Honesty in economic experiments

Johan Birkelund

A dissertation for the degree of philosophiae doctor

School of Business and Economics
UiT – the Arctic University of Tromsø

June 2020

Summary

This thesis investigates preferences for honesty in economic experiments. Preferences for honesty have attained a fair amount of attention in the last couple of decades, from multiple areas within the social sciences, including economics. This thesis may serve as an introduction to the field. First, it provides an introduction to experiments in economics, before it dives into how economists conduct research on honesty. The literature in the thesis provides background to the three research papers reported in the last chapter. Through different experimental tasks for studying honesty, this thesis provides new insights to several subfields of the literature. The first paper investigates how differences in equality affect preferences for honesty and generosity through a real effort dictator game, allowing for dishonest reports in the production stage. Comparing behavior across treatments, we find that when participants know they are at a relative disadvantage, they are significantly more dishonest and significantly less generous. Results speak to the literature on moral balancing, self-justification for dishonest behavior and generosity. The second paper combine a survey instrument designed to measure individual cultural worldviews and a coin-flip honesty task. The data fail to support the conjecture that adherents of certain worldviews are more honest on the aggregate level. However, when data is disaggregated by gender, the worldviews help in explaining the observed gender effect in the data. Results suggest that cultural worldviews may help provide a more nuanced understanding of the gender differences generally observed in the honesty literature. The final paper of this thesis investigates whether the choice of payment scheme affect the level of honesty in a real-effort honesty task. Through both a laboratory experiment and a meta-study, the paper finds no evidence of difference between the schemes. This result is in contrast to a study reporting such an effect in an honesty experiment using random outcomes.

Acknowledgements

First, I would like to thank my supervisors Stein Østbye and Jan Ynge Sand for their fruitful comments, letting me find my own way, and for bringing me along to the University of Alaska Anchorage in the fall of 2015. During this trip, I met Todd L. Cherry, who later became my mentor and co-author, and has played a pivotal role in me becoming a better economist and experimentalist. I also want to express deep gratitude to Eirik E. Heen, Agne Kajackaite and Philip Gerlach for many insightful discussions about experimental economics in general, and work related to this thesis especially. I also need to thank Øystein Myrland, for sound advice in all things statistics; you've treated many of my headaches by quickly answering my rambling e-emails around the clock. Without the support, comments and discussions with the aforementioned superstars, this thesis would not be nearly as good.

Last but not least, I want to extend my gratitude to my life partner, Ida, for always supporting me, and staying with me through ups and downs in this rollercoaster of a project. You inspired me to pursue a Ph.D., and for that I will always be grateful.

Table of Contents

Papers included in this thesis:

Paper 1:

Birkelund, J. and Cherry, T.L., 2020. Institutional inequality and individual preferences for honesty and generosity. *Journal of Economic Behavior & Organization*.

Paper 2:

Birkelund, J., (2020) *Honesty and Cultural Worldviews.*

Paper 3:

Birkelund, J., (2020) *Cash or Report – comparing two payment schemes in a real effort honesty task.*

1. Introduction

Many situations in life involve asymmetric information, which in turn allows for misreporting of such information, in return for economic benefits. Classical economic theory (e.g. Becker, 1968), suggests that whenever an economic agent faces a decision where she can misrepresent private information for private gain, she will conduct a cost benefit analysis weighing the benefit of misreporting against the risk, and possible sanctions, from being detected. If the utility maximization problem consists of these two parameters only, we should expect people to be dishonest in all situations where there is no chance of detection. Indeed, immoral and dishonest behavior is prevalent in all walks of life. Reports of misconduct for private gains are often cited in the media. From corporate scandals like Enron and the Volkswagens diesel case, politicians subjecting themselves to corruption, teenagers downloading copyrighted materials and dishonest employees taking advantage of their employers for personal gain. The Association of Certified Fraud Examiners estimated that the occupational fraud cost of U.S. organizations alone, accounted for 7% of their annual revenues (ACFE, 2008). Naturally, the loss of efficiency due to dishonesty has drawn the attention of researchers within several distinct fields. In this research, experiments within economics, and social psychology, have proven especially prolific. With strict rules of incentives, and no tolerance for deception, experimental economics is well suited for investigating preferences for honesty. Interestingly, this emerging field of research has found that individuals are not as dishonest as one would expect from the classic model, and a recent meta-study even imply that participants in experiments leaves most of the available funds on the table (Abeler et al., 2019). An important part of the emerging literature on honesty has been focused on understanding why we are not observing dishonesty to the degree that traditional economic theory predicts. Through controlled laboratory experiments, and online survey and a meta-study, this thesis contributes to the literature.

The remainder of this thesis is organized as follows: part 2 of the thesis, provides an overview of how economists make use of experiments, and to some of its early findings,

relevant to this thesis. Part 3 gives an introduction to experiments in honesty, how such experiments are conducted, and gives an introduction to theories of why we are honest and provides background for the papers included. Part 4 presents the research papers included in the dissertation.

2. The experimental method

Since the economy encompasses vast amounts of agents, information, and potential confounds, economics was for a long time considered a non-experimental science. Due to the vastness of potential confounds, econometricians have for long grappled with identifying proper counterfactuals (List et al., 2011). The identification problem arises since no individual can be observed in two states at once. One can only observe an agent's action, either under one set of conditions or treatment, or, in absence of said conditions. However, starting in the latter part of last century, experiments where increasingly used to study, and understand, the behavior of economic agents. Since laboratory experiments allow for strict control over relevant confounding factors, hence, rendering the researcher control over the data generating process, economists can use experiments to pinpoint causal inference. When the researcher is in control of the data generating process, one can make sure that only one group of subjects receive the treatment, and as long as subjects are properly randomized between those receiving the treatment, and a control group is not receiving the treatment, the identification problem is a lot less severe (List et al., 2011).

At a very basic level, economic experiments consist of three things: an environment, an institution and economic agents. Within the experiment, the researcher governs the institution, which dictate the rules and choice sets of the experiment, creating the environment where agents make their decisions. In addition to the rules, commonly provided through thoroughly written instructions, agents are often endowed with financial means. The agents, all with their own individual preferences over outcomes, then act to the environment, and are paid based on their decisions.

2.1 Experimental Design

At the heart of any experiment lies the ability to identify causal effects. According to the Oxford dictionary, causality is defined as "the operation or relation of cause and effect". If follows, that for an effect to be causal, it has to be directly tied to the cause. As long as researchers maintain control over all relevant factors in an experimental environment,

changing only one parameter between treatments, one can make ceteris paribus comparisons, ensuring that the effect measured is caused by a change in the given parameter. The effect of experiments, are often measured as an average treatment effect. It is assumed, that each individual in a treatment has her own treatment effect, and all individual treatment effects are assumed to randomly vary around an average. Since one cannot observe an individual both receiving, and not receiving a treatment, we are interested in the average behavior of those receiving the treatment. It follows, that since we are interested in average effect size, one need to make sure, that all participants are equally likely to receive a treatment. If participants holding some observable or non-observable characteristic, is more likely to receive the treatment than not, one cannot be sure whether an effect observed in the experiment is due to the treatment, or the characteristic. Economic experiments also follow a strict set of rules, two of which will be discussed in this thesis, incentives for the participants and deception[1].

Incentives

Since economists are interested in how economic agents act in the face of incentives, it is important that when testing theory in an experiment, participants are deciding over real payoffs (Croson, 2005). The preferred reward medium used to incentivize in economic experiments is money. According to Friedman et al. (2004), three conditions have to be met for a reward medium to offset any innate characteristics of the participants, namely, monotonicity, saliency, and dominance. First, since more money is better than less, it meets the monotonicity requirement. Second, as long as there is a clear link between the decisions made in the experiment and the associated rewards, as defined by the institution (or rules of the experiment), money is salient. Third, changes in participants' utility have to primarily be caused by changes in rewards. To summarize, when participants are paid based on their decisions in an experiment, we assume that they maximize their utility according to their individual preferences. In addition, Camerer and Hogarth (1999) argue that variance in decision tasks are reduced when participants have financial incentives.

[1] A full framework of how to run experiments in economics is beyond the scope of this thesis. For readers interested in the rules and guidelines of experiments in economics, please see Croson (2005) and List et al. (2011)

Providing incentives is (almost) always required in economic experiments (Croson, 2005).

Deception

Deception is strictly prohibited in economic experiments. This includes all forms of deception, like information about other participants, payoffs associated with decisions and the purpose of the experiment. Croson (2005) argues that breaking any of these forms of deception can result in inferior test of the underlying theory. If participants don't trust the experimenter, their behavior may be affected, providing uncertainty about their decisions. The worst-case scenario from deceiving participants is that they may no longer trust experimental instructions in the future.

To summarize, researchers are interested in the average treatment effect in the experiments. By randomizing subjects between treatments, one can infer causal relationships. Amongst several rules in economic experiments, two are particularly central when researching honesty in experiments. Participants in honesty experiments make decisions over real income, and since they can rely on all information given, they should not expect sanctions if they misreport or act dishonestly, unless it is clearly stated in the instructions that they can be audited.

2.2 Early discoveries

For a long time, economists made use of some extremely useful and simplifying assumption about individuals' behavior. One of the main assumptions behind utility maximization was that individuals are purely self-interested. In an early attempt to understand whether these assumptions were true, Güth et al. (1982) developed the Ultimatum Game (UG). In the UG, subjects are matched in pairs of two. One of the subjects, the Sender, is asked to allocate a fixed sum between herself and the other subject, the Receiver. After the Receiver is presented with the sum allocated, she has the decision of either accepting the allocation, or rejecting it. Should the Receiver choose to accept, both players keep the sum allocated by the Sender. If the Receiver rejects the offer, both players receive 0. Now, if the Sender is purely self-interested and only motivated by monetary payoff, and she believes the same is true for the Receiver, she

should offer the Receiver the minimum sum possible. The Receiver, only caring about her own monetary payoff, would be happy to receive free money, and accept the offer. This is not what Güth et al. (1982) observed. They seldom observed the minimum offer, which clearly contradicts the predictions based on the simplified assumptions about economic behavior. This led researchers to the next natural question, why are we observing this? Are the offers from the Senders motivated purely by fear of rejection? This question led to a new game, the Dictator Game (Forsythe et al., 1994; Kahneman et al., 1986). This game is played as a pure allocation game. The main idea is the same; the Dictator receives a fixed sum to allocate between herself and the Receiver. It differs from the UG in the sense that it is a pure allocation decision by the Sender; the Receiver can no longer reject the offer. Since offers can no longer be rejected, a purely self-interested monetary payoff-maximizing agent should now offer 0 to the Receiver. Again, this is not what is observed in the literature. Indeed, Dictators tend to offer less than in the UG, but the offers are clearly higher than what classical economic theory would predict[2]. It seems that the utility maximization problem of both Senders and Dictators, include something more than their own monetary payoff. These classic games sparked a whole field of new experiments and theories regarding social norms, and social preferences, and by now it is well established that such preferences contribute to the utility maximization of individuals (Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Fehr and Schmidt, 1999). With this in mind, we return to the cost benefit analysis of dishonest behavior (Becker, 1968).

[2] Both the Ultimatum- and Dictator-game have been replicated ad nauseam with similar results. For a review, see Oosterbeek et al. (2004) and Engel (2011).

3 Honesty in Economic Experiments

Becker (1968) postulated, that the decision to act dishonestly was a pure cost/benefit analysis of the potential gains of acting dishonestly, versus the cost (possible sanctions) one was facing if being caught. If the benefits outweigh the cost, we should expect individuals to be dishonest. Bear in mind, that any form of deception is clearly prohibited in economic experiments. This entails, that, unless it is clearly stated in the experimental instructions, the researchers have no means of sanctioning participants. If the individual decision to act dishonestly only involves considerations of monetary benefit and its potential cost, we should expect that participants in experiments, when given the opportunity, should dishonestly inflate their earnings to the maximum extent. Through numerous experiments, this is not what is observed in the literature, where subjects forego about 70% of the potential gains (Abeler et al., 2019; Gerlach et al., 2019). This suggests that in addition to any pecuniary motives, individuals have some preference for being honest. Similar to how social preferences contributed to the utility maximization problem in section 2, the problem, in respect to honesty, now also involve some cost of being dishonest.

In the following two subsections, we will first look into some of the key theories for preferences for honesty, before outlining the most widely used experimental paradigms for research on honesty. The focus of these theories and paradigms will be those most relevant for the research papers included in this thesis. The last subsection of this part, will introduce some literature relevant for the papers presented in section 4.

3.1 Why are we honest?

As we have previously demonstrated, economic decisions are not solely based on pecuniary motives. We know, that individuals hold some preferences over the welfare of others, which the literature indicate stems from an inherent aspiration to adhere to social norms, such as fairness or reciprocity. Since individual behavior in simple distribution games, such as the ultimatum game and dictator game, is affected by social norms, it is intuitive to assume that these norms also play a role in decisions involving dishonest behavior (Weibull and Villa, 2005). The literature makes an effort to explain why

individuals are honest, and several theories have been proposed. The surprisingly (to some) honest behavior of individuals, indicate that we have some intrinsic cost associated with dishonesty. Misreporting private information to increase ones own payoff, even if the chance of detections is zero, comes at a cost. In a simple extension to the classic economic theory, where individuals only care about increasing their income, several frameworks have included an intrinsic cost of dishonesty in the utility function (Abeler et al., 2019). Abeler et al. (2019) outline three broad interpretations of why people are so honest: either lying entails a cost; they are concerned about their reputation associated with dishonesty; or adhere to some social norm or social comparison which affect their decisions. In the following, this thesis will seek to explain some of the theories most relevant to the research presented.

3.1.1 The Models of Moral Balance, and Moral Self-Concept Maintenance

In the early nineties, the moral balancing model was introduced by Nisan (1991). His model suggests that individuals have some conception of their own moral ideal, which is based of all past relevant moral actions within a given time frame. This moral ideal, serve as a lower bound of how immoral one can act. Further, Nisan postulates that individuals strive to maintain this moral ideal over time, which indicates, that if you have been good and done morally right actions in a previous time period, you may allow yourself to do something self-serving in another time period, as long as you keep moral ideal above your lower limit. Similarly, Mazar et al. (2008) postulate a theory of moral self-concept maintenance, where moral behavior is a result of individuals' self-concept and moral identity. As long as individuals do not have to update their perceived moral identity, they can allow themselves acts of dishonesty. This insight has been pivotal in explaining why we are observing a high degree of partial dishonesty in experiments.

3.1.2 Self-Serving Justifications

Shalvi et al. (2015) propose a framework of self-serving justifications, where individuals' preference for honesty is shaped by their ability to justify their actions for themselves. In line with both Mazar et al. (2008) and Nisan (1991), Shalvi et al. acknowledge that individuals are conflicted between temptations to dishonestly increase

their income, while maintaining their own perception as being honest. He further proposes that individuals employ different internal self-justification strategies, which drive individual decision-making, and may debilitate psychological costs of acting dishonest. These self-justification strategies may occur both before and after an action. Pre-violation justifications may be used to justify immoral behavior before they occur, and post-violation justifications can be used to mitigate discomfort related to dishonest behavior. Common for both justifications, is that they allow the individual to act dishonest, without changing their moral self-image. As an example, when Shalvi et al. (2011) told subjects to roll a die three times, instead of once, and report the first outcome, they were more inclined to report a high outcome. It is likely that subjects bend the rules, that is, report the higher of the three outcomes rather than the first, employing a self-justification for dishonest behavior.

Paper 1 of this thesis contributes to both the literature of moral balancing and self-justification. We investigate how unequal treatment can provide participants in our experiment with a self-justification strategy for being dishonest. Participants first earn their income in a real-effort matrix task, allowing for dishonest reports, before they play the dictator game with their payment. This design allows us to first investigate how unequal treatment affect honesty, before shedding light on whether dishonest individuals seek to restore their moral balance by offering some of their earnings to anonymous recipients.

3.2 Experimental Paradigms in Honesty

In principle, there are three main paradigms of experimental tasks used within the honesty literature. We have Sender-Receiver games (e.g. Gneezy, 2005), real effort tasks (e.g. Mazar et al., 2008), and experiments with random outcome (e.g. Bucciol and Piovesan, 2011; Fischbacher and Föllmi-Heusi, 2013). At the core, all experiments in honesty involve (at least) one player with private information, which can be misrepresented to another player, or to the experimenter, in order to increase ones payoff. The paradigms differ in the sense that some allow for individual level data, some entail

strategic considerations and some allow for partial dishonesty. In the following, I will present each of these paradigms, and explain their main differences.

3.2.1 Sender-Receiver games

In Sender-Receiver games (e.g Gneezy, 2005) participants are usually matched in pairs. One of them, the sender, observes some state of the world, where one is more profitable for the sender and the other more profitable for the receiver, and is asked to send a message about which state is observed to the receiver. Which of these states the sender communicates to the receiver is up to the sender. She has the option to send a false message, which is more profitable for herself, or a true message, which is more profitable for the Receiver. In essence, the Sender faces a tradeoff between sending a costly true message, or a false but more profitable message. After receiving the message, the Receiver acts on the message, and both players receive the associated payoff. This game is usually played as a zero-sum game, where the gains for one player are the loss of the other. Since the Receiver acts on the message from the Sender, and choose which state she thinks is the true state, this game entails strategic considerations.

In this game, the researcher has knowledge about the true state for every decision, and which message is being sent. This gives the researcher individual level data on honesty. A potential limitation of this design is that the decision to act dishonest is entirely dichotomous; the message can be either truthful or dishonest, it does not allow for partial dishonesty. Also, since this game is a zero-sum game, the senders' decision may be more influenced by social preferences, than in games where the decision bears no direct cost for another participant. Given the element of strategic interaction, the motivation for dishonest behavior may be affected by strategy, and thus introduce potential confounds; this thesis will not include any experiments using Sender/Receiver games.

3.2.2 Random Outcome.

In experiments with random outcomes, the literature use one of two experiments. Either a die-under-a-cup (Bucciol and Piovesan, 2011), or a coin-flipping task

(Fischbacher and Föllmi-Heusi, 2013)[3]. In die-under-a-cup, the participant is given a six-sided dice, and is asked to roll it in private, usually under a cup with a peeking-hole on the bottom to allow for perfect privacy. Usually, each eye on the die represents different payoffs, where one of the outcomes pays zero (e.g. 1 = $1, 2 = $2, 3 = $3, 4 = $4, 5 = $4, 6 = $0). The number reported by the participant bears no cost for other players, only for the researcher, and is usually interpreted as a non-zero-sum game. Unless the die-roll is embedded in a software, or survey, the researcher has no way of observing the outcome. This means that the collected data is at the aggregate level, and any dishonest behavior has to be derived from a theoretical prediction, or by comparing outcomes to a group where the outcome is observed (not allowing for dishonest reporting). Also, since the outcome of a die-roll is non-dichotomous, this design allows for partial dishonesty. A participant rolling 2 on the dice may report that the outcome was 4, and thus be dishonest, although not to the maximum extent (which would be reporting 5).

In the coin-flip task, the two outcomes are associated with different payoffs (e.g. heads = $1, tails = $0). As in die-under-a-cup, this game entail no other player, and is therefore considered a non-zero-sum game, without any strategic considerations. Also, the researcher usually has no means of observing the true outcome, and the data has to be analyzed in the same way as for the die-under-a-cup. In the nature of a coin-flip with two outcomes, the decision to act dishonestly will be dichotomous, either the participant report truthful or not. Since the outcome is dichotomous, half of the participants will receive the positive outcome by chance. Assuming that no participants will be dishonest in order to *decrease* their income[4], only half of the participants are presented the opportunity to dishonestly report their outcome. An alternative approach is to provide participants with multiple coin-flips making the outcome continuous (e.g. Abeler et al., 2014; Balasubramanian et al., 2017; Cohn et al., 2014).

Since tasks with randomly generated outcomes, like die-rolls and coin-flips, are

---

[3] Although this study was published in 2013, they are considered the originators of this paradigm. Their working-paper was made available in 2008.

[4] Although this behavior has been observed in a study on Franciscan nuns (Utikal and Fischbacher, 2013), it is reasonable to assume it is seldom the case.

generally carried out in private, the true outcome is impossible to observe by researchers, and dishonest behavior has to be determined statistically. One way to avoid this, and thus obtain individual level data, is to embed the random process into the experimental software or survey, such as letting subjects watch a video of the given random process in private, and then report the outcome. If the software or survey store information about which video the subject watched, one would have access to individual level data, however this may affect the results[5]. In a recent paper, Lilleholt et al. (2020) tested whether there were a difference in honesty between computerized and non-computerized random process experiments. Their results reveal that when the random process is implemented within the survey framework, and thus theoretically could be observed in real time by the researchers, subjects reported more honestly.

3.2.3 Real Effort Tasks

Real effort tasks refer to any task where participants are paid by exhorting effort in solving a given problem[6]. This can be simple mathematical search matrices (e.g. Mazar et al., 2008), or visual inspection tasks (Mazar and Zhong, 2010). In the matrix-task, participants are usually presented with a set of matrices consisting of 12 three-digit numbers, and given the task to search for two different numbers within each matrix that sum to exactly 10.00. Similarly, in visual inspection task, participants are presented with a set of boxes each containing 20 dots, with a diagonal line separating them. The task is to decide which side of the diagonal contains more dots. Common for both of these tasks, is that participants are paid a piece price per correctly solved task (e.g. $1). Participants are given a time limit to search all matrices/boxes, and when the time is up, they are paid according to their self-reported performance.

---

[5] A design like this could be interpreted as using deception, however, in an experiment embedding a random-outcome, Hermann and Mußhoff (2019) argue that participants could conclude that the true outcome and the reported outcome were observed. Hence, it would not be deception.

[6] Real-effort tasks are commonly used in economic experiments. For a review, see Charness et al. (2018).

Real-effort tasks entail no strategic considerations. If the researcher codes all materials given to the participant, the researcher will have access to the tasks after the fact, and thus have individual level data. If not, data obtained from these tasks can be compared to a control group where participants are not given the opportunity to dishonestly inflate their performance, and analyzed on an aggregate level. Since participants solve more than one problem, real effort tasks allow for partial lying, and as such have a continuous measure of honesty.

There are two commonly used payment-schemes in real effort tasks. Most commonly, subjects self-report the number of tasks solved to a research-assistant in private, and is paid in cash according to their report. Alternatively, participants are given an envelope containing cash corresponding to the number of matrices. After the time is up, and subjects have counted the number of matrices solved, they pay themselves from this envelope in private, before leaving the lab. The difference between these payment-schemes have recently been interpreted as entailing different moral considerations (Hermann and Mußhoff, 2019), and has been reported to affect the level of honesty in an experiment with random outcome. However, no effort has been taken in the literature to differentiate between the two in experiments with real-effort, despite the fact that both payment-schemes are commonly used. As far as the author knows, results from experiments using either payment-scheme have been interpreted in the same way, as preferences for honesty. Herein, Paper 3 of this thesis seeks to shed light on whether the choice of payment-scheme has an effect on the level of honesty in the matrix task, through an experiment and a meta-study.

3.3 Culture

A debated topic of the literature is whether cultural differences play a role in preferences for honesty. A common approach for studies on cultural differences is to compare behavior across different geographical locations. Results from research on cultural differences in cheating tasks have found mixed evidence of cultural effects. Pascual-Ezama et al. (2015) utilized a coin-flip experiment on student samples across 16 countries, where students were rewarded with a chocolate if they reported the favorable outcome. While they found that 62% of the subjects reported the favorable outcome, they

did not find any differences in dishonesty across countries. However, for each country, 30 subjects were randomized into the coin-flip task, this means that on average 15 of the subjects would get the favorable outcome, and the other 15 would face the decision to report honestly or not. This makes it hard to determine whether the failure to find differences across countries was due to homogenous behavior in honesty, or insufficient statistical power to detect differences. In a similar study, Mann et al. (2016) applied a die-roll experiment using both student- and general public-sample across five countries. Results reveal that while subjects act dishonestly, and students more so than the general public, the level of dishonesty was more or less the same between countries. An experiment on tax compliance by Andrighetto et al. (2016) in Sweden and Italy also fails to find any significant difference between countries. These studies suggest that dishonesty levels are similar across countries. In contrast, Hugh-Jones (2016) reports large differences in honesty between countries in a coin flip experiment with an online survey sample across 15 countries. Gächter and Schulz (2016) also observe differences between countries. Some papers in the literature consider culture at an individual level. For instance, collectivism has been linked to unethical behavior. In a priming experiment where subjects read a scenario, and is asked to mark words as either collectivistic or individualistic, followed by a thought-experiment whether or not to bribe in order to make a business deal, Mazar and Aggarwal (2011) report a causal relationship between collectivism and bribery. The authors explained their result by collectivists perceiving themselves as less accountable for their actions, which reduces individual cost of unethical behavior. However, this experiment involves a hypothetical decision to act immorally, hence it did not monetarily incentivize dishonesty. In an experiment comparing individuals with family background from either East or West Germany, Ariely et al. (2019) suggests that mere exposure to socialism, in this case, having a family background from East Germany, increase dishonest behavior. Herein Paper 2 of this thesis consider cultural worldviews as a predictor for dishonest behavior, by combining an online survey on cultural worldviews with a random process honesty task. Results from Paper 2, also speak to one of the big debates within the literature, whether there are gender differences in preferences for honesty. The debate was sparked by an early finding, where Dreber and Johannesson (2008) found that males where more likely to act

dishonestly in a sender/receiver framework. However later attempts to replicate this finding has had various success. In the aforementioned meta-study by Gerlach et al. (2019) they find that men are significantly more dishonest than women, although the effect size is rather small. Gerlach et al. report that men on average report 6% higher in sender-receiver games, 4% higher in coin-flip, die-roll tasks, and 2% higher in matrix tasks.

3.4 Generalizability

Generalizability of results from laboratory experiments in economics has been a hot debate. Steven Levitt and John List (Levitt and List, 2007a, b)[7] have been fierce critiques of laboratory experiments in the past, advocating the use of field experiments in stead. Their main concern about laboratory experiments is the claim that they hold little applications for the "real world", and hence, lacks generalizability. One of their major concerns is that the traditional subject pool in laboratory experiments are students, and that the incentives are relatively small. In the aforementioned meta-study by Gerlach et al. (2019), fails to find any difference between students and non-students, and except for sender-receiver games, incentives did not affect the standardized reports. However, they point out, that it is conceivable that since a large proportion of studies included were conducted on student populations, that any gender effect, and potentially all other effects found in their meta-study may be solely due to the disproportionate participant samples. They conclude, that any results from experiments on honesty, should be interpreted with caution, and its desirable for future research to include more representative participant pools.

[7] For a response to this critizism, see Camerer (2011)

4. Summary of papers in this dissertation

The dissertation consists of three papers, all within the topic of preferences for honesty.

4.1 Summary of paper 1: Institutional inequality and individual preferences for honesty and generosity

The first paper, investigates how inequality in advantage shapes preferences for honesty and generosity. In a two-stage laboratory experiment, subjects first earn money in a real effort matrix task, before playing the dictator game with their earnings. To introduce inequality in the real effort task, we provide some of the participants with the solution, effectively giving them an extreme advantage over those who did not. Knowledge about this advantage is varied between treatments, ensuring that some know that they are at a relative disadvantage. This design let us examine how advantage, and knowledge about an advantage, affected both preferences for honesty and generosity. The paper reports three main findings. First, participants knowing they are at a relative disadvantage engage in considerable more dishonest behavior. Second, participants who dishonestly inflate their earnings, give more in the dictator game, in line with the expectation from moral balancing. Third, the participants at a relative disadvantage engage in less giving in the dictator game, indicating that their self-justification mitigate their need for moral balancing.

4.2 Summary of paper 2: Honesty and cultural worldviews

The second paper reports results from an online study, combining a survey on cultural worldviews, and a coin-flip task. Whether culture affects levels of honesty is debated in the literature. In the literature, country is commonly used as a proxy for culture, whereas this paper considers an individual level measure of culture, cultural worldviews. In essence, the cultural worldviews framework is developed on the seminal work by Douglas and Wildavsky (1983), and is used to place individuals within two cross cutting cultural worldviews dimensions. Each dimension reflects distinct features

individuals hold over how they believe society should be structured. These features influence the decisions these individuals make, and it is suggested from the literature that individuals adhering to individualism and hierarchical worldviews may hold stronger social norms (Dake, 1991; Kahan, 2008). The conjecture is that individualistic and hierarchical individuals will report more honestly compared to communitarian and egalitarian individuals. The data fail to confirm this conjecture at the aggregate level. However, when analyzing the data by gender, I find that these two worldviews explain the gender difference observed in the data.

4.3 Summary of paper 3: Cash or report – comparing two payment schemes in a real-effort honesty task

This paper considers whether the choice of payment scheme affects preferences for honesty in a real effort matrix task. The paper consists of two parts. First, the paper reports results from an experiment with two treatments, each mimicking the standard payment schemes used in this experimental task. Either the subjects pay themselves in private, or they report their performance to a research assistant. Part two considers a meta-study on the same issue, using experiments with matrix tasks reported in Gerlach et al. (2019). Each of the experiments included are coded according to the original authors choice of payment-scheme. Results from the meta-study are in line with the experimental results in the first part, indicating that choice of payment-scheme in the real-effort task has no effect on preferences for honesty.

References

Abeler, J., Becker, A., Falk, A., 2014. Representative evidence on lying costs. Journal of Public Economics, 113, 96-104.

Abeler, J., Nosenzo, D., Raymond, C., 2019. Preferences for truth‐telling. Econometrica, 87(4), 1115-1153.

ACFE, A., 2008, Report to the nations on occupational fraud and abuse. Association of Certified Fraud Examiners Austin, TX.

Andrighetto, G., Zhang, N., Ottone, S., Ponzano, F., D'Attoma, J., Steinmo, S., 2016. Are some countries more honest than others? Evidence from a tax compliance experiment in Sweden and Italy. Frontiers in psychology, 7, 472.

Ariely, D., Garcia-Rada, X., Gödker, K., Hornuf, L., Mann, H., 2019. The impact of two different economic systems on dishonesty. European Journal of Political Economy.

Balasubramanian, P., Bennett, V.M., Pierce, L., 2017. The wages of dishonesty: The supply of cheating under high-powered incentives. Journal of Economic Behavior & Organization, 137, 428-444.

Becker, G.S., 1968. Crime and Punishment: An Economic Approach. Journal of Political Economy, 76(2), 169-217.

Bolton, G.E., Ockenfels, A., 2000. ERC: A theory of equity, reciprocity, and competition. American economic review, 90(1), 166-193.

Bucciol, A., Piovesan, M., 2011. Luck or cheating? A field experiment on honesty with children. Journal of Economic Psychology, 32(1), 73-78.

Camerer, C., 2011. The promise and success of lab-field generalizability in experimental economics: A critical reply to Levitt and List. Available at SSRN 1977749.

Camerer, C.F., Hogarth, R.M., 1999. The effects of financial incentives in experiments: A review and capital-labor-production framework. Journal of risk and uncertainty, 19(1-3), 7-42.

Charness, G., Gneezy, U., Henderson, A., 2018. Experimental methods: Measuring effort in economics experiments. Journal of Economic Behavior & Organization, 149, 74-87.

Charness, G., Rabin, M., 2002. Understanding social preferences with simple tests. The quarterly journal of economics, 117(3), 817-869.

Cohn, A., Fehr, E., Maréchal, M.A., 2014. Business culture and dishonesty in the banking industry. Nature, 516(7529), 86-89.

Croson, R., 2005. The method of experimental economics. International Negotiation, 10(1), 131-148.

Dake, K., 1991. Orienting dispositions in the perception of risk: An analysis of contemporary worldviews and cultural biases. Journal of Cross-Cultural Psychology, 22(1), 61-82.

Douglas, M., Wildavsky, A., 1983, Risk and culture: An essay on the selection of technological and environmental dangers. Univ of California Press.

Dreber, A., Johannesson, M., 2008. Gender differences in deception. Economics Letters, 99(1), 197-199.

Engel, C., 2011. Dictator games: a meta study. Experimental Economics, 14(4), 583-610.

Fehr, E., Schmidt, K.M., 1999. A theory of fairness, competition, and cooperation. The quarterly journal of economics, 114(3), 817-868.
Fischbacher, U., Föllmi-Heusi, F., 2013. Lies in disguise—an experimental study on cheating. Journal of the European Economic Association, 11(3), 525-547.
Forsythe, R., Horowitz, J.L., Savin, N.E., Sefton, M., 1994. Fairness in simple bargaining experiments. Games and Economic Behavior, 6(3), 347-369.
Friedman, D., Cassar, A., Selten, R., 2004, Economics lab: an intensive course in experimental economics. Psychology Press.
Gerlach, P., Teodorescu, K., Hertwig, R., 2019. The truth about lies: A meta-analysis on dishonest behavior. Psychological bulletin, 145(1), 1.
Gneezy, U., 2005. Deception: The role of consequences. American economic review, 95(1), 384-394.
Güth, W., Schmittberger, R., Schwarze, B., 1982. An experimental analysis of ultimatum bargaining. Journal of Economic Behavior & Organization, 3(4), 367-388.
Gächter, S., Schulz, J.F., 2016. Intrinsic honesty and the prevalence of rule violations across societies. Nature, 531(7595), 496.
Hermann, D., Mußhoff, O., 2019. I might be a liar, but I am not a thief: An experimental distinction between the moral costs of lying and stealing. Journal of Economic Behavior & Organization, 163, 135-139.
Hugh-Jones, D., 2016. Honesty, beliefs about honesty, and economic growth in 15 countries. Journal of Economic Behavior & Organization, 127, 99-114.
Kahan, D.M., 2008. Cultural cognition as a conception of the cultural theory of risk. HANDBOOK OF RISK THEORY, S. Roeser, ed., Forthcoming, 08-20.
Kahneman, D., Knetsch, J.L., Thaler, R.H., 1986. Fairness and the assumptions of economics. Journal of business, S285-S300.
Levitt, S.D., List, J.A., 2007a. On the generalizability of lab behaviour to the field. Canadian Journal of Economics/Revue canadienne d'économique, 40(2), 347-370.
Levitt, S.D., List, J.A., 2007b. What do laboratory experiments measuring social preferences reveal about the real world? Journal of Economic perspectives, 21(2), 153-174.
Lilleholt, L., Schild, C., Zettler, I., 2020. Not all computerized cheating tasks are equal: A comparison of computerized and non-computerized versions of a cheating task. Journal of Economic Psychology, 102270.
List, J.A., Sadoff, S., Wagner, M., 2011. So you want to run an experiment, now what? Some simple rules of thumb for optimal experimental design. Experimental Economics, 14(4), 439.
Mann, H., Garcia-Rada, X., Hornuf, L., Tafurt, J., Ariely, D., 2016. Cut from the same cloth: Similarly dishonest individuals across countries. Journal of Cross-Cultural Psychology, 47(6), 858-874.
Mazar, N., Aggarwal, P., 2011. Greasing the palm: Can collectivism promote bribery? Psychological science, 22(7), 843-848.
Mazar, N., Amir, O., Ariely, D., 2008. The dishonesty of honest people: A theory of self-concept maintenance. Journal of marketing research, 45(6), 633-644.
Mazar, N., Zhong, C.-B., 2010. Do green products make us better people? Psychological science, 21(4), 494-498.

Nisan, M., 1991. The moral balance model: Theory and research extending our understanding of moral choice and deviation.
Oosterbeek, H., Sloof, R., Van De Kuilen, G., 2004. Cultural differences in ultimatum game experiments: Evidence from a meta-analysis. Experimental Economics, 7(2), 171-188.
Pascual-Ezama, D., Fosgaard, T.R., Cardenas, J.C., Kujal, P., Veszteg, R., de Liaño, B.G.-G., Gunia, B., Weichselbaumer, D., Hilken, K., Antinyan, A., 2015. Context-dependent cheating: Experimental evidence from 16 countries. Journal of Economic Behavior & Organization, 116, 379-386.
Shalvi, S., Dana, J., Handgraaf, M.J., De Dreu, C.K., 2011. Justified ethicality: Observing desired counterfactuals modifies ethical perceptions and behavior. Organizational Behavior and Human Decision Processes, 115(2), 181-190.
Shalvi, S., Gino, F., Barkan, R., Ayal, S., 2015. Self-serving justifications: Doing wrong and feeling moral. Current Directions in Psychological Science, 24(2), 125-130.
Utikal, V., Fischbacher, U., 2013. Disadvantageous lies in individual decisions. Journal of Economic Behavior & Organization, 85, 108-111.
Weibull, J.W., Villa, E., 2005. Crime, punishment and social norms, SSE/EFI Working Paper Series in Economics and Finance.

Journal of Economic Behavior and Organization 170 (2020) 355–361

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Institutional inequality and individual preferences for honesty and generosity

Johan Birkelund [a], Todd L. Cherry [b,c,*]

[a] School of Business and Economics, UiT – The Arctic University of Norway, NO-9037 Tromsø, Norway
[b] Department of Economics, Appalachian State University, Boone NC 28608-2051 USA
[c] CICERO Center for International Climate Research – Oslo, P.O. Box 1129 Blindern, 0318 Oslo, Norway

ARTICLE INFO

Article history:
Received 17 February 2019
Revised 14 December 2019
Accepted 16 December 2019
Available online 10 January 2020

JEL Classification:
C9
D1
D2

Keywords:
Cheating
Giving
Inequality
Behavioral economics
Experiments

ABSTRACT

This paper reports on an experiment that investigates how inequality in advantage affects individual preferences for honesty and generosity. In a two-stage experiment, subjects first earn money according to self-reported production, which can include honest and dishonest reports. Subjects then play the dictator game and decide how much, if any, of their earnings to share with an anonymous recipient. Treatments investigate how equal and unequal advantages in production affect the extent of cheating in stage one and the subsequent offers in stage two. When advantage randomly benefits only some of the group, the relatively disadvantaged are significantly more dishonest and exhibit significantly less other-regarding behavior. Considering the interplay between cheating and giving, we find that greater cheating was followed with greater giving. And comparing this relationship across treatments suggests self-justification for dishonest behavior makes subjects feel more entitled to their ill-gotten gains.

1. Introduction

The efficacy of social and economic institutions often depends on social trust (La Porta et al., 1997; Zak and Knack, 2001). Those same institutions however can create contexts that influence people's preferences for honesty (Kimbrough and Vostroknutov, 2016; Rodriguez-Sickert et al., 2008). That preferences for honesty may be institution-specific suggests that the policy choices that define institutions not only matter for the outcomes they generate but also for the society they cultivate.

Research from the behavioral sciences offer important insights about the interplay between institutional fairness and preferences for honesty. Early experimental evidence from psychology indicate that paying subjects less than what they were told to expect leads to more cheating (Greenberg, 1990). Similarly, recent work finds more cheating when earnings are below the expectations set by distributional norms (Galeotti et al., 2017). Beyond expectations, studies also suggest that inequities in earnings can affect the level of cheating. For instance, John et al. (2014) finds that people cheat more when they are aware that other people are earning more for the same task. In a prominent economics experiment, Houser et al. (2012) reports that subjects were more likely to overstate earnings in a self-reported coin-flipping task when they reported

* Corresponding author at: Department of Economics, Appalachian State University, Boone NC 28608-2051 USA.
E-mail addresses: johan.birkelund@uit.no (J. Birkelund), cherrytl@appstate.edu (T.L. Cherry).

https://doi.org/10.1016/j.jebo.2019.12.014
0167-2681/

being treated unfairly in a previous, unrelated dictator game. The findings indicate that individual preferences for honesty are shaped by procedural fairness as well as distributional fairness (e.g., Bolton et al., 2005; Frey et al., 2004; Greiner et al., 2012). It follows that people may self-justify dishonest behavior when their outcomes emerge from an unfair process. If so, cheating will depend on the perceived fairness of the institutions that govern outcomes. While inequality may matter, *how* the inequality materializes also matters.

Herein we conduct a laboratory experiment to contribute to the evidence on the interplay between institutional fairness and preferences for honesty. Following the literature (Friesen and Gangadharan, 2012; Mazar et al., 2008), we use an individual real-effort task to collect individual-level data on dishonesty.[1] We extend this line of inquiry by introducing unearned (i.e., random) institutional inequities. While some subjects completed the task on their own, other subjects benefited from receiving assistance. The question is whether the unearned advantage enjoyed by some, increases the likelihood and magnitude of cheating among those that are relatively disadvantaged. Results may speak to how fairness in our social and economic systems can shape the norms of individual honesty and social trust.

This study also contributes to the growing literature on self-justification and moral balancing. Researchers have argued that people seek to hold their moral self-image constant over time, which may entail balancing good behavior with bad behavior, or vice versa (Nisan and Horenczyk, 1990). We extend the analysis to include an unannounced dictator game as a second stage to the experimental design. Using earnings from the previous individual task, subjects decided how much, if any, of their earnings to give to an anonymous recipient. By introducing an ex post giving stage, we can investigate how the interplay of institutional fairness and cheating affects subsequent giving. In particular, how does cheating affect subsequent giving, but potentially more interesting, how does cheating justified by institutional inequities affect subsequent giving. Any divergent cheating behavior may contribute evidence to the literature on moral balancing.

Results indicate that institutional fairness and inequities shape individual preferences for honesty and giving. In line with the literature on self-justification, we find that people with an undeserving relative disadvantage cheat at significantly higher rates and higher levels. And consistent with moral balancing, people that increased earnings by cheating in turn increased their giving to anonymous recipients. Interestingly, people that appeared to self-justify cheating gave less, which suggests that self-justification for dishonest behavior makes subjects feel more entitled to their ill-gotten gains and less compelled to morally balance their misdeeds.

2. Experimental design

2.1. Baseline framework

The experiment employed a modified real effort dictator game, which allowed for cheating in the effort stage.[2] Following the literature (Mazar et al., 2008), subjects could earn money in the production stage by solving a series of simple mathematical matrix tasks. Each matrix contained 12 three-digit numbers that ranged between 0.00 and 9.99, and to solve, subjects had to identify the unique combination of two numbers that sum up to exactly 10.00. Subjects were provided a sheet of 15 matrices and earned one USD for each matrix solved.

To collect individual level data, we follow a protocol adapted from Friesen and Gangadharan (2012). Subjects were randomly assigned to stations, at which they found materials for the first stage of the experiment. The materials were coded by station and included the instructions, the sheet of matrices, and two envelopes—a *money envelope* with 15 one-dollar bills (USD) and an empty *earnings envelope* that will hold their earnings. Instructions were opened and read aloud. After answering any questions, subjects had five minutes to solve as many matrices as possible. For each solution, subjects were instructed to circle the two numbers on the matrix that summed to 10.00. Subjects were informed that some matrices did not have a solution, which allowed top performers ample opportunity to cheat. At the end of the five-minute period, subjects were provided solutions and instructed to self-report their overall performance at a designated place on the matrix sheet.[3] Subjects then paid themselves by taking their earnings from the money envelope and placing it in the earnings envelope. Any unclaimed money was left in the money envelope. The number of bills taken above the number of matrices correctly solved represents earnings obtained by cheating. Subjects then placed their completed matrices sheet and money envelope in a large envelope and sealed it. The large envelope was inserted in the slot of a closed box that was brought to each subject's station in sequence. It was announced that the box would remain closed until after the session. This concluded the first stage.

The second stage began without delay. Subjects were informed of the second stage only after completing the first stage. Materials, again coded by stations, were provided for the standard dictator game—all playing the role of the dictator. Subjects anonymously decided how much, if any, of their stage one earnings to give to an anonymous recipient that was recruited

[1] Note that most studies on cheating have relied on group-level data, so a strength of this study is that the experimental design draws from Friesen and Gangadharan (2012) to yield individual-level data for the analysis.

[2] In a real-effort dictator game, subjects perform a task to earn their endowment, which they subsequently allocate. To ensure subjects were earning money for themselves, subjects were informed of the allocation decision only after completing the task.

[3] To avoid subjects altering their answers after receiving the solutions, subjects were provided a blue pen with the matrices. They used a blue pen to solve the matrices. When the answers were provided, the blue pen was collected and a red pen was provided. They used the red pen for the remaining tasks.

from the subject pool. Using an *offer envelope* provided with the instructions, dictators put their offer in the offer envelope and kept the remaining money in their earnings envelope. Subjects were then called individually to leave the room. They inserted the offer envelope in the slot of the same closed box. Within 48 h, the contents of the envelopes were recorded and delivered to recipients in a separate pre-scheduled session.

The experiment was conducted at Appalachian State University with a total of 192 subjects in one of eleven sessions. Each of these sessions lasted about 40 min. Dictators earned about 14 USD, which included a 5 USD participation payment. Recipients in the dictator game were randomly selected from the subject pool, and did not participate in the experiment, other than receiving the offers ex post.

2.2. Treatments

The experiment followed a 2 × 2 design with two treatment variables—*advantage* (yes or no) and *equal* (yes or no). In the advantage treatments, subjects with *no advantage* solved the matrices without assistance. Subjects with *advantage* solved the matrices with the help of marks that indicated the solutions. Put simply, advantaged subjects were essentially provided solutions, while the disadvantaged subjects were not.[4] This extreme case of advantage provides ample salience for possible treatment effects.

In the equal treatments, subjects in the *equal* condition had the same advantage status as their peers—i.e., other subjects in the session. The status was common knowledge. Subjects in the *unequal* condition did not share the same advantage status as their peers. Subjects knew that half of the session's participants were randomly determined to have *advantage*, with the other half randomly determined to have *no advantage*. Thus, subjects in the unequal condition knew whether they were relatively advantaged or disadvantaged.

The experimental design yields three treatments and four fairness conditions. The *equal no advantage* treatment serves as the baseline—*all* subjects in the session solved the matrices without assistance. The *equal advantage* treatment introduces an evenly shared advantage—*all* subjects in the session solved the matrices with assistance. The *unequal advantage* treatment introduces inequality in advantage and creates an uneven playing field that yields two conditions for subjects. For the *unequal advantage* condition, subjects have assistance with solving matrices when others do not. For the *unequal no advantage* condition, subjects solve the matrices without assistance when others have assistance. Again, the treatments, equal or advantaged, were common knowledge. The unequal treatment therefore creates unearned inequities that raise fairness concerns. Comparisons will reveal the potential impact of advantage, equal and unequal, on the tendency to cheat in stage one and exhibit pro-social behavior in stage two.

2.3. Hypotheses

From the 2 × 2 experimental design, we organize the primary research questions by the decisions in the two stages of the experiment—two for each stage. The first stage, in which we observe possible cheating, allows us to consider how advantage and relative advantage affects individual preferences for honesty. We note the advantage treatments are defined by institutional unfairness, which encompasses both the assistance and resulting benefits of the randomly assigned advantage treatments. The design does not disentangle the individual components of the unfairness, though previous work shows the influence of unequal effort and opportunity dominate that of unequal budgets (e.g., Cherry and Shogren, 2008).

The first research question is whether absolute advantage affects cheating, which is informed by testing the null that cheating is equivalent across the equal no advantage and the equal advantage treatments. Gravert (2013) reports that more demanding tasks lead to more cheating, so we expect less cheating in the equal advantage treatment than in the equal no advantage treatment. The second research question considers how relative advantage may impact cheating. We test the null that cheating in the unequal no advantage treatment is equivalent to cheating in equal no advantage treatment. From the literature on self-justification (e.g., Shalvi et al., 2011), we expect cheating to be more prevalent among the relatively disadvantaged subjects.

The second stage of the experiment, in which we observe giving, provides opportunities to examine possible linkages between cheating and giving across institutional contexts. We again note that, given that the second stage depends on decisions in the first stage, giving behavior is conditional and should be interpreted as such. The third research question is whether correlations of individual cheating and giving behaviors are consistent with moral balancing—offsetting past immoral acts with subsequent moral acts (moral cleasing) and offsetting past moral acts with subsequent immoral acts (moral licensing). We address this research question by conducting a conditional analysis to test the null that there is no relationship between an individual's level of cheating in stage one and her level of giving in stage two. Moral balancing suggests that subjects that increase earnings by cheating will offset that behavior with greater giving. Thus, we expect that cheating will have a positive effect on subsequent giving. A fourth research question is how the relationship between cheating and giving differs across the treatments that vary institutional fairness. To the extent that self-justification of cheating varies across institutional fairness, we expect to observe corresponding variation in moral balancing. Specifically, in the equal treatments,

[4] A keen reviewer pointed out that subjects in the no advantage treatments knew some matrices did not have solutions while subjects in the advantage treatments could infer exactly how many matrices did not have solutions. Any possible confounding effect does not affect the analyses that speak to the primary research questions.

Table 1
Production and fraudulent behavior by treatment.

	Equal		Unequal		
	No Advantage	Advantage	No Advantage	Advantage	Pooled
Production					
Actual	4.52	9.54	3.51	9.87	6.84
	(2.56)	(1.22)	(2.19)	(0.45)	(3.39)
Reported	6.52	10.08	9.57	10.32	9.09
	(3.63)	(1.65)	(5.47)	(1.38)	(3.76)
Over-reported	2.00	0.54	6.06	0.45	2.25
	(3.53)	(1.35)	(5.21)	(1.27)	(3.96)
Fraud Rates					
Take Some	38.00	16.67	70.21	17.02	35.42
	(49.03)	(37.66)	(46.23)	(37.99)	(47.95)
Take All	6.00	4.17	42.55	6.38	14.58
	(23.99)	(20.19)	(49.98)	(24.71)	(35.39)
% Taken	17.53	9.64	54.45	8.94	22.49
	(29.03)	(25.24)	(44.77)	(25.30)	(36.80)
N	50	48	47	47	192

Note: Standard Deviation in parenthesis.

we expect subjects that cheated in the first stage to moral balance (i.e., moral cleansing) their cheating with relatively high giving in the second stage. However, in the unequal no advantage condition, subjects may not engage in moral balancing because they self-justify their cheating in the first stage. Thus, we expect relatively low offers from the dishonest subjects in the unequal no advantage condition.

3. Results

3.1. Stage one: cheating

The top section of Table 1 provides the actual and self-reported performance on the matrix task by treatment. Fig. 1 complements the numbers with a visual comparison of actual and self-reported earnings by treatment. Overall, subjects correctly solved an average of 6.84 matrices, but they self-reported solving 9.09 matrices. Thus, across all treatments, subjects overreported their performance by 2.25 solutions or 32.9%. The bottom section of Table 1 provides additional measures of fraudulent behavior.[5] The numbers show that 35.4% of all subjects over-reported to some extent (i.e., 65% were honest). About 15% of subjects maximized over-reporting. And, on average, subjects only collected 22.49% of the potential fraudulent takings. This corresponds well to previous reports in the literature (e.g., Rosenbaum et al., 2014). To address the two research questions for stage one, we look at the numbers by treatment.

The first research question offers an introductory step by considering whether absolute advantage affects cheating. Focusing on the equal treatments in Table 1, we compare the advantage and no advantage treatments. As expected, actual performance was significantly higher in the advantage treatment than in the no advantage treatment (9.54 vs. 4.52; $p<0.001$).[6] The advantage treatment, in turn, appears to have significantly reduced the level of over-reporting relative to the no advantage treatment (0.54 vs. 2.0; $p = 0.013$). Further, the numbers show that, relative to the equal no advantage treatment, the introduction of equal advantage lowers the rate of cheating (38.0 vs. 16.7; $p = 0.019$), lowers the rate of maximal cheating (6.0 vs. 4.2; $p = 0.682$), and reduces the average share taken (17.5 vs. 9.6; $p = 0.031$). The collection of findings indicates that absolute advantage reduces over-reporting, which follows previous reports that less demanding tasks lead to less cheating (Gravert, 2013). However, we note this finding also may reflect that advantaged subjects inherently have more knowledge about matrices without a solution.

The second research question presents a key hypothesis for this study: do institutional inequities affect individual preferences for honesty. In our setting, we consider if over-reporting is significantly different when the lack of advantage is a relative disadvantage—equal no advantage vs. unequal no advantage. From Table 1, we first note that subjects with a relative disadvantage (i.e., unequal no advantage) had lower actual performance than those with an absolute disadvantage (i.e., equal no advantage)—3.51 vs. 4.52 ($p = 0.052$). This is consistent with previous work reporting that effort is negatively impacted by unfairness.[7] Moving to the research question, a review of the two no advantage treatments shows that relative advantage matters. Table 1 reports that, among subjects with no advantage, those in the unequal treatment over-reported a great deal more than those in the equal treatment (6.06 vs. 2.0; $p<0.001$). Additionally, among subjects with no advantage, subjects in the unequal treatment exhibited a higher rate of cheating (70.2 vs. 38.0; $p = 0.002$), a higher rate of maximal cheating

[5] Note that we report alternative metrics (e.g., binary variables, means, rates, etc.) to mitigate concerns that results are driven by the higher earnings in the advantaged treatments.

[6] All tests reported, are two-sided Wilcoxon rank-sum tests. Results were similar when using Welch's *t*-tests.

[7] For instance, Gächter and Thöni (2010) finds a significant reduction in effort, when workers are paid less than their peers for the same task.

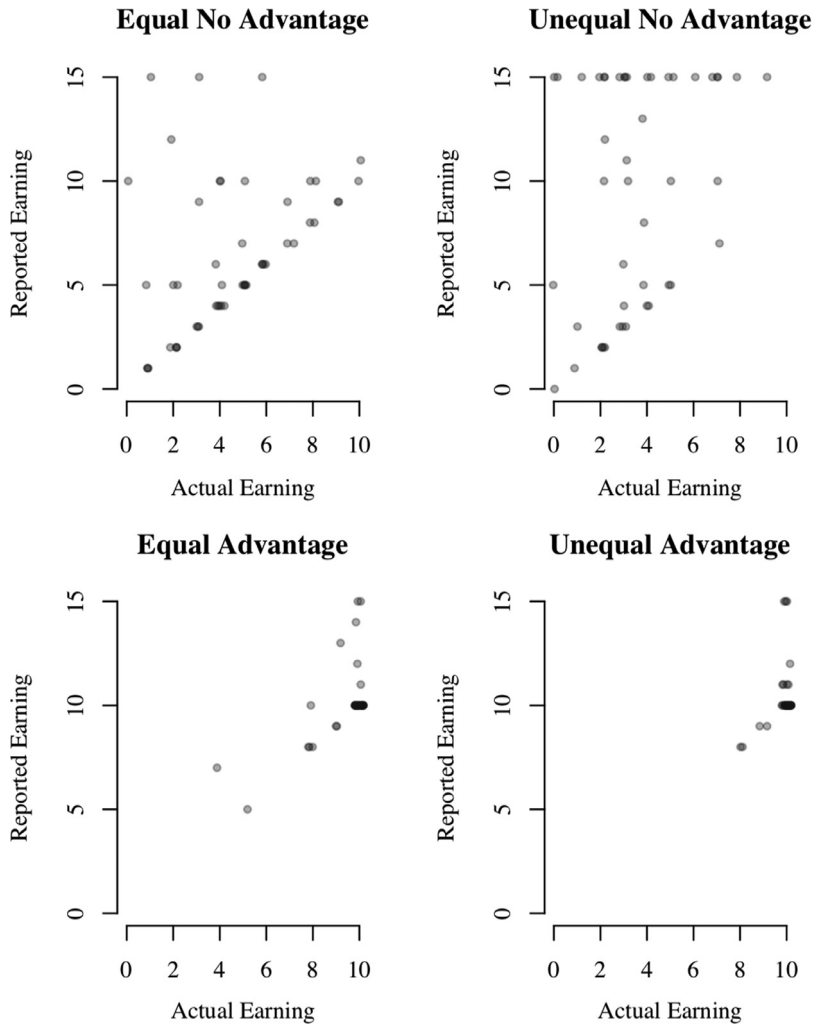


Fig. 1. Scatterplot of reported and actual earnings by treatment.

(42.6 vs. 6.0; $p<0.001$), and took more of the available fraudulent earnings (54.5 vs. 17.5; $p<0.001$). Conversely, subjects with advantage behave similarly whether the advantage is shared equally or not. The tests provide compelling evidence that institutional fairness affects individual preferences for honesty. Tests indicate that unequal advantage can lead to greater cheating among the relatively disadvantaged. This finding corresponds to previous studies that context matters (e.g., Cherry and Shogren, 2008) and is consistent with the notion that people self-justify dishonest behavior (Shalvi et al., 2011).

3.2. Stage two: giving

Table 2 reports the giving behavior observed in stage two of the experiment. We follow the literature and report four measures of giving by treatment—the overall mean offer, the percentage of dictators that made a positive offer, the percent of offers that were equal splits, and offer as a percentage of total earnings. Across all treatments, the mean offer was 1.14. About 40% of subjects made a positive offer, and 5.2% offered an equal split. Subjects, on average, offered 11.2% of their earnings. In general, offers from our subjects correspond with previous dictator games in the literature. Reviewing the data by treatment indicates that absolute advantage tends to increase offers and introducing relative advantage leads to higher offers among the advantaged and lower offers among the disadvantaged. These findings follow previous reports that unearned gains are given away more freely (Cherry et al., 2002) and unearned inequality affects giving (Korenok et al., 2012). We now disaggregate the data to investigate the two research hypotheses for stage two.

The third research question considers if observed behavior across stage one and two (cheating and giving) is consistent with moral balancing—i.e., do subjects offset cheating in stage one with giving in stage two? We estimate a simple regression model of giving, where the offer amount is a function of legitimate and fraudulent earnings conditioned on the treatment.

Table 2
Dictator behavior by treatment.

	Equal		Unequal		
	No Advantage	Advantage	No Advantage	Advantage	Pooled
Mean Offer ($)	0.76	1.38	0.51	1.91	1.14
	(1.39)	(1.67)	(1.23)	(1.90)	(1.65)
Positive Offer (%)	32.00	52.08	19.15	59.57	40.63
	(47.12)	(50.49)	(39.77)	(49.61)	(49.24)
Equal Split (%)	6.00	4.17	0.00	10.64	5.20
	(23.99)	(20.19)	(0.00)	(31.17)	(22.28)
Offer pct. of Total Earning (%)	9.22	13.20	3.78	18.49	11.19
	(15.23)	(15.44)	(8.80)	(17.95)	(15.61)
N	50	48	47	47	192

Note: Standard deviation in parenthesis.

Table 3
OLS estimates of offer model.

	Pooled	Equal No Advantage	Equal Advantage	Unequal No Advantage	Unequal Advantage
Constant	0.326	0.096	−0.845	−0.122	0.932
	(0.367)	(0.786)	(0.655)	(0.760)	(0.882)
Legitimate Earnings	0.025	0.026	0.208	0.035	0.088
	(0.685)	(0.676)	(0.289)	(0.662)	(0.890)
Fraudulent Earnings	0.161	0.273	0.444	0.084	0.252
	(0.000)	(0.000)	(0.014)	(0.016)	(0.267)
Equal Advantage	0.727	–	–	–	–
	(0.088)				
Unequal No Advantage	−0.880	–	--	--	–
	(0.009)				
Unequal Advantage	1.274	–	–	–	–
	(0.000)				
F	9.78	20.01	3.43	3.19	0.67
R^2	0.208	0.460	0.132	0.127	0.029
N	192	50	48	47	47

Notes: dependent variable is $-amount of offer; estimated treatment effects are relative to the omitted equal-no-advantage treatment; p-value are reported in parentheses.

Results are reported in Table 3. From the pooled model, estimates reveal a significant positive relationship between fraudulent earnings and giving ($p<0.001$). This is in contrast to the absence of a relationship between legitimate earnings and giving ($p = 0.685$). We note this general finding is quite consistent across the treatment-specific models. Estimates therefore suggest that greater cheating in stage one is associated with more giving in stage two, which is consistent with moral balancing. In this case, people acted to balance past immoral behavior with a current moral behavior (Conway and Peetz, 2012; Jordan et al., 2011).

To address the fourth research question, we move to the treatment-specific models to consider how institutional fairness affects moral cleansing behavior. The conjecture is that moral balancing will be less prevalent when people justify immoral behavior because of institutional unfairness. When unfairness justifies dishonest behavior, there is little need for moral cleansing. In our setting, subjects randomly assigned a relative disadvantage (unequal no advantage) may perceive their relative position as unfair, and if so, they may self-justify their cheating and have no need for moral balancing. Thus, relative to the other treatments, fraudulent earnings in the relatively disadvantaged condition (i.e., unequal no advantage) will have a smaller effect on second stage giving.

Results are consistent with the conjecture. In the two equal treatments, the level of cheating (i.e., fraudulent earnings) in stage one led to significantly more giving in stage two. Interestingly, the magnitude of the effect was considerably higher in the advantage treatment, which is likely due to the relative ease and size of stage one earnings. Moving to the unequal treatments, estimates show that fraudulent earnings has a smaller effect on subsequent giving. In the unequal no advantage condition (i.e., relatively disadvantaged), the estimated coefficient is substantially lower than the other treatments. Relative to equal no advantage treatment, the estimated effect is about 65% smaller—0.084 vs. 0.273. We note the insignificance of the estimated coefficient in the unequal advantage model, which is due to the relatively large standard error. From the treatment-specific models, we find additional evidence for the conjecture that people will not be compelled to offset previous cheating (i.e., moral balancing) if the institutional unfairness justifies the cheating. Institutional unfairness appears to not only motivate people to self-justify bad behavior but also diminishes their desire to morally cleanse bad behavior.

4. Conclusion

Studies provide overwhelming evidence that behavior is context-dependent. It follows that behavior is dependent on the social and economic institutions that shape the processes and outcomes that define contexts. This paper examines the role of institutional fairness on preferences for honesty, and by examining connections between cheating and subsequent giving; it considers the interplay between self-justification and moral balancing. Findings correspond to previous reports that effort and inequality can influence cheating and giving behavior, but the results also offer new insights on how institutional fairness affects these behaviors.

Our results offer strong evidence that preferences for both honesty and giving are shaped by institutional fairness. We note three main findings from our study. First, in accordance with previous reports that people self-justify dishonest behavior when facing an unfair situation, we find that people with a relative disadvantage engage in considerably more cheating, both in propensity and magnitude. Second, when considering the relationship between cheating and giving, we observe behavior in line with moral balancing (e.g., Ploner and Regner, 2013; Gneezy et al., 2014). People that earned more from cheating subsequently made more generous offers to anonymous recipients. Third, we find that the relationship between cheating and giving varies according to institutional fairness. When the playing field is uneven, people with a relative disadvantage cheat considerably more and give considerably less. Thus, relatively disadvantaged subjects appear to not only self-justify their decision to cheat, but they also feel more entitled to their ill-gotten gains.

The findings speak to current debates on inequities in economic opportunity and mobility, which depend on our social and economic institutions. Results contribute to the growing evidence that institutions not only matter for the rules they impose but also for the contexts they create. The interaction between inequality and fairness can dictate the legitimacy of outcomes and therefore shape individual behavior, such as cheating and giving, which has implications for broader social and economic well-being.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgments

We appreciate the helpful comments received from Agne Kajackaite, Abhijit Ramalingam, Brock Stoddard, and participants of the 6th Spring School in Behavioral Economics at UCSD and the 2018 ESA World Meeting in Berlin. Two reviewers and the editor also provided valuable comments. This research was funded by the Walker College of Business at Appalachian State University.

References

Bolton, G.E., Brandts, J., Ockenfels, A., 2005. Fair procedures: evidence from games involving lotteries. Econ. J. 115 (506), 1054–1076.
Cherry, T.L., Frykblom, P., Shogren, J.F., 2002. Hardnose the dictator. Am. Econ. Rev. 92 (4), 1218–1221.
Cherry, T.L., Shogren, J.F., 2008. Self-interest, sympathy and the origin of endowments. Econ. Lett. 101 (1), 69–72.
Conway, P., Peetz, J., 2012. When does feeling moral actually make you a better person? Conceptual abstraction moderates whether past moral deeds motivate consistency or compensatory behavior. Person. Soc. Psychol. Bull. 38 (7), 907–919.
Frey, B.S., Benz, M., Stutzer, A., 2004. Introducing procedural utility: not only what, but also how matters. J. Inst. Theor. Econ. JITE 160 (3), 377–401.
Friesen, L., Gangadharan, L., 2012. Individual level evidence of dishonesty and the gender effect. Econ. Lett. 117 (3), 624–626.
Galeotti, F., Kline, R., Orsini, R., 2017. When foul play seems fair: exploring the link between just deserts and honesty. J. Econ. Behav. Organ. 142, 451–467.
Gneezy, U., Imas, A., Madarász, K., 2014. Conscience accounting: emotion dynamics and social behavior. Manag. Sci. 60 (11), 2645–2658.
Gravert, C., 2013. How luck and performance affect stealing. J. Econ. Behav. Organ. 93, 301–304.
Greenberg, J., 1990. Employee theft as a reaction to underpayment inequity: the hidden cost of pay cuts. J. Appl. Psychol. 75 (5), 561.
Greiner, B., Ockenfels, A., Werner, P., 2012. The dynamic interplay of inequality and trust—an experimental study. J. Econ. Behav. Organ. 81 (2), 355–365.
Gächter, S., Thöni, C., 2010. Social comparison and performance: experimental evidence on the fair wage–effort hypothesis. J. Econ. Behav. Organ. 76 (3), 531–543.
Houser, D., Vetter, S., Winter, J., 2012. Fairness and cheating. Eur. Econ. Rev. 56 (8), 1645–1655.
John, L.K., Loewenstein, G., Rick, S.I., 2014. Cheating more for less: upward social comparisons motivate the poorly compensated to cheat. Organ. Behav. Hum. Decis. Process. 123 (2), 101–109.
Jordan, J., Mullen, E., Murnighan, J.K., 2011. Striving for the moral self: the effects of recalling past moral actions on future moral behavior. Person. Soc. Psychol. Bull. 37 (5), 701–713.
Kimbrough, E.O., Vostroknutov, A., 2016. Norms make preferences social. J. Eur. Econ. Assoc. 14 (3), 608–638.
Korenok, O., Millner, E.L., Razzolini, L., 2012. Are dictators averse to inequality? J. Econ. Behav. Organ. 82 (2–3), 543–547.
La Porta, R., Lopez-de-Silanes, F., Shleifer, A., Vishny, R.W., 1997. Trust in large organizations. Am. Econ. Rev. 87 (2), 333.
Mazar, N., Amir, O., Ariely, D., 2008. The dishonesty of honest people: a theory of self-concept maintenance. J. Mark. Res. 45 (6), 633–644.
Nisan, M., Horenczyk, G., 1990. Moral balance: the effect of prior behaviour on decision in moral conflict. Br. J. Soc. Psychol. 29 (1), 29–42.
Ploner, M., Regner, T., 2013. Self-image and moral balancing: an experimental analysis. J. Econ. Behav. Organ. 93, 374–383.
Rodriguez-Sickert, C., Guzmán, R.A., Cárdenas, J.C., 2008. Institutions influence preferences: evidence from a common pool resource experiment. J. Econ. Behav. Organ. 67 (1), 215–227.
Rosenbaum, S.M., Billinger, S., Stieglitz, N., 2014. Let's be honest: a review of experimental evidence of honesty and truth-telling. J. Econ. Psychol. 45, 181–196.
Shalvi, S., Dana, J., Handgraaf, M.J., De Dreu, C.K., 2011. Justified ethicality: observing desired counterfactuals modifies ethical perceptions and behavior. Organ. Behav. Hum. Decis. Process. 115 (2), 181–190.
Zak, P.J., Knack, S., 2001. Trust and growth. Econ. J. 111 (470), 295–321.

Honesty and Cultural Worldviews

Johan Birkelund

School of Business and Economics, UiT – The Arctic University of Norway

Abstract

This paper reports findings from an on online experiment that provides new evidence on gender differences in preferences for honesty. The experimental design combines a self-reported coin-flip honesty task with a survey instrument that measures individual cultural worldviews across two dimensions, individualist – communitarian and hierarchical – egalitarian. This paper reports three main findings. First, there is no difference in the reported number of successful coin-flips across either worldview dimension at the aggregate level. Second, and in line with previous research, when the data is disaggregated by gender, there are differences in honesty. Third, females identifying with individualistic and hierarchical worldviews, report significantly fewer successful coin-flips than males with the same worldviews. The proportion of females with these worldviews account for the observed gender difference.

JEL Classification: C91; C11; J16

Keywords: Honesty; Cultural Worldviews; Gender; Behavioral Economics; Experiments

Contact information: School of Business and Economics, UiT – the Arctic University of Norway, NO-9037 Tromsø, Norway, tel: +47, e-mail: johanbirkelund@gmail.com

Acknowledgements: The author would like to express gratitude towards Todd L. Cherry and Dave McEvoy for help in designing and running the survey, and Øystein Myrland for help on Bayesian modeling. I also appreciate helpful comments received from Eirik E. Heen, Stein Østbye, Sverre T. Braathen and participants at the 2019 ESA North American conference.

1. Introduction

A huge literature within economics and social psychology the last decades have provided mounting evidence that preferences for honesty vary within the population. In a prominent meta study, Abeler et al. (2019) report that while people are dishonest in experiments, only a small proportion (one quarter), of the potential gains of dishonesty is taken, indicating that people exhibit strong preferences for honesty. An important part of the ongoing research in this field is focused on identifying which characteristics affect individual preferences for honesty. In an effort to identify such factors, an emerging part of the literature considers cultural differences. When considering cultural differences, researchers often compare behavior across geographical locations, using location as a proxy for culture. Results from studies in various honesty tasks between different locations have found inconsistent evidence for differences. While some find that honesty levels differ (Cohn et al., 2019; Dieckmann et al., 2016; Gächter and Schulz, 2016; Hugh-Jones, 2016), others fail to detect any difference (Mann et al., 2016; Pascual-Ezama et al., 2015). In contrast to comparing honesty across geographical location, some researchers have applied a different approach for examining cultural differences. For instance, when Mazar and Aggarwal (2011) primed their subjects as either individualists or collectivists, they found that collectivistic subjects were more likely to use bribes in a hypothetical decision. By using family background from East Germany as a proxy for exposure to socialism, Ariely et al. (2019) found family background to be a good predictor for dishonest behavior, indicating that people brought up in socialism are less honest. Rather than using priming, geographical location or family background as a proxy for culture, this paper extend this literature by considering a measure of individual cultural worldviews as a predictor for preferences for honesty. Cultural worldviews, defined as a socially constructed orientation that dictates how one interpret and interact with society, has previously shown to be a strong predictor of individual preferences in various social issues such as gun control and climate change (Kahan et al., 2011), free-riding in public goods games (Cherry et al., 2017b) and opposition to environmental policies (Cherry et al., 2017a). Following the cultural cognition literature, which postulates that adherents of hierarchical and individualistic worldviews are less tolerant towards social deviance (Dake, 1991; Kahan, 2008), the

conjecture is that these individuals will have higher preferences for honesty. To inform the conjecture, this paper combines a short form survey of cultural worldviews based on Kahan et al. (2011), and a variant of a coin-flip task commonly used in honesty experiments (e.g. Bucciol and Piovesan, 2011), which enables an investigation of possible interactions between cultural worldviews, gender and honesty in a coin-flip task. Results indicate that individual cultural worldviews matter for females, but not for males. In line with the literature, I observe gender differences in honesty. By including cultural worldviews, this paper provides new evidence contributing to a more nuanced understanding of the gender difference in preferences for honesty.

2. Study design

This paper utilize a variation of the commonly used coin-flip task from the honesty literature, where subjects are asked to report the outcome of ten coin-flips each paying \$0.25 per "heads" [1][2]. Since it was impossible to observe the actual outcome of the coin-flips, subjects could over-report with impunity. The drawback of this approach is that any difference in honesty can only be determined at the aggregate level. By comparing the reported number of successful coin-flips between genders and worldviews, it is possible to determine differences in honesty.

Immediately following the coin-flip task, subjects were asked to complete a survey designed to measure their individual cultural worldviews. In this study, and following the literature (e.g. Kahan et al., 2011), cultural worldview is defined as distinct preferences individuals have over how they think society should be structured, which in turn influence how they interact with society. This study used a short form survey based on Kahan et al. (2011), which characterize subjects' worldviews along two dimensions: Hierarchy-Egalitarianism and Individualism-Communitarianism. The survey consists of 8 statements that subjects can agree or disagrees with on a 7 point

[1] In order to help subjects that did not have a coin nearby, I provided a link to a third party website (https://justflipacoin.com/).

[2] The experiment originally included two treatments, a private good and a public good. However, there were no difference in honesty between the treatments, which could be caused by the treatments not being salient enough. Kolmogorov-Smirnof tests revealed that the data had the same distribution in both treatments, and the data was subsequently pooled. Instructions can be found in the appendix.

Likert-scale. Four statements relates to each dimension. When interpreting the data, subjects are given a score based on their response, which is then added for each dimension, before subjects are classified based on a median split within each dimension. Scores, and medians are graphically shown in Figure 1.

Following the seminal work by Douglas and Wildavsky (1983), Kahan et al. (2007) developed a framework of cultural cognition, where individuals could be placed within two cross-cutting cultural worldview dimensions. The first dimension, "Hierarchy – Egalitarianism" determine individuals relative orientation toward High/Low grid, relating to preferences for social ordering, while the other dimension, "Individualist – Communitarian" relates the individuals relative orientation towards weak or strong group ways of life (Kahan, 2008). Literature within cultural cognition hints that individuals holding hierarchical and individualistic worldviews may hold stronger social norms. According to Dake (1991) "Adherents of hierarchy scrutinize social behavior for acts of social deviance because they find insubordination to authority a threat to their preferred form of social relations" (Dake, 1991, p. 66). Kahan (2008) suggests that individualists may be more concerned with social deviance, as it might threaten social order and lead to worse outcomes for the public. Since being dishonest easily can be considered breaking social norms, the conjecture is that hierarchical and individualists will be less inclined to act dishonest in this study.

I originally recruited 702 subjects through Amazons Mechanical Turk. However, upon closer inspection, several duplicate IP-addresses were discovered. All but the first observation from each unique IP-address was deleted, leaving a total of 632 observations. Following the recent debate (e.g. Dennis et al., 2019; Kennedy et al., 2018) on how international respondents on MTurk have been able to answer surveys designed for US respondents through Virtual Private Servers (VPS) or proxies, the data collected was screened through the R-package "rIP" by Kennedy et al. (2018). This process uncovered several problematic responses flagged as 1 (21) and/or from without the US (23), these observations were subsequently dropped from the dataset, leaving 590 observations in total[3].

[3] Dropping observations from 702 to 590 did not alter the results in any way.

Table 1 Sample demographics

Gender		Age		Education		Income	
Male	62%	18-30	29%	Less than high school	0%	Less than $15,000	10%
Female	38%	31-48	52%	High school	16%	$15,000 to 24,999	13%
		49-67	17%	Some college	22%	$25,000 to 49,999	28%
		68 +	2%	2 year college	12%	$50,000 to 74,999	24%
				Bachelor degree	40%	$75,000 to 99,999	14%
				Graduate degree	10%	$100,000 to 124,999	6%
						$125,000 +	5%

3. Hypothesis

The research questions are organized in three parts. The first research question is focused around the two dimensions of cultural worldviews, *individualist – communitarian* and *hierarchical – egalitarian*. In line with the cultural cognition literature, the conjecture is that hierarchical and individualistic individuals report more honest results (Dake, 1991; Kahan, 2008).

The second research question relates to gender effects. The question is informed by testing the null hypothesis that there is a difference in the reported number of heads between genders. Following the literature, I expect males to report a higher number of successful coin-flips than females.

The third research question examines any possible interplay between gender and worldviews, and consists in two parts. Part one examines any difference in the reported number of heads between Individualist – Communitarian and Hierarchical – Egalitarian worldviews, within genders. Following the conjecture that individualists and hierarchicals consider social deviant behavior as a threat to their worldview, the conjecture is that individuals identifying with individualistic and hierarchical worldviews to report a lower number of successful coin-flips as compared to

collectivistic and hierarchical males. The second part considers possible gender differences within worldviews. I expect males to report a higher number of successful flips than females for all worldviews.

4. Results

The results are organized around the three research questions. The first research question address differences in honesty between worldviews. The data reveal no significant difference in the number of heads reported between any worldviews at the aggregate level.

Result 1: *There are no differences in reports between any of the four worldview-dimensions.*

Table 2: Average reported number of heads with associated *p-values*

	Number of Heads		
	Male	Female	*p-value*
Pooled	6.82	6.35	0.0049
Worldview			
Individualist	6.85	6.14	0.0025
Communitarian	6.77	6.57	0.4221
Hierarchical	6.95	5.93	0.0000
Egalitarian	6.60	6.82	0.3639

In order to address the following research questions, the data is disaggregated by gender. The second research question considers whether there are gender differences in the reported number of successful coin-flips. Table 2 reports the average number of heads for males and females with the associated *p*-value[4] between them. Figures 1 and 2 accompany this table to give a graphical presentation of the distributions of reported heads. From table 2 we can see that males report on average 0.47 more successful coin-flips than females (p=0.0049). This result is in line with several results in the literature, stating that males tend be less honest than females, for a recent review of the literature, please see Gerlach et al. (2019) or Abeler et al. (2019).

Result 2: *There are gender differences.*

[4] *p*-values reported are Welch's t-tests with unequal variance.

The first part of the third research question focus on the possible interaction between cultural worldviews and gender. The point of departure is examining whether different worldviews affect the level of reported successes for each gender. The data reveal no difference for males - individual worldviews does not matter. I observe no significant differences in either the Individualist – Communitarian-dimension (6.85 vs. 6.77; $p = 0.6932$), or the Hierarchical – Egalitarian dimension (6.95 vs. 6.60; $p = 0.1272$).

Result 3.1: *Worldviews does not matter for males.*

On the contrary, individual worldviews seems to matter for females. There is a marginally significant difference in the Individualist – Communitarian dimension (6.14 vs. 6.57; $p = 0.0916$), and a large and significant difference in the Hierarchical – Egalitarian dimension (5.93 vs. 6.83; $p = 0.0004$).

Result 3.1: *Worldviews matter for females.*

The second part of my third research question relates to gender differences within worldviews. Focusing first on the individual-communitarian dimension, I find that individualist males report significantly more successes than individualist females (6.85 vs. 6.14; $p = 0.0025$). There is no observed difference between male and female communitarians (6.77 vs. 6.57; $p = 0.4221$). In the Hierarchical – Egalitarian dimension, hierarchical males report 1.02 more successes than hierarchical females, which is significantly more (6.95 vs. 5.93; $p = 0.0000$), and there are no gender differences in egalitarian worldviews.

Result 3.2: *Large and significant gender differences within individualistic and hierarchical worldviews.*

4.2 Bayesian analysis

To accompany the t-tests, I have estimated a Bayesian regression model with number of heads as the dependent variable and gender and worldviews as independent variables. Since the data generating process behind 10 fair coin-flips is known, the nature of the data collected in this study is ideal for a Bayesian analysis. This prior information about the data generating process can be incorporated into the process of estimating parameters in a Bayesian model.

$$NumberOfHeads = Gender_{1\ or\ 0} * Egalitarian_{1\ or\ 0} * Communitarian_{1\ or\ 0}$$

The model is estimated with the prior distribution set to the theoretical predicted outcome of 10 coin-flips for an honest individual (Normal distribution, mean = 5, with standard deviation 1.5). Model output is summarized in table 2.

Table 2: Estimates of the Bayesian regression model.

	Estimate	Est. Error	Lower 95%	Upper 95%	R-hat	Bulk ESS	Tail ESS
Intercept	5.88	0.21	5.48	6.29	1	2641	2951
Male	1.00	0.26	0.50	1.51	1	2446	3004
Egalitarian	0.79	0.37	0.05	1.54	1	1856	2286
Communitarian	0.11	-0.62	-0.62	0.82	1	1907	2715
Male Egalitarian	-0.91	0.53	-1.95	0.12	1	1830	2365
Male Communitarian	0.14	0.48	-0.83	1.07	1	1845	2399
Egalitarian Communitarian	0.11	0.54	-0.93	1.14	1	1558	2126
Male Egalitarian Communitarian	-0.60	0.74	-2.01	0.85	1	1597	2265

Note: **Parameters where the lower and upper 95 credibility interval does not include 0, can be interpreted in the frequentist way "significant".**

The Intercept of this model is interpreted as a female with individualistic and egalitarian worldview. In order to compare the estimated outcome between genders and worldviews in a sensible way, the corresponding estimates from the model is added and reported in Table 3[5]. This table is accompanied by Figure 5, which provides a visualization of the full posterior distributions, including the 95% credibility intervals.

[5] The estimations in the table is done using the *Hypothesis* function in the *R*-package *brms*, which also provide the correct 95% Credible Intervals.

Table 3: Added estimates from the Bayesian model, with credibility intervals.

	Female vs. Male		
	Estimate	**Lower 95%**	**Upper 95%**
Individualist Hierarchical	-1	-1.51	-0.5
Individualist Egalitarian	-0.09	-0.98	0.83
Communitarian Hierarchical	-1.14	-1.93	-0.34
Communitarian Egalitarian	0.37	-0.23	0.96

The model estimates that females with individualistic and hierarchical worldviews on average report 1 coin-flip less than males with the same worldview, with a 95% certainty that this estimate lies between 0.5 and 1.51 coin-flips, which indicate that females holding these worldviews are more honest than their comparable males. Comparing Communitarian-Hierarchical females to males, the model estimates an average of 1.14 less successful coin-flips with a 95% certainty between 1.93 and 0.34, which implies that Communitarian-Hierarchical worldviews effects the level of honesty between genders. However, when comparing for Individualist Egalitarian and Communitarian-Egalitarian, the credible interval contains zero, which provides no evidence for differences.

These results provides strong evidence that individualist-hierarchical and communitarian-hierarchical females are more honest that their male counterparts.

5. Conclusion

By comparing the reported number of heads between genders, the results are in line with the literature, indicating less honesty amongst men. However, when considering individual cultural worldviews, I find that for some worldviews, specifically those who identify with communitarian and egalitarian worldviews, men and women are equally (dis)honest. These results indicate that the gender difference in preferences for honesty may be more nuanced than previously thought. The gender differences observed in this study are driven by a group of females, which are significantly more honest than everyone else. The main result of this paper materializes when

investigating the interaction between gender and cultural worldview. I identify that females scoring high in two specific cultural worldviews, are significantly more honest than all other groups in the study, and that they alone explain the difference in honesty between genders. As far as the author knows, this is the first paper to investigate the possible linkage between gender and cultural worldviews in an honesty task. Surprisingly, this is also the first paper to use a Bayesian framework for analyzing results from an honesty task. Since the data generating process is known, this data is well suited for Bayesian analysis. In short, this paper sheds new light on the previously reported findings that males in general are less honest than females, and makes a compelling argument that individual cultural worldviews is a good predictor for the gender differences in honesty.

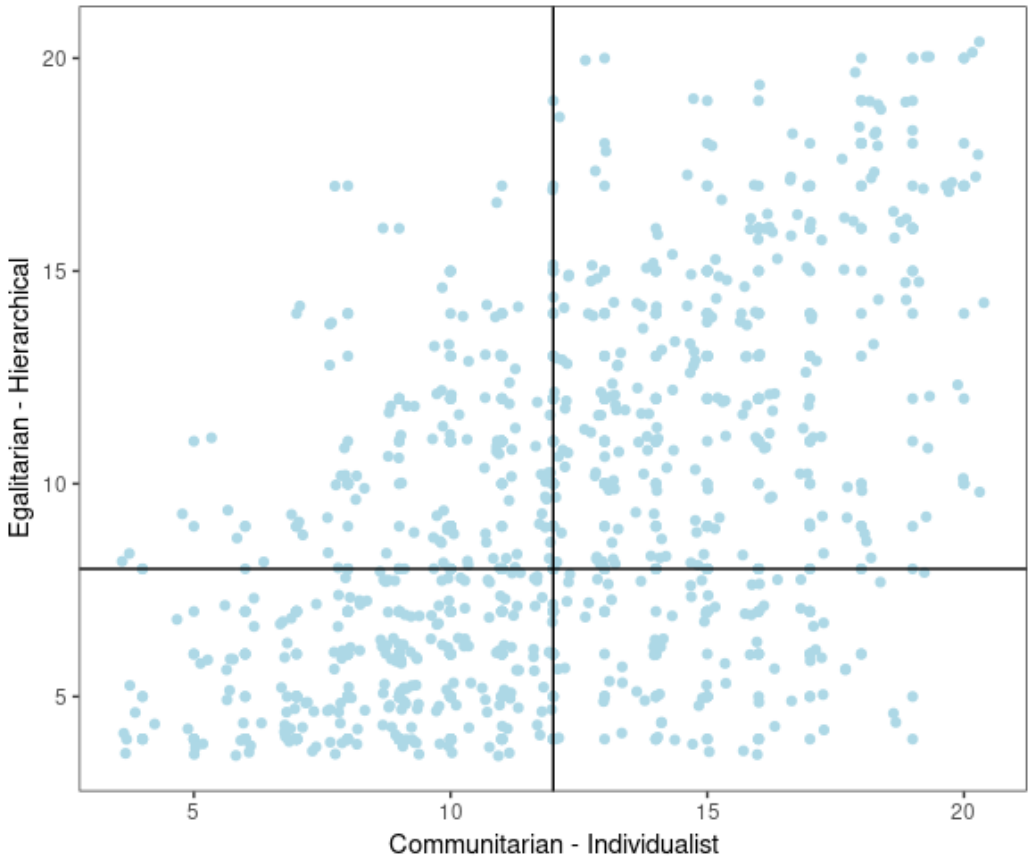


Figure 1: Scatterplot with jitter for measurement scales, lines represent median for each dimension. Subjects scoring below median in the Communitarian-Individualist dimension are classified as Communitarian. Likewise, subjects scoring above median in Egalitarian-Hierarchical are classified as Hierarchical.

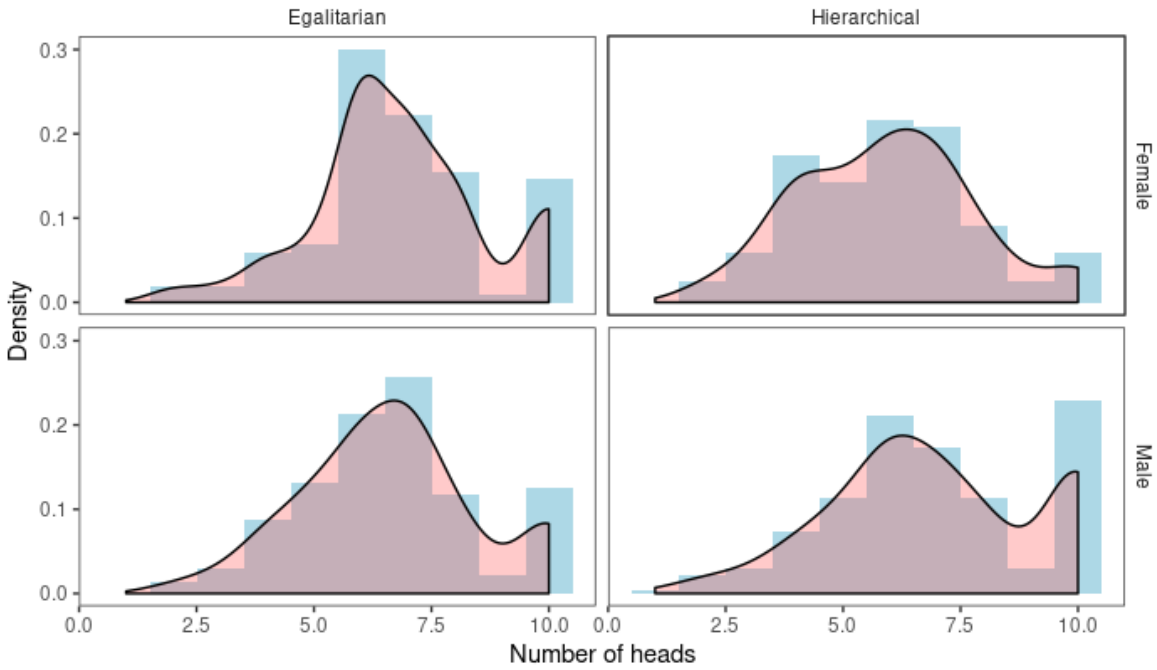


Figure 2: Egalitarian - Hierarchical. Histogram of reported coin-flips, with density plot. Disaggregated by gender.

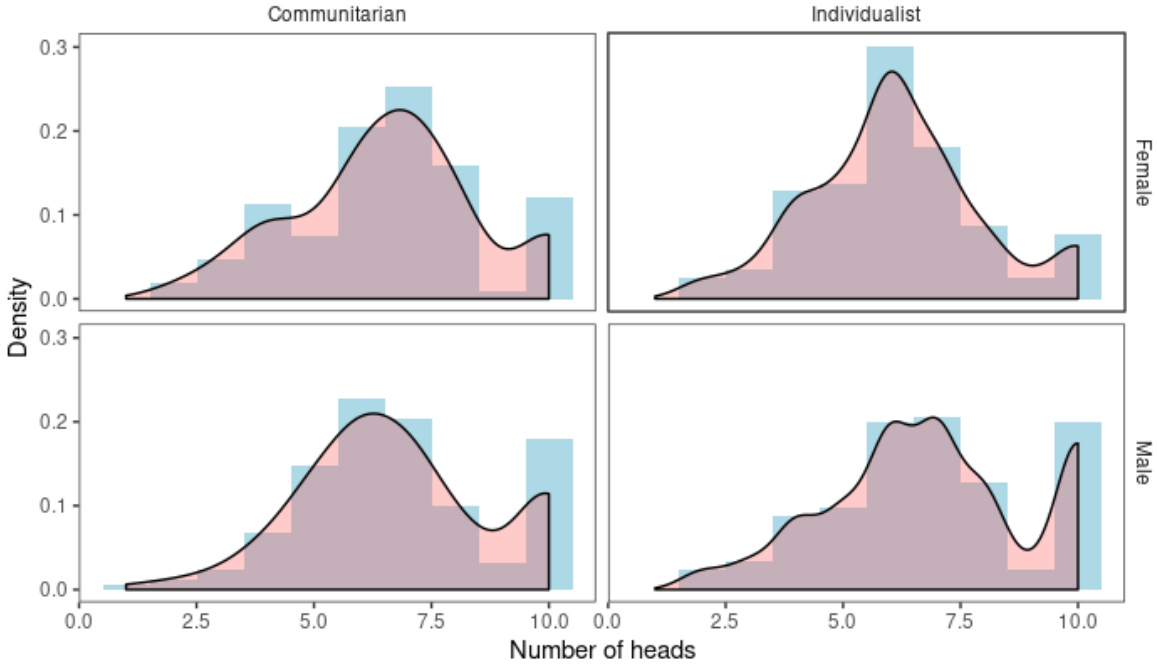


Figure 3: Communitarian - Individualist. Histogram of reported coin-flips, with density plot. Disaggregated by gender.

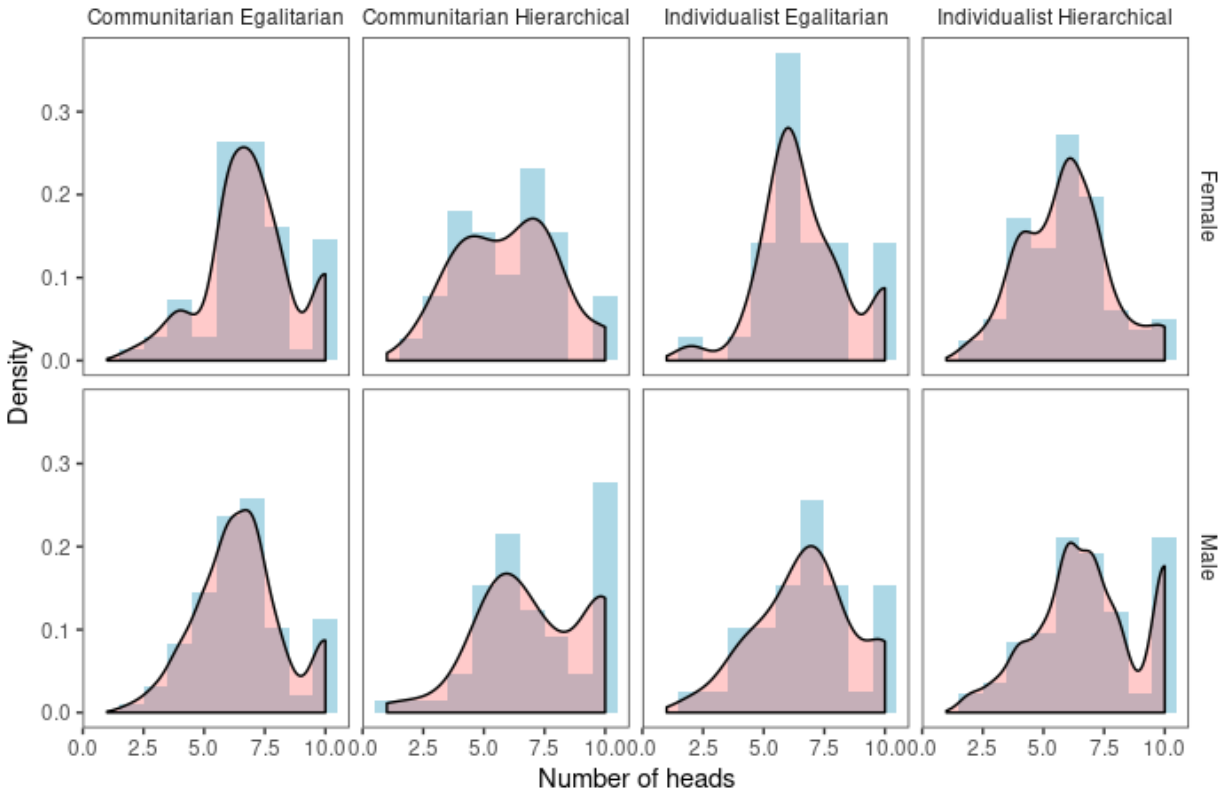


Figure 4: Histogram with density-plot for number of heads by worldviews. Disaggregated by gender.

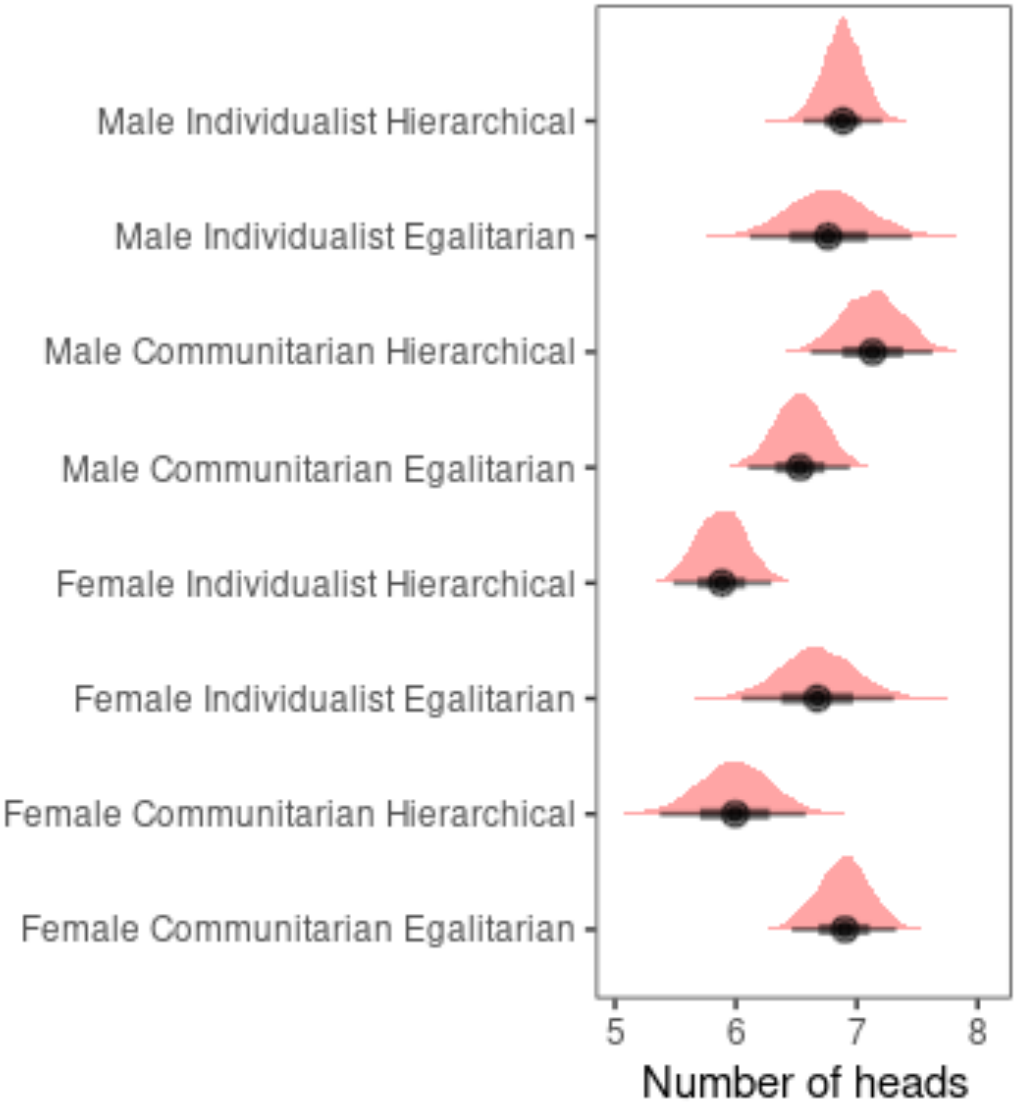


Figure 5: Posterior distribution of reported coin-flips by gender and worldview.

		Number of Heads		
	Pooled	Female	Male	*p-value*
Pooled	6.64	6.35	6.82	0.0049
By Worldview				
Communitarian – Egalitarian	6.87	6.90	6.53	0.2187
Communitarian – Hierarchical	6.70	6.00	7.12	0.01115
Individualist – Egalitarian	6.73	6.68	6.77	0.8538
Individualist – Hierarchical	6.56	5.90	6.88	0.0002444

References:

Abeler, J., Nosenzo, D., Raymond, C., 2019. Preferences for truth‐telling. Econometrica, 87(4), 1115-1153.

Ariely, D., Garcia-Rada, X., Gödker, K., Hornuf, L., Mann, H., 2019. The impact of two different economic systems on dishonesty. European Journal of Political Economy.

Bucciol, A., Piovesan, M., 2011. Luck or cheating? A field experiment on honesty with children. Journal of Economic Psychology, 32(1), 73-78.

Cherry, T.L., Kallbekken, S., Kroll, S., 2017a. Accepting market failure: Cultural worldviews and the opposition to corrective environmental policies. Journal of Environmental Economics and Management, 85, 193-204.

Cherry, T.L., McEvoy, D.M., Sælen, H., 2017b. Conditional cooperation and cultural worldviews. Economics Letters, 158, 51-53.

Cohn, A., Maréchal, M.A., Tannenbaum, D., Zünd, C.L., 2019. Civic honesty around the globe. Science, eaau8712.

Dake, K., 1991. Orienting dispositions in the perception of risk: An analysis of contemporary worldviews and cultural biases. Journal of Cross-Cultural Psychology, 22(1), 61-82.

Dennis, S.A., Goodson, B.M., Pearson, C., 2019. Virtual private servers and the limitations of IP-based screening procedures: Lessons from the MTurk quality crisis of 2018. Available at SSRN 3233954.

Dieckmann, A., Grimm, V., Unfried, M., Utikal, V., Valmasoni, L., 2016. On trust in honesty and volunteering among Europeans: Cross-country evidence on perceptions and behavior. European Economic Review, 90, 225-253.

Douglas, M., Wildavsky, A., 1983, Risk and culture: An essay on the selection of technological and environmental dangers. Univ of California Press.

Gerlach, P., Teodorescu, K., Hertwig, R., 2019. The truth about lies: A meta-analysis on dishonest behavior. Psychological bulletin, 145(1), 1.

Gächter, S., Schulz, J.F., 2016. Intrinsic honesty and the prevalence of rule violations across societies. Nature, 531(7595), 496.

Hugh-Jones, D., 2016. Honesty, beliefs about honesty, and economic growth in 15 countries. Journal of Economic Behavior & Organization, 127, 99-114.

Kahan, D.M., 2008. Cultural cognition as a conception of the cultural theory of risk. HANDBOOK OF RISK THEORY, S. Roeser, ed., Forthcoming, 08-20.

Kahan, D.M., Braman, D., Gastil, J., Slovic, P., Mertz, C., 2007. Culture and identity‐protective cognition: Explaining the white‐male effect in risk perception. Journal of Empirical Legal Studies, 4(3), 465-505.

Kahan, D.M., Jenkins‐Smith, H., Braman, D., 2011. Cultural cognition of scientific consensus. Journal of risk research, 14(2), 147-174.

Kennedy, R., Clifford, S., Burleigh, T., Jewell, R., Waggoner, P., 2018. The shape of and solutions to the MTurk quality crisis. Available at SSRN 3272468.

Mann, H., Garcia-Rada, X., Hornuf, L., Tafurt, J., Ariely, D., 2016. Cut from the same cloth: Similarly dishonest individuals across countries. Journal of Cross-Cultural Psychology, 47(6), 858-874.

Mazar, N., Aggarwal, P., 2011. Greasing the palm: Can collectivism promote bribery? Psychological science, 22(7), 843-848.

Pascual-Ezama, D., Fosgaard, T.R., Cardenas, J.C., Kujal, P., Veszteg, R., de Liaño, B.G.-G., Gunia, B., Weichselbaumer, D., Hilken, K., Antinyan, A., 2015.

Context-dependent cheating: Experimental evidence from 16 countries.
Journal of Economic Behavior & Organization, 116, 379-386.

Appendix:

Common for both treatments:

Appalachian STATE UNIVERSITY.

This survey is being conducted by researchers at Appalachian State University in Boone, North Carolina.

The survey should take less than 10 minutes to complete.

Participation is completely voluntary, and even after you begin, you can change your mind and stop at any time. However, payment will only be made for complete surveys. Once you complete the survey, a confirmation code will be displayed. YOU MUST ENTER THE CONFIRMATION CODE IN THE DESIGNATED BOX ON THE MTURK WEBSITE.

We promise that all responses will be kept confidential, and we will not access any personally identifiable information about you that you may have put on your Amazon public profile page. We will delete your MTurk worker ID after the survey is complete, and any resulting data will not be associated with you.

If you have questions about this survey, you may contact Professor McEvoy at mcevoydm@appstate.edu

By continuing to the research procedures, I acknowledge that I am at least 18 years old, have read the above information, and agree to participate.

>>

Appalachian STATE UNIVERSITY.

Thank you for participating. In addition to the $0.75 you earn just for completing this MTurk HIT, you will earn more money as a **BONUS** payment.

The amount of money you earn as a BONUS will depend partly on chance and partly on your decisions (your BONUS could range from $0.00 to $2.50)

Please pay attention to the instructions on the next page.

>>

Private Good Treatment:

Appalachian STATE UNIVERSITY

INSTRUCTIONS

You are not part of a group. Your earnings only depend on your decisions.

TASK: Your task is to flip a coin 10 times and report the number of HEADS you flipped.

EARNINGS: For every HEADS you flip, you will earn $0.25. Your total earnings will be $0.25 x the number of HEADS flipped out of the 10 coin flips.

INDIVIDUAL ACCOUNT: Your total earnings will be placed an individual account.

PAYMENT: **Your payment will be the earnings placed in your individual account**. You will receive your payment as a BONUS on MTurk after the experiment concludes (within three days).

>>

Public Goods Treatment:

Appalachian STATE UNIVERSITY

INSTRUCTIONS

You will be placed in a group of four. Your earnings depend on your decisions and the decisions of the members in your group.

TASK: Your task is to flip a coin 10 times and report the number of HEADS you flipped.

EARNINGS: For every HEADS you flip, you will earn $0.25. Your total earnings will be $0.25 x the number of HEADS flipped out of the 10 coin flips.

GROUP ACCOUNT: **Your total earnings will be placed a group account**. The earnings of the other three members of your group will also be placed in the same group account. The total earnings placed in the group account from all four members will be multiplied by 1.6 and shared equally among you and the other three group members.

PAYMENT: **Your payment will be the earnings from the group account.** The research team will calculate the earnings for the group and you will receive your payment as a BONUS on MTurk after the experiment concludes (within two days).

Common for both treatments:

Appalachian STATE UNIVERSITY.

START

Flip a coin 10 times. It might help to jot down the result of each flip to keep track of your 10 flips.
If you don't have a coin handy, you may visit the following website and flip a virtual coin.

http://justflipacoin.com

How many HEADS did you get out of 10 flips?

>>

Appalachian STATE UNIVERSITY.

To receive payment you must complete the remainder of the survey questions and correctly input your confirmation code back into MTURK.

>>

Short form Cultural Worldviews Survey

Appalachian STATE UNIVERSITY

For the next set of questions, please indicate how much you agree or disagree with the following statements.

Government interferes too much in our everyday lives.

- ○ Strongly Agree
- ○ Agree
- ○ Neither Agree nor Disagree
- ○ Disagree
- ○ Strongly Disagree
- ○ Don't Know

Sometimes government needs to make laws that keep people from hurting themselves.

- ○ Strongly Agree
- ○ Agree
- ○ Neither Agree nor Disagree
- ○ Disagree
- ○ Strongly Disagree
- ○ Don't Know

The government should do more to advance society's goals, even if that means limiting the freedom and choices of individuals.

- ○ Strongly Agree
- ○ Agree
- ○ Neither Agree nor Disagree
- ○ Disagree
- ○ Strongly Disagree
- ○ Don't Know

It's not the government's business to try to protect people from themselves.

- ○ Strongly Agree
- ○ Agree
- ○ Neither Agree nor Disagree
- ○ Disagree
- ○ Strongly Disagree
- ○ Don't Know

Our society would be better off if the distribution of wealth was more equal.

- ○ Strongly Agree
- ○ Agree
- ○ Neither Agree nor Disagree
- ○ Disagree
- ○ Strongly Disagree
- ○ Don't Know

Sometimes government needs to make laws that keep people from hurting themselves.

- ○ Strongly Agree
- ○ Agree
- ○ Neither Agree nor Disagree
- ○ Disagree
- ○ Strongly Disagree
- ○ Don't Know

The government should do more to advance society's goals, even if that means limiting the freedom and choices of individuals.

- ○ Strongly Agree
- ○ Agree
- ○ Neither Agree nor Disagree
- ○ Disagree
- ○ Strongly Disagree
- ○ Don't Know

Society as a whole has become too soft.

- ○ Strongly Agree
- ○ Agree
- ○ Neither Agree nor Disagree
- ○ Disagree
- ○ Strongly Disagree
- ○ Don't Know

>>

Appalachian STATE UNIVERSITY

What region of the United States do you live in?

- ○ Northeast
- ○ Midwest
- ○ South
- ○ West

What is your gender?

- ○ Male
- ○ Female
- ○ Other

What is your age?

- ○ 18-30
- ○ 31-48
- ○ 49-67
- ○ 68 +

Please specify your ethnicity

- ○ White, non-Hispanic
- ○ Hispanic or Latino
- ○ Black or African-American
- ○ Native American
- ○ Asian or Pacific Islander
- ○ Other

Generally speaking, you usually think of yourself as a ...

- ○ Strong Conservative
- ○ Conservative
- ○ Independent
- ○ Liberal
- ○ Strong Liberal
- ○ Other
- ○ Don't Know
- ○ Refuse

What is the highest level of education that you have completed?

- ○ Less than High School
- ○ High School (or G.E.D.)
- ○ Some College (no degree)
- ○ Techical/Associates Degree
- ○ Bachelor's Degree
- ○ Graduate Degree

As close as you can recall, what is your household's total annual income before taxes?

- ○ Less than $15,000
- ○ $15,000 to 24,999
- ○ $25,000 to 49,999
- ○ $50,000 to 74,999
- ○ $75,000 to 99,999
- ○ $100,000 to 124,999
- ○ $125,000 +

>>

Cash or Report – comparing two payment schemes in a real effort honesty task.

Johan Birkelund

School of Business and Economics, UiT – the Arctic University of Norway

Abstract: Through a real effort experiment, this paper compares honesty levels between two payment-schemes in the commonly used matrix task. Subjects in the experiment could either dishonestly inflate their earnings by paying themselves more than entitled to in private, or, report to have solved more matrices than entitled to. In addition to the experiment, this paper also reports from a meta-study on 101 experimental real effort matrix task treatments using one of the two payment schemes. In contrast to studies using randomly generated outcomes, this paper provides evidence that the choice of payment scheme in the matrix task has no significant effect on the level of honesty.

JEL Classification: C91

Keywords: Honesty; Behavioral Economics; Experiments

Contact information: School of Business and Economics, UiT – the Arctic University of Norway, NO-9037 Tromsø, Norway, tel: +47 992 78 222, e-mail: johanbirkelund@gmail.com

Acknowledgements: I would like to express gratitude to Eirik E. Heen for help in designing and conducting the experiment and providing helpful comments along the way. In addition, I want to thank Heidi M. Hemmingsen and Ingrid Dyrkorn Heimland, for excellent research assistance in the laboratory, Stein Østbye for helpful comments, Agne Kajackaite for fruitful discussion, and Philip Gerlach for invaluable help in the meta-analysis.

1. Introduction

Honesty, and trust towards others, can be considered a crucial factor in economic transactions, and of the social contract in general. In the late sixties, Becker (1968) explained how dishonest behavior was a result of a rational cost-benefit analysis. The basic concept was that in any situation involving private information where someone could increase their income through misrepresentation of information, without any risk of sanctions, we should expect them to choose the income maximizing strategy. Surprisingly to some, this is not what we observe. Recent meta-studies like Abeler et al. (2019) and Gerlach et al. (2019), even show that people leave most of the potential gains from misrepresenting private information on the table. The past decades have resulted in a plethora of research papers from several fields, including economics and social psychology, in an effort to understand individuals' preferences for honesty. These preferences, have been shown to be context dependent, and influenced by amongst others, unequal treatment (e.g. Birkelund and Cherry, 2020), unfair treatment (Houser et al., 2012), loss framing (e.g. Garbarino et al., 2018; Schindler and Pfattheicher, 2017) and normative cues (e.g. Mazar et al., 2008). In addition, the literature indicates that preferences for honesty may be dependent on the source of potential gains. For instance, Gravert (2013) report that dishonest behavior was more prevalent for subjects exerting real effort, rather than when the outcome was determined at random. In contrast, when Kajackaite (2018) directly compares honesty levels between real-effort and random-draw task, she finds that subjects report less honestly when the gains origins from a random-draw. This suggests that researchers ought to be careful when comparing results from honesty experiments using randomly generated outcomes, to those where subjects exert real effort.

In a recent die-roll experiment, Hermann and Mußhoff (2019) aim to shed light on the moral cost of lying and stealing. In their experiment, they separate these moral costs by having subjects in one treatment report the outcome of a fair dice, hence lying, while in the other treatment subjects are given an envelope containing cash from which they can pay themselves according to the outcome of the dice, hence stealing. Their paper report that subjects were more dishonest when the task was framed as lying rather than stealing, arguing that subjects may have a lower moral cost associated with lying. However, a different mechanism regarding this finding, which is also mentioned by the authors, could be that subjects experienced a

tangibility effect in the honesty task. Merely dishonestly reporting an outcome may entail lower moral cost, than pocketing actual cash. This effect has been observed in an experiment where subjects paid themselves more, when they could pay themselves in tokens rather than cash (Mazar et al., 2008). Regardless of which mechanism leads to Hermann and Mußhoff (2019)'s result, it is clear that their treatments have an effect on preferences for honesty in an experiment regarding randomly generated outcomes.

Within the other main paradigm of the honesty literature, the matrix task, subjects have to exert effort in solving simple mathematical matrices. This task usually employ one of two payment schemes, which are remarkably similar to the treatments in Hermann and Mußhoff (2019). If there is a meaningful difference in the moral cost of lying and stealing, which is measurable in the same way as in Hermann and Mußhoff (2019), then, it follows, that experiments using the matrix task may be measuring two different things due to its interchangeable use of the two payment schemes. This calls for a thorough investigation. Herein, this paper report results from a real effort matrix task experiment, with treatments corresponding to the two payment schemes, and a meta-analysis of previously published matrix task experiments mediated by payment scheme.

2. Experimental Design

2.1 Honesty task

This experiment utilizes the matrix task from Mazar et al. (2008), where subjects can earn money by solving simple mathematical problems. Subjects are presented with 15 different matrices consisting of 12 three-digit numbers. Their task is to find two unique numbers within each matrix summing to exactly 10.00, and are told they would earn 10 NOK (at the time about 1.2 USD) for each correctly solved matrix. To allow for individual level data, the current research follow the protocol of Friesen and Gangadharan (2012), where all materials, instructions, envelopes and questionnaires were coded with a unique number allowing for identifying all decisions after the fact. Subjects were assigned a station in the laboratory at random, at which they found

general instructions and two questionnaires[1]. After an instructor read the instructions[2] out aloud and answered questions in private, subjects had 15 minutes to complete both of the questionnaires. Immediately following the questionnaire part, subjects were provided instructions for the honesty task, as well as a large envelope containing the matrices, a cash- or report-envelope and their payment envelope. Instructions for the matrix-task were read aloud while subjects followed on paper, and questions were answered individually. Subjects had three minutes to solve as many matrices as they could. After the three minutes, subjects were instructed to count their correct answers and fill out the *report*, or allocate *cash* to their *payment* envelope, depending on treatment.

The experiment was conducted at the Laboratory for Economic Experiments in Tromsø (LEET) in January and February 2019. A total of 120 subjects were initially recruited through lectures at UiT the Arctic University of Norway, however, only 76 (31 female) subjects showed up for one of four sessions. The experimental sessions each lasted a total of 30 minutes. Participants earned on average about 94 NOK, including a 50 NOK participation payment.

2.2 Treatments

The experiment had two treatments. *Cash* and *Report*. In the *Cash* treatment, subjects were provided with a *cash envelope* containing ten 10 NOK coins. After solving as many matrices as they could in three minutes, they were instructed to allocate their earnings, corresponding to the number of correctly solved matrices, between the *cash envelope* and their *earnings envelope*. In the *Report* treatment, subjects were provided with a *report envelope* in which they found a payment slip with the wording "I correctly solved ___ matrices, paying 10 NOK each". Subjects were instructed to report the number of correctly solved matrices on this slip.

After the three minutes for controlling matrices and filling out the payment slip or allocating cash/tokens to their payment envelope, subjects were called individually to receive their 50 NOK show up fee as well as any payment corresponding to the reported number of matrices.

[1] These were a short form cultural worldviews-, and honesty-humility-survey. Results from these questionnaires will not be discussed in this paper.
[2] Complete instructions (in Norwegian) available in the appendix.

2.4 Hypothesis

The research hypothesis is focused between treatments, *Cash vs. Report*. We test the null, that dishonest behavior is equal between the *Cash*- and *Report* treatment. We will consider three different measures of honesty, the proportion of dishonest subjects, the proportion of subjects being maximal dishonest, and the proportion of available funds taken. In line with Hermann and Mußhoff (2019), the conjecture is that subjects will experience a higher cost of dishonesty in the *Report* condition, and thus report more truthfully in this condition, compared to in *Cash.*

3. Results

3.1 Experimental results

Results from the experiment are reported in Table 1, with a graphical representation of dishonesty in Figure 1. The top section of the table shows actual number of correctly solved matrices, the number of matrices reported, and the difference between these.

Figure 1: Scatterplot of Actual and Reported Earnings by treatment.

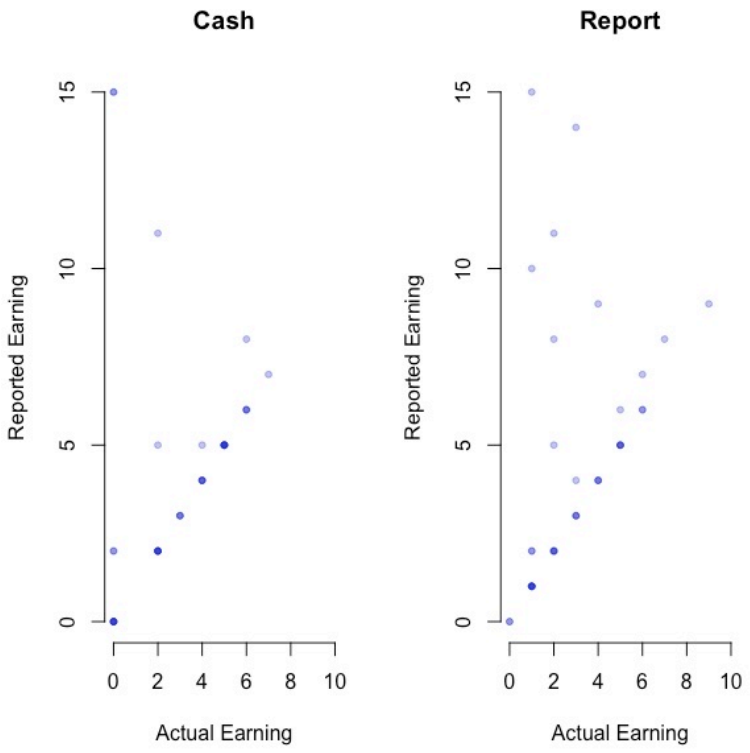


Table 1: Actual and reported earnings, by treatment

	Cash	**Report**
Production		
Correctly solved	2.92 (2.22)	3.0 (2.13)
Reported solved	4.21 (3.55)	4.67 (3.71)
Over-reported	1.29 (3.64)	1.66 (3.49
Fraud rates		
Take Some	21.05% (41,30)	34.21% (48.10)
Take All	5.26% (22.60)	2.63% (16.20)
% Taken	9.23% (24.81)	13.05% (26.92)
N	38	38

Starting with the top section of table 1, we first consider correctly solved matrices. Whether subjects pay themselves from an envelope containing cash, or report their outcome has no impact on their actual performance in the task (Cash vs. Report: 2.92 vs. 3.0; $p = 0.654$). In addition, there is no difference in how many matrices subjects reported to have solved (Cash vs. Report: 4.21 vs. 4.67; $p = 0.593$). Although subjects in the Report treatment over-report slightly higher than in Cash, this difference is not significant (Cash vs. Report: 1.29 vs. 1.66; $p = 0.653$).

Turning to the bottom section of table 1, we now consider three different measures of honesty. About one fifth of the subjects in the Cash treatment are dishonest, while in Report one third claimed to have solved more than they actually did. This difference was not significant (Cash vs. Report: 21.05 vs. 34.21: $p = 0.205$). Neither the proportion of subjects taking all of the available funds (Cash vs. Report: 5.26 vs. 2.63; $p = 0.562$), nor the percentage of available funds taken (Cash vs. Report: 9.23 vs. 13.06; $p=0.518$) was significant different.

This striking similarity in honesty levels between treatments, are in sharp contrast to the recent results reported from an honesty experiment where subjects act

on the result of a randomly generated outcome. In a die-roll experiment by Hermann and Mußhoff (2019) with otherwise similar instructions and treatments (Cash vs. Report), subjects in the report treatment claims to have higher success in the task than those in the Cash treatment.

3.2 Meta-analysis

This section of the paper complements the laboratory experiment by conducting a meta-analysis, extending on the meta analysis by Gerlach et al. (2019)[3]. To address our main research question, we have surveyed the 32 papers in the matrix-paradigm honesty task, containing 101 experimental treatments from the Gerlach et al. (2019) data, and added a variable with information about which payment scheme was used[4]. This allows for comparison of (dis)honest behavior between the two.

Following previous meta analyses on honesty (e.g. Abeler et al., 2019; Gerlach et al., 2019), this study also make use of standardized reports (SR) for comparing levels of honesty between Cash and Report experiments. The standardized reports used in the current research, are the same as those reported in Gerlach et al. (2019)[5].

Standardized reports are computed in the following way:

$$SR = \frac{m - t}{t - t_{min}} \; if \; m < t$$

and

$$SR = \frac{m - t}{t_{max} - t} \; if \; m \geq t$$

where m is the mean number of matrices reported as solved for each experimental treatment and t is the expected truthful report. For matrix tasks with individual level data, the t is known, and for matrix task using aggregate level data, t is the average number of solved matrices in the control group. The minimum and maximum number of matrices possible to claim as solved are represented by t_{min} and t_{max}.

---

[3] We gratefully acknowledge the permission from the authors to use their data.

[4] 14 papers (32 treatments) used the cash-scheme and the remaining used self-reported outcome.

[5] This paper will only focus on the two payment schemes, cash and report that where coded specifically for our purpose. For a complete description of how studies were selected, and different control variables, please see Gerlach et al. (2019)

Figure 2: Violin plot of Standardized Report, by payment scheme.

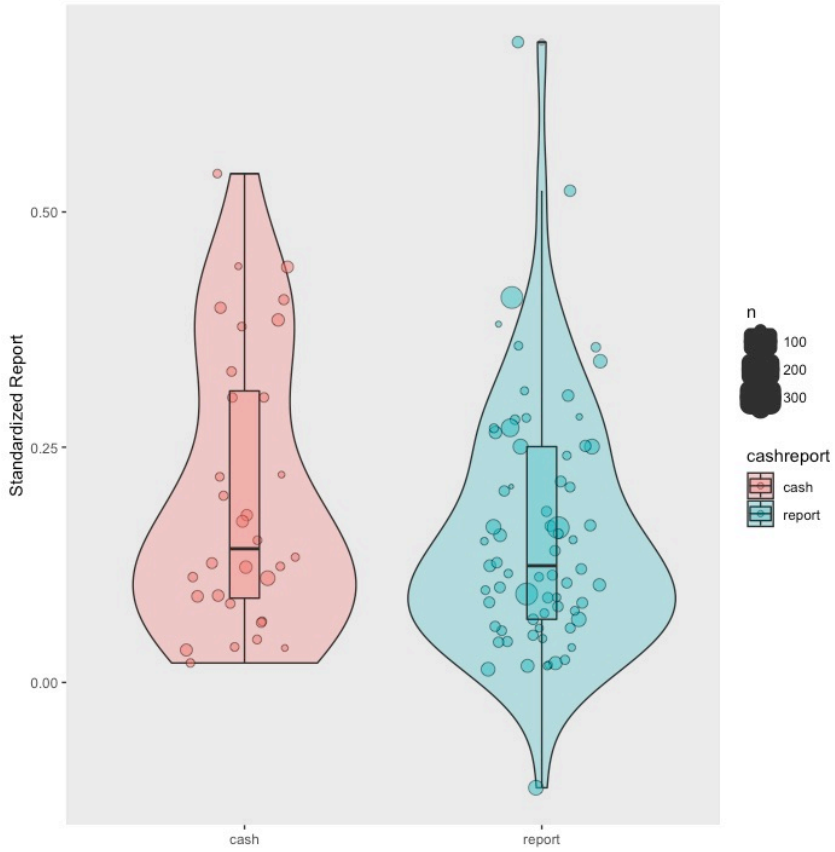


Figure 2 depicts a graphical representation of all matrix-task experiments, and is divided by payment scheme. The dots represent studies within each payment scheme, with the size according to number of participants. Within each violin, there is a boxplot indicating the mean Standardized Report, and interquartile ranges. For a forest plot of all included treatments, see figure 3. The forest plot is organized so standardized reports are in ascending order, and all treatments using reported outcome are presented first. By visually inspecting the violin- and forest-plot, it seems that there is a higher standardized report in experiments using *Cash*. However, the formal meta-analysis with *Cash* as a moderator, which is reported in table 2, shows that the coefficient for the moderator, *Cash*, is insignificant (3%, *p*-value = 0.360).

Table 2: Meta analytic random effect model, with Cash as moderator.

	Estimate	95% CI	z-value	p-value
Intercept	15% (0.016)	[12%,19%]	9.65	<0.000
Cash	3% (0.030)	[-3%, 9%]	0.916	0.360
Observations	$k = 101$			
	$n = 6093$			
Heterogeneity	$I^2 = 93.27\%$			
	$\tau^2 = 0.02$			
	$R^2 = 0.00\%$			

Note: Coefficients are the standardized report, with standard errors in parenthesis. *k* is the number of treatments, *n* is the combined number of participants, $\boldsymbol{I^2}$ is the study variance independent of the number of experiments, $\boldsymbol{\tau^2}$ is the between-study variance.

Table 3 reports the regression coefficients of a linear mixed effect model with random effects at study level. Based on the data from Gerlach et al. (2019), and adding a variable for cash, the model estimates the coefficients based on 101 experimental matrix-task treatments, from 32 unique studies. Considering the research question, whether one of the two commonly used payment schemes affect the level of honesty, we see that when subjects pay themselves in cash, the standardized report increase by 3%, however, this predictor is not significant (*p*-value = 0.408). Thus, we have supporting evidence that the way subjects receive their payment does not affect the levels of honesty in the matrix task.

In an effort to make honesty salient, several papers remind subjects to be honest, or tell subjects about the level of honesty of other participants (e.g. Gino et al., 2009; Mazar et al., 2008). The consensus from the literature is that such ethical cues work, where ethical reminders reduce dishonesty, and telling subjects about the dishonesty of others increase dishonesty (Gerlach et al., 2019). In line with the literature, we can see that normative cues reduce the standardized report by 10.67% (*p*-value = 0.0245).

Table 3: Predictors of Standardized Report.

	Standardized Report
Intercept	25% (13.64)
Cash Envelope	3% (3.75)
Laboratory Experiment	
Online/Telephone	- 4%(14.54)
Field experiment	- 6% (9.44)
Non-economics students	
Non-students	- 5% (14.03)
Economics students	5% (6.89)
Normative cues	- 11% (4.62) *
Experimental deception	3% (3.70)
Maximal Externality	0% (0.23)
Maximal gain	0% (0.05)
Observations	k =101
	n = 6093

Note: Linear mixed-effect model with random effects at study level. Standard errors in parenthesis. * p < 0.5.

4. Conclusion

Recently, it has been suggested that preferences for honesty differ when payment scheme differs (Hermann and Mußhoff, 2019). This has been interpreted as subjects having different moral costs associated with lying and stealing, and that the cost of lying is lower than for stealing, in an experiment with randomly generated outcome. Through an experiment and a meta-analysis, this paper has investigated whether this difference is observable in an honesty experiment with real-effort. Both the experiment and meta-study provide new evidence. The choice of payment-scheme has no effect on the level of honesty in the matrix task. The lack of generalizability of the effect, from a task with randomly generated outcome, to one with real effort, may be a result of different costs of dishonesty between the two. For instance, when Kajackaite (2018) directly compare levels of honesty between randomly generated outcomes and performance, she finds that subjects are less honest when they can lie about random

outcomes. The result reported in this paper is in line with the recent meta-analysis by Gerlach et al. (2019), which report that most effects found within one experimental paradigm, is not transferrable to other paradigms. This may also be the case for different payment schemes. Whether these payment schemes entail different moral considerations related to lying or stealing remains unclear. A difference between the two could be caused by tangibility. Future research should make an effort to disentangle the potential confound of tangibility in the cost of lying and stealing.

References

Abeler, J., Nosenzo, D., Raymond, C., 2019. Preferences for truth‐telling. Econometrica, 87(4), 1115-1153.

Becker, G.S., 1968. Crime and Punishment: An Economic Approach. Journal of Political Economy, 76(2), 169-217.

Birkelund, J., Cherry, T.L., 2020. Institutional inequality and individual preferences for honesty and generosity. Journal of Economic Behavior & Organization.

Friesen, L., Gangadharan, L., 2012. Individual level evidence of dishonesty and the gender effect. Economics Letters, 117(3), 624-626.

Garbarino, E., Slonim, R., Villeval, M.C., 2018. Loss aversion and lying behavior. Journal of Economic Behavior & Organization.

Gerlach, P., Teodorescu, K., Hertwig, R., 2019. The truth about lies: A meta-analysis on dishonest behavior. Psychological bulletin, 145(1), 1.

Gino, F., Ayal, S., Ariely, D., 2009. Contagion and differentiation in unethical behavior the effect of one bad apple on the barrel. Psychological science, 20(3), 393-398.

Gravert, C., 2013. How luck and performance affect stealing. Journal of Economic Behavior & Organization, 93, 301-304.

Hermann, D., Mußhoff, O., 2019. I might be a liar, but I am not a thief: An experimental distinction between the moral costs of lying and stealing. Journal of Economic Behavior & Organization, 163, 135-139.

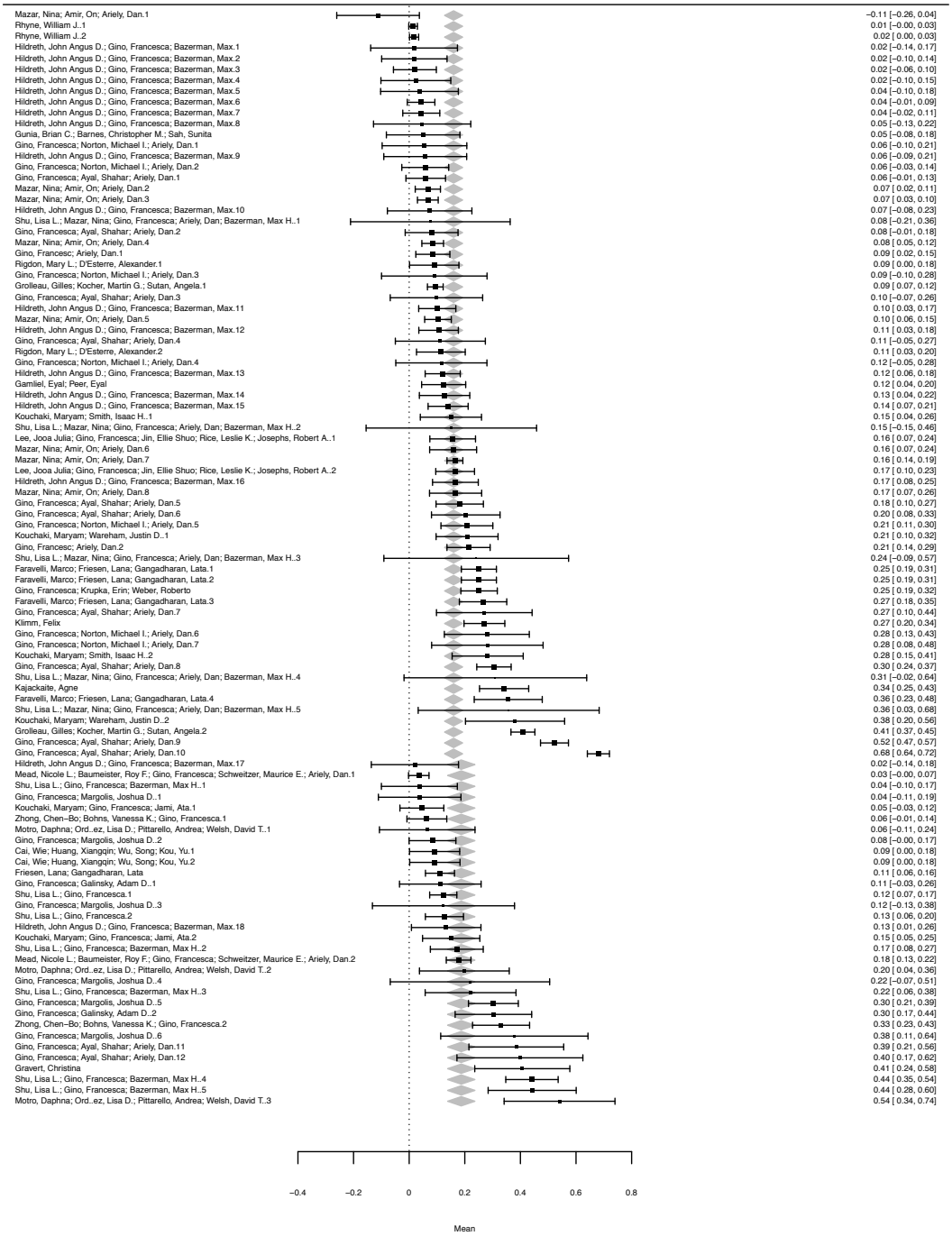
Houser, D., Vetter, S., Winter, J., 2012. Fairness and cheating. European Economic Review, 56(8), 1645-1655.

Kajackaite, A., 2018. Lying about luck versus lying about performance. Journal of Economic Behavior & Organization, 153, 194-199.

Mazar, N., Amir, O., Ariely, D., 2008. The dishonesty of honest people: A theory of self-concept maintenance. Journal of marketing research, 45(6), 633-644.

Schindler, S., Pfattheicher, S., 2017. The frame of the game: Loss-framing increases dishonest behavior. Journal of Experimental Social Psychology, 69, 172-177.

Figure 3 Forest plot of included treatments



Note: Treatments are ordered by standardized report in ascending order, and separated by payment scheme. Treatments using report are on stop.

Appendix

Takk for at du deltar i dette eksperimentet. Vennligst les instruksjonene nøye. En god forståelse av instruksjonene vil hjelpe deg å gjøre gode beslutninger og kan øke fortjenesten din. Det du tjener i dette eksperimentet er kun avhengig av dine beslutninger. **Det er ikke tillatt å kommunisere med noen andre deltakere**. Dersom du har noen spørsmål, vennligst rekk opp hånden, og en av eksperimentatorene vil hjelpe deg. Selv om det er mange deltakere i dagens eksperiment, så jobber alle individuelt. Dette betyr at det du tjener kun er basert på dine beslutninger, og hva andre gjør vil ikke ha noen innvirkning på deg eller det du tjener.

Alle beslutninger du tar i dag blir lagret gjennom en anonym deltaker-ID, og kun bli brukt i forbindelse med forskning. Det finnes ingen måte å koble din identitet til din deltaker ID. Alle dine beslutninger vil forbli anonym. Dine beslutninger i dette eksperimentet vil ikke på noen måte kunne få konsekvenser for deg senere.

Ettersom dette er et økonomisk eksperiment, kan du være trygg på at all informasjon du mottar fra oss er sannferdig, og at vi på ingen måte kan gi deg villedende informasjon. Dersom det kommer frem at vi har løyet eller villedet deltakerne, vil studiet ikke kunne publiseres.

Eksperimentet består av to deler:

I del en vil du få 15 minutter til å besvare spørreundersøkelsen som ligger på stasjonen din.

Instruksjoner for del to gis når alle har besvart del 1.

Del 2:

I den store konvolutten på stasjonen din, vil du finne to ark med 15 matriser av samme type som den du ser i eksemplet under. Vennligst ikke åpne konvolutten før vi starter eksperimentet.

3.91	0.82	3.75
1.11	1.69	7.94
3.28	2.52	6.26
9.81	6.09	2.46

Din oppgave, er å lete etter to tall i hver matrise, som sammen summerer til akkurat 10.00.

Når du har funnet to tall som summerer til 10.00, merker du tallene, og krysser av boksen under matrisen med teksten «Klarte det», slik som i eksemplet under:

3.91	0.82	3.75
1.11	1.69	7.94
3.28	2.52	6.26
9.81	6.09	2.46

Klarte det ☑

Du vil tjene 10 kroner for hver matrise du finner korrekt løsning til.

Du vil få tre (3) minutter til å fullføre denne oppgaven. Når de tre minuttene er gått, må du gjøre følgende:

1. Tell antallet matriser du fant løsningen på. Dette er din fortjeneste.
2. I den store konvolutten vil du også finne en liten konvolutt med femten (15) ti-kroninger. Du skal nå betale deg fra denne konvolutten. La ekstra ti-kroninger ligge igjen, før du limer igjen konvolutten.
3. Putt alle materialene på stasjonen, **bortsett fra betalingen din**, i den store konvolutten og lim den igjen. Den vil forbli lukket til alle deltakere har forlatt lokalet.
4. En av de som jobber på eksperimentet vil gå rundt med en lukket boks til hver stasjon, putt den store konvolutten som inneholder alt utenom betalingen din i boksen. Denne boksen vil ikke bli åpnet før alle deltakere har forlatt lokalet.
5. Bli sittende på stasjonen din inntil du blir henvist til en funksjonær, der du må signere for å motta deltakerbetalingen din (50kr). **Din signatur vil ikke kunne knyttes til dine beslutninger i eksperimentet.**

Del 2:

I den store konvolutten på stasjonen din, vil du finne to ark med 15 matriser av samme type som den du ser i eksemplet under. Vennligst ikke åpne konvolutten før vi starter eksperimentet.

3.91	0.82	3.75
1.11	1.69	7.94
3.28	2.52	6.26
9.81	6.09	2.46

Din oppgave, er å lete etter to tall i hver matrise, som sammen summerer til akkurat 10.00.

Når du har funnet to tall som summerer til 10.00, merker du tallene, og krysser av boksen under matrisen med teksten «Klarte det», slik som i eksemplet under:

3.91	0.82	3.75
1.11	1.69	7.94
3.28	2.52	6.26
9.81	6.09	2.46

Klarte det ☑

Du vil tjene 10 kroner for hver matrise du finner korrekt løsning til.

Du vil få tre (3) minutter til å fullføre denne oppgaven. Når de tre minuttene er gått, må du gjøre følgende:

1. Tell antallet matriser du fant løsningen på. Dette er din fortjeneste.
2. I den store konvolutten vil du også finne en liten konvolutt med en betalingslapp. Fyll inn hvor mange matriser du fant løsningen til på denne lappen.
3. Putt alle materialene på stasjonen, **bortsett fra betalingslappen din**, i den store konvolutten og lim den igjen. Den vil forbli lukket til alle deltakere har forlatt lokalet.
4. En av de som jobber på eksperimentet vil gå rundt med en lukket boks til hver stasjon, putt den store konvolutten som inneholder alt utenom betalingslappen din i boksen. Denne boksen vil ikke bli åpnet før alle deltakere har forlatt lokalet.
5. Bli sittende på stasjonen din inntil du blir henvist til en funksjonær. Når det er din tur til å motta betaling, gir du betalingslappen til vår funksjonær som vil betale deg tilsvarende det du har fylt inn. Etter dette må du signere for å motta

deltakerbetalingen din (50kr). **Din signatur vil ikke kunne knyttes til dine beslutninger i eksperimentet.**