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Abstract

Leaders often have to weigh ethical against monetary consequences. We experimentally analyze such a dilemma. We measure individual honesty preferences in a payoff reporting game. Subsequently, subjects can apply for leadership and report payoffs for a group. In a control, we assign leadership randomly. Women behave less dishonestly than men when reporting for themselves. They increase dishonesty when deciding as leaders. In the control, female leaders do not increase dishonesty. We find that honesty preferences do not explain women's selection into leadership. A follow-up reveals that women who select into leadership become dishonest when believing that group members prefer dishonesty.

JEL-Codes: C910, H260, J160.

Keywords: leadership, decision for others, lab experiment, gender differences, dishonesty.

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1 Introduction

Pressing global challenges such as climate change, health crises, and inclusive growth demand ethical decisions of leaders. 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; 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 (Swamy et al., 2001; Chattopadhyay and Duflo, 2004), 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 outcomes (Garretsen et al., 2020).

To explore the motivation and the ethical behavior of female and male leaders, we study two research questions in this paper.¹ First, are there gender differences in ethical decision-making as a leader? That is, do women and men show similar ethical preferences as a leader than in individual decision-making contexts? Second, do they select into leadership positions based on their ethical preferences? In this study, 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 are assuming responsibility for a group. Moreover, we investigate the role of honesty preferences for 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 an incentive to misreport outcomes, particularly to the entities relevant for their performance evaluation (Burns and Kedia, 2006). Second, leaders' decisions impact the payoffs of different stakeholders, e.g., shareholders in the case of managers or politicians' staff members (Berman et al., 1999). Since a leader is, at least partially, evaluated based on the satisfaction of the stakeholders' needs and aspirations, his or her decisions are shaped by the beliefs about stakeholders' preferences. Moreover, the payoff externalities of leadership decisions indicate the potential role of social preferences or norms for decision-making.

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

¹As 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 males and females.

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 misreport to increase company returns (e.g., Burns and Kedia, 2006; Bollen and Pool, 2009), by misreporting sale figures of teams (Church et al., 2012), or figures to evade taxes (Joulfaian, 2000). The die-rolling paradigm measures honesty in a setting with practically no chance of being publicly exposed 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 this study is on changes in behavior across contexts (individual vs. group) 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), free riding in public transportation (Potters and Stoop, 2016), and refraining from reporting over-payments (Dai et al., 2018).

In our within-subjects experiment, participants report the outcome of a die roll twice. First, subjects only report for themselves, which serves as a proxy for individual honesty preferences. Subsequently, we measure dishonest behavior when assuming responsibility for a group as a leader. That is, subjects report the outcome of a die roll in the role of a potential group leader, which may determine their payoff and the payoff of two group members. Before they make this decision, we analyze subjects' willingness to take up leadership by asking them whether they want to become a leader or not. They learn that if more than one person says "yes," a random draw will select one of the applicants.³ Measuring these leadership preferences allows us two things. First, we can study whether individual honesty preferences affect the decision to become a leader. Second, we can analyze whether the willingness to take up leadership affects dishonest behavior for groups. To isolate the effects of endogenous leadership, we ran a control treatment without the possibility to apply for 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; Muehlheusser et al., 2015; Grosch and Rau, 2017). This gender difference vanishes when subjects make their second reporting decision in the role of a group leader. It can be explained by women increasing dishonesty as leaders, particularly those who want to assume responsibility as a leader. We find that men with a preference for dishonesty self-select into leadership and show similar misreporting behavior in the group domain as in the individual domain. By contrast, women's willingness to take over leadership is not related to their individual honesty preferences. Our

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

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

control treatment reveals that women only increase dishonest behavior for groups when they can apply for leadership, but not when there is 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 the explicit decision to act as a leader *and* making decisions on behalf of others.

To further investigate the underlying mechanisms of women increasing dishonesty as leaders, we conducted a follow-up study. The design is similar to the first study but we additionally elicit leaders’ beliefs on individual honesty preferences of group members. 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 for groups. Perceived norms seem to be the key driver for female leaders to increase misreporting for groups; particularly for women who want to become a leader. By contrast, for women who did not apply for leadership, we do not observe this finding.

Our study contributes to the scarce experimental evidence on gender differences in leadership behavior. The data demonstrate that women alter their ethical behavior when they act as leaders and applied for a position as compared to individual ethical behavior 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 historical gender-role attitudes (e.g., Alesina et al., 2013), gender differences in preferences (Croson and Gneezy, 2009; Azmat and Petrongolo, 2014) are a potential explanation for why women partly sort out. Our experiment adds to this literature by analyzing the relation of honesty preferences on the decision to become a leader. Our data analysis suggests that women’s general aversion to behaving dishonestly cannot explain their hesitance to apply and take up leadership positions. Moreover, we provide evidence of 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 of women’s honesty when promoted to leaders suggests that a mandatory quota for women in management positions may not result in overall higher levels in 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. The data are used for another experiment on unincentivized vs. incentivized elicitation of preferences (Grosch et al., 2021).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 deciding on individual payoffs (“individual preferences for honesty”) and group payoffs

⁴Note that no feedback is provided 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.

(“preferences for honesty in groups”). For each part, subjects receive new instructions and, this way, we inform them step by step about the subsequent parts. Subjects are told that at the end of the session, the computer would randomly select one of the parts for payoff. Each session ends with a questionnaire on socio-demographics.

2.1 Individual Preferences for Honesty

To measure subjects’ individual honesty 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 that yields no payment to mitigate the risk of introducing a focal point (Fischbacher and Föllmi-Heusi, 2013). This part is essential for our within-subjects experiment as it enables us to compare these individual honesty preferences to the situation, where subjects can misreport for groups, explained in the following.

2.2 Preferences for Honesty in Groups

In this part, we measure dishonest behavior when subjects decide as leaders of groups. 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 the payoff of each group member is determined by the report made by a group leader. 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 these applicants for leadership. When no one applies, the random draw selects one person among the three group members.⁵ The choice

⁵This case has not occurred in our data.

mechanism enables us to relate the subjects’ individual honesty preferences to their willingness to act as a leader. Moreover, we can analyze whether a subject’s choice to assume responsibility affects dishonest behavior when making ethical decisions as a leader.

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 the leader. 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 enter the payoff they want to report should they become person A (leader). They know that this decision only becomes relevant if they are selected as the leader. That procedure allows us to compare the honesty 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 they are informed about the report made by this subject at the end of the experiment. However, they are not informed about the real die outcome of this subject. We illustrate the sequence of actions in Figure 1.

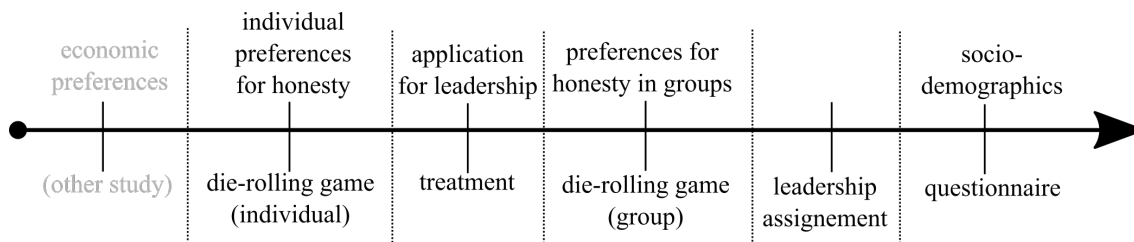


Figure 1: Sequence of actions.

Our experimental design models the hierarchical decisions of leaders. That is, subjects report the payoffs for the group in the role of leaders in a non-strategic situation. In contrast, Kocher et al. (2017) also analyze honesty decisions in groups but in their experiment, no leader makes decisions on behalf of the group but the group members all report own payoffs. Moreover, in their scenario, decision-making is strategic and group payoffs increase when group members coordinate in their reporting decision, i.e., subjects only earn a positive amount if they report the same die outcome as the other group members.⁶ Another difference is that our subjects decide independently of others, whereas subjects in Kocher et al. (2017) meet in a chat before they make their reporting decisions. We deliberately refrain from a strategic group context to avoid confounds for answering our research question on leaders’ decision to report payoffs on behalf of their teams.

2.3 Control Treatment

To disentangle the effects of endogenous leadership on dishonest behavior, we run a control treatment called *exogenous leadership* with a random external appointment of leaders without

⁶In a control treatment, they also analyze simultaneous group decisions of reporting individual payoffs when subjects do not have to achieve commonality.

group members having the possibility to apply. 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 a third, i.e., we tell all three group members that their probability of becoming a leader is a third, and a 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 a half.⁷ Except for the exogenous leader choice, where subjects are told the probabilities of becoming a leader, everything else is similar in the control treatment as compared to the main treatment.

2.4 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 in the experiments (144 in the main treatment; 138 in the control treatment). After subjects made their decisions, we ran a questionnaire to verbally elicit their preferences. This is part of another project on the relation of non-incentivized and incentivized elicitation of preferences (Grosch et al., 2021). 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 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. Subjects were paid in cash at the end of the experiment and earned an average of €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).

3 Study 1: Hypotheses

In our analysis of honesty preferences, we focus on the incident of either misreporting payoffs or not but in our experiment, no other party can be betrayed. 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 (Dreber and Johannesson, 2008; Houser et al., 2012; Conrads et al., 2014; Grosch and Rau, 2017) and in the field (Azar et al., 2013; Bucciol et al., 2013). This derives our first hypothesis on individual reporting behavior.

Hypothesis 1:

Men behave more dishonestly than women when deciding for themselves.

⁷We do not have groups in which only one group member could become the leader for sure as this is a very rare case in the endogenous treatment. This only occurred in five of 144 cases.

Misreporting for the group generates a benefit for the team 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 deciding for themselves. 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 for groups as compared to misreport for individual payoffs.

We expect gender differences when being dishonest as a leader based on the following reasoning. Women have been found, on average, to be more prosocial than men (e.g., Eckel and Grossman, 1998; Andreoni and Vesterlund, 2001; Croson and Gneezy, 2009; Rand et al., 2016). They are also expected to demonstrate higher prosociality at work compared to men (Brañas-Garza et al., 2018). A behavior that can be internalized. When they do not follow this stereotypical expectation, they are evaluated more negatively than men with similar prosocial behavior (Heilman and Chen, 2005). Deciding to be dishonest for the group could be perceived as an act of being prosocial. In this case, there are two reasons why women may show a more pronounced increase of dishonest behavior when deciding for the group than men. First, they are *expected* to misreport when making decisions concerning others' payoffs. Second, they have more pronounced prosocial preferences than men.

Hypothesis 2:

(a) Subjects increase dishonest behavior when reporting payoffs for a group compared to reporting payoffs for themselves only.

(b) The increase in dishonest behavior when deciding for a group as compared to for oneself is more pronounced for women than for 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 select into leadership positions based on individual characteristics that resonate with the characteristics of the decision environment, e.g., risk preferences, overconfidence, or competitive preferences (e.g., Barber and Odean, 2001; Eckel and Grossman, 2002; Niederle and Vesterlund, 2007; Ertac and Gurdal, 2012; Reuben et al., 2012; Niederle et al., 2013; Alan et al., 2020; Ertac and Gurdal, 2012). In our setting, we expect that individual honesty preferences determine subjects' decisions to become leaders because in our decision context dishonesty pays off. Therefore, subjects with an individual preference for dishonesty may apply for leadership to ensure that the individual profit is also maximized when deciding in the group domain. For honest subjects, we do not believe that they apply for leadership to enforce honest behavior in the group domain. The reason is that when an honest subject does not become a leader and another leader behaves dishonestly, the subject who did not apply for leadership is not responsible for this decision. In contrast, dishonest subjects have strong monetary incentives for becoming a leader and to misreport in

the group domain.

Hypothesis 3:

Subjects who behave dishonestly for themselves are more likely to apply for the leadership position.

An integral part of a leader’s job is to assume responsibility for making decisions on behalf of others. These decisions can be different in nature and gender differences in attitudes toward responsibility can be an important driver behind the observed lack of women in leadership positions (Alan et al., 2020). Thus, subjects in our design can express their willingness to assume responsibility when they apply for the leadership position. The literature on endogenous institutions emphasizes that subjects are more willing to stick to a certain behavior after they have deliberately voted for institutions. More precisely, subjects contribute more in public good games after they have voted for a punishment institution that intends to prevent free-riding (Kosfeld et al., 2009; Sutter et al., 2010). Moreover, there is evidence that leaders who volunteered to act as a leader contribute more to a public good than leaders who are exogenously appointed (Haigner and Wakolbinger, 2010; Rivas and Sutter, 2011). Thus, we expect in our set-up that subjects increase dishonest behavior for the group when they applied for leadership.

Hypothesis 4:

The increase in misreporting for groups is driven by dishonest subjects who applied for leadership.

4 Study 1: Results

In this section, we present our main results on dishonest behavior where we compare individual behavior when making reporting decisions only for oneself with reporting decisions as a leader of a group. Moreover, we analyze sorting into leadership positions. When applying non-parametric tests, we always report two-sided p-values throughout.

4.1 Main results

First, we focus on subjects’ misreporting behavior in our baseline 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.⁸ 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 reporting decisions of individual payoffs using white bars and the reporting decisions

⁸The 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.

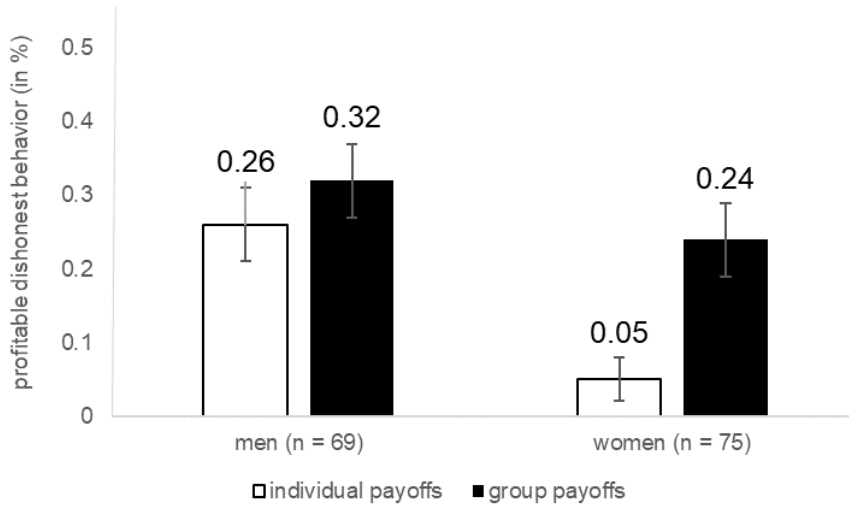


Figure 2: Percentage of profitable dishonest reports in *endogenous leadership*. White (black) bars present the reports for individual payoffs (group payoffs). Standard error bars included.

of group payoffs with black bars in Figure 2. The diagram 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). That is, men are (26%) five times more frequently dishonest than women (5%) (Fisher’s exact test, $p=0.001$).

We turn to our first research question and analyze misreporting behavior when deciding as group leaders. It can be seen that 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 a similar behavior in both contexts (individual payoffs: 26%; group payoffs: 32%) (Wilcoxon matched-pairs test, $p=0.346$). 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) who analyzed group decisions in a strategic setting with pre-play communication and without leaders.

Table 1 demonstrates that our results are confirmed by random effects Probit regressions on the likelihood of behaving dishonestly. Model (1) includes a gender dummy (*female*), which is positive for women and *group payoff*, a dummy which is positive when subjects make decisions affecting group payoffs. In model (2), we interact *female* and *group payoff*. In model (3), we include controls for age and whether subjects study economics. As we elicited our preference measures with two different orders, we also include a dummy variable controlling for the order. The order dummy is 1 if the preference elicitation is at the beginning of the experimental session

Table 1: Random effects Probit regressions on dishonest reports in *endogenous leadership*.

	<i>dishonest reports</i>		
	(1)	(2)	(3)
<i>female</i>	-0.140*** (0.052)	-0.266*** (0.076)	-0.253*** (0.076)
<i>group payoff</i>	0.126*** (0.039)	0.046 (0.048)	0.046 (0.049)
<i>female</i> × <i>group payoff</i>		0.202** (0.082)	0.202** (0.083)
controls ^a	no	no	yes
obs.	288	288	288
groups	144	144	144
Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

Note: The regressions are clustered on the subject level (n = 144) and report average marginal effects.

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

and 0 otherwise.⁹ Each subject makes two reporting decisions (n = 288). The regressions are clustered at the subject level (n = 144). We report average marginal effects (see Table 5 in Appendix A for the corresponding Probit model).

In model (1), we find that *female* is highly significant with a negative sign. Thus, women generally behave less dishonestly than men. Moreover, *group payoff* is highly significant, i.e., subjects increase their dishonest behavior when being responsible for the payoffs of group members. This result supports Hypothesis 2a. Model (2) highlights that the increase in dishonest behavior for groups is driven by women. When controlling for the interaction effect of *female* × *group payoff*, the coefficient of *group payoff* becomes insignificant. At the same time, the interaction is significant and positive. Thus, the interaction effect offsets the negative coefficient of *female* which confirms the existence of the gender differences only for the individual domain. Thus, we confirm Hypothesis 1. Model (3) shows that the finding is robust to the inclusion of control variables. The significant interaction effects of *female* and *group payoff* confirm Hypothesis 2b. We summarize our findings.

Result 1: Dishonest Behavior for Individual and Group Payoffs

- (a) *Women behave less dishonestly than men when reporting individual payoffs.*
- (b) *Women behave as dishonestly as men when acting as leaders.*

⁹The dummy is not significant in any of our regressions of the whole study.

4.2 The Impact of Endogenous Leadership

In this section, we test hypotheses 3 and 4 to better understand the influencing factors of dishonest behavior for groups. For this, we focus on the impact of endogenous leadership. In our *endogenous leadership* treatment, subjects can actively apply to become a leader and saying “yes” to this opportunity increases the chance of ending up as a leader. Thus, the choice to apply may work as a selection device in a way that subjects who apply might differ in their individual honesty preferences from the subjects who decided against it.

This idea follows our second research question and Hypothesis 3, which expects that individual honesty preferences are a predictor for the willingness to become a leader. Interestingly, it turns out that an insignificantly smaller share of women (71%) than men (80%) (Fisher’s exact test, $p=0.250$) apply for leadership. Since women generally behave more honestly for themselves, the rather high share of women applying for leadership suggests that women may have other reasons to apply for leadership than honesty preferences. To test Hypothesis 3 more thoroughly, we run Probit regressions (Table 2), analyzing the determinants of subjects’ willingness to become a leader.

Table 2: Probit regressions on wanted leadership and dishonest reports for groups (*endog. leadership*).

	<i>wanted leadership</i>			<i>dishonest reports for groups</i>	
	(1)	(2)	(3)	(4)	(5)
<i>female</i>	-0.057 (0.073)	-0.038 (0.075)	-0.042 (0.076)	-0.061 (0.073)	0.043 (0.074)
<i>wanted leadership</i>				0.196** (0.091)	0.197** (0.089)
<i>misreported for themselves</i>	0.211* (0.122)	0.287* (0.156)	0.287* (0.153)		
<i>female</i> × <i>misreported for themselves</i>		-0.244 (0.266)	-0.211 (0.265)		
controls ^a	no	no	yes	no	yes
obs.	144	144	144	144	144
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.

In models (1)–(3), we analyze whether the decision to apply for leadership is motivated by individual honesty preferences. To test this, we include a dummy (*misreported for themselves*) that is positive when subjects behaved dishonestly when deciding on individual payoffs and zero

otherwise. We also control for subjects' gender (*female*) and the interaction effect of gender and individual misreporting (*female* \times *misreported for themselves*). Model (3) includes the same set of controls as before. The models report average marginal effects (see Table 6 in Appendix A for the corresponding Probit model).

We find that the coefficient of *misreported for themselves* is significant at the 10-percent level (model (3), $p=0.060$), i.e., subjects who behave dishonestly at the individual level are more likely to become leaders. *Female* is not significant which confirms our finding that there are no gender differences in the willingness to become a leader. A closer look at models (2)–(3) reveals that the positive effect of honesty preferences is entirely driven by men. That is, *misreported for themselves* is only significant for men but not for women (Wald test, $p=0.723$). Thus, selection effects based on individual honesty preferences are only in place for men but not for women, which supports Hypothesis 3 only for male leaders.

We turn to Hypothesis 4 to shed more light on the effects of endogenous leadership. We test whether subjects' deliberate decision to act as the leader may impact their behavior when reporting the payoffs of groups. Models (4)–(5) present this analysis and focus on dishonest reports for groups. The models include a dummy variable (*wanted leadership*) that is positive when subjects applied for leadership. The models use a gender dummy and the same controls as in models (2)–(3). We find that subjects who applied for leadership are more likely to behave dishonestly when deciding for group payoffs, demonstrated by the positive and significant coefficient of *wanted leadership*. Model (5) highlights that among subjects who wanted to become leaders, the probability of a dishonest report is higher by more than 19 percentage points. Thus, we find support for Hypothesis 4. Wilcoxon matched-pairs tests show that the increase in dishonest behavior is driven by women who applied for leadership. These women significantly behave more dishonestly for groups as compared to when they are deciding for themselves ($p<0.001$). By contrast, this is not the case for men who want to become leaders ($p=0.607$). We summarize our findings.

Result 2: Determinants of Leadership and Consequences on Dishonesty in Groups

- (a) *Men who behave dishonestly for themselves are more likely to become leaders.*
- (b) *Women's decision to become a leader is not driven by individual honesty preferences.*
- (c) *Subjects who applied for leadership increase dishonesty for groups.*

The regressions indicate that the endogenous choice to apply for leadership may work as sort of a selection device for dishonest men. That is, dishonest men apply for leadership and again behave dishonestly when deciding in the group domain. By contrast, we do not support Hypothesis 3 for women. Since selection effects cannot explain women's increase in dishonest behavior, the question remains: *Why do women increase dishonesty when making decisions for groups?* Evidence on gender differences in preferences reveals that women oftentimes behave context-dependent (Croson and Gneezy, 2009). If this also applies to our case of honesty preferences, it

may be that women do not behave dishonestly for themselves but start to behave dishonestly when deciding for a group. Moreover, it is possible that women’s increase in dishonesty depends on an interaction effect of the deliberate decision to assume leadership and the payoff decision for the group. To disentangle the effect of the active application of assuming leadership and a mere preference change due to the context, we ran a control treatment (*exogenous leadership*). This control treatment isolates subjects’ active application as a leader from the pure responsibility of deciding on group payoffs.

4.3 The Impact of Exogenous Leadership

In the control treatment (*exogenous leadership*), subjects cannot apply for leadership. To isolate the effects of the decision to assume leadership, we compare the findings of the baseline treatment to the results in *exogenous leadership*. In this control treatment, we apply a similar sequence of actions as before. The only difference is that this treatment disables the leadership choice and leadership is exogenously determined by a random draw with varying probabilities (1/3 or 1/2). Before the analysis, we check whether dishonest behavior for groups depends on the probability of becoming a leader. The data show that the probability (1/3 vs. 1/2) does not significantly influence the fraction of dishonest reports in the groups (Fisher’s exact test, $p=0.323$) and increased misreporting when deciding for groups (a dummy, which is positive when subjects misreported for groups, but not for themselves) (Fisher’s exact test, $p=0.439$).¹⁰ We also run two Probit regressions (on dishonesty in groups and the increase of dishonest behavior for groups) which confirm the non-parametric test results and show that the probability of becoming a leader does not affect ($p>0.427$).¹¹ Thus, we merge these data.

Figure 3 presents the share of profitable dishonest reports for individual payoffs and group payoffs when leadership is exogenously determined. Again, we find a gender difference in dishonest behavior when subjects decide for themselves. That is, 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, $p=0.263$). In contrast to the endogenous treatment, women show a less pronounced and insignificant increase of dishonest behavior from the individual (12%) to the group domain (18%) (Wilcoxon matched-pairs test, $p=0.453$). As a consequence, the gender difference in dishonest behavior does not disappear for group decisions in the treatment when leaders are exogenously determined (Fisher’s exact test, $p=0.001$).¹²

¹⁰In the endogenous treatment, we did a similar analysis and find that subjects’ beliefs of becoming a leader do not significantly affect dishonest behavior for groups (Fisher’s exact test, $p=0.463$), or the dishonest behavior for groups ($p=0.223$).

¹¹In 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 tables 1 and 2.

¹²In the endogenous treatment, subjects stated the probability of ending up as a leader. A possible explanation for the treatment effect could be that this probability distribution is significantly different from the communicated probability distribution of becoming a leader in the exogenous treatment. However, we do not find a statistically significant difference across treatments (Fisher’s exact test, $p=0.206$).

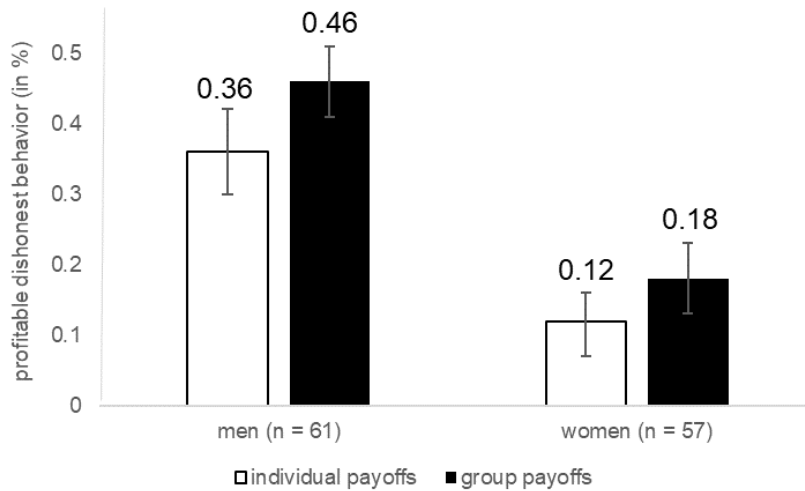


Figure 3: Percentage of profitable dishonest reports under *exogenous leadership*. White (black) bars present the reports for individual payoffs (group payoffs). Standard error bars included.

The finding that women do not increase dishonest behavior for groups without explicit application for leadership demonstrates that their dishonest behavior is not purely context-dependent. Probit regressions confirm this treatment difference in dishonest behavior for groups. These results are similar to the findings of studies that demonstrate that the type of selection mechanisms impact the behavior of people who deliberately selected into these positions (e.g. Estrada, 2019; Cotton and Price, 2014; Grönqvist and Lindqvist, 2016; Kuhn and Weinberger, 2006). That is particularly true if selection options, such as affirmative action, allow women to get access to desirable positions (Ibañez and Riener, 2018).

The regressions in Table 3 compare women’s increase in dishonest behavior in the endogenous treatment and the exogenous treatment. For this, we apply subsample regressions. Models (1)–(3) compare the increase in dishonest behavior of women who wanted to be leaders in *endogenous leadership* with *all* women in the exogenous treatment. That is, the models focus on a dummy, which becomes positive when subjects increased dishonest behavior in the group domain, as compared to reporting for themselves. Models (4)–(6) compare the increase in dishonest behavior of women who opted against the leadership in *endogenous leadership* with *all* women in the exogenous treatment. In all models, we identify women in the exogenous treatment with a dummy variable (*exogenous leadership*), which becomes positive in the exogenous treatment. In models (3) and (6), we include the same control variables as in the previous regressions. In models (2)–(3) and (5)–(6), we also add a control variable that accounts for subjects’ expected chance that they will end up as a leader (*(perceived) chance of becoming leader*) in the two treatments. To compute the (perceived) chance in the endogenous treatment, we used subjects’ guesses on how many other subjects want to become a leader. In the exogenous treatment, the variable is the communicated probability of ending up as a leader. All regressions report average

Table 3: Probit regressions on women’s increase in dishonesty for groups (*endo. vs. exo.*).

	<i>increased dishonesty for groups</i>					
	wanted leadership			did not want leadership		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>exogenous leadership</i>	-0.138**	-0.138**	-0.125*	-0.045	-0.050	-0.039
	(0.067)	(0.067)	(0.067)	(0.072)	(0.080)	(0.077)
<i>(perceived) chance of becoming leader</i>		0.263	0.200		-0.035	0.009
		(0.408)	(0.404)		(0.277)	(0.277)
controls ^a	no	no	yes	no	no	yes
Obs.	110	110	110	79	79	79
	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.

marginal effects (see Table 7 in Appendix A for the corresponding Probit model).

In models (1)–(3), the negative significant coefficients of *exogenous leadership* demonstrate that women who could not apply for leadership increase dishonest behavior less often than women who actively applied for leadership in the endogenous treatment. We do not find treatment differences when we compare women who did not apply for leadership in the endogenous treatment and women in the exogenous condition. That is, in models (4)–(5) *exogenous leadership* is insignificant and model (6) shows that the results are robust to the inclusion of controls. Thus, the treatment difference of leadership behavior is not driven by differences in the expected chance of becoming the leader. We ran the same regression models for male subjects and find no significant treatment effect in any of the four models (see Table 9 in Appendix A).

Result 3: Dishonest Behavior under Exogenous Leadership

(a) Under *exogenous leadership*, women behave less dishonestly for groups than men.

(b) Under *exogenous leadership*, women behave less dishonestly for groups than women who actively wanted to assume leadership in the endogenous treatment.

So far, the results show that selection effects cannot explain the increase in women’s misreporting behavior for groups. Furthermore, we rule out that women’s honesty preferences are solely context-dependent, i.e., whether they have to decide for individual or group payoffs. We demonstrated that as soon as we disable the choice option to become a leader, women do not behave more dishonestly in the group than in the individual domain. Hence, we conclude that the treatment difference in the increase of dishonesty for groups is related to the leader-choice option. The active choice to assume leadership may interact with the decision to report payoffs

for a group. The decision to assume leadership may lead to a behavioral change for female leaders who then try to align their behavior to the honesty preferences of their group members. Thus, we examine whether female leaders' dishonest behavior reflects their beliefs on the individual honesty preferences of group members. For this, we conduct a follow-up experiment (Study 2) to learn more about the mechanisms of women's behavioral change in dishonest behavior when deciding on group payoffs.

5 Study 2: Channels of Dishonest Behavior

In this section, we report the results of our follow-up study. In this study, we focus on the endogenous treatment to get a better understanding of the interaction effect of women's willingness to assume leadership and their decision when reporting group payoffs. We investigate two potential channels. First, the active choice to decide on behalf of the group may activate an increased sense of responsibility. Assuming responsibility for the group may heighten female leaders' focus on the other group members' preferences. Thus, female leaders may try to behave in line with the individual honesty preferences of other group members. To examine this mechanism, we measure women's beliefs on the individual reporting decisions of group members concerning their individual payoffs.

Second, since the decisions of female leaders impact the payoffs of other group members, women's prosocial preferences captured by social value orientation (SVO) may affect the misreporting behavior for groups. Prosocial women in particular, in contrast to women who are more concerned with maximizing only their individual payoff, may increase dishonest behavior for the group. Therefore, we elicit subjects' SVO. We also conduct a post-experimental questionnaire to learn more about subjects' motives for behaving dishonestly for the group.

5.1 Study 2: Experimental Design

The experiment is almost identical to the first study. However, it is a follow-up and not a replication study. Put differently, we applied several important changes to dig deeper into the mechanisms of women's behavior change under endogenous leadership. For this, we elicit subjects' beliefs on the individual honesty preferences of group members as well as individual social value orientation (SVO).¹³ The experiment comprises several parts and one of them is randomly determined to be payoff-relevant.

In the first block, we measure economic preferences of subjects. First, we elicit social value orientation with the ring measure of Liebrand and McClintock (1988). Here, subjects are repeatedly confronted with two possible payoff allocations between themselves and another subject. In each decision set, the allocations vary in payoff differences and the total payoff for both subjects.

¹³Furthermore, we measure subjects' beliefs on the dishonest behavior of other group members when deciding as leaders on the payoffs of groups.

Subjects have to decide for one payoff allocation and trade-off between more money for themselves and more equal/more money in total for both. Based on their choices, one can calculate an angle value for each subject (see Appendix B for details). Again, we measure additional economic preferences, which are part of another study (Grosch et al., 2021).

In the second block, we elicit individual honesty preferences similar to Study 1. Afterward, we analyzed preferences for honesty in groups. We alter the design slightly by exogenously varying the groups' gender compositions. We create groups with three women, a woman and two men, and two women and one man.¹⁴ The design is similar to Study 1. However, to study the potential impact of group members' identities we additionally inform subjects on the gender, age, and study terms of the other group members.¹⁵ Subjects receive this information after making their leader-choice decision and before making their group-reporting decision. This sequence of actions rules out that the decision to become a leader is affected by the information on the gender composition in the group.

In the third block, we elicit subjects' *beliefs on dishonesty in groups*, i.e., their beliefs about the reporting decision of the other group members when deciding as leaders. Thereby, we can analyze whether women's beliefs on dishonest behavior in the group change when varying the gender composition of the group. First of all, subjects learn that the computer randomly rolls a die for the whole group. After the die-roll is displayed, they make their reporting decision. Thereafter, we elicit their beliefs on other group members' reporting decisions. For this, we again show the subjects information (gender, age, and semester number) of the other two group members. Then they have to guess which number each of the two group members reported. Subjects receive €1 for each correct guess. Afterward, we elicit subjects' *beliefs on individual honesty preferences*, i.e., their beliefs about other group members' reporting decisions when reporting their own payoffs. The measure is our main interest regarding beliefs, as it resembles an unbiased measure of subjects' belief on other group members' honesty preferences. To measure this, we ask subjects about the reporting decision of a randomly selected group member when reporting her own payoffs. We show them the information of the selected group member and we elicit the belief function to get a precise estimate. Subjects have to state the group member's reported number for each possible die number between one and six.¹⁶ To derive the *beliefs on individual honesty preferences*, we compute the mean of the six guesses. Finally, we ask questions on subjects' motives for becoming/or not becoming a leader. We also ask about their motives for the die-rolling report. The study design of Study 2 is illustrated in Figure 4. We highlight the changes in black whereas the initial study design is illustrated in gray.

In the follow-up study, 219 subjects (90 male; 129 female) participated. We only invited

¹⁴We use a special algorithm that aims at generating mainly these three different compositions. With excessive men in the session, the algorithm forms groups consisting only of men.

¹⁵We elicit this information at the very beginning of the experiment. Here, subjects are told that this information may be communicated to other subjects in the course of the experiment.

¹⁶We apply the strategy method to elicit their beliefs. Subjects learn that if the belief for the actual die-roll of the corresponding group member is correct, they earn €1 in addition.

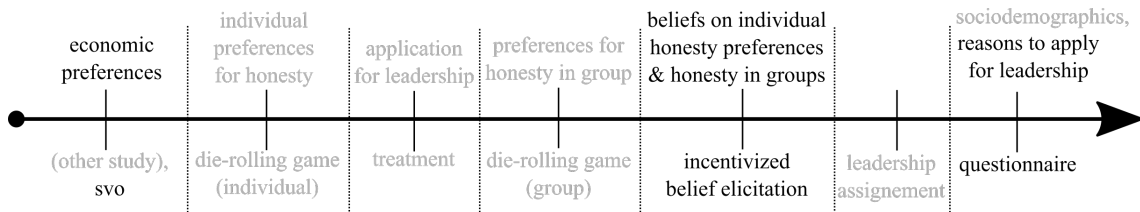


Figure 4: Experimental sequence of study two; in black the additional elements to study 1.

subjects who did not participate in Study 1. On average, the sessions lasted 70 minutes and subjects earned €12.64.

5.2 Study 2: Results

In the following analyses, we focus on women’s misreporting decisions since the purpose of Study 2 is to learn more about women’s behavioral change when they assume leadership. The aggregate data of Study 2 confirm our previous findings of Study 1. Women significantly increase dishonest behavior from 12% to 23% when deciding on the group payoffs (Wilcoxon matched-pairs test, $p=0.004$).¹⁷

To start with, we analyze the impact of group compositions concerning gender on reporting decisions. We find that female leaders’ dishonest behavior does not differ across all possible group compositions (Fisher’s exact test on joint data, $p=0.531$). The same holds for the beliefs on dishonesty in groups (Fisher’s exact test on joint data, $p=0.546$). In line with these findings, we also find no influence of group compositions when focusing on women’s *increase* of dishonest behavior of individual and group decision-making (Fisher’s exact test on joint data, $p=0.755$). Thus, we merge these data for further analyses.

5.2.1 The Impact of Beliefs and Social Value Orientation

In this section, we explore whether women’s reporting decisions for the group are influenced by their beliefs about the individual honesty preferences of other group members and their individual general concern for others’ payoffs as measured by social value orientation. We start our analysis by focusing on the effects of the leadership choice on dishonest behavior. The data support our previous findings, i.e., women who apply for leadership behave significantly more dishonestly (24%) in the group compared to the individual domain (11%) (Wilcoxon matched-pairs test, $p=0.005$).¹⁸

Next, we turn to the first potential channel for women’s increased misreporting as leaders, i.e., their belief on individual honesty preferences. We focus on this belief since it is more

¹⁷For men, we find that the increase is less pronounced (from 13% to 21%) and just falls short of significance (Wilcoxon matched-pairs test, $p=0.108$).

¹⁸Focusing on men who wanted to become a leader, we also support the previous findings. That is, the impact of endogenous leadership is less pronounced on men as compared to women, i.e., men show an insignificant increase of dishonest behavior from 13% to 22% (Wilcoxon matched-pairs test, $p=0.180$).

informative than the belief we measure on the group level to test for the effects of information on demographics. The belief on the group level may also reflect the decision-maker’s beliefs about other group members’ social preferences. In our further analyses we refer to the beliefs on individual honesty preferences, as beliefs on individual dishonesty.

Figure 5 overviews women’s increase in dishonest behavior between the individual and the group domain. It distinguishes between women who did (left panel) or did not (right panel) wanted to become a leader. The figure also differentiates on women’s *belief on individual dishonesty* and presents the effects conditional on the median split (3.0). We distinguish between a below/equal-median belief (white bars) and an above-median belief (black bars).

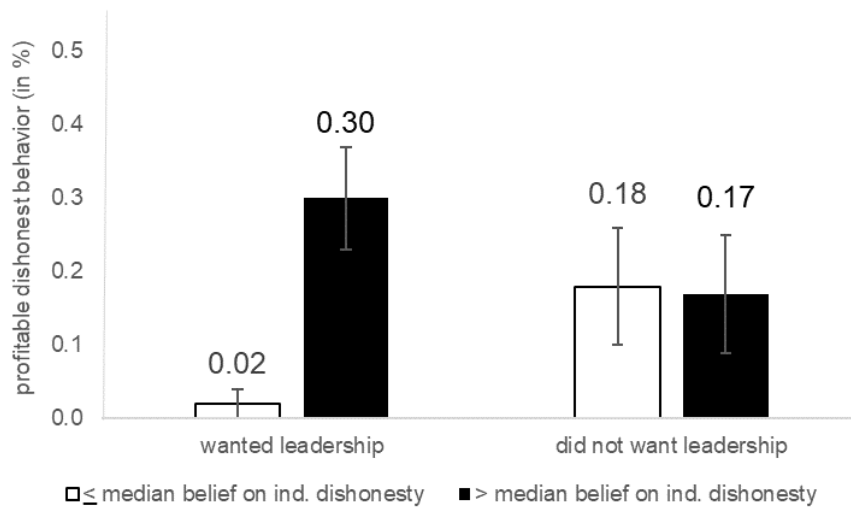


Figure 5: Women’s increase in dishonesty when deciding on group payoffs, conditional on leader choice and on their *belief on individual dishonesty*. Standard error bars included.

The diagram shows that women who wanted to assume leadership align their dishonest behavior for groups based on their belief of other group members’ individual dishonesty. When these women have an above-median belief on group members’ individual misreporting behavior, they show a substantially higher increase (30%) in dishonesty as compared to women who hold a below-median belief (2%) (Fisher’s exact test, $p < 0.001$). Turning to women who did not want to assume leadership, we do not find such a pattern. In this case, misreporting behavior is independent of their beliefs and women who hold a below-median belief show a similar increase (18%) in dishonest behavior to women who hold an above-median belief (17%) (Fisher’s exact test, $p = 1.000$).

Regarding our second channel of the increased misreporting in groups, we apply Probit regression analyses while controlling for social value orientation. In models (1)–(2), we use the full sample of women’s decisions, unconditional on the willingness to assume leadership. Models (3)–(4) focus on the subsample of women who wanted to assume leadership whereas models (5)–(6) concentrate on the subsample of women who did not want to assume leadership.

Table 4: Probit regressions on women’s increase in dishonesty conditional on their leadership preferences.

	<i>increase in dishonest behavior</i>					
	all data		wanted leadersh.		did not want leadersh.	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>belief on ind. dishonesty</i>	0.110*** (0.029)	0.113*** (0.028)	0.128*** (0.031)	0.126*** (0.030)	0.055 (0.063)	0.079 (0.068)
<i>svo</i>	-0.000 (0.001)	-0.000 (0.001)	-0.002 (0.002)	0.000 (0.002)	-0.001 (0.001)	-0.001 (0.002)
controls ^a	no	yes	no	yes	no	yes
obs.	129	129	84	84	45	45

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The regressions are clustered at the group level and report average marginal effects.

^a Controls: age and whether subjects study economics.

We always control for women’s *belief on ind. dishonesty* of group members. The models also include *svo*, which is the angle value of subjects’ social value orientation. In models (2), (4), and (6), we also include control variables, i.e., subjects’ age and whether they study economics. All models are clustered on subjects’ matching groups of the communicated demographics of group members. The regressions report average marginal effects (see Table 10 in Appendix A for the corresponding Probit model). The regressions confirm the findings of Figure 5 regarding the importance of women’s beliefs of individual dishonesty of other group members. That is, in models (1)–(2), the highly significant positive coefficients of *belief on ind. dishonesty* demonstrate that the likelihood of dishonest behavior significantly increases in women’s beliefs on group members’ individual dishonesty. One may argue that this relation could be a consequence of “motivated beliefs.” However, we mitigated this concern by incentivizing the belief elicitation.

Models (3)–(4) emphasize that the effect of women’s belief on individual dishonesty is only relevant for women who wanted to assume leadership. We find that the coefficients of women’s *belief on ind. dishonesty* are positive and highly significant and they become insignificant as soon as we focus on women who did not want to assume leadership (models (5)–(6)). Turning to *svo*, we find that the coefficients are never significant. Hence, only women’s beliefs on individual honesty preferences play a role, in the case when they wanted to become leaders.

Result 4: Beliefs on Individual Honesty Preferences and Dishonesty in Groups

Women who want to assume leadership misreport significantly more for groups when they believe that group members have a preference for dishonesty.

5.2.2 Questionnaire

In the questionnaire, we asked for the reasons to apply for leadership and to report specific payoffs. Subjects answered in free-form texts. We classified women’s answers based on certain dimensions: (i) importance of the active role in the group decision; (ii) acting as a leader is more interesting; (iii) enforcement of individual honesty preferences; (iv) assuming responsibility for the group. Among women who applied for leadership, we find two large groups, which we report henceforth. First, 30.9% state that they wanted to have an active role to influence the reporting decision. Second, 29.8% argue that they wanted to become leaders to assume responsibility for the group. A closer look shows that this share increases to 45% when focusing on women with an above-median belief that group members have a preference for individual dishonesty. Moreover, 56% of them misreport for the group.

This pattern is supported when we focus on women’s reasons for their reporting behavior. Women who have an above-median belief in individual dishonest behavior of group members state in 23% of cases that they misreported to achieve a high payoff for group members. In contrast, for female leaders with a below-median belief in individual dishonest behavior, we find that this share is significantly smaller (4.4%) (Fisher’s exact test, $p=0.022$). Hence, the questionnaire supports the idea that women who applied for leadership are shaped by a norm to fulfill the needs of group members.

Finally, when we focus on women who did not want to become leaders, we find that the majority (67.5%) argue that they do not want to assume responsibility for the group.

6 Conclusion

In this paper, we analyzed gender differences in ethical decision-making *as a leader* in two studies, where we focus on dishonest behavior. In the first study, we conduct a within-subjects experiment with two stages, where subjects first decide for themselves and subsequently report payoffs for their groups. Moreover, we model subjects’ deliberate decisions to apply for leadership. We can analyze whether this decision is related to individual honesty preferences and whether it impacts reporting behavior in a group context. Our second study focused on the mechanisms of women’s dishonest behavior in the group domain.

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 and want to assume leadership whereas men act similarly in the individual and the group domain. A closer look at the leader-choice option reveals that men’s decision to become a leader is shaped by their individual honesty preferences. By contrast, although women behave more dishonestly for groups, we do not find that their decision to apply for leadership is linked to their honesty preferences. This 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 control treatment disables the leader-choice option and highlights that women’s decision to behave dishonestly is not context-dependent, i.e., does not differ across the individual and the group context. The control treatment shows that women do not increase dishonesty per se when they act in a leadership position. Our results suggest that women’s explicit decision to assume responsibility for a group leads to a behavioral change.

The results of the follow-up study disclose a potential channel for female leaders’ increase of dishonesty. That is, the explicit decision to become a leader motivates them to adjust their behavior to the honesty preferences of group members. This phenomenon does not depend on the group context *only*. It occurs as a combination of the decision to become a leader who subsequently has to report payoffs for a group.

Our paper improves the understanding of women’s (and men’s) motivation to apply for leadership. We also shed light on gender differences in dishonest behavior when deciding for groups. We showed that women’s decision to apply for leadership is not correlated to their honesty preferences. This highlights that the gender gap in the application to leadership positions may not be associated with women’s less pronounced preference for dishonesty. This may imply that there is no need to address differing honesty preferences of men and women in workplace policies. Interestingly, we show that although women may not behave dishonestly for themselves, actively assuming responsibility motivates them to behave dishonestly for others. This highlights the importance of promotion mechanisms since the opportunity to apply for leadership may lead to a behavioral change for women who decide for a group. Adams and Funk (2012) demonstrate for Sweden’s top directors that women are more benevolent, 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 to expectations or stereotypical thinking. However, when leadership demands women to implicitly change their preferences due to others’ expectations as in our context, it could have a range of consequences. For instance, affirmative action policies in the form of a women’s quota may not result in higher ethical standards at the management level per se. Decision-makers should keep in mind that the procedural design of the hiring process might matter, i.e., whether women actively apply for a promotion or whether the company/institution commends their promotion which may be essential for the leader’s ethical behavior. 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 working part-time (Manning and Petrongolo, 2008) in the long term. There is scarce experimental evidence on how employee’s 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 5: Random effects Probit regressions on dishonest reports in *endogenous leadership*.

	<i>dishonest reports</i>		
	(1)	(2)	(3)
<i>female</i>	-0.747** (0.303)	-1.529*** (0.502)	-1.420*** (0.486)
<i>group payoff</i>	0.674*** (0.225)	0.265 (0.280)	0.261 (0.280)
<i>female</i> × <i>group payoff</i>		1.160** (0.503)	1.133** (0.495)
<i>constant</i>	-1.179*** (0.274)	-1.000*** (0.290)	6.147 (67.277)
controls ^a	no	no	yes
obs.	288	288	288
groups	144	144	144
Wald Chi2	13.00	13.35	15.79
Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

Note: The regressions are clustered on the subject level (n = 144).

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

Table 6: Probit regressions on wanted leadership and dishonesty for groups (*endog. leadership*).

	<i>wanted leadership</i>			<i>dishonest reports for groups</i>	
	(1)	(2)	(3)	(4)	(5)
<i>female</i>	-0.185 (0.238)	-0.123 (0.246)	-0.137 (0.249)	-0.189 (0.227)	-0.136 (0.234)
<i>wanted leadership</i>				0.607** (0.290)	0.625** (0.295)
<i>misreported for themselves</i>	0.684* (0.405)	0.934* (0.518)	0.946* (0.512)		
<i>female × misreported for themselves</i>		-0.796 (0.870)	-0.694 (0.875)		
<i>constant</i>	0.696*** (0.187)	0.659*** (0.190)	-12.150 (47.125)	-0.973*** (0.292)	2.880 (51.973)
controls ^a	no	no	yes	no	yes
obs.	144	144	144	144	144
pseudo R2	0.030	0.035	0.045	0.034	0.060
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 7: Probit regressions on women's increase in dishonesty for groups (*endo. vs. exo.*).

	<i>increased dishonesty for groups</i>					
	<i>wanted leadership</i>			<i>did not want leadership</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>exogenous leadership</i>	-0.604** (0.303)	-0.608** (0.304)	-0.569* (0.311)	-0.258 (0.409)	-0.284 (0.457)	-0.236 (0.470)
<i>(perceived) chance of becoming leader</i>		1.161 (1.804)	0.907 (1.839)		-0.198 (1.576)	0.056 (1.678)
<i>constant</i>	-0.751*** (0.191)	-1.205 (0.733)	-67.161 (95.018)	-1.097*** (0.335)	-0.994 (0.884)	-50.235 (112.163)
controls ^a	no	no	yes	no	no	yes
obs.	110	110	110	79	79	79
pseudo R2	0.044	0.048	0.072	0.008	0.008	0.071
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 8: Probit regressions on men's increase in dishonesty for groups (*endo. vs. exo*) Marginal effects reported.

	<i>increased dishonesty for groups</i>					
	wanted leadership			did not want leadership		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>exogenous leadership</i>	0.050	0.047	0.046	0.076	0.098	0.112
	(0.073)	(0.073)	(0.073)	(0.125)	(0.131)	(0.133)
<i>(perceived) chance of becoming leader</i>		0.237	0.237		0.293	0.340
		(0.354)	(0.359)		(0.439)	(0.455)
controls ^a	no	no	yes	no	no	yes
obs.	116	116	116	75	75	75
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.

Table 9: Probit regressions on men's increase in dishonesty for groups (*endo. vs. exo*).

	<i>increased dishonesty for groups</i>					
	wanted leadership			did not want leadership		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>exogenous leadership</i>	0.184	0.174	0.173	0.272	0.354	0.408
	(0.271)	(0.272)	(0.274)	(0.452)	(0.475)	(0.489)
<i>(perceived) chance of becoming leader</i>		0.883	0.885		1.057	1.240
		(1.324)	(1.344)		(1.599)	(1.673)
<i>constant</i>	-0.980***	-1.332**	19.202	-1.068**	-1.585*	-3.002
	(0.202)	(0.571)	(56.367)	(0.414)	(0.901)	(64.273)
controls ^a	no	no	yes	no	no	yes
obs.	116	116	116	75	75	75
pseudo R2	0.004	0.008	0.010	0.005	0.011	0.018
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 10: Probit regressions on the increase in dishonesty of women conditional on their leadership preferences.

	<i>increase in dishonest behavior</i>					
	all data		wanted leadersh.		did not want leadersh.	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>belief on ind. dishonesty</i>	0.513*** (0.148)	0.546*** (0.148)	0.703*** (0.181)	0.705*** (0.188)	0.218 (0.256)	0.324 (0.291)
<i>svo</i>	-0.002 (0.004)	-0.001 (0.005)	-0.001 (0.009)	0.000 (0.009)	-0.005 (0.006)	-0.005 (0.007)
<i>constant</i>	-2.746*** (0.576)	30.997 (87.678)	-3.581*** (0.756)	59.765 (103.182)	-1.467 (0.935)	-19.958 (206.610)
controls ^a	no	yes	no	yes	no	yes
obs.	129	129	84	84	45	45
pseudo R2	0.129	0.157	0.226	0.244	0.031	0.064

Standard errors in parentheses
*** p<0.01, ** p<0.05

Note: The regressions are clustered at the group level and report average marginal effects.

^a Controls: age and whether subjects study economics.

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 her 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 11: Overview of the lottery choices in part 1. Risk is measured as standard deviation of 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 to a list with 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).¹⁹ 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 is to 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 in 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. At the 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 in 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 accepted 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 up five two-digit numbers. An example of the real-effort task (a math problem to be solved) is presented in Table 12. Subjects have to enter the answer in the blank box. Having submitting 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	
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Table 12: Example of a problem in the real-effort task

In part four subjects work for five minutes in 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

¹⁹Extending the right vectors to (21, 21) allows us to account for negative β 's (Blanco et al., 2011).

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 compete 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 with 50 red and black balls. Whereas, they know that the composition of the ambiguous urn (Option B) is unknown. Before subjects are presented the choice list, they have to bet on a color (red or black). They are told that they receive the 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%²⁰ The switch point determines 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 - Preference elicitations (Study 2)

[The elicitation of risk preferences and competitive preferences was similar as in study 1.]

part two - elicitation of social value orientation

We elicit social value orientation with the ring measure of Liebrand and McClintock (1988). In this part subject are randomly matched with another participant and are subsequently presented to 32 pairs (“opportunity A” and “opportunity B”) of allocation decisions between themselves and the matched person. In each of the 32 decision situations, subjects either have to choose opportunity A or B. In these

²⁰Recall, that subjects bet on one of two colors.

cases subjects decide about points, which will be converted at an exchange rate of 1 point = €0.02, if the part becomes payoff relevant. For each pair, subjects have to trade off allocations, which differ in the degree of disadvantageous/advantageous inequality between the subject and the matched subject. At the same time, subjects are also matched with a different subject, who also allocates points to herself and the matched subject. Subjects know that if part two will become payoff relevant, they will earn the sum of the point, that they allocated to themselves plus the points, which were allocated to them by the second matching partner. If the part becomes payoff relevant, subjects are also informed on the total amount of points they received. Using subject's mean allocations for self and for the matched participant, shifting the base of the resulting angle to the center and taking the ratio between these means one can estimate the subject's motivational vector. This gives us the social value orientation angle of the subjects.