geq 0))$	-0.196***	-0.198	-0.219**
	(0.074)	(0.122)	(0.087)
$INFO$X$(Prra$X$\mathbb{1}(Prra < 0))$	0.167**	0.075	0.396***
	(0.068)	(0.078)	(0.109)
Constant	2.522	1.785	3.124
	(2.725)	(3.308)	(3.630)
Session	YES	YES	YES
N	864	432	432
χ^2	265.345	170.549	151.394

Standard errors in parentheses
GLS Regressions for the impact of leader's perception on risk shift
* $p < .1$, ** $p < .05$, *** $p < .01$

[8]Note that the observed information in the election screen and the leader's belief about relative risk aversion are highly correlated.

The findings can be summarized as:

Result 2.3. The risk shift occurs due to the leader's concern for satisfying the group's expectation. This result confirms the prediction of Hypothesis 2.3.

2.4.2.2 Do Motives for the Risk Behavior Change Differ for Female and Male Leaders?

To investigate whether female and male leaders have different motives behind the change in their risk preferences, the same analysis is conducted by restricting the leader's gender. As it is shown in Table 2.6 (NO INFO stage), there is an increase in the male leaders' risky option allocations for the low level of returns, while female leaders do not change their risky option allocation. In the INFO stage, on the other hand, the risky option allocations of female leaders drastically increases in each return level while the male leaders' allocations are not affected (see Table 2.7). The results indicate that the change in risk attitudes on behalf of the group is motivated by the leadership effect among male leaders, whereas the main motivation of female leaders is to comply with the group's expectation. The findings can be summarized as:

Result 2.4. The risk shift is motivated by the leadership effect among male leaders, while it is driven by the group concerns among female leaders.

Table 2.6: Risky Allocation of Female and Male Leaders in Individual and NO INFO Stages

Dependent var:	Female leader				Male leader			
Risky allocation	k=1.5	k=2.0	k=2.5	k=3.0	k=1.5	k=2.0	k=2.5	k=3.0
Group stage	0.513	1.783	-0.018	1.023	6.036***	3.718**	0.845	1.782
	(1.968)	(1.774)	(1.435)	(2.058)	(2.128)	(1.651)	(1.464)	(2.265)
Being leader more than once	-0.802	0.292	9.247	15.418**	10.498	14.049	7.459	7.879
	(7.860)	(6.151)	(5.779)	(6.277)	(13.189)	(13.143)	(16.640)	(18.750)
Constant	42.712***	47.640***	51.178***	54.166***	44.982***	56.391***	69.327***	70.234***
	(11.294)	(8.853)	(8.303)	(9.054)	(11.246)	(11.150)	(14.060)	(15.898)
Session	YES	YES	YES	YES	YES	YES	YES	YES
N	118	118	118	118	75	75	75	75
χ^2	5.220	6.068	7.192	17.458	21.130	22.977	9.116	8.341

Standard errors in parentheses

GLS Regressions for the impact of group period on riskiness

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 2.7: Risky Allocation of Female and Male Leaders in Individual and INFO Stages

Dependent var:	Female leader				Male leader			
Risky allocation	k=1.5	k=2.0	k=2.5	k=3.0	k=1.5	k=2.0	k=2.5	k=3.0
Group stage	6.183*	7.677**	5.639**	7.880***	1.351	-0.902	2.202	4.626
	(3.652)	(3.209)	(2.790)	(3.021)	(2.960)	(2.596)	(2.576)	(3.686)
Being leader more than once	4.117	7.376	9.084	9.718	-2.084	1.942	4.774	12.058
	(6.984)	(6.824)	(6.869)	(7.992)	(6.243)	(7.679)	(9.618)	(9.701)
Constant	31.557**	37.362***	51.858***	64.449***	43.213***	54.193***	64.910***	72.555***
	(14.323)	(13.960)	(14.012)	(16.282)	(7.275)	(8.758)	(10.873)	(11.131)
Session	YES	YES	YES	YES	YES	YES	YES	YES
N	88	88	88	88	102	102	102	102
χ^2	9.242	11.467	10.562	15.678	25.051	18.587	10.085	14.168

Standard errors in parentheses

GLS Regressions for the impact of group period on riskiness

* $p < .1$, ** $p < .05$, *** $p < .01$

2.4.3 Qualitative Analysis

As a supporting analysis, the motivation for the risk attitude change is also examined via survey answers. According to the survey results, being a leader influences 44% of the participants' investment decisions. Participants are asked to explain briefly their objectives for being affected/not being affected by the leadership status[9]. The responses reveal three different types of leaders:

1. Selfish leaders: This participant type states that her investment decisions have not been affected by their leadership status. The objective is to maximize own utility for this participant type. Subject 29, for instance, explains it as "because I was already trying to do the best decision for myself".

2. Other-regarding leaders: This participant type predominantly states that their investment decisions are affected by their concerns for the group's expectations. As an example, Subject 51 explains it as "I was trying to make decisions the others expected from me". The prevalence of this type of leader supports my findings for the motivation of risk attitude change.

3. Selfish/Other-regarding leaders: This participant type regards her status of

[9]For more information about the questions, please refer to question 10 and 11 in Appendix B.3.3. Note that question 11 is a short answer question where participants write a brief explanation as an answer. The rest of the questions are multiple-choice questions.

being elected as having the desired risk attitude in the group. Therefore, she prefers not to change her risk attitude. As an example, Subject 182's response is as follows: "People chose me based on my investment decisions. Thus why should I change it?". Although it is unclear whether this leader type is selfish or other-regarding, she clearly makes her decision based on the implications of the election result.

All participants are classified based on their verbal reports in question 11[10]. The percentage of subjects in each type is as follows: 37.04% of the subjects are selfish, 38.10% of the subjects are other-regarding, 6.35% of the subjects are selfish/other-regarding and 18.52% of the subjects are unclassified. The assignment is also made by restricting the sample to the subjects who are elected as leaders: the other-regarding type is more prevalent among female leaders (45.38% of them are other-regarding and 40.34% of them are selfish), while the selfish type is more common among the male leaders (43.30% of them are selfish and 30.93% of them are other-regarding).

Table 2.8: Type of Leaders

Type	Female Leaders	Male Leaders	Total
Selfish	48	42	90
	40.34%	43.30%	41.67%
Other-regarding	54	30	84
	45.38%	30.93%	38.89%
Selfish/Other-regarding	10	6	16
	8.40%	6.19%	7.41%
Unclassified	7	19	26
	5.88%	19.59%	12.04%
Total	119	97	216

In order to observe whether there is a selection in terms of the prior belief about relative risk aversion, the belief of leaders in each type is analyzed in the NO INFO treatment. The results ensure that there is no dominant belief among any type of leaders. In the INFO treatment, on the other hand, both selfish

[10]After reading the answers, I classified each participant as selfish, other-regarding, selfish/other-regarding and unclassified.

and other-regarding leaders believe themselves to be more risk-seeking than their unsuccessful competitors in the high-return levels (see Table B.1 in Appendix B.1).

In addition, I run the Model (2.1) by splitting the sample as selfish and other-regarding leaders based on this classification (see Table B.2 in Appendix B.1). The results show that only other-regarding leaders respond to their belief which is formed by the observed information in the INFO stage: the other-regarding leaders who perceive themselves as being more risk-seeking or having the same risk attitude with their unsuccessful competitors (based on the observed information) become more risk-seeking at the group stage.

2.5 Conclusion

In daily life, many settings require economic decisions that involve social risk affecting the well-being of others. There is evidence for a change in risk preferences when making a decision on behalf of a group in comparison to a decision for himself or herself. However, the mechanism that creates this change is still unclear.

This paper is the first experimental study that provides a comprehensive analysis of the potential factors causing leaders to deviate from the individual risk when deciding for a group. Specifically, I focus on the effect of being an elected leader and the leader's concern for satisfying the group's expectation. The results provide some interesting insights. One important finding is that leaders become more risk-seeking when deciding on behalf of the group. This risk shift is more likely to be motivated by the tendency of the leader to meet the expectations of the group members than the leadership effect. Another important result is that the female and male leaders have different motives behind this observed shift in risk attitudes: it is motivated by the leadership effect among male leaders, while it is driven by the group concerns among female leaders.

Although this study provides evidence for the proposed questions, a few issues need to be addressed in order to provide a clear insight. In the INFO treatment, in addition to the risk information, the information of candidates' gender is also provided. This may confound the impact that risk information has on the risk attitude of the leader. In order to eliminate the confounder, the INFO treatment should be conducted without the gender information and the robustness of the results should be checked. Additionally, I do not have information for checking whether being the "winner" of the election affects the risk perception of the leader. This may be relevant in explaining the effect of competition on the risk behavior. Therefore, it would be beneficial to collect data about the leader's risk perception before and after the election result. In an extension study, I am planning to address these two issues.

In the current design, the election result is not as informative about the group's expectation due to the two-candidate setting. In an election with multiple candidates, the leader may deviate in the vicinity of her own position with respect to the other candidates. Therefore, the magnitude of the change (and possibly the direction) in risk attitude may be different than the current results. It is worthwhile to investigate it as a further study.

With the current setting, one of the reasons for the change in DM's risk behavior seems to be other-regarding concerns of the DM. In a setting where re-election and reputation-building are allowed, the change may be strategic as well as other-regarding. As an extension, it will be interesting to explore the dominant motivation in the revised design.

Chapter 3

The Role of Verifiability and Privacy in the Strategic Provision of Performance Feedback: Theory and Experimental Evidence

Abstract

We theoretically and experimentally analyze the role of verifiability and privacy in strategic performance feedback using a "one principal-two agent" context with real effort. We confirm the theoretical prediction that information transmission occurs only in verifiable feedback mechanisms and private-verifiable feedback is the most informative mechanism. Yet, subjects also exhibit some behavior that cannot be explained by our baseline model, such as telling the truth even when this will definitely hurt them, interpreting "no feedback" more optimistically than they should, and being influenced by feedback given to the other agent. We show that a model with individual-specific lying costs and naive agents can account for some, but not all, of these findings. We conclude that although agents do take into account the principal's strategic behavior in forming beliefs in a Bayesian fashion, they are on average overly optimistic and interpret positive feedback to the other agent more pessimistically than they should.

Keywords: Lab experiments, Performance feedback, Strategic communication, Cheap talk, Disclosure, Persuasion, Multiple audiences, Lying.

JEL Classification: C72, C92, D23, D82, D83, M12, M54.

3.1 Introduction

This paper provides a theoretical and experimental study of the role of verifiability and privacy in the strategic communication of interim performance information. Performance feedback (also known as performance review or performance appraisal) is one of the most commonly used management practices. Almost every organization, be it a major corporation, a small company, a high school, or a hospital uses some form of performance feedback.[1] Although it is considered an indispensable part of any organization, performance feedback has also been the object of a heated debate. Employees usually dread it and many business experts and consultants are fierce opponents. One of the most critical voices, Samuel Culbert, states that "[i]t's a negative to corporate performance, an obstacle to straight-talk relationships, and a prime cause of low morale at work." (Culbert (2008)).

Ideally, performance feedback gives an unbiased report on past performance and provides guidance regarding how to improve future performance. This aspect, i.e., accuracy or unbiased communication, has been regarded as a crucial aspect of performance feedback. In practice, however, the accuracy of feedback may be tainted due to various biases that arise from the evaluator's self-interest. In particular, supervisors may be vague in their assessments or avoid giving negative feedback to their subordinates for strategic reasons.[2] Forced ranking systems may overcome this deficiency but they cause problems of their own, potentially undermining employee confidence and motivation.

[1]One source estimates that "97.2% of U.S. companies have performance appraisals, as do 91% of companies worldwide" (see "Should Performance Reviews Be Fired?"). Also see evidence cited in Murphy and Cleveland (1991).

[2]See Schraeder et al. (2007) for a summary of research in psychology, management, and organizational behavior. Culbert (2008) claims that "any critique [involved in performance review] is as much an expression of the evaluator's self-interests as it is a subordinate's attributes or imperfections." Longenecker et al. (1987) report (interview) evidence that the main concern of the executives in performance appraisals is not accuracy but rather to motivate and reward subordinates. Accordingly, they systematically inflate the ratings in order to increase performance. In the Forbes article titled "Ten Biggest Mistakes Bosses Make In Performance Reviews," the number 1 item is 'Too vague,' number 2 is 'Everything's perfect – until it's not and you're fired,' while number 8 is 'Not being truthful with employees about their performance' (Jackson (2012)).

Clearly, there are various pros and cons of performance feedback along a multitude of dimensions, but its effectiveness as a tool of communication seems to be one of the most contentious aspects. In this paper we focus on precisely this aspect. In a setting where feedback is given strategically by a supervisor, we theoretically and experimentally analyze how subordinates interpret the feedback they receive in forming an opinion of themselves and whether feedback communicates the actual performance information in a truthful manner.

In our experiment there is a supervisor (called principal) and two subordinates (called agents) who work for (potentially) two periods. In each period agents perform a real effort task and succeed if their performance is greater than a randomly determined threshold, which plays the role of chance or other unpredictable exogenous factors such as market conditions and organizational standards. The principal, and only the principal, observes the first-period performance (i.e., success or failure) of the agents and then decides whether and what type of feedback to provide to the agents. The agents observe the feedback (or lack thereof), update their beliefs about their likelihood of succeeding in the second period, and choose whether to perform the task again in the second period or not.[3]

The agents receive monetary payoff from their performances in the two periods, while the principal receives a payoff only from the agents' second-period performances. In addition, the principal's payoff depends on the minimum of the two agents' performances. That is, the principal obtains an extra payoff only if both agents end up performing in the second-period task. This captures "weakest-link" type performance settings, where it is important that every agent achieve a certain level of performance. With such a payoff function, the principal prefers both agents to have a high perceived likelihood of success in the second-period task, i.e., to have high self-confidence. This also makes feedback a strategic choice: if the first-period performance is positively correlated with second-period performance, then the principal has an incentive to get the agents to believe that they

[3]More precisely, subjects state the probability with which they believe they will succeed in the second-period task, which is elicited using a Becker-De Groot-Marschak type procedure.

succeeded in the first period task.[4]

We analyze the effectiveness of performance feedback mechanisms along two dimensions: (1) verifiability of the feedback; (2) privacy of the feedback. Our baseline scenario is truthful private feedback, in which each agent privately and truthfully learns whether he succeeded in the first period task or not. In the verifiable feedback case, the principal has to reveal the true performance or reveal no information at all, while in unverifiable feedback, she may lie about performance without incurring any monetary cost. The feedback may be private, in which case each agent receives feedback only about his own performance, or public, in which case both agents observe the feedback on each agent's performance. Therefore, in addition to the baseline scenario, we have four different treatments: (1) private-verifiable; (2) public-verifiable; (3) private-unverifiable; (4) public-unverifiable.

In reality, some performance measures are indeed objective and hence verifiable, while others are subjective and unverifiable. For example, a supervisor may have access to evaluations - by higher ranking administrators, co-workers, customers, or students - that can be reproduced if needed. Similarly, sales or productivity figures, customer ratings, exam grades of students, and long-term mortality rates after surgeries are all objectively measurable and verifiable performance measures. Subjective or judgmental evaluations by supervisors, on the other hand, are by their very nature unverifiable, i.e., cheap talk. Likewise, feedback is sometimes provided in a private manner, as in many performance review interviews, while in other cases it is public, as in 'employee of the month' types of feedback. The question of whether feedback should be provided publicly is especially relevant for contexts where it is important to preserve the "morale" of all agents. Given that most organizations have some freedom in determining their feedback mechanisms along the lines we consider, our results can have significant

[4]The experiment is designed so that the likelihood of success for each agent is independent of the likelihood of success for the other agent. This implies that the performance of the other agent is not informative about the likelihood of own success. Furthermore, feedback has no direct payoff consequences, which lets us isolate the communication phase involved in the feedback process from other strategic considerations.

policy implications for firms and for educational settings.

In Section 3.4 we analyze a theoretical model and derive several predictions. Our main prediction is that information transmission occurs only in verifiable feedback mechanisms and private-verifiable feedback is the most informative feedback mechanism. Section 3.5.1 presents strong evidence in support of this prediction. We therefore conclude that, if effective communication is the main objective, organizations should try to provide measurable and verifiable forms of feedback and they would be better off if they do this privately.

We also find that positive and negative feedback have significant effects on beliefs in all treatments except private-unverifiable feedback, whereas giving no feedback has no significant effect on beliefs. Since "no feedback" must be interpreted as bad news, especially in verifiable feedback mechanisms, this finding contradicts the predictions of our model.

Our data provides evidence that when feedback is public, agents' beliefs about their likelihood of success are influenced by the feedback provided to the other agent. More precisely, they become more optimistic if the other agent receives negative feedback and less optimistic if the other agent receives positive feedback. We further find that this effect is significant only when own feedback is positive, and stronger for public-unverifiable than for public-verifiable feedback. Since, in our experimental design, the other agent's performance has no informative content regarding own performance, these findings are also at odds with our model.

Finally, we find a positive effect of beliefs but no significant effect of feedback on performance. In other words, performance reviews are at most a weak instrument for boosting employee performance.

In Section 3.5.2, we analyze principals' behavior and find that, in all the treatments, some (but not all) subjects tell the truth. This goes against our prediction that in unverifiable feedback mechanisms, principals should always provide positive feedback. Furthermore, we find that principals expect positive feedback to be interpreted more optimistically and negative feedback more pessimistically than they actually are. In other words, some of them give bad news even though they actually believe that it will be interpreted as such, which leads us to conclude that lying imposes individual-specific costs.

In Section 3.6.1 we extend our baseline model to include individual-specific costs of lying and naive agents. We show that it can account for most of our empirical findings as well as some of the above discrepancies between the baseline model and the data. In particular, the extended model predicts that all principals will report truthfully if the agent is successful, but if the agent has failed, then some will still report truthfully but the rest will lie if they can, or give no feedback.

Interestingly, the model also shows that in public-unverifiable feedback, it is indeed rational for an agent who received positive feedback to be influenced adversely by the other agent's positive feedback. This is because, in equilibrium, the principal provides positive feedback to, say, agent 1 and negative feedback to agent 2 only when the outcome is success for agent 1 and failure for agent 2, whereas she provides positive feedback to both agents after all four possible outcomes, which includes failure for agent 1. A similar effect, however, does not exist if own feedback is negative, which is also in line with the evidence. This still does not explain why this effect also exists in public-verifiable feedback. However, the fact that it is stronger in public-unverifiable feedback and significant only when own feedback is positive indicates that agents do consider the principal's strategy in forming their beliefs.

We address this issue in more detail in Section 3.6.2 by comparing agents' actual post-feedback beliefs with hypothetical beliefs that a Bayesian agent would form if he perfectly predicted the (empirical) strategy used by the principals. Our analysis suggests that Bayesian updating plays a significant role in the formation of beliefs, but agents are, on average, overly optimistic in responding to their own feedback and interpret positive feedback to the other agent more pessimistically than they should.

Overall, we conclude that private-verifiable feedback is the most informative mechanism while unverifiable feedback is not informative, and public feedback interferes with the informativeness of positive feedback, especially when it is unverifiable.

3.2 Related Literature

To the best of our knowledge, this is the first comprehensive study that explores, both theoretically and experimentally, the impact of verifiability and audience on strategic information transmission in a realistic performance feedback context. Previous theoretical and experimental studies of performance feedback have mostly focused on the effects of *truthful* feedback on effort decisions and future performance. Theoretical work has generally used principal-agent models to study optimal information revelation mechanisms under the assumption of truthful feedback, taking into account the effects of the feedback on agents' actions (see, for example, Ertac (2005), Ederer (2010), Aoyagi (2010)). The experimental literature has mostly studied the motivational effects of truthful performance feedback in both organizational and educational settings and documented varying results. With flat wages, the majority of papers find that provision of relative performance feedback leads to higher effort on average, whereas evidence is more mixed in performance-pay settings.[5] Our major departure from this literature is that we consider *strategic* rather than truthful feedback and focus on the communication aspects.

Ederer and Fehr (2009) is one of the few experimental papers that study strategic performance feedback. They analyze the effect of private-unverifiable feedback on (induced) effort in a dynamic tournament with two agents. In their setting, the principal has an incentive to underreport the true performance difference between the agents. Hence, relative performance feedback should be completely uninformative and agents should not respond to feedback in equilibrium. In contrast,

[5] See, among others, Azmat and Iriberri (2010), Azmat and Iriberri (2012), Bandiera et al. (2010), Blanes i Vidal and Nossol (2011), Charness et al. (2010), Eriksson et al. (2009), Gerhards and Siemer (2014), Gill et al. (2015), and Kuhnen and Tymula (2012).

their results show that even though agents discount the information they receive from the principal, they still respond to it and some principals provide feedback that is close to the truth while others consistently underreport.[6]

In our private-unverifiable feedback treatment, we find a similar result to Ederer and Fehr (2009) in the sense that some principals tell the truth while others lie; however, in our case, agents heavily discount such feedback, which renders it uninformative. From a design perspective, our work is distinct from Ederer and Fehr (2009), as well as from the other papers in this literature, along several lines: (1) We vary treatments along the dimensions of both audience and verifiability and study their interaction, while Ederer and Fehr (2009) study only private-unverifiable feedback. This enables us to compare different feedback mechanisms along dimensions that may be discretionary in organizational and educational settings and to draw policy conclusions; (2) We study a non-tournament setting where information about the other agent's performance is irrelevant. These two aspects of our research allow us to uncover, both theoretically and experimentally, a novel finding: When feedback is public and the other agent receives positive feedback, agents interpret their own feedback more pessimistically - apparently by making inferences about the principal's strategy. Furthermore, this effect is stronger if feedback is unverifiable and own feedback is also positive; (3) We measure the impact of feedback directly on beliefs, rather than effort, using an incentive-compatible mechanism that is also robust to risk aversion. This allows us to isolate, in a clean manner, the strategic communication aspect of performance feedback, which is the main focus of this paper; (4) We use a real, rather than induced, effort setting, which creates an ego-relevant environment that should contribute to the external validity of our results.[7]

[6]In a one principal/one agent setting with unverifiable feedback and induced effort, Mohnen and Manthei (2006) find similar results: Some principals tell the truth but deception is also widespread. Rosaz (2012) also studies unverifiable feedback in a one principal/one agent setting, but limits the principal's ability to lie. She finds that the principal indeed manipulates the feedback but the agent increases effort in response.

[7]This obviously has a cost in terms of control over unobservables and makes the match with theory more difficult. Since the focus of our work is not the effect of feedback on effort, we believe that the benefits of using real effort outweigh the costs.

Gürtler and Harbring (2010) also study the effect of performance feedback on effort in a tournament setting, but unlike in Ederer and Fehr (2009), feedback is public and verifiable in their design. The theory, in this case, predicts that agents should interpret no feedback as bad news and full revelation of relative performance should occur. They find that although there is evidence that no feedback is regarded as bad news, the effect on effort is not as strong as the theory predicts.

Verifiable feedback mechanisms induce a strategic communication game that is known as a "disclosure" (or persuasion) game in the literature, pioneered by Grossman (1981) and Milgrom (1981), while unverifiable feedback mechanisms induce what is known as a "cheap talk" game, first studied by Crawford and Sobel (1982).[8] Therefore, our paper is also related to the literature that experimentally tests the predictions of cheap talk and disclosure games. In these strands of the literature, Dickhaut et al. (1995) and Cai and Wang (2006) find support for the qualitative predictions of the basic cheap talk model of Crawford and Sobel (1982), i.e., less information is transmitted as preferences of the sender and the receiver diverge, and Battaglini and Makarov (2010) find overall support for the predictions of Farrell and Gibbons (1989), which extends the basic model to the case of multiple receivers. Blume et al. (2001), Cai and Wang (2006), and Battaglini and Makarov (2010) find evidence for over-communication, i.e., a tendency for the senders to reveal more information than predicted by theory as well as a tendency for the receivers to rely on the information sent by the senders.[9] Drugov et al. (2013) test the two-receiver model by using five states rather than

[8] We discuss the relevant theoretical literature in Section 3.4.

[9] Cai and Wang (2006) explains this over-communication behavior using level-k behavior and quantal response equilibrium. Using information from eye-tracking technology, Wang et al. (2010) shows that senders look at payoffs in a way that is consistent with a level-k model. However, over-communication that persists over rounds, as in Blume et al. (2001) is difficult to explain by level-k reasoning. A potential explanation for over-communication on the part of senders is "lying aversion". Gneezy (2005) reports experimental evidence that subjects have a tendency to tell the truth even if it is against their material interests. Gneezy et al. (2013) study the same question using a new method and find that subjects are heterogenous with regard to their tendency to lie. See Charness and Dufwenberg (2006), Hurkens and Kartik (2009), Sutter (2009a), Sanchez-Pages and Vorsatz (2009), Abeler et al. (2012), and Fischbacher and Föllmi-Heusi (2013) for further experimental evidence on lying aversion.

two and, similar to our setting, also run a private communication mode. They report evidence of a disciplining effect of public communication.[10]

The experimental literature on the strategic communication of verifiable information is smaller. Early work on experimental tests of disclosure games has studied disclosure in the context of markets, where the seller is better-informed and discloses quality to the buyer(s). Forsythe et al. (1989) find that full information revelation is achieved, but only as subjects become more sophisticated over repeated rounds of play. King and Wallin (1991) analyze a market setting where the seller may or may not be informed, which is unknown to the buyers, and find that full disclosure does not occur. Forsythe et al. (1999) find that imposing "anti-fraud" rules that constrain message sets to include the true state improves efficiency in comparison to cheap talk messages. More recently, Benndorf et al. (2015) find, in a labor-market experiment with a lemons structure where workers can reveal their productivity, that revelation takes place less frequently than predicted in equilibrium. The experimental context and decision settings used in these papers (e.g., asset markets, auction context) tend to include elements that may affect behavior independently of the basic strategic considerations in verifiable information disclosure. Jin et al. (2015) use a more direct test of the "no news is bad news" prediction in disclosure games, and find that receivers do not interpret no information sufficiently negatively. Hagenbach and Perez-Richet (2015) test the predictions of Hagenbach et al. (2014) by considering payoff structures for the sender that are not necessarily monotonic in the receiver's action, and find that whether the game is cyclic or acyclic matters for the receivers in forming skeptical beliefs and thereby for information transmission.

While some of our results, such as the tendency to tell the truth with unverifiable feedback and insufficient strategic discounting of no feedback with verifiable feedback are also reported in the existing experimental studies of strategic com-

[10] See Crawford (1998) for an early survey of experimental work on strategic communication. There is also more recent experimental work on extensions of the basic cheap talk model to multiple dimensions and multiple senders, such as Lai et al. (2015) and Vespa and Wilson (2015).

munication, our work is distinct along several dimensions. First, we elicit agents' beliefs and principal's expectations on agents' beliefs directly, while previous work has studied the effect of information on other strategic choices, which may be confounded by risk aversion or other factors specific to the decision environment. This allows us to more clearly focus on the motives behind giving feedback and its interpretation.[11] Second, previous work has tested the predictions of cheap talk or disclosure games usually by varying the preferences of the players, while we take the preferences as fixed and vary both verifiability and audience. As we have mentioned before, this allows us to study the interaction between these two dimensions and leads to novel findings. Third, our main purpose is to test informativeness of different performance feedback mechanisms in a real-effort context, while the previous work has either used a neutral framework to test game theoretical predictions or studied other specific environments such as auctions or labor markets.

3.3 Experimental Design

The experimental design is based on studying interim performance feedback in a one principal-two agent real effort context. The performance feedback technology available to the principal is the treatment variable, and we study five treatments in a within-subject design. Therefore, the experiment consists of five periods with each period corresponding to a different feedback mechanism, and within each period there are two rounds. To eliminate potential wealth effects, we use a random payment scheme, i.e., one of the ten rounds is chosen randomly and subjects are paid according to their payoffs in the chosen round.

[11] This feature of the design relates the paper to the experimental literature on the effects of noisy but non-strategic feedback on beliefs. Our finding that agents respond to feedback more optimistically than they should is in line with and complements the findings in this literature that subjects may process information differently and exhibit biases of asymmetry or conservatism (in comparison to Bayesian updating) when the context is ego-relevant (Ertac (2011), Eil and Rao (2011), Mobius et al. (2011)). However, we study feedback that is provided strategically, which makes a difference because, as we will show, many principals indeed act strategically and agents take that into account in updating their beliefs.

At the start of the experiment, subjects are randomly assigned to the roles of either "Principal" or "Agent", and these roles do not change. In each period, 3-person groups, which consist of one principal and two agents, are formed. We use a "strangers" matching protocol, where new groups are randomly formed at the start of every period.

For participants in the role of agents, we use two different real-effort tasks: an addition task and a verbal task (see Appendix C.2.1 for details). The verbal task consists of general knowledge questions as well as verbal classification and number-letter matching questions. The addition task involves adding four or five two-digit numbers.

In each period, agents are randomly assigned to one of these tasks and perform the same task in both rounds of that period. For both tasks, subjects are asked to solve as many questions as possible within a limited time (2 mins.). At the end of each round, the number of correct answers is compared to a "target score", randomly determined for that specific period.[12] The same target score is employed in both rounds of the period. If a subject's score is greater than or equal to the target score, the subject is "successful", and has failed otherwise. Note that the target score is subject-specific and there is no common shock applied to the performance of subjects.

3.3.1 Belief elicitation

To elicit self-confidence, we use a crossover mechanism developed independently by Karni (2009) and Mobius et al. (2011), which is a Becker-De Groot-Marschak-type procedure for eliciting beliefs truthfully and independently of risk preferences. In this mechanism, subjects are presented with two alternative lotteries to determine their second-round payoff. In the performance-based lottery, the reward is based on the agent's second-round performance. That is, the agent receives

[12]The target score is a number which is randomly chosen from the interval [4,13] at the beginning of each period. The range of the target score was determined based on data from a pilot session.

the reward if his outcome is "successful" in the second-round performance stage. In the chance-based lottery, the agent earns the reward with probability X, regardless of his second-round performance. At the end of the first performance round, subjects are asked to report the minimum probability of winning in the chance-based lottery that would make them willing to choose the chance-based lottery as opposed to the performance-based one. The computer then draws X randomly. If the randomly drawn X is at least as large as the agent's stated minimum, the chance lottery applies. Otherwise, the agent is rewarded based on his second-round performance. This mechanism gives agents an incentive to truthfully report the subjective probability with which they think they will succeed in the second round. In order to study the within-person effect of performance feedback on beliefs, we ask the subjects to make this decision twice: once before and once after receiving feedback. To maintain incentive compatibility, we randomly choose either the pre-feedback or post-feedback beliefs to determine whether the performance or chance mechanism will be implemented.

The timeline of a period for agents is as follows:

1. Pre-feedback performance: Subjects perform the assigned task within 2 minutes.

2. Pre-feedback beliefs: Without receiving any information, subjects state the minimum probability of winning that would induce them to leave their second-round payoff to chance.

3. Feedback: Feedback is received, in the form of a message whose content changes between treatments, as will be explained in Section 3.3.2.

4. Post-feedback beliefs: After seeing the message (or no message), subjects are allowed to update their previously reported beliefs. (At this stage, the subjects can see their previously reported beliefs on the screen.)

5. Performance/chance mechanism: If the self-reported probability of winning (either pre- or post-feedback, depending on which was selected) is higher

than the probability of winning in the chance mechanism (drawn by the computer), then the subject performs the same type of task for two minutes again, as in the first round. Otherwise, they do not perform the task, and their second-round payoff is determined by chance, according to the winning probability drawn by the computer.

3.3.2 Feedback mechanism

Note that after the first round, agents do not have exact knowledge of whether they were successful, although they will have subjective beliefs. Principals, on the other hand, observe the true first-round outcomes (success or failure) of the two agents they have been matched with. After stating their priors, agents may receive a message about whether they were successful in the first round. There are five types of feedback mechanisms used throughout the experiment, which differ in the provider, audience, and content of the feedback. In terms of content, we have the following types:

1. Truthful feedback: In this mechanism, subjects receive an accurate message (success or failure) from the computer. This is the baseline mechanism in our design.

2. Verifiable feedback: In this mechanism, performance feedback is reported by the principal. The principal can choose either to transmit the true outcome (success or failure), or to withhold the information. Sent messages always have to be correct, and agents know that there can be no deception.

3. Unverifiable feedback (cheap talk): As in the verifiable mechanism, the feedback comes from the principal, but she does not have to report the actual outcome, i.e., she can lie. In addition, she has an option to send no message.

Within the verifiable and unverifiable mechanisms, we also employ two different feedback types that differ in the audience of the messages:

1. Private feedback: In this mechanism, the principal reports the feedback independently and privately to the agents, and agents only see the message targeted to them.

2. Public feedback: In this mechanism, the principal has to announce the feedback publicly. That is, each agent observes the other agent's message, in addition to his own.

This design leaves us with five different feedback treatments, which are implemented within-subject: truthful feedback, private-verifiable feedback, public-verifiable feedback, private-unverifiable feedback, and public-unverifiable feedback. In the public-verifiable case, the principal has to decide either to release the truthful outcome to both of the agents publicly, or to withhold the information. On the other hand, in the public-unverifiable case, the feedback for each agent is chosen separately from the three options explained above (success, failure or no information) and the messages for both agents are delivered publicly to all.

Finally, in order to get a better insight into the feedback strategy employed by the principals, they are asked to guess agents' post-feedback beliefs.

3.3.3 Payoffs

The payoffs of participants in the role of agents depend on their performance outcomes as well as their decisions. To incentivize performance in the first round, we use differential rewards based on a performance target: 300 ECU (experimental currency unit), if the agent succeeds, and 100 ECU, if he fails. (1 ECU = 0.06 Turkish Liras (TL).)

In the second round, if the agent ends up doing the task, his payoff depends on whether he succeeds or fails, exactly as in the first round. If, however, the agent ends up with the chance mechanism, then his second-round earnings are 300 ECU with probability X, and 100 ECU with probability $(1-X)$, where X is the randomly chosen probability of winning.

The principal's payoff, on the other hand, depends on the second-round entry behavior and performance outcomes of the two agents. For the principal, we use a payoff function in which the performances of the two agents are complements. Specifically, the payoff function is:

$$V_t = \begin{cases} 100, & t = 2n-1 \\ 50 + 10(g_{1t} + g_{2t}) + \min\{q_{1t}, q_{2t}\}, & t = 2n \end{cases}$$

where $n \in \{1,2,3,4,5\}$ is the period number, q_{it} is the return from the second-round performance of agent i in period t, and g_{it} is an indicator variable that takes the value of 1 if the principal's guess in period t for agent i is correct, i.e., in the $\pm$ 5 interval of agent's actual belief, 0 otherwise. Return from the performance of agent i in period t is equal to

$$q_{it}(c_{it}, e_{it}) = \begin{cases} 20 * c_{it}, & e_{it} = 1 \\ 0, & e_{it} = 0 \end{cases}$$

where c_{it} denotes the number of correct answers of agent i in period t, while e_{it} represents the entry of agent i to the performance stage (as opposed to taking the chance mechanism). In the first round the principal's payoff is a constant amount, 100 ECU. The second-round payoff is composed of three elements: a constant amount, 50 ECU, an extra 10 ECU for each correct guess about the agents' beliefs, and the minimum of the returns from both agents. As can be seen from the above payoff function, for the principal to earn an extra return over the fixed endowment, both agents must end up doing the task. This, together with complementarity, implies that the principal should aim to (1) convince both agents that they are likely to succeed in the second round task, and (2) maximize the post-feedback performance of the worst-performing agent in the second round.

3.3.4 Procedures

The experiment was programmed using the z-Tree experimental software (Fischbacher (2007)), and implemented at the Koç University and Bilgi University computer labs in the Spring term of 2013. We collected data from 132 subjects in total (72 subjects from Koç University and 60 subjects from Bilgi University). The experiment was conducted in 13 sessions, with 8 sessions at Koç University and 5 sessions at Bilgi University. Our sample consists of 68 male and 64 female participants, who are mostly undergraduates. At the end of each session, we conducted a survey to collect demographic data such as age, gender, major and GPA (see Appendix C.2.3). In order to mitigate potential order effects, we used 6 different configurations that differ in the sequence of treatments.[13] Sessions lasted about 50 minutes, and subjects earned between 15 TL and 28 TL (on average 20.23 TL), including a show-up fee.

3.4 Theory and Predictions

In this section we will analyze a stylized model of our experimental design and derive theoretical predictions that will form the basis for the empirical analysis in Section 3.5. There are two agents, indexed by $i = 1, 2$, and a principal, denoted by P. For each agent i, a state of the world θ_i is realized and observed only by the principal. In our experimental design, this state corresponds to either "success" or "failure", denoted by s and f, respectively. We assume that states are independently distributed across agents and the probability of success for agent i is equal to $p_i \in (0, 1)$. We will also assume for simplicity of exposition that s and f are real numbers with $s > f$.

After observing (θ_1, θ_2), the principal provides feedback to the agents. As we have explained in Section 3.3, this feedback might be verifiable, in which case, the principal cannot lie but still choose to give no information, or might be

[13] The configurations were as follows: TVU, TUV, VUT, UVT, VTU and UTV, where T, V and U correspond to Truthful, Verifiable and Unverifiable feedback mechanisms, respectively.

unverifiable, i.e., might be cheap talk, in which case the principal can lie about the actual state of the world or provide no information. Feedback is either private, in which case the principal provides feedback on θ_i to each agent i independently and privately, or public, in which case both agents observe the common feedback about (θ_1, θ_2). After receiving feedback, each agent independently chooses an action and the game ends. In our experimental design, this action corresponds to the choice made by the agent in the belief elicitation round. As we have explained before, our belief elicitation mechanism is designed so that it is optimal for each agent to choose the probability with which he believes that he will be successful in the second-round task.

Payoff function of agent i is given by $u_i(a_i, \theta_i)$, where $a_i \in A_i$ is the action choice of agent i and A_i is a compact and convex set of real numbers. Principal's payoff function is $v(a, \theta)$, where $a = (a_1, a_2)$ and $\theta = (\theta_1, \theta_2)$. We assume that players are expected payoff maximizers. If agent i believes that he is successful, i.e., $\theta_i = s$, with probability μ_i, his expected payoff is equal to $U_i(a_i, \mu_i) = \mu_i u_i(a_i, s) + (1 - \mu_i) u_i(a_i, f)$. We assume that, for each $\mu_i \in [0, 1]$ there is a unique maximizer of $U(a_i, \mu_i)$, denoted $a_i^*(\mu_i)$, which is in the interior of A_i and strictly increasing in μ_i. From now on, whenever we say that agent i has high beliefs we mean that μ_i is high.

We also assume that the principal's payoff function is strictly increasing in a_i, $i = 1, 2$. This makes feedback a strategically important choice for the principal because she has an incentive to induce a high belief by each agent. This, of course, may render her feedback unreliable in equilibrium and the extent to which this happens may depend on the feedback technology itself, i.e., whether the feedback is private or public and verifiable or not. The main theoretical issue we deal with in this section is the informativeness of the feedback provided by the principal in these different cases.

Denote the set of states as $\Theta = \{f, s\}$ and the set of messages that can be potentially sent by the principal as $M = \{f, s, \emptyset\}$, where $\emptyset$ denotes no information.

Let $M(\theta)$ be the set of messages that are feasible when the state is $\theta = (\theta_1, \theta_2)$. The following describes the set of strategies available to the principal under different treatments:

1. Private Feedback: A pure strategy of the principal is a pair of functions $\rho = (\rho_1, \rho_2)$, where $\rho_i : \Theta^2 \to M_i(\theta)$. If feedback is unverifiable, then $M_i(\theta) = M$, i.e., there are no restrictions on the feasible messages. If feedback is verifiable, then $M_i(\theta) = \{\theta_i, \emptyset\}$, i.e., principal either tells the truth or provides no information to an agent.

2. Public Feedback: A pure strategy of the principal is a function $\rho : \Theta \to M(\theta)$. If feedback is unverifiable, then $M(\theta) = M^2$, i.e., there are no restrictions on the feasible messages. If feedback is verifiable, then $M(\theta) = \{\theta, \emptyset\}$, i.e., principal either tells the truth or provides no information to both agents.

After observing feedback r, agent i forms beliefs on the state of the world $\mu_i(r) \in [0,1]^2$ and chooses an action $\alpha_i(r) \in A_i$. Let μ_i^i denote the probability that agent i's beliefs put on the event $\theta_i = s$ and μ_i^{-i} the probability on $\theta_{-i} = s$. Let $\mu = (\mu_1, \mu_2)$ and $\alpha = (\alpha_1, \alpha_2)$ denote, respectively, an agent belief profile and strategy profile. An assessment is composed of a strategy for each player and beliefs by the agents: (ρ, α, μ).

An assessment is a perfect Bayesian equilibrium (PBE) if strategies are optimal given beliefs and beliefs are formed by using Bayes' rule whenever possible. In what follows we will analyze the set of pure strategy perfect Bayesian equilibria of each extensive form game defined by one of the four possible feedback mechanisms: (1) private-verifiable; (2) public-verifiable; (3) private-unverifiable; (4) public-unverifiable.

A verifiable feedback mechanism induces a game of strategic communication known as a "disclosure game", pioneered by Grossman (1981) and Milgrom (1981), while an unverifiable feedback mechanism induces a "cheap talk game", introduced by Crawford and Sobel (1982). These basic models and the main results have been later generalized and extended in several directions.[14] Most relevant for us are Farrell and Gibbons (1989) and Koessler (2008), both of which consider a two-receiver, two-state, and two-action model and analyze public and private communication. Farrell and Gibbons (1989) consider only the cheap talk case, while Koessler (2008) extends it to verifiable messages. Our model differs from theirs in that the state is multidimensional (which is formally equivalent to four states) and there is a continuum of actions. None of our results on public feedback follows directly from the analyses in these two papers, but the reasoning behind the existence of partially informative equilibrium in public-verifiable feedback is similar to the case of the mutual subversion in Koessler (2008), and the partially informative equilibrium in public-unverifiable feedback resembles the mutual discipline case in Farrell and Gibbons (1989).[15]

3.4.1 Verifiable Feedback

Each agent has (or updates) his beliefs regarding the other agent's type as well as his own type. However, since types are independent and only own type affects payoffs, what matters strategically is only beliefs on own type. Accordingly, we say that an equilibrium is fully informative if each agent can infer his type from the principal's report and completely uninformative if agents learn nothing about their own type.

Our first result shows that if feedback is verifiable, then agents receive perfect information about their own types.

Proposition 1. *If feedback is private and verifiable, then all equilibria are fully informative.*

[14] For the literature on disclosure games see Seidmann and Winter (1997), Mathis (2008), Giovannoni and Seidmann (2007), and Hagenbach et al. (2014). The basic cheap talk model in Crawford and Sobel (1982) has also been extended in many directions. See Sobel (2013) for a recent survey of this large literature.

[15] We should also mention Goltsman and Pavlov (2011), which generalizes Crawford and Sobel (1982) to the case of two receivers with different preferences and compares public with private feedback. Again, our model's state space and payoff structure are different in a way that makes direct application of their results impossible.

Proof. All proofs are relegated to Appendix C.1. □

Proof of Proposition 1 is very easy. If feedback is verifiable and the principal learns that an agent is successful, then she can simply send the message that he is successful and induce the best beliefs and the highest action on the part of that agent. Since feedback is verifiable, the other type of the principal, i.e., the type who observed that the agent has failed cannot mimic this feedback. This full revelation result is well known in the literature and follows from two aspects of our model: (1) every type has a message that only that type can send; (2) the principal's payoff is monotonic in each agent's beliefs.

In public feedback, the principal cannot change her reporting strategy regarding one agent's performance without the other agent observing this change. This creates the main difference between private and public feedback for equilibrium analysis. Indeed, if feedback is public and verifiable, then full information revelation is an equilibrium but, in contrast to private feedback, there is also a partially informative equilibrium.

Proposition 2. *If feedback is public and verifiable, then in equilibrium there is either full information revelation or* $\rho(s,s) = (s,s)$ *and* $\rho(\theta) = \emptyset$ *for all* $\theta \neq (s,s)$.

It is easy to construct a fully revealing equilibrium by specifying strategies $\rho(\theta) = \theta$ for all θ and beliefs as $\mu_i(\emptyset) = 0$ for $i = 1, 2$. The following example shows that there is also a partially informative equilibrium.[16]

Example 1. Let $s = 7$, $f = 1$, $p_i = 1/2$, and payoff functions be $u_i(a_i, \theta_i) = \theta_i a_i - \frac{1}{2}a_i^2$ and $v(a, \theta) = a_1 a_2 w(\min\{\theta_1, \theta_2\})$, where w is a strictly increasing function with $w(1) > 0$. It can be shown that the following assessment is an equilibrium: $\rho(s,s) = (s,s)$, $\rho(\theta) = \emptyset$ for all $\theta \neq (s,s)$, $\mu_i^i(\theta) = 1$ if $\theta_i = s$ and $\mu_i^i(\theta) = 0$ otherwise, $\mu_i^i(\emptyset) = 1/3$, $\alpha_i(\theta) = \theta_i$, $\alpha_i(\emptyset) = 3$. What makes this example work is the form of the principal's payoff function, which is similar to the one in

[16] Results in Milgrom (1981) and Seidmann and Winter (1997) imply that there is an equilibrium with full information revelation in our game. However, their uniqueness result does not apply to the public feedback case because the action and the type spaces are multidimensional. In fact, the example shows that there is an equilibrium with less than full information revelation.

our experiment and has the property that intermediate beliefs by both agents is better for the principal than extreme beliefs. It is easy to show that this property exists as long as principal's payoff function is symmetric, concave, and strictly supermodular in a.[17]

The above results and Bayes' rule imply the following prediction:

Prediction 1. In verifiable feedback:

1. Beliefs increase conditional on success and decrease conditional on failure;
2. Beliefs increase after positive feedback and decrease after negative or no feedback;
3. Beliefs do not depend on the feedback provided to the other agent;
4. If feedback is private, the principal reports truthfully to the agent who succeeds and either reports truthfully or gives no feedback to the agent who fails. If feedback is public, principal reports truthfully if both agents succeed and either reports truthfully or gives no feedback if one of the agents fails.

3.4.2 Unverifiable Feedback

If feedback is unverifiable and private, then there is no information transmission in equilibrium.

Proposition 3. *If feedback is private and unverifiable, then all equilibria are completely uninformative.*

Proof of this result is also simple. If some message induces higher beliefs for some agent, then all types of the principal would have an incentive to send that message, contradicting the hypothesis that this message induces higher beliefs. This again simply follows from the fact that principal's payoff is monotonic in each agent's beliefs.

[17] We should note that the partially informative equilibrium constructed in the example depends on the common knowledge assumption on p_i, which may not be satisfied in the experiments. We are grateful to a referee for pointing this out.

If feedback is public, then there is always a completely uninformative equilibrium and never a fully informative equilibrium. Furthermore, in any equilibrium, types (s, s) and (f, f) must always give the same feedback.

Proposition 4. *If feedback is public and unverifiable, then there is a completely uninformative equilibrium. In any equilibrium $\rho(s, s) = \rho(f, f)$ and hence fully informative equilibrium does not exist.*

Therefore, equilibrium is at most partially informative and whether feedback provides any information at all, depends on the payoff function of the principal. For instance, in Example 1 all equilibria are completely uninformative. Since principal's payoff function in that example is similar to the one in the experiment, we expect feedback to be uninformative in the experiment as well.

Since messages have no intrinsic meaning and are completely costless in our model, there is no precise prediction regarding the principal's strategy and agents' beliefs after feedback. However, in our experiment, as well as in real life, reports have a natural meaning and hence it is plausible to expect that a principal who observes success always reports success. This implies that, in equilibrium, the principal who observes failure must also report success. Therefore, we have the following prediction:

Prediction 2. In unverifiable feedback:

1. Beliefs do not change conditional on actual state;

2. Beliefs do not change in response to positive feedback and decrease or stay the same after negative or no feedback;

3. Principal always provides positive feedback.

The above analysis also implies the following prediction:

Prediction 3. Private-verifiable feedback is the most informative mechanism and private-unverifiable feedback is not informative.

3.5 Results

The main focus of our study is whether performance feedback is informative and whether this depends on the verifiability and privacy of the feedback. Section 3.5.1 mainly presents our results on this issue. Our model also produces theoretical predictions regarding the principal's behavior in different treatments. We therefore present a summary of the principals' behavior in Section 3.5.2 and discuss how it fits with the theoretical predictions.

3.5.1 Analysis of Agents' Behavior

We start with some summary statistics about task performance. On average (in both rounds), subjects attempted to solve 8.79 questions in the addition task and 10.47 questions in the verbal task, and correctly solved 7.08 and 7.82 questions, respectively. The answers to a survey question that asks whether it is important for subjects to succeed independently of its monetary payoff reveal that a majority of subjects do care about success per se.[18] This shows that we have managed to create an ego-relevant performance environment for subjects in our experiment, which is important for analyzing belief updating in a realistic fashion.

We first examine the initial (pre-feedback) beliefs of the subjects who have been assigned the role of an agent. Pre-feedback beliefs show that most agents prefer to perform in the second round: Average belief is 0.66 while the median is 0.7. In other words, on average, they believe that they will succeed with probability 0.66 if they were to perform the task. Since only 51% of the subjects successfully pass the target score upon entry, we conclude that participants overestimate their performance, i.e., they are overconfident. This is consistent with results from other real-effort experiments in the literature (e.g., Hoelzl and Rustichini (2005)), and highlights the benefit of using real effort, because in reality, overconfidence or self-serving biases may influence how agents interpret feedback given by the

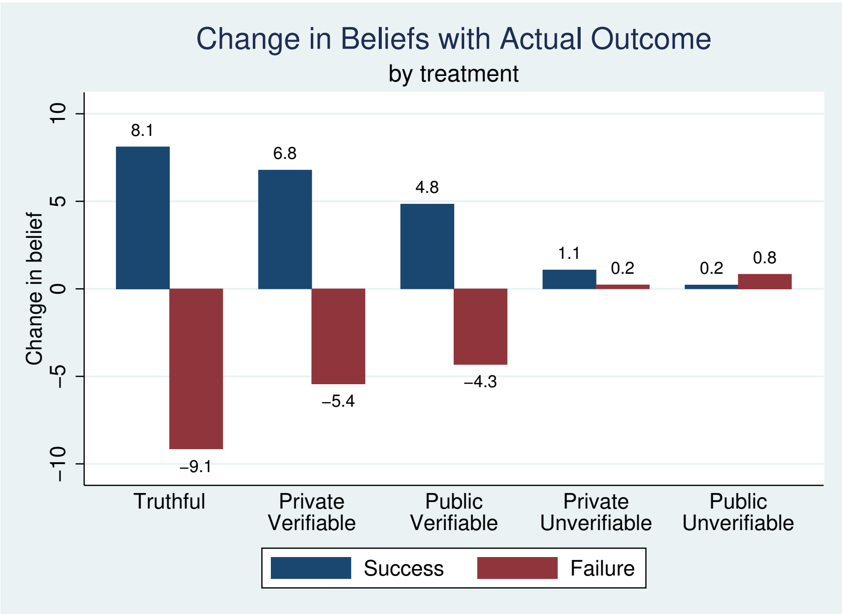
[18] The mean assessment of subjects is 3.73 on a 1-5 scale and 75.84% of them choose either the important or very important option (Appendix C.2.3, question 10).

principal.[19]

3.5.1.1 Information Transmission

We start by analyzing how beliefs change conditional on the actual performance outcome of the agent. If there is information transmission, then beliefs should move up for successful agents and down for unsuccessful ones. Figure 3.1 shows that there is information transmission in verifiable feedback and no information transmission in unverifiable feedback cases. Wilcoxon sign-rank tests indicate that the actual outcome has a significant effect on agents' beliefs in truthful and verifiable feedback treatments, while it has no significant impact in unverifiable feedback treatments.[20]

Figure 3.1: Information Transmission in Different Treatments



[19]In order to ensure that there is no selection bias, we test whether prior beliefs are independent of treatments and order configurations, and find no significant differences. Neither do we find differences in the prior beliefs of Bilgi and Koç University students. Related regressions are available upon request

[20]The p-values of Wilcoxon signed rank test for the hypothesis of a zero change in beliefs in each treatment is as follows: Truthful with $p = 0.0002$, private-verifiable with $p = 0.0004$, public-verifiable with $p = 0.013$, private-unverifiable with $p = 0.110$, public-unverifiable with $p = 0.787$ for success; truthful with $p = 0.0002$, private-verifiable with $p = 0.023$, public-verifiable with $p = 0.017$, private-unverifiable with $p = 0.696$, public-unverifiable with $p = 0.148$ for failure cases.

Likewise, regression analysis shows that private-verifiable feedback is not significantly different from truthful feedback under either success or failure, while public-verifiable feedback is significantly different from truthful feedback only under success (and only at the 10% level). In contrast, both types of unverifiable feedback lead to a change in beliefs that is further away from the effect of truthful feedback (see Table 3.1).[21]

Table 3.1: Information Transmission in Different Treatments

Dependent var:	(1)	(2)
Change in beliefs	Success	Failure
Private-Verifiable	-3.786	4.321
	(2.789)	(3.784)
Public-Verifiable	-5.039*	5.796
	(2.661)	(3.896)
Private-Unverifiable	-8.822***	10.883***
	(2.845)	(3.831)
Public-Unverifiable	-11.123***	10.745***
	(2.740)	(4.094)
Session	YES	YES
N	195	252
χ^2	27.145	22.272

GLS Regressions, standard errors in parentheses
Baseline is truthful feedback
* $p < .1$, ** $p < .05$, *** $p < .01$

Finally, Figure 3.1 and Table 3.1 show that private-verifiable feedback is the closest mechanism to truthful feedback.

We can summarize our findings as follows:

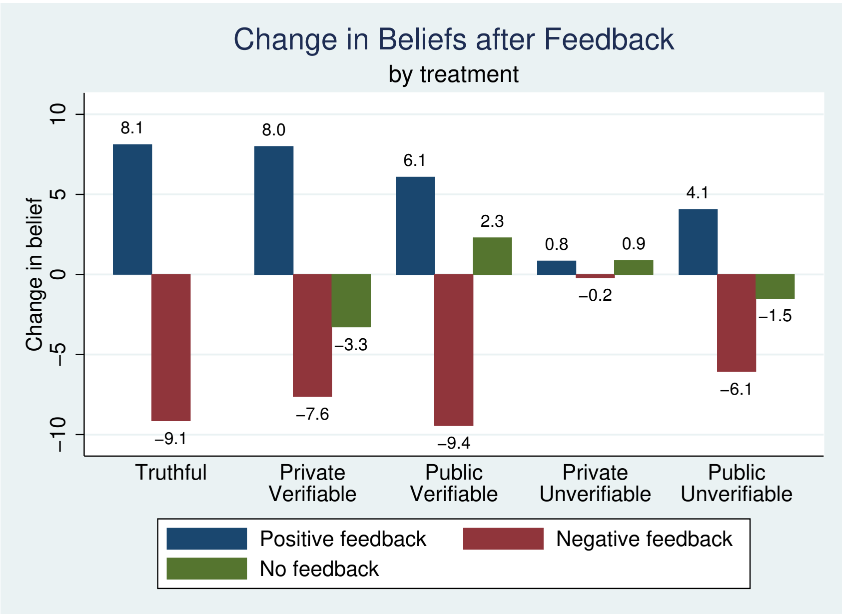
Result 3.1. Verifiable feedback is informative while unverifiable feedback is not. Private-verifible feedback is the most informative mechanism. These results confirm Predictions 1.1, 2.1, and 3.

[21] Note that we collect data over different rounds from the same subject in all of the treatments. Thus, to account for correlation, we use random effects model in regressions that use multiple observations from the same subject.

3.5.1.2 Impact of Feedback on Beliefs

Figure 3.2 and Wilcoxon signed-rank tests show that both positive and negative feedback have significant (positive and negative, respectively) effects on beliefs in all treatments except the private-unverifiable feedback treatment.[22] On the other hand, the change in beliefs after no feedback is not significantly different from zero in any of the treatments.[23]

Figure 3.2: Impact of Feedback on Beliefs



The regressions in Table 3.2 further explore the differences in the impact of feedback on beliefs across treatments. The table shows that in terms of direction, agents tend to discount the principal's feedback in all the treatments: positive feedback is interpreted less optimistically than truthful positive feedback and negative feedback less pessimistically. However, in response to positive feedback, the change in beliefs under private-verifiable feedback is not significantly different from that under truthful feedback, while all the other treatments induce significantly

[22] The p-values of Wilcoxon signed rank test for each treatment are as follows: Truthful with $p = 0.0002$ and $p = 0.0002$, private-verifiable with $p = 0.0002$ and $p = 0.047$, public-verifiable with $p = 0.002$ and $p = 0.003$, public-unverifiable with $p = 0.005$ and $p = 0.090$, private-unverifiable with $p = 0.236$ and $p = 0.487$, for positive and negative feedback cases respectively.

[23] The p-values of Wilcoxon signed rank test for each treatment are $p = 0.211$, $p = 0.980$, $p = 0.674$, $p = 0.710$, for private-verifiable, public-verifiable, private-unverifiable, and public-unverifiable, respectively.

lower beliefs than truthful positive feedback (see column (1)).[24] When subjects receive negative feedback, none of the treatments, except private-unverifiable feedback, is different from truthful feedback. Under no feedback, on the other hand, we find that there is no significant difference across treatments.

Table 3.2: Impact of Feedback on Beliefs

	(1) Change in belief (Positive feedback)	(2) Change in belief (Negative Feedback)	(3) Change in belief (No Feedback)	(4) Change in belief (Own positive feedback)	(5) Change in belief (Own negative feedback)
Private-Verifiable	-2.318	2.391	-1.079	-0.759	2.926
	(2.945)	(5.519)	(4.408)	(2.842)	(5.460)
Private-Unverifiable	-9.383***	12.224**	-0.449	-7.697***	12.116**
	(2.599)	(5.552)	(5.828)	(2.479)	(5.445)
Public-Verifiable	-5.017*	1.673	3.055		
	(2.879)	(5.063)	(4.617)		
Public-Unverifiable	-6.500**	4.101			
	(2.778)	(6.176)			
PUBLIC-VERIFIABLE					
Other positive feedback				-6.090*	-2.029
				(3.565)	(5.992)
Other negative feedback				-1.170	7.033
				(3.524)	(7.129)
PUBLIC-UNVERIFIABLE					
Other positive feedback				-6.909**	-5.040
				(3.143)	(8.665)
Other negative feedback				3.376	12.992
				(5.078)	(9.353)
Session	YES	YES	YES	YES	YES
N	207	149	91	207	149
χ^2	21.788	15.753	.	23.927	18.988

Standard errors in parentheses
GLS Regressions for Different Feedback Mechanisms
* $p < .1$, ** $p < .05$, *** $p < .01$

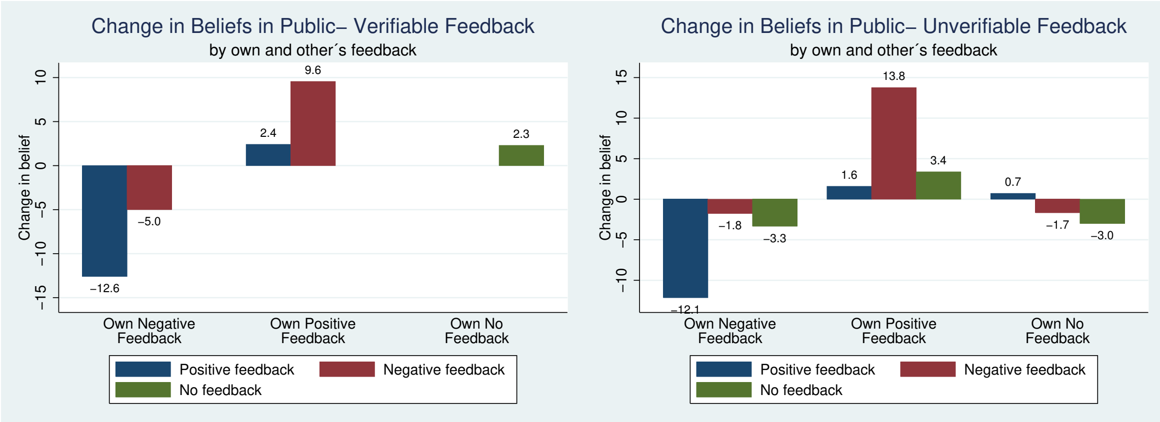
We can summarize our findings as follows:

Result 3.2. In all feedback mechanisms positive feedback increases and negative feedback decreases beliefs, but in private-unverifiable feedback these changes are insignificant. In all mechanisms, no feedback leads to only insignificant changes in beliefs. Predictions 1.2 and 2.2 are confirmed except that no feedback does not decrease beliefs in verifiable feedback and positive feedback increases beliefs in public-unverifiable feedback.

[24] Note, however, that the coefficient of public-verifiable feedback is only marginally significant.

Figure 3.3: Change in Beliefs in Public Feedback

(a) Public-Verifiable Feedback (b) Public-Unverifiable Feedback



The above findings suggest that public feedback can have quite different effects than private feedback. To explore this further, we look at whether beliefs are affected by the feedback provided to the other agent in public feedback treatments. Figures 3.3a and 3.3b show, for each type of own feedback received, whether beliefs respond to the other person's feedback in verifiable and unverifiable cases, respectively. We can see that in both treatments beliefs are affected adversely when the other agent has received positive feedback as opposed to negative feedback. Secondly, the magnitude of this effect is larger under unverifiable feedback than under verifiable feedback.

Columns (4) and (5) of Table 3.2 test whether, in public feedback treatments, the other agent's feedback makes a difference in belief updating, when own feedback is positive and negative, respectively. The results support the conclusions we have drawn from Figure 3.3 and further show that the adverse effect of the other agent's positive feedback is significant only if own feedback is positive as well and that the effect is significant only at the 10% level in public-verifiable feedback. Column (4) of Table 3.2 also shows that the less optimistic response to verifiable positive feedback, i.e., the negative coefficient of public-verifiable feedback in column (1), comes from observations where own positive feedback is accompanied with positive feedback to the other agent.[25]

[25]These results are robust to taking the dependent variable to be the posterior beliefs and

Therefore, we conclude that:

Result 3.3. In public feedback, beliefs are affected adversely when the other agent also receives positive feedback. This effect is stronger if own feedback is also positive and larger under unverifiable feedback. Therefore, Prediction 1.3 is rejected.

3.5.1.3 Impact of Beliefs and Feedback on Performance

Although it is not the focus of our study, we also examine how beliefs and feedback affect second-round performance. Note that in our experiment, only the agents whose posterior beliefs are larger than a randomly determined threshold perform in the second round and the rest simply receive a randomly determined payoff. In order to minimize ability-based selection and to be able to observe the effect of beliefs on the second-round performance for a relatively unbiased set of subjects, the random device in the belief elicitation mechanism was skewed toward inducing subjects to enter.[26] Consequently, 87% of the subjects performed in the second round. Table 3.3 shows that, controlling for the first round performance, higher beliefs lead to higher second-round performance, and hence the principal has an additional incentive to induce higher beliefs. We also checked the impact of feedback on performance, both overall and in each treatment separately, and found no significant effect. (These results are available upon request.) Overall, although our experiment is not designed to analyze this issue, we have the following result.

Result 3.4. Inducing higher beliefs increases performance but interim performance feedback is not an effective tool in this respect.

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controlling for the prior beliefs as a regressor.

[26]Note that this does not affect the incentive compatibility of the mechanism.

Table 3.3: Impact of Beliefs on Performance

	(1)
	Post-feedback Performance
Pre-feedback Performance	0.442***
	(0.049)
Change in beliefs	0.013*
	(0.008)
Session	YES
N	392
χ^2	130.599

GLS regressions, standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

3.5.2 Analysis of Principals' Behavior

We now turn to explore the principals' side. We first categorize the messages sent by the principals under different feedback mechanisms, depending on the actual outcome (Table 3.4). As expected, if the actual outcome is success and the principal can privately convey it, a positive message is transmitted in almost all cases, both verifiable (97%) and unverifiable (94%). The percentage of positive messages under public feedback when the actual outcome is success is somewhat lower (82% in verifiable and 85% in unverifiable). This difference between private and public reporting is statistically significant only in the verifiable case ($p = 0.045$ in verifiable and $p = 0.265$ in unverifiable feedback, according to a test of proportions).

Table 3.4: Feedback to Individual Agents under Different Treatments

	Private-Verifiable (message)			Public-Verifiable (message)			Private-Unverifiable (message)				Public-Unverifiable (message)			
Actual	Info	No Info	Total	Info	No Info	Total	S	F	No	Total	S	F	No	Total
S	31	1	32	31	7	38	30	0	2	32	29	2	3	34
	(96.88)	(3.13)	(39.02)	(81.58)	(18.42)	(46.34)	(93.75)	(0.00)	(6.25)	(39.02)	(85.29)	(5.88)	(8.82)	(45.95)
F	22	28	50	25	19	44	27	17	6	50	15	10	15	40
	(44.00)	(56.00)	(60.98)	(56.82)	(43.18)	(53.66)	(54.00)	(34.00)	(12.00)	(60.98)	(37.50)	(25.00)	(37.50)	(54.05)
Total	53	29		56	26		57	17	8		44	12	18	
	(64.63)	(35.37)		(68.29)	(31.71)		(69.51)	(20.73)	(9.76)		(59.46)	(16.22)	(24.32)	

Number of agents receiving each message type, percentages in parentheses.

S= *Successful*, F= *Failed* and No= *No message*.

Table 3.4 shows that principals prefer to transmit information 44% of the time when the outcome is failure in the private-verifiable case, while the frequency of transmission is 57% under public-verifiable feedback. So in both cases, around half of the time the bad outcome is revealed. This might be either because in equilibrium verifiable negative feedback and no feedback are interpreted similarly, or more likely because some principals have a preference for reporting truthfully.

Similarly, Table 3.4 shows that when the outcome is failure principals lie and give positive feedback in 54% of the cases in private-unverifiable and 38% of the cases in public-unverifiable feedback. On the other hand, when the outcome is success, they report truthfully in 94% of the cases in private-unverifiable and 85% of the cases in public-unverifiable feedback. This is again consistent with lying aversion. A Pearson chi-square test shows that reports significantly change according to the actual outcome when talk is cheap ($p = 0.0003$ in private-unverifiable and $p = 0.0002$ in public-unverifiable feedback). This confirms that principals consider the actual outcome in reporting, rather than sending random or always positive signals regardless of the true state.

Although the number of observations is small, Table 3.5 and 3.6 provide further detail that may help identify the reporting strategies used by the subjects. It seems that when the outcome is success principals always report truthfully, whereas when the outcome is failure, some report truthfully, some lie if they can, and others report no information.

Table 3.5: Feedback to Both Agents in Private Feedback

	Private-Verifiable (message)				Private-Unverifiable (message)								
Actual	Both Info	Info,No Info	No Info, Info	Both No Info	SS	SF	FS	FF	No-No	SNo	NoS	FNo	NoF
SS	5	0	0	0	6	0	0	0	0	0	0	0	0
	(100.00)	(0.00)	(0.00)	(0.00)	(100.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
SF	7	4	1	0	7	1	0	0	0	1	0	0	0
	(58.33)	(33.33)	(8.33)	(0.00)	(77.78)	(11.11)	(0.00)	(0.00)	(0.00)	(11.11)	(0.00)	(0.00)	(0.00)
FS	5	0	5	0	4	0	5	0	0	2	0	0	0
	(50.00)	(0.00)	(50.00)	(0.00)	(36.36)	(0.00)	(45.45)	(0.00)	(0.00)	(18.18)	(0.00)	(0.00)	(0.00)
FF	4	1	0	9	5	1	0	5	1	1	2	0	0
	(28.57)	(7.14)	(0.00)	(64.29)	(33.33)	(6.67)	(0.00)	(33.33)	(6.67)	(6.67)	(13.33)	(0.00)	(0.00)

Number of agents receiving each message type, percentages in parentheses

The abbreviations in message part represents message pairs.

S= *Successful*, F= *Failed* and No= *No message*. SNo refers to (*Successful*, *No message*) message pair.

Table 3.6: Feedback to Both Agents in Public Feedback

	Public-Verifiable (message)		Public-Unverifiable (message)								
Actual	Both Info	Both No Info	SS	SF	FS	FF	No-No	SNo	NoS	FNo	NoF
SS	8	0	3	0	0	0	0	0	1	0	0
	(100.00)	(0.00)	(75.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(25.00)	(0.00)	(0.00)
SF	6	3	4	3	0	1	0	4	0	0	1
	(66.67)	(33.33)	(30.77)	(23.08)	(0.00)	(7.69)	(0.00)	(30.77)	(0.00)	(0.00)	(7.69)
FS	9	4	7	1	3	0	0	1	1	0	0
	(69.23)	(30.77)	(53.85)	(7.69)	(23.08)	(0.00)	(0.00)	(7.69)	(7.69)	(0.00)	(0.00)
FF	5	6	1	0	0	1	5	0	0	0	0
	(45.45)	(54.55)	(14.29)	(0.00)	(0.00)	(14.29)	(71.43)	(0.00)	(0.00)	(0.00)	(0.00)

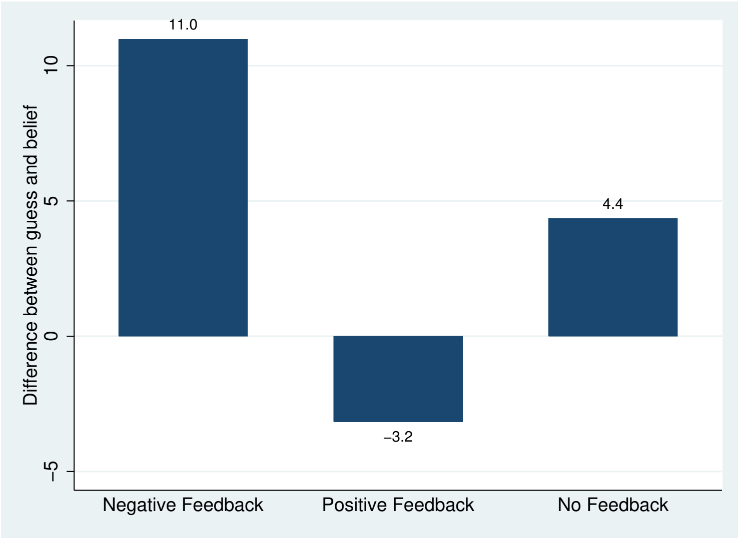
Number of agents receiving each message type, percentages in parentheses

The abbreviations in message part represents message pairs.

S= *Successful*, F= *Failed* and No= *No message*. SNo refers to (*Successful*, *No message*) message pair.

Our design also allows us to observe the expectations of the principals regarding how agents will update their beliefs. This can potentially give insights into the rationale behind the principals' strategy. As shown in Figure 3.4, principals expect the positive feedback they send to be interpreted more optimistically than it actually is (although this is not significant in a Wilcoxon test), and negative messages to be evaluated significantly more pessimistically ($p = 0.003$ in a Wilcoxon test). Thus, principals generally overestimate the response of agents' beliefs to the feedback, especially when the feedback is negative. The expectation of a pessimistic response to negative feedback reveals that at least some principals take into account its adverse effect on beliefs but provide negative feedback anyway, which is consistent with an aversion to lying.

Figure 3.4: Difference between Principal's Guess and Actual Belief



Finally, we examine principals' expectations regarding how agents' beliefs will be influenced by the feedback given to the other agent. As Table 3.7 shows, principals expect that a positive feedback to the other agent will adversely influence the beliefs of an agent when his own feedback is also positive and when feedback is public and unverifiable, but expect no significant impact if own feedback is negative or feedback is verifiable.[27] Interestingly, this is a feature of the equilibrium of the model with lying costs and naive agents, which will be analyzed in Section 3.6.

Table 3.7: Principal's Expectations in Public Feedback-Extended

	Own Positive Feedback			Own Negative Feedback		
	(1)	(2)	(3)	(4)	(5)	(6)
	Guess	Guess	Guess	Guess	Guess	Guess
		(Public-Verifiable)	(Public-Unverifiable)		(Public-Verifiable)	(Public-Unverifiable)
Other Positive Feedback	-3.151	3.708	-10.195**	-1.550	-0.333	-3.323
	(3.198)	(5.692)	(4.176)	(8.969)	(7.903)	(17.649)
Public-Verifiable	10.188***			4.722		
	(2.960)			(3.169)		
N	75	31	44	37	25	12
χ^2	14.735	0.424	5.959	2.250	0.002	0.035

Standard errors in parentheses
GLS Regressions for Different Feedback Mechanisms
* $p < .1$, ** $p < .05$, *** $p < .01$

We can summarize our findings as follows.

[27]Note, however, that the number of observations is small in some of these regressions.

Result 3.5. Some principals prefer to tell the truth even when they know that this might adversely affect their payoff. Prediction 1.4 is confirmed but 2.3 is rejected.

3.6 Discussion

Overall, our theoretical model in Section 3.4 does a good job in terms of explaining the relative informativeness of different feedback mechanisms. There are, however, three major discrepancies between our theoretical predictions and empirical findings: (1) Some principals report truthfully even when they believe that this may hurt them; (2) Agents do not interpret "no feedback" as pessimistically as the theory suggests; (3) Positive feedback is interpreted less optimistically if the other agent also receives positive feedback and this effect is stronger in public-unverifiable than in public-verifiable feedback.

The finding that some principals have a tendency to tell the truth is in line with previous empirical studies of strategic communication and suggests that individuals suffer from cost of lying and this cost varies among them. The second finding might be due to naiveté in belief formation, i.e., agents interpret the feedback literally and when they receive "no information", they keep their priors more or less unchanged. Another finding that supports the naive agent hypothesis is that, even in private-verifiable feedback, a significant fraction of principals provide no information when the agent has failed. Since "no information" and negative feedback must both be interpreted in the same (pessimistic) way in private-verifiable feedback, this is not rational if there is even a minimal preference for telling the truth. If, however, principals believe that some of the agents are naive, then this may be optimal. Indeed, Figure 3.4 and the preceding discussion have indicated that principals expect agents to respond to feedback in a somewhat naive way. Therefore, we conclude that at least some agents are naive and that principals expect them to act naively. The third finding could be due to the fact that agents make (non-Bayesian) social comparisons in forming their beliefs or they believe that the difficulty of the tasks are correlated in such a way that if the other agent has succeeded, then probability of own success in the next task is smaller. Another possible explanation of this finding is that agents are rational and such beliefs simply follow from the principals' strategy and Bayes' rule.

In the next section we extend our theoretical model to allow for individual-specific cost of lying (and cost of withholding information) for the principals and naiveté on the part of the agents. We will see that such an extension can account for most of our empirical findings as well as some of the above discrepancies between the predictions of the original model and the data.

3.6.1 Cost of Lying and Naive Agents

Suppose that lying or providing no information has an individual specific cost associated with it. Let $c(r|\theta)$ be the cost of sending report r when the state is θ and assume that it is distributed according to the probability distribution $F_{r|\theta}$ in the population. Also assume that (1) telling the truth is costless; (2) there are some individuals for whom the cost of lying is small; (3) there are some who always prefer to tell the truth; (4) there are some for whom the difference between the cost of lying and cost of withholding information is small enough; and (5) there are some who prefer withholding information to lying.[28]

A fraction $\eta \in (0, 1)$ of agents are naive, i.e., they believe that the state is exactly equal to the principal's report and if the report is "no information", then they keep their prior unchanged. Let $q_i(r|\theta)$ denote the fraction of principals with type θ who send report r to agent i in private feedback, and $q(r|\theta)$ denote the same fraction in public feedback.

Before we present our results, we should briefly discuss the few existing theoretical studies of cheap talk games with lying costs and naive agents. Kartik et al.

[28]These assumptions are equivalent to the following: (1) $F_{\theta|\theta}(x) = 1$ for all $x \geq 0$; (2) $F_{r|\theta}(x) > 0$ for all r, θ and $x > 0$; (3) $F_{r|\theta}(v(a_1(1), a_2(1), \theta) - v(a_1(0), a_2(0), \theta) < 1$; (4) $c(s, r_{-i}|f, \theta_{-i}) - c(\emptyset, r_{-i}|f, \theta_{-i})$ is a non-negative random variable with probability distribution $G(.|r_{-i}, \theta_{-i})$ such that $G(x|r_{-i}, \theta_{-i}) > 0$ for all r_{-i}, θ_{-i} and $x > 0$; (5) $G(v(a_i(1), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) - v(a_i(0), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i})|r_{-i}, \theta_{-i}) < 1$ for all r_{-i}, θ_{-i}.

(2007) show that if the message space is not bounded, then there is a fully revealing equilibrium. Our message space is bounded, which makes full information revelation impossible in the unverifiable feedback case. Kartik (2009) assumes that the sender has a convex cost of lying and characterizes a class of monotone equilibria in which low types separate while high types pool. Chen (2011) analyzes a related model in which the sender is honest and the receiver is naive with positive probabilities and shows that dishonest senders exaggerate the state of the world. Our results do not immediately follow from these two studies because we assume both cost of lying and naive agents and allow cost of lying to differ among senders. Also, we allow sending "no information" and analyze verifiable messages as well as cheap talk.

3.6.1.1 Verifiable Feedback with Lying Cost and Naive Agents

As the following result shows, under private-verifiable feedback, equilibrium behavior is uniquely determined.

Proposition 5. *If feedback is private and verifiable, then for any i and θ_{-i}*

$$q_i(s|s,\theta_{-i}) = 1,\ q_i(\emptyset|f,\theta_{-i}) > 0,\ q_i(f|f,\theta_{-i}) > 0.$$

Therefore, in equilibrium, if the agent is successful, then the principal gives positive feedback, while if he has failed, then those principals with small costs of withholding information give no feedback while those with large costs report failure. Proportion of principals who give no feedback increases in the fraction of naive agents and the extra benefit of letting the agent keep the prior beliefs. Note that in our model of Section 3.4, which assumed lying is costless, behavior of the principal when the agent has failed was indeterminate, i.e., sending negative feedback and no feedback were both compatible with equilibrium. In the current model, principal's behavior is unique given his cost of lying. Also note that if there were no naive agents, then in equilibrium we would not observe any principal who provides no feedback.

Proposition 5 and Bayes' rule imply that beliefs significantly increase after positive feedback and decrease after negative feedback, while beliefs after no feedback decrease but at a magnitude smaller than beliefs after negative feedback. Beliefs conditional on success increase and conditional on failure decrease. (See Appendix C.1 for the calculation of beliefs in this section.)

Equilibrium behavior is also unique in public-verifiable feedback.

Proposition 6. *If feedback is public and verifiable, then* $q(ss|ss) = 1$, $q(\emptyset|ff) > 0$, *and* $q(ff|ff) > 0$. *If*

$$\eta^2 v(a_1(p), a_2(p), sf) + 2\eta(1-\eta)v(a_1(p), a_2(0), sf) + (1-\eta)^2 v(a_1(0), a_2(0), sf) > v(a_1(1), a_2(0), sf) \quad (3.1)$$

then, $q(\emptyset|sf) > 0$ *and* $q(\emptyset|fs) > 0$. *If condition (3.1) does not hold, then there is an equilibrium in which* $q(\emptyset|sf) = q(\emptyset|fs) = 0$.

This result shows that if both agents are successful, then the principal truthfully reports it. If both have failed, then some tell the truth while others give no feedback. The fraction of principals who provide no feedback increases with the prior and the proportion of naive agents.

Behavior of the principal when only one of the agents has succeeded depends on condition (3.1), which is likely to hold if the fraction of naive agents is high and the agents' action are complements. Since in our experiment there are strong complementarities between the agents' actions, we expect this condition to hold and hence some principals with types sf and fs to give no feedback. This is exactly the type of behavior we observe in the data (see Table 3.6).

Therefore, we assume that condition (3.1) holds, in which case Bayes' rule implies that beliefs increase after positive feedback and decrease after negative feedback. Direction of change in beliefs after no feedback is ambiguous, but they decrease less than they do in private-verifiable feedback. If beliefs about the other agent is uniform, then average beliefs conditional on failure is smaller than the prior but it is not clear whether beliefs conditional on success is greater than the prior.

In summary, we have the following predictions:

Prediction 4. If lying is costly and some agents are naive, then in verifiable feedback:

1. If feedback is private, beliefs increase conditional on success and decrease conditional on failure. If feedback is public, beliefs decrease conditional on failure but the magnitude of change is smaller than it is under private feedback;

2. Beliefs increase after positive feedback and decrease after negative feedback. If feedback is private, beliefs also decrease after no feedback;

3. Beliefs after negative feedback are smaller than beliefs after no feedback;

4. If feedback is public, beliefs do not depend on the feedback provided to the other agent;

5. If feedback is private, all principals report truthfully to the agent who succeeds while some report truthfully and some give no feedback to the agent who fails. If feedback is public, all principals report truthfully if both agents succeed, while some tell the truth and some give no feedback if one of the agents fails.

Our empirical findings verify prediction 4.1 (see Figure 3.1) as well as 4.2 and 4.3 (see Figure 3.2), except that the decrease in beliefs after no feedback is not statistically significant in private feedback. Note that prediction 4.3 is novel in the new model and follows from the existence of naive agents. Also note that in the data, beliefs after no feedback increase in public-verifiable feedback, which cannot be explained with our original model. In the model with lying costs, this could happen if each agent assigns a disproportionately high likelihood to the event that he has succeeded and the other has failed, i.e., agent 1 believes that the state is sf while agent 2 believes that it is fs. Finally, while prediction 4.5 is verified (see Tables 3.5 and 3.6), 4.4 is rejected (see Figure 3.3a and Table 3.2 columns (4) and (5)). Overall, empirical observations are very close to theoretical predictions except that in the data beliefs are somewhat more pessimistic if own positive feedback is accompanied by positive feedback to the other agent.

3.6.1.2 Unverifiable Feedback with Lying Cost and Naive Agents

The most significant difference between the models with and without lying costs appears under unverifiable feedback. In particular, and unlike the original model, the model with lying costs and naive agents uniquely pins down the principal's behavior under private-unverifiable feedback. If the agent is successful, the principal sends positive feedback and if he has failed, then those with high costs of lying and withholding information report truthfully, those with small costs of lying report success, and those with larger costs of lying but small costs of withholding give no feedback.

Proposition 7. *If feedback is private and unverifiable, then for any i and θ_{-i}*

$$q_i(s|s,\theta_{-i}) = 1,\ q_i(s|f,\theta_{-i}) > 0,\ q_i(\emptyset|f,\theta_{-i}) > 0,\ q_i(f|f,\theta_{-i}) > 0.$$

Equilibrium behavior under public-unverifiable feedback may not be unique. However, if v is separable, i.e., $v(a,\theta) > v(a',\theta)$ implies $v(a,\theta') > v(a',\theta')$, then the following is true.

Proposition 8. *If v is separable and feedback is public and unverifiable, then $q(ss|ss) = 1$, $q(ss|\theta) > 0$ for some $\theta \neq ss$ and $q_i(\theta|\theta) > 0$ for all θ.*

A natural extension of the private-unverifiable feedback equilibrium to public case along the lines suggested by Proposition 8 is the following: (1) Type ss sends ss; (2) Type ff sends ss, $\emptyset\emptyset$, or ff; (3) Type sf sends ss, $s\emptyset$, or sf; (4) Typefs

sends ss, $\emptyset s$, or fs. This is exactly the type of behavior we observe in the data. Therefore, we assume that this is the equilibrium that our subjects play.

Using Bayes' rule to derive the beliefs, we have the following prediction.

Prediction 5. If lying is costly and some agents are naive, then in unverifiable feedback:

1. Beliefs conditional on success are smaller than those in private-verifiable feedback;

2. Beliefs after positive feedback are smaller than those in verifiable feedback;

3. Beliefs decrease after negative and no feedback at a magnitude similar to those in verifiable negative feedback;

4. Beliefs after positive feedback are smaller if the other agent receives positive feedback as well, while beliefs after negative feedback are not affected by the feedback to the other agent;

5. All principals report truthfully to the agent who succeeds but, to the agent who fails, some principals report success, some no information and some failure.

Our empirical findings verify predictions 5.1 (see Figure 3.1) and 5.2. Prediction 5.3 is not supported because the decrease in beliefs after negative or no feedback is smaller compared with verifiable feedback (see Figure 3.2). Except for a few outliers, Tables 3.5 and 3.6 give strong support to 5.5. Perhaps most remarkably, item 5.4 is strongly supported (see Figure 3.3b and Table 3.2 columns (4) and (5)). Note that this prediction is novel to the new model and follows from the fact that feedback (s, f) is given only by the principal who observed (s, f) whereas (s, s) is given by types (s, s), (s, f), (f, s), and (f, f). Therefore, a Bayesian agent 1 who receives feedback (s, f) is sure that he succeeded, while if he receives feedback (s, s), then he assigns a positive probability that he failed. Finally, the new model, as well as the original one, predicts the private-verifiable feedback to be the most informative mechanism, which is supported by our findings.

Overall, the model fits the data quite well, and in some respects better than the original model, but there are still two deviations of the model's predictions from what we observe in the data: (1) Agents interpret other agent's success pessimistically even in verifiable feedback; (2) Agents do not interpret negative or no feedback as pessimistically as they should, particularly in private-unverifiable feedback.

3.6.2 Are Agents Bayesian?

Suppose that agents know (or predict) the strategy employed by the principals in our experiment and use Bayes' rule to update their beliefs. How would their beliefs change upon observing feedback? How do actual beliefs compare with such Bayesian beliefs?

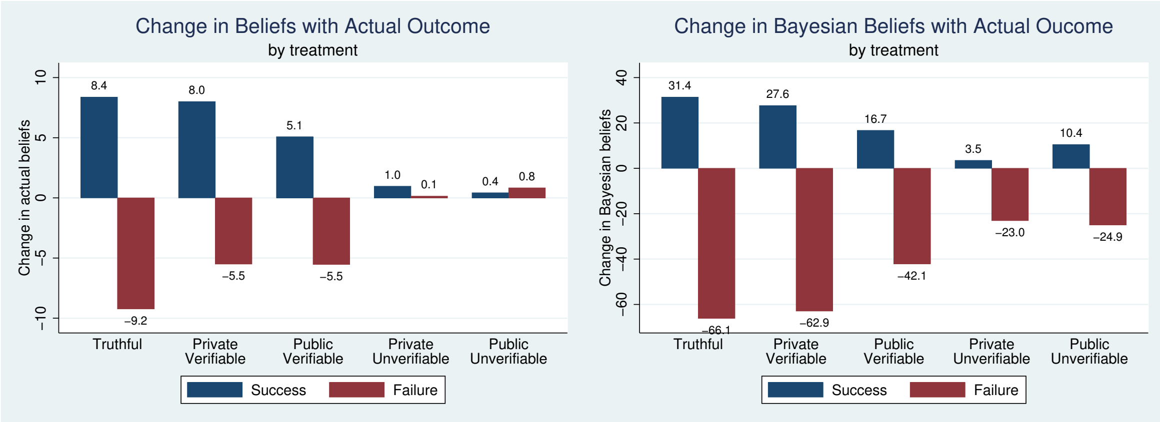
In order to answer these questions, we estimate the principals' strategy using the data in Table 3.5 and 3.6 and then use each agents' pre-feedback beliefs, the feedback they received, and Bayes' rule to calculate post-feedback beliefs.[29] Before we start presenting our findings, we should stress that we are subjecting the agents to quite a stringent test. A perfect fit between the actual and Bayesian beliefs requires not only that they use Bayes' rule correctly to update their beliefs but also that they predict the principals' strategy perfectly.

Figure 3.5 plots the average change in actual and Bayesian beliefs in each treatment conditional on the actual outcome of the agent. We can see that the direction of change is the same in actual and Bayesian beliefs, except under unverifiable feedback when the actual outcome is failure. Also, compared with the Bayesian case, overall information transmission is much weaker when the actual

[29]In calculating principals' strategy we eliminated some outliers in tables 3.5 and 3.6: in private-verifiable, row SF column No Info,Info; in private-unverifiable, row FS column SNo; in public-unverifiable, row SS column NoS, row FS columns SF and SNo, row SF columns FF and NoF.

Figure 3.5: Information Transmission: Actual vs. Bayesian

(a) Actual Beliefs (b) Bayesian Beliefs



state is failure.[30]

Figure 3.6 plots average change in actual and Bayesian beliefs in response to feedback. We again see that the direction of change in beliefs is the same in actual and Bayesian beliefs (except those in public-verifiable and private-unverifiable treatments after no feedback). This figure also supports our conclusion from the previous section that agents do not interpret negative or no feedback as pessimistically as they should, especially when they are unverifiable.

Finally, we compare the change in beliefs in response to the other agent's feedback in public-unverifiable feedback. As Figure 3.7 shows, as long as the direction of change in the beliefs are concerned, agents on average act in a Bayesian manner. However, and as we have discovered before, they seem to interpret positive feedback to the other agent more pessimistically than is justified by Bayesian updating alone.

[30]We should note that the scale of the graphs in 3.5a and 3.5b are different because agents update their beliefs by amounts that are much smaller than the theoretical ones. For example, under truthful feedback, Bayes' rule requires that beliefs go up to 1 after success and down to 0 after failure, whereas in reality they go up to 0.77 and down to 0.57, respectively. This is simply because in the theoretical model beliefs refer to the probability that they have been successful in the task they have just finished, while in the experiment they measure the probability with which they believe they will be successful in the next task. We expect the latter to be strictly increasing in the former but not necessarily identical with it. Also note that numbers in Figure 3.5a are slightly different than those in Figure 3.1. This is because we had to drop a few observations for which we could not apply Bayes' rule in calculating beliefs. In order to maintain comparability between the actual and Bayesian beliefs we also dropped those observations in calculating the average change in actual beliefs. These comments also apply to the the other graphs in this section.

Figure 3.6: Change in Beliefs with Feedback: Actual vs. Bayesian

(a) Actual Beliefs (b) Bayesian Beliefs

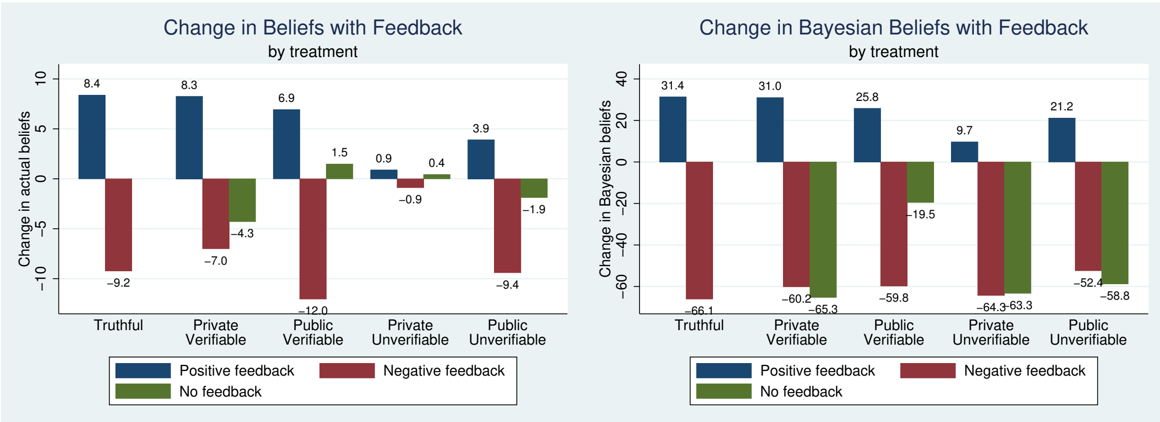


Figure 3.7: Change in Beliefs with Other's Feedback: Actual vs. Bayesian

(a) Actual Beliefs (b) Bayesian Beliefs

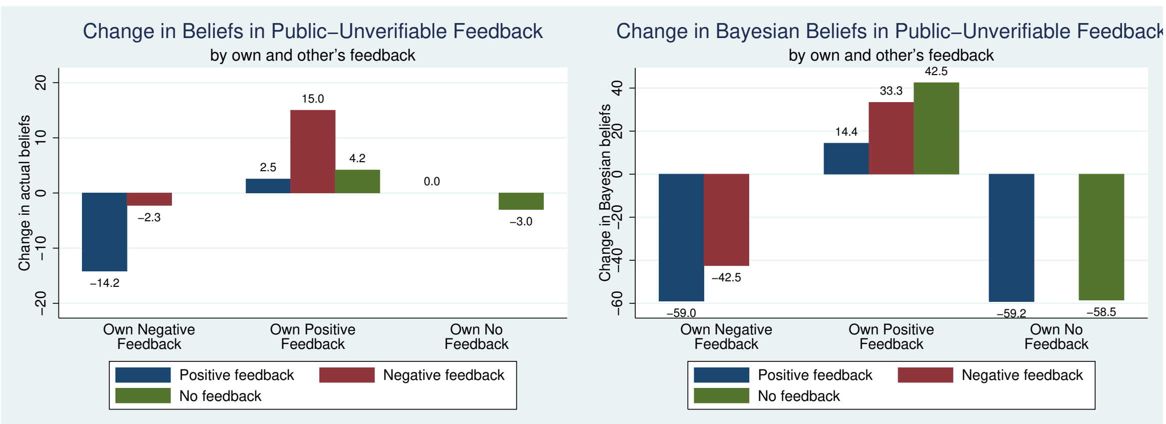


Table 3.8: Post-Feedback Beliefs

	(1) Post-Feedback Belief	(2) Post-Feedback Belief	(3) Post-Feedback Belief
Bayesian Belief	0.198***	0.154***	0.156***
	(0.024)	(0.022)	(0.022)
Pre-Feedback Belief		0.655***	0.649***
		(0.057)	(0.059)
Private-Verifiable			1.723
			(2.724)
Public-Verifiable			-0.433
			(3.028)
Private-Unverifiable			0.362
			(2.819)
Public-Unverifiable			-1.278
			(2.749)
N	386	386	386
χ^2	67.137	213.684	232.519
R^2	0.205	0.544	0.545

GLS Regressions, standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

In Table 3.8, we present regression results which show that Bayesian updating plays a significant role in the formation of actual beliefs and explains about 20% of their total variation. We also see that, together with the prior, Bayesian updating explains about half of the total variation in the posterior beliefs. Furthermore, the relationship between actual and Bayesian beliefs do not depend on the feedback mechanism in a significant way.

Overall, we conclude that agents' beliefs are consistent with the strategy employed by the principals and Bayesian updating, except that they respond to negative or no feedback more optimistically and interpret positive feedback to the other agent more pessimistically than they should. However, Bayesian updating does not explain the entire variation in beliefs. This could be due to agents' inability to correctly anticipate the principal's strategy, their naiveté, or other biases they suffer in processing information, such as self-serving biases and non-Bayesian social comparisons.

3.7 Conclusion

In this paper, we employ a theoretical model and data from a laboratory experiment to examine the role of verifiability and privacy in strategic interim performance feedback. Our baseline theoretical model predicts that information about agents' performances can be credibly revealed only when the performance information is verifiable and, furthermore, private-verifiable feedback is the most informative mechanism. These predictions are strongly supported by our empirical analysis.

However, the baseline model cannot account for some interesting features of the data: (1) many principals tell the truth even when they believe this may hurt them; (2) agents do not interpret "no feedback" as pessimistically as they should; and (3) positive feedback is interpreted less optimistically if the other agent has also received positive feedback, and this effect is stronger in the case of public-unverifiable than in the case of public-verifiable feedback. We then analyze a model with individual-specific lying costs and naive agents, and show that it can account for many of these findings. We also find that while many agents do take into account the principal's strategic behavior to form beliefs in a Bayesian fashion, some are naive and act in a non-Bayesian manner, particularly when informed about other agents' feedback. From a more practical point of view, we conclude that credible communication of interim performance requires verifiability and it is best to keep feedback private.

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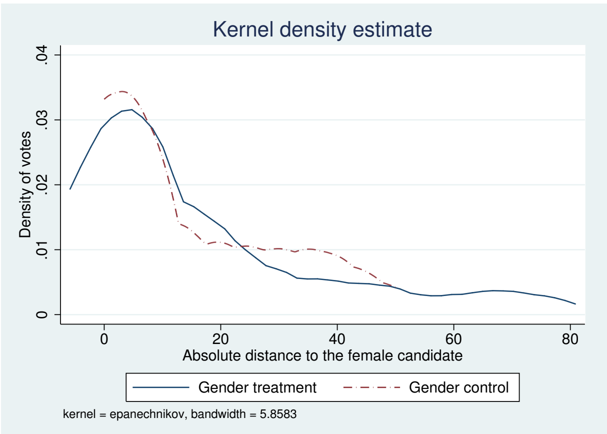
Appendix A

Appendix: Chapter 1

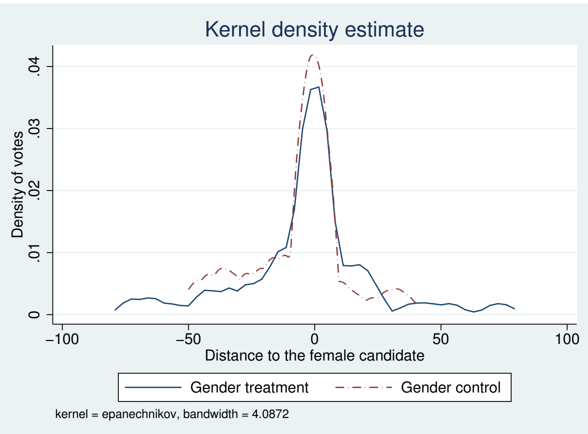
A.1 Tables and Figures

Figure A.1: Same-sex votes

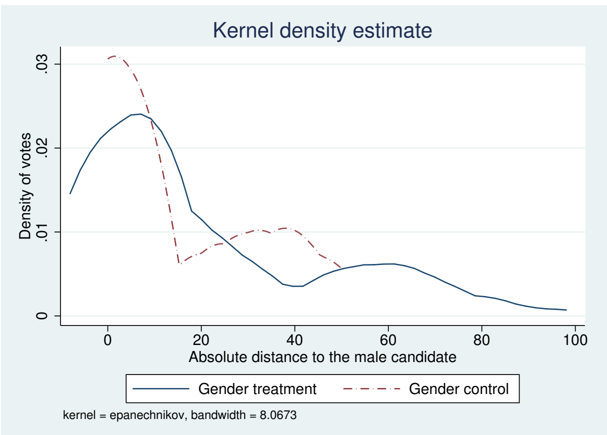
(a) Absolute distance-Female voter to Female candidate



(b) Distance- Female Voter to Female candidate



(c) Absolute distance- Male Voter to Male candidate



(d) Distance- Male Voter to Male candidate

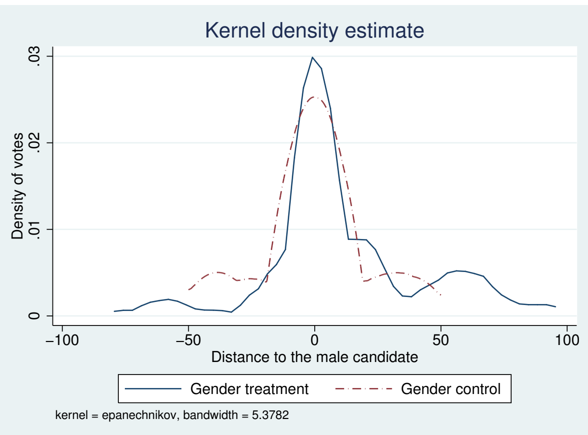
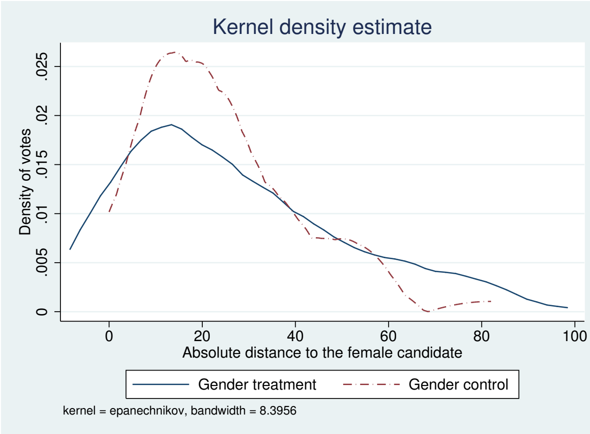
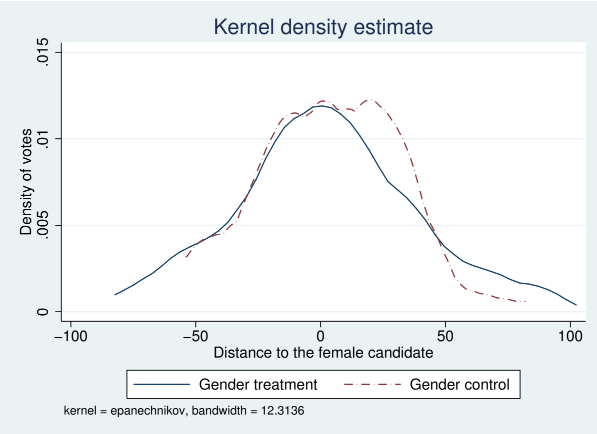


Figure A.2: Opposite-sex votes

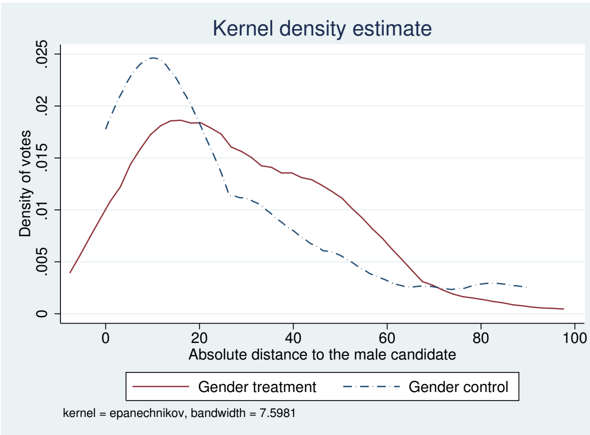
(a) Absolute distance- Male Voter to Female candidate



(b) Distance- Male Voter to Female candidate



(c) Absolute distance- Female Voter to Male Candidate



(d) Distance- Female Voter to Male candidate

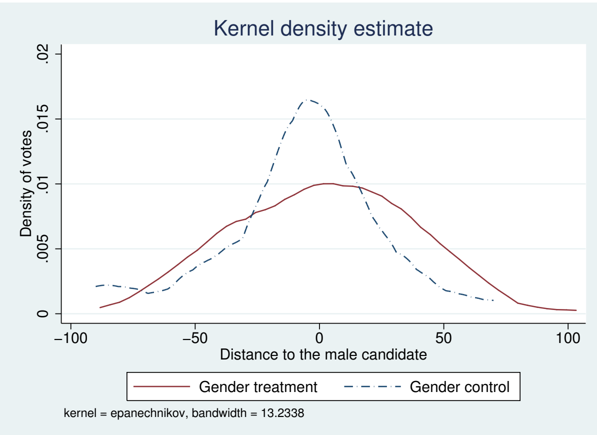
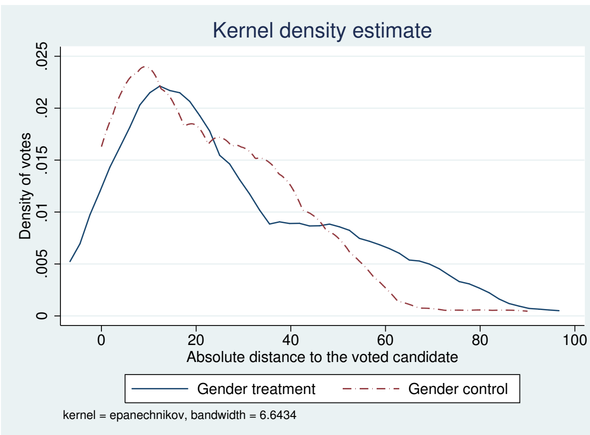


Figure A.3: Comparison of Vote Distributions over Gender Treatment and Control Groups (with exclusion of candidates)

(a) Absolute distance to the Voted Candidate



(b) Distance to the Voted Candidate

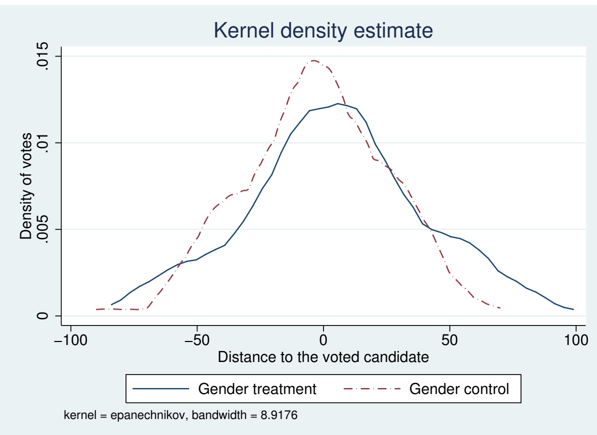
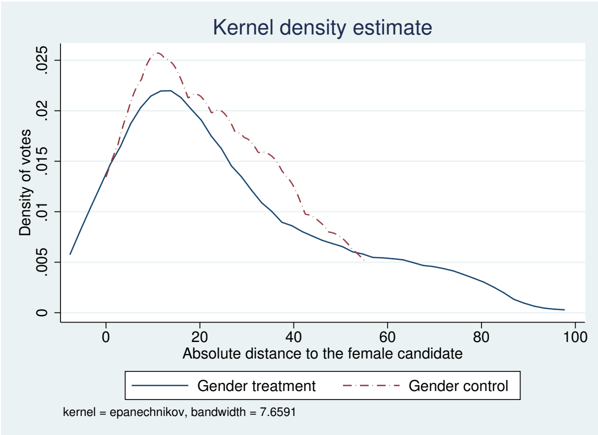
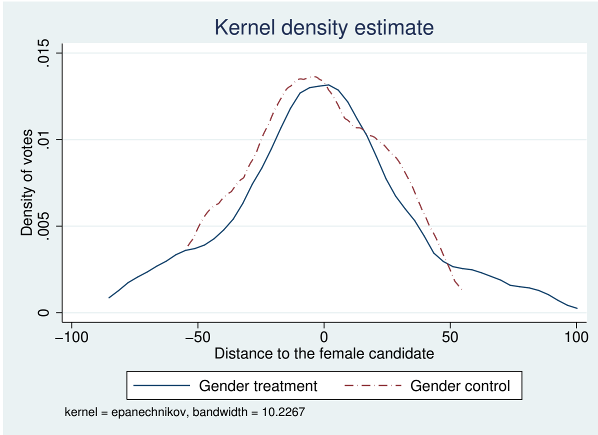


Figure A.4: Comparison of Female and Male Vote Distributions over Gender Treatment and Control Groups (with the exclusion of candidates)

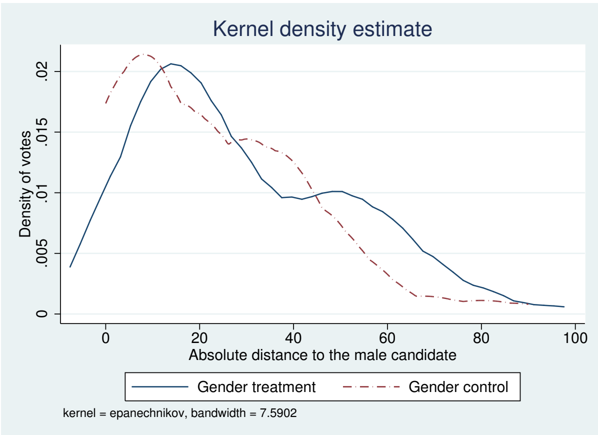
(a) Absolute distance- Votes for Female candidate



(b) Distance- Votes for Female candidate



(c) Absolute distance- Votes for Male candidate



(d) Distance- Votes for Male candidate

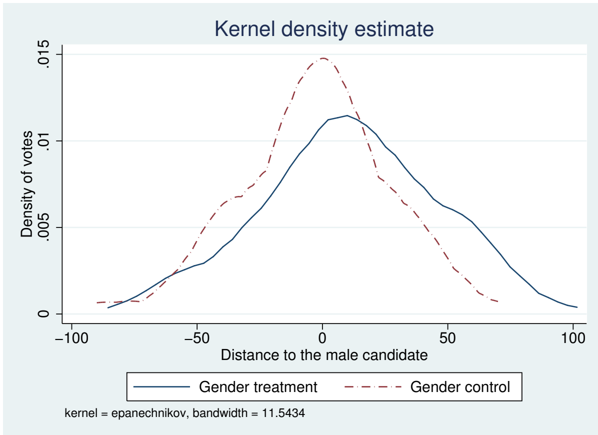


Table A.1: Voting decision with respect to Historical Proximity of the candidates both in main and extension experiments (with the exclusion of candidates)

	Closer Candidate											
	Main experiment									Extension experiment		
	Overall			Treatment			Control			Treatment only		
Vote	Male	Female	Equal	Male	Female	Equal	Male	Female	Equal	Male	Female	Equal
Male Candidate	145	59	8	84	29	6	61	30	2	121	56	19
	(72.86)	(29.65)	(36.36)	(70.59)	(28.43)	(31.58)	(76.25)	(30.93)	(66.67)	(77.07)	(26.79)	(55.88)
Female Candidate	54	140	14	35	73	13	19	67	1	36	153	15
	(27.14)	(70.35)	(63.64)	(29.41)	(71.57)	(68.42)	(23.75)	(69.07)	(33.33)	(22.93)	(73.21)	(44.12)

Number of votes for each candidate, percentages in parentheses

Table A.2: Model (1.1)- Historical proximity (with the exclusion of candidates)

Logit (robust std. errors)			
Dependent var:	(1)	(2)	(3)
Vote for Candidate B (Male candidate)	Control	Treatment	Overall
$\mathbb{1}(DD_{historical} > 0)$	0.302***	0.420***	0.313***
	(0.106)	(0.083)	(0.095)
$DD_{historical}*\mathbb{1}(DD_{historical} > 0)$	0.003	-0.004	0.003
	(0.003)	(0.002)	(0.003)
$DD_{historical}*\mathbb{1}(DD_{historical} < 0)$	0.001	0.003	0.001
	(0.003)	(0.002)	(0.002)
Gender Treatment X $\mathbb{1}(DD_{historical} > 0)$			0.093
			(0.114)
Gender Treatment X $(DD_{historical}*\mathbb{1}(DD_{historical} > 0))$			-0.006*
			(0.004)
Gender Treatment X $(DD_{historical}*\mathbb{1}(DD_{historical} < 0))$			0.002
			(0.002)
N	180	240	420
χ^2	25.184	27.431	52.590

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
GLS Regressions for the impact of distances to the voting behavior
Gender Treatment X $\mathbb{1}(DD_{historical} > 0)$, *Gender Treatment X* $(DD * \mathbb{1}(DD_{historical} > 0))$ and *Gender Treatment X* $(DD * \mathbb{1}(DD_{historical} < 0))$ are the interaction terms of variables with the gender treatment.

Table A.3: Switching behavior in Conformity Stage both in main and extension experiments (with the exclusion of candidates)

Logit (robust std. errors)	Main experiment			Extension experiment		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent var:	Switch	Switch	Switch	Switch	Switch	Switch
Gender treatment	0.013	0.013	0.009			
	(0.027)	(0.027)	(0.027)			
Minority	0.173***	0.165***	0.114***	0.059**	0.059**	0.024
	(0.026)	(0.025)	(0.043)	(0.028)	(0.028)	(0.026)
Female		0.052*	0.008		-0.003	-0.034
		(0.027)	(0.041)		(0.021)	(0.029)
Minority X Female			0.077			0.072
			(0.055)			(0.044)
Period	YES	YES	YES	YES	YES	YES
N	420	420	420	400	400	400
χ^2	42.814	44.571	48.430	13.674	13.690	14.976

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
GLS Regressions for the analysis of the switch behavior
Average marginal effects are presented in the table.

Table A.4: Comparison of Switching Behavior among Minority Voters in Conformity and Imprecision Stages (with the exclusion of candidates)

Logit (robust std. errors)	
	(1)
Dependent var:	Switch
Imprecision Treatment	0.049
	(0.075)
Period	YES
N	220
χ^2	3.328

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
GLS Regressions for the analysis of the switch behavior
Average marginal effects are presented in the table.
Imprecision Treatment = 1 if Imprecision treatment
0 if Conformity Treatment

A.2 Treatments

The other two treatments implemented in the experiment are as following:

- **Imprecision treatment**

  The only difference from the conformity treatment is that each subject receives an imprecise signal about the candidates' risk preferences. Below the investment decision information, the following message is displayed:

  "Note that the precision of the investment decision information of candidates is x%."

  The precision of the provided information is determined randomly for each subject. For example if the appointed precision level is 70% as in Figure A.10, then 7 out of 10 decisions of both candidates made in individual stage are averaged.

  The aim of this treatment is to create an information asymmetry across voters. Therefore, if participants switch their vote under this treatment, then it is both about pure taste and social learning. Comparing the results of this treatment with the conformity treatment reveals the impact of social learning.

- **Public treatment**

  Different from conformity treatment, the final votes is publicly shown with the id of the participant within the group (see Figure A.11). Note that the announced participant id is independent of their id within the experiment in order to conserve the anonymity. However, to create a public environment, I keep group id constant over 3 periods of public stage so that group members are allowed to keep track of other voters. Therefore, a more flexible anonymity principal is employed in this experiment.

  Hence, in comparison with conformity stage, the change in propensity to conform shows the impact of public announcement. Also this treatment allows me to observe a potential impact of the gender composition of group.

A.3 Instructions of Main and Extension Experiments and Survey

A.3.1 Instructions of Main Experiment

Thank you for considering participating in this economics experiment. The experiment you are going to take part is about economic decision making. Throughout the session, you are expected to make allocation decisions regarding an investment task and some additional decisions which will be explained later on and that will influence your earning. You will earn £2.5 for your participation. In addition to this participation fee, your earnings in the experiment will depend on your decisions and chance. The rules explained below are accurate and your payment will be determined in accordance with them.

Participation in this study is entirely voluntary. After the rules are explained, you are free to quit before the experiment starts. Your decisions during the experiment will not be matched with your real identity, rather they will be recorded under an anonymous subject number. Note that communication of any kind is prohibited during the experiment. Please make sure your mobile phones are on silent and away.

The main experiment consists of five periods. Within each period, there are several rounds: ten rounds in period 1 and three rounds in each subsequent period (22 rounds in total). There will be further explanation at the beginning of each period. At the end of the experiment, one out of 22 rounds will be randomly selected and the payoff in that round will determine your earnings. There will also be a final additional period in which your earning will be counted as a bonus. Payments will be made privately in cash at the end of the experiment.

During the experiment, all amounts will be expressed in ECU (“experimental currency unit"): 1 ECU equals £0.04 (i.e. 4p).

Different rules will apply in each of the five periods. Just before each period, the rules of the corresponding period will be explained in detail.

The decisions you need to make will now be explained. Once you understand the general task, the instructions for each of the five periods will be explained period by period.

General Rules

Imagine that you are an investment manager in a financial firm. In every financial period, you must decide how to split the assets of the firm between two investment options. Your earning depends on your allocation decision over these two options. At the beginning of each round, you start with 100 ECU. You need to choose the share X_1 to allocate to option 1 and the share of X_2 to allocate to option 2:

- Option 1: The riskless option, e.g. investing in a bond, which secures the allocated amount with probability 1. So if you invest X_1, you get X_1 at the end of that round.

- Option 2: The risky option, e.g. investing in a stock, that yields $k * X_2$ with probability $\frac{1}{2}$ and 0 otherwise. Here k is the multiplier for risky option which will be chosen randomly from [1.5,3] at the beginning of each round. So in other words, if the stock performs well, you get your allocated money as multiplied whereas you lose your money when the stock performs badly.

You will choose how much to allocate to Option 2, which can be any amount between nothing (0) and all of your assets (100). Whatever you do not allocate to Option 2 is assigned to Option 1.

Example: k=2.00. Let's consider you chose X_2=60 ECU. This means you put 40 ECU to riskless option. Your earnings will be formed as:

Riskless option:	You will receive 40 token for sure,
Risky option:	You will receive 120 token (60*2.00), if the risky option is successful (50% of chance)
	or 0 token (you might lose your 60 token), if the risky option is failed (50% of chance)

So the total return of your investment will be either 40 token or 160 token.

Stage 1(period 1)

This stage involves 10 rounds of performing the investment task for randomly chosen k values. Your screen will be like in Figure A.5.

[Screen-shot of investment task is attached at the back of the information form.]

In the last round (round 10), you will make the investment decision for a list of k values rather than a single one. The k value will be randomly chosen from these specific values and your corresponding choice will be evaluated as in the previous 9 rounds. Figure A.6 shows the screen you will see for round 10. Before you start performing the investment tasks, you need to complete the following form on your screen.

[Participants fill the form that acquire information about gender and age.]

Do you have any question?

Stage 2

[The instructions in this stage (in each period) is shown on the screen of the participants and read outloud by the researcher.]

General Instructions

In this stage, you will play each period within a group. Note that each period has three rounds. At the beginning of each round, you will be randomly allocated to a group of 7 people. Your payoff will depend on the decision made by a group leader who will be elected by your group. Note that the rounds will be independent and the groups will be newly formed at the beginning of each round.

Period 2

The computer will choose two candidates from your group, referred to as Candidate A and B for confidentiality reasons. You will observe a randomly chosen round of investment decisions made by both candidates in the previous stage. With the given information, you will vote for the candidate you prefer as the group leader. Figure A.7(a) and A.7(b) [Figure A.7(c) and A.7(d)] are the examples of the screenshots you will see.

[Note that the stated figures in the instructions are for gender treatment sessions, whereas the figures in brackets are for gender control sessions.]

After the voting process, the candidate who receives the most votes will be the leader of the group and will perform the investment task (again with a randomly chosen k) on behalf of the group.

The payoff of all group members will be computed based on the decision of the elected leader.

Note that candidates will be allowed to vote as well.

While the leader makes the investment decision for the group, the rest of the group members(members who are not candidate) are expected to guess how much the candidates allocated to risky option under specific k-values in round 10(see Figure A.8(a) [Figure A.8(b)]).

One of the k-values will be randomly chosen. If your guess for that k-value is within +3 and -3 interval of the candidate's actual allocation, your guess will be correct. For each correct guess (for each candidate), you will earn a 5 ECU bonus.

Let's consider candidate allocated 60 for the chosen k. If your guess is in the interval [57,63], your guess will be correct.

At the end of the experiment, your total bonus in the chosen round will be added to your payoff from that round.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Period 3

In this period, there will be a poll before the actual voting. The poll results will be shown to all voters in terms of vote shares, then each voter will cast an actual vote. (see Figure A.9(a) [Figure A.9(b)])

Note that your vote in the poll and actual voting are equally important since one of them will be randomly chosen and considered as your final vote.

As in the previous rounds, the elected leader will make the investment decision on behalf of the group. You will then follow the same procedure as in period 2.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Period 4

In this period, the process will be the same as in period 3. However, unlike in the previous periods, you will observe the average X2 decisions of both candidates with a certain precision level (see Figure A.10(a) [Figure A.10(b)]).

In this example, the voter receives "70% precise information" which means that randomly 7 out of 10 decisions of both candidates are averaged.

This precision level will be randomly chosen for each voter, and will vary within group. Therefore some voters will receive more precise information than others.

You will then follow the same procedure as in period 3.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Period 5

In this period, the process will be the same as in period 3. However, rather than revealing vote shares after the poll, each vote will be observed separately in a list presenting all votes of group members. Also the information of the candidates will include only one random round as in the period 3. Therefore, there won't be any variation on the precision levels across group members(see Figure A.11(a) [Figure A.11(b)).

You will appointed with a voter ID which is different than your participant ID.

Your voter ID will not change over the three periods so that each voter's decision will be traceable by others. If you want, you can take notes on the paper provided on your desk.

You will then follow the same procedure as in period 3.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Additional Stage

Now you have additional series of investment decisions from which your earnings will be counted as a bonus to your overall earnings. You will be asked to make ten decisions in which each decision is a paired choice between Gamble A and Gamble B.

Consider we have a ten sided die with each face numbered from 1 to 10. After you have made all of your choices, the computer will generate the outcomes of two rolls of the die, first to select one of the ten decisions to be used, and second to determine what your payoff is for the option you chose, A or B, for the particular decision selected.

Even though you make ten decisions, only one of them will be selected. Therefore each decision has an equal chance to affect your final payoff.

Let's look at the first decision case in the Figure A.12. Here in Gamble A you have 1 out of 10 chances to win 45 ECU and 9 out of 10 chances to win 30 ECU. If the outcome of the second roll is 1, then you will earn 45 ECU. If the outcome is between 2 and 10, you will earn 30 ECU.

Please choose the gamble you preferred for each decision case. If the options appear as selected, please change them according to your preferences.

[Additional stage starts]

Survey

The experiment is over. There is a short survey. After you finish it, please remain seated. Your payment in an envelope will be brought to your workspace.

Thank you for your participation.

Figure A.5: Investment decision

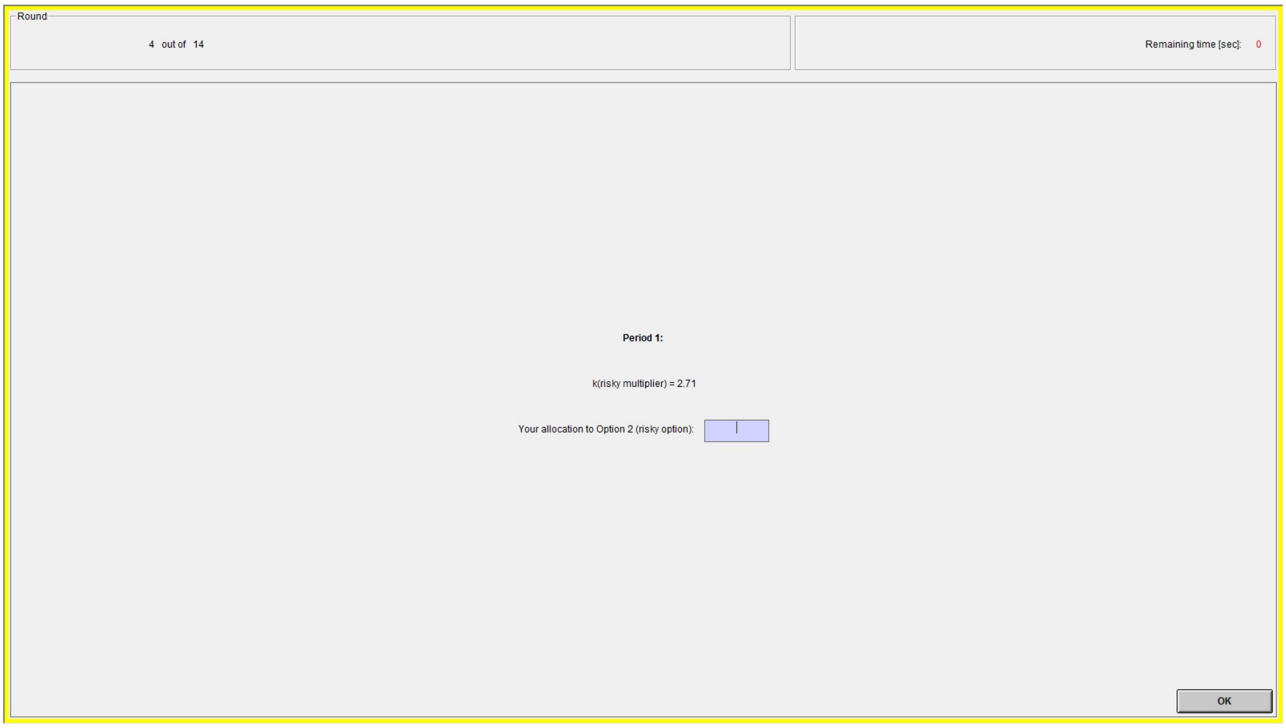


Figure A.6: Investment decision at round 10

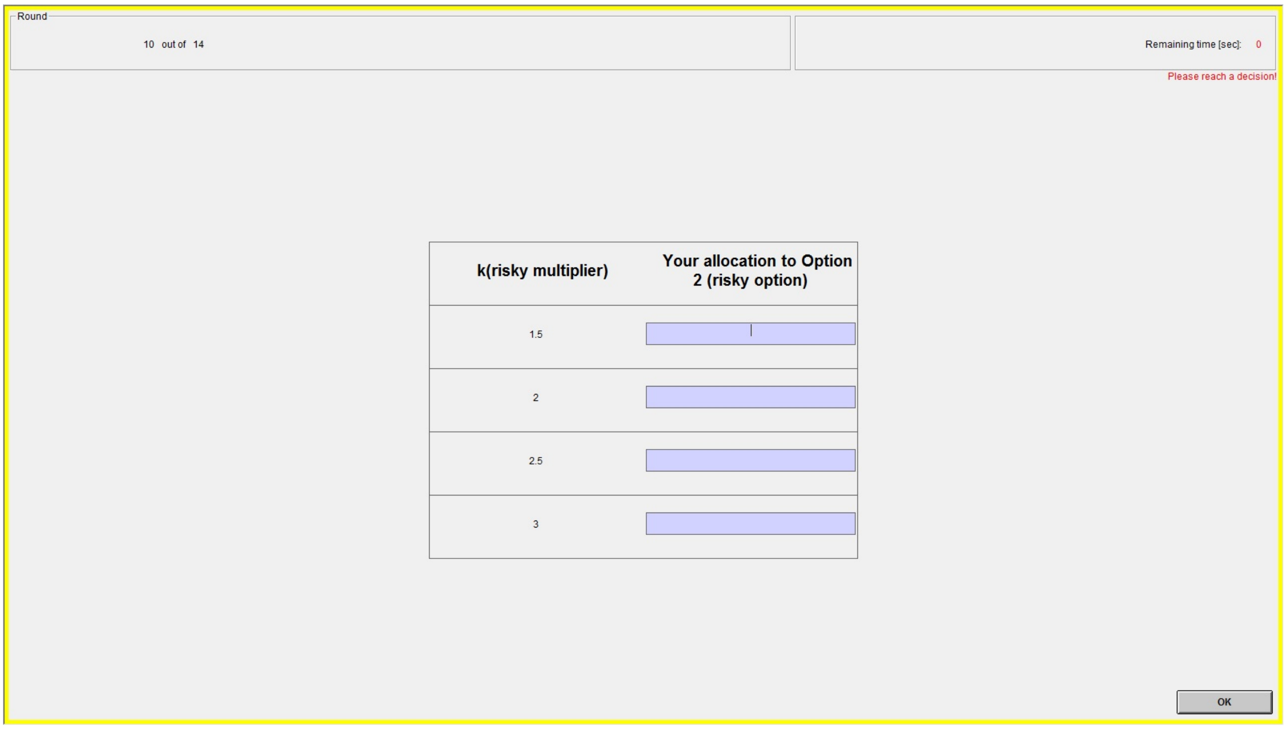


Figure A.7: Voting screen

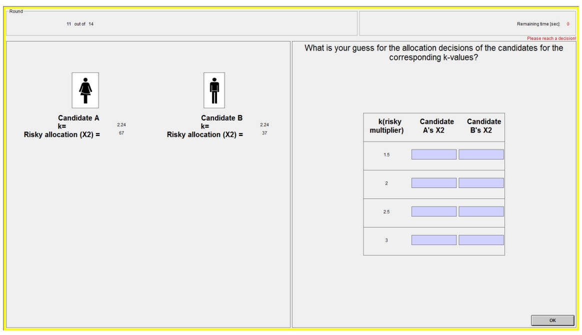
(a) In gender treatment group- Non-candidate

(b) In gender treatment group- Candidate

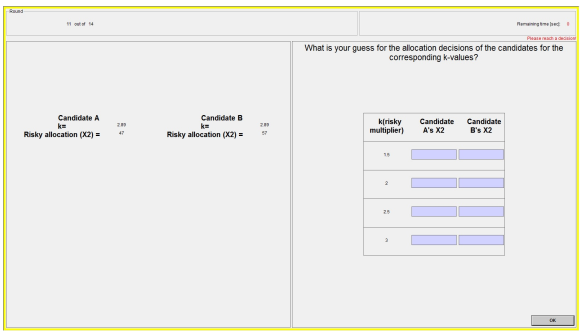
(c) In gender control group- Non-candidate

(d) In gender control group- Candidate

Figure A.8: Guessing screen

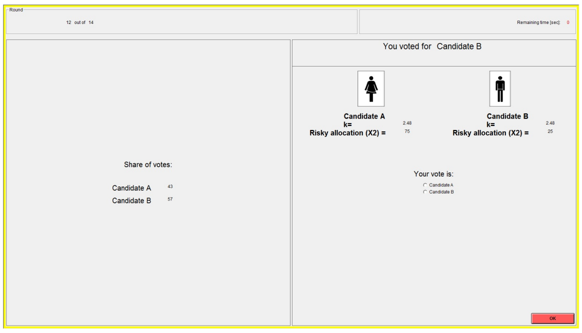


(a) In gender treatment group

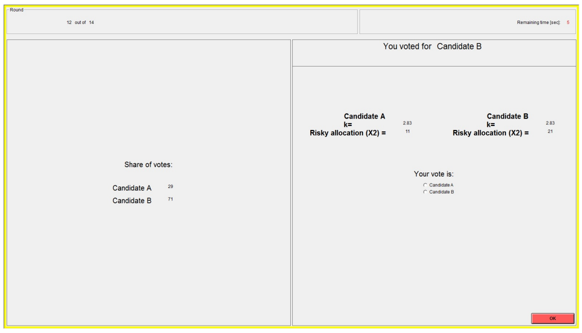


(b) In gender control group

Figure A.9: Poll results

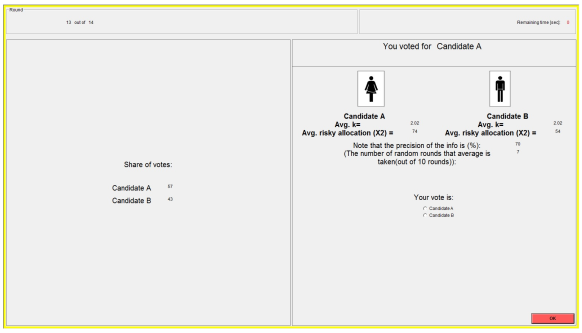


(a) In gender treatment group

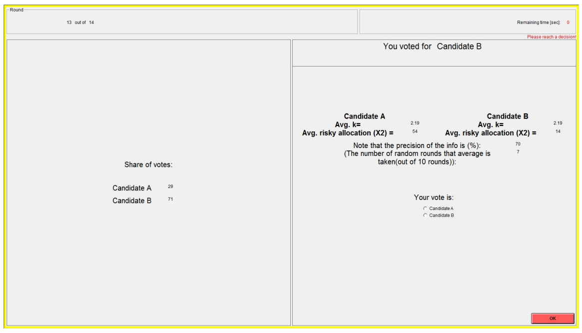


(b) In gender control group

Figure A.10: Imprecision stage screen

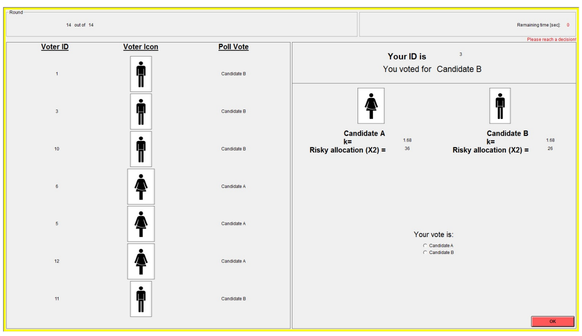


(a) In gender treatment group

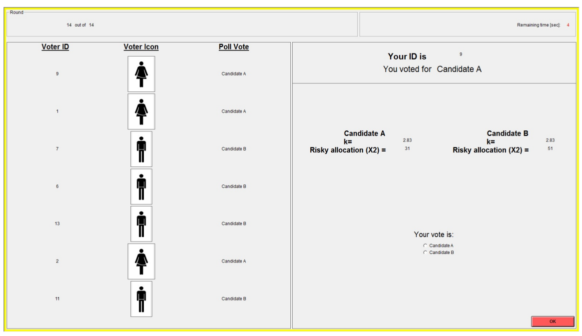


(b) In gender control group

Figure A.11: Public stage screen

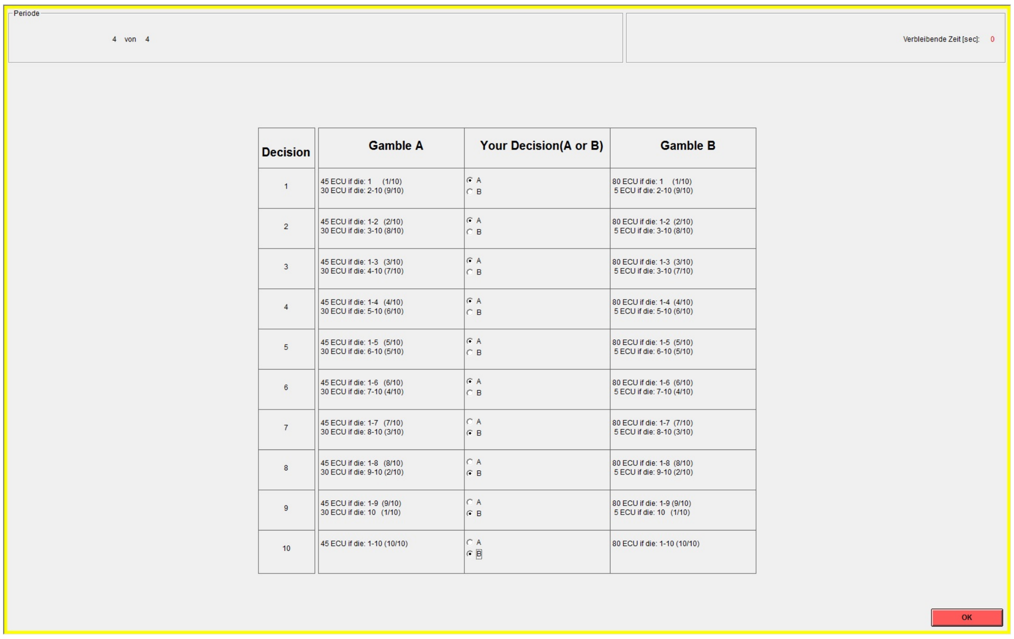


(a) In gender treatment group



(b) In gender control group

Figure A.12: Holt-Laury task



Decision	Gamble A	Your Decision(A or B)	Gamble B
1	45 ECU if die: 1 (1/10) 30 ECU if die: 2-10 (9/10)	A B	80 ECU if die: 1 (1/10) 5 ECU if die: 2-10 (9/10)
2	45 ECU if die: 1-2 (2/10) 30 ECU if die: 3-10 (8/10)	A B	80 ECU if die: 1-2 (2/10) 5 ECU if die: 3-10 (8/10)
3	45 ECU if die: 1-3 (3/10) 30 ECU if die: 4-10 (7/10)	A B	80 ECU if die: 1-3 (3/10) 5 ECU if die: 3-10 (7/10)
4	45 ECU if die: 1-4 (4/10) 30 ECU if die: 5-10 (6/10)	A B	80 ECU if die: 1-4 (4/10) 5 ECU if die: 5-10 (6/10)
5	45 ECU if die: 1-5 (5/10) 30 ECU if die: 6-10 (5/10)	A B	80 ECU if die: 1-5 (5/10) 5 ECU if die: 6-10 (5/10)
6	45 ECU if die: 1-6 (6/10) 30 ECU if die: 7-10 (4/10)	A B	80 ECU if die: 1-6 (6/10) 5 ECU if die: 7-10 (4/10)
7	45 ECU if die: 1-7 (7/10) 30 ECU if die: 8-10 (3/10)	A B	80 ECU if die: 1-7 (7/10) 5 ECU if die: 8-10 (3/10)
8	45 ECU if die: 1-8 (8/10) 30 ECU if die: 9-10 (2/10)	A B	80 ECU if die: 1-8 (8/10) 5 ECU if die: 9-10 (2/10)
9	45 ECU if die: 1-9 (9/10) 30 ECU if die: 10 (1/10)	A B	80 ECU if die: 1-9 (9/10) 5 ECU if die: 10 (1/10)
10	45 ECU if die: 1-10 (10/10)	A B	80 ECU if die: 1-10 (10/10)

A.3.2 Instructions of Extension Experiment

[Note that the instructions of the extension experiment and main experiment are same until stage 2 .]

Stage 2

[The instructions in this stage (in each period) is shown on the screen of the participants and read outloud by the researcher.]

General Instructions

In this stage, you will play each period within a group. Note that each period has four rounds. At the beginning of each round, you will be randomly allocated to a group of 7 people. Your payoff will depend on the decision made by a group leader who will be elected by your group. Note that the rounds will be independent and the groups will be newly formed at the beginning of each round.

Period 2

The computer will choose two candidates from your group, referred to as Candidate A and B for confidentiality reasons. You will observe a randomly chosen round of investment decisions made by both candidates in the previous stage. With the given information, you will vote for the candidate you prefer as the group leader. Figure A.7(a) and A.7(b) are the examples of the screenshots you will see.

After the voting process, a voter from the group will randomly be picked as “decisive voter” by computer. The candidate who received vote of the decisive voter will be leader of the group. Note that everyone in the group has equal chance to be picked as decisive voter. The elected leader will perform the investment task on behalf of the group. The payoff of all group members will be computed based on the decision of the elected leader.

While the leader makes the investment decision for the group, the non-candidate group members are expected to guess how much the candidates allocated to risky option under specific k-values in round 10(see Figure A.8).

One of the k-values will be randomly chosen. If your guess for that k-value is within +3 and -3 interval of the candidate’s actual allocation, your guess will be correct. For each correct guess (for each candidate), you will earn a 5 ECU bonus.

Let’s consider candidate allocated 60 for the chosen k. If your guess is in the interval [57,63], your guess will be correct.

At the end of the experiment, your total bonus in the chosen round will be added to your payoff from that round.

If you have any questions please raise your hand. If you don’t, you can click ‘Continue’.

[Period starts.]

Period 3

In this period, there will be a poll before the actual voting. The poll results will be shown to all voters in terms of vote shares, then each voter will vote again. (see Figure A.9(a))

Note that your vote in the poll and actual voting are equally important since one of them will be randomly chosen and considered as your final vote. The final vote of the decisive voter will determine the leader.

You will then follow the same procedure as in previous period.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Period 4

In this period, the process will be the same as in previous period. However, unlike in the previous periods, you will observe several rounds of investment information rather than a single round. The number of rounds each group member observes will be randomly chosen for each voter, and will vary within group. As an example, consider the appointed number of rounds you are allowed to observe is 4 (out of 10) as in Figure A.13. You will see the investment decisions of both candidates in random 4 rounds of the first stage. Meanwhile, another member/voter in your group will observe 8 rounds as in Figure A.14. Therefore some voters will observe more rounds than others. You will then follow the same procedure as in previous periods.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Survey

The experiment is over. There is a short survey. After you finish it, please remain seated. Your payment in an envelope will be brought to your workspace.

Thank you for your participation.

Figure A.13: Voting screen in Period 4

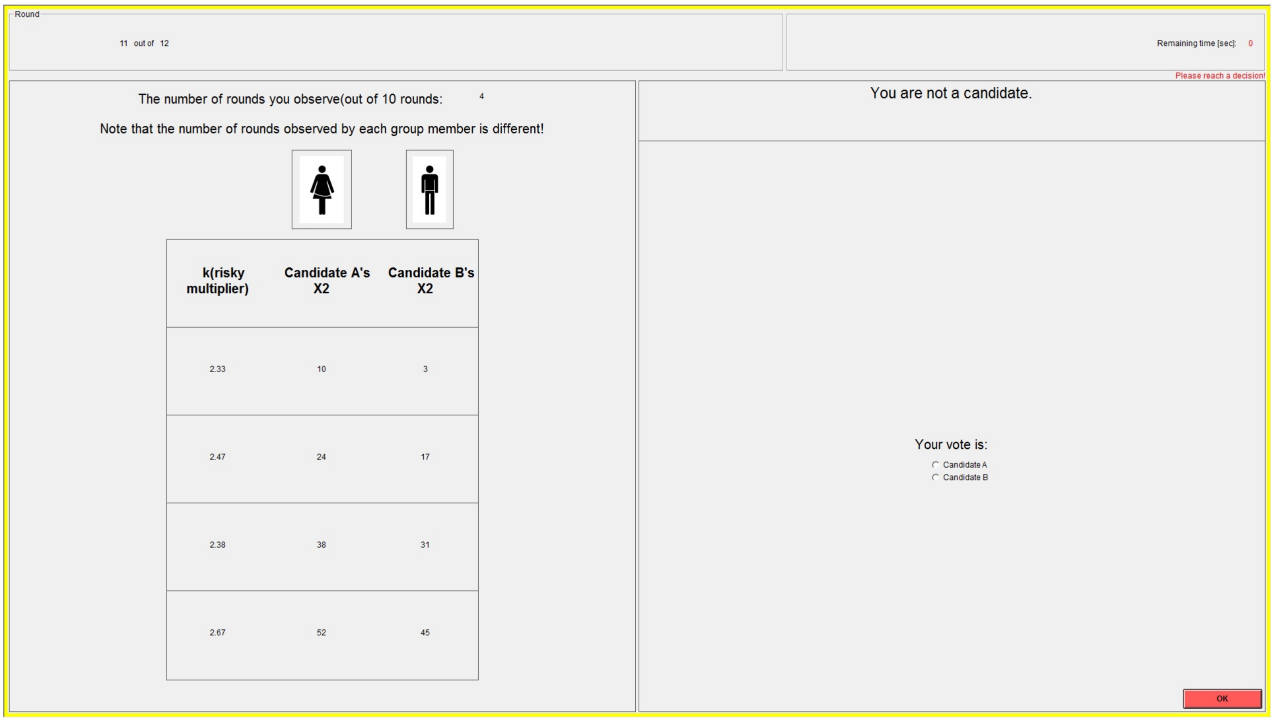
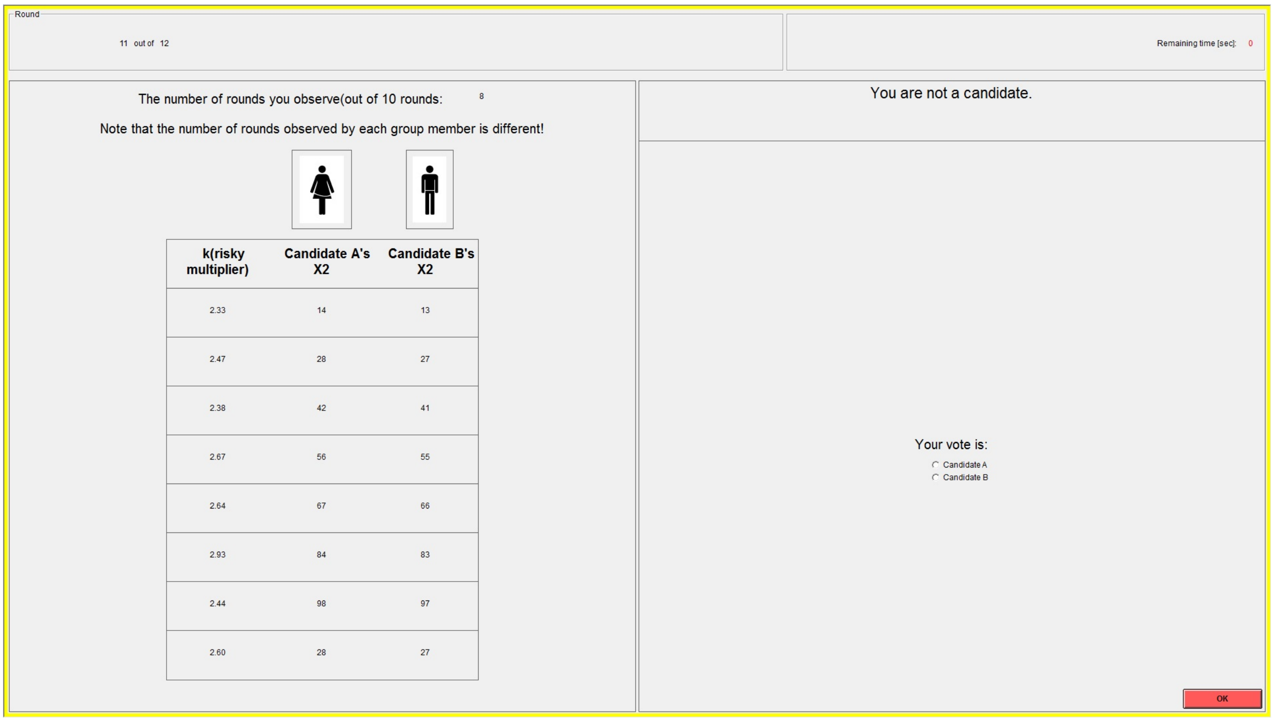


Figure A.14: Voting screen of another voter in Period 4



A.3.3 Post-experiment Survey

1. Are you a student or a staff?

2. Please identify your department or school.

3. Please indicate your year.

4. What is your ethnic group?

5. Please identify your annual personal income. (If you are not working, please specify the amount of your budget.)

6. How many brothers and sisters do you have?

7. How many of your siblings are female?

8. Where were you born in relation to your sibling(s)? Click on the button that best descibes your birth order.

9. Please rate the difficulty of the experiment from 1 to 5 (1: easy- 5:hard).

Appendix B

Appendix: Chapter 2

B.1 Tables and Figures

Table B.1: Beliefs among Each Type of Leaders

		NO INFO		*INFO*	
Type	*Belief of Leader*	k_{low}	k_{high}	k_{low}	k_{high}
Selfish	Prob(same risk behavior)	0.207***	0.268***	0.153***	0.082***
	Prob(more risk-averse)	0.415***	0.329***	0.408***	0.337***
	Prob(more risk-seeking)	0.378***	0.402***	0.439***	0.582***
Other-regarding	Prob(same risk behavior)	0.278***	0.222***	0.077**	0.102***
	Prob(more risk-averse)	0.355***	0.300***	0.474***	0.333***
	Prob(more risk-seeking)	0.367***	0.478***	0.449***	0.564***
Selfish/Other-regarding	Prob(same risk behavior)	0.150*	0.100	0.167	0.333**
	Prob(more risk-averse)	0.600***	0.250**	0.417***	0.333**
	Prob(more risk-seeking)	0.250**	0.650***	0.417***	0.333**

Table B.2: Model (2.1) for Other-regarding and Selfish Leaders

Dependent var:	Other-regarding leaders			Selfish leaders		
$\Delta(group, individual)$	Overall	k_{low}	k_{high}	Overall	k_{low}	k_{high}
$\mathbb{1}(Prra \geq 0)$	3.278	4.550	-1.999	-0.291	-2.312	1.130
	(2.855)	(3.198)	(4.727)	(2.767)	(4.372)	(3.081)
$Prra\text{X}\mathbb{1}(Prra \geq 0)$	-0.340***	-0.070	-0.331*	-0.342***	-0.414**	-0.229*
	(0.120)	(0.174)	(0.179)	(0.117)	(0.175)	(0.136)
$Prra\text{X}\mathbb{1}(Prra < 0)$	0.804***	0.865***	0.607**	0.136	0.171	0.134
	(0.166)	(0.199)	(0.298)	(0.084)	(0.115)	(0.105)
$INFO\text{X}(\mathbb{1}(Prra \geq 0))$	3.892	3.278	7.875*	-1.465	1.023	-0.597
	(2.862)	(3.405)	(4.226)	(2.976)	(4.671)	(3.240)
$INFO\text{X}(Prra\text{X}\mathbb{1}(Prra \geq 0))$	-0.392***	-0.761***	-0.344*	0.197	0.120	0.151
	(0.133)	(0.181)	(0.197)	(0.151)	(0.241)	(0.164)
$INFO\text{X}(Prra\text{X}\mathbb{1}(Prra < 0))$	-0.075	-0.255*	0.198	0.394***	0.248**	0.528***
	(0.152)	(0.155)	(0.279)	(0.090)	(0.122)	(0.116)
Constant	2.025	-1.248	5.778	7.945	8.404	6.176
	(4.794)	(5.066)	(6.996)	(5.644)	(7.214)	(6.725)
Session	YES	YES	YES	YES	YES	YES
N	336	168	168	360	180	180
χ^2	230.843	177.737	105.396	101.676	47.569	63.124

Standard errors in parentheses
GLS Regressions for the impact of group period on riskiness
* $p < .1$, ** $p < .05$, *** $p < .01$

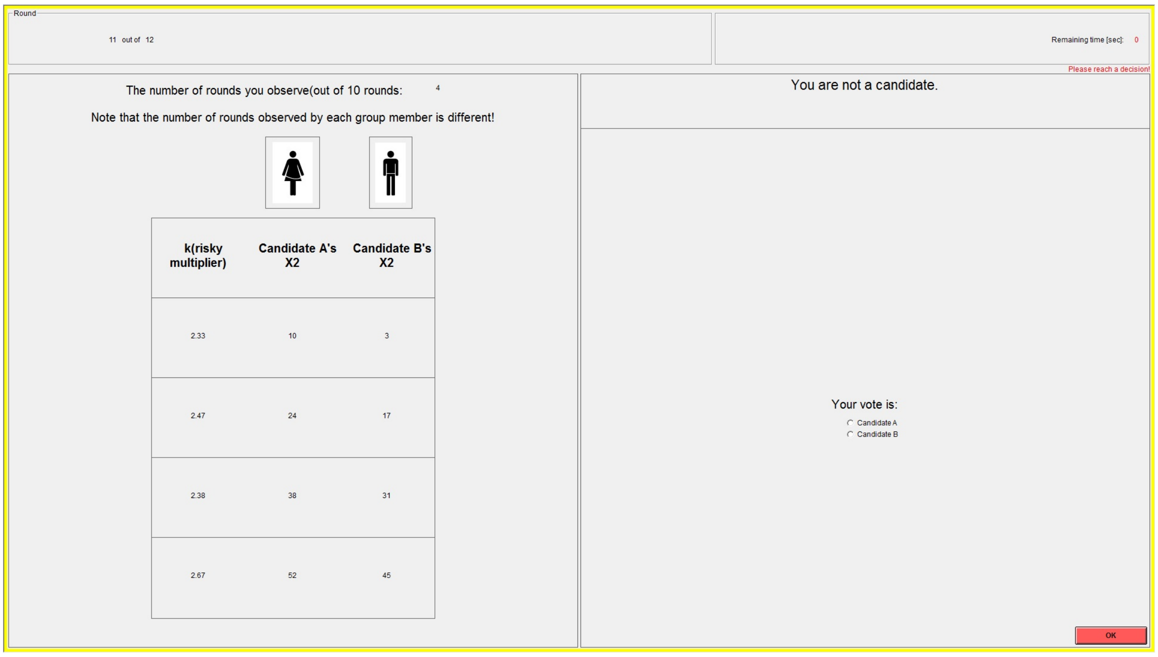
B.2 Treatments

- **Conformity treatment:** Differently than from baseline, there is a poll before actual voting. After the poll shares revealed to everyone, subjects are allowed to revise their votes. Note that the voting decision of the individual is randomly chosen from either the poll or the actual voting decisions in order to incentivize the decisions in poll stage.

The new version of the other treatment:

- **Imprecision treatment:** The problem in this treatment seems the failure to create an "imprecise" environment. In the previous design, the amount of information is averaged according to the chosen precision level for each voter. The idea was creating an information asymmetry across voters in order to observe whether they regard the poll results as a signal about the candidate's risk behavior. Therefore to introduce information asymmetry, the amount of information provided will vary across voters. Rather than averaging as in the old design, I plan to provide the information as a list. For example, consider the precision level is appointed as 40% for the voter. The voter will receive the risky allocation information of both candidates made in 4 previous rounds, as presented in Figure B.1 [1].

Figure B.1: The screenshot of Imprecision treatment



B.3 Instructions, Quiz and Survey

B.3.1 Instructions

The experiment you are going to take part is about economic decision making. Throughout the session, you are expected to make allocation decisions regarding an investment task and some additional decisions which will be explained later on and that will influence your earning. You will earn £2.50 for your participation. In addition to this participation fee, your earnings in the experiment will depend

[1] Note that there are 10 rounds in individual stage.

on your decisions and chance. The rules explained below are accurate and your payment will be determined accordance with them.

Participation in this study is entirely voluntary. After the rules are explained, you are free to quit before the experiment starts. Your decisions during the experiment will not be matched with your real identity, rather they will be recorded under an anonymous subject number. Note that communication of any kind is prohibited during the experiment. Please make sure your mobile phones are on silent and away.

The main experiment consists of five periods. Within each period, there are several rounds: ten rounds in period 1 and four rounds in each subsequent period (26 rounds in total). There will be further explanation at the beginning of each period. At the end of the experiment, one out of 26 rounds will randomly be selected and the payoff in that round will determine your earnings. Payments will be made privately in cash at the end of the experiment.

During the experiment, all monetary activities will be done with tokens where 1 token equals to £0.04 (i.e. 4p).

Different rules will apply in each of the five periods. Just before each period, the rules of the corresponding period will be explained in detail.

The decision you need to make will now be explained. Once you understand the general task, the instructions for each of 5 periods will be explained period by period.

General Rules

Imagine that you are an investment manager in a financial firm. In every financial period, you must decide how to split the assets of the firm between two investment options. Your earning depends on your allocation decision over these two options. At the beginning of each round, you start with 100 tokens. You need to choose the share X_1 to allocate to option 1 and the share of X_2 to allocate to option 2:

- Option 1: The riskless option, e.g. investing in a bond, which secures the allocated amount with probability 1. So if you invest X_1, you get X_1 at the end of that round.

- Option 2: The risky option, e.g. investing in a stock, that yields $k * X_2$ with probability $\frac{1}{2}$ and 0 otherwise. Here k is the multiplier for risky option which will be chosen randomly from [1.5,3] at the beginning of each round. So in other words, if the stock performs well, you get your allocated money as multiplied whereas you lose your money when the stock performs badly.

You will be asked how much you want to allocate to Option 2 which can be any amount between nothing (0) and all of your assets (100). Whatever you do not allocate to Option 2 is assigned to Option 1.

Example: k=2.00. Let's consider you chose X_2= 60 token. This means you put 40 token to riskless option. Your earnings will be formed as:

Riskless option:	You will receive 40 token for sure,
Risky option:	You will receive 120 token (60*2.00), if the risky option is successful (50% of chance)
	or 0 token (you might lose your 60 token), if the risky option is failed (50% of chance)

So the total return of your investment will be either 40 token or 160 token.

Stage 1(period 1)

This stage involves 10 rounds of performing the investment task for randomly chosen k values. Your screen will be like in Figure B.2.

[Screen-shot of investment task will be attached at the back of information form.]

In the last round (round 10), you will make investment decisions for a list of k values rather than a single one. Each investment in this list is independent of each other: For each k-value, you have 100 tokens initially. Your investment for round 10 will be randomly chosen from those four investment decisions on the list.

Remember that every decision has an equal chance of being selected for payment. Figure B.3 shows the screen you will see for round 10.

Now there is a short quiz to test your understanding of the task before we start the experiment. After each question, you will observe the explained solution of it. So if you are hesitant about the answer, please read the solution carefully. Note that your answers in the quiz will not affect your earnings from the experiment. Rather, it is aimed to improve your understanding of the task. It is important for you to understand the task since your decision on it will affect your earnings. Therefore, please raise your hand if anything is unclear in the quiz.

[Quiz starts. For more details about the quiz, please see Appendix 2.]

If you don't have any question about the instructions explained until now, we start the experiment. Please fill the form correctly in the first screen appeared and then continue to the experiment.

[Participants fill the form that acquire information about gender and age.]

Stage 2

[The instructions in this stage (in each period) is shown on the screen of the participants and read outloud by the researcher.]

General Instructions

In this stage, you will play each period within a group. Note that each period has three rounds. At the beginning of each round, you will be randomly appointed to a group of 7 people. Your payoff will depend on the decision made by a group leader who will be elected by your group. Note that the rounds will be independent and the groups will be newly formed at the beginning of each round.

Period 2

The computer will choose two candidates from your group, referred to as Candidate A and B for confidentiality reasons. The non-candidates will observe a randomly chosen round of investment decisions made by both candidates in the previous stage with an icon that represents candidate's gender. The candidates, on the other hand, will observe a screen that only state their status and which candidate they are. With the given information, you will vote for the candidate you prefer as the group leader. In Figure B.4 and Figure B.5, you can see examples of non-candidate and candidate screens.

[Screen-shot of voting process is shown on the screens of each subject.]

After the voting process, the candidate who receives the most votes will be the leader of the group and will perform the investment task on behalf of the group.

The payoff of all group members will be computed based on the decision of the elected leader (see Figure B.6 for the leader's investment screen).

After the leader makes the investment decision for the group, all group members are expected to guess how much the candidates allocated to risky option under specific k-values in round 10. Candidates (both leader and non-leader) are also expected to guess the other candidate's investment decisions (see Figure B.7 and Figure B.8 for the non-candidate and candidate guess screens).

One of the k-values will be randomly chosen. If your guess for that k-value is within +3 and -3 interval of the candidate's actual allocation, your guess will be correct. For each correct guess (for each candidate), you will earn a 5 token bonus.

Let's consider candidate allocated 60 token to risky option for the chosen k. If your guess is in the interval [57,63], it will be correct.

At the end of the experiment, your total bonus in the chosen round will be added to your payoff from that round.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Period 3

In this period, the process will be same as in the previous period. However, candidates will also observe the information about both of the candidates. In Figure B.4 and Figure B.9 , you can see examples of non-candidate and candidate screens. As in previous period (period 2), the elected leader will make the investment decision for the group. You are also expected to guess candidates' risky allocations in round 10 of the first stage (see Figure B.7 and Figure B.10 for the examples of non-candidate and candidate guess screens). The same rules will apply on the evaluation of guesses and the distribution of the bonus for each correct guess.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Period 4

In this period, there will be a poll before the actual voting. The poll results will be shown to all voters in terms of vote shares, then each voter will cast an actual vote. (see Figure B.11)

Note that your vote in the poll and actual voting are equally important since one of them will be randomly chosen and considered as your final vote.

You will then follow the same procedure as in previous period.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Period 5

In this period, the process will be the same as in period 4. However, unlike in the previous periods, you will observe several rounds of investment information rather than a single. The number of rounds each group member observes will be randomly chosen for each voter, and will vary within group. As an example, consider the appointed number of rounds you are allowed to observe is 4 (out of 10) as in Figure B.12. You will see the investment decisions of both candidates in random 4 rounds of the first stage. Meanwhile, another member/voter in your group will observe 8 rounds as in Figure B.13. Therefore some voters will observe more rounds than others. You will then follow the same procedure as in previous periods.

If you have any questions please raise your hand. If you don't, you can click 'Continue'.

[Period starts.]

Figure B.2: Investment decision

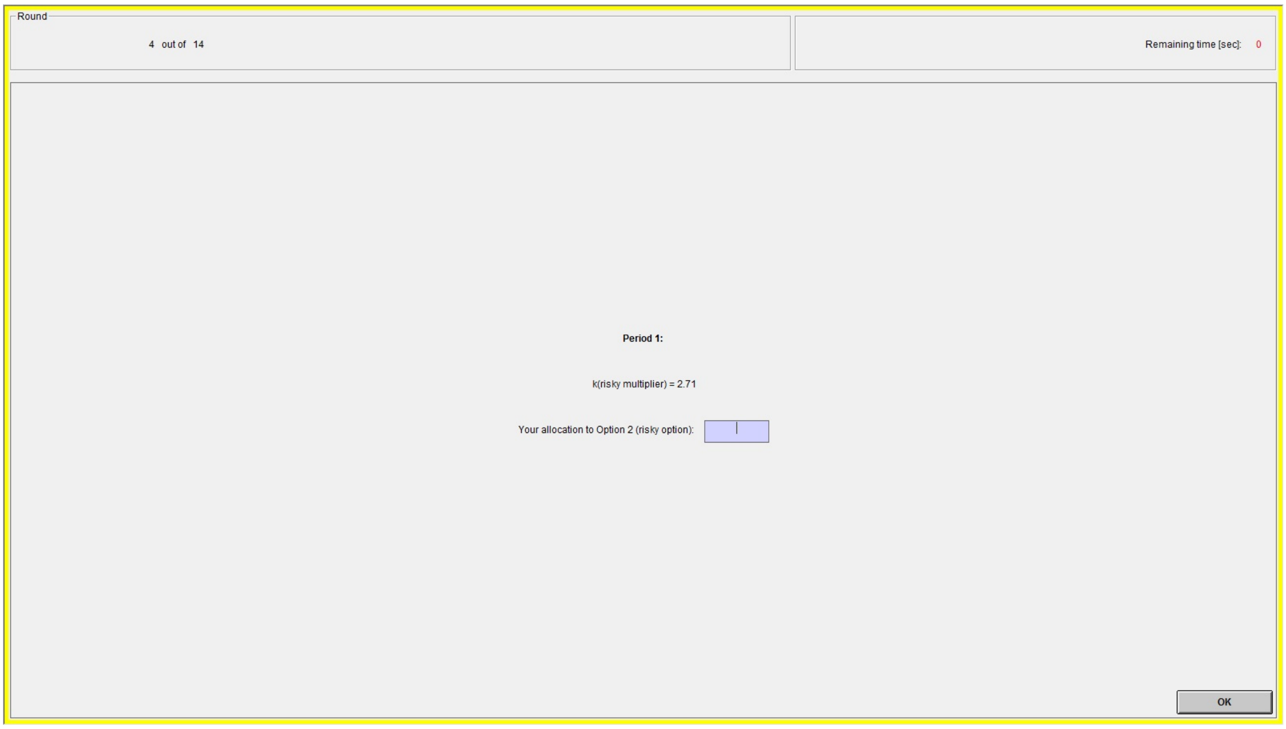


Figure B.3: Investment decision at round 10

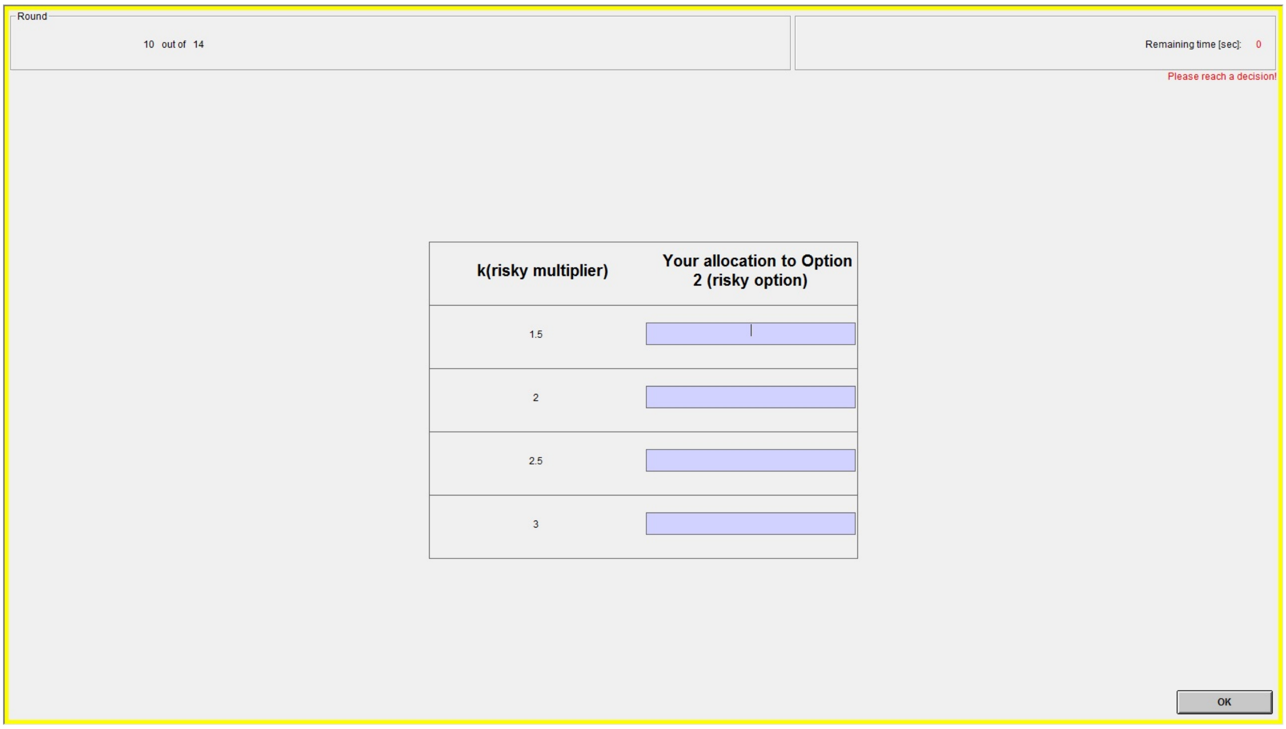


Figure B.4: Voting screen of the non-candidate both in NO INFO and INFO treatments

Figure B.5: Voting screen of the candidate in NO INFO treatment

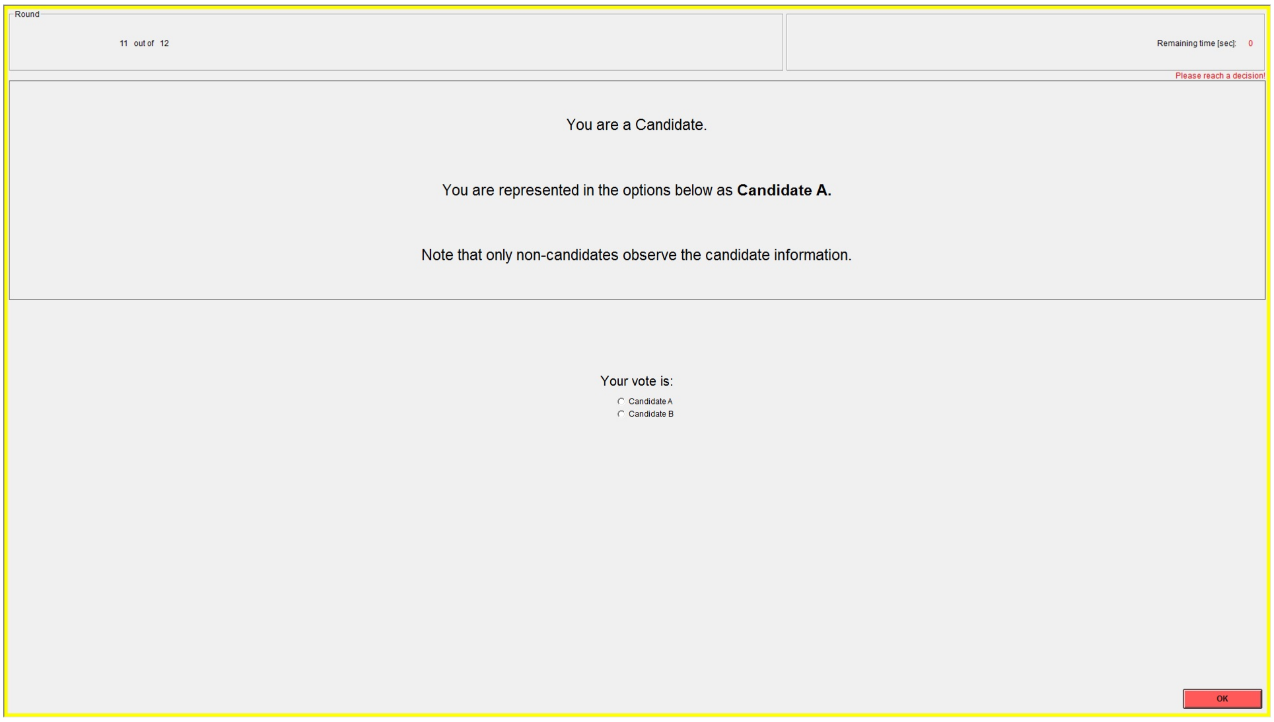


Figure B.6: Investment screen of the leader

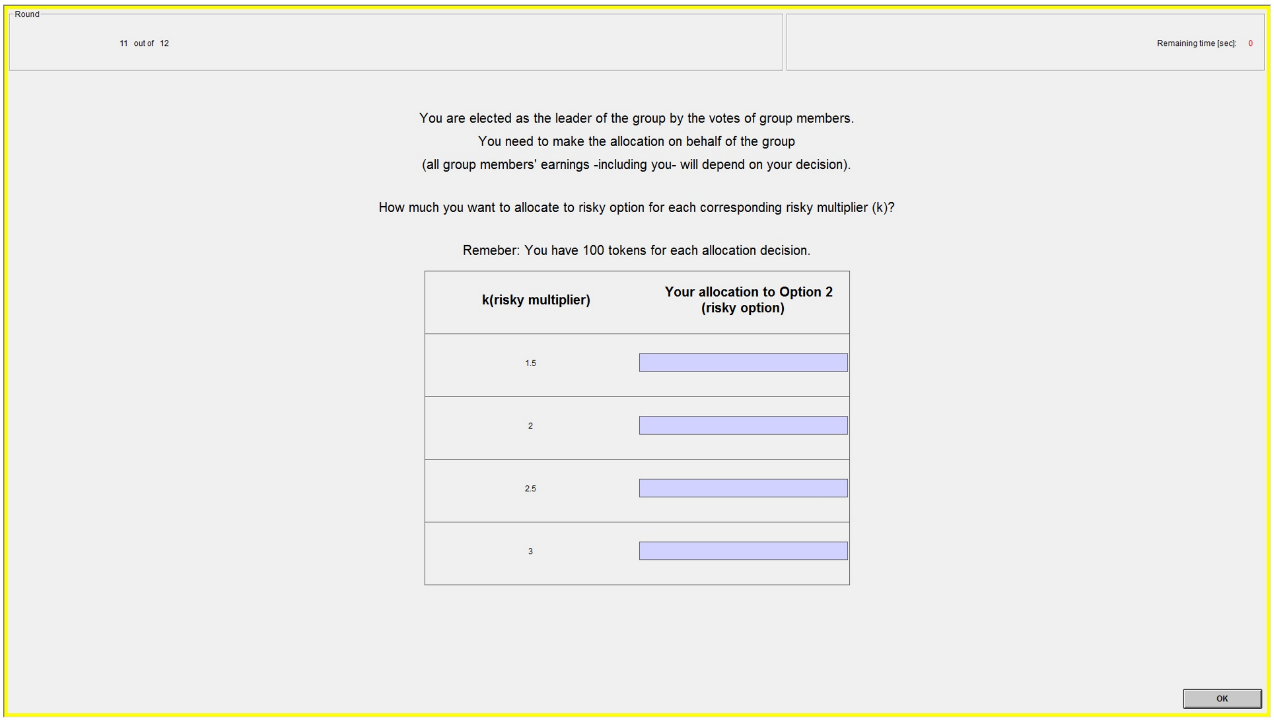


Figure B.7: Guess screen of non-candidates both in NO INFO and INFO treatments

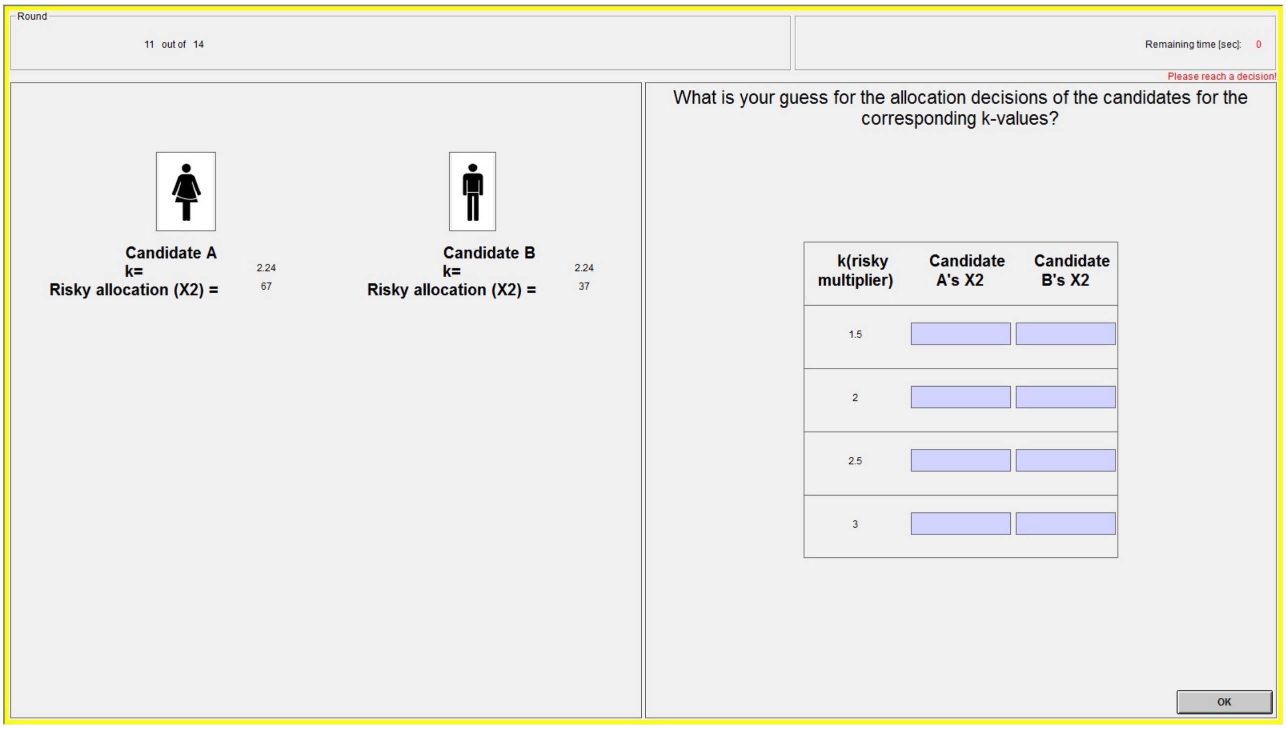


Figure B.8: Guess screen of candidates in NO INFO treatment

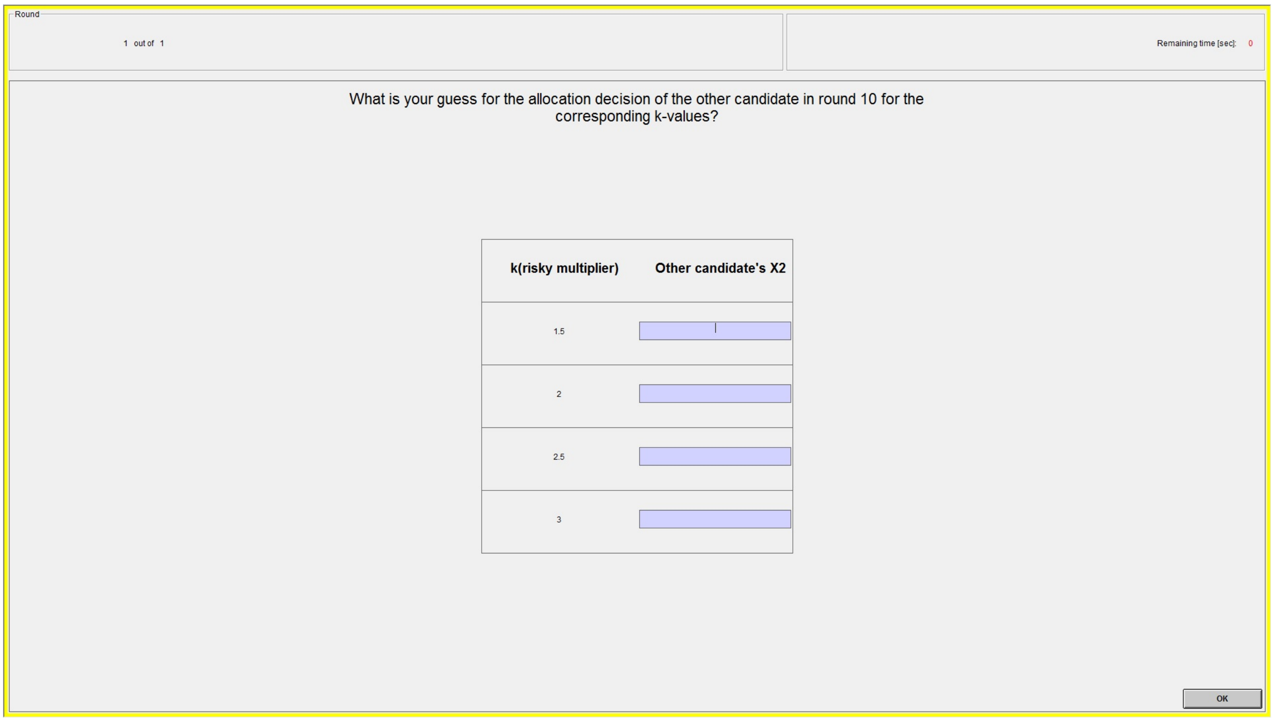


Figure B.9: Voting screen of the candidate in INFO treatment

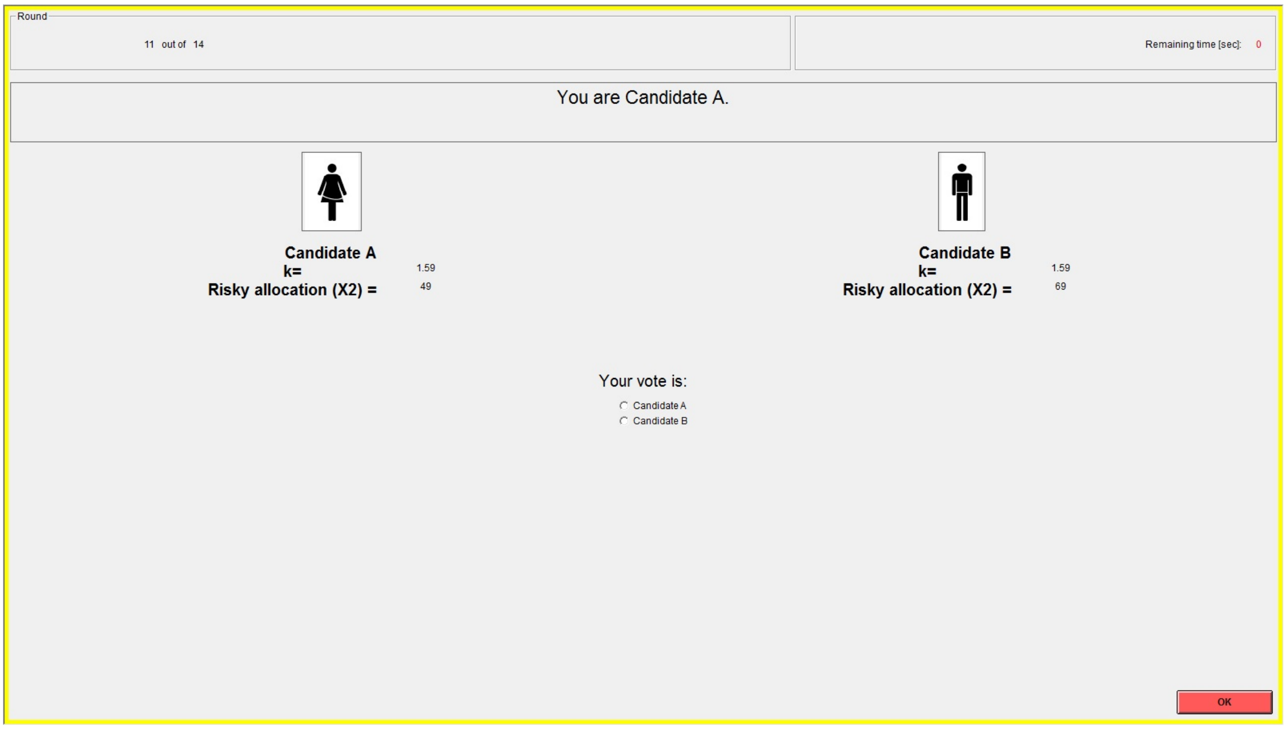


Figure B.10: Guess screen of the candidate in INFO treatment

Figure B.11: Poll screen in Period 4

Figure B.12: Voting screen in Period 5

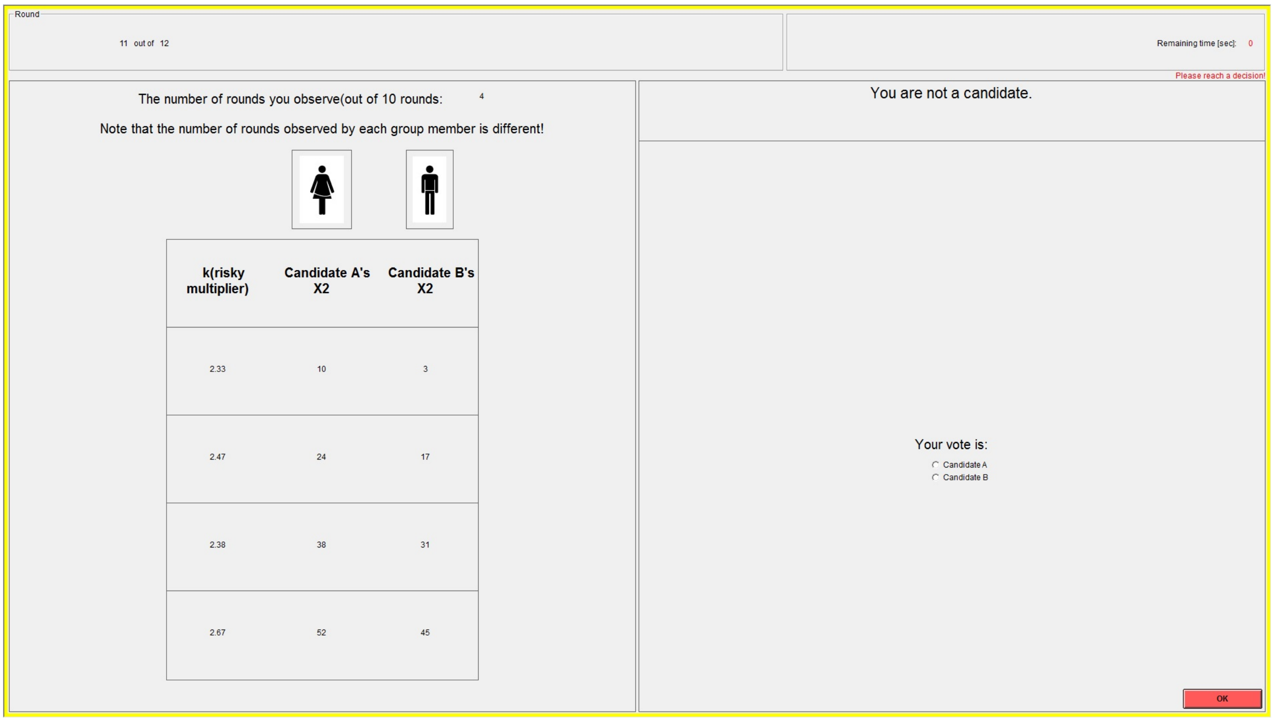
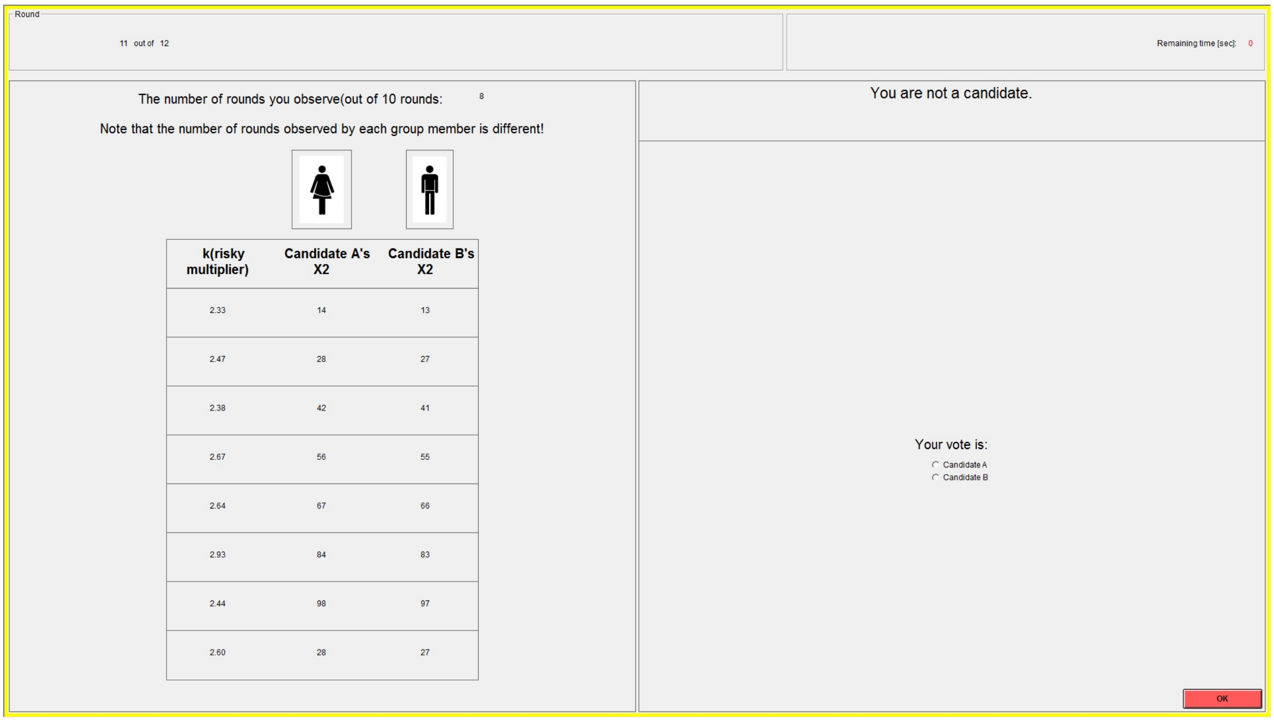


Figure B.13: Voting screen of another voter in Period 5



B.3.2 Quiz

1. Suppose that the chosen risky multiplier is 2.30. For the corresponding multiplier, you allocated 60 token of your endowment to the risky option. How much do you automatically allocated to the riskless option?

   (a) 40 token

   (b) 60 token

   (c) 0 token

   Answer: Allocation to riskless option= 100 token − Allocation to risky option. Therefore (a) 40 token is the correct answer.

2. At the end of this round, how much token could you earn with that allocation (both risky and riskless investment)? Please choose all suitable.

   Remember that k(risky multiplier)= 2.30 and you allocated to risky option 60 token.

   (a) 0 token

   (b) 40 token

   (c) 60 token

   (d) 138 token

   (e) 178 token

   Answer:

The return of riskless option:	40 token
The return of risky option:	60*2.30= 138 token, if the risky option is successful (50% of chance)
	0 token, if the risky option is failed (50% of chance)

	Risky option is Successful	Risky option is Failed
Riskless option	40	40
Risky option	138	0
Total	178	40

So the possible total returns from your allocation are either 40 token or 178 token. The correct answers are (b) and (e).

3. In the previous round the chosen risky multiplier was 2.30. This round, the risky multiplier is 2.80. How does this affect the return and the risk of the option?

   (a) Both the risk and the return of the option increases.

   (b) The risk increases but the return of the option stays same.

   (c) The risk doesn't change, but the return of the option increases.

   (d) Neither the risk nor the return of the option changes.

   Answer: The risky multiplier is only related to the return of the risky option. The risk of the option is same since the success/ failure probability of the risky option is same across rounds.

   Consider you allocated the same amount of tokens to the risky option with the new multiplier. Remember that for k=2.30, you allocated 60 token and the possible returns were 40 tokens and 178 tokens.

   For k= 2.80:

The return of riskless option:	40 token
The return of risky option:	60*2.80= 168 token, if the risky option is successful (50% of chance)
	0 token, if the risky option is failed (50% of chance)

	Risky option is Successful	Risky option is Failed
Riskless option	40	40
Risky option	168	0
Total	208	40

Therefore the risk stays same, but with a higher risky multiplier the return of the option increases. So the correct answer is (c).

B.3.3 Post-experiment Survey

1. Are you a student or a staff?
2. Please identify your department or school.
3. Please indicate your year.
4. What is your ethnic group?
5. Please identify your annual personal income. (If you are not working, please specify the amount of your budget.)
6. How many brothers and sisters do you have?
7. How many of your siblings are female?
8. Where were you born in relation to your sibling(s)? Click on the button that best descibes your birth order.
9. Please rate the difficulty of the experiment from 1 to 5 (1: easy- 5:hard).
10. Is being elected as a leader affected your investment decision? (If you haven't been elected as a leader, would being elected affect your investment?)
11. Please explain briefly why being elected as a leader affects/doesn't affect your investment decision.

Appendix C

Appendix: Chapter 3

C.1 Proofs and Supporting Results

Proof of Proposition 1. Suppose, for contradiction, that there is an equilibrium in which $\mu_i\left(\rho_i\left(\theta_i,\theta_{-i}\right)\right)$ puts probability less than one on type θ_i. If $\theta_i = s$, then principal with type (s,θ_{-i}) can deviate and report s to agent i while keeping her report to the other agent unchanged. This increases agent i's beliefs and hence his action, without changing the other agent's action, and benefits the principal. If $\theta_i = f$, then the principal with type $\theta' = \left(s,\theta'_{-i}\right)$, for some θ'_{-i}, must be sending $\mu_i\left(\rho_i\left(f,\theta_{-i}\right)\right)$ with positive probability. This implies that $\mu_i\left(\rho_i\left(f,\theta_{-i}\right)\right)$ puts probability less than one on $\theta_i = s$ as well. But then principal with type θ' can deviate and report s to agent i while keeping her report to the other agent unchanged. This increases agent i's beliefs and benefits the principal. □

Proof of Proposition 2. We will first show that $\rho(s,s) = (s,s)$ in any equilibrium. Suppose, for contradiction, that there exists a $\theta \neq (s,s)$ such that $\rho(\theta) = \rho(s,s)$. This implies that $\mu_i^i(\rho(s,s)) < 1$ for some $i = 1,2$. But then, type (s,s) has a profitable deviation to $r = (s,s)$. Therefore, we conclude that $\rho(s,s) = (s,s)$ in any equilibrium. Suppose now that $\rho(f,f) = (f,f)$. If $\rho(s,f) \neq \rho(f,s)$, then there is full information revelation. If $\rho(s,f) = \rho(f,s) = \emptyset$, then type (f,f) has a profitable deviation to $\emptyset$. Suppose, alternatively, that $\rho(f,f) = \emptyset$ but $\rho(s,f) \neq \emptyset$.

If $\rho(f,s) \neq \emptyset$ as well, then there is full information revelation. If $\rho(f,s) = \emptyset$, then type (f,s) has a profitable deviation. Similarly, if $\rho(f,f) = \emptyset$ but $\rho(f,s) \neq \emptyset$. □

Proof of Proposition 3. Suppose, for contradiction, that agent i learns something about his own type from the principal's equilibrium report. In other words, suppose that there exist i, θ and θ' such that $\mu_i^i(\rho_i(\theta)) > \mu_i^i(\rho_i(\theta'))$. This implies that $\alpha_i(\rho_i(\theta)) > \alpha_i(\rho_i(\theta'))$. But then, principal with type θ' can deviate and report $\rho_i(\theta)$ to agent i while keeping her report to the other agent unchanged. This increases agent i's action, without changing the other agent's action, and benefits the principal. □

Proof of Proposition 4. It is easy to construct a completely uninformative equilibrium.[1] It is also easy to see that there is no equilibrium in which the agents receive full information. In fact, we can show that type (f,f) must always send the same feedback with some other type. Because, otherwise, at least one of the agents must have beliefs that put positive probability on s after some message r and type (f,f) would have a profitable deviation to r. In order to prove that $\rho(s,s) = \rho(f,f)$, suppose, for contradiction, that $\rho(f,f) \neq \rho(s,s)$. Then, the previous argument implies that $\rho(f,f) = \rho(f,s)$ or $\rho(f,f) = \rho(s,f)$. Suppose first that $\rho(f,f) = \rho(f,s)$. This implies that $\rho(s,f) = \rho(s,s)$ since otherwise type (s,f) has a profitable deviation to $\rho(s,s)$. But this implies that type (f,f) has a profitable deviation to $\rho(s,s)$. We obtain a similar contradiction if $\rho(f,f) = \rho(s,f)$. □

Proof of Proposition 5. Fix i and θ_{-i} and let $\theta = (s, \theta_{-i})$. For any $r \in M_{-i}(\theta)$

$$v(a_i(1), a_{-i}(\mu_{-i}(r)), s, \theta_{-i}) \geq v(a_i(\mu_i^S(\emptyset)), a_{-i}(\mu_{-i}(r)), s, \theta_{-i}).$$

Therefore, no feedback does not increase the payoff for type s but it has a cost,

[1] This is well known in the literature: in a cheap-talk game, there is always a babbling equilibrium.

which implies that $q_i(s|s,\theta_{-i}) = 1$. Now let $\theta = (f, \theta_{-i})$. For any $r \in M_{-i}(\theta)$,

$$(1-\eta)v(a_i(\mu_i^S(\emptyset)), a_{-i}(\mu_{-i}(r)), f, \theta_{-i}) + \eta v(a_i(p), a_{-i}(\mu_{-i}(r)), f, \theta_{-i}) \\ > v(a_i(0), a_{-i}(\mu_{-i}(r)), f, \theta_{-i}),$$

and hence there exists a positive measure of principals with small enough costs $c(\emptyset, r|f, \theta_{-i})$ who prefer sending no information. This implies that $q_i(\emptyset|f,\theta_{-i}) > 0$ and $\mu_i^S(\emptyset) = 0$. Since there is also a positive measure of principals who always tell the truth, we also have $q_i(f|f,\theta_{-i}) > 0$. □

Beliefs in private-verifiable feedback with lying cost and naive agents: Bayes' rule implies that beliefs of the sophisticated agents are $\mu_i^S(s) = 1,\ \mu_i^S(f) = 0,\ \mu_i^S(\emptyset) = 0$ while average beliefs are $\mu_i(s) = 1,\ \mu_i(f) = 0,\ \mu_i(\emptyset) = \eta p$. Average beliefs conditional on state are $\mu_i(\theta_i = s) = 1$ and

$$\begin{aligned} \mu_1(\theta_1 = f) &= \left(\frac{p(ff)}{p(ff)+p(fs)} q_1(\emptyset|ff) + \frac{p(fs)}{p(ff)+p(fs)} q_1(\emptyset|fs)\right)\eta p \\ \mu_2(\theta_2 = f) &= \left(\frac{p(ff)}{p(ff)+p(sf)} q_2(\emptyset|ff) + \frac{p(sf)}{p(ff)+p(sf)} q_2(\emptyset|sf)\right)\eta p \end{aligned}$$

Proof of Proposition 6. Since $\mu_i(ss) = 1$ and sending no information is costly, $q(ss|ss) = 1$. Since there are some naive agents and principals with small costs of not telling the truth as well as those with large costs, $q(\emptyset|ff) > 0$ and $q(ff|ff) > 0$. The left hand side of (3.1) is equal to the worst payoff that type sf expects from sending $\emptyset$. Therefore, if this condition holds, then there will be some principals who will find sending $\emptyset$ profitable. If it does not hold, then there is an equilibrium in which $q(\emptyset|sf) = q(\emptyset|fs) = 1$, which is supported with beliefs $\mu_i^S(\emptyset) = 0$. □

Beliefs in public-verifiable feedback with lying cost and naive agents: Beliefs are given by $\mu_i^S(s, m_{-i}) = 1,\ \mu_i^S(f, m_{-i}) = 0$ for all m_{-i} and

$$\mu_1^S(\emptyset) = \frac{q(\emptyset|sf)p(sf)}{q(\emptyset|sf)p(sf) + q(\emptyset|fs)p(fs) + q(\emptyset|ff)p(ff)}$$
$$\mu_2^S(\emptyset) = \frac{q(\emptyset|fs)p(fs)}{q(\emptyset|fs)p(fs) + q(\emptyset|sf)p(sf) + q(\emptyset|ff)p(ff)}$$

Note that beliefs of agent 1 after no feedback decrease (i.e., are smaller than the prior) if and only if

$$q(\emptyset|sf)\frac{p(sf)}{p(ss)+p(sf)} < q(\emptyset|fs)\frac{p(fs)}{p(ff)+p(fs)} + q(\emptyset|ff)\frac{p(ff)}{p(ff)+p(fs)}.$$

In particular, this is the case in a symmetric equilibrium where $q(\emptyset|sf) = q(\emptyset|fs)$ and beliefs about the other agent is uniform, i.e., $p(ss) = p(sf)$ and $p(fs) = p(ff)$. Average beliefs are $\mu_i(s, m_{-i}) = 1,\ \mu_i(f, m_{-i}) = 0$ for all m_{-i} and $\mu_i(\emptyset) = (1 - \eta)\mu_i^S(\emptyset) + \eta p$. Also, note that beliefs after no information are larger than those under private-verifiable feedback. Average beliefs conditional on the actual state are

$$\mu_1(\theta_1 = s) = \frac{p(ss)}{p(ss)+p(sf)} + \frac{p(sf)}{p(ss)+p(sf)}[q(sf|sf) + q(\emptyset|sf)\mu_1(\emptyset)]$$
$$\mu_1(\theta_1 = f) = \frac{p(ff)q(\emptyset|ff) + p(fs)q(\emptyset|fs)}{p(ff)+p(fs)}\mu_1(\emptyset)$$
$$\mu_2(\theta_2 = s) = \frac{p(ss)}{p(ss)+p(fs)} + \frac{p(fs)}{p(ss)+p(fs)}[q(fs|fs) + q(\emptyset|fs)\mu_2(\emptyset)]$$
$$\mu_2(\theta_2 = f) = \frac{p(ff)q(\emptyset|ff) + p(fs)q(\emptyset|sf)}{p(ff)+p(sf)}\mu_2(\emptyset)$$

Proof of Proposition 7. Fix i and θ_{-i}, let $\theta = (s, \theta_{-i})$ and r_{-i} be the message sent to agent $-i$. Suppose, for contradiction, that there exists $r_i \neq s$ such that $q_i(r_i|s, \theta_{-i}) > 0$. This implies that

$$v(a_i(\mu_i(r_i)), a_{-i}(\mu_{-i}(r_{-i})), s, \theta_{-i}) > v(a_i(\mu_i(s)), a_{-i}(\mu_{-i}(r_{-i})), s, \theta_{-i}),$$

for otherwise type s would not find sending r_i optimal. Therefore, $a_i(\mu_i(r_i)) >$

$a_i(\mu_i(s))$ and hence for all r_{-i}

$$v(a_i(\mu_i(r_i)), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) > v(a_i(\mu_i(s)), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}).$$

Since cost of r_i is smaller than cost of s for type f, this implies that $q_i(s|f, \theta_{-i}) = 0$. By assumption, there exist s types whose lying costs are so large that they tell the truth, i.e., $q_i(s|s, \theta_{-i}) > 0$. Therefore, $\mu_i^S(s) = 1$, which contradicts

$$v(a_i(\mu_i(r_i)), a_{-i}(\mu_{-i}(r_{-i})), s, \theta_{-i}) > v(a_i(\mu_i(s)), a_{-i}(\mu_{-i}(r_{-i})), s, \theta_{-i}).$$

Also, by assumption, there exist f types whose lying costs are so large that they tell the truth, i.e., $q_i(f|f, \theta_{-i}) > 0$. This implies that $\mu_i^S(f) = 0$. Now, suppose, for contradiction, that $q_i(s|f, \theta_{-i}) = 0$. This implies that $\mu_i^S(s) = 1$ and hence for any r_{-i}

$$v(a_i(1), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) > v(a_i(0), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}).$$

Assume first that $q_i(\emptyset|f, \theta_{-i}) = 0$ so that $q_i(f|f, \theta_{-i}) = 1$. Since

$$F\left(v(a_i(1), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) > v(a_i(0), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i})\right) > 0$$

deviation to s is profitable for some principals with small enough costs $c_{sr_{-i}|f\theta_{-i}}$, contradicting $q_i(f|f, \theta_{-i}) = 1$. Assume now that $q_i(\emptyset|f, \theta_{-i}) > 0$ so that $\mu_i^S(\emptyset) = 0$. Expected payoff to s is greater than the expected payoff to f and $\emptyset$, i.e.,

$$x = v(a_i(1), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) - v(a_i(0), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) > 0$$

$$\begin{aligned} y = v(a_i(1), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) - (1 - \eta)v(a_i(0), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) \\ - \eta v(a_i(\mu_i(p)), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) > 0 \end{aligned}$$

which imply that $F(x) > 0$ and $G(y) > 0$. These imply that for some principals reporting s is strictly better than reporting f and $\emptyset$, contradicting $q_i(s|f,\theta_{-i}) = 0$.

Note that $q_i(s|s,\theta_{-i}) = 1$ and $q_i(f|f,\theta_{-i}) > 0$ imply $\mu_i^S(s) > p$. Suppose, for contradiction, that $q_i(\emptyset|f,\theta_{-i}) = 0$. Then

$$x = (1-\eta)v(a_i(\mu_i^S(\emptyset)), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) + \eta v(a_i(\mu_i(p)), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) - v(a_i(0), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) > 0$$

and hence $F_{\emptyset r_{-i}|f\theta_{-i}}(x) > 0$, i.e., there exist principals for whom $\emptyset$ is better than f. Also

$$G\left(v(a_i(1), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) - v(a_i(0), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i})|r_{-i}, \theta_{-i}\right) < 1$$

implies that there exist principals for whom

$$(1-\eta)v(a_i(\mu_i^S(\emptyset)), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) + \eta v(a_i(\mu_i(p)), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) - c_{\emptyset r_{-i}|f\theta_{-i}}$$
$$> (1-\eta)v(a_i(\mu_i^S(s)), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) + \eta v(a_i(1), a_{-i}(\mu_{-i}(r_{-i})), f, \theta_{-i}) - c_{sr_{-i}|f\theta_{-i}}$$

so that they prefer $\emptyset$ to s. This proves that $q_i(\emptyset|f,\theta_{-i}) > 0$. □

Beliefs in private-unverifiable feedback with lying cost and naive agents: Bayes' rule implies that beliefs of the sophisticated agents are $\mu_i^S(f) = 0$, $\mu_i^S(\emptyset) = 0$

$$\mu_1^S(s) = \frac{p(ss) + p(sf)}{p(ss) + p(sf) + p(fs)q_1(s|fs) + p(ff)q_1(s|ff)}$$
$$\mu_1^S(s) = \frac{p(ss) + p(fs)}{p(ss) + p(fs) + p(sf)q_2(s|sf) + p(ff)q_2(s|ff)}$$

while average beliefs are $\mu_i(s) = (1-\eta)\mu_i^S(s) + \eta$, $\mu_i(f) = 0$, $\mu_i(\emptyset) = \eta p$. Average beliefs conditional on state are $\mu_i(\theta_i = s) = \mu_i(s)$ and

$$\begin{aligned}
\mu_1(\theta_1 = f) &= \left(\frac{p(fs)}{p(fs)+p(ff)} q_1(s|fs) + \frac{p(ff)}{p(fs)+p(ff)} q_1(s|ff) \right) \mu_1(s) \\
&+ \left(\frac{p(fs)}{p(fs)+p(ff)} q_1(\emptyset|fs) + \frac{p(ff)}{p(fs)+p(ff)} q_1(\emptyset|ff) \right) \eta p \\
\mu_2(\theta_2 = f) &= \left(\frac{p(sf)}{p(sf)+p(ff)} q_2(s|sf) + \frac{p(ff)}{p(sf)+p(ff)} q_2(s|ff) \right) \mu_2(s) \\
&+ \left(\frac{p(sf)}{p(sf)+p(ff)} q_2(\emptyset|sf) + \frac{p(ff)}{p(sf)+p(ff)} q_2(\emptyset|ff) \right) \eta p
\end{aligned}$$

Note that $\mu_i^S(s) > p$ and hence $\mu_i(s) > p$.

Proof of Proposition 8. Suppose, for contradiction, that there exists $r \neq ss$ such that $q(r|ss) > 0$. Then $v(a(\mu(r)), ss) > v(a(\mu(ss)), ss)$, which implies that $v(a(\mu(r)), \theta) > v(a(\mu(ss)), \theta)$ for all θ. But then no type would find it optimal to send ss. Since there are some ss types who send ss because of high lying costs, this implies that $\mu_i(ss) = 1$ for $i = 1, 2$. This, however, shows that ss induces the highest beliefs and actions and hence all types ss should send ss, a contradiction. For small costs of lying this also implies that some other type must also send ss. Finally, there exist principals with high enough lying costs who always tell the truth. □

Beliefs in public-unverifiable feedback with lying cost and naive agents: The strategy is given by

$$q(ss|ss) = 1$$

$$q(ss|sf) \in (0,1),\ q(s\emptyset|sf) \in (0,1),\ q(sf|sf) = 1 - q(ss|sf) - q(s\emptyset|sf) > 0$$

$$q(ss|fs) \in (0,1),\ q(\emptyset s|fs) \in (0,1),\ q(fs|fs) = 1 - q(ss|fs) - q(\emptyset s|fs) > 0$$

$$q(ss|ff) \in (0,1),\ q(\emptyset\emptyset|ff) \in (0,1),\ q(ff|ff) = 1 - q(ss|ff) - q(\emptyset\emptyset|ff) > 0$$

Bayes' rule implies that beliefs are given by

$$
\begin{aligned}
\mu_1^S(ss) &= \frac{p(ss)+p(sf)q(ss|sf)}{p(ss)+p(sf)q(ss|sf)+p(fs)q(ss|fs)+p(ff)q(ss|ff)} \\
\mu_2^S(ss) &= \frac{p(ss)+p(fs)q(ss|fs)}{p(ss)+p(fs)q(ss|fs)+p(sf)q(ss|sf)+p(ff)q(ss|ff)} \\
\mu_1^S(sf) &= 1, \mu_2^S(fs)=1, \mu_1^S(fs)=0, \mu_2^S(sf)=0 \\
\mu_1^S(s\emptyset) &= 1, \mu_2^S(\emptyset s)=1, \mu_1^S(\emptyset s)=0, \mu_2^S(s\emptyset)=0 \\
\mu_i^S(\emptyset\emptyset) &= 0, \mu_i^S(ff)=0, \mu_i^S(\emptyset f)=\text{free}, \mu_i^S(f\emptyset)=\text{free}
\end{aligned}
$$

$$\mu_1(ss)=\eta+(1-\eta)\mu_1^S(ss)$$

$$\mu_2(ss)=\eta+(1-\eta)\mu_2^S(ss)$$

$$\mu_1(sf)=1, \mu_2(fs)=1, \mu_1(fs)=0, \mu_2(sf)=0$$

$$\mu_1(s\emptyset)=1, \mu_2(\emptyset s)=1, \mu_1(\emptyset s)=\eta p, \mu_2(s\emptyset)=\eta p$$

$$\mu_i(\emptyset\emptyset)=\eta p, \mu_i(ff)=0,$$

$$\mu_1(\emptyset f)=\eta p+(1-\eta)\mu_1^S(\emptyset f), \mu_2(\emptyset f)=(1-\eta)\mu_2^S(\emptyset f)$$

$$\mu_1(f\emptyset)=(1-\eta)\mu_1^S(f\emptyset), \mu_2(f\emptyset)=\eta p+(1-\eta)\mu_2^S(f\emptyset)$$

Beliefs as a function of own feedback are given by

$$
\begin{aligned}
\mu_1^S(s) &= \frac{p(ss)+p(sf)[q(ss|sf)+q(sf|sf)]}{p(ss)+p(sf)[q(ss|sf)+q(sf|sf)]+p(fs)q(ss|fs)+p(ff)q(ss|ff)} \\
\mu_1^S(f) &= \frac{p(ff)}{p(fs)+p(ff)}\mu_1^S(ff)+\frac{p(fs)}{p(fs)+p(ff)}\mu_1^S(fs)=0 \\
\mu_1^S(\emptyset) &= \frac{p(\emptyset\emptyset)}{p(\emptyset\emptyset)+p(\emptyset s)}\mu_1^S(\emptyset\emptyset)+\frac{p(\emptyset s)}{p(\emptyset\emptyset)+p(\emptyset s)}\mu_1^S(\emptyset s)=0
\end{aligned}
$$

$$
\begin{aligned}
\mu_1(s) &= \eta + (1-\eta)\mu_1^S(s) \\
\mu_1(f) &= 0 \\
\mu_1(\emptyset) &= \eta p
\end{aligned}
$$

Average beliefs conditional on state are

$$
\begin{aligned}
\mu_1(\theta = s) &= \frac{p(ss)}{p(ss)+p(sf)}\mu_1(ss) \\
&+ \frac{p(sf)}{p(ss)+p(sf)}[q(ss|sf)\mu_1(ss) + q(s\emptyset|sf)\mu_1(s\emptyset) + q(sf|sf)\mu_1(sf)] \\
&= \frac{p(ss)}{p(ss)+p(sf)}\mu_1(ss) + \frac{p(sf)}{p(ss)+p(sf)}[q(ss|sf)\mu_1(ss) + 1 - q(ss|sf)] \\
\mu_1(\theta = f) &= \frac{p(fs)}{p(fs)+p(ff)}[q(ss|fs)\mu_1(ss) + q(\emptyset s|fs)\mu_1(\emptyset s) + q(fs|fs)\mu_1(fs)] \\
&+ \frac{p(ff)}{p(fs)+p(ff)}[q(ss|ff)\mu_1(ss) + q(\emptyset\emptyset|ff)\mu_1(\emptyset\emptyset) + q(ff|ff)\mu_1(ff)] \\
&= \frac{p(fs)}{p(fs)+p(ff)}[q(ss|fs)\mu_1(ss) + q(\emptyset s|fs)\eta p] \\
&+ \frac{p(ff)}{p(fs)+p(ff)}[q(ss|ff)\mu_1(ss) + q(\emptyset\emptyset|ff)\eta p]
\end{aligned}
$$

C.2 Instructions, Quiz, and Survey

C.2.1 Instructions

[2]Welcome. Thank you for participating in our study, which is about economic decision making. During the study, you will be asked to solve a set of questions and make decisions regarding your performance. You will earn 10 TL for your participation. Besides this show-up fee, your earnings in the experiment will depend on your performance, your decisions and chance. There is no misleading or deception in this study. The rules that we will state are completely correct and your payment will be determined accordingly. Payments will be made privately and in cash at the end of the experiment. Your decisions during the experiment will be recorded under a subject number and will never be matched with your identifying information.

The experiment consists of 5 periods and within each period there are 2 rounds. At the end, one round out of the 10 rounds will be randomly selected and your earnings will be determined according to your payoff in that chosen round. During the experiment, all monetary earnings will be denoted in ECU ("experimental currency unit"), where 1 ECU equals 0.06 TL.

For each of the 5 periods, there will be different rules. We will now explain the decision task which you will encounter in all rounds. Specific instructions that pertain to each of the 5 periods will be explained just before that period starts.

General Rules

At the beginning of the experiment, participants will be randomly assigned to one of the roles of "Principal" and "Agent". The roles will be fixed throughout the experiment. Now, we are going to explain the rules that will be implemented for agents.

---

[2]Original instructions were in Turkish and are available upon request. Note that verbal instructions were supplemented with graphical slides to ease understanding. These are also available upon request. The instructions given here are for the following treatment order configuration: Truthful, Private-Verifiable, Public-Verifiable, Private-Unverifiable and Public-Unverifiable feedback.

Agents:

If you are in the role of agent, you will perform a task. There are 2 types of tasks:

1. Addition task

2. Verbal task

One of these will be assigned to you randomly. In the addition task, you are asked to solve questions involving the addition of 4 or 5 two-digit numbers in 120 sec. (e.g. 11+ 48+ 96+24=?). In the verbal task, you are asked to solve in 120 sec. the following type of questions:

- General knowledge questions (geography, literature, sports...). For example:
  - Which country has the capital city of *Ankara*?
  - Who is the writer of the novel *Crime and Punishment*?
- Verbal classification questions. For example:
  - Which word does not belong to the group?
  - Which of the following has the relationship between *Tea:Sugar*?
- Number-word matching questions. Here we assign each letter to a number and ask the following type of question:
  - ALI= 123
  - ILE= 324
  - AILE=? (where the answer is AILE= 1324.)

For each period, a "target score" will be determined for each type of task (addition and verbal), independently and separately. You will work on only one type of task in the two rounds of a period. The outcome will be determined as follows: if the number of correct answers is:

- higher than or equal to the target score, *Success*,
- lower than the target score, *Failure.*

The target score will be kept fixed throughout the two rounds of the same period. That is, your number of correct answers in both rounds within the same period will be compared with the same target score. You will not be informed about the target score.

The payoffs to success and failure in the 1st round are as follows:

If you are successful (you scored at least as high as the target score), you will earn 300 ECU. If you have failed (you scored less than the target score), you will earn 100 ECU.

After the first round, you have two options for the second round. You can either choose to base your 2nd round payoff on your second-round performance, or leave it to a chance mechanism. As an agent:

- If you perform in the second round,
  - You will perform the same type of task as in the first round.
  - If you are successful in the second round (meet/pass the target score) you will get 300 ECU,
  - If you fail in the second round (fall below the target score) you will get 100 ECU.
- If the chance mechanism is implemented in the second round,
  - You will not perform in the 2nd round.
  - You will earn 300 ECU with $X\%$ chance, and 100 ECU with $(100 - X)\%$.

You will make this performance/chance mechanism decision by answering the following question:

What is the minimum % chance of winning in the chance mechanism (X), that will make you willing to leave your second-round payoff to the chance mechanism rather than your own performance?

In other words, you will state for what winning chances you will choose to base your payoff on your own performance and for what winning chances you prefer the chance mechanism. How you make this decision is related to how confident you are about your success in the second round. Consider the following example:

Suppose that someone thinks there is a 80% chance of succeeding in the 2nd performance task. In this case, this person would need the chance mechanism to give at least an 80% chance of winning, to be convinced to leave his/her payoff to chance. This is because he/she has higher expected earnings from performing in the 2nd round (as opposed to picking the chance mechanism), if there is a less than 80% chance of winning in the chance mechanism.

The probability of winning in the chance mechanism (X) will be determined randomly by the computer. If you choose the performance mechanism at the stated chance probability, then you will perform in the second round. Otherwise, your payoff will depend on the chance mechanism (you will earn 300 ECU with probability X% and 100 ECU with probability $(100 - X)$%).

Groups:

In each period, 3-person groups that consist of one principal and two agents will be formed. At the beginning of each period, groups will be reshuffled and formed again. The two agents in the same group will always perform different type of tasks (if one does addition the other does verbal) and will be evaluated according to different target scores.

Principals:

Participants in the role of principals will not perform any task. However, they will be able to see whether each agent in their group has been successful or not on their screen.

The payoffs of principals are as follows (valid for all periods):

1st round of each period: A fixed payoff of 100 ECU

2nd round of each period: The payoff will depend on agents' decisions and performances.

Return of agent i to principal in the second round ($return_i$):

- If the agent performed in the second round: $return_i$= Number of correct answers in the second round * 20 ECU

- If the agent did not perform (if the chance mechanism was implemented): $return_i$= 0 ECU

Payoff function of the principal for the second round= *50ECU (constant payoff)*+ $\min(return_1, return_2)$, where $return_1$ and $return_2$ refer to the returns from agent 1 and agent 2, respectively.

[Examples were graphically shown on slides. They are available upon request.]

Therefore if at least one of the agents chooses the chance mechanism, the principal will not earn a payoff from the agents' performance. So in order to receive a payoff from agents' performance, both of the agents must perform in the second round.

Now, we have a short quiz about the rules we have just explained. Your answers will not affect your earnings. Rather, they are aimed to check your understanding of the rules. Please do not hesitate to ask questions if there is any part you do not understand.

[Quiz: see Section C.2.2]

Part I (Truthful Feedback):

As explained before, 3-person groups, consisting of 1 principal and 2 agents, will be formed.

Agents will observe their performance outcome (Success/Failure) before making their second (post-feedback) performance/chance decision. This feedback will be sent by the computer and it will certainly be correct information. Agents will make the performance/chance mechanism decision twice: before and after they receive the feedback on their performance outcome (Success or Failure). One of the two (pre-feedback and post-feedback) stated minimum probabilities will be randomly chosen and used to determine whether the agent will perform or not in the 2nd stage.

The timeline for the agents will be as follows *[shown graphically on slides]*

1. (1st round) Task will be performed.
2. The decision about performance/chance mechanism for the 2nd round will be made.
3. Feedback about the 1st round outcome will be given (Success/Failure).
4. The pre-feedback performance/chance decision can be changed or kept the same.
5. One of the pre- and post-feedback decisions will be randomly chosen. The stated minimum will be compared to the (randomly chosen) chance probability X and either the performance or the chance mechanism will be applied in the 2nd round accordingly.
6. (2nd round) If the performance mechanism is implemented: The same type of task as in the first round will be performed and your earnings will depend on whether you succeed or not (300 ECU/100 ECU). If the chance mechanism is implemented: There will be no performance. 300 ECU with probability X and 100 ECU with probability $(100 - X)$ will be earned.

As agents, the feedback provides you with information about your performance. It can help you evaluate your performance with respect to the target score. Note that the difficulty of the questions might be different between rounds.

Principals will see the 1st-round performance outcomes of the two agents in their group. Principals will also be asked to guess, for each agent, the required minimum winning chance (%) to use the chance mechanism instead of performing in the 2nd stage, as stated by that agent. That is, the principals will guess at which winning probabilities in the chance mechanism each agent will enter/stay out. This guess concerns the post-feedback entry decisions of each agent. If the principal's guess is within ± 5 percentage points of the agent's actual stated minimum winning chance, he/she will earn 10 ECU extra (for each successful guess). At the end of the experiment, these extra earnings in the chosen round will be added to the payoff from that round.

Do you have any questions?

Part II (Private Verifiable Feedback):

In this part, agents may receive feedback about their performance outcome from the principal. Any information that is sent by the principal has to be true. However, the principal also has the option to withhold the information, i.e. not send any message.

The messages from principal will be privately sent. In other words, principals can choose different messages (either the true outcome or no information) for different agents and agents will only see the message the message sent to them, not to the other agent.

As in the previous part, agents make a pre-feedback and post-feedback decision, and one is randomly chosen to be implemented. Principals are asked to guess the required minimum winning probabilities in the chance mechanism (%), stated by the agents post-feedback. The same rules apply for bonus calculation.

[Screenshots of the principal and the agent decision screens were shown on the slides. They are available upon request.]

Do you have any questions?

Part III (Public Verifiable Feedback):

In this part, agents may receive feedback about their performance outcome from the principal. Any information that is sent by the principal has to be true. However, the principal also has the option to withhold information.

The messages from the principal will be publicly sent, meaning that both agents can see the set of messages. The principal can choose to send either truthful information to both of the agents or no information to either.

If the principal chooses to send the information, the agents will see the message sent to the other agent along with their own. Note that the other agent performs another type of task and is evaluated with a different target score. Therefore, the other agent's outcome is independent of yours. However, the message sent to the other agent might provide you with information about the principal's strategy.

As in the previous part, agents make a pre-feedback and post-feedback decision, and one is randomly chosen to be implemented. Principals are asked to guess the required minimum winning probabilities in the chance mechanism (%), stated by the agents post-feedback. The same rules apply for bonus calculation.

[Screenshots of the principal and the agent decision screens were shown on the slides. They are available upon request.]

Do you have any questions?

Part IV (Private Unverifiable Feedback):

In this part, agents may receive feedback about their performance outcome from the principal. Information sent by the principal does not need to be true. The principal also has the option to withhold information.

The principal will observe the actual performance of the two agents and after that, will choose to send one of the following messages: "Success", or "Failure" or "No Information".

The messages from the principal will be privately sent. In other words, principals can choose different messages for agents and agents will only see the message about their own performance.

As in the previous part, agents make a pre-feedback and post-feedback decision, and one is randomly chosen to be implemented. Principals are asked to guess the required minimum winning probabilities in the chance mechanism (%), stated by the agents post-feedback. The same rules apply for bonus calculation.

[Screenshots of the principal and the agent decision screens were shown on the slides. They are available upon request.]

Do you have any questions?

Part V (Public Unverifiable Feedback):

In this part, agents will receive feedback about their performance outcome from the principal. Information sent by the principal does not need to be true. The principal also has the option to withhold information.

The principal will observe the actual performance of the two agents and after that, will choose to send one of the following messages: "Success", or "Failure" or "No Information".

The messages from the principal will be publicly sent, meaning that agents will see the message sent to the other agent along with their own. Note that the other agent performs another type of task and is evaluated with a different target score. Therefore, other agent's outcome is independent of yours. However, the message sent to other agent might provide you with information about principal's strategy.

As in the previous part, agents make a pre-feedback and post-feedback decision, and one is randomly chosen to be implemented. Principals are asked to guess the required minimum winning probabilities in the chance mechanism (%), stated by the agents post-feedback. The same rules apply for bonus calculation.

[Screenshots of both the principal and the agent screen were shown on the slides. They are available upon request.]

Do you have any questions?

Survey:

Now you will be asked to answer several questions that will come up on your screen. Thank you again for your participation.

C.2.2 Quiz

1. Assume that you state a 60% chance of being successful in the 2nd stage, and the computer picks the winning probability in the chance mechanism as 55%. Which of the following is true?

   (a) My probability of earning 300 ECU is 55% and my payoff is independent of my performance.

   (b) My payoff is 300 ECU if I am successful and 100 ECU otherwise.

2. Assume that you state a 30% chance of being successful in the 2nd stage, and the computer picks the winning probability in the chance mechanism as 55%. Which of the following is true?

   (a) My probability of earning 300 ECU is 55% and my payoff is independent of my performance.

   (b) My payoff is 300 ECU if I am successful and 100 ECU otherwise.

3. Suppose Person A stated 80% as the minimum winning probability that makes her leave her payoff to chance, while Person B stated 45% for the same decision. Which of those people is more self-confident about their success in the 2nd period?

   (a) A

   (b) B

C.2.3 Post-experiment Survey Questions

1. How old are you?

2. What is your gender?

3. Which year of your degree program are you in?

4. What is your major/faculty?

5. What is your current GPA?

6. Were the rules of the experiment clear and understandable? Please answer on a scale of 1 to 10: 1= not understandable at all, 10= extremely understandable.

7. How difficult was the addition task? Please answer on a scale of 1 to 5: 1= not difficult at all, 5= extremely difficult.

8. How difficult was the verbal task? Please answer on a scale of 1 to 5: 1= not difficult at all, 5= extremely difficult.

9. As agents, when you were making the decision about performance/chance mechanism, was the thought of "if I don't do the performance task in the 2nd period, I will get bored" a factor in your decision? (Yes/No)

10. Disregarding the monetary payoff it brings, how important was it personally for you to be "Successful" (to score higher than the target score)? Please answer on a scale of 1 to 5: 1="Not important at all", 2= "Not important", 3= "Neither important nor unimportant", 4="Important", 5= "Extremely important"