Christian Schitter

Honesty, anonymity, shame and culture.

An experimental study in Austria and the United States.

Final report of a research project,
funded by the Austrian Marshall Plan Foundation.

September 2018

Abstract

The present study investigates whether the effect on honesty from making unverified reports public differs between Austria and the USA, and whether any such difference is influenced by a varying degree of proneness to shame between those two countries. In a series of experiments, I find that lifting anonymity of reporters of unverified claims increases honesty in both countries. Proneness to shame, however, is no significant factor of influence for the decision to report honestly neither under anonymity nor under no anonymity, and neither in Austria nor in the USA.

Keywords: Honesty, anonymity, shame, culture
JEL: D62, D82, D91, G22, H26

Acknowledgements

This research project was conducted as part of my doctoral studies at the University of Graz, the University of Innsbruck, and the University of California at Berkeley. These studies would not have been possible without the financial support from the Austrian Marshall Plan Foundation and the University of Graz, which is gratefully acknowledged. In Austria, I thank the staff of the labs of the Universities of Graz and Innsbruck for facilitating my research, as well as my supervisor Prof. Stefan Palan for his advice throughout my studies. In the USA, I thank the Institute of European Studies for hosting me while being in Berkeley, and Prof. Terrance Odean together with the XLab staff for providing the infrastructure and valuable comments for my experiments.

Contents

List of Tables

List of Figures

1. Introduction

1.1. Honesty and economic decision making[1]

Many economic situations and interactions heavily depend on the truthfulness of people or entities filing mostly unverified reports. At the same time, dishonesty can be profitable, and therefore occurs regularly, be it some minor cheating or outright criminal fraud. This ranges from fraudulent behavior with direct monetary consequences to everyday interaction and communication. Examples include corporate reporting fraud, (social) insurance claims fraud, tax evasion, expense claiming fraud, misrepresenting personal references and achievements, or overstating the quality of a product for sale.

The total damage that arises from dishonesty is inherently difficult to assess, but Köneke et al. (2015) estimate insurance fraud, for example, to amount to about 10% of total insurance premia paid. Tax evasion is likely to be an even more substantial issue: Slemrod (2007) report on estimates of the net tax gap after accounting for evaded taxes that are recovered by auditing to be 290 billion USD in the USA in 2001, or 13.7% of the estimated total tax liability. However high the real numbers are, there is no doubt that dishonest reporting is a significant source of economic inefficiency and that it binds considerable resources, which are lost due to fraud directly or which are invested in preventing and uncovering fraud. Understanding

[1]The majority of this section is identical to section 1.1 of the author's dissertation.

which factors foster or reduce honesty is therefore of great importance to address these issues.

When looking at this topic from an economic perspective, Becker (1968) proposes a theory that today is referred to as the economics-of-crime approach. Put in simple formal terms (Yaniv and Siniver, 2016), an agent in this model considers the following expected utility EU when deciding whether to commit a crime:

$$EU = (1 - p)U(g) + pU(g - f)$$

Here, g is the direct (monetary) payoff of committing the crime, p is the probability of being detected and uncovered, f is the monetary equivalent of the punishment if uncovered, and $U()$ is the agent's utility function. The agent will commit the crime whenever $EU > 0$. It is clear from this formulation that an agent in this model can only be deterred from committing a crime by punishment that is reasonably likely to occur. Consequently, the size of the punishment and the degree of auditing together define the total amount of criminal activity in a society. Individual differences between agents arise only due to different risk attitudes, as reflected in $U()$. In the extreme case where there is no penalty ($f = 0$) or no audit ($p = 0$) for a crime, everyone should always commit that crime. This model has shaped much of the discourse on crime in economics in general, but also on fraud in particular for the last decades.

Two questions come to mind here. The first question is in my view quite intuitive, but also an immediate consequence of the findings of behavioral economics that people are influenced by psychological and sociological mechanisms in economic decision-making (Rabin, 1998; DellaVigna, 2009). Unsurprisingly, it has gained increasing attention over recent years:

(A) Is it realistic to assume that people only consider the danger of punishment when deciding whether or not to defraud? The parameters of the economics-of-crime model have all been shown to play a role for the decision to commit fraud (e.g. see Alm et al., 1992 in the context of tax evasion), but it seems reasonable that many other factors can play a role as well (which has also been discussed early on in the context of tax evasion, see Slemrod, 2007).

The second question is considered more rarely, but nevertheless is an important one:

(B) How can the economics-of-crime model be used to address fraud in situations where auditing is overly costly compared to the stakes, where fraud is genuinely hard or impossible to detect, or where there are no substantial consequences from lying or dishonesty? For insurance fraud, for example, Viaene and Dedene (2004) estimate that as much damage is caused by small, opportunistic transgressions (e.g. slightly overstating an insurance claim) as by large-scale, planned fraud (e.g. staging an accident). Some insurance claims cannot be completely verified, like the content of a stolen piece of luggage, or the content of a burnt-down house. There are also situations where dishonesty can be beneficial and virtually impossible to punish, for example when (slightly) understating the number of kilometers one expects to drive in the following years for a car insurance contract. It is clear that even in

these cases the menace of auditing can act as a deterrence for many, and that smart use of contract features can increase the effectiveness of auditing.[2] But might there be other, potentially cheaper and more effective measures to prevent people from dishonesty in such situations?

Concerning (A), a growing body of literature has proposed over the last years that there are factors beyond auditing and punishment that substantially influence the decision to give in to or refrain from dishonesty. This thriving area of research by now is sometimes referred to as "behavioral ethics" or as the "truth-telling literature" (Jacobsen et al., 2018; Irlenbusch and Villeval, 2015; Tenbrunsel and Chugh, 2015; Rosenbaum et al., 2014). On a high level, there are three important findings contradicting the economics-of-crime model in this stream of literature:

First, many people (usually the majority) are honest, even when it is against their monetary self-interest, and even in the complete absence of any punishment or audit. This is consistent with empirical observations of tax compliance, where, given the generally low audit rates in personal income taxes, compliance is surprisingly high (Slemrod, 2007). This cannot be explained by the economics-of-crime model, where risk aversion would need to be unreasonably high to allow for the observed levels of compliance.

Second, there is a high amount of partial dishonesty in the absence of audit and punishment, i.e. people lying only a little instead of to the full extent possible (Fischbacher and Föllmi-Heusi, 2013). In the classical economics-of-crime model it does not make sense to not exploit such a punishment-free situation to the full extent.

Third, a wide range of factors, like framing, priming, group behavior, emotions, social preferences, self-image, loss aversion, identity, personality traits, and others, systematically influence the overall level of honesty. While it might be argued that those factors are implicitly included in the economics-of-crime model by reducing or increasing the payoff of the crime g, the model's setup is not instructive on which factors might play a role in which way and to what degree.

Consequently, knowledge about factors affecting honesty behavior are not just of academic interest, but can be used in reality, thus addressing (B): There is potential to design contracts, institutions and processes to foster honest reporting without changing the underlying economic mechanisms and monetary incentives by using results from behavioral ethics.

One example for such a mechanism is based on the finding that people are more honest when signing a statement of compliance before writing down a report than when signing it after writing down the report and before sending it. In a field experiment with an insurance company, average reported car mileage was significantly higher by 10.25% (a disadvantage for customers in terms of insurance premia) among people signing a statement of compliance at the top of the reporting form instead of at the bottom (Shu et al., 2012). Such a simple change in a reporting form could also be used – at almost no additional cost – for expense claims, tax filings and other

[2] As an example, again in the context of insurance fraud, see Picard (2012) for extensions of the economics-of-crime model in insurance contracts using, e.g., deductibles.

reporting processes.

Another example relies on experimental results that people are more likely to lie to avoid a loss than to realize a gain (see, e.g., Grolleau et al. 2016). This leads for example to a higher propensity of evasive behavior if taxes must be filed and paid after receiving salaries (and are thereby experienced as a loss) than when taxes are deducted directly from the salary and can then be claimed back later (thereby experiencing this situation as a gain). Structuring tax collection processes accordingly can help reduce tax evasion (Engström et al., 2015).

Knowledge about patterns of honesty behavior can also help to focus auditing efforts on the most affected groups. Effron et al. (2015), for example, show that dishonesty is significantly higher when a situation is perceived as the last opportunity to be dishonest. The consequence is that auditing should be intensified if a report is the last chance for someone to defraud.

Contrary to the economics-of-crime approach, where conclusions about compliance are derived from few assumptions within a neo-classical economic model, the truth-telling literature approaches this topic from a broader range of theories from psychology, sociology and behavioral economics, which are then tested by empirical research. More formal models in this area have only recently begun to be developed (Gneezy et al., 2018; Khalmetski and Sliwka, 2017; Dufwenberg and Dufwenberg, 2016).

In the next section, I will discuss in more detail aspects of reporter anonymity and culture as (potentially) relevant factors for the decision whether to be or not to be honest, and the resulting research questions of the present study.

1.2. The effect of anonymity and culture on honesty

It is by now well established that both (a) the degree of anonymity of the reporter and (b) the reporter's cultural background influence the decision whether to report honestly or not.

Regarding (a), a recent meta-analysis of 72 honesty experiments found that from a range of hypotheses to explain why people are honest even in the absence of auditing and punishment, all but two are dismissed by experimental data. The remaining explanations are (1) a pure preference for being honest and (2) the preference to appear honest (Abeler et al., 2016). The fact that people pay close attention to appearing honest is also supported by other studies on this topic (Hao and Houser, 2010). Existing measures in reality already seem to rely on the wish to appear honest to reduce fraud by increasing report transparency to peers and stakeholders. There is, for example, a range of transparency initiatives for government subsidies (usually targeted at firms), like the country databases about agricultural subsidies in the EU[3]. Some countries, like Norway or Japan, publish tax payments publicly with the hope for beneficial effects on tax honesty (Bø et al., 2015; Hasegawa et al., 2012). A new branch of online insurance providers focuses on the application of micro-collectives in

[3]http://ec.europa.eu/agriculture/cap-funding/beneficiaries/shared_en, retrieved 14-02-2017

Peer-2-Peer insurance, where peers in an insurance pool are informed about claims of any other member of the pool (Köneke et al., 2015). Micro-insurance systems in developing countries also incorporate transparency between insureds about any claims incurred (Biener et al., 2016). In all those cases, the transparency is only about the claim/tax reports, without any hint on whether they are honest or not. The guiding intuition seems to be that making reports public is already enough to commit (some/more) people to be more honest. This notion is also supported by recent studies on this topic (Schitter et al., 2017; Ostermaier and Uhl, 2017). While many of these transparency initiatives are available for companies, few seem to address individuals (except for the Norway tax database). This might be due to a fear that some people could refrain from claiming what they are entitled to due to fear of public exposure. There is some, even though very limited, evidence of situations where psychological costs of such transparency become such a burden that they lead people to lie to their disadvantage in order to appear honest beyond any doubt to outside observers (Utikal and Fischbacher, 2013). However, it is yet unknown how far lying to ones financial disadvantage is really present and under which circumstances it appears.

From a psychological standpoint, the wish to appear honest might be linked to an anticipation of costs that arise due to the invocation of self-conscious emotions like shame, which has been found to be a driver of honesty in other situations (Greenberg et al., 2015; Coricelli et al., 2010, 2014). If this is the case, then the effects observed from varying transparency could also be dependent on culture. On the one hand, there is already substantial evidence for differences in honesty across societies (Gächter and Schulz, 2016; Hugh-Jones, 2016). On the other hand, experiments show that differences in the degree of collectivism versus individualism in a society change the intensity by which these emotions are experienced (Fessler, 2004; Singelis and Sharkey, 1995). In turn, this should influence the amount of psychological costs incurred in situations where a reporter remains anonymous vs. when he is known to others. Effectiveness of increasing transparency for self-reports could therefore strongly depend on the country/region in which these measures are implemented. The latter is still an unanswered question.

To add to the literature on honesty and anonymity in a cultural context, the present study aims at addressing the following three questions with lab experiments: (1) Does honesty behavior differ between situations, where unverified reports are open to public scrutiny compared to situations of anonymous reports? (2) Are differences in reporting behavior between public versus non-public situations driven by individuals' proneness to shame? (3) Does behavior differ between people from more individualistic (USA) and more collectivist (Austria) societies?

The remainder of the paper is structured as follows: Section 2 details methodological considerations and the experimental procedures for this study. Section 3 poses hypotheses to be tested in the results section 4. A discussion of the findings in section 5 concludes.

2. Material and methods

2.1. The experimental method to investigate honesty[4]

Both lab and field experiments are by now a major source for empirical data in the economic sciences (Falk and Heckman, 2009; Harrison and List, 2004). They are very well-suited to establish causal inference in economic behavior by tightly controlling variations in the experimental environment. For investigating honesty, they are likely even more relevant than for researching many other situations. This is due to three challenges of empirical research, that are not exclusive to the topic of honesty, but particularly relevant in this area:

(1) Naturally occurring data is usually not well-suited for studying the amount of honesty. Fraud occurs when private information is dishonestly communicated or misrepresented. Given legal and social implications, people have a high incentive to conceal fraudulent activities and keep their information private even at a cost. Without auditing every single report (which is virtually impossible) the complete amount of fraud is therefore usually impossible to uncover in naturally occurring data (a rare exception in the context of newspaper purchases is provided in Pruckner and Sausgruber, 2013). The consequence are mostly unreliable estimates of the extent of fraud and dishonesty when using such data.

(2) When dishonesty is clearly identifiable by an observer, or when every report is audited, many people may refrain from being dishonest due to a preference to appear honest. This applies across the board to unincentivized (e.g. survey) methods, (incentivized) experiments and naturally occurring data that do not clearly prohibit the researcher from establishing who is honest and who is not (Gneezy et al., 2018; Halevy et al., 2014). Deceiving subjects in telling them that their actions are private and secretly recording their decision to be honest or not is usually not a feasible way to address this issue: Participants might be suspicious when they can imagine how their reports could be recorded and therefore not believe the experimenter anyway. This is also one of the main reasons why deception is generally considered a taboo in economic experiments (Cooper, 2014). Unsurprisingly, for this reason, economists rarely and cautiously use results from psychology, where deception is much more common. However, as presented later, experiments can be structured to fully preserve the unobservability of participants' decisions to be or not to be honest, while still allowing for a reliable estimate of the aggregate amount of dishonesty.

(3) Context can influence economic behavior, particularly if the context is emotionally charged (Alekseev et al., 2016). Findings from one field (like tax evasion) might therefore not be generalizable to other fields like insurance fraud – even if the behavior relies on common underlying mechanisms. For this reason, neutral lab experiments might be better suited to explore general mechanisms influencing honesty independent of context. Note that the question of context is different from the question of external validity (i.e. whether results from economic experiments can be

[4]The majority of this section is identical to section 1.2 of the author's dissertation.

translated into the real world). The question of context is rather about whether using wording that reflects real world situations makes a difference in behavior compared to neutral wording, but assuming both internal and external validity of the research within the specific context.

Experimental economic research has been recognized early as a good method to at least address challenge (1) in research on fraud. Lab experiments can easily be designed to make the amount of fraud transparent and controlled variation allows to infer the degree of influence of any factor in question. This method has been used extensively in the area of tax fraud since the late 1980s (see Alm, 2012 for an overview), but also more recently in the areas of corruption and bribery (e.g. Abbink et al., 2002 or Barr and Serra, 2009), and to a smaller extent for insurance fraud (Puchstein et al., 2014). Participants in these experiments are placed into a game representing the respective situation (paying taxes, filing insurance claims), where they have the possibility to increase their payoff by fraudulent reporting. By varying treatments, causal effects of changes in the situation and of interventions can be measured. However, these studies might have limitations with respect to challenges (2) and (3): They usually do not control for participants noticing that their decisions to be (dis)honest are clearly recorded and therefore observable to the experimenter, and they explicitly try to introduce the context of a specific setting like paying taxes.

More recently, a growing literature started to experimentally study pure honesty behavior to understand the underlying mechanisms in a neutral setting, thereby avoiding challenges (1) and (3). While early examples use non-standardized methods that are therefore difficult to compare (Rosenbaum et al., 2014), three standardized types of lab experiments have evolved that have been extensively used in the last decade:

The first type of experiment is the sender-receiver game (Gneezy, 2005; Hurkens and Kartik, 2009), where subjects are matched in pairs and the first subject (the sender) has the option of increasing his payoff by lying to the other subject (the receiver). In this setting, a lie usually has a direct negative financial consequence for another participant, and not just for the experimenter. This design element introduces an additional social context, which must be considered when interpreting results of such experiments (Jacobsen et al., 2018). Individual behavior is also still clearly observable in this setting, thereby not addressing challenge (2).

The second method, the matrix task (Mazar et al., 2008), uses individual reporting decisions without interactions, and makes an effort to conceal subjects' actions from observers. In its basic version, it can therefore only be used to study dishonesty on an aggregate level: Participants are given a set of matrices of 12 numbers, where each matrix contains one number pair that sums exactly up to 10. They are then asked to find those two numbers in as many matrices as possible within a given time. For each matrix solved, participants receive a predefined monetary reward. To establish a baseline of results, a control group of participants fills the matrix task. The experimenter in this setting checks their results and pays them accordingly. In the treatment group, participants just self-report the number of matrices they solved

without the experimenter checking them, and then get paid accordingly. To make it even more clear that results cannot be checked in the treatment group, participants are sometimes additionally asked to shred their answer sheets before self-reporting their results.[5] The amount of dishonesty is inferred from comparing mean results in the control and in the treatment condition. This is the first method to address all mentioned challenges (1) through (3) comprehensively. However, investigating the result of interventions on honesty with this method is quite costly, as it requires a 2x2 design (control and treatment, with and without intervention).

The third method is Fischbacher and Föllmi-Heusi (2013)'s die game and variations (e.g. Jiang, 2013 or Effron et al., 2015) thereof. This method has many advantages: It is simple, easily understandable to subjects, versatile, replicable, comparable across experiments and cheaper than the matrix task, as it requires no multifactorial design to study interventions. Under this method, participants are given a physical randomization device (e.g. a die or coin) that they use to create an outcome in private. Each outcome is connected to a predefined payoff (e.g. 1 EUR for reporting heads, 0 EUR for reporting tails). There are therefore incentives to report a higher outcome even if it is not observed. It is clear from the setup that dishonesty in this experiment cannot be uncovered on an individual level, thus transparently preserving anonymity of the reporting decision of participants. On an aggregate level, however, the researcher can establish an estimate of dishonesty by comparing the realized to the expected outcome distribution. The die game is by now the dominating experimental method for honesty research: A meta study by Abeler et al. (2016) already reports 72 published studies using this paradigm. This method also shows strong explanatory power outside the lab: Honesty behavior in experiments using versions of the die game correlate to honesty behavior in the field, e.g. to fare evasion (Dai et al., 2017) or to cheating in school (Cohn and Maréchal, 2017).

Taken together, experiments appear to be a good method to study honesty. The challenges and historical development of experimental honesty research indicates that three points need to be addressed when designing such experiments: First, is there a reason not to use a neutral context? Second, is there a reason not to use a standardized experimental method? Third, can the experiment be designed to keep individuals' decisions clearly unobservable by the experimenter?

The following lines out the design and subject features of the experiments in Austria and the US, including a discussion of design choices in light of the methodological considerations presented here.

[5] Note, however, that contrary to this initial design, in which participants can be informed without deceiving them that their results cannot be checked in treatment, some experiments let people throw away their answer sheets that are then retrieved again without subjects' knowledge. This way, the amount of dishonesty can be assessed without doubt and even at an individual level. One example for such a design is Gino et al. (2011). In such a case it is important to assess what information participants were given to rule out deception: While false information is always deception, missing information is not generally considered to be deception.

2.2. Design

Given the brevity of the deployed tasks (less than 20 minutes in total), sessions were run both in Austria and the USA as addenda to other experiments. However, participants were clearly instructed that this is a separate task from the preceding experiments. Results from the Austrian part of the experiment were also used in Schitter et al. (2017), which includes a discussion similar to 2.2.1 and 2.3.1.

2.2.1. Austria

At the beginning of the experiment, the experimenter takes a picture of each participant's face. At their PC workstation, each subject finds a sealed envelope. Participants are informed that (1) everyone in the experiment faces the same task, (2) that every participant has a cash amount of between 0.00 and 1.00 euros (in 10 cent steps) in her envelope, and (3) that not everyone has the same envelope content. There is no statement on the distribution of envelope contents, nor how contents were chosen. In the end, envelopes only contain 30 or 70 cents, and the individual envelope contents are known to the experimenter. The experimenter instructs subjects to open the envelope, to look at the content and then pocket the content, which they do not need to show to anyone, including to the experimenter.

In the next step, subjects are told that every participant is entitled to 1 euro in the main stage of the experiment, which they can obtain by claiming the difference between the content of their envelope and 1 euro. Subjects are asked to enter their claim, which they know will not be reviewed any further and be paid out at the end of the experiment together with any other money they might have earned in this session. Clearly, there is an incentive to overstate the claim to increase one's payoff.

Note, that with this design choice overstated claims can be identified without doubt, contrary to many established tasks that preserve the anonymity of the decision maker completely and transparently. The most likely effect of subjects suspecting the experimenter of knowing the envelope content should be a reduction in dishonesty, mainly by having less partial liars (that is, subjects not lying to the full possible amount, as found for example in Gneezy et al. (2018) or Abeler et al. (2016)). However, two reasons give confidence about the validity of this design choice: First, there is a significant amount of partial liars in the collected data (23.9% of total), hinting at participants not suspecting the experimenter to know the envelope content or not caring for this too much. One reason why participants might not take more interest in this aspect could lie in the fact that the instructions omitted any reference to whether or not participants' decision would be identifiable. Second, the absolute level of dishonesty is not relevant for any of the research questions. A potential reduction in dishonesty should only reduce any identified treatment effects, rendering the findings in this paper – if anything – more conservative. Further discussion and analysis of these topics can be found in Schitter et al. (2017), section 3.3.2.

The claim stage is followed by the TOSCA-3 test of self-conscious effect (Tangney et al., 2000) in the German version of Rüsch et al. (2007), which measures proneness

to feeling shame, guilt, externalization and unconcern, of which only the shame score is used here. A higher score is connected to a higher propensity to the relevant emotion. Finally, participants are asked to provide basic demographic information. Participants were paid separately for filling in the TOSCA-3 and demographic survey information.

To investigate the effect of anonymity on honesty in this setting, participants are randomly allocated to one of the following treatment conditions: NOPUBLIC, which follows the stages exactly as described before, and PUBLIC, in which subjects are assigned to a group of four participants in the current session. In the latter treatment, after the claim stage, photos of the four group members together with the amount claimed are displayed to all group members. Figure 1 provides an example of this screen.[6] However, it is made clear that no information about the truthfulness of the claim is shared between members. Subjects are also informed that the stage displaying the photos does not influence their payoffs.

The full instructions (translated into English) are provided in Appendix A.1.

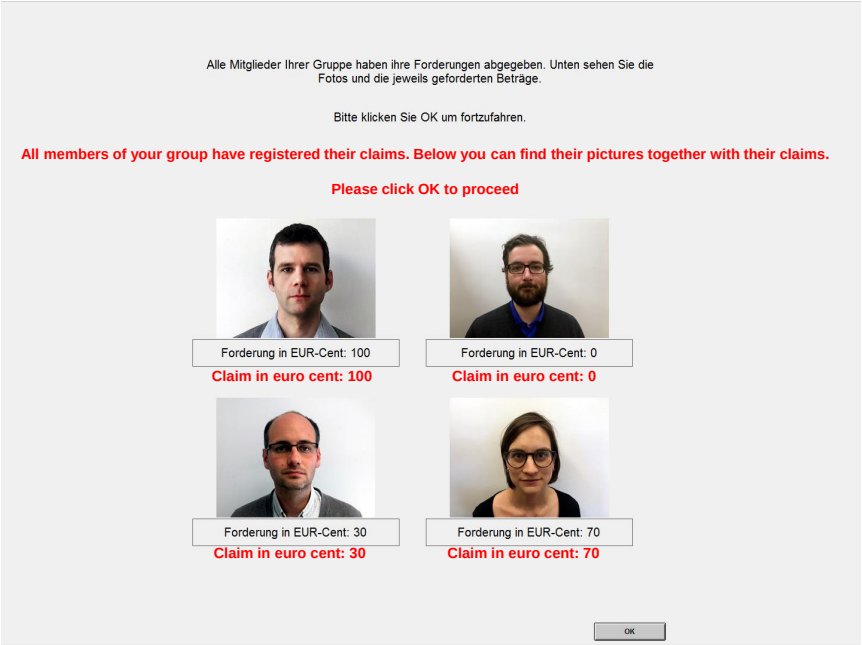

Figure 1: Screen used in transparency stage of experiment. English translations added in red font (grey in grey-scale printouts) were not part of the original screen.

2.2.2. USA

In the USA, I used a version of the well established Fischbacher and Föllmi-Heusi (2013) die task.

[6]Note that the example photos do not picture actual participants, but the authors and an author's spouse.

At the beginning of the session, the experimenter takes a picture of each participant's face upon entering the lab. Participants find a die on their desk and are informed that their payoff depends on the outcome of the die roll. They will receive a payoff equivalent to the number they report times 0.2 USD. This means, e.g., for a report of 1, a participant would receive 0.20 USD, and for a report of 6 she would receive 1.20 USD. Participants are asked to file their claim, which will not be checked any further and paid out with any other money participants might earn in the experiment at the end of the session. Again, there is a clear incentive to dishonestly overstate the outcome of the die roll to increase one's payoff. Note, that in this case there is no possibility to identify overclaiming on an individual level.

This stage of the experiment is again followed by the TOSCA-3 questionnaire and a questionnaire on basic demographic data, as described in section 2.2.1.

To investigate the effect of anonymity on honesty in this setting, again, participants are randomly allocated to one of the following treatment conditions. NOPUBLIC, which follows the stages exactly as described before, and PUBLIC, in which subjects are assigned to a group of four participants in the current session. In the latter treatment, as in the Austrian version, after the claim stage, photos of the four group members together with the amount claimed are displayed to all group members, as shown in figure 1.

The full instructions are provided in Appendix A.2.

2.3. Participants

2.3.1. Austria

I conducted experiments in 2016 and 2017 in the labs of the Universities of Graz and Innsbruck. The final sample consists of 163 students with a mean age of 24.5 (SD=3.8), of which 56.4% are female. Participants were recruited via ORSEE in Graz and via hroot in Innsbruck (Greiner, 2015; Bock et al., 2012). The experiment was computerized using z-Tree 3.6.7 (Fischbacher, 2007). Using an F-test power analysis, I would expect to find a small- to medium-sized treatment effect with power 0.8 at the 5%-confidence level ($f^2 = 0.06$).

2.3.2. USA

I conducted sessions at the XLab of the University of California at Berkeley in 2017 (CPHS protocol number 2017-09-10298) . The final sample consists of 119 students with a mean age of 20.2 (SD=2.6), of which 69.7% are female. The experiment was computerized using z-Tree 3.6.7 (Fischbacher, 2007). Using an F-test power analysis, I would expect to find a medium-sized treatment effect with power 0.8 at the 5%-confidence level ($f^2 = 0.08$).

3. Hypotheses

Given results from previous experiments, people often refrain from (extensive) dishonesty in order to appear honest, despite financial losses connected with this behavior (Fischbacher and Föllmi-Heusi, 2013; Abeler et al., 2016). Assuming that psychological proximity of the observer (in the sense of specific knowledge of the observer and the observed situation) increases emotional costs associated with transparency, I expect that transparency between peers will increase honesty. Therefore, I posit:

AT-H1 Average claims are lower in treatment PUBLIC than in treatment NOPUBLIC in Austria.

US-H1 Average claims are lower in treatment PUBLIC than in treatment NOPUBLIC in the USA.

Dishonest self-reports can incur emotional cost for the reporter (Coricelli et al., 2010). I assume that the intensity of fear to appear dishonest is linked to the intensity of experiencing self-conscious emotions (particularly shame). People with a higher sensitivity to shame would consequently be more likely to be honest under increased transparency. The following hypothesis is an immediate consequence of this argument:

AT-H2 The increase in honesty in treatment PUBLIC compared to NOPUBLIC in Austria is driven by participants with a higher proneness to shame.

US-H2 The increase in honesty in treatment PUBLIC compared to NOPUBLIC in the USA is driven by participants with a higher proneness to shame.

Differences in the experience of shame and embarrassment are known to exist between collectivist and individualistic cultures (Fessler, 2004; Singelis and Sharkey, 1995) and therefore are assumed to depend on the degree of individualism prevalent in a society. Assuming the difference in behavior for increasing transparency is driven by self-conscious emotions, emotional costs would differ between countries. I therefore expect the effectiveness of transparency on honesty to be lower in the US, which is a more individualistic society that should show less sensitivity to shame, than in Austria, which is a less individualistic society that should show higher sensitivity to shame[7]. Consequently, I would expect to find differences in the effectiveness of fostering honesty by increasing public scrutiny between the USA and Austria:

H3 The difference between treatments PUBLIC and NOPUBLIC are more pronounced in Austria than in the USA.

[7] http://geerthofstede.com/research-and-vsm/dimension-data-matrix/, retrieved 27-02-2017

4. Results

4.1. Measurements

4.1.1. Austria

I define the following variables for the analysis in the results section:

$\text{CLAIM}_i \in \{0, 10, 20, ..., 90, 100\}$ is the reported claim by participant i in euro cents. $\text{SHAME\_SCORE}_i \in \left[1, 5\right]$ is the result of the TOSCA-3 shame scale for participant_i. SHAME_SCORE and CLAIM indicate the respective means of a group of participants. FEMALE_i is a binary variable indicating whether the participant is female (FEMALE=1) or male (FEMALE=0). FEMALE is used as a control, as previous research shows female participants to report more honestly than male subjects (Abeler et al., 2016).

4.1.2. USA

$\text{CLAIM}_i \in \{1, 2, 3, 4, 5, 6\}$ is the reported claim by participant i, which translates into payoffs of $\{0.20, 0.40, 0.60, 0.80, 1.00, 1.20\}$ USD. SHAME_SCORE_i[8] and FEMALE_i are the same as in Austria, as are the variables for the respective averages by dropping the index.

4.2. Shame scores

First, I start by comparing the distribution of shame scores between Austria and the USA.

In the given sample, there are higher shame scores in the US than in Austria (figure 2), with a significant difference of the mean score (Wilcoxon-Rank-Sum W=5371.5, $p<0.01$).

However, absolute levels of SHAME_SCORE could simply depend on general differences in how people in different cultures fill in questionnaires, and not directly be linked to a different level of shame. A Levene test for differences in the standard deviation between the two sample supports this notion, as the dispersion of both the US and the Austrian sample are not significantly different (Levene Test F=0.97, p=0.3255). There is also evidence for internal consistency of the TOSCA-3 questionnaire in both the English and German version (Tangney et al., 2000; Rüsch et al., 2007), giving confidence in the suitability of this measure for the current study.

4.3. Behavior in Austria

4.3.1. Treatment effects

I compare mean claims in Austria between treatments in figure 3. Average claims in PUBLIC amount to 60.9, and in NOPUBLIC to 69.4. In both treatments, claims

[8]Scores by participants not answering all survey questions were scaled to make them comparable.

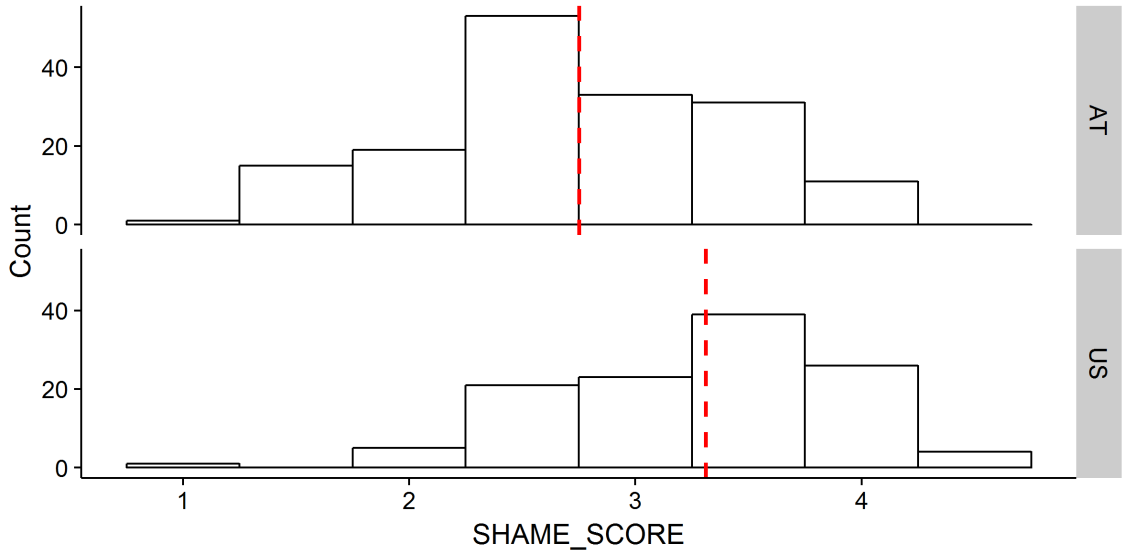

Figure 2: Distributions of SHAME_SCORE in Austria (AT) and the USA (US). Red dotted lines indicate mean.

are substantially above the honest level of 50 euro cents. Comparing treatments, claims are significantly higher in treatment NOPUBLIC than in treatment PUBLIC (Wilcoxon-Rank-Sum W=2592, p=0.010). This leads to the first result for Austria:

AT-R1 Making reports transparent between peers significantly increases honesty in the Austrian sample. This supports hypothesis AT-H1.

4.3.2. Relationship between proneness to shame and honesty

Investigating whether the degree of shame proneness mediates the effect of reducing transparency on honesty, I start by comparing average CLAIM for subjects below and above the median SHAME_SCORE by treatment in figure 4. There is some hint at an effect for participants with low SHAME_SCORE: Claims are substantially lower for this group in PUBLIC than in NOPUBLIC. However, there is no such effect visible for the group of subjects with shame score above the median.

Taking a closer look at the relation between SHAME_SCORE and CLAIM in figure 5, there is also some indication that higher SHAME_SCORE leads to lower claims, and that this effect is more pronounced in treatment PUBLIC than in treatment NOPUBLIC. However, standard errors are too high to make a confident statement.

A simple linear regression model with dependent variable CLAIM shows that the observations above are not significant: Increasing SHAME_SCORE reduces CLAIM both in PUBLIC and NOPUBLIC, however not significantly so (first and second column of table 1). While claims are significantly lower in treatment PUBLIC (third

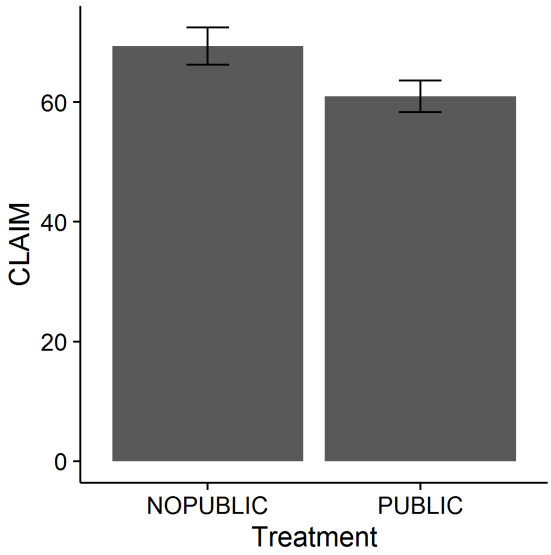

Figure 3: Average claims per treatment in Austria. Error bars indicate standard deviation.

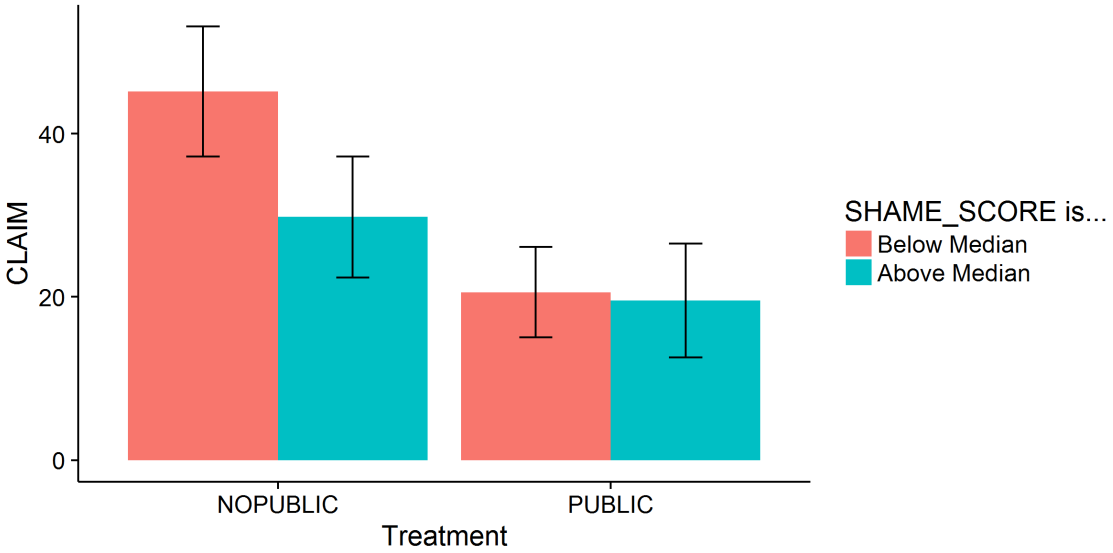

Figure 4: Average claims split by SHAME_SCORE above and below Median per treatment in Austria. Error bars indicate standard deviation.

column), there is no interaction between PUBLIC and SHAME_SCORE (fourth column), leading to the following result:

AT-R2 There is no significant effect of proneness to shame on the identified treatment effect in Austria, though the direction of the effect is as posited. Hypothesis AT-H2 is not supported.

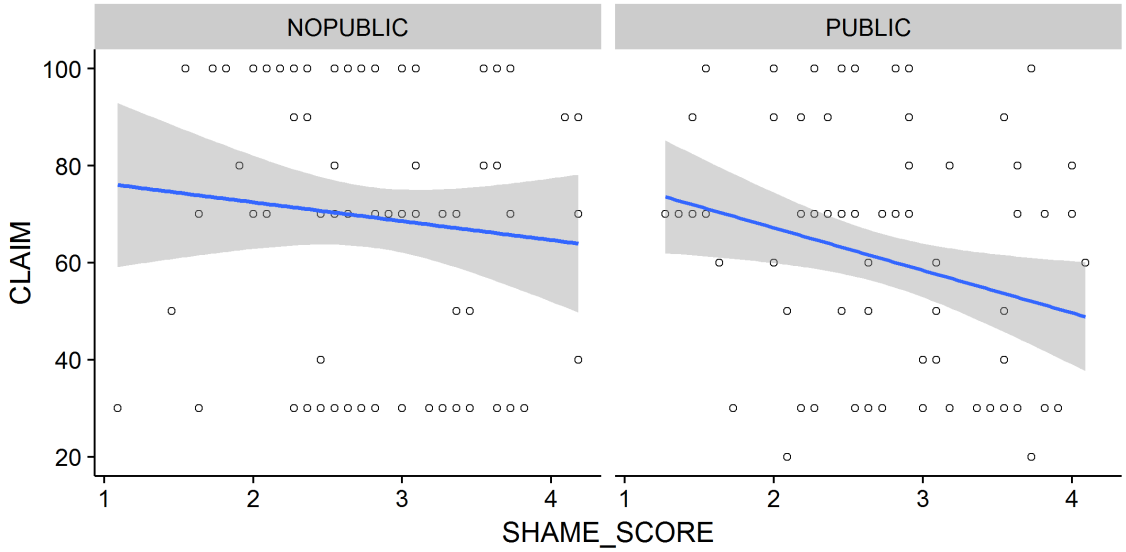

Figure 5: CLAIM versus SHAME_SCORE in Austria. Line indicates fitted linear regression; Grey area indicates standard errors.

	NOPUBLIC	PUBLIC	Combined	Combined with Interaction
Intercept	58.43**	37.98**	57.22***	58.41***
	(23.49)	(17.11)	(14.85)	(21.15)
PUBLIC			−17.96**	−20.17
			(6.95)	(28.61)
SHAME_SCORE x PUBLIC				0.07
				(0.92)
SHAME_SCORE	−0.56	−0.41	−0.49	−0.53
	(0.77)	(0.60)	(0.48)	(0.68)
FEMALE	−6.13	−9.93	−7.98	−8.00
	(11.57)	(9.38)	(7.39)	(7.42)
Num. obs.	80	83	163	163

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 1: Econometric models for Austria with dependent variable CLAIM.

4.4. Behavior in the USA

4.4.1. Treatment effects

I compare average claims in the USA between treatments in figure 6. Average claims in PUBLIC amount to 4.89, and in NOPUBLIC to 5.27. In both treatments, claims are substantially above the honest level of 3.5. Comparing treatments, claims are only weakly significantly (p between 0.05 and 0.1) higher in treatment NOPUBLIC than in treatment PUBLIC (Wilcoxon-Rank-Sum W=1432, p=0.053). However, given the lower power to identify a medium sized effect compared to the Austrian sample, I take this as indicative evidence for a treatment effect. This leads to the following

first result for the USA:

US-R1 Making reports transparent between peers increases honesty in the US sample, but the difference in claims is only marginally significant. Hypothesis US-H1 is therefore weakly supported.

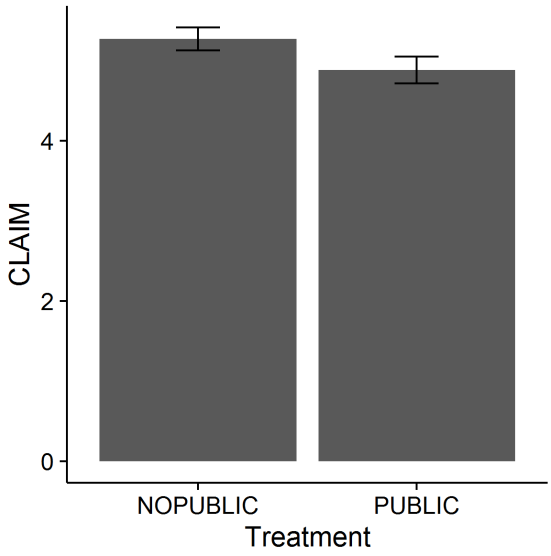

Figure 6: Average claims per treatment in the USA. Error bars indicate standard deviation.

4.4.2. Relationship between proneness to shame and honesty

Comparing average claims by treatment and whether a participant's SHAME_SCORE is above or below the median, there is no difference between any of these groups visible in figure 7. The scatterplot of SHAME_SCORE vs. CLAIM displays a similar picture in figure 8. Except for generally lower levels of CLAIM in PUBLIC than in NOPUBLIC (which is in line with the identified treatment effect in section 4.4.1), there is no consistent or substantial effect of SHAME_SCORE on CLAIM visible.

Investigating this further with a simple linear regression in table 2 confirms these first findings: There is an almost clear 0-effect of SHAME_SCORE on CLAIM in all of the presented models, and almost no interaction at all between SHAME_SCORE and treatment PUBLIC in the model including the interaction in the fourth column. Therefore, I posit:

US-R2 There is no significant effect of proneness to shame on the identified treatment effect in the USA. Hypothesis US-H2 is not supported.

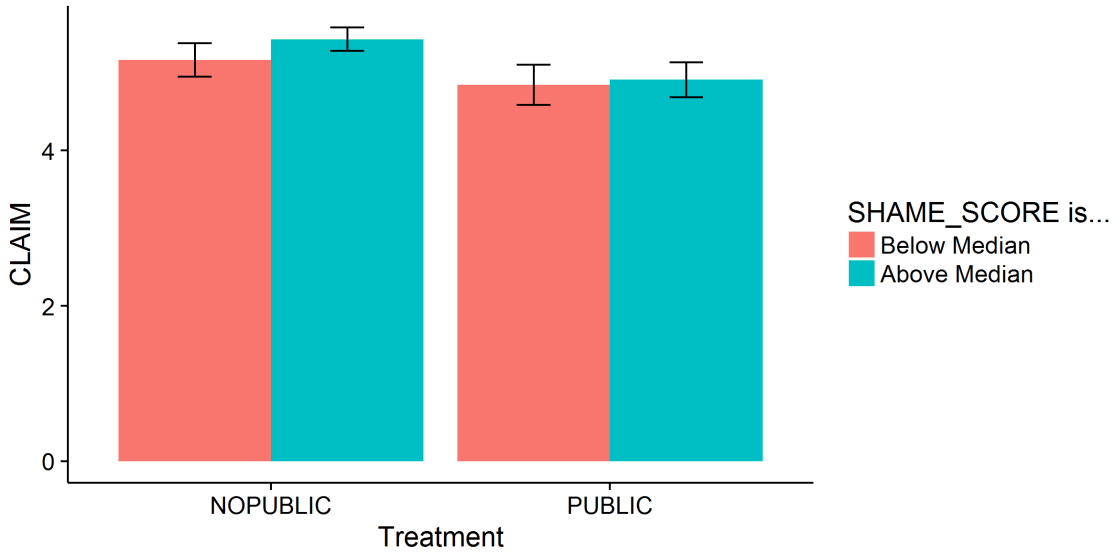

Figure 7: Average claims split by SHAME_SCORE above and below Median per treatment in the USA. Error bars indicate standard deviation.

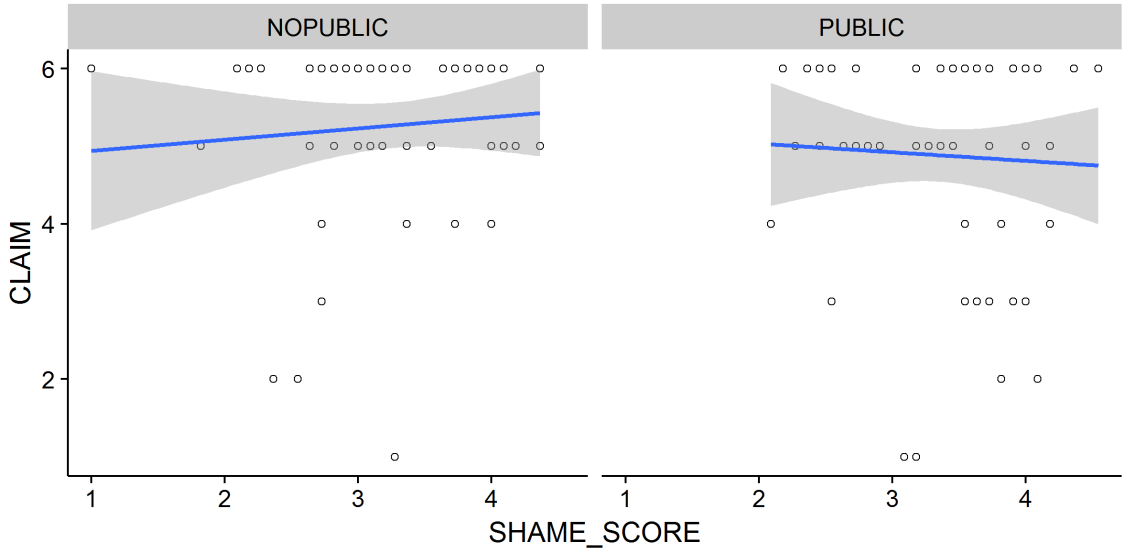

Figure 8: CLAIM versus SHAME_SCORE in the USA. Line indicates fitted linear regression; Grey area indicates standard errors.

4.5. Comparison of behavior in Austria and the USA

Comparing the results from Austria and the United States, no clear differences in behavior appear: In both countries, making claims public reduces dishonesty significantly (sections 4.3.1 and 4.4.1). In both countries, there is no significant effect of proneness to shame on honesty or the effect of making claims public (sections 4.3.2 and 4.4.2). However, while not being significant, there are some hints of a

	NOPUBLIC	PUBLIC	Combined	Combined with Interaction
Intercept	4.89***	5.27***	5.23***	4.84***
	(0.73)	(0.98)	(0.60)	(0.79)
PUBLIC			−0.41*	0.48
			(0.23)	(1.19)
SHAME_SCORE x PUBLIC				−0.02
				(0.03)
SHAME_SCORE	0.02	−0.01	0.00	0.02
	(0.02)	(0.03)	(0.02)	(0.02)
FEMALE	−0.32	−0.02	−0.15	−0.16
	(0.34)	(0.36)	(0.25)	(0.25)
Num. obs.	59	60	119	119

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Table 2: Econometric models for the USA with dependent variable CLAIM.

relationship between SHAME_SCORE and CLAIM in the Austrian sample, though the effect in the given sample is too small and not significant. While Schitter et al. (2017) in a larger sample find that higher proneness to shame indeed leads to more honesty, they are however also not able to identify a significant interaction between SHAME_SCORE and the treatments. In total, there is no convincing evidence for a size-able difference in behavior between the USA and Austria. This leads to the following result:

R3 Behavior under anonymity and no anonymity is qualitatively the same between participants in the USA and Austria, and independent of proneness to shame. Hypothesis H3 is not supported.

5. Conclusion

Factors influencing whether to be honest or not in economic decision making have gained substantial attention over the last years. This paper set out to investigate the effect of anonymity on honesty, and whether any such effect can be further explained by proneness to shame or cultural differences.

While the results of this paper confirm the already established fact that making unverified reports public to peers increases honesty, the other proposed hypotheses could not be confirmed: Proneness to shame does not significantly mediate over-claiming, and cultural differences between Austria and the US do not seem to change behavior within those two groups substantially. While there are some hints at proneness to shame playing a stronger role in Austria, any such effects appear to be too small to be captured with certainty with the given sample size.

In conclusion, reducing anonymity of reporters filing unverified claims has an equally positive effect on increasing honesty in the US and Austria, indicating that cultural differences are not substantial factors influencing this effect. These findings

give a first indication that both researchers and practitioners investigating or relying on such an effect in their work can be confident about its robustness across societies.

References

Abbink, K., Irlenbusch, B., Renner, E., 2002. An experimental bribery game. Journal of Law, Economics, and Organization 18 (2), 428–454.

Abeler, J., Raymond, C., Nosenzo, D., 2016. Preferences for Truth-Telling. CESifo Working Paper 6087.

Alekseev, A., Charness, G., Gneezy, U., 2016. Experimental Methods: When and Why Contextual Instructions are Important. Journal of Economic Behavior and Organization 134, 48–59.

Alm, J., 2012. Measuring, explaining, and controlling tax evasion: Lessons from theory, experiments, and field studies. International Tax and Public Finance 19 (1), 54–77.

Alm, J., McClelland, G. H., Schulze, W. D., 1992. Why do people pay taxes? Journal of Public Economics 48, 21–38.

Barr, A., Serra, D., 2009. The effects of externalities and framing on bribery in a petty corruption experiment. Experimental Economics 12 (4), 488–503.

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

Biener, C., Eling, M., Landmann, A., Pradhan, S., 2016. Can Group Incentives Alleviate Moral Hazard? The Role of Pro-Social Preferences. Working Paper.

Bø, E. E., Slemrod, J., Thoresen, T. O., 2015. Taxes on the internet: Deterrence effects of public disclosure. American Economic Journal: Economic Policy 7 (1), 36–62.

Bock, O., Nicklisch, A., Baetge, I., 2012. hroot: Hamburg registration and organization online tool. WiSo-HH Working Paper.

Cohn, A., Maréchal, M. A., 2017. Laboratory Measure of Cheating Predicts Misbehavior at School. The Economic Journal. Forthcoming.

Cooper, D. J., 2014. A Note on Deception in Economic Experiments. Journal of Wine Economics 9 (2), 111–114.

Coricelli, G., Joffily, M., Montmarquette, C., Villeval, M. C., 2010. Cheating, emotions, and rationality: An experiment on tax evasion. Experimental Economics 13 (2), 226–247.

Coricelli, G., Rusconi, E., Villeval, M. C., 2014. Tax evasion and emotions: An empirical test of re-integrative shaming theory. Journal of Economic Psychology 40, 49–61.

Dai, Z., Galeotti, F., Villeval, M. C., 2017. Cheating in the Lab Predicts Fraud in the Field. An Experiment in Public Transportations. Management Science. Forthcoming.

DellaVigna, S., 2009. Psychology and Economics: Evidence from the Field. Journal of Economic Literature 47 (2), 315–372.

Dufwenberg, M., Dufwenberg, M., 2016. Lies in Disguise – A Theoretical Analysis of Cheating. CESifo Working Paper 6208.

Effron, D. A., Bryan, C. J., Murnighan, J. K., 2015. Cheating at the End to Avoid Regret. Journal of Personality and Social Psychology 109 (3), 395–414.

Engström, P., Nordblom, K., Ohlsson, H., Persson, A., 2015. Tax compliance and loss aversion. American Economic Journal: Economic Policy 7 (4), 132–164.

Falk, A., Heckman, J. J., 2009. Lab experiments are a major source of knowledge in the social sciences. Science 326 (5952), 535–538.

Fessler, D., 2004. Shame in Two Cultures: Implications for Evolutionary Approaches. Journal of Cognition and Culture 4 (2), 207–262.

Fischbacher, U., 2007. z-Tree: Zurich toolbox for ready-made economic experiments. Experimental Economics 10, 171–178.

Fischbacher, U., Föllmi-Heusi, F., 2013. Lies in disguise-an experimental study on cheating. Journal of the European Economic Association 11 (3), 525–547.

Gächter, S., Schulz, J. F., 2016. Intrinsic honesy and the prevalencce of rule violations across societies. Nature 531 (7595), 1–11.

Gino, F., Schweitzer, M. E., Mead, N. L., Ariely, D., 2011. Unable to resist temptation: How self-control depletion promotes unethical behavior. Organizational Behavior and Human Decision Processes 115 (2), 191–203.

Gneezy, U., 2005. Deception: The Role of Consequences. The American Economic Review 95 (1), 384–394.

Gneezy, U., Kajackaite, A., Sobel, J., 2018. Lying Aversion and the Size of the Lie. The American Economic Review 108 (2), 419–453.

Greenberg, A. E., Smeets, P., Zhurakhovska, L., 2015. Lying, Guilt, and Shame. Working Paper.

Greiner, B., 2015. Subject pool recruitment procedures: Organizing experiments with ORSEE. Journal of the Economic Science Association 1, 114–125.

Grolleau, G., Kocher, M. G., Sutan, A., 2016. Cheating and Loss Aversion: Do People Cheat More to Avoid a Loss? Management Science 62 (12), 3428–3438.

Halevy, R., Shalvi, S., Verschuere, B., 2014. Being honest about dishonesty: Correlating self-reports and actual lying. Human Communication Research 40 (1), 54–72.

Hao, L., Houser, D., 2010. Honest lies. George Mason Department of Economics 11-16 (March), 1–30.

Harrison, G. W., List, J. A., 2004. Field Experiments. Journal of Economic Literature 42 (4), 1009–1055.

Hasegawa, M., Hoopes, J. L., Ishida, R., Slemrod, J., 2012. The effect of Public Disclosure on reported taxable Income: Evidence from Individuals and Corporations in Japan. Working Paper, 1–60.

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

Hurkens, S., Kartik, N., 2009. Would I lie to you? On social preferences and lying aversion. Experimental Economics 12 (2), 180–192.

Irlenbusch, B., Villeval, M. C., 2015. Behavioral ethics: How psychology influenced economics and how economics might inform psychology? Current Opinion in Psychology 6, 87–92.

Jacobsen, C., Fosgaard, T. R., Pascual-Ezama, D., 2018. Why Do We Lie? A Practical Guide To the Dishonesty Literature. Journal of Economic Surveys 32 (2), 357 – 387.

Jiang, T., 2013. Cheating in mind games: The subtlety of rules matters. Journal of Economic Behavior and Organization 93, 328–336.

Khalmetski, K., Sliwka, D., 2017. Disguising Lies – Image Concerns and Partial Lying in Cheating Games. CESifo Working Paper 6347.

Köneke, V., Müller-Peters, H., Fetchenhauer, D., 2015. Versicherungsbetrug verstehen und verhindern. Springer Gabler.

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.

Ostermaier, A., Uhl, M., 2017. Spot On For Liars ! How Public Scrutiny Influences Ethical Behavior. PLoS ONE 12 (7).

Picard, P., 2012. Economic Analysis of Insurance Fraud. Working Paper.

Pruckner, G., Sausgruber, R., 2013. Honesty on the Streets - A Natural Field Experiment on Newspaper Purchasing. Journal of the European Economic Association 11, 661–679.

Puchstein, K., Schiller, J., von Bieberstein, F., 2014. Can we trust consumers' survey answers when dealing with insurance fraud? – Evidence from an experiment. Working Paper.

Rabin, M., 1998. Psychology and Economics. Journal of Economic Literature 36 (1), 11–46.

Rosenbaum, S. M., Billinger, S., Stieglitz, N., 2014. Let's be honest: A review of experimental evidence of honesty and truth-telling. Journal of Economic Psychology 45, 181–196.

Rüsch, N., Corrigan, P. W., Bohus, M., Jacob, G. A., Brueck, R., Lieb, K., 2007. Measuring shame and guilt by self-report questionnaires: A validation study. Psychiatry Research 150 (3), 313–325.

Schitter, C., Fleiß, J., Palan, S., 2017. To claim or not to claim: Anonymity , reciprocal externalities and honesty. KFU Graz SOWI Working Paper 2017-01.

Shu, L. L., Mazar, N., Gino, F., Ariely, D., Bazerman, M. H., 2012. Signing at the beginning makes ethics salient and decreases dishonest self-reports in comparison to signing at the end. Proceedings of the National Academy of Sciences of the United States of America 109 (38), 15197–15200.

Singelis, T. M., Sharkey, W. F., 1995. Culture, Self-Construal, and Embarrassability. Journal of Cross-Cultural Psychology 26 (6), 622–644.

Slemrod, J., 2007. Cheating Ourselves: The Economics of Tax Evasion. Journal of Economic Perspectives 21 (1), 25–48.

Tangney, J. P., Dearing, R. L., Wagner, P., Gramzow, R., 2000. The test of self-conscious affect-3 (TOSCA-3). Fairfax, VA: George Mason University.

Tenbrunsel, A. E., Chugh, D., 2015. Behavioral ethics: A story of increased breadth and depth. Current Opinion in Psychology 6, 205–210.

Utikal, V., Fischbacher, U., 2013. Disadvantageous lies in individual decisions. Journal of Economic Behavior and Organization 85, 108–111.

Viaene, S., Dedene, G., 2004. Insurance fraud: issues and challenges. The Geneva Papers on Risk and Insurance - Issues and Practice 29 (2), 313–333.

Yaniv, G., Siniver, E., 2016. The (honest) truth about rational dishonesty. Journal of Economic Psychology 53, 131–140.

A. Experimental instructions

In this appendix, I provide the translations of the instructions given to the participants. The Austrian instructions were originally written in German, and only translations are presented here. The original instructions are available on request from the author.

A.1. Instructions - Austria

A.1.1. Treatment PUBLIC

You were randomly allocated to a group of four participants for this experiment. The three other participants in your group face the same task as you do. Each participant is entitled to a total payoff of 1.00 euro. This 1.00 euro stems from two sources.

(1) First, there is an envelope on your desk. This envelope contains a cash amount of between 0.00 and 1.00 euros (in 10-cent steps). The money in your envelope belongs to you. The amounts in the envelopes of different participants may differ – not every participant's envelope holds the same amount. Please look into your envelope now. You may pocket the contents of your envelope. You do not have to show these contents to any other participant, nor to the experimenter. During the entire experiment, no other participant will learn the contents of your envelope.

(2) In order to obtain the difference towards the total payoff of 1.00 euro you are entitled to, you will enter a request on the next screen. The amount you request may be between 0.00 and 1.00 euros (in 10-cent steps) and will not be verified. At the end of the experiment the experimenter will pay out to you the amount you requested.

After you have entered your request, this request, together with your photo, will be shown to the other three group members. You will also see the photos of the other group members and the request each of them made. This step does not affect the payment of your request.

A.1.2. Treatment NOPUBLIC

Every participant in this experiment faces the same task. Each participant is entitled to a total payoff of 1.00 euro. This 1.00 euro stems from two sources.

(1) First, there is an envelope on your desk. This envelope contains a cash amount of between 0.00 and 1.00 euros (in 10-cent steps). The money in your envelope belongs to you. The amounts in the envelopes of different participants may differ – not every participant's envelope holds the same amount. Please look into your envelope now. You may pocket the contents of your envelope. You do not have to show these contents to any other participant, nor to the experimenter. During the entire experiment, no other participant will learn the contents of your envelope.

(2) In order to obtain the difference towards the total payoff of 1.00 euro you are entitled to, you will enter a request on the next screen. The amount you request may be between 0.00 and 1.00 euros (in 10-cent steps) and will not be verified. At the end of the experiment the experimenter will pay out to you the amount you requested.

A.2. Instructions - USA

A.2.1. Treatment PUBLIC

In this task we ask you to do the following:

You were randomly allocated to a group of four participants for this part of the experiment. The three other participants in your group face the same task as you do.

On your desk you find a fair, 6-sided die. This means each outcome when rolling the die is equally likely at a probability of 1/6. Please do not yet roll the die until we ask you to do so.

On the next screen you will be asked to roll this die in private and report the outcome. If you like, you can roll the die several times, but we will ask you to report the first outcome only. Please do not show the outcome of your die roll to anyone. No one will verify your report.

The number you report (either 1, 2, 3, 4, 5 or 6) will be multiplied by 20 cents, and the resulting amount will be added to your final payoff.

After you have entered your report, this report, together with your photo, will be shown to the other three group members. You will also see the photos of the other group members and the report each of them made. This step does not affect your payoff in any way.

A.2.2. Treatment NOPUBLIC

In this task we ask you to do the following:

On your desk you find a fair, 6-sided die. This means each outcome when rolling the die is equally likely at a probability of 1/6. Please do not yet roll the die until we ask you to do so.

On the next screen you will be asked to roll this die in private and report the outcome. If you like, you can roll the die several times, but we will ask you to report the first outcome only. Please do not show the outcome of your die roll to anyone. No one will verify your report.

The number you report (either 1, 2, 3, 4, 5 or 6) will be multiplied by 20 cents, and the resulting amount will be added to your final payoff.