The effect of communication channels on dishonest behavior ☆

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A B S T R A C T

The present research investigates the effects of various communication channels on dishonest behavior. We rely on a simple truth-telling experiment (i.e., a repeated coin-flip) and let subjects report their outcome through communication channels that differ in distance and anonymity (face-to-face, in-lab telephone, in-lab web-form, and home web-form). We find dishonest behavior across all communication channels, with important treatment differences. Reporting of extreme outcomes that maximize payoff increases in distance and anonymity. To the contrary, partial lying decreases in distance and anonymity. Furthermore, we find gender to moderate the effects and women tend to drive these results. The findings have important implications for the design of real-world communication structures that are relevant when honest reporting is particularly relevant, for example in insurance claims, income reports for tax purposes, or applicant screenings in labor markets.

1. Introduction

A key topic for current behavioral and experimental economic research is the scope and intensity of individual lying behavior (e.g., Abeler Becker, & Falk 2014; Conrads et al., 2013, Föllmi-Heusi & Fischbacher, 2013; Mazar, Amir, & Ariely 2008). An often-applied paradigm was developed by Föllmi-Heusi and Fischbacher (2013) in which subjects are asked to secretly roll a die and report the outcome. This outcome is tied to a financial payoff, therefore creating a material incentive to untruthfully report it, for example by claiming having seen "5" instead of "2" as this leads to a higher payoff for the subject. Laboratory evidence suggests that lying is frequent. However, Abeler, Becker, and Falk (2014) report data from a representative sample using a similar coin-flip paradigm, which suggests that hardly any lying occurs. Subsequently, Abeler, Becker, and Falk (2014) address the issue in a laboratory study and – again – find evidence of lying. Our research is designed as a follow-up to this work to further scrutinize this finding.

Interestingly, this difference between experiments may emerge due to at least two different reasons. First, the subject pool in the representative sample was different from the subject pool used for the laboratory experiment. Thus, differences may occur due to structural differences in the subject pools, implicating that student participants report outcomes dishonestly more frequently than the general population. Second, the communication channel was (slightly) different and may have impacted the individual inclination to report one's outcome in otherwise similar populations. While the representative sample reported their outcome using the telephone, the laboratory study asked the participants to go to an adjacent room to call an experimenter while present in a laboratory and after having participated in another (unrelated) experiment. Although it was a close approximation of the telephone-experiment, the degree of anonymity and distance was different in the two settings as visual interaction between researchers and participants has occurred before or after the experiment.

Therefore, we aim to contribute to the behavioral and experimental economic literature by addressing the impact of various communication channels on lying behavior using the same subject pool across all treatments. Thus, we are able to identify differences in communication channels. Beyond this contribution to the behavioral economic literature our research has an applied focus as it is highly interesting for the design of real-world reporting tools. Within and beyond organizational settings, we communicate with others through various communication channels. Routinely, we have to decide whether to visit friends directly to ask a favor, to call them, or to simply message them using a computer or phone. Within organizational settings, decision makers have to decide which communication channel

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to rely on when organizing communication among employees, with consumers and suppliers, or with regulatory bodies. All instances rely on honest reporting behavior.

Our central research question is most easily explainable by an example: let us assume an insurance company which offers two methods of reporting a lost or stolen item, either via the telephone or via an online questionnaire. Typically, reporting a stolen item – let us use a bike for the remainder of this illustration – involves answering various questions. Has the bike been looked properly? Where was it stolen? When was it stolen? Quite critically, the answering pattern will determine if the insurance company reimburses the victim, depending on the terms of service. Therefore, a customer has a material incentive to lie about any of the fine-print to make sure that the insurance company pays. But is reporting behavior influenced by the communication channel?

Despite the increasing research interest in (dis)honest behavior and despite the high practical relevance beyond the stated example (e.g., online vs. offline screening questions for job applicants, online vs. offline dating, etc.), experimental economic research has rarely investigated the effect of the communication channel on people's behavior (cf. Brosig, Joachim, & Ockenfels 2003; Brosig, 2006; Valley et al., 2002). Our research is designed to fill that gap. In particular, we recruit all our participants from the same subject pool that almost entirely consists of students. We compare four different treatments that vary the communication channel with which the outcome of the random draw is reported: face-to-face (*F-t-F*), phone, computerized within the lab (*C-lab*), or computerized via an internet connection from home (i.e., outside the laboratory environment, *C-remote*) and our research is exploratory as the literature provides us with good arguments that would support various hypotheses.

For instance, as material incentives exist, dishonest reporting may be prevalent throughout all communication channels. To the contrary, if lying aversion (e.g., Gneezy, Rockenbach, & Serra-Garcia 2013) is sufficiently high, we should observe little dishonest behavior. However, if there is an aversion to straightforwardly lie into one's face (e.g., Williams, 1977, DePaolo, 1996), there could be observable treatment differences, showing that dishonest reports increase as a function of distance and anonymity of the communication channel. This pattern of behavior may also be supported by the belief how well one expects the research assistant to be able to detect cheaters (Frank & Ekman, 1997). Theories such as self-concept maintenance theory (Mazar, Amir, & Ariely 2008) are mute on differences in communication channels while a preference not to violate someone's expectations (Battigalli & Dufwenberg, 2007) might be elevated by direct and personal communication.

As many good theoretical explanations exist that may support various patterns of behavior, we explore subjects' behavior across various communication channels that are designed according to how much they reflect realistic communication channels outside of academic research. Thus, we contribute to the results presented in Abeler, Becker, and Falk (2014) by exploring in more detail how communication channels affect reporting behavior in the coin-flip paradigm. Our study also augments recent literature that shows some differences in behavior across various communication channels (Pascual-Ezama et al., 2015) in a cross-cultural study. The authors investigate dishonesty in 16 countries while also varying the distance between the sender (participant) and receiver (researcher) of the report (face-to face, written, or self-payments). The results indicate vast amount of honesty with some differences across the communication channel. Thus, our research critically augments the existing experimental results while holding constant the subject pool and only varying the communication channel.

2. Experiment

A total of 246 participants ($M_{age} = 24.06$, $SD_{age} = 3.96$, 49 % females) were recruited from the 2000-student subject pool of the University Duisburg-Essen using ORSEE (Greiner, 2004). The experiment itself was realized using the software BoXS (Seithe, 2012). Each participant had the following decision task. S/he could earn money by privately flipping a coin four times in a row. Each time a participant reports tails as the outcome of the coin toss, s/he receives 1 euro. As this method does not allow us to compare reported and actual outcome directly, the main dependent variable is the distribution of reported outcomes in each treatment, which is tested against the expected (equal) distribution. Accordingly, participants can earn an amount between 0 and 4 euros, plus a flat compensation of 7 euros for completing a post-decision survey that included demographics and a few survey questions assessing individual differences in personality (i.e., the German version of a short BIG 5 measure, see Rammstedt & John, 2007) and a questionnaire designed to assess personal values (i.e., the German version of PVQ5X, Schwarz et al., 2012).

No participant participated in more than one treatment. Treatments were collected in independent sessions to avoid that any participant was aware about different procedures in his or her treatment. Consistent with Abeler, Becker, and Falk (2014), we mainly chose the coin-flip task instead of the die-rolling paradigm (Föllmi-Heusi & Fischbacher, 2013) as it is more likely that subjects in the *C-remote* treatment have a coin readily available, which may not be the case for a set of dice.

The experiment included four treatments. The treatments differed in the communication channel in a way that we varied how distant communication was, using either no technology at all or increasingly "distant" or "anonymous" communication tools. Importantly, our experimental treatments are not perfect manipulations of social distance or anonymity. They have rather been chosen according to how well they reflect real-world communication channels. We do argue that the treatments become increasingly distant and increasingly anonymous (i.e., that they are a monotonic function of the two). In *face-to-face* communication (*F-t-F*), subjects report the number of tails directly to a research assistant in their cabins, who knocks on their doors after they have finished flipping the coin. In *phone* communication, the research assistant contacted the subject via phone (i.e., using the software *Skype*), for which each cabin was equipped with a headset and speaker. In *PC-lab* communication, participants entered their ostensible outcome via a web-form, which is transmitted to the research assistant. Finally, in *C-remote*, the subject faced an identical web-form, but could access the site via the internet from home. Participants in the *C-remote* treatment gave us their bank account information in the end of the post-experimental questionnaire and the money was directly wire-transferred after they had finished.

Naturally, there are some differences between online and laboratory experiments that we were not able to control or hold constant. Although unlikely, we cannot entirely rule out that participants completed the online-study with another person present. Furthermore, concentration levels may be lower at home (or elsewhere) compared to the laboratory as participants may have been distracted. As the main interest of the paper lies in the effect of different (realistic) communication channels on dishonesty, we nevertheless opted to include the *C-remote* treatment despite these uncontrollable influences on behavior.

3. Results

Dishonest behavior was prevalent in all experimental treatments (see Fig. 1). The distributions of reported outcomes in all four treatments are significantly different from the truthful distributions (based on Kolmogorov-Smirnov tests, all *p*'s < 0.01, confirmed by binomial tests). Despite the tendency to report dishonestly across all treatments, we find interesting differences in the treatments in line with what we expect.
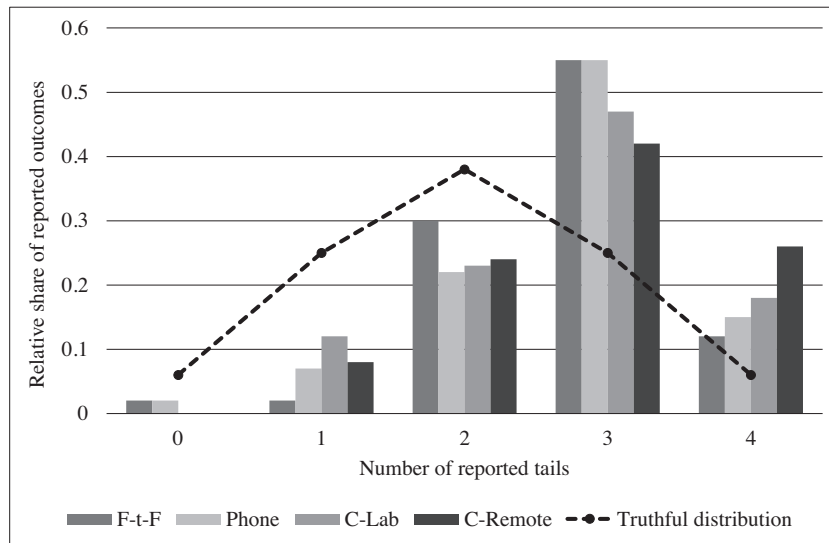
Fig. 1. Relative share of reported outcomes by treatment. Notes: Based on $N = 246$ observations.

Table 1
Overview of results.

Treatment	n	M	Reported outcome (relative frequency)				
			0	1	2	3	4
F-t-f	60	2.85	0.02	0.02***	0.3	0.55***	0.12
Phone	60	2.89	0.02	0.07**	0.22***	0.55***	0.15**
C-lab	60	2.80	0.00*	0.12**	0.23**	0.47***	0.18***
C-remote	66	2.86	0.00**	0.08***	0.24**	0.42**	0.25***
Honest distribution		2	0.06	0.25	0.38	0.25	0.25
JT Test			0.097	0.065	0.729	0.051	0.017

Note: n displays the number of observations in each treatment, *M* denotes the mean reported number by treatment, and reported outcomes are represented as a share of total outcomes by treatment. *** indicates difference from honest distribution at 1% level, ** at 5% level, * at 10% level, based on one-sided binomial testing. JT tests refers to the one-sided *p*-values of the Jonckheere-Terpstra test for ordered alternatives by reported number with treatment as the independent variable.

Reported outcomes of "4" are particularly interesting, as this implies the highest possible payoff for participants. One possibility is that they are more readily willing to report extremely beneficial (yet unlikely) outcomes as the "distance" or "anonymity" in the communication *increases*. And indeed, using a Jonckheere-Terpstra test for ordered alternatives, reports of the payoff maximizing outcome significantly increases with "distance" and "anonymity" in the communication channel ($p = 0.017$, one-sided)[1]. Pairwise comparison qualified this result, showing that *F-t-F* and *C-remote* significantly differed from each other (12 % vs. 26 %, $p = 0.02$, χ^2 test, one-sided), while the other communication channels fell between the two (for a summary, see Table 1). No other statistical differences exist between treatments, based on pairwise χ^2 tests.

Exploring this result more closely, the effect is qualified by an interesting pattern suggesting a gender-x-treatment interaction depending on distance from the lab. Using probit regressions (Table 2, model 4–6) that include the treatment variation (remote vs. in lab) as well as gender as independent variables, we find that the main effect ($p = 0.056$, model 4) of the treatment is no longer significant ($p = 0.891$), but instead find a significant gender-x-treatment interaction ($p = 0.056$, model 5). To compare the effect of distance more accurately in supplementary regressions (Table 2, model 6), we included only the computerized treatments that differed in location (*lab* vs. *remote*). This analysis also shows a significant gender-x-treatment interaction ($p = 0.025$), suggesting that the over-reporting of 4's in increasingly distant communication is driven by women's higher propensity to report "4" in high distance (see Fig. 2).

Next, we turn to partial lying, which can be conceptualized by reporting outcomes that are dishonest, yet not to a payoff maximizing extent (Föllmi-Heusi & Fischbacher, 2013). We find that partial lying is more prevalent as the channel of communication is *less* anonymous. Although only marginally significant, subjects are more inclined to report "3" as the distance and anonymity decrease (JT test, $p = 0.051$, one-tailed). Again, comparing *C-lab* and *C-remote*, this effect seems to be more pronounced for women than men, as indicated by a marginally significant interaction term ($p = 0.08$, Table 2, model 7), women are more likely to report "3" in the lab, while men's behavior does not differ. As a consequence, this general over-picking of "3's" and "4's" also mirrors in marginally significant relative under-reporting of "1" (JT test, $p = 0.065$, one-tailed) and "0" (JT test, $p = 0.097$, one-tailed).

Finally, we analyzed potential differences in other demographic variables besides gender as well as broad psychological traits. Age did not have an effect on behavior; neither did religiousness, income, risk attitudes, or any BIG 5 personality trait. We did find a significant negative correlation between conformity (i.e., an individual's propensity to stick to norms and reporting of low numbers, see Table 2, model 3).

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[1] We relied on a Jonckheere–Terpstra test to account for our assumption that our treatment variations are ordered in terms of distance and anonymity. The subsequent χ^2 tests show which treatments drive this effect. Importantly, our research aimed to test if dishonest behavior is an increasing function of distance and anonymity. A post-hoc power analysis suggests that our sample was powered to address this hypothesis, but underpowered to find treatment differences in each treatment.

Table 2
Overview of regression analyses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Phone	0.102	0.072	0.057	–	-	-	-
	(0.303)	(0.307)	(0.312)				
C-lab	0.014	−0.006	−0.001	-	-	-	-
	(0.330)	(0.332)	(0.349)				
C-remote	0.335	0.337	0.232	-	-	-	-
	(0.333)	(0.337)	(0.341)				
Lab vs. remote				0.386*	0.038	−0.240	0.291
(1 if yes)				(0.202)	(0.276)	(0.335)	(0.316)
Age	-	−0.004	-	-	-	-	-
		(0.031)					
Gender	-	−0.083	-		−0.545**	−0.977**	0.684**
(1 if female)		(0.242)			(0.239)	(0.427)	(0.331)
Treatment x gender					0.785*	1.217***	−0.798*
					(0.411)	(0.542)	(0.455)
Conformity	-	-	1.207**	-	-	-	-
			(0.415)				
Observations	246	246	246	246	246	126	126
Pseudo R^2	0.002	0.004	0.014	0.015	0.041	0.055	0.027

Notes: Models 1-3: Ordered logit estimates with reported outcome as dependent variable. Robust standard errors in parentheses. Reference group is treatment F-t-F. Phone, C-lab, C-remote are dummy coded for the respective treatments. Models 3-6: Probit regressions with reported "4" as dichotomous dependent variable. Model 7: Probit regression with reported "3" as dichotomous dependent variable. Significance is presented at the 1, 5, and 10 percent level and denoted by ***, **, *, respectively.
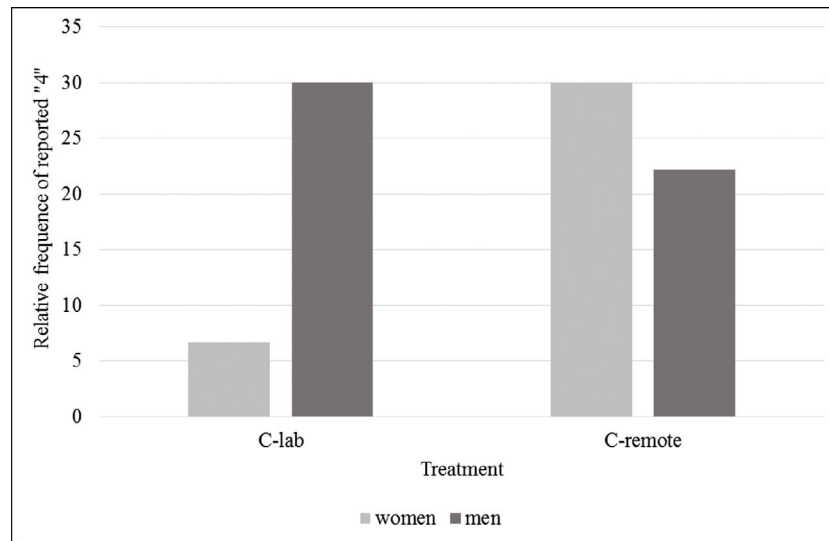
Fig. 2. Gender times treatment interaction in C treatments (lab vs. remote). Note: Based on 126 observations in C treatments.

4. Discussion and conclusion

The present research investigated the impact of different communication channels on dishonesty using a coin-flip task. While dishonesty was prevalent in all communication channels, *increasingly* distant and anonymous communication increases dishonesty with respect to extreme, payoff maximizing responses, while partial dishonesty (i.e., slightly overstating one's outcomes) is more prevalent in *decreasingly* distant and anonymous communication. Thus, individual "lying costs" may indeed be affected by social distance concerns (Bohnet & Frey, 1999; Hoffman et al., 1996) in a similar way that social preferences are affected by social distance (e.g., Charness & Gneezy, 2008).

Interestingly, gender seems to be an important variable as it moderated the effect of over-reporting "4" and "3". Noteworthy, this finding seems to match well into the recent literature that suggests that women are more sensitive towards experimental manipulations across several domains of social behavior, for instance in social dilemmas (Ellingsen, Johanesson, & Mollerstrom 2013) or dictator games

(Lotz, 2015). The analysis of other demographic and psychometric variables suggests that individual values of conformity are a buffer against dishonesty, but we did not find any other moderators besides gender.

In total, the results inform previous findings that could not clearly disambiguate these differences in dishonest reporting between differences in the subject pool and differences in the communication channel (Abeler, Becker, & Falk 2014). Thus, our research provides an answer to an important question for behavioral economic research, suggesting that communication channels are a critical feature for individual responses and that women might be more responsive to differences in communication channels.

4.1. Shortcomings and directions for future research

There are still a number of questions that need to be answered, both regarding the current results as well aspects that need to be addressed in future research. Importantly, the non-finding about different distributions may be due to limited statistical power. While

we are able to infer differences in certain types of behavior (i.e., extreme lying), our results are mute on potential differences in distributions. Potentially, the differences in distributions are quite small so that one needs a very large sample to identify significant treatment differences.

Another shortcoming is that we could not assign participants to treatments in a fully random way. Because laboratory administration software always requires an announcement whether a particular experiment is done in the laboratory or online before sign-ups, participants know the location when registering for an experiment. Noteworthy, all participants in the online-treatment are also generally registered (i.e., receive invitations) for experiments in the lab and vice versa. We believe that this does not present a serious problem as the treatments involved participants from the same pool and the topic of the experiment was unknown at the point of registration. Furthermore, the online treatment did not over-represent subgroups in terms demographic variables (e.g., gender, focus of studies, or religiousness) that may relate to reporting of dishonest behavior. Although unlikely, it cannot be completely ruled out that there are unobservable variables that underlie the differences. Research suggests that – in general – online experiments are comparable to offline experiments (Amir & Rand, 2012; Buhrmester, Kwang, & Gosling, 2009) and, thus, the effects are likely attributable to differences in the communication channels. Ideally, participants would learn after sign-up whether their session is in the laboratory or via the internet.

There are several directions that future research could take. First, one could investigate whether individuals also strategize about the communication channel and choose their preferred communication channel to allow or hamper cheating. For instance, they could deliberately choose a communication channel that makes it relatively easy to cheat, for instance distant communication via computerized messaging. On the other hand, if one fears to be the victim of dishonesty, they could prefer non-anonymous communication channels, for instance a personal meeting or video-conference. Although some research suggests that humans generally perform poorly in predicting characteristics like cooperativeness or trustworthiness (Olivola & Todorov, 2010; Porter, England, Juodis, ten Brinke, & Wilson, 2008; Zebrowitz, Voinescu, & Collins, 1996), there seem to be important individual differences in the ability to detect cheaters. In addition, this strategic selection of communication channels could extend to instances of social preferences, apart from public goods (Brosig, Weimann, & Ockenfels 2003; Brosig, 2006). Research around reluctant altruism (Cain, Dana, & Newman 2014; Dana et al., 2006; Lotz et al., 2013) would suggest that reluctant altruists would choose communication channels that allows a lower degree of altruism or pro-social behavior towards others, for instance in cases of donations.

Another interesting line of research could address whether the effect of the communication channel is also prevalent in samples that more closely represent the general public. Finding a positive result of the communication channel does not automatically imply that there are no differences in dishonest behavior between student samples and the general public. Even if students and the general public score similarly in their innate preference for truth-telling, the mere familiarity with participation in economic experiments may drive some behavior, as has been suggested by research on the role of naivety on treatment differences (e.g., Amir & Rand 2012).

Our results can also be interpreted in terms of social vs. moral norms as recently put forward by Schram and Charness (2015). The authors disambiguate social and moral norms and argue that social norms are extrinsically driven while moral norms are intrinsically motivated. Clearly, increasingly distant and anonymous communication affects relative importance of social and moral norms. While moral norms might present a buffer against dishonest behavior independent of the social nature of the interaction, social norms might guide individuals' behavior more strongly in direct communication. Future research could systematically vary the balance between the two and measure how this affects dishonest behavior and other behavioral outcomes.

Finally, while our research varied treatments based on real-world communication channels that vary in distance and anonymity, it did not systematically vary distance and anonymity as such. Therefore, future research could investigate the isolated and combined effects in a full factorial design to see how the two elements affect dishonest reporting of outcomes. The focus of this research could be to exactly disentangle the psychological mechanisms while discarding the applicability of the communication channel in real-world contexts.

4.2. Conclusion

To sum up, our research suggests that the communication channel matters with respect to dishonest behavior. While extremely remote communication increases extreme dishonesty, decreased distance may elevate to partially dishonest behavior. As in the previous literature, our research suggests dishonesty to be much more nuanced than one would think and more than a dichotomy between fully dishonest vs. fully honest. There are several interesting real-world applications for this finding. If individuals respond differently to various communication channels it affects a "neutrality" assumption of communication. Thus, decision makers in organizations need to take into account that their design choice affects behavior and might lead to detrimental outcomes effects (i.e., increased dishonesty) despite being well-spirited (i.e., reducing bureaucracy). Thus, this research also relates to the emerging field of behavioral decision design (e.g., Thaler & Sunstein, 2008) that highlights the importance of subtle differences in the choice architecture that can promote (e.g., Ebeling & Lotz, 2015) or hamper (e.g., Brown & Krishna, 2004) an organizations', an individual's, or society's goals.

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