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Selection into Leadership and Dishonest Behavior of Leaders: A Gender Experiment

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Abstract

Leaders often weigh ethical against monetary consequences. We experimentally study such a dilemma where leaders can benefit their groups at the expense of moral costs. First, we measure individual dishonesty preferences and, second, leaders' reporting decisions *for a group* by using payoff-reporting games. We focus on an endogenous leadership setting, where subjects can apply for leadership. Women have less pronounced dishonesty preferences than men, but increase dishonesty as leaders. The increase disappears when leadership is randomly assigned. A follow-up study reveals that women leaders behave dishonestly when they believe their group members prefer dishonesty.

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1 Introduction

Pressing global challenges such as climate change, health crises, and inclusive growth demand ethical decisions from leaders. Moreover, higher ethical conduct of leaders contributes to higher levels of ethical behavior within companies such as a higher prevalence of honesty, reciprocity, and less toxic competition among co-workers (d'Adda et al., 2017; Alan et al., 2022). So far, women are underrepresented in leadership positions *per se* and in sectors in which ethical decision-making would be particularly required (European Institute for Gender Equality, 2012; Gobillon and Roux, 2015; Fernandez-Mateo and Fernandez, 2016; Flabbi et al., 2019; Zenger and Folkman, 2019). Thus, the question has been raised if more women in leadership positions could be a way forward (United Nations, 2019). Indeed, empirical findings, including quasi-experimental evidence from affirmative-action policies, have shown that women in leadership positions can contribute to ethical decision-making, e.g., reducing corruption and increasing the provision of public goods in the political domain (Chattopadhyay and Duflo, 2004; Swamy et al., 2001), increasing social responsibility ratings, and showing greater concern for workers' vulnerability to unemployment risk in the business domain (Bear et al., 2010; Matsa and Miller, 2013). Despite this first evidence, we lack *basic* knowledge on the role of gender in ethical decision-making in leadership positions. In general, research calls for more causal evidence between leadership and its impact on economic outcomes (Garretsen et al., 2020).

To explore female and male leaders' ethical behavior in leadership positions, we study two main research questions in this paper.[1] First, do women and men also behave differently as leaders in ethical decision contexts? Second, do people's ethical preferences influence their decision to select into leadership positions? We address these questions focusing on honesty, a prerequisite for trustworthiness, as one of the core ethical values in business (Schwartz, 2005) and politics (Caselli and Morelli, 2004). That is, we study how people's dishonesty is affected by leadership, i.e., when they decide as leaders who assume responsibility for a group. Moreover, we investigate the role of individual dishonesty preferences in the motivation to become a leader.

There are two key motivations for behaving dishonestly as a leader. First, leaders benefit personally since they are typically compensated and promoted based on their performance. Thus, leaders have the incentive to misreport outcomes, particularly to the entities relevant to their performance evaluation (Burns and Kedia, 2006; Necker and Paetzel, 2022). Second, leaders' decisions impact the payoffs of different stakeholders, e.g., managers' shareholders or politicians' staff members (Berman et al., 1999). Since leaders are, at least partially, evaluated based on the satisfaction of their stakeholders' needs and aspirations, beliefs about stakeholders' preferences may shape leaders' decisions. Moreover, the payoff externalities of leadership decisions indicate the potential role of social preferences and norms for decision-making.

To study dishonest behavior, we conducted an experiment in which participants repeatedly have to report the realization of a private signal, and misreporting can be beneficial to them and others. Specifically, they roll a die and receive a payoff that increases in the

[1] As we have only very few observations of people who did not identify as female or male, we cannot consider these people when analyzing our data. Thus, the rest of the paper only differentiates between men and women.

reported number on the die. This method is known as the die-rolling game by Fischbacher and Föllmi-Heusi (2013). Although the experiment is stylized, it encompasses characteristics that may model dishonest behavior in business situations.[2] For instance, the reporting set-up resembles situations in which managers know the real outcome and may intentionally increase company returns (e.g., Bollen and Pool, 2009; Burns and Kedia, 2006), by misreporting sale figures of teams (Church et al., 2012), the quality of products (Belot and Van de Ven, 2017; Belot and Van De Ven, 2019), or figures to evade taxes (Joulfaian, 2000). The die-rolling paradigm measures dishonesty in a setting with practically no chance of being publicly exposed for misreporting. This is a relevant simplification, as many real-life situations are characterized by a relatively low chance of getting caught and punished. The focus of our study is on changes in behavior across contexts (decisions that affect individual payoffs vs. decisions that affect group payoffs) while keeping incentives and the chance of being caught constant. Importantly, the die-rolling paradigm has been demonstrated to predict real-life behavior in the fields of corrupt behavior in India (Hanna and Wang, 2017) and Denmark (Barfort et al., 2019), wearing masks improperly (Tobol et al., 2020), and free riding in public transportation (Dai et al., 2018; Potters and Stoop, 2016).

In our within-subjects experiment, participants report the outcome of a die roll twice. First, they report for their own outcome only, which serves as a proxy for individual dishonesty preferences. Subsequently, subjects report the outcome of a die roll in the role of a potential group leader, which determines their payoff and the payoff of two group members. Before they make this decision, we analyze subjects' willingness to take up the leadership role by asking them whether they want to become a leader or not (*endogenous leadership*). They learn that if more than one person says "yes," a random draw will select one of the applicants.[3] By measuring subjects' dishonest behavior in an individual context and an endogenous leadership setting allows us two things. First, we can study whether individual dishonesty preferences affect the decision to become a leader. Second, we can analyze how an institutional context with self-chosen (*endogenous*) leadership affects preferences for dishonesty as a group leader. To isolate the effects of endogenous leadership, we ran a control treatment without the choice to apply for leadership, i.e., leaders are randomly chosen (*exogenous leadership*).

The results demonstrate that women behave less dishonestly than men when deciding on individual payoffs. This is in line with lab-experimental evidence that predominantly demonstrates that women behave more ethically than men, e.g., in lying situations when lying only benefits the person who lies and hurts somebody else (e.g., Dreber and Johannesson, 2008; Houser et al., 2012; Muehlheusser et al., 2015; Houser et al., 2016; Grosch and Rau, 2017). This gender difference vanishes when subjects make their second reporting decision as group leaders. The reason is that women increase dishonesty as leaders, while men are similarly dishonest in both decisions. Moreover, we find that dishonest men in the individual decision tend to self-select into leadership and show similar misreporting behavior for individual and group payoffs. By contrast, women's willingness to take over leadership is not related to their individual dishonesty preferences. Our control treatment reveals that women only increase

[2]See Abeler et al. (2019) for a meta-study analysis on dishonest behavior.

[3]If no subject applies, one of *all* three group members is randomly selected as group leader. However, this case has not occurred in our data.

their misreporting from individual to group payoffs when they can apply for leadership, but not with an external appointment. These results demonstrate that women's increase in dishonesty is not driven by the group context *per se*. It is induced by a combination of giving subjects the choice to assume leadership *and* making decisions on behalf of others.

To further investigate why women leaders increase their dishonesty, we conducted a follow-up study. The design is similar to the first study, but we additionally elicit leaders' beliefs on group members' individual dishonesty preferences. We interpret this measure as the leader's perceived group norm when reporting joint payoffs. The study also controls for social value orientation to account for a possible relation between prosociality and misreporting group payoffs. Perceived group norms seem to be the key driver for female leaders to increase misreporting group payoffs.

Our study contributes to the scarce experimental evidence on gender differences in leadership behavior. The data demonstrate that women who can apply for leadership act more unethically as group leaders compared to an individual context, while men do not. Moreover, we contribute to a better understanding of the lack of female leaders. So far, there are various explanations for why women are underrepresented in leadership positions. Besides firms' discrimination in hiring (Kübler et al., 2018), historical gender-role attitudes (e.g., Alesina et al., 2013), and a lack of female role models (Beaman et al., 2012), gender differences in preferences (Azmat and Petrongolo, 2014; Croson and Gneezy, 2009) are potential explanations. Our experiment adds to this literature by analyzing the relationship between preferences for dishonesty and the decision to become a leader. Our data suggest that women's general aversion to behaving dishonestly cannot explain their hesitance to apply and take up leadership positions. Moreover, we investigate whether men and women show behavioral changes when promoted to leadership. This may help to better anticipate the impact of personnel decisions on managerial consequences. The behavioral change in women's dishonesty when they can apply for leadership suggests that a mandatory quota for women in management positions may not result in overall higher levels of ethical decision-making.

2 Study 1: Experimental Design

In this section, we describe the design of our within-subjects experiment. In the beginning, we elicit data on economic preferences in several consecutive parts. We use these preference data as pilot data for another experiment on unincentivized vs. incentivized elicitation of preferences (Grosch et al., 2023).[4] Afterward, we collect the main data for this experiment, i.e., we apply modifications of the die-rolling game introduced by Fischbacher and Föllmi-Heusi (2013) to measure dishonest behavior when misreporting individual payoffs ("individual preferences for dishonesty") and group payoffs ("preferences for dishonesty as a group leader"). For each part, subjects receive new instructions and, this way, we inform them step by step about each part. Subjects are told that at the end of the session, the computer randomly selects one of the parts for payoff. Each session ends with a questionnaire on socio-demographics.

[4]Note that we do not provide any feedback before the end of the experiment. Furthermore, these parts are identical across treatments and can, therefore, not induce any treatment differences. We report the experimental procedure of these parts in detail in Appendix B.

2.1 Individual Preferences for Dishonesty

To measure subjects' individual dishonesty preferences, we implement a modification of the method by Fischbacher and Föllmi-Heusi (2013). In this part, subjects have to report the outcome of a die-roll. To have control over individual misreporting behavior, we apply a computerized version of the die-rolling game that records the real die outcome. This approach is similar to Kocher et al. (2017). Although subjects are anonymous per design, they cannot disguise their lies and, therefore, we expect subjects to be less dishonest than in the original die-rolling game (Kajackaite and Gneezy, 2017). To demonstrate to subjects that the die is fair, they can repeatedly press a button for 20 seconds that randomly displays one side of a six-sided die whenever they press the button on the computer screen. At the end of the 20 seconds, subjects are asked to press the button one more time and to report the outcome of the actual die roll. They know that the report determines their payment in this task. The payment of each report corresponds to the reported number times three. For instance, a one yields €3, two yields €6, .., five yields €15. The only exception is the number six which yields no payment to mitigate the risk of introducing a focal point (Fischbacher and Föllmi-Heusi, 2013). This first part of our within-subjects experiment allows us to compare individual dishonesty preferences to the situation, where subjects can misreport group payoffs, explained in the following.

2.2 Preferences for Dishonesty as a Group Leader

In this part, we measure dishonest behavior when subjects decide as group leaders. For this, we play a die-rolling game similar to the previous one. That is, subjects again roll a six-sided die and report the outcome. We apply the same payoff structure as in the previous part (e.g., reporting four yields a payoff of €12). The crucial difference to the previous part is that subjects learn that they have been randomly matched in groups of three and that each group member's payoff is determined by the group leader's report. The experimental instructions point out that each of the other two group members receives the same payoff as the one reported by the leader. We do not use the word "leader" in the instructions, and call the person who determines the group payoff "person A." Before subjects roll the die, they can choose whether they want to be in the role of "person A" (leader) or not. When only one person within a group states her willingness to become the leader, she will become the group leader. When more than one person says "yes," a random draw selects one of the applicants for leadership. When no one applies, the random draw selects one person among the three group members.[5] Because of the choice option, we call this treatment *"endogenous leadership."* The choice mechanism enables us to relate the subjects' individual dishonesty preferences to their willingness to act as a leader. Moreover, we can analyze whether a subject's choice to assume responsibility affects dishonesty in the leadership position.

After subjects decide whether they want to act as a leader or not, we elicit their beliefs about how many of the other group members wanted to become leaders. Subjects receive €1 for a correct guess. Next, we apply the strategy method (Selten, 1967) to measure subjects' misreporting behavior as a leader. Here, all subjects are told to roll the die once and to

[5]This case has not occurred in our data.

simultaneously enter the payoff they want to report should they become person A (leader).[6] They know that this decision only becomes relevant if they are selected as the leader. That procedure allows us to compare the dishonesty preferences of all subjects, independent of whether they want to become leaders or not. Subjects are told the anonymous id (subject 1, 2, or 3) of the selected leader and the report made by this subject at the end of the experiment. However, they are not informed about this subject's real die roll.

Our experimental design models the selection into leadership and the hierarchical decisions of leaders. That is, subjects decide to become leaders and afterward they are solely responsible for the payoffs of the group, which they report in a non-strategic situation. In contrast, the decisions in Kocher et al. (2017) and Lohse and Simon (2021) are not hierarchical and focus on strategic contexts to analyze dishonest decisions in groups. In Kocher et al. (2017) group members have to achieve payoff commonality, i.e., subjects only earn a positive amount if they report the same die outcome as the other group members.[7] Whereas, in Lohse and Simon (2021) coordinating on the same reported own payoffs guarantees that a dishonest report of subjects' own payoff is feasible. Another difference compared to these studies is that we analyze subjects' decision and the consequences of selection into leadership. In this respect, our leaders decide independently on behalf of others, whereas subjects in Kocher et al. (2017) and Lohse and Simon (2021) meet in a chat before they make their reporting decisions. We deliberately refrain from a strategic group context to avoid confounds in answering our research question on leaders' decision to misreport payoffs on behalf of their group members.

2.3 Procedure

The experiment was conducted at a German university, and it was programmed with the software z-Tree (Fischbacher, 2007). Subjects were recruited with the subject-pool software ORSEE (Greiner, 2015). In total, 282 subjects participated (144 in the main treatment; 138 in the control treatment[8]). After subjects made their decisions, we ran a questionnaire to verbally elicit their preferences. This is part of a pilot study for a project on the relationship between non-incentivized and incentivized elicitation of preferences (Grosch et al., 2023). To control for order effects, we conducted some sessions with the questionnaire at the beginning. At the very end of the experiment, we asked for the subjects' socio-demographics. Participants were from various disciplines with a mean age of 23.60. In our sample, 50.4% of the subjects are women. Sessions lasted approximately 70 minutes. We paid subjects in cash at the end of the experiment, and earnings were on average €10.81, including a show-up fee of €5. In the following, we start reporting the hypotheses and results of our main study (Study 1).

[6] This approach is different from experiments with sequential designs where leaders move first, and other subjects may imitate their behavior to analyze the role of "leading-by-example" (e.g., Amore et al., 2022; Gächter et al., 2012; Güth et al., 2007).

[7] In a control treatment, Kocher et al. (2017) also analyze simultaneous group decisions of reporting individual payoffs when subjects do not receive an incentive for reporting similar outcomes.

[8] We describe the details of the control treatment in section 4.2.2.

3 Study 1: Hypotheses

In this study, we focus on misreporting behavior where no other party can be betrayed or deceived. Therefore, we refrain from the term "lying" and use the term "dishonest behavior."

The experimental literature on gender differences in individual dishonest behavior finds predominantly that men behave more dishonestly than women for selfish black lies, i.e., when being dishonest benefits oneself and harms another person/a third party in the lab (Conrads et al., 2014; Dreber and Johannesson, 2008; Houser et al., 2012, 2016; Grosch and Rau, 2017), in face-to-face interactions (Lohse and Qari, 2021), and in the field (Azar et al., 2013; Bucciol et al., 2013). This derives our first hypothesis on individual reporting behavior.

Hypothesis 1:
Men are more often dishonest than women when reporting individual payoffs.

Misreporting as a group leader generates a benefit for the group members and can, therefore, be seen as a Pareto improvement over telling the truth. Thus, subjects may receive an extra utility from being dishonest as a leader than when reporting individual payoffs. In line with that, Gino et al. (2013) demonstrate that the more other people benefit from misreporting, the more people are willing to be dishonest. Hence, we expect that subjects are more likely to misreport group payoffs compared to individual payoffs.

Moreover, we expect gender differences in dishonesty of leaders, based on the following reasoning. Compared to men, women are expected to demonstrate higher prosociality at work (Brañas-Garza et al., 2018) and have been found, on average, to be more prosocial (e.g., Andreoni and Vesterlund, 2001; Croson and Gneezy, 2009; Eckel and Grossman, 1998; Rand et al., 2016). The leader's decision to be dishonest for the group could be perceived as a prosocial act. Since women's level of prosociality is more pronounced than men's, the increase in misreporting between the individual and the group context may be stronger for women than for men.

Hypothesis 2:
(a) Subjects are more often dishonest when they report group payoffs compared to individual payoffs.
(b) Women switch to dishonest behavior when misreporting group payoffs compared to individual payoffs more often than men.

People's attitudes towards dishonesty may be vital for applying for leadership when leadership may demand to behave unethically. Other studies have shown that people chose leadership positions based on individual characteristics that resonate with the characteristics of the decision environment, e.g., risk preferences, overconfidence, competitive preferences, preferences for free-riding behavior (e.g., Alan et al., 2020; Barber and Odean, 2001; Cappelen et al., 2016; Eckel and Grossman, 2002; Ertac and Gurdal, 2012; Niederle et al., 2013; Niederle and Vesterlund, 2007; Reuben et al., 2012). In our setting, we expect that individual dishonesty preferences determine subjects' decisions to become leaders because dishonesty pays off in our decision context. Therefore, subjects with an individual dishonesty preference may apply for leadership to ensure that they maximize their individual profit when deciding

to misreport group payoffs. Honest subjects, on the other hand, may not apply for leadership to enforce honest behavior in the group domain since they do not have monetary incentives to do so and they can shift responsibility to group members who applied. In contrast, dishonest subjects have strong monetary incentives for becoming a leader and misreporting group payoffs.

Hypothesis 3:
Subjects with an individual dishonesty preference are more likely to apply for leadership.

4 Results: Study 1

In this section, we present the findings of Study 1, i.e., our main results that compare subjects' misreporting behavior of individual and group payoffs. Thereafter, we report the results of Study 2, an online experiment to replicate the findings and to learn more about the underlying channels of behavioral changes when acting as leaders.

4.1 Main results

First, we focus on subjects' misreporting behavior in our main treatment, where subjects can apply for leadership (*endogenous leadership*). We categorize cases as "profitable dishonest reports" when subjects increased their payoffs by misreporting the real outcome of the die roll. In this case, the dummy variable "dishonest behavior" is one, otherwise, the variable is zero.[9] This translates into potential dishonesty for die-roll outcomes between one and four, where subjects inflated their statements by reporting higher numbers between two and five. We show the share of misreporting individual payoffs using white bars and the share of misreporting group payoffs with black bars in Figure 1. The figure conditions on men (left panel) and women (right panel). When reporting individual payoffs, our data confirm commonly found gender differences in dishonesty (e.g., Conrads et al., 2014; Grosch and Rau, 2017; Kocher et al., 2017; Jacobsen et al., 2018). That is, men (26%) are five times more frequently dishonest than women (5%) (Fisher's exact test, p=0.001), supporting Hypothesis 1.

We turn to our first research question and analyze misreporting behavior when deciding as group leaders. The gender difference in dishonesty disappears when subjects report group payoffs (Fisher's exact test, p=0.353). In the group domain, women significantly increase dishonest behavior by more than four times from 5% to 24% (Wilcoxon matched-pairs test, p<0.001). In contrast, men demonstrate similar dishonest behavior in both contexts (individual payoffs: 26%; group payoffs: 32%) (Wilcoxon matched-pairs test, p=0.346). Thus, we find support for Hypothesis 2b. In general, we find that subjects behave more dishonestly when reporting group payoffs (28%) than individual payoffs (15%) (Wilcoxon matched-pairs test, p=0.002). This is in line with the results from Kocher et al. (2017) and Lohse and Simon (2021) who analyze group decisions in a strategical setting with pre-play communi-

[9]The dummy variable is set to "0" when subjects reported the real outcome, or when they reported an outcome that was to their disadvantage. However, we did not observe the latter case in Study 1.

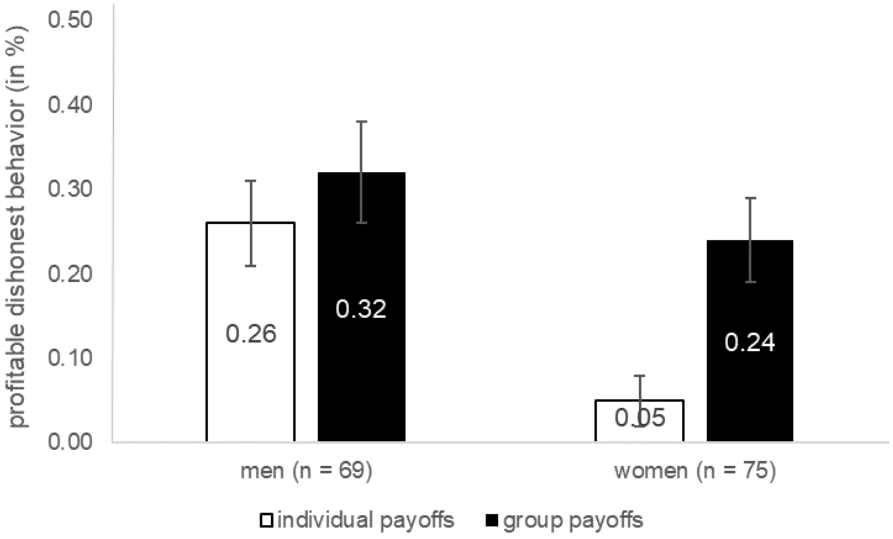

Figure 1: Percentage of profitable dishonest reports in *endogenous leadership*. White (black) bars present misreporting for individual payoffs (group payoffs). Standard error bars included.

cation and without leaders.[10] This supports Hypothesis 2a. We summarize our results as follows.

Result 1: Dishonest Behavior for Individual and Group Payoffs
(a) Women behave less dishonestly than men when reporting individual payoffs.
(b) Only women significantly increase dishonest behavior when deciding about group payoffs. Consequently, women behave as dishonestly as men when acting as leaders.

4.2 Potential drivers of the main result

To better understand leaders' motivation to behave dishonestly when deciding about group payoffs, we focus on potential drivers that affect dishonest behavior in *endogenous leadership*. We start with regression analyses to investigate the impact of individual characteristics and individual motives on misreporting group payoffs.

4.2.1 The impact of individual determinants

Table 1 presents probit regressions on subjects' likelihood to misreport group payoffs. Precisely, the regressions focus on the influence of subjects' gender (*female*), and their individual preferences for dishonesty, captured by a dummy (*misreported ind. payoffs)* which is one (zero) when subjects misreported (truthfully reported) individual payoffs.

[10]Castillo et al. (2022) replicate Kocher et al. (2017) and show that groups are not more dishonest than individuals when a local charity is hurt by subjects' dishonesty.

Table 1: Probit regressions on misreporting group payoffs (Study 1: *endogenous leadership*).

	misreporting group payoffs					
	all		*female*		*male*	
	(1)	(2)	(3)	(4)	(5)	(6)
misreported ind. payoffs	0.351***	0.322***	0.429**	0.485**	0.338***	0.234**
	(0.083)	(0.085)	(0.083)	(0.190)	(0.187)	(0.100)
female	0.009	0.022				
	(0.074)	(0.074)				
(perceived) chance of becoming a leader		0.252		0.238		0.302
		(0.194)		(0.268)		(0.277)
controls[a]	no	yes	no	yes	no	yes
obs.	144	144	75	75	69	69
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Note: The regressions report average marginal effects.
[a] Controls: age, whether subjects study economics, and an order dummy.

Moreover, we include subjects' perceived chance of becoming a leader (i.e., their guess on the number of how many other subjects 0-2 want to become a leader). This is captured by the variable *(perceived) chance of becoming a leader* in models (2), (4), and (6). Models (1)–(2) present the results from all subjects. Contrary to that, models (3)–(4) present the results from female subjects only and the last two models present the results from male subjects only. That way, we can see, whether the findings in our data are gender-specific to learn about the causes for women to increase their dishonest behavior in the group domain.[11] Finally, we include subjects' age in years (*age*), a dummy whether they study economics (*econ*), and an order dummy that controls in models (2), (4), (6) for the timing of the verbal elicitation of preferences (beginning vs. end of the sessions). All regressions report marginal effects with standard errors in parentheses. We report regressions with standard coefficients in Table 4 of the Appendix. All models highlight that subjects' dishonesty preferences positively correlate with their dishonest behavior as leaders. Precisely, the highly significant positive coefficient of *misreported ind. payoffs* shows that subjects who misreported individual payoffs are also more likely to misreport group payoffs. Moreover, the models do not indicate that this result is gender-specific. Results are robust to the inclusion of controls (models (2), (4), and (6)). Furthermore, subjects' perceived chance of becoming a leader is always insignificant. Lastly, models (1) and (2) confirm that women and men equally misreport group payoffs.

[11] An obvious solution to test for a potential gender-specific effect of individual dishonesty preferences or their perceived chance of becoming a leader on subjects' propensity to behave dishonestly as a leader would be to include the interaction of *female* and *misreported ind. payoffs* or of *female* and *(perceived) chance of becoming a leader*. However, including interaction terms in probit models is problematic (see Ai and Norton, 2003). We test the robustness of all our probit results in OLS regressions, and we also test for the respective interaction terms in OLS regressions. The results from the OLS regressions do not deviate from the results reported in the paper.

4.2.2 The role of subjects' decision to become a leader

Our analysis of participants' reporting decisions as group leaders shows a strong correlation with their individual dishonesty preferences. The decision-maker faces two changes in the group domain compared to the individual domain. First, they can apply for leadership, second, their reporting decision affects other persons' payoffs. To isolate the effect of the payoff externalities on subjects' misreporting group payoffs, we run a control treatment called *"exogenous leadership."* Compared to the *endogenous treatment* (that may resemble a job posting), in the *exogenous leadership* treatment, employees do not apply for the leadership position, but they are exogenously appointed to it (e.g., by a third party). The treatments may also offer valuable insights from a managerial perspective, as they allow us to shed light on the implications of different appointment procedures (see e.g., Bohnet et al., 2016; Murciano-Goroff, 2022).

While we keep the sequence of actions similar to the main treatment, we disable the leadership choice and a random draw determines leadership in this control treatment. To account for the possibility that subjects in our main treatment may hold different beliefs on the likelihood of ending up as a leader, we apply different probabilities of becoming a leader in the *exogenous treatment*. The probabilities vary between one-third, i.e., we tell all three group members that their probability of becoming a leader is one-third, and one-half, i.e., we tell one group member that she cannot become a leader for sure, while the other two group members are told that the probability of becoming a leader is one half.[12]

The data show that the probability (1/3 vs. 1/2) does not significantly influence the fraction of misreporting group payoffs (Fisher's exact test, p=0.323) and does not increase misreporting from individual to group payoffs (a dummy, which is positive when subjects misreported group but not individual payoffs) (Fisher's exact test, p=0.439).[13] We also run two Probit regressions on misreporting group payoffs and the increase in misreporting from individual to group payoffs, which confirm the non-parametric test results. The regressions show that the probability of becoming a leader in the exogenous leadership treatment does not affect the probability for misreporting group payoffs (p=0.757) and the increase in misreporting from individual to group payoffs (p=0.339).[14] Thus, we merge these data.

Figure 2 presents the share of misreports for individual and group payoffs when leadership is exogenously determined. As expected, we confirm that men behave significantly more often dishonestly (36%) than women (12%) (Fisher's exact test, p=0.003). Focusing on group payoffs, we find a moderate but insignificant increase for men (from 36% to 46%) (Wilcoxon matched-pairs test, p=0.180). In contrast to the *endogenous treatment*, women show a less

[12] We do not have groups in which only one group member becomes the leader for sure as this is a very rare case in the endogenous treatment. It only occurred in one of 144 cases where a subject applied for leadership and at the same time believed that no other group member would.

[13] In the *endogenous treatment*, we run a similar analysis and find that subjects' beliefs of becoming a leader do not significantly affect misreporting group payoffs (Fisher's exact test, p=0.256) and the increase in misreporting from individual to group payoffs (Fisher's exact test, p=0.527). In the endogenous treatment, the perceived chances to become a leader depends on the decision-maker's willingness to become a leader and on her belief about the other group members' willingness. Thus, the perceived chance to become a leader takes on values in $\{0, \frac{1}{3}, \frac{1}{2}, 1\}$.

[14] In the two regressions, we include a dummy controlling for the two probabilities of ending up as a leader. We also include a gender dummy and the same controls as in Table 1.

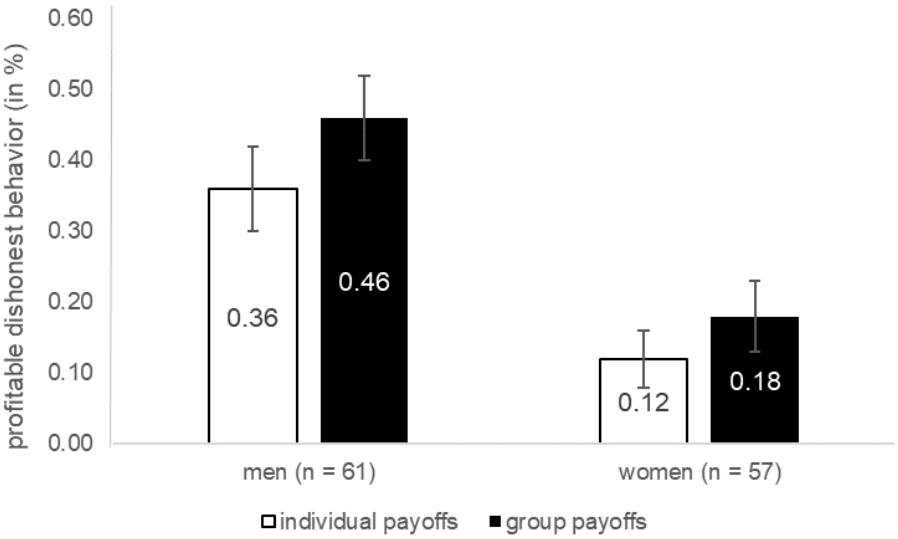

Figure 2: Percentage of misreports under *exogenous leadership*. White (black) bars present the reports for individual payoffs (group payoffs). Standard error bars included.

pronounced and insignificant increase of dishonest behavior from the individual (12%) to the group domain (18%) (Wilcoxon matched-pairs test, p=0.257). Hence, the gender difference in leaders' dishonest behavior remains when leaders are exogenously determined (Fisher's exact test, p=0.001).

To sum up, our control treatment highlights that women's increase in dishonesty as group leaders vanishes when they cannot apply for leadership. We do not find such an effect for men. The finding suggests that the driver for Result 1b is the opportunity to apply for leadership positions.

Result 2: Dishonest Behavior as Leaders under Exogenous Leadership
Under exogenous leadership, women show no increase in dishonest behavior. Consequently, the gender difference in individual dishonesty preferences remains when deciding as leaders.

4.2.3 Determinants of subjects' willingness to become a leader

In the following, we test Hypothesis 3, which expects that subjects with individual dishonesty preferences are more likely to apply for leadership. Motivated by our previous results, we also want to explain why women compared to men misreport group payoffs more often than individual payoffs. Therefore, we investigate the willingness to become a leader for men and women separately.

Figure 3 gives an overview of women's and men's share of applying for the leadership role. The diagram conditions on subjects' individual dishonesty preferences, i.e., their reporting decision when deciding for themselves. It can be seen that the willingness to become a leader is higher by 19 percentage points for men with individual dishonesty preferences, as compared to men who did not misreport their individual payoff. We find that this difference is weakly

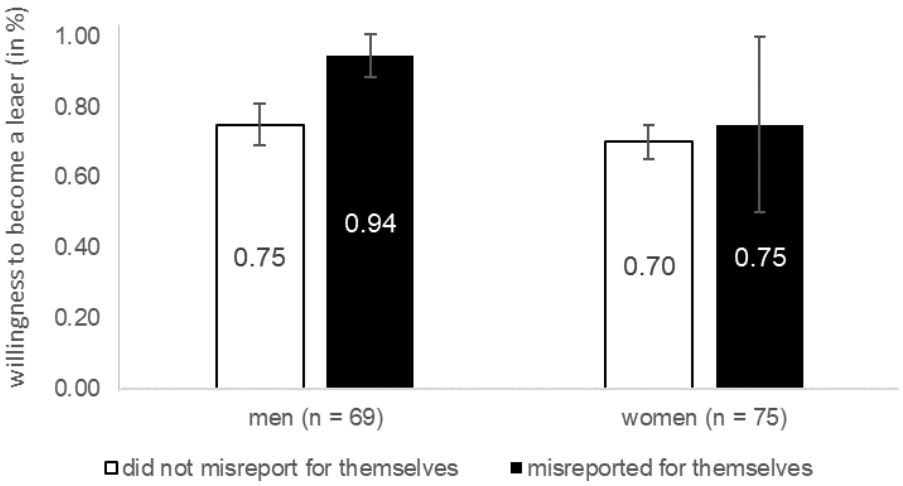

Figure 3: Percentage of subjects who want to become a leader, conditional on gender in *endogenous leadership*. White (black) bars present subjects who reported truthfully for themselves (misreported). Standard error bars included.

statistically significant (Fisher's exact test: p=0.094). A conspicuous finding is that for women, individual dishonesty preferences do not determine their willingness to become a leader (Fisher's exact test: p=1.000). In sum, we find that dishonest men tend to be more willing to become a leader. By contrast, individual dishonesty preferences do not matter for women. Thus, we only find support for Hypothesis 3 when focusing on men.[15]

5 Study 2: Channels of Changes in Dishonest Behavior

In Study 1, we found that under endogenous leadership, women increase their dishonest behavior as leaders. Moreover, our control treatment emphasizes that women only increase dishonest behavior when they can apply for leadership. Although this highlights the importance of the endogenous leadership choice, we know little about the behavioral channels for the behavioral change of women.[16] Therefore, we pre-registered and conducted a follow-up study (Study 2).[17] Based on the findings in Study 1, we pre-registered the hypotheses that men behave more dishonestly than women when deciding for themselves (H1) and that under

[15]Generally, there is no statistically significant difference between men and women in the willingness to apply for leadership (Fisher's exact test: p=0.250).

[16]To prepare Study 2, we conducted a pilot lab experiment in a similar within-subjects setting as in the endogenous leadership treatment of Study 1. Afterward, we elicit leaders' beliefs on the dishonest behavior of a randomly selected team member in the individual domain. A disadvantage of this approach is that we have to apply the strategy method and that we have to compute mean beliefs of the guesses, as the die task may result in six different outcomes. The results of the pilot study suggest that women who wanted to become leaders increase dishonesty for groups when holding an above-median belief on the reported die number of their team members in the individual domain. Based on the findings in the pilot, we designed Study 2 to improve the analysis of dishonesty beliefs. Therefore, we conducted a well-powered pre-registered online experiment, applying a simpler dishonesty task with an easier belief elicitation (see above).

[17]The pre-registration can be found here: `https://aspredicted.org/gm9v3.pdf`

endogenous leadership women increase dishonesty from the individual to the group domain more strongly than men (H2). In Study 1, we also found indicative evidence that women who wanted to become a leader more often switched from individual honest preferences to dishonest behavior for groups (23%) than women who did not want to become a leader (14%). We did not find such an effect for men. Thus, we pre-registered a third hypothesis (H3), which expects that women who assume leadership show a stronger increase in misreporting from individual to group payoffs than women who do not apply for leadership.

Study 2 aims at two goals. First, it attempts to replicate our findings of Study 1 by using a different subject pool in an online experiment. The replication of the findings is necessary to compare the results to Study 1. Moreover, the online subject pool helps us to test the robustness and to increase generalizability. Second, we add additional measures to learn more about the individual motives and the underlying channels of women's behavioral change when acting as leaders. Given the payoff externalities of leaders' reporting decisions on group members' payoffs, the leaders' dishonest behavior might resonate with their prosociality. Related to this, leaders might not just care about payoff consequences for their group members but also about making a reporting decision that reflects their group members' dishonesty preferences, i.e., taking an action that aligns with the group members' individual dishonesty preferences. To analyze these two channels of women's behavioral change, we added two additional measures. First, as a measure of subjects' prosociality, we elicit their social value orientation. Second, to control for a belief-driven behavioral change in the leadership role, we measure subjects' beliefs about their group members' individual dishonesty preferences.

5.1 Experimental design

The experimental setup is almost identical to the first study. The main difference is the use of a different dishonesty measure than in Study 1. In Study 2, we use the dots task (Gino et al., 2010), in which we ask participants to report on which half of a quadratic area ("left" or "right") they see more dots. Reporting "right" corresponds to misreporting and leads to a higher payoff than reporting "left" which is a truthful report.

The study comprises four parts, and one of them is randomly determined to be payoff-relevant. In part one, we elicit subjects' social value orientation (SVO) with the slider measure introduced by Murphy et al. (2011). Here, subjects are repeatedly confronted with two possible payoff allocations between them and another subject. In each decision set, the allocations vary the payoff differences and subjects have to trade off different money allocations. Based on their choices, we calculate an SVO angle for each subject (see Appendix B for instructions as well as for a screenshot of one of the allocation decisions; for the angle's calculation see Murphy et al., 2011). In part two, we measure dishonest behavior and subjects reported individual payoffs. A truthful report leads to a payoff of £0.20, and a dishonest report to a payoff of £2.00. Part three is similar to the group-dishonesty measure in the former experiment, except for the different dishonesty game. A truthful report leads to a payoff of £0.20 for each group member, and a dishonest report £2.00 for each group member. Thereafter, in part four, we elicit subjects' beliefs on the group members' individual dishonesty preferences in an incentivized way. They are asked about their belief of how many

other group members indicated "right" in part two of the experiment. A correct guess yields a payoff of £2.00. Finally, we asked several survey questions to gather additional evidence on women's and men's motivation to act as a leader. We asked them if they wanted to influence payoffs and/or have the power of decision-making.[18]

We recruited 156 subjects (76 male and 80 female) using Prolific (Palan and Schitter, 2018) and surveyed them using Qualtrics. The sample is limited to UK citizens with a high school degree or a higher education. The average participant was 32.57 years old. The average time spent on the experiment was 6.81 minutes. Participants earned £1.89 on average, including a show-up fee of £1.00.

5.2 Replication of the results

We start with our results on gender differences in dishonest behavior. We condition dishonest behavior on men (left panel) and women (right panel). Figure 4 presents women's and men's percentages of dishonest reports when misreporting individual payoffs (black bars) and group payoffs (white bars) in Study 2.

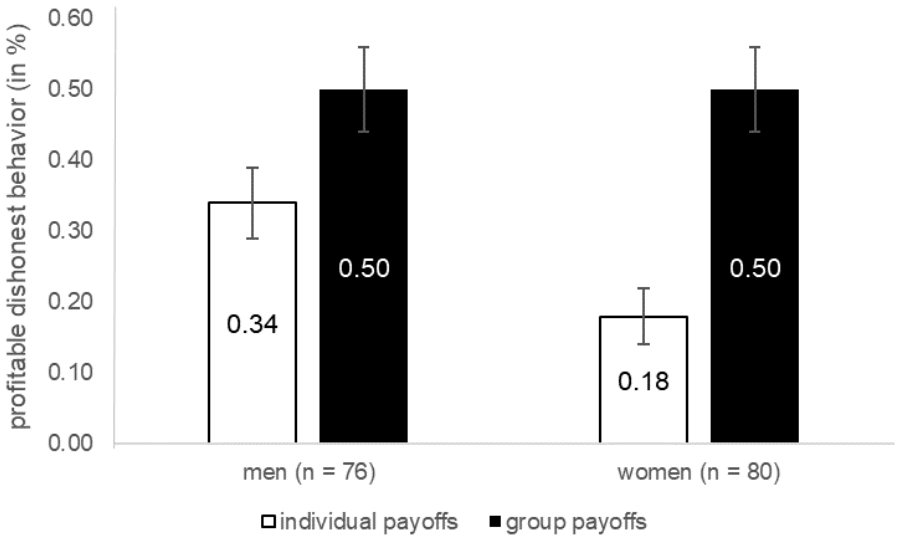

Figure 4: Percentage of misreporting in Study 2. White (black) bars present misreporting individual payoffs (group payoffs). Standard error bars included.

As can be seen in Figure 4, the results in Study 2 look similar to the results in Study 1. Again, we find a gender difference in individual dishonesty preferences. Men behave significantly more dishonestly than women (Fisher's exact test: p=0.018). The frequency of men's dishonest reports is almost two times higher (34%) than women's (18%). The result supports H1, and it is in line with Study 1. By contrast, the gender difference is no longer statistically significant when subjects act as leaders (Fisher's exact test: p=1.000), as both genders misreport group payoffs in 50% of the cases. In Study 2, the increase in dishonest behavior is positive and statistically significant for both genders (Wilcoxon signed-rank test,

[18]Survey responses do not differ between men and women. Therefore, we do not report them in the paper.

men: p=0.007; women: p<0.001). Importantly, a significantly higher fraction of women switches from an individual truthful report to misreporting group payoffs as compared to men (Fisher's exact test: p=0.075).[19] This supports H2 and the findings of Study 1, which suggest that in *endogenous leadership* particularly women increase dishonest behavior when deciding as leaders. A closer look shows that the percentage of women who switch from honest to dishonest behavior is similar for women who apply for leadership (36%) and for women who do not apply for leadership (32%) (Fisher's exact test: p=0.797). Thus, we do not find support for H3.

Following the structure in Study 1, we now analyze the behavioral drivers behind leaders' motivations to report dishonestly. In Table 2, we run similar regressions as in Study 1 on subjects' likelihood to misreport group payoffs, as in Table 1.

Table 2: Probit regressions on misreporting group payoffs (Study 2).

	misreporting group payoffs					
	all		*female*		*male*	
	(1)	(2)	(3)	(4)	(5)	(6)
misreported ind. payoffs	0.477***	0.501***	0.457***	0.449***	0.471***	0.559***
	(0.069)	(0.071)	(0.136)	(0.137)	(0.064)	(0.066)
female	0.080	0.076				
	(0.074)	(0.074)				
(perceived) chance of becoming a leader		0.094		0.292		-0.137
		(0.198)		(0.286)		(0.271)
controls[a]	no	yes	no	yes	no	yes
obs.	156	156	80	80	76	76
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Note: The regressions report average marginal effects.
[a] Controls: age, and whether subjects hold a university degree.

Again, we control for the impact of individual dishonesty preferences, gender, and subjects' beliefs on the number of other subjects who want to become a leader. We include similar controls as in Study 1, report marginal effects (we report regressions with standard coefficients in Table 5 in the Appendix), and present standard errors in parentheses.[20]

The results highlight that only individual dishonesty preferences, captured by the highly significant positive coefficient, *misreported ind. payoffs*, explain whether subjects misreport group payoffs. All other variables are insignificant. This supports the idea to focus on further analyses on the belief about other group members' dishonesty preferences. In sum, we replicate the findings of Table 1 (Study 1).

In the next step, we focus on the subjects' willingness to become a leader. The results are generally in line with our previous results. We find a tendency that people with individual

[19] The variable *switch* is coded as one if a person is honest when deciding for herself and dishonest when deciding as a leader. Otherwise, the variable is coded as zero.
[20] The results are also robust for OLS regressions with and without included interaction terms.

dishonesty preferences are more likely to apply for leadership. More precisely, the percentage of men who want to become leaders is higher by 12 percentage points (88% vs. 76%) when they show an individual preference for dishonesty. We find that this difference is less pronounced for women (79% vs. 71%). We do not find that these differences are significant in this Study 2 (Fisher exact tests, men: p=0.238; women: p=0.747).[21] In summary, we can replicate most of the results of Study 1. Importantly, in Study 2 we again observe that women switch from individual truthful reporting to dishonest behavior as leaders more often than men. As a consequence, the gender gap in misreporting behavior closes in the group domain, similar to Study 1.

5.3 Potential Drivers of Switching Behavior

In this section, we look deeper into the potential drivers of the main result. First, we analyze whether our two additional measures are different across genders. Potential differences may help to explain why the observed increase in dishonesty is particularly strong among women. In line with the literature (e.g., Grosch and Rau, 2017), we find women to be more prosocial according to the social value orientation measure as compared to men (Mann-Whitney test: p=0.059). We find no difference in average beliefs (Mann-Whitney test: p=0.777). Next, we turn to the relevance of the two potential channels for leaders' reporting decisions.

As we find that both genders increase their dishonest behavior significantly as leaders, we now focus on subjects' decisions to switch from an individual truthful report to misreporting group payoffs. We compare the impact of individual beliefs on group members' dishonesty preferences and individual social value orientation (SVO) on subjects' decisions to switch.

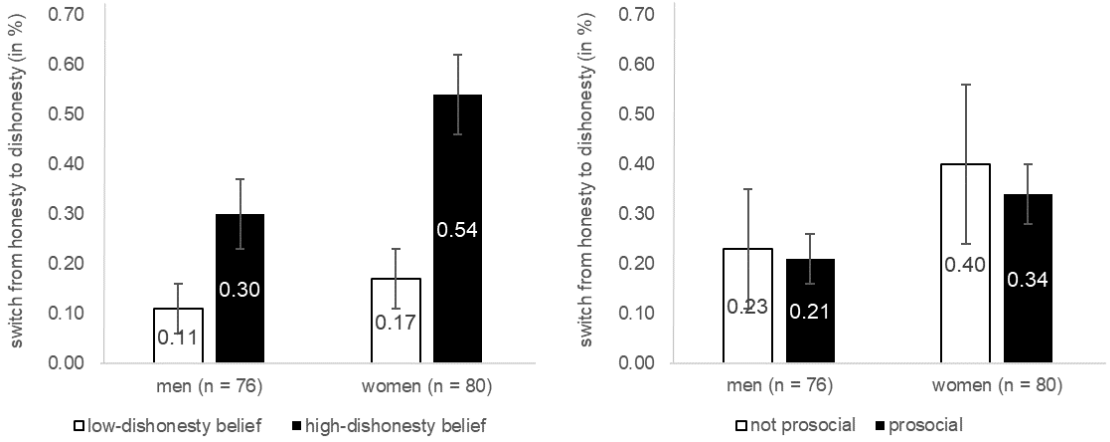

Figure 5: Percentage change from an individual truthful report to misreporting group payoffs conditional on gender and channel (left panel: beliefs, right panel: prosocial behavior) in Study 2. White (black) bars present the reports for low levels (high levels). Standard error bars included.

Figure 5 gives an overview of the percentage change from reporting truthful individual payoffs to misreporting group payoffs. We condition on gender and the potential channel (left

[21] Exactly like in Study 1 we do not find gender differences in the willingness to become a leader (Fisher's exact test: p=0.266).

panel: beliefs, right panel: prosocial behavior). We define that subjects hold a "high-dishonesty belief" ("low-dishonesty belief") if they believed that the strict majority, i.e., all (less than two) other group members behaved dishonestly at the individual stage. The variable "prosocial" is defined as 1 (0) if a person can (cannot) be categorized as prosocial according to our social value orientation measure.

Figure 5 highlights that men and women with a high-dishonesty belief switch more often than men and women with a low-dishonesty belief. Importantly, the effect size, as well as the statistical significance of this difference, is larger for women (54% vs. 17%) than for men (30% vs. 11%) (Fisher's exact test, women: p=0.001; men: p=0.053). Thus, we find a statistically significant gender difference in the group that holds a high-dishonesty belief, i.e., women are significantly more likely to switch than men (Fisher's exact test, p=0.041). The gender difference disappears among leaders with a low-dishonesty belief (Fisher's exact test, p=0.528). By contrast, the right panel of the diagram demonstrates that being prosocial does not affect the switching behavior of either gender (Fisher's exact tests, men: p=1.000; women: p=0.734). These results indicate that leaders' beliefs but not their prosociality predict their behavioral change. Moreover, beliefs matter more for women's dishonest behavior as leaders than for men's. We confirm these results using probit regression analyses in Table 3.

Table 3: Probit regressions on subjects' likelihood to switch from an individual truthful report to misreporting group payoffs (Study 2)

	switch from honest to dishonest behavior					
	all		*female*		*male*	
	(1)	(2)	(3)	(4)	(5)	(6)
dishonesty belief	0.221***	0.222***	0.301***	0.318***	0.139**	0.141**
	(0.049)	(0.049)	(0.064)	(0.066)	(0.069)	(0.061)
social value orientation	0.000	0.000	-0.002	-0.002	0.002	0.000
	(0.004)	(0.004)	(0.006)	(0.006)	(0.005)	(0.004)
female	0.138**	0.129*				
	(0.066)	(0.066)				
controls[a]	no	yes	no	yes	no	yes
obs.	156	156	80	80	76	76
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Note: The regressions report average marginal effects.
[a] Controls: age, and whether subjects hold a university degree.

In the regressions, we include our two main variables of interest, i.e., subjects' belief about the individual dishonesty preferences of the other group members (*dishonesty belief*) and the measure of their *social value orientation*. Models (1) and (2) additionally include a gender dummy (*female*), whereas models (3)–(4) and (5)–(6) estimate models (1) and (2) separately

for women and men. Regressions (2), (4), and (6) include subjects' age and whether they have a university degree as control variables. The regressions report marginal effects (we report regressions with standard coefficients in Table 6 of the Appendix).[22]

The analyses show that leaders with a higher dishonesty belief about their group members' dishonesty preferences are more likely to switch, whereas subjects' social value orientation does not predict switching behavior. Models (3)–(6) show that these findings hold for both genders. The subsample regressions show that the coefficient of dishonesty beliefs is more than twice as large for women than for men ((3)–(4) vs. (5)–(6)). This suggests that dishonesty beliefs matter more for women than for men, confirming previous results. We estimate a linear probability model which includes an interaction between *female* and *dishonesty belief*. The coefficient of the interaction is positive and significant (p=0.078). Finally, in line with our previous results, models (1)–(2) confirm that women are generally more likely than men to increase dishonesty from the individual to the group domain.

Taken together, we established that irrespective of gender, leaders' beliefs about other group members' dishonesty preferences but not their prosociality predicts their behavioral change. Moreover, the belief channel seems more pronounced for women, which partially explains women's stronger behavioral change. Finally, despite the observed gender difference in prosociality, this difference cannot explain the more pronounced behavioral change among women.

Result 3: Channels for switching from honesty to dishonesty
(a) The likelihood that women (and men) switch from an individual truthful report to misreporting group payoffs is highly positively correlated with their belief about their group members' individual dishonesty preferences. Moreover, this relation is more pronounced for women.
(b) Leader's prosociality does not affect switching behavior.

Reviewing the results from both studies, we can conclude that subjects are especially likely to switch from individual truthful reporting to dishonest reporting as leaders in environments where they can apply for leadership. This effect is more pronounced for women than for men. While dishonest behavior is different for men and women in the individual domain, the gender gap closes in the leadership domain, as women are more likely to be dishonest as leaders compared to the individual domain. Our Study 2 replicates these findings for another (online) subject pool. It demonstrates that subjects' switch is driven by the belief about group members' individual dishonesty preferences, which is particularly pronounced for women. A behavioral change from honest to dishonest reporting is most likely for subjects believing that the majority in their group has individual dishonesty preferences. By contrast, the social value orientation does not seem to have any explanatory power for the behavioral change (of women) in the leadership role. Thus, we can conclude that women increase dishonesty as leaders to adjust their behavior to their perceived group norm, closing the dishonesty gender gap.

[22]The results are also robust when using OLS regressions.

6 Conclusion

In this paper, we analyze gender differences in ethical decision-making (dishonesty) of *leaders* in a lab and in an online study. Our experiments are based on within-subjects settings with two stages, where subjects first decide on their individual payoffs and subsequently report payoffs for their groups as leaders. Moreover, we model subjects' deliberate decisions to apply for leadership. We can analyze whether this decision is related to individual dishonesty preferences and whether it impacts misreporting behavior in a group context.

Our laboratory experiment (Study 1) demonstrates that men behave more dishonestly than women in the individual domain, corroborating the predominant evidence. A novel finding of this study is that women alter their behavior when they act as leaders after they could apply for leadership, whereas men act similarly in the individual and the group domain. Our control treatment disables the leader-choice option and highlights that under these conditions, women's decision to behave dishonestly does not differ across the individual and the group domain. The control treatment shows that women do not increase dishonesty *per se* as a leader. In fact, our results suggest that women leaders' ethical behavior only erodes when they have the opportunity to apply for leadership.

Our online experiment (Study 2) tests the robustness of the findings and analyzes two potential mechanisms of women's increase from individual truthful reporting to misreporting as leaders. The results disclose a channel explaining women's increase in dishonesty as leaders. That is, the opportunity to apply for leadership motivates them to adjust their behavior to the group members' dishonesty preferences. This phenomenon does not depend on the group domain *alone*. It occurs as a combination of the opportunity to apply for leadership and the perceived group norm. Individual preferences for prosociality do not explain leaders' dishonesty.

Our paper also improves the understanding of women's (and men's) motivation to apply for leadership positions. While men with an individual preference for dishonesty tend to apply more often for leadership, such a relationship does not exist for dishonest women. This result contributes to the literature studying the role of gender differences in attitudes/preferences explaining the lack of women in leadership positions, e.g., risk preferences and overconfidence (Ertac and Gurdal, 2012; Reuben et al., 2012). Our finding that women's decision to apply for leadership is not correlated with their individual dishonesty preferences highlights that the gender gap in applying for leadership positions is not associated with women's less pronounced individual dishonesty preferences. Hence, workplace policies may not need to address the differing dishonesty preferences of men and women. Interestingly, we show that having the choice to assume group responsibility motivates women to behave dishonestly as leaders despite their individual honesty preferences. This highlights the importance of promotion mechanisms, since the opportunity to apply for leadership may lead to women's behavioral change. Adams and Funk (2012) demonstrate for Sweden's top directors that women are more benevolent and care less about achievement and power than their male counterparts, consistent with character trait distribution for the general population. This suggests that women's traits are not always malleable.

However, when leadership implicitly demands a certain behavior as in our context, women might adapt their preferences. This can have a range of consequences. For instance, affirma-

tive action policies in the form of a women's quota may not result in higher ethical standards at the management level *per se*. In line with this, Larkin et al. (2013) demonstrate that a firm's likelihood to appear on the list of the "Most Ethical Companies" increases with the number of women on the board of directors, and it considerably jumps when the board comprises 33 percent or more women. Moreover, it also offers an explanation for the finding of Nekhili et al. (2022) that the boards' gender diversity negatively correlates with the number of unethical business transactions. Interestingly, this negative correlation is driven by female directors involved in the board's monitoring duties (female independent directors and members of the audit committee) and does not hold for female inside directors. This heterogeneity across tasks might result from female board members involved in monitoring duties holding more positive beliefs about the other board members' dishonesty preferences than female inside directors.

We conclude that decision-makers should keep in mind that the procedural design of the hiring process might matter for ethical leadership behavior, i.e., whether women apply for a promotion or whether the company/institution commends their promotion. Moreover, if female executives have to adapt to a leadership style that is not in line with their individual preferences, then this may result in higher perceived mental stress (Gardiner and Tiggemann, 1999). This may ultimately make women give up the leadership position or work part-time (Manning and Petrongolo, 2008) in the long term. There is scarce experimental evidence on how employees' individual behavior changes when their role transforms from employee to leader. Our study is novel in this regard, observing individual changes in ethical decision-making, and may spur further research in the future.

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Appendix A - Tables

Table 4: Probit regressions on misreporting group payoffs (Study 1: *endogenous leadership*).

	misreporting group payoffs					
	all		*female*		*male*	
	(1)	(2)	(3)	(4)	(5)	(6)
misreported ind. payoffs	1.157***	1.088***	1.477**	1.725**	1.069***	0.828**
	(0.318)	(0.327)	(0.702)	(0.754)	(0.359)	(0.392)
female	0.030	0.074				
	(0.243)	(0.250)				
(perceived) chance of becoming a leader		0.852		0.845		1.068
		(0.664)		(0.962)		(0.998)
controls[a]	no	yes	no	yes	no	yes
obs.	144	144	75	75	69	69
pseudo R2	0.088	0.113	0.060	0.090	0.106	0.200
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

[a] Controls: age, whether subjects study economics, and an order dummy.

Table 5: Probit regressions on misreporting group payoffs (Study 2).

	misreporting group payoffs					
	all		*female*		*male*	
	(1)	(2)	(3)	(4)	(5)	(6)
misreported ind. payoffs	1.402***	1.485***	1.259***	1.258***	1.488***	1.881***
	(0.276)	(0.291)	(0.443)	(0.452)	(0.351)	(0.423)
female	0.236	0.226				
	(0.219)	(0.222)				
(perceived) chance of becoming a leader		0.277		0.819		-0.462
		(0.589)		(0.816)		(0.912)
controls[a]	no	yes	no	yes	no	yes
obs.	156	156	80	80	76	76
pseudo R2	0.137	0.143	0.085	0.098	0.193	0.238
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

[a] Controls: age, and whether subjects hold a university degree.

Table 6: Probit regressions on subjects' likelihood to switch from an individual truthful report to misreporting group payoffs (Study 2)

	switch from honest to dishonest behavior					
	all		*female*		*male*	
	(1)	(2)	(3)	(4)	(5)	(6)
dishonesty belief	0.742***	0.756***	0.972***	1.043***	0.509*	0.576**
	(0.190)	(0.192)	(0.276)	(0.295)	(0.262)	(0.266)
social value orientation	0.002	0.001	-0.007	-0.007	0.009	0.001
	(0.013)	(0.013)	(0.018)	(0.019)	(0.018)	(0.018)
female	0.462**	0.439*				
	(0.229)	(0.234)				
controls[a]	no	yes	no	yes	no	yes
obs.	156	156	80	80	76	76
pseudo R2	0.118	0.132	0.154	0.163	0.054	0.154
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Note: The regressions report average marginal effects.

[a] Controls: age, and whether subjects hold a university degree.

Appendix B - Preference elicitations (Study 1)

part one - elicitation of risk preferences

In the risk elicitation task of Eckel and Grossman (2002), subjects had to choose one out of six lotteries. With a 50% probability, each lottery leads to a low or a high payoff. Subjects' lottery choice can be interpreted as a measure of their degree of risk aversion, i.e., higher lottery numbers reflect riskier lotteries

Choice	Low Payoff (€)	High Payoff (€)	Exp. payoff	Implied CRRA Range
1	5.60	5.60	5.60	$3.46<r$
2	7.20	4.80	6.00	$1.16<r<3.45$
3	8.80	4.00	6.40	$0.71<r<1.16$
4	10.40	3.20	6.80	$0.50<r<0.71$
5	12.00	2.40	7.20	$0.00<r<0.50$
6	14.00	0.40	7.20	$r<0$

Table 7: Overview of the lottery choices in part 1. Risk is measured as the standard deviation of the expected payoff.

part two - elicitation of advantageous inequality aversion

In part two, the modified dictator game (MDG) of (Blanco et al., 2011) was used to measure subjects' aversion to advantageous inequality (β in Fehr and Schmidt, 1999). In the MDG, participants are presented with a list of 22 pairs of payoff vectors. They choose one of the two payoff vectors for all 22 pairs. Both vectors represent a money split between the dictator and the recipient. The left vector is constant and always (20, 0). If the participants choose it, they receive €19 and the recipients earn €1. All vectors on the right-hand side are increasing equal-money splits: from (1, 1) to (21, 21).[23] The task aims to find out when subjects switch from (20, 0) to the equal split. The table contains 22 buttons, located above all decisions between an unequal and an equal split. Subjects know that clicking on a button has the effect that all equal splits below the button are marked for selection and all unequal splits above the button are also marked for selection. If a subject prefers all equal splits from (3, 3) to (20, 20) over the unequal split, she should click on the third button. Whereas, if a subject only prefers all equal splits starting from (9, 9) she should click on button 9. The earlier a subject switches to the equal split, the more pronounced her aversion toward advantageous inequality.

part three - elicitation of disadvantageous inequality aversion

In part three we measured subjects' aversion to disadvantageous inequality (α in Fehr and Schmidt, 1999) using the method of Blanco et al. (2011). In an ultimatum game using the strategy method (Selten, 1967) participants decide on the role of proposers and recipients. They know that after the experiment is finished, the computer will randomly pair two players and determine their role (proposer or recipient) and the payoff-relevant decision. In the

[23] Extending the right vectors to (21, 21) allows us to account for negative β's (Blanco et al., 2011).

beginning, all subjects decide as proposers. They have to decide how much of €20 they are willing to propose to the recipient. afterward, all subjects decide on the role of recipients. In this respect, they indicate which minimum proposer offer they would accept. Subjects are given a table with 22 rows of different proposals for each possible integer allocation of the €20 between the two players. They have to indicate whether they would reject or accept each individual proposal. Therefore, all proposals have to be marked for rejection or acceptance. The goal is to find out when subjects switch from rejecting an offer to accepting it. Therefore, the table contains 22 buttons which are located above each proposal. Subjects are told that clicking on a button would mean that all proposals below the button would be marked for acceptance, whereas all proposals above the button would be marked for rejection. For instance, if a subject wants to accept all proposals between 0 and 20, she has to click on the first button. If she wants to accept all proposals starting from €4, she would click on button 4. The higher the minimum acceptable offer is, the higher a subject's aversion to disadvantageous inequality.

part four, five, and six - elicitation of competitive preferences

In parts, four to six subjects participate in the mathematical real-effort task introduced by Niederle and Vesterlund (2007). Here, subjects have to add five two-digit numbers. An example of the real-effort task (a math problem to be solved) is presented in Table 8. Subjects have to enter the answer in the blank box. Having submitted an answer, subjects are presented with the next problem without being informed of whether the answer was correct or not.

75	33	12	19	25	

Table 8: Example of a problem in the real-effort task

In part four subjects work for five minutes on the real-effort task. We follow Niederle and Vesterlund (2007) and pay subjects a piece rate of €0.50 for each correctly solved problem. In part five subjects are matched in groups of four and participate in a tournament. They again spend five minutes completing the real-effort task. Here, their individual payments depend on their own performance compared to the performance of the three other participants in their group. If a subject achieves the best performance in the group, she receives €2 for each correct answer. However, if a subject does not achieve the best performance, she earns nothing. We measure subjects' competitiveness preferences in part four, as their willingness to participate in a tournament. Therefore, subjects have to decide whether they want to participate in a tournament against three other participants, or whether they want to work under a piece rate. After subjects make their choices, they were given another five minutes to complete the real-effort task. If subjects work under the tournament, their performance is compared to the performance of the other three members of the group they are assigned to in part five (Niederle and Vesterlund, 2007).

part seven - elicitation of ambiguity attitudes

In this part, subjects can earn Talers. We apply an exchange rate of 1 Taler = €0.05. To elicit individual ambiguity preferences, subjects decide in a multiple price list (MPL) design by Gneezy et al. (2015) with 20 rows. Each row involves a choice between a risky gamble in the left column (Option A) and an ambiguous gamble in the right column (Option B). Subjects are told that the payoffs of the options depend on the color which is drawn out of two urns that are filled with a certain number of red and black balls. They know that the risky urn (Option A) is exactly composed of 50 red and black balls. Whereas, they know that the composition of the ambiguous urn (Option B) is unknown. Before subjects are presented with the choice list, they have to bet on a color (red or black). They are told that they receive a high payoff if this color will be drawn in the urn draw. After subjects make their bet, they have to decide for all 20 rows of the MPL, whether they prefer the risky lottery (Option A) or the ambiguous lottery (Option B). The possible payoff of Option A is constant for all 20 rows, i.e., when choosing Option A subjects always can win 200 Taler with a probability of 50%. Whereas, the payoff of Option B is increasing when subjects go down by one row. It starts from 164 Talers (row 1) and ends at 316 Talers (row 20). Subjects receive these payoffs with a subjective probability of 50%[24] The switch point determines the subjects' ambiguity attitude. That is, subjects who switch early (late) from Option A to Option B are characterized by a lower (higher) degree of ambiguity preferences. Subjects know that if part five would become payoff relevant, a random draw would select one of the 20 rows. Subjects' choice in this row would be selected to be payoff relevant. If subjects have selected Option A, they are playing a random draw with a probability of 50%. If however, Option B was selected, then subjects play the corresponding lottery. The composition of the ambiguous urn is randomly determined by a computer.

Appendix B - Complete Instructions (Study 2)

In the following, we provide screenshots of the instructions that have been displayed to participants in Study 2. On top of the screens, one can see a label of the particular instructions. That label has not been displayed to participants in the experiment. The course of the online study was as follows:

- Subjects see the "WELCOME SCREEN"
- Subjects receive the "Instructions SVO"
- Subjects play the social value orientation (SVO): Here an "Example Screen SVO" of the first Allocation decision is displayed. The Screens for Allocations 2-6 look identical with the exception of different payoff distributions on the buttons.
- Subjects receive the "Instructions Individual Dishonesty Decision"
- Subjects play the dots game deciding for themselves
- Subjects receive the "Instructions Person A's Dishonesty Decision"
- Subjects decide whether they want to act as "Person A"

[24] Recall, that subjects bet on one of two colors.

- Subjects "Belief about other's Willingness to become Person A" is measured
- Subjects are reminded, of their own "Indication of Willingness to become Person A"
- Subjects play the dots game deciding for the group as "Person A"
- Subjects receive the "Instructions Believe about Others' Individual Dishonesty"
- Subjects indicate the above-mentioned belief
- Subjects receive a short questionnaire before they are redirected to Prolific

(WELCOME SCREEN)

Welcome to our scientific study!

Brief general information

- This experiment consists of **four parts**: Part 1, Part 2, Part 3, and Part 4.
- **In each of these parts,** you have to make decisions.
- After the experiment, you will be asked to answer a short questionnaire.
- The study is expected to take approximately 10 minutes to complete.
- Your participation is completely voluntary.
- Your data will remain confidential and will be treated anonymously.
- You must be 18 years or older to participate.

Brief information on payment

- You will receive a fixed payment of £1.00 for participating in the experiment.
- You can earn additional money **depending on your decisions and depending onthe decisions of others in Parts 1-4.**
- Note that at the end of the experiment **only one of the four parts** will be randomly chosen to be paid out.

For reasons of simplicity we use in these instructions only male notions.

Please enter your Prolific ID:

(Instructions SVO)

Part 1: Instructions

In Part 1 you are **randomly matched with another participant**. You and the other participant are asked to make some allocation decisions.

You are presented six screens. On each screen you are presented allocation decisions,where you can allocate money to yourself and to the matched participant. In each allocation decision, you are asked to allocate money using a button.

If Part 1 will be paid out the payoff is determined in the following way:

- At the end of the experiment, the computer randomly chooses **one of the six allocation decisions** to be payoff relevant.
- Further, the computer randomly chooses, **whether your decision or the decision of the other participant** for that particular allocation decision determines your and the other participant's payoff.

Please choose in the following six allocation decisions, which allocations you prefer. In each allocation decision, you can choose how much money will be distributed to you andhow much money will be distributed to another randomly chosen participant, if your decisions will become payoff relevant.

Please press the "continue" button to start making your allocation decisions.After

the completion of this part you will receive the instructions for Part 2.

(Example Screen SVO)

Part 1: Allocation 1

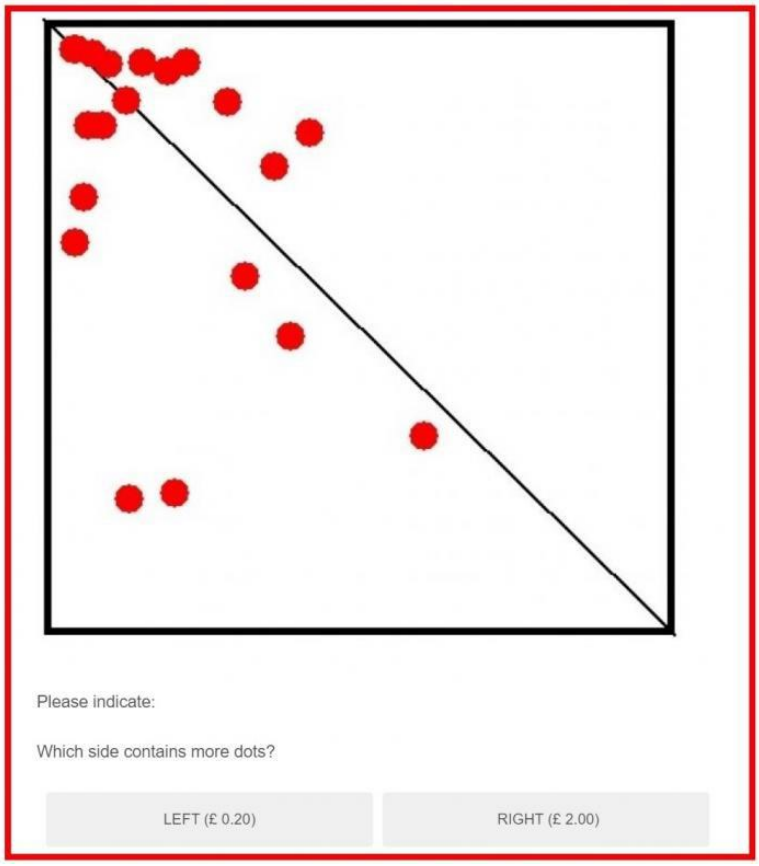
Please choose your preferred allocation.

You: **£1.28;** Other: **£1.28**	You: **£1.28;** Other: **£1.14**	You: **£1.28;** Other: **£1.02**	You: **£1.28;** Other: **£0.89**	You: **£1.28;** Other: **£0.75**	You: **£1.28;** Other: **£0.62**	You: **£1.28;** Other: **£0.50**	You: **£1.28;** Other: **£0.36**	You: **£1.28;** Other: **£0.23**

(Instructions Individual Dishonesty Decision)

Part 2: Instructions

- In Part 2, you will see a box containing multiple red dots (see example below).
- Your task is to indicate whether there are more dots on the right side of the square or on the left side of the square.
- (Note that it is possible that a dot will be on the line between the two sides.)
- If Part 2 will be paid out, your payoff is determined by your indication regarding the dots in the following way:
  - If you indicate LEFT, your payoff is £0.20.
  - If you indicate RIGHT, your payoff is £2.00.



Please press the "continue" button to make your decision.

After the completion of this part you will receive the instructions for Part 3.

(Instructions Person A's Dishonesty Decision)

Part 3: Instructions

Please read the complete instructions carefully. Scroll down if necessary.

Overview

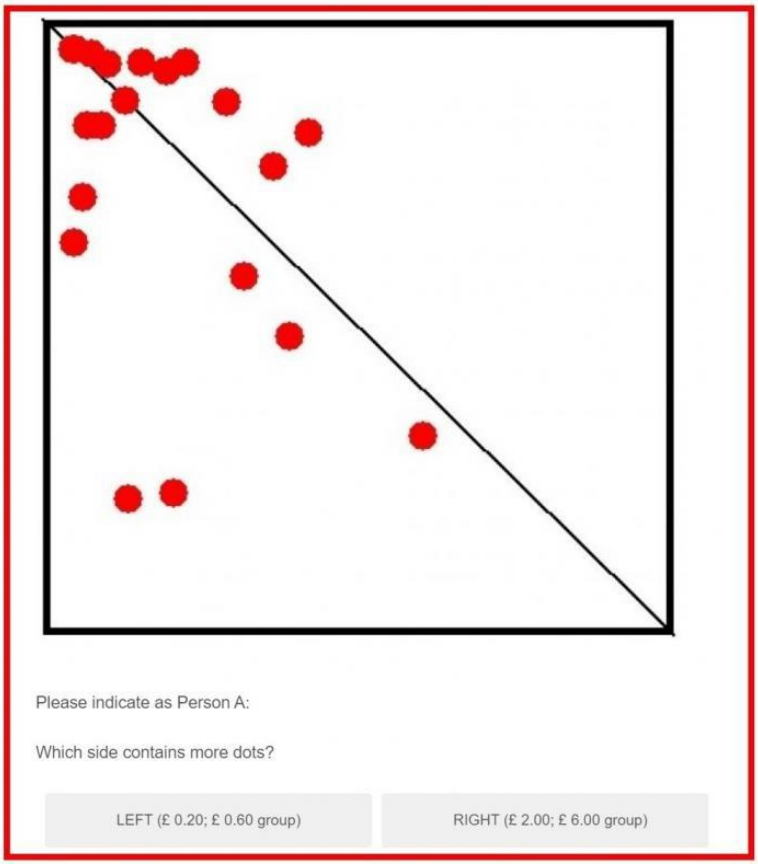
- In Part 3, you will be matched in a **group of three** people.
- In the beginning of Part 3, each group member will be asked whether he wants to actas Person A.
- You can reply to this question with "yes" or "no."
- **Only at the end** of the experiment, **one of the three** group members **will eventually become Person A**, while the remaining group members will become Persons B.
- After all group members have responded to the above-mentioned question (either by"yes" or by "no"), **all** of them have to act **as Person A** without knowing, whether theywill eventually become Person A.
- **Note that the decision of the one group member, who will eventually become Person A determines his payoff as well as the payoff of the other two group members (Persons B).**

How is Person A determined?

- Your and the other two group members' response to the question, whether you want to act as Person A determines who will eventually become Person A.
    - If all three group members said "yes" or if all three group members said "no" a random draw decides who of the three group members will become Person A.
    - If two group members said "yes" a random draw decides who of these two group members will become Person A.
    - If one group member said "yes" that group member will automatically become Person A.
- All group members, who do not become Person A, automatically become Persons B.

Acting as Person A

- You will see a **new box** containing multiple red dots (see example below) and without knowing, whether you will become Person A you have to **make an indication as Person A**.
- (Note that it is possible that a dot will be on the line between the two sides.)
- Your task is to indicate whether there are more dots on the right side of the square oron the left side of the square.
- If Part 3 will be paid out and if at the end of the experiment you will become Person A**your payoff (as Person A) and the payoff of the two group members** being Persons B is determined by your indication regarding the dots in the following way:
    - If you indicate LEFT, your payoff as Person A is £0.20. The payoff of each Person B is £0.20. Hence, the total payoff (group) is £0.60.
    - If you indicate RIGHT, your payoff as Person A is £2.00. The payoff of each Person B is £2.00. Hence, the total payoff (group) is £ 6.00.



Please press the "continue" button to make your decisions.

After the completion of this part you will receive the instructions for Part 4.

(Measure of Belief about other's Willingness to become Person A – before deciding as Person A but after own Indication of the Willingness)

Part 3: Additional question

How many of your two other group members do you think stated that they want to act as **Person A**? If your guess is correct, you will receive **£0.30 extra**, if Part 3 will be paidout.

0 group members	1 group member	2 group members

(Reminder of own Indication of Willingness to become Person A – both Variants printed here)

Part 3: Reminder

You have responded to the question, whether you want to act as Person A by **"no"**.

- If **at least one** of your group members said "yes", the computer will randomly selectone of the group members who said "yes" to become Person A.
- If **no** of your group members said "yes", the computer will randomly select one person from **all** three group members to become Person A.

You have responded to the question, whether you want to act as Person A by **"yes".**

- If **at least one** of your group members said "yes", the computer will randomly selecteither you or one of the other group members who said "yes" to become Person A.
- Otherwise the computer will select you directly to become Person A.

(Instructions Believe about Others' Individual Dishonesty)

Part 4: Instructions

Short reminder of the Parts 1-3

- In Part 1 you were asked to make six allocation decisions between you and a randomly matched participant.
  - If Part 1 will be paid out and your decision will be randomly chosen to be payoff relevant, it determines your payoff and the payoff of the matched participant.
- **In Part 2 you were asked to indicate whether there were more dots on the rightside of a square or on the left side of a square.**
  - **If Part 2 will be paid out that indication determines your payoff.**
- In Part 3 you were asked to say, whether you wanted to act as Person A and to indicate as Person A whether there were more dots on the right side of another square or on the left side of that square.
  - If Part 3 will be paid out and if you will become Person A that indication determines your payoff (as Person A) and the payoff of your two group members (Persons B).

Your task in Part 4

- In Part 4 you are asked to guess how many of your two group members you think indicated "RIGHT (£ 2.00)" **in Part 2** of the experiment.
- Afterwards, the computer checks, whether your guess about the group members' indication is correct.
- If Part 4 will be paid out, your payoff is determined by your guess in the followingway:
  - If your guess is correct, your payoff is £2.00.
  - If your guess is not correct, your payoff is £0.00.

Please press the "continue" button to start stating your guesses.

After the completion of this part you will receive no further instructions. However, you willbe asked to answer a few questions.