ECONSTOR
Make Your Publications Visible.

A Service of
ZBW
Leibniz-Informationszentrum Wirtschaft
Leibniz Information Centre for Economics

Chan, Ho Fai; Dulleck, Uwe; Fooken, Jonas; Moy, Naomi; Torgler, Benno

Working Paper

Cash and the hidden economy: Laboratory and artefactual field experimental evidence on fighting tax evasion in small business transactions

CREMA Working Paper, No. 2021-01

Provided in Cooperation with:
CREMA - Center for Research in Economics, Management and the Arts, Zürich

Suggested Citation: Chan, Ho Fai; Dulleck, Uwe; Fooken, Jonas; Moy, Naomi; Torgler, Benno (2021) : Cash and the hidden economy: Laboratory and artefactual field experimental evidence on fighting tax evasion in small business transactions, CREMA Working Paper, No. 2021-01, Center for Research in Economics, Management and the Arts (CREMA), Zürich

This Version is available at:
https://hdl.handle.net/10419/234616

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

WWW.ECONSTOR.EU

Mitglied der Leibniz-Gemeinschaft

CREMA

Center for Research in Economics, Management and the Arts

Cash and the Hidden Economy: Laboratory and Artefactual Field Experimental Evidence on Fighting Tax Evasion in Small Business Transactions

Working Paper No. 2021-01

CREMA Südstrasse 11 CH - 8008 Zürich www.crema-research.ch

Cash and the Hidden Economy:

Laboratory and Artefactual Field Experimental Evidence on Fighting Tax Evasion in Small Business Transactions

Ho Fai Chan[1], Uwe Dulleck[1,2,3], Jonas Fooken[4], Naomi Moy[5], and Benno Torgler[1,6*]

Abstract: Increasing the tax compliance of self-employed business owners (particularly of trade-specific service providers) remains an ongoing challenge for tax authorities. From a compliance point of view, cash transactions are particularly problematic when services are paid for on the spot, as such exchanges are difficult to audit. As a novelty we present experimental evidence testing 11 different policy strategies in a setting that allows for cash transactions. Our sample includes both students and non-students active in service industries characterised by the opportunity to engage in cash transactions. While our results offer a positive outlook for the interventions reporting a significant effect, they particularly speak to the potential of moral suasion to increase compliance, as it may be implemented at relatively low cost. However, a carrot (offering support in tax declarations) as well as a stick (increasing the threat of audits) approach may be promising for increased compliance, especially where there is an evasion opportunity in cash-for-service payments between small businesses and individual customers who may share a common benefit from tax evasion. A stick approach is particularly efficient for those inclined to use cash transactions.

[*] Corresponding Author: e-mail: benno.torgler@qut.edu.au

[1]Centre for Behavioural Economics, Society and Technology and School of Economics and Finance, Queensland University of Technology, Brisbane, Australia
[2]Crawford School of Public Policy, Australian National University, Canberra, Australia
[3]CESifo, Munich, Germany
[4]Centre for the Business and Economics of Health, University of Queensland, St Lucia, Australia
[5]Department of Sociology and Economic Law, University of Bologna, Bologna, Italy
[6]CREMA – Centre for Research in Economics, Management, and the Arts, Zürich, Switzerland

Introduction

Although maintaining a high level of tax compliance is central to a well-functioning society, one crucial challenge for research in this area is that the inherently hidden nature of tax evasion makes data collection difficult. Increasing self-employed business owners' tax compliance (particularly of trade-specific service providers) remains an ongoing challenge for tax authorities. Cash transactions are particularly problematic from a compliance point of view when services are paid for on the spot. Such exchanges are difficult to audit, allowing businesses to conceal income. The building, renovating, and repairing services are particularly prone to unreported cash transactions. Data from several OECD countries indicate that the building, renovating, or repairing sector is responsible for almost 50% of illicit work (Schneider and Enste 2002).

Enforcement certainly matters in enhancing compliance, as it affects the financial considerations that motivate – at least in part – an individual's compliance choice. Yet, evidence has shown that individuals do not only pay taxes because of the economic consequences of punishment (Torgler 2002, 2007). Recently, the understanding of individual choice processes has been expanded by introducing aspects of behaviour or motivation that can be classified under the general rubric of "behavioural economics". This is broadly defined as an approach that uses methods and evidence from a variety of social sciences to inform the analysis of individual and group decision making. It is therefore valuable to explore potential instruments by applying a more complete understanding of individual (and group) decisions, and one that is more consistent with empirical evidence. Behavioural economics has demonstrated that many individuals are motivated by social norms and intrinsic motivation, and that individuals are capable of learning social norms (Ostrom 2005, Torgler 2007). Research in the area of tax compliance has convincingly argued that successful tax collection is not only the exercise of power (Alm et al. 2010, Alm and Torgler 2011, Kirchler 2007, Torgler 2007): tax compliance, like all human behaviour and institutions, is comprised of a mixture of "love" and "fear" (Boulding 1981). More recently, however, both researchers and tax administrations have placed more emphasis on integrating the "love" aspect, especially given that citizens' consent to pay taxes reflects identification with the taxing authority's objectives (Boulding 1981). In this study we therefore use laboratory experiments to test a set of different instruments so that we may understand compliance behaviour in a situation where cash transactions are possible. We focus on (1) offering assistance from the Australian Tax Office (ATO) to increase compliance, (2)

conducting a clamp down on certain industries, essentially by increasing audits and communicating this increase, (3) changing the frequency of reporting, (4) using moral suasion by emphasising the importance of paying taxes, (5) reminding customers about their role in requesting formal transactions in order to reduce evasion opportunities, and (6) informing sellers of their relative reporting compared to industry peers. Exploring these policies relative to a baseline provides the foundation for the experiment.

Due to the difficulty in collecting primary data on tax evasion, laboratory experiments are an essential tool of thought and exploration in the tax compliance area as researchers strive to generate their own data (for an overview, see Andreoni, Erard and Feinstein 1998, Alm 1999, Torgler 2002, Alm 2012). The beauty of this approach lies in the ability to experimentally test researcher interests while isolating the effect under exploration. Internal validity is also high because other scientists can replicate the experimental conditions. Thus, it is not surprising that we observe an increasing number of laboratory experiments since the 1990s (Torgler 2002, Torgler 2016).

Treatment Background

Moral Suasion

Economists are generally sceptical about the effects of moral suasion, particularly in the long term or in competitive environments (for a discussion see Torgler 2004). On the other hand, social psychologists have demonstrated the power of moral suasion or moral appeals (see, e.g., Cialdini's 2007 seminal work on persuasion). Research in marketing relies heavily on persuasion as a tool to influence human behaviour, as the goal of marketing is to form and change attitudes and activities (Torgler 2013). Less evidence is available, however, on how moral suasion or moral appeals shape tax compliance (for a discussion see Torgler 2004, 2013). Field experimental evidence indicates only very limited support for the proposition that moral suasion matters, reporting barely any effect on tax compliance, even when the field experiment was conducted at the local level where moral suasion might be more effective than at the national level (Blumental, Christian, and Slemrod 2001, Torgler 2004, Torgler 2013). Thus, the mixed results obtained thus far indicate the necessity of generating further evidence at the lab and field experimental level.

Assistance

Providing assistance to taxpayers encourages identification with the task and with the entity offering assistance, which in this case is the tax administration. It can promote reciprocity through increased mutual obligations. Humans tend to repay what another has provided, feeling internally duty-bound to reciprocate favours (Boulding 1981); this web of indebtedness is a deeply rooted adaptive mechanism in human nature (Cialdini 2007). Such reciprocity can contribute to the development of integrative structures between taxpayers and the tax administration. Failing to comply in future would trigger internal discomfort and psychological costs (Erard and Feinstein 1994). Mazar et al. (2008, p. 634) stress that if "a person fails to comply with his or her internal standards for honesty, he or she will need to negatively update his or her self-concept, which is aversive. Conversely, if a person complies with his or her internal standards, he or she avoids such negative updating and maintains his or her positive self-view in terms of being an honest person" (p. 634). As a consequence, Mazar et al. (2008) maintain that people will comply with their internal standards even if compliance necessitates an investment of effort and sacrifice. Previous research has found neuroscientific evidence for this principle (Dulleck et al. 2016). Using data from the US and Turkey, Torgler, Demir, Macintyre and Schaffner (2008) analyse interactions with the tax administration, finding that positive attitudes towards the tax authority (e.g., how taxpayers rated tax administrations' job, their honesty and fairness, and their helping and information behaviour) significantly increase tax morale. A respectful and fair treatment of taxpayers induces respect for the tax system and therefore promotes co-operation (Smith 1992).

Peer Effects

There is substantial evidence that peer effects matter in the area of tax compliance (for an overview see Frey and Torgler 2007, Spicer and Becker 1980, Webley, Morris and Amstutz 1985). Such peer effects have been observed in other illegal or non-compliant activities such as assassinations, hijackings, corruption, kidnappings, serial murders, and littering (Bikhchandi, Hirshleifer and Welch 1998, Dong, Dulleck, and Torgler 2012, Torgler, Frey and Wilson 2009). Kahan (1998) suggests that the decision to commit crimes in general is highly interdependent, based on the perceived behaviour of others: "When they perceive that many of

their peers are committing crimes, individuals infer that the odds of escaping punishment are high and the stigma of criminality is low. To the extent that many persons simultaneously draw these inferences and act on them, moreover, their perceptions become a self-fulfilling reality" (p. 394).

Increasing Audit Rates

Traditionally, audit rates have been a key variable of analysis (particularly among economists). Evidence from lab experiments tends to indicate that a higher audit rate leads to more compliance (Alm 1999, Torgler 2002). A meta-study focusing on lab experiments indicates that raising the audit rates will increase tax compliance (Blackwell 2010).

Endogenous Audit Selection Rules

Many tax compliance experiments have integrated endogenous audit selection rules to increase external validity (for a discussion see Torgler 2002). Tax agencies often do not select tax returns randomly for audit but use instead information from the returns to determine audit. For example, the Internal Revenue Service (IRS) uses the Discriminant Index Function (DIF, formula) based on items reported on current tax returns in its selection of returns (Alm, Cronshaw and McKee 1993). Other countries follow a similar practice (Roth, Scholz and Witte 1989). Thus, the probability of audit is endogenous in the sense that it depends on the behaviour of taxpayers. There are different ways of introducing selection rules. A common one is to use past behaviour, increasing the audit probabilities for those taxpayers known to have been noncompliant in the past. We will rely on such an approach in our analysis. However, one could also apply a cut-off rule which means that taxpayers who report less than some cut-off level of income will be audited with certainty. Experimental results indicate that endogenous audit rules are able to generate significantly greater compliance than random audit rules (Torgler 2002). Endogenous audit rules are also able to smooth the level of tax collection over time by reducing the variation in individual compliance rate (Alm, Cronshaw and McKee 1993). In general, Slemrod et al. (2001) emphasise that "heightened audit threat should be carried out simultaneously with a rethinking of how the audits themselves are carried out" (p. 482).

Audit Remorse

The notion behind audit remorse is that the tax authority treats taxpayers with respect and fairness, inducing respect for the tax system, which may lead to a higher level of co-operation. Some people become non-compliant by mistake and such individuals are usually willing to correct their behaviour and transform into honest taxpayers. Thus, informing taxpayers about their non-compliance, and allowing them to correct their behaviour offers the chance to integrate accidental non-compliers into the taxation system. It may also provide a signal to the taxpayers that they are expected to sort out their affairs properly in future. This is especially the case if audit remorse is combined with a higher penalty/punishment when the non-complier is again caught evading taxes after having reconsidered their declaration. For a tax system to be sustainable long-term, it requires a strategy that penalises those who may attempt to cheat the system. Overall, such a strategy may enhance the internal commitment to reciprocating behaviour, as it provides those who are non-compliant with a fair warning to take corrective action. Changes in the penalty may also indicate to the taxpayers that the tax administration cares about solving the taxpayers' non-compliance problems. Another advantage is that it increases the level of information available, generating possibilities for tax education. It also provides the taxpayers with more procedural information, increasing the capacity for cooperation (Ostrom 2005). There is some evidence that intensification of enforcement efforts offers a successful strategy for increasing tax compliance after a tax amnesty (Alm, McKee and Beck 1990). It might be seen as a fair warning, especially for those taxpayers who were honest before the tax amnesty; the goal is to convince tax delinquents that tax evasion is morally wrong (Fisher, Goddeeris and Young 1989).

It is useful to think about procedural fairness and reciprocity while taking into account a balanced concept between trust and power in cases of non-compliance (Kirchler et al. 2008, Batrancea et al. 2019). The compliance process requires trust, motivation and commitment (Torgler and Schneider 2009). Such an audit remorse structure signals that if the proffered trust is not reciprocated, harsher consequences are used.

Pre-Filled Reports

Pre-filled reports provide a method of assisting taxpayers, reducing the transaction costs and uncertainty or even anxiety costs. We will apply an experimental treatment similar to that

received by paying for the assistance of a tax practitioner (service at a small cost). However, in our setting there is no intention to exploit the tax system to the advantage of taxpayers. Many studies show that the average level of noncompliance is higher for returns prepared with paid assistance. Erard (1993) found that the use of a tax practitioner significantly increases tax cheating. In general, taxpayers with professional help tend to have more complex tax forms, which increases the possibility of tax evasion or tax avoidance.

A pre-populated tax form is a particularly attractive solution for non-lodgement, although this is not an issue in our experimental setting. The problem is that pre-populated tax forms are subject to many issues (Fonseca and Grimshaw 2015); for example, they reduce the incentive to report income and deduction items that are not pre-filled (Kotakorpi and Laamanen 2015). Thus, inaccurate information leads to a decrease in compliance. The risk of overdeclaration produces additional administrative costs, anger among taxpayers, and can affect the reputation of the tax administration. Moreover, the self-assessment system promotes self-regulation (Torgler and Murphy 2004). Regulatory scholars (e.g., Ayres and Braithwaite 1992, Sparrow 2000) consistently argue that if those being regulated are allowed to self-regulate in the first instance, then this serves to improve their voluntary compliance in the long-term. This is because self-regulation is important for building and maintaining trust among regulators and the regulated. The problem is that pre-filled forms promote incomplete learning regarding the complexity of the tax system and tax salience (Kotakorpi and Laamanen 2015). Thus, there are concerns that pre-filled forms reduce taxpayers' engagement and tax and financial knowledge (Cordes and Holen 2010) and therefore their opportunities for learning experiences.

Positive Feedback

Positive feedback (via an instrument such as a thank you note for being compliant) is intended to motivate and reward desired behaviour. Instead of raising the relative cost of not paying taxes, the instrument of rewards raises the benefits of paying taxes (Feld, Frey and Torgler 2006). Rewards are widely used in daily business activities and in society in general, as an acknowledgement of the desired compliance or behaviour. A thank you note might be perceived as supportive, bolstering future compliance and strengthening the attractiveness of rewarding "good" taxpayers. The power of rewards in shaping human behaviour has long been a topic among social psychologists (see, e.g., Thorndike 1911, 1932, Skinner 1953, Nuttin and

Greenwald 1968). Rewards are expected to change the relative prices such that paying taxes becomes a more attractive alternative to evading taxes. However, this does not necessarily mean that the effect is big enough to actually enhance compliance. Moreover, although it may produce sustainable compliance among generally honest taxpayers, it is unclear how taxpayers who are inclined to be non-compliant would respond to a positive acknowledgement such as a thank you note. From the tax administration's perspective, giving a thank you note is a low-cost policy strategy. The tax administration always faces a trade-off between the costs and benefits of giving rewards and the costs and benefits of other incentives, particularly the costs involved with punishment. To be cost effective, rewards must raise net tax revenues, i.e. the gross revenues after deducting the cost of rewards (Feld, Frey and Torgler 2006).

Experimental Design and Procedures

In order to address the question of tax compliance by small business owners in the presence of different tax enforcement regimes, an experimental design was employed using stylised interaction between service providers and customers. Participants were told that they would make decisions in a service provider-customer framework and would interact with other participants in these roles throughout the experiment.

Upon arrival at the laboratory participants were welcomed and given an explanation that they would make decisions throughout the experiment, for which they would be paid (see *Figure A1* in the Appendix). This ensures that participants have an incentive to do what they like best – if they would like to choose the dishonest declaration, they could do so and reap the economic benefits from this course of action. If they made an honest declaration instead, they would need to give up some monetary income at the end of the experiment.

After familiarising participants with the rules that would determine their payments (see *Figure A2* in the Appendix), the structure of the experimental interaction was explained (see *Figure A3*). They were told that there would be two roles, those of the service providers and those of the customer. In each round of interaction, the customer would need to get a job done in order not to lose 80 experimental dollars (from the 100 experimental dollars the customer would receive each period). On the other side of the interaction, the service provider was able to make an offer to the customer for getting the job done. Participants were told that they could think of it as the relationship between a house owner and a tradesperson:

> "To understand the interaction between service providers and consumers consider the consumer to be a house owner who needs some job to be done in the house which he cannot do himself. In real life this could be the repair of your swimming pool, of your hot water system or the refurbishment of a fence around your house. Whilst these jobs may not need to be fixed straight away, there is a greater cost of ignoring the problem. The service provider in turn is someone who can do the job, such as a pool repair person, a plumber or someone specialised in fixing fences" (see *Figure A*3).

Furthermore, participants were told that the price paid by customers to service providers constituted income, and the service providers had to pay taxes on that income. The experiment tax rate was 40% (see *Figure A4*). Furthermore, service providers were asked if they wanted to offer a cash option to customers, and customers were asked if they would accept a cash offer (both were asked independently and the cash offer was only implemented if both had opted for the cash option). Cash offers implied that the price to be paid by the customer would be 10% lower. Furthermore, wherever the cash option was implemented, the income (hence, the price minus 10%) of the service provider was not automatically taxed and instead, the service provider had to declare how much had been received. Hence, in case of cash transactions, it is possible to evade taxes by under-declaring income. Non-cash transactions were in turn automatically "declared" correctly. Participants were also informed that money collected as taxes would not be lost, but paid to a university charity.

In addition, participants were told that after each declaration there was a possibility that the service provider would be audited. The audit probability (in the baseline treatment) was set to 10%, and there were consequences for failing the audit. If a service provider was audited and the amount declared was lower than the correct amount, the service provider would have to pay a fine, which corresponded to the underpaid amount plus the difference of the underpaid taxes.

Participants were then advised that there would be two parts of the experiment (see *Figure A5*). In the first part they would be either a service provider or a customer and then they would switch roles in the second part. Participants were also told that each part had 6 rounds and that in every round a different customer would interact with a different service provider. Participants then proceeded to the decision round where they were advised that there would potentially be further instructions on later screens and were therefore asked to read these carefully. In experimental terms, these further instructions represent the different treatments

throughout the experiment. The treatments are outlined in further detail below. Furthermore, before starting with the decision making, participants had to answer two control questions to ensure that they understood the game and how they would be paid based on their decisions. Control questions had to be answered correctly before participants were able to advance in the experiment. The full instructions showing the buyer and seller interactions – including the auditing – are reported in the Appendix (see *Figures A6* to *A11*).

The experiment utilised the baseline as described above, and six further treatments. Treatments were introduced by further descriptions on participants' computer screens. The following lists the treatments used (see also *Figures A6.1* to *A7.3* in the Appendix):

1. The *Baseline*, participants in this control group received full information about the experiment as outlined above with an audit probability of 10%.
2. The *ATO assist*, hence, it was pointed out to participants that they could request further help on how to comply by asking a research assistant who was available for compliance questions.
3. The *Clamp down*, which notified participants that in the following three periods the industry of service providers had come under special scrutiny, implying a doubling of the audit probability (relevant for the sellers).
4. The *Infrequent* reporting, meaning that service providers did not have to report their income every period but rather after 3 periods as one large instalment.
5. The *moral suasion*, meaning that both customers/buyers and service providers/sellers were reminded that tax money served a common good and that it would be paid to charity, namely a food bank at the university (QUT). Therefore, it was pointed out that paying taxes in this experiment was important from a common good perspective.
6. The *peer effects buyer*, which pointed out to buyers that accepting cash offers would provide sellers with the opportunity to evade taxes – and that buyers could consequently play a part in increasing compliance.
7. The *peer effects seller*, for which sellers were informed that their declaration was below/about/above the industry average of declared income based on previous experiments. While not asking sellers to reconsider their declaration, they had the option to then adapt the amount declared.

To these initial treatments we added four further treatments (see also *Figures A8.1* to *A8.4* in the Appendix):

8. The *audit remorse*, in which sellers who had been audited and identified as under reporting were informed that the amount declared appeared to be too low. They were told that they had the opportunity to reconsider their declaration, and were also informed that if they were caught declaring too little (again) after reconsidering, the penalty would be tripled.
9. The *endogenous audit*, for which participants were informed at the time of their tax declaration that their personal audit probability would be doubled if they were found to be non-compliant.
10. The *infrequent pre-filled* reporting, which was based on the infrequent condition. However, participants were given the opportunity to have their tax declaration pre-filled based on their income. They could receive this pre-filling service at a small cost.
11. The *positive feedback*, which included a message of thanks to those participants who had made a compliant declaration. This message was not provided to non-compliant participants (independent of them being audited or not).

Furthermore, in order to increase statistical power when making comparisons between treatments, we also increased the number of sessions in the *baseline* and *infrequent* treatments.

The actual tax declaration screen is shown in *Figure A9*. Once the experiment was completed (i.e., after the two rounds of 6 periods each) participants were asked to fill in a post-experimental questionnaire, which provided further demographic information and self-reported attitudes of participants. The full list of questions is included in the Appendix (see *Figure A14*). Questions relating to tax compliance, demographic questions (gender, age, nationality) and business ownership were compulsory, while others (e.g. income, religion) were answered on a voluntary basis.

The experiment was conducted in a computer laboratory at Queensland University of Technology in Queensland, Australia, between June 7 and June 24 2016 with 200 volunteer participants. Of these, 16 were used in a pilot session, and were therefore excluded from later analysis, resulting in a final number of 184 participants of which 42.93% were female, the average age was 23 and 47.83% of participants were Australian nationals. The sample of participants consisted of undergraduate and postgraduate students, and the largest proportion

of participants were undergraduate students (71.67% Bachelors, 19.44% Master and 8.89% PhD).[2] We control for these characteristics, including the nationality of participants.

Additional sessions for treatments 9 – 11 were conducted between October 18 and October 25 with 82 volunteer participants of which 46.34% were female, the average age was 24 and 32.92% were Australian nationals. As before, the sample consisted of undergraduate and postgraduate students (66.67% Bachelors, 24.36% Masters and 8.97% PhD). Collecting this additional data allowed us to control for various factors in the analysis of experimental results. In addition, we recruited further participants who were active in sectors that are known for their potential for dealing with numerous cash transactions. We invited 100 participants from such areas; 87 participated and 67 completed all parts of the study (some participants had to be sent home due to overbooking and 3 individuals had to leave early in one of the sessions, hence, still providing data points without specifying their characteristics in the questionnaire). Most participants were active in typical trades. The most commonly declared occupations were carpenters (23 participants), electricians (7), in the construction sector (tilers, etc., 7), air conditioning and refrigeration (5) and plumbing (3). Their average age was 23 years old (despite the similar average to students, there are several participants in this group who are older than in the student group). 83.14 of these participants were male and 82.66% were Australian nationals. All these non-student subjects acted in the role of a service provider in the experiment, as all treatments were targeted at the sellers. Students took the role of the customers, and were seated in a nearby (but physically separated) laboratory. As customer decisions are not the focus of this part of the study, data from these participants were not used in the analysis below.

At the end of the experiment, participants were paid for their decisions. Experimental payments were first calculated in experimental dollars based on two (three) randomly chosen interactions and subsequently exchanged into the Australian dollar at a rate of 0.75 experimental dollars = 1 AUD for sessions with student (non-student) participants as announced at the start of the experiment. On average, participants in sessions with student (non-

[2] Due to the restricted time frame the baseline was conducted by itself whilst the remaining six treatments were grouped into a session as follows, *ATO assist* and *clamp down*, *infrequent* and *moral suasion*, and *peer effects buyer* and *peer effects seller*. Of the seven treatments conducted, *infrequent* and *moral suasion* have the most participants (54), whilst the *baseline* has the greatest number of interactions (204). *ATO assist* and *clamp down* had 50 participants, followed by *peer effects buyer* and *seller* with 46 participants. Due to technical difficulties, the observations of 17 participants in *moral suasion* were affected, resulting in 142 interactions instead of 150.

student) participants earned, including a show-up fee of 10 (0 or 120) Australian dollars for participating in the experiment and 40.55 (159.97 or 54.18) Australian dollars [SD = 11.08 (15.76 or 13.23)] throughout the experiment, which lasted approximately 55 (60) minutes.[3]

Results

The data generated by experimental decisions were loaded into the STATA statistical software for analysis. The decision variables of the experiment can be summarised as follows: The average amount earned was 65 experimental dollars (standard deviation = 14), the frequency of cash offers was 58% and the average declared amount was 51 experimental dollars (*SD* = 30) for service provider decisions. For customers, 78% of offers were accepted, and the frequency of customer acceptance of cash offers was 84%. These numbers show that there was significant scope for evasion and that cash offers were frequently used and substantial amounts were evaded. It is important to note that caution is necessary when applying absolute levels observed in the laboratory. The absolute values reflect decisions inside the laboratory and similar levels in the real world are coincidental, i.e., levels in the real world may be substantially different. This also includes potential level differences between student and non-student participants: in the experiment, the overall compliance rate of non-student participants is higher. However, this does not indicate that tax compliance of service providers is higher than the compliance rate of students. What is informative, however, are differences between the different treatments within the group of students and non-students. This methodology focuses on the difference between the treatments and the control group, as the qualitative effects of treatments or interventions have been shown to be similar in the laboratory and the real world.

Based on the experimental design, the main variable of interest is the amount declared by service providers (also including the cases where no cash offer was provided). Hence, we are not just looking at tax evasion but rather the final declared amount, which is of crucial interest for the tax administration (how much tax money comes in). In our setting, we can directly focus on the declared amount, as we have the same tax rate for all the relevant

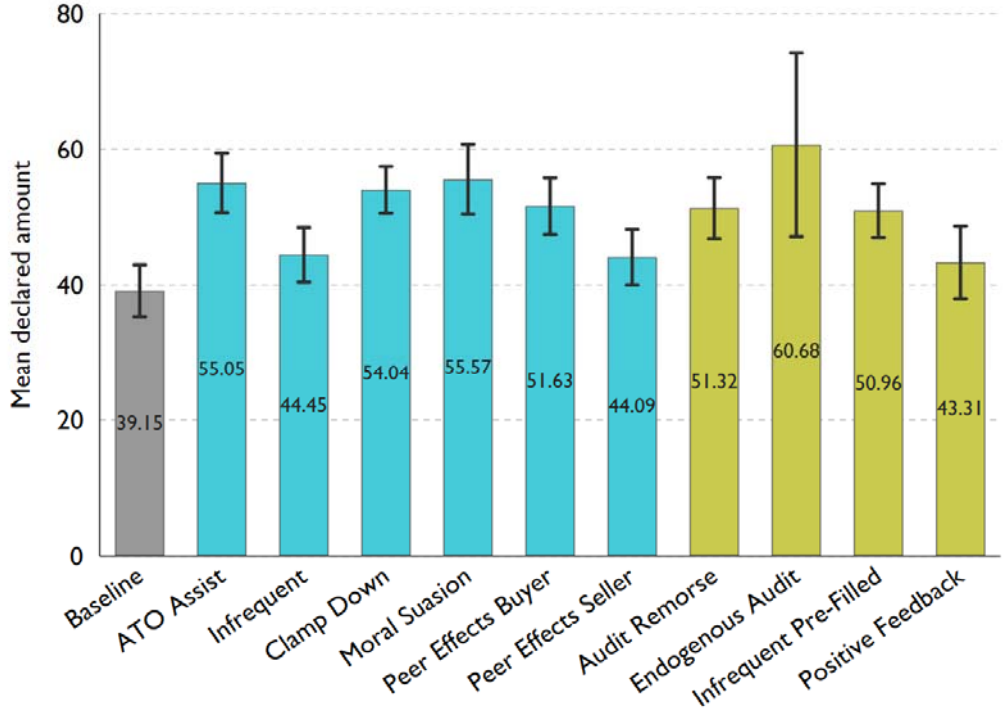
[3] Non-student participants received a show-up fee of 120 AUD in non-student sessions. Student participants did not receive any show-up fee in these sessions, because using three rounds for payment in these sessions ensured that student participants were paid a sufficiently high amount for participating.

treatments and the control group. We first focus on our results using student participants before scrutinising our findings by introducing non-student participants. *Figure 1* shows the average amount declared, separated by treatments (the new treatments are represented by black instead of grey bars). As can be seen, the amount declared appears to be higher in some treatments than in others, whereas all relevant treatments appear to increase compliance. It is clear from *Table 1a* that the differences between the control and the relevant treatment groups are statistically significant (with the exception of the infrequent treatment). Moral suasion, assistance and endogenous audits seemed to have the strongest effect on income declaration.

Similarly, in *Figure 2* we report the declared income by treatment for non-student participants. The results are consistent, with moral suasion and assistance leading to higher levels of compliance. However, the results of *Table 1b* indicate that these effects may not be significant for non-student participants. For this group, peer effects with other service providers have a positive effect on the declared amount, indicating that for non-student participants the ability for comparison plays a significant role. From the results in *Table 1b*, it is evident that infrequent reporting leads to significantly lower levels of compliance for non-student participants.

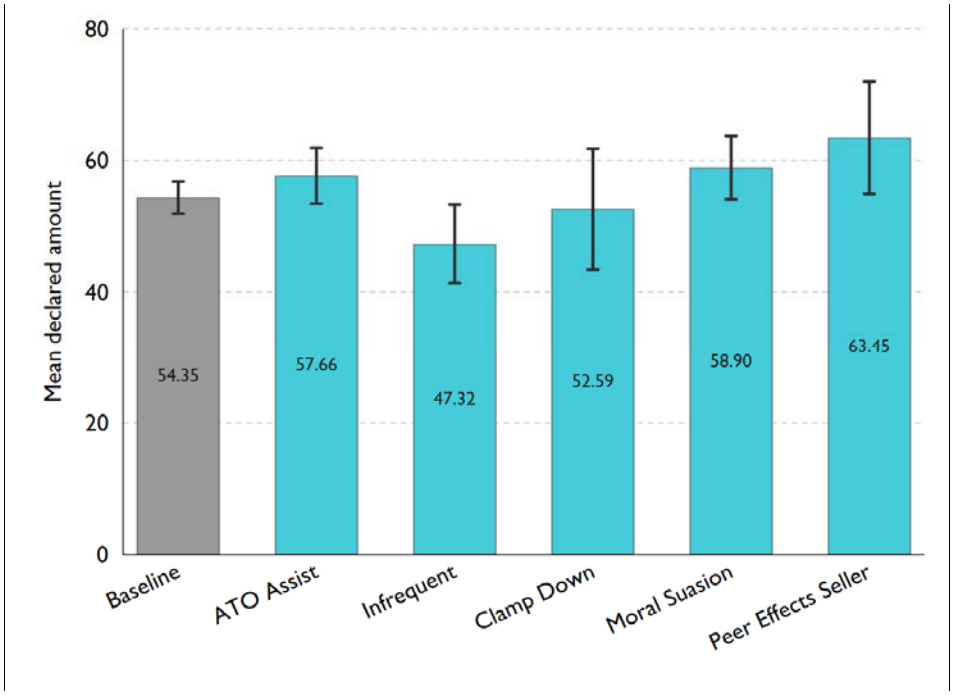
We initially excluded the peer effect buyer treatment for student participants, as this treatment should only have an effect in terms of increasing the rejection rate of cash offers by the sellers. From a seller's perspective, such a treatment is similar to a control group. To our surprise, this treatment reported a higher declaration rate. Thus, we added that treatment to the control group in *Figure A14*. The treatments of moral suasion, assistance, and clamp down still show a statistically significant difference from the control group as shown in *Table 2a*.

Figure 1: Declared amount in the control group and the treatment groups (Student session)



Note: Error bars represent 95% confidence intervals.

Figure 2: Declared amount in the control group and the treatment groups (Non-student session)



Note: Error bars represent 95% confidence intervals.

We also compare the two participant groups of students and non-students (for their respective treatments). As can be seen in *Table 2b*, non-students declare significantly more than students in the baseline condition as well as in the seller peer effects condition. This indicates that there may be a level effect, with non-students being more compliant in the experiment. We take this fact into account when further investigating the effects that treatments have on decisions of the two groups, which appear to be in the same direction, but potentially with differing strengths.

Table 1a: Differences between the groups (Student session)

Treatment	**Mean difference**	**t-statistic**	**N (treatment)**	**N (baseline)**	**Rank-sum**
ATO Assist	15.91***	-5.15	100	185	-4.79***
Clamp Down	14.89***	-5.6	149	185	-4.72***
Infrequent	5.3*	-1.88	162	185	-1.88*
Moral Suasion	16.42***	-5.18	137	185	-4.9***
Peer Effects Seller	4.94*	-1.66	108	185	-1.22
Audit Remorse	12.17***	-3.76	85	185	-3.19***
Endogenous Audit	21.53***	-4.04	79	185	-2.85***
Infrequent Pre-Filled	11.81***	-3.72	83	185	-3.21***
Positive Feedback	4.16	-1.2	75	185	-0.98

Notes: Results for two-sample mean comparison *t*-test (column 1 and 2) and non-parametric Wilcoxon rank-sum test (column 5). Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance. Baseline consists of data from both experiments. Mean declared amount of the baseline in first experiment is 38.58 (N=144) and 41.12 (N=41) in the second experiment. The mean difference between the baselines is not statistically significant (p=0.58).

Table 1b: Differences between the groups (Non-student session)

Treatment	**Mean difference**	**t-statistic**	**N (treatment)**	**N (baseline)**	**Rank-sum**
ATO Assist	3.31	-1.17	86	390	-2.31**
Clamp Down	-1.76	0.52	87	390	1.56
Infrequent	-7.03**	2.3	80	390	1.98**
Moral Suasion	4.55	-1.55	82	390	-2.82***
Peer Effects Seller	9.1***	-2.73	85	390	-2.11**

Notes: Results for two-sample mean comparison *t*-test (column 1 and 2) and non-parametric Wilcoxon rank-sum test (column 5). Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance. Baseline for non-student group is the first six rounds.

Table 2a: Differences between the groups (Student only)

Treatment	Mean difference	t-statistic	N (treatment)	N (baseline)	Rank-sum
ATO Assist	11.36***	-3.99	100	291	-4.06***
Clamp Down	10.35***	-4.27	149	291	-3.56***
Infrequent	0.76	-0.3	162	291	-0.39
Moral Suasion	11.88***	-4.23	137	291	-4.26***
Peer Effects Seller	0.4	-0.14	108	291	0.55
Audit Remorse	7.63**	-2.53	85	291	-1.97**
Endogenous Audit	16.99***	-3.74	79	291	-1.4
Infrequent Pre-Filled	7.27**	-2.44	83	291	-1.81*
Positive Feedback	-0.38	0.12	75	291	0.45

Notes: Results for two-sample mean comparison *t*-test (column 1 and 2) and non-parametric Wilcoxon rank-sum test (column 5). Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance. Baseline includes peer effects buyer.

Table 2b: Differences between the students and non-students

Treatment	Mean difference	t-statistic	N (Non-student)	N (Student)	Rank-sum
Baseline	15.2***	-6.78	390	185	-5.72***
ATO Assist	2.61	-0.84	86	100	-0.89
Clamp Down	2.87	-0.8	80	162	-0.42
Infrequent	-1.45	0.35	87	149	1.77*
Moral Suasion	3.33	-0.87	82	137	-1.71*
Peer Effects Seller	19.36***	-4.34	85	108	-4.54***

Notes: Results for two-sample mean comparison *t*-test (column 1 and 2) and non-parametric Wilcoxon rank-sum test (column 5). Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance. Baseline for student excludes peer effects buyer.

Next, we compare the peer effects buyer treatment with all the other treatments – including the control group – to take a closer look at whether text introduced in the peer effects buyer treatment reduces the likelihood of accepting a cash offer. In *Figure A15* we can see that the introduction of such text only has a small effect that is not statistically significant.

In the multivariate analysis we first present empirical results using the student participants and adding the peer effects buyer treatment to the control group (see specifications A to C), therefore generating conservative estimates. In specifications D to E we then exclude

the peer effects buyer treatment from the specifications. We conduct a multivariate analysis, because it is possible that the effects visible in the descriptive analysis may be driven by the fact that individuals were observed repeatedly. Furthermore, as indicated by the design features described in footnote 1, there may be session effects. For this reason, statistical differences were tested using multilevel regressions, which are a statistical tool designed to control for such effects. *Table 3* shows the results from this analysis, comparing the amount and successively introducing further controls, namely demographics (B, E) as well as the price offered to the consumer (C, F). Additionally, the domestic status of the participant was further controlled for (see appendix Table A1), where stating Australian nationality is statistically significant in increasing the amount declared. As can be seen in the Appendix, the results presented are robust when controlling for this characteristic of participants. The result is also represented graphically as the difference from the control group in *Figures 3a* and *A16* of the Appendix. As can be seen, *ATO assist*, *clamp down, moral suasion,* and *endogenous audit* significantly increase the amount declared. *Infrequent* reporting leads to (insignificantly) lower amounts declared and *seller peer effects* leads to slightly higher (but not statistically significant) declared income. The same observation of (insignificantly) higher declarations is observable for the *audit remorse* and *pre-filled infrequent* treatments. Furthermore, providing positive feedback appears to decrease the declared amount, however, this effect is statistically insignificant.

We subsequently investigate the treatment effects using the sample of non-student participants. *Table 3b* shows the results from this analysis, using the same estimation approach as for student participants, with the introduction of further control variables. As can be seen, the significance level of the treatments for this group is small and less significant for *ATO assist*, *clamp down*, and *moral suasion*. However, a significant positive effect for *peer effects seller* and a significant negative effect for *infrequent* reporting is reported. Similarly, these treatment differences for non-students are illustrated graphically in *Figure 3b*. However, these differences in significance levels may be partly due to differing sample sizes. Hence, we investigate if responses to the treatments are significantly different between the groups and whether a treatment effect remains when considering both groups together (discussed further below).

Table 3a: Multilevel random-effects regressions of determinants of the declared amount (Student only)

Dep. Var. = Declared amount	**A**	**B**	**C**	**D**	**E**	**F**
treatments						
ATO Assist	10.99**	10.50**	7.94	15.07***	15.28***	12.29**
	(2.10)	(2.02)	(1.60)	(2.68)	(2.73)	(2.30)
Clamp Down	11.33**	8.44*	10.30**	15.39***	13.19**	14.54***
	(2.39)	(1.68)	(2.15)	(2.97)	(2.43)	(2.82)
Infrequent	2.76	1.55	3.36	6.81	6.24	7.55
	(0.60)	(0.33)	(0.76)	(1.35)	(1.24)	(1.58)
Moral Suasion	10.77**	10.25**	7.37	14.86***	15.04***	11.73**
	(2.16)	(2.06)	(1.55)	(2.75)	(2.79)	(2.29)
Peer Effects	1.22	1.40	5.40	5.28	6.22	9.66*
Seller	(0.23)	(0.26)	(1.06)	(0.92)	(1.09)	(1.77)
Audit Remorse	8.07	7.20	5.36	12.14*	11.95*	9.66
	(1.36)	(1.21)	(0.95)	(1.93)	(1.90)	(1.61)
Endogenous	17.92***	15.90**	17.85***	21.99***	20.58***	22.00***
Audit	(2.94)	(2.57)	(3.03)	(3.41)	(3.16)	(3.55)
Infrequent Pre-	7.49	6.30	7.99	11.56*	11.04*	12.21**
Filled	(1.26)	(1.06)	(1.40)	(1.83)	(1.75)	(2.03)
Positive	-0.06	-0.27	5.71	4.01	4.51	9.93
Feedback	(-0.01)	(-0.04)	(0.98)	(0.62)	(0.70)	(1.61)
Further controls						
male		-4.86*	-4.70*		-5.27*	-5.05*
		(-1.79)	(-1.82)		(-1.86)	(-1.88)
age		0.40	0.40*		0.42	0.43*
		(1.61)	(1.68)		(1.63)	(1.72)
control for			0.63***			0.62***
price offered			(9.70)			(9.01)
Constant	43.95***	42.73***	2.68	39.87***	38.05***	-1.05
	(16.09)	(5.73)	(0.33)	(11.68)	(4.79)	(-0.12)
N	1269	1249	1249	1163	1143	1143
Prob. > chi^2	0.044	0.025	0.000	0.012	0.006	0.000

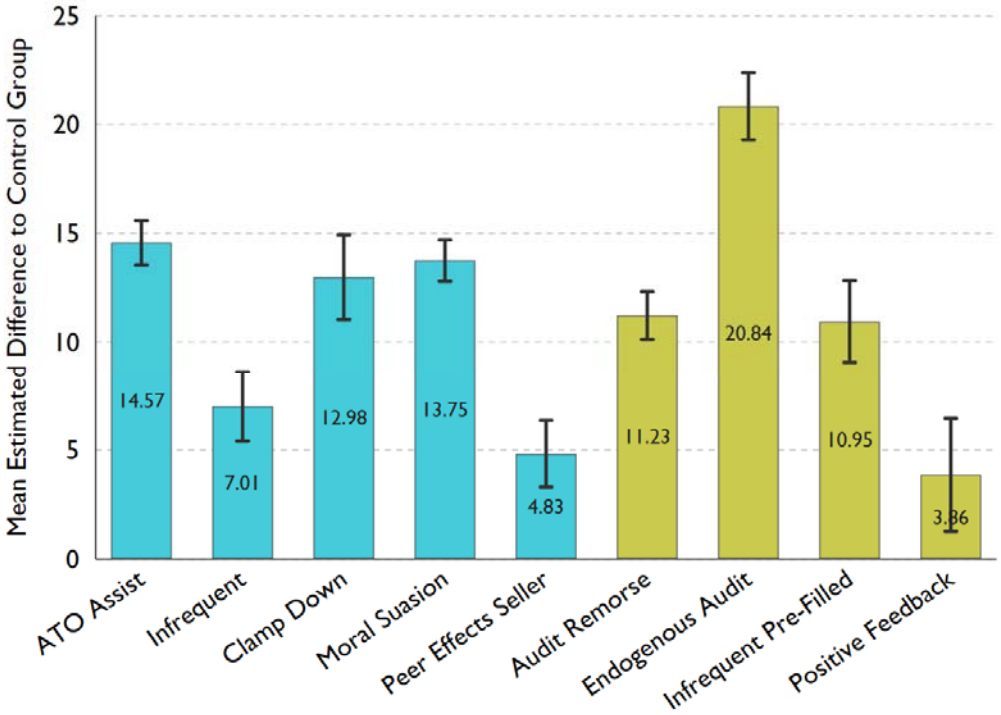
Notes: Reference in A, B, and C: baseline + peer effects buyer treatment. D, E, F only baseline. Standard errors are clustered by individual and session. Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance.

Table 3b: Multilevel random-effects regressions of determinants of the declared amount (Non-student only)

Dep. Var. = Declared amount	**A**	**B**	**C**
treatments			
ATO Assist	0.89	1.20	-0.52
	(0.26)	(0.36)	(-0.16)
Clamp Down	5.22	5.00	4.29
	(1.49)	(1.41)	(1.26)
Infrequent	-6.45*	-6.43*	-9.67***
	(-1.78)	(-1.78)	(-2.77)
Moral Suasion	3.53	3.07	-1.37
	(1.02)	(0.87)	(-0.40)
Peer Effects Seller	7.24**	7.45**	5.70*
	(2.08)	(2.14)	(1.70)
Further controls			
male		-3.26	-1.73
		(-0.64)	(-0.36)
age		0.17	-0.10
		(0.52)	(-0.33)
control for price offered			0.67***
			(8.49)
Constant	54.31***	56.51***	19.21
	(25.78)	(4.30)	(1.44)
N	810	810	810
Prob. > chi^2	0.054	0.110	0.000

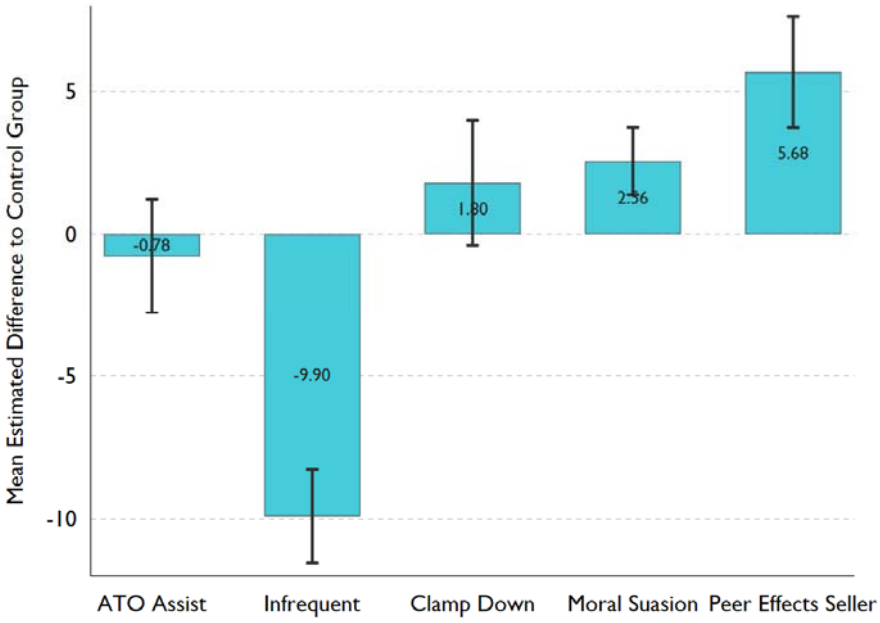
Notes: Reference in A, B, and C: baseline. Standard errors are clustered by individual and session. Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance.

Figure 3a: Estimated differences of declared amount to baseline (Student session)



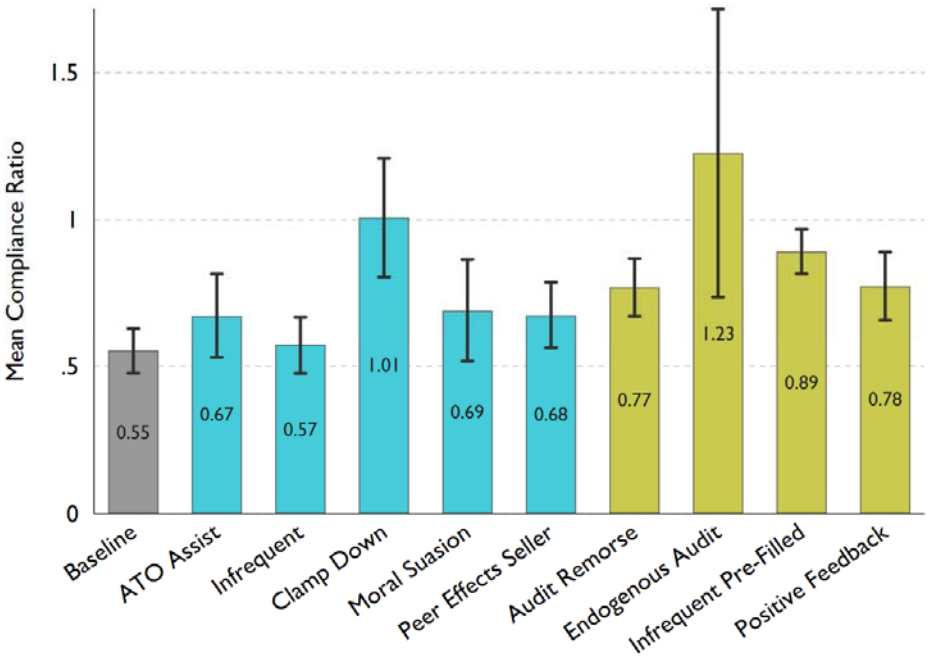
Note: Control group is equal to *Baseline*. *Peer Effect Buyer* is excluded. Error bars represent 95% confidence intervals.

Figure 6b: Estimated differences of declared amount to baseline (Non-student session)



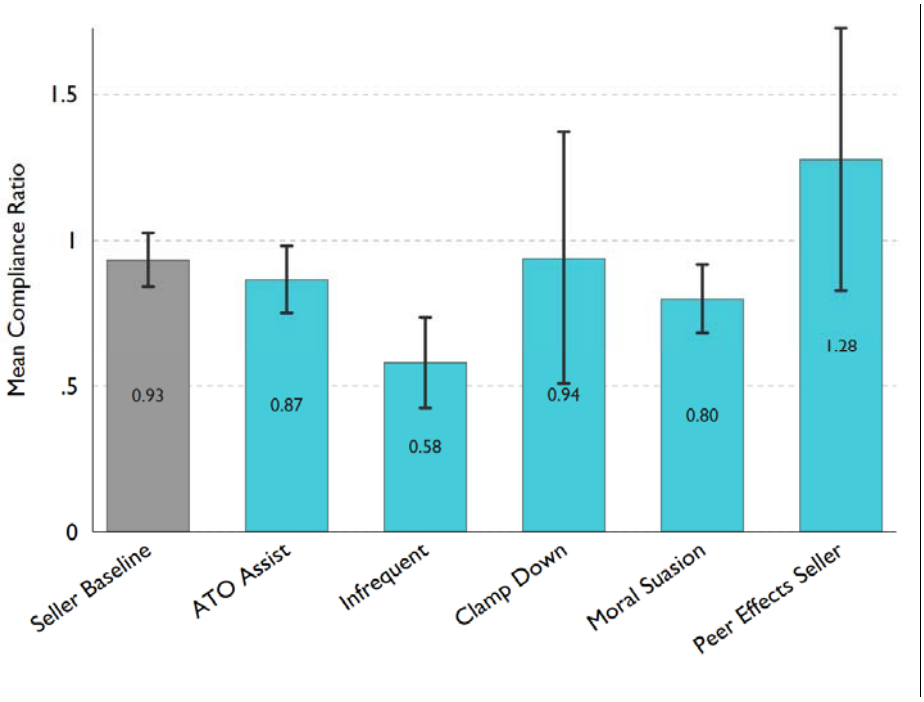
Note: Control group is equal to *Baseline*. Error bars represent 95% confidence intervals.

Figure 7: Tax compliance with the baseline (Student session)



Note: Control group is equal to *Baseline*. *Peer Effect Buyer* is excluded. Error bars represent 95% confidence intervals.

Figure 8: Tax compliance with the baseline (Non-student session)



Note: Control group is equal to Baseline. Error bars represent 95% confidence intervals.

Next, we take a closer look at the tax compliance of sellers whose suggested cash transaction was accepted by the buyers. Overall, such sellers might be more inclined to evade taxes and we therefore predict that softer factors such as assistance or moral suasion would be less effective. The results reported in *Figures 7* and *A18* and *Table 4a* support these predictions. The *endogenous audit* and the *clamp down* treatments clearly have the strongest effect (coefficient is always statistically significant). *Table 4b* and *Figure 8* uses non-student instead of student participants and slightly modifies this conclusion, indicating no significant effect for the *clamp down* treatment, but a marginally positive influence of *peer effects*.

We also pool both groups together – students and non-students – to investigate the degree to which our findings hold for both types of participants. We first investigate the difference-in-difference between the treatments used for both groups. *Table 5* shows the estimated difference in the treatment effect for all five treatments used in the two groups. It also includes a difference-in-

difference measure for potential level effects between the groups. *Figures 17a-e* furthermore illustrate the difference in effects for each treatment graphically. As can be seen in Table 5, non-students declare more, but do not necessarily differ in their response to the treatment. However, they respond significantly less to *ATO assist* and *moral suasion*, and essentially reduce the detected treatment effect to a zero-treatment-effect. Additionally, non-students' reaction to the *infrequent* treatment is significantly different, indicating that infrequent reporting significantly reduces their declarations (while it does not for students). Finally, the reactions to the *clamp down* treatment are not significantly different, indicating that a focus on their industry (a stick approach) works similarly well for the non-students. Finally, there does not appear to be a significant interaction effect for the *peer effects* treatment. However, as peer effects have a significant influence on the non-students (as well as on the joint sample when not including the difference-in-difference measure, results not shown in *Table 5*), it appears that peer effects generally have a positive effect on non-students. Finally, the results in *Table 5* show that the treatment effect of *infrequent* reporting is different for non-students who report significantly less when reporting frequency is reduced. Hence, in summary, non-student participants make similar decisions to student participants; however, they do not always react the same for the different treatments (compared to students). That is, their reaction to "soft" interventions is weaker, such as *moral suasion* and *assistance*. Yet, they react similarly to a stick approach, at least as measured by their reaction to the clamp down treatment. Finally, the effects of infrequent reporting (which was not a recommended intervention based on findings from student participants) show that non-student participants report significantly less under this condition.

Table 4a: Tax compliance for those sellers who used cash transactions (Student only)

Dep. Var. = Tax Compliance Ratio	**G**	**H**	**I**	**J**	**K**	**L**
treatments						
ATO Assist	0.08	0.09	0.12	0.12	0.14	0.17
	(0.55)	(0.58)	(0.77)	(0.72)	(0.82)	(1.03)
Clamp Down	0.39***	0.38***	0.37***	0.43***	0.42***	0.42***
	(2.81)	(2.61)	(2.66)	(2.80)	(2.70)	(2.78)
Infrequent	-0.02	-0.03	-0.06	0.01	0.01	-0.01
	(-0.17)	(-0.25)	(-0.44)	(0.08)	(0.09)	(-0.04)
Moral Suasion	0.18	0.17	0.21	0.21	0.22	0.26*
	(1.24)	(1.17)	(1.47)	(1.36)	(1.38)	(1.69)
Peer Effects	0.08	0.09	0.06	0.11	0.14	0.11

Seller	(0.52)	(0.60)	(0.39)	(0.69)	(0.84)	(0.69)
Audit Remorse	0.16	0.14	0.16	0.19	0.19	0.21
	(0.94)	(0.87)	(1.01)	(1.07)	(1.07)	(1.23)
Endogenous	0.48***	0.45***	0.45***	0.52***	0.49***	0.50***
Audit	(2.93)	(2.66)	(2.73)	(2.93)	(2.73)	(2.84)
Infrequent Pre-	0.22	0.20	0.19	0.26	0.24	0.24
Filled	(1.33)	(1.17)	(1.13)	(1.44)	(1.34)	(1.33)
Positive	0.13	0.13	0.08	0.17	0.18	0.14
Feedback	(0.78)	(0.78)	(0.50)	(0.92)	(1.00)	(0.77)
Further controls						
male		-0.12	-0.12		-0.12	-0.12
		(-1.58)	(-1.61)		(-1.44)	(-1.47)
age		0.01	0.01		0.01	0.01
		(0.85)	(0.73)		(0.91)	(0.80)
control for			-0.01***			-0.01***
price offered			(-3.12)			(-3.00)
Constant	0.62***	0.68***	1.12***	0.59***	0.61***	1.05***
	(8.23)	(3.14)	(4.42)	(6.29)	(2.62)	(3.91)
N	705	698	698	654	647	647
Prob. > chi^2	0.059	0.053	0.002	0.070	0.067	0.004

Notes: Multilevel random-effects regressions of determinants of the tax compliance ratio. Standard errors are clustered by individual and session. Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance. J, K, L exclude peer effects buyer completely.

Table 4b: Tax compliance for those sellers who used cash transactions (Non-student only)

Dep. Var. = Tax Compliance Ratio	**G**	**H**	**I**
treatments			
ATO Assist	-0.13	-0.15	-0.09
	(-1.07)	(-1.17)	(-0.74)
Clamp Down	0.05	0.06	0.07
	(0.37)	(0.47)	(0.53)
Infrequent	-0.40***	-0.40***	-0.33**
	(-3.05)	(-3.03)	(-2.53)
Moral Suasion	-0.08	-0.07	-0.01
	(-0.73)	(-0.60)	(-0.09)
Peer Effects Seller	0.23*	0.22	0.26*
	(1.67)	(1.58)	(1.88)
Further controls			
male		0.03	0.03
		(0.16)	(0.19)
age		-0.01	-0.01
		(-1.19)	(-0.84)
control for price offered			-0.01**
			(-2.38)
Constant	0.94***	1.21**	1.58***
	(13.40)	(2.55)	(3.21)
N	394	394	394

Prob. > chi^2	0.014	0.026	0.005

Notes: Multilevel random-effects regressions of determinants of the tax compliance ratio. Standard errors are clustered by individual and session. Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance.

Finally, we also explored variations in the declared amount (see Figures A19a and A19b) and the tax compliance ratio (see Figures A20a and A20b) to explore which treatments smooth the level of tax collection over time (disregarding the infrequent treatments). Interestingly, for students, the endogenous audits have the largest standard deviation and therefore the largest variation. Thus, contrary to Alm, Cronshaw and McKee (1993) endogenous audits do not seem to reduce the variation in individual compliance rates. The lowest variation on compliance is observed for moral suasion, a result that is robust when controlling for additional factors (such as the compliance ratio or the number of cash transactions) in a multivariate analysis (see Table A2a). With respect to the variation on the declared amount, audit remorse and clamp down report the lowest levels of variation. For non-students, the ATO assist and moral suasion report the lowest variation both for declared amounts and tax compliance (see Figures 19b and 21a) – a result that is also robust in the multivariate analysis (see Table A2b).

Table 5: Difference-in-difference approach

Dep. Var. = Declared amount	*ATO Assist*	*Clamp Down*	*Infrequent*	*Moral Suasion*	*Peer Effects Seller*
Non-student	16.41***	16.62***	15.82***	15.93***	16.52***
	(4.27)	(4.16)	(4.00)	(3.93)	(4.27)
Treatment	15.26***	13.48***	6.30	14.99***	6.28
	(3.13)	(2.73)	(1.39)	(3.02)	(1.25)
Non-student #	-14.66***	-7.64	-12.47**	-12.12**	0.99
Treatment	(-2.67)	(-1.31)	(-2.30)	(-2.12)	(0.17)
Further controls					
male	-5.13	-5.22	-4.55	-4.16	-5.35
	(-1.45)	(-1.43)	(-1.31)	(-1.14)	(-1.48)
age	0.24	0.28	0.36	0.37	0.29
	(0.87)	(1.03)	(1.28)	(1.27)	(1.03)
Constant	41.95***	41.14***	38.49***	37.54***	41.15***
	(4.74)	(4.48)	(4.34)	(4.01)	(4.60)
N	761	791	817	794	768
Prob. > chi^2	0.001	0.000	0.001	0.001	0.000

Notes: Standard errors are clustered by individual and session. Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance.

Conclusions

This is one of the first laboratory experiments to investigate the implications of different policies on income declaration in an environment where cash transactions are possible. Thus, the results contribute to the literature on the shadow economy that mostly relies on macro data to derive policy implications. Most of the studies on tax compliance have focused on personal income tax, and the idea of introducing a laboratory study to the business setting has received very little attention. This is surprising in light of the economic importance of the business sector and the high rates of illicit work or frequent use of cash transactions in the building, renovating, and repairing sector. The study also provides insights as to how different policies work in an environment where we still do not have enough insights as to the robustness of such instruments. A similar conclusion is true for the use of non-student participants in tax compliance experiments, particularly for participants who are likely to make decisions regarding the cash economy versus tax compliance on a daily basis in their job.

Contrary to the results of several field experiments (see previous discussion) we observe that *moral suasion* is a very powerful tool in increasing tax revenues. Moreover, we observe that providing *assistance* is also crucial for maintaining a high level of compliance and cooperation. This can be seen as evidence that perceived procedural fairness is a key factor in guaranteeing sustainable compliance. However, these findings are mediated by the observation that such effects cannot be observed for non-student participants.

The results in the *clamp down* and *endogenous audit* treatments also indicate that deterrence matters. The *endogenous audit* treatment has the strongest effect on declarations. However, from a comparative point of view, *assistance* and *moral suasion* also seem to have a stronger effect on tax revenues than the *clamp down* treatment. Thus, taxpayers are not only driven by extrinsic motivation. It is important to maintain a high level of intrinsic motivation, which can be encouraged with moral appeals and assistance. However, for those who are inclined to use cash transactions, deterrence seemed to have the strongest influence on tax compliance. Our results also indicate the importance of intrinsic and extrinsic reasons for compliance. One driver of differences between student and non-student participants could be that students do not face evasion opportunities on a regular basis and can therefore maintain a higher level of intrinsic motivation when responding to *moral suasion* and *assistance*.

In summary, our results may permit some recommendations from a policy perspective. The first is that deterrence works; that is, judging by the effect size and the consistent results across participant groups, *clamp down* and *endogenous audit* were two of the approaches that indicated the safest potential for increasing compliance. Second, on the reverse side, decreasing the frequency of declarations appeared to lower compliance; this, in turn, suggests that establishing higher frequencies of reporting may increase declared amounts. Thirdly, although results were significantly less strong for non-student participants, policies addressing the intrinsic motivation to comply should not be disregarded. Several previous studies have shown that "soft" measures can have positive effects, and have the advantage of simplicity in implementation. Therefore, we would suggest these treatments: clamp down, endogenous audit, moral suasion, assistance and the reduction of reporting frequencies as the next step. It would be important to first design a randomised controlled trial in the community to assess the (cost-)effectiveness of such approaches in the real world.

Acknowledgement: We acknowledge financial support and feedback provided by the Australian Tax Office (ATO). We are also thankful for the feedback provided by participants of the TTPI Conference on Behavioural Economics and Public Policy, ANU, August 2018; and the XXXVIII Tax Day MPI Munich, May, 2019, Max Planck Institute for Tax Law and Public Finance.

Compliance with Ethical Standards

Conflict of interest: The authors declare they have no known conflicts of interest.

Ethical Approval: All procedures performed in the experiments were in accordance with the ethical standards of the Queensland University of Technology (ethics clearance numbers: 1300000142; 1600000918).

Informed Consent: All participants provided informed consent prior to completing the experiment.

References

Alm, J. (1999). Tax compliance and administration. In W.B. Hildreth, J.A. Richardson (Eds.) *Handbook on taxation* (pp. 741-768). New York: Marcel Dekker.

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

Alm, J., Cronshaw, M. B., & McKee, M. (1993). Tax compliance with endogenous audit selection rules, *Kyklos, 1*, 27-45.

Alm, J., Martinez-Vazquez, J., & Torgler, B. (eds.) (2010). *Developing alternative frameworks for explaining tax compliance*. London: Routledge.

Alm, J., McClelland, G. H., & Schulze, W. D. (1992). Why do people pay taxes? *Journal of Public Economics, 48,* 21-48.

Andreoni, J., Erard, B., & Feinstein, J. (1998). Tax compliance, *Journal of Economic Literature*, *36,* 818-860.

Ayres, I., & Braithwaite, J. (1992). *Responsive regulation: Transcending the deregulation debate*. Oxford: Oxford University Press.

Batrancea, L., Nichita, A., Olsen, J., Kogler, C., Kirchler, E., Hoelzl, E., ... & Schaffner, M. (2019). Trust and power as determinants of tax compliance across 44 nations. *Journal of Economic Psychology, 74*, 102191.

Bikhchandani, S., Hirshleifer, D., & Welch, I. (1998). Learning from the behavior of others: Conformity, fads, and informational cascades. *Journal of Economic Perspectives, 12*, 151-170.

Blackwell, C. (2010). A meta-analysis of incentive effects in tax compliance experiments. In J. Alm, J. Martinez-Vazquez, and B. Torgler (Eds.) *Developing alternative frameworks explaining tax compliance* (pp. 97-112). London: Routledge.

Blumenthal, M., Christian, C., & Slemrod, J. (2001) Do normative appeals affect tax compliance? Evidence from a controlled experiment in Minnesota. *National Tax Journal*, *54*, 125-138.

Boulding, K. E. (1981). *A preface to grant economics: The economy of love and fear*. New York: Praeger.

Cialdini, R. B. (2007). *Influence: The psychology of persuasion*. New York: Harper Collins.

Cordes, J., & Holen, A. (2010). Should the government prepare individual income tax returns?, Technology Policy Institute, Washington.

Dong, B., Dulleck, U., & Torgler, B. (2012). Conditional corruption, *Journal of Economic Psychology*, *33*, 609-627.

Dulleck, U., Fooken, J., Newton, C., Ristl, A., Schaffner, M., & Torgler, B. (2016). Tax compliance and psychic costs: Behavioral experimental evidence using a physiological marker, *Journal of Public Economics*, *134*, 9-18.

Erard, B. (1993). Taxation with representation: An analysis of the role of tax practitioners in tax compliance, *Journal of Public Economics*, *52*, 163-197.

Feld, L. P., Frey, B. S., & Torgler, B. (2006). Rewarding honest taxpayers. In H. Elffers, P. Verboon and W. Huisman (Eds.) *Managing and maintaining compliance* (pp. 45-61). The Hague: Boom Legal Publishers.

Fisher, R. C., Goddeeris, J. H., & Young, J. C. (1989). Participation in amnesties: The individual income tax, *National Tax Journal, 42*, 15-27.

Fonseca, M., & Grimshaw, S. (2015). Does using behavioural prompts in pre-populated tax forms affect compliance? Results from an artefactual field experiment with real taxpayers, Tax Administration Research Centre, Discussion Paper: 015-15.

Frey, B. S., & Torgler, B. (2007). Tax morale and conditional cooperation, *Journal of Comparative Economics, 35*, 136-159.

Kahan, D. M. (1998). Social meaning and the economic analysis of crime. *Journal of Legal Studies*, *27*, 609-622.

Kirchler, E. (2007). *The economic psychology of tax behaviour*. Cambridge: Cambridge University Press.

Kirchler, E., Hoelzl, E., & Wahl, I. (2008). Enforced versus voluntary tax compliance: The ‘‘slippery slope” framework, *Journal of Economic Psychology, 29*, 210–225.

Kotakorpi, K., & Laamanen, J.-P. (2015). Complexity, salience and income tax reporting behaviour: Evidence from a natural experiment, University of Turku, mimeo.

Mazar, N., Amir, O., & Ariely, D. (2008). The dishonesty of honest people: A theory of self-concept maintenance, *Journal of Marketing Research*, *XLV*, 633-644.

Nuttin, J., & Greenwald, A. G. (1968). *Reward and punishment in human learning*. New York: Academic Press.

Ostrom, E. (2005). Policies that crowd out reciprocity and collective action. In H. Gintis, S. Bowles, R. Boyd, and E. Fehr (Eds.) *Moral sentiments and material interests* (pp. 253-275). *Cambridge*, Massachusetts: MIT Press.

Roth, J. A., Scholz, J. T., & Witte, A. D. (eds.) (1989). *Taxpayer compliance, Vol. 1 and Vol. 2.* Philadelphia: University of Pennsylvania Press.

Schneider, F., & Enste, D. H. (2002). *The shadow economy: An international survey*. Cambridge: Cambridge University Press.

Skinner, B. F. (1938). *The behavior of organism*. New York: Academic Press.

Slemrod, J., Blumenthal, M., & Christian, C. (2001) Taxpayer response to an increased probability of audit: Evidence from a controlled experiment in Minnesota. *Journal of Public Economics*, *79*, 455-483.

Smith, K. W. (1992). Reciprocity and fairness: Positive incentives for tax compliance. In J. Slemrod (Ed.) *Why people pay taxes: Tax compliance and enforcement* (pp. 223-258). Ann Arbor: University of Michigan Press.

Sparrow, M. (2000). The regulatory craft: Controlling risks, solving problems, and managing compliance. Washington, DC: Brookings Institution Press.

Thorndike, E. L. (1911). *Animal intelligence*. New York: Macmillan.

Thorndike, E. L. (1932). *The fundamentals of learning*. New York: Teachers College, Columbia University.

Torgler, B. (2002). Speaking to theorists and searching for facts: Tax morale and tax compliance in experiments, *Journal of Economic Surveys, 16*, 657-684.

Torgler, B. (2004a). Moral suasion: An alternative tax policy strategy? Evidence from a controlled field experiment in Switzerland, *Economics of Governance*, *5*, 235-253.

Torgler, B. (2007). *Tax compliance and tax morale: A theoretical and empirical analysis*. Cheltenham, UK: Edward Elgar.

Torgler, B. (2013). A field experiment in moral suasion and tax compliance focusing on underdeclaration and overdeduction, *FinanzArchiv: Public Finance Analysis*, *69*, 393-411.

Torgler, B. (2016). Tax compliance and data: What is available and what is needed, *Australian Economic Review, 49*, 352-364.

Torgler, B., & Murphy, K. (2004). Tax morale in Australia: What factors shape it and has it changed over time?, *Journal of Australian Taxation*, *7,* 298-335.

Torgler, B., & Schneider, F. (2009). The impact of tax morale and institutional quality on the shadow economy, *Journal of Economic Psychology*, *30*, 228-245.

Torgler, B., Frey, B. S., & Wilson, C. 2009. Environmental and pro-social norms: Evidence on littering. *B. E. Journal of Economic Analysis & Policy, 9*(1), Article 18.

Torgler, B., Demir, I. C., Macintyre, A., & Schaffner, M. (2008). Causes and consequences of tax morale: An empirical investigation, *Economic Analysis & Policy*, *38,* 313-339.

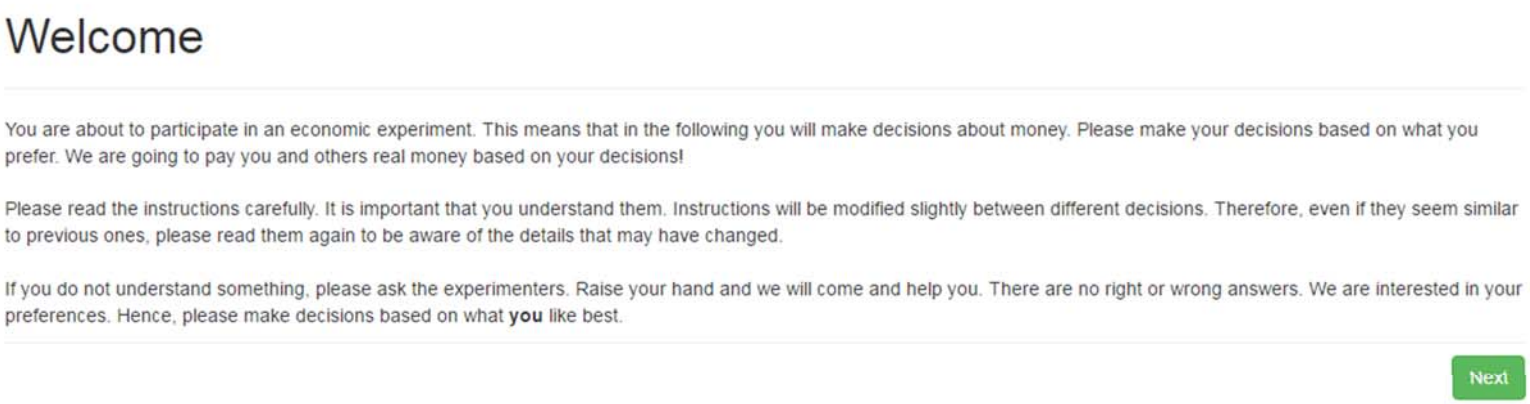
Webley, P., Robben, H., Elffers, H., & Hessing, D. (1991). *Tax evasion: An experimental approach*. Cambridge: Cambridge University Press.

Appendix

Screenshots

The following pages provide a full set of screenshots for one experimental condition (treatment) of conducted experiments.

Figure A1: Welcome screen



Welcome

You are about to participate in an economic experiment. This means that in the following you will make decisions about money. Please make your decisions based on what you prefer. We are going to pay you and others real money based on your decisions!

Please read the instructions carefully. It is important that you understand them. Instructions will be modified slightly between different decisions. Therefore, even if they seem similar to previous ones, please read them again to be aware of the details that may have changed.

If you do not understand something, please ask the experimenters. Raise your hand and we will come and help you. There are no right or wrong answers. We are interested in your preferences. Hence, please make decisions based on what **you** like best.

Next

Figure A2: Experimental payment

Welcome

Experimental Payments

Throughout the experiment we talk about money in experimental dollars. This is done to simplify decisions. Experimental dollars directly exchange into Australian dollars at the following rate:

1 experimental dollar = 0.50 Australian dollar.

Two randomly selected sets of rounds will be chosen for your final payment.

We use this random selection so you consider every round as individual, as you cannot secure some money in the first periods and gamble a bit in later periods (or vice versa). You will not know which periods will determine the payment, only that two will be selected.

Next

Figure A3: Structure of the experimental interaction

Welcome

Two roles and the scenario

Business and Consumers

In the course of the experiment you will interact with other participants.

Interactions take place between the service provider and consumer, where the consumer requires some service and the service provider can deliver it. You will take on both roles as a participant.

To understand the interaction between service provider and consumers, consider the consumer to be a house owner who needs a job to be done on the house which they cannot do themselves. In real life this could be the repair of your swimming pool, of your hot water system or the refurbishment of a fence around your house. Whilst theses jobs may not need to be fixed straight away, there is a greater cost of ignoring the problem.

The service provider in turn is someone who can do the job, such as a pool repair person, a plumber or someone specialized in fixing fences. Within the experiment consumers and providers will interact as follows:

- The consumer originally has 100 experimental dollars at the beginning of each round, but something breaks down and if they do not get it fixed, they lose 80 experimental dollars.
- The consumer goes to a service provider. The service provider can make a price offer to fix the problem.
- If the consumer accepts the offer they do not incur the loss, but has to pay the service provider. If the service provider does not accept, the consumer loses the 80 (hence, keeping 20) and the service provider will get no income from this round.

This is the basic framework of the experiment. You will go through this interaction for several rounds. You can think about it as such that a house owner needs a number of maintenance jobs done throughout the year.

Next

Figure A4: Cash offer

Welcome

Cash Offers and taxes

However the interaction between consumers and service providers is not so simple and can include the following:

- One is that service providers have to pay taxes on their income (i.e., the experimental payments they receive from consumers). More precisely, service providers have to pay 40% of their declared income in taxes.
- Furthermore, service providers can offer a formal or an informal transaction to the customer to get the job done. Formal is an invoice to be paid through the bank whereas informal is a cash payment. If they offer the informal cash option, the final price that the customer has to pay will be 10% cheaper (discount for paying cash).
- When receiving the offer the customer can, upon acceptance, indicate if they would like to take up the cash offer in case it is available. The customer will be asked about this option independent of whether the service provider offers cash payment. The cash option will only be realized if both the customer and service provider opted for cash.
- For the service provider all income based on formal (non-cash) offers will automatically be declared and taxes have to be paid on the respective amount. For cash transactions, however, service providers are required to declare the amount themselves.
- As the cash transaction opens the possibility to declare less than the received amount, there will be random tax audits. There is a 10% chance service providers will be audited. If they are audited and an amount declared is lower that the correct amount, they have to pay the amount they underpaid in taxes plus a fine which corresponds to this underpaid amount.

Finally, please note that the money collected by taxes will not be lost. We (the experimenters) will collect this money and give it to a charitable university initiative (QUT FOODBANK).

Next

Figure A5: Roles

Welcome

Your role in two parts

Your role:

In the following rounds you will take either the role of the service provider or the consumer. You will then interact with different counterparts for 2 sets of 6 rounds (i.e., for a total of 12 rounds). In each round you will interact with a different participant in the laboratory. You can think of it as hiring different service providers for different jobs that need to be done in your house. After 6 rounds your role will change and you will take the other role. Hence if you had the role of a service provider in the first 6 rounds, you will be a customer in the second 6 rounds. If you were a customer first, you will become a service provider.

The payment rounds:

As mentioned before, you will be paid based on your decisions, however, you will only be paid for 2 randomly selected rounds. More precisely, you will be paid once as a service provider and once as a customer.

Further instructions:

The basic structure of the interaction between service providers and customers will be as described in the current sections. However, there may be additional details which may be added in the course of the experiment. Therefore, when additional information is included, please read through it carefully before moving on to your next decision.

Next

Figure A6.1: Assistance Treatment

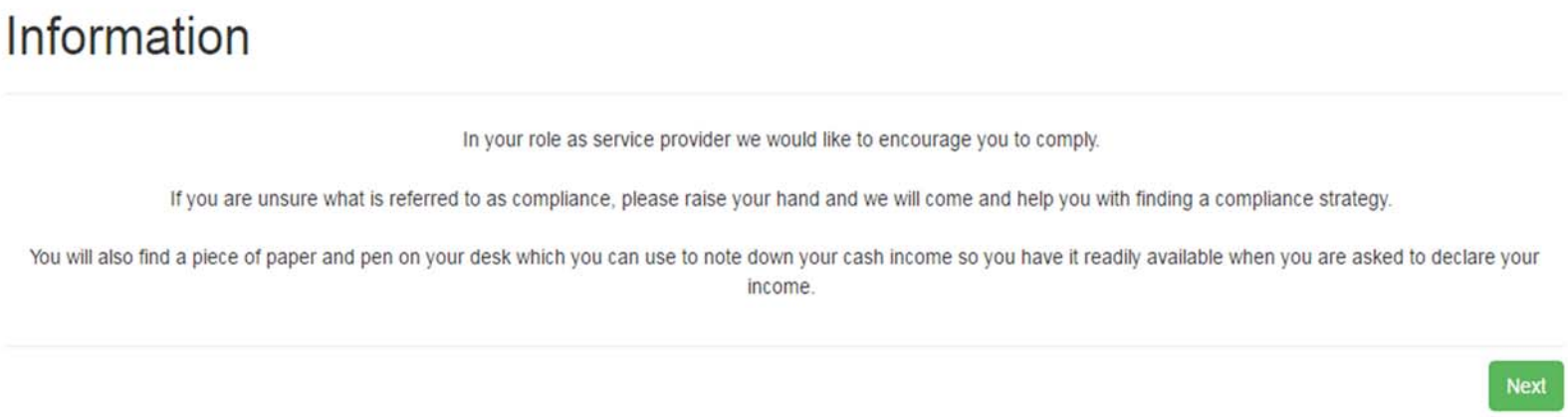


Figure A6.2: Clamp Down Treatment

Information

We would like to inform you that in the next three rounds there is a special focus on auditing the service providers.

As a result, the probability of audits will be two times as high as in the other sets of rounds.

Next

Figure A6.3: Moral Suasion

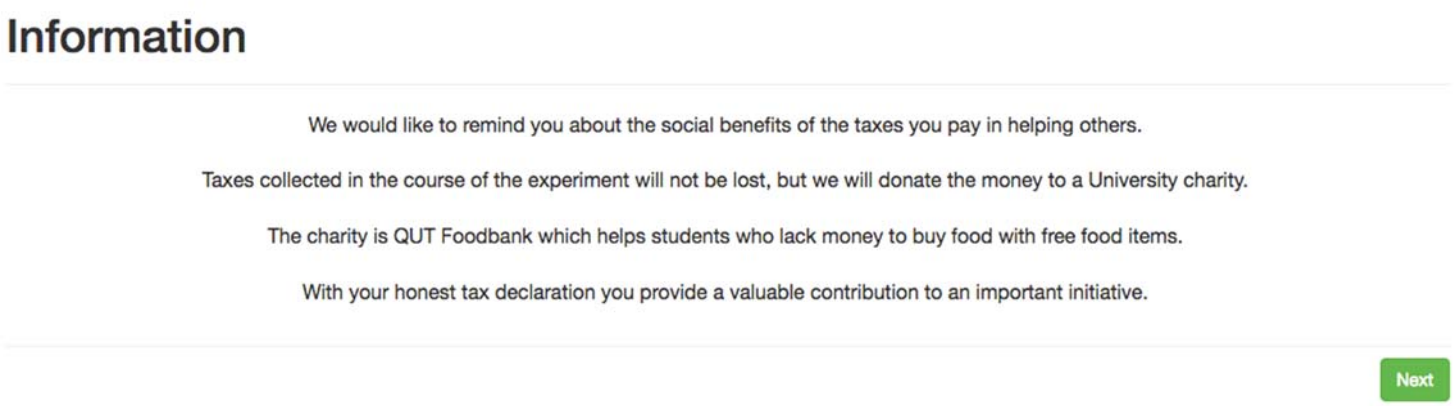


Figure A6.4: Buyer Peer Effect

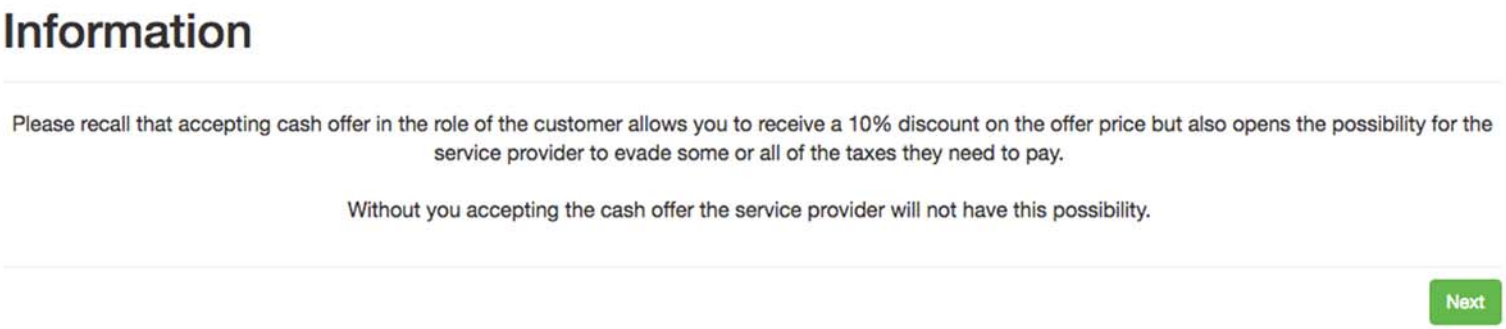


Figure A7.1: Seller's offer

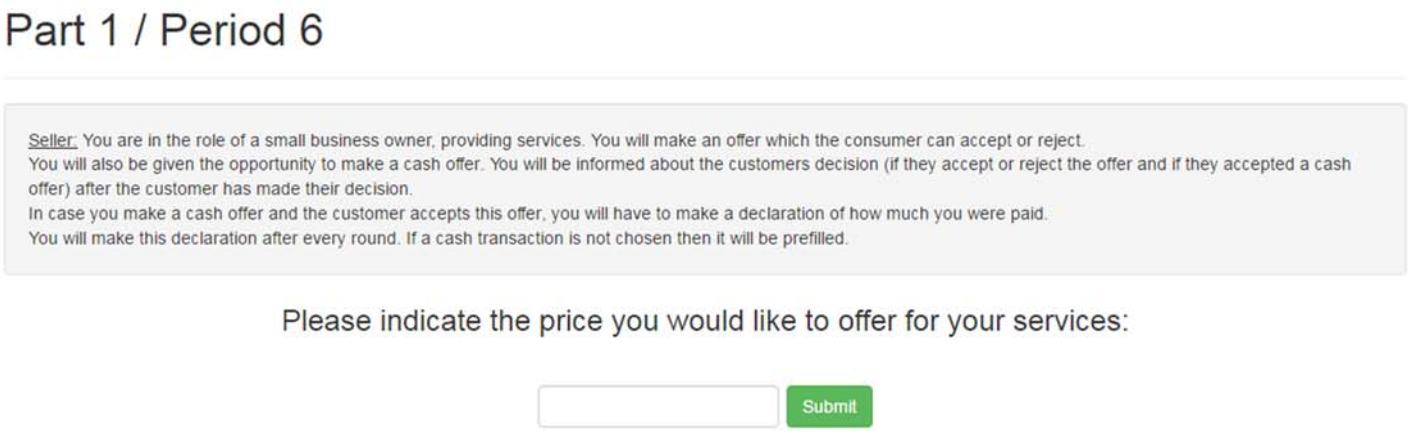


Figure A7.2: Seller's cash offer

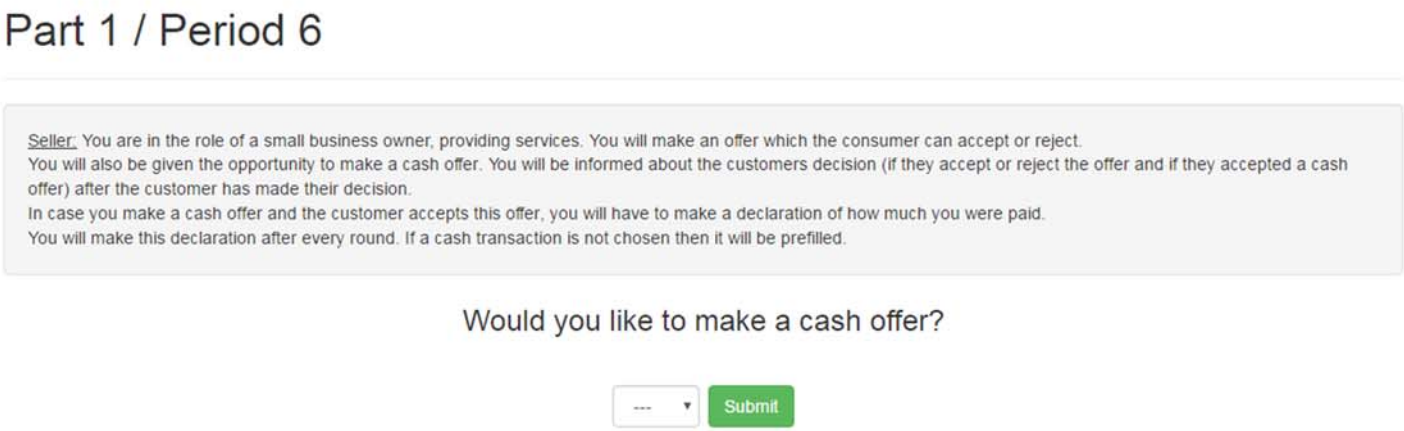


Figure A7.3: Seller Peer Effect Treatment

Part 1 / Period 6 - Tax Declaration

Seller: You are in the role of a small business owner, providing services.

Tax information:
Tax rate: 40%
**If you are selected for an audit, the actual and the declared income are compared. If you do not fully comply, a fine is aplpied.*

Your income tax declaration has been recorded.

Before you lodge your final declaration, please be informed that your reported income from cash transactions is **below** the industry average (based on previous experiments) for these transactions.

You may modify your declaration by clicking "Back" or "Submit" your current declaration amount.

Back Submit

Figure A8.1: Audit Remorse Treatment

Part 1 / Period 6 - Tax Declaration

Seller: You are in the role of a small business owner, providing services.

Tax information:
Tax rate: 40%
**If you are selected for an audit, the actual and the declared income are compared. If you do not fully comply, a fine is applied.*

Your income tax declaration has been recorded.

The amount declared appears to be too low.

Please note, if you are caught making a non-compliant declaration your penalty will tripple in amount, depending on audit probabilities.

You may modify your declaration by clicking "Back" or "Submit" your current declaration amount.

Back Submit

Figure A8.2: Endogenous Audit Treatment

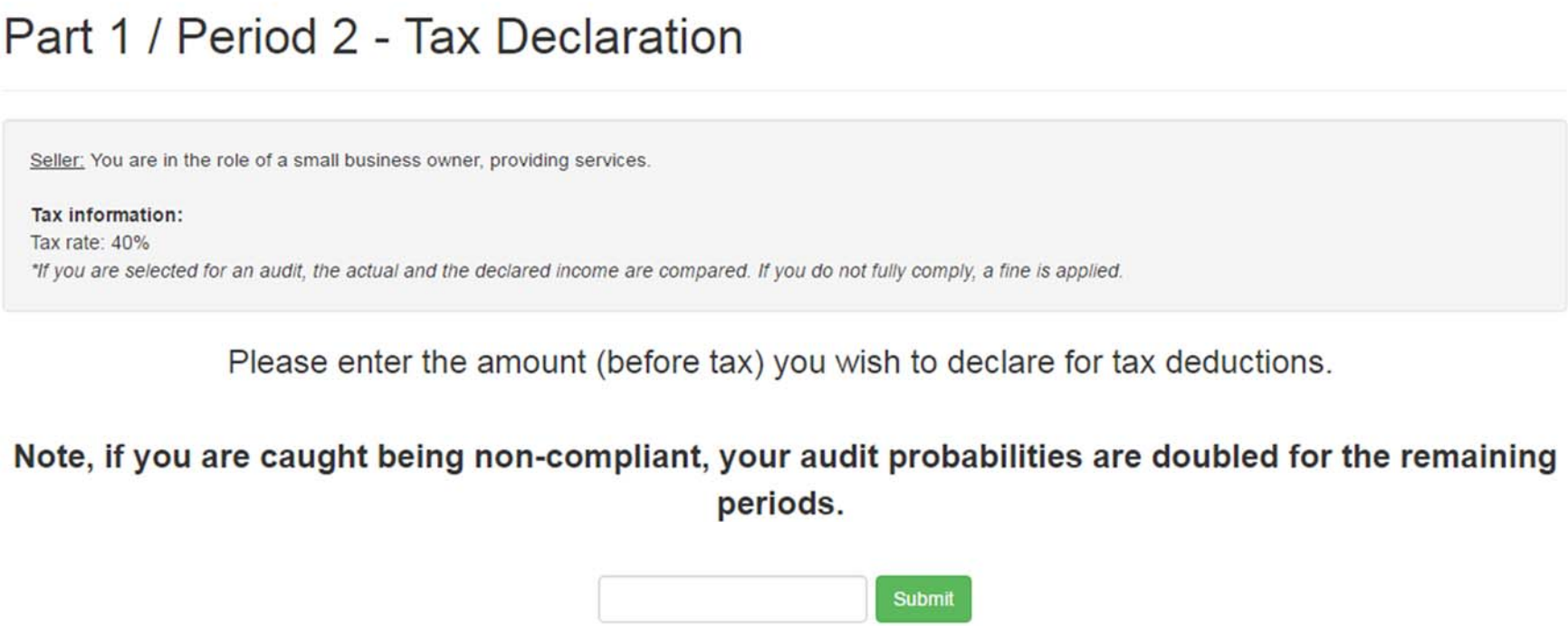


Figure A8.3a: Infrequent Pre-Filled Treatment

Part 2 / Period 3 - Tax Declaration

Seller: You are in the role of a small business owner, providing services.

Tax information:
Tax rate: 40%
**If you are selected for an audit, the actual and the declared income are compared. If you do not fully comply, a fine is applied.*

Please enter the amount (before tax) you wish to declare for tax deductions.
This declaration is for the past 3 periods.

Submit

To view your income for the past 3 periods please click on the button below. You will be charged **5 experimental dollars** for this service.

Show Income History

Figure A8.3b: Infrequent Pre-Filled Treatment

Part 2 / Period 3 - Tax Declaration

Seller: You are in the role of a small business owner, providing services.

Tax information:
Tax rate: 40%
**If you are selected for an audit, the actual and the declared income are compared. If you do not fully comply, a fine is applied.*

Please enter the amount (before tax) you wish to declare for tax deductions.
This declaration is for the past 3 periods.

Submit

Your income history:

Period 1	72
Period 2	76.5
Period 3	54

Figure A8.4: Positive Feedback Treatment

Part 1 / Period 1 - Tax Declaration

Seller: You are in the role of a small business owner, providing services.

Tax information:
Tax rate: 40%
**If you are selected for an audit, the actual and the declared income are compared. If you do not fully comply, a fine is applied.*

We have recorded that the non-cash transaction has been chosen and your income declared automatically.

Thank you for making an honest declaration.

Next

Figure A9: Tax Declaration

Part 2 / Period 6 - Tax Declaration

Seller: You are in the role of a small business owner, providing services.

Tax information:
Tax rate: 40%
**If you are selected for an audit, the actual and the declared income are compared. If you do not fully comply, a fine is applied.*

Please enter the amount (before tax) you wish to declare for tax deductions.

Submit

Figure A10: Auditing the seller

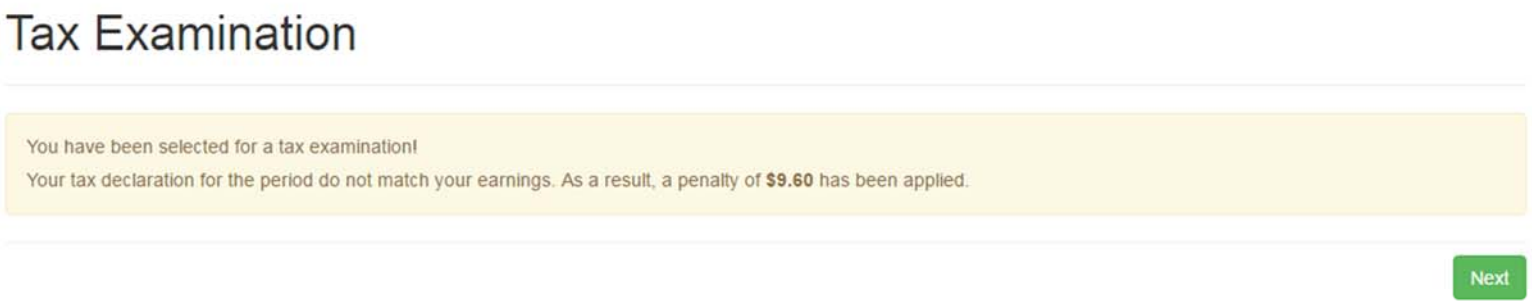


Figure A11: Buyer's offer acceptance screen

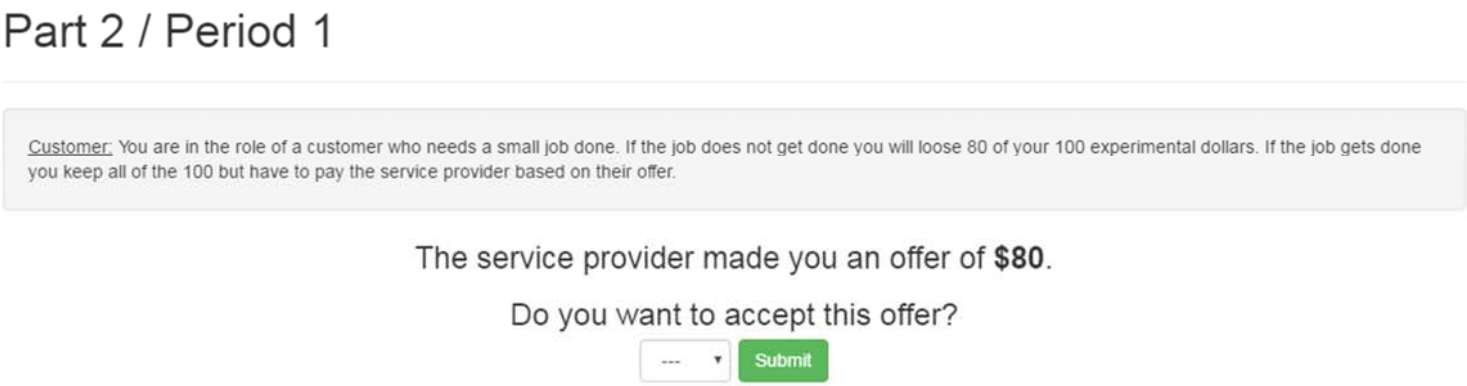
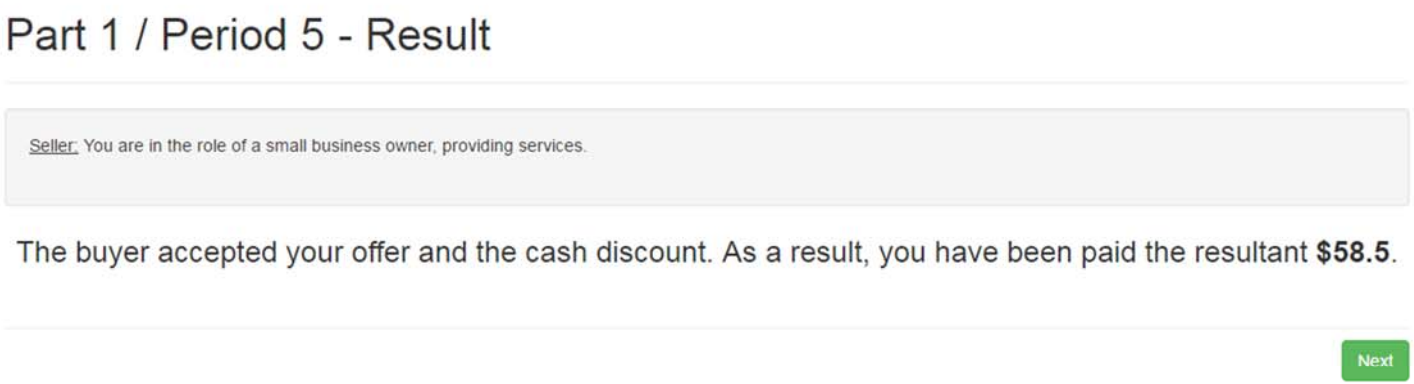


Figure A12: Buyer's response to a cash offer

Figure A13: Buyer's acceptance of a cash offer



Controlling for nationality of participants in the experiments

Given the relatively large proportion of non-domestic participants in the experiment, the following regression results demonstrate that the outcomes presented in the report are robust, if not stronger, when controlling for this characteristic.

Table A1: Multilevel random-effects regressions of determinants of the declared amount

Dep. Var. = Declared amount treatments	**M**	**N**	**O**	**P**	**R**	**S**
ATO Assist	10.48**	7.89	15.07***	11.94**	1.41	-0.40
	(2.02)	(1.60)	(2.70)	(2.25)	(0.42)	(-0.12)
Clamp Down	8.70*	10.66**	13.21**	14.59***	6.02*	4.39
	(1.74)	(2.24)	(2.44)	(2.85)	(1.66)	(1.29)
Infrequent	2.28	4.31	6.68	8.21*	-6.41*	-9.71***
	(0.49)	(0.98)	(1.32)	(1.72)	(-1.74)	(-2.78)
Moral Suasion	10.02**	7.05	14.64***	11.10**	2.76	-1.49
	(2.02)	(1.49)	(2.72)	(2.17)	(0.77)	(-0.43)
Peer Effects	0.80	4.65	5.49	8.62	7.00**	5.71*
Seller	(0.15)	(0.92)	(0.96)	(1.58)	(1.97)	(1.71)
Audit Remorse	8.11	6.53	12.53**	10.49*		
	(1.36)	(1.15)	(1.99)	(1.76)		
Endogenous Audit	16.47***	18.60***	20.85***	22.42***		
	(2.66)	(3.17)	(3.20)	(3.64)		
Infrequent Pre-	5.93	7.53	10.52*	11.44*		
Filled	(1.00)	(1.33)	(1.66)	(1.91)		
Positive Feedback	0.66	6.95	5.12	10.90*		
	(0.11)	(1.19)	(0.79)	(1.78)		
Further controls						
male	-5.08*	-4.99*	-5.35*	-5.17*	0.18	0.06
	(-1.88)	(-1.94)	(-1.89)	(-1.93)	(0.03)	(0.01)
age	0.50*	0.53**	0.51*	0.56**	0.25	-0.10
	(1.95)	(2.16)	(1.89)	(2.18)	(0.78)	(-0.31)
domestic	3.98	5.12*	3.38	5.05*	-7.04	-3.85
	(1.37)	(1.86)	(1.10)	(1.74)	(-1.39)	(-0.78)
control for price		0.64***		0.63***		0.67***
offered		(9.79)		(9.12)		(8.41)
Constant	38.69***	-2.82	34.58***	-6.71	53.66***	19.09
	(4.84)	(-0.32)	(4.06)	(-0.72)	(4.05)	(1.44)
N	1249	1249	1143	1143	810	810
Prob. > chi^2	0.021	0.000	0.007	0.000	0.093	0.000

Notes: Reference in M, and N: baseline + peer effects buyer treatment. O, P, R and S only baseline. R and S are non-student sample. Standard errors are clustered by individual and session. Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance.

Table A2a: Ordinary least squares regressions of determinants of the variation of compliance ratio (Student only)

Dep. Var. = Standard deviation of tax compliance ratio				
ATO Assist	0.02	0.06*	-0.0008	0.01
	(0.66)	(1.94)	(-0.01)	(0.18)
Clamp Down	0.03	0.05	-0.18**	-0.14*
	(0.41)	(0.65)	(-2.67)	(-2.08)
Moral Suasion	-0.10***	-0.08***	-0.12***	-0.11***
	(-6.92)	(-6.18)	(-4.58)	(-3.93)
Audit Remorse	0.03	0.01	-0.11	-0.09
	(0.59)	(0.16)	(-1.52)	(-0.91)
Endogenous Audit	0.27	0.27	-0.004	0.06
	(1.02)	(1.07)	(-0.04)	(0.79)
Peer Effects Seller	0.005	0.03	-0.04	-0.05
	(0.05)	(0.31)	(-0.44)	(-0.44)
Positive Feedback	0.06	0.07	-0.04	-0.05
	(0.87)	(0.87)	(-0.39)	(-0.46)
Further controls				
number of cash transaction periods		0.04	0.04*	0.04*
		(1.49)	(1.84)	(1.98)
mean compliance ratio			0.46***	0.46***
			(3.17)	(3.16)
male				0.03
				(0.93)
age				-0.01**
				(-2.48)
Constant	0.13***	-0.02	-0.27*	-0.07
	(8.90)	(-0.22)	(-1.84)	(-0.46)
N	137	137	137	136
R^2	0.055	0.070	0.542	0.567

Notes: Standard errors are clustered by session. Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance. Infrequent and peer effects buyer treatments are excluded.

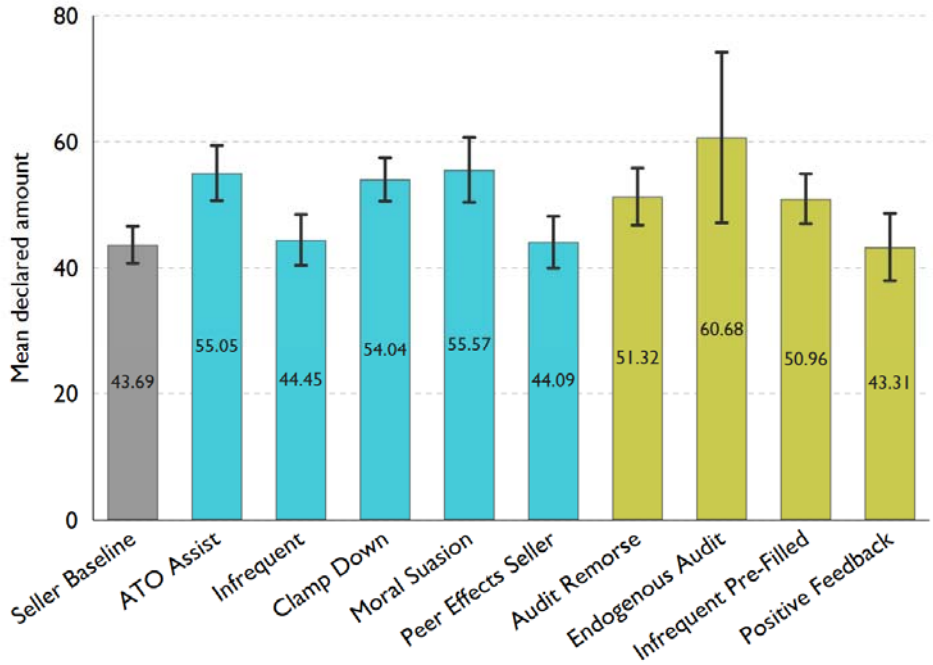
Table A2b: Ordinary least squares regressions of determinants of the variation of compliance ratio

(non-student only)

Dep. Var. = Standard deviation of tax compliance ratio				
ATO Assist	-0.19**	-0.19**	-0.16**	-0.19***
	(-3.74)	(-3.39)	(-4.44)	(-5.70)
Clamp Down	0.32***	0.32**	0.32***	0.36***
	(6.16)	(4.55)	(5.71)	(6.82)
Moral Suasion	-0.12*	-0.12	-0.08	-0.06
	(-2.26)	(-2.04)	(-1.93)	(-1.71)
Peer Effects Seller	0.29***	0.29**	0.04	0.03
	(5.71)	(3.86)	(0.26)	(0.21)
Further controls				
number of cash transaction periods		0.002	0.02	0.02
		(0.03)	(0.54)	(0.60)
mean compliance ratio			0.39	0.37
			(2.07)	(1.97)
male				-0.07
				(-2.07)
age				-0.02*
				(-2.64)
Constant	0.24***	0.24	-0.17	0.40
	(4.76)	(1.12)	(-0.70)	(0.95)
N	95	95	95	95
R^2	0.098	0.098	0.366	0.392

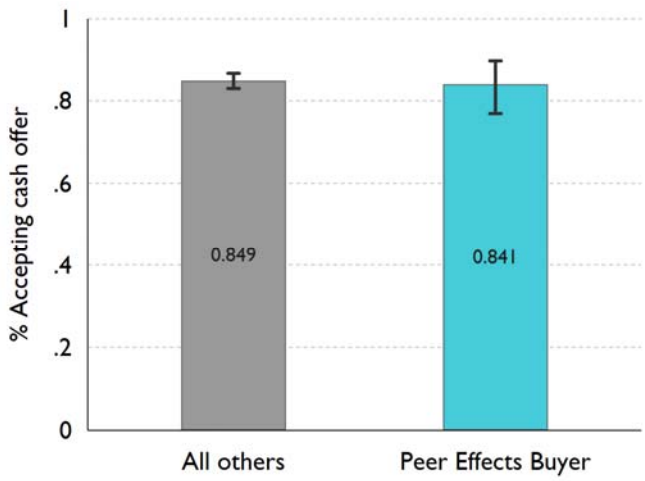
Notes: Standard errors are clustered by session. Significance is indicated as follows: * indicates 10% significance, ** 5% significance and *** 1% significance. Infrequent treatment is excluded.

Figure A14: Adding the baseline and the peer effects buyer into the control group (Student session)



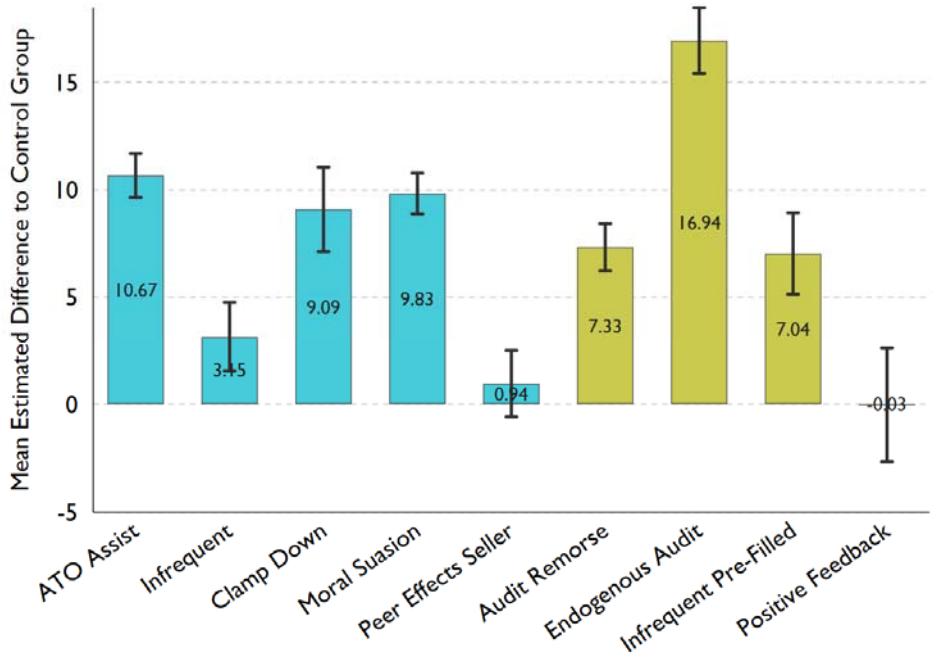
Note: Error bars represent 95% confidence intervals.

Figure A15: Acceptance of cash transfers



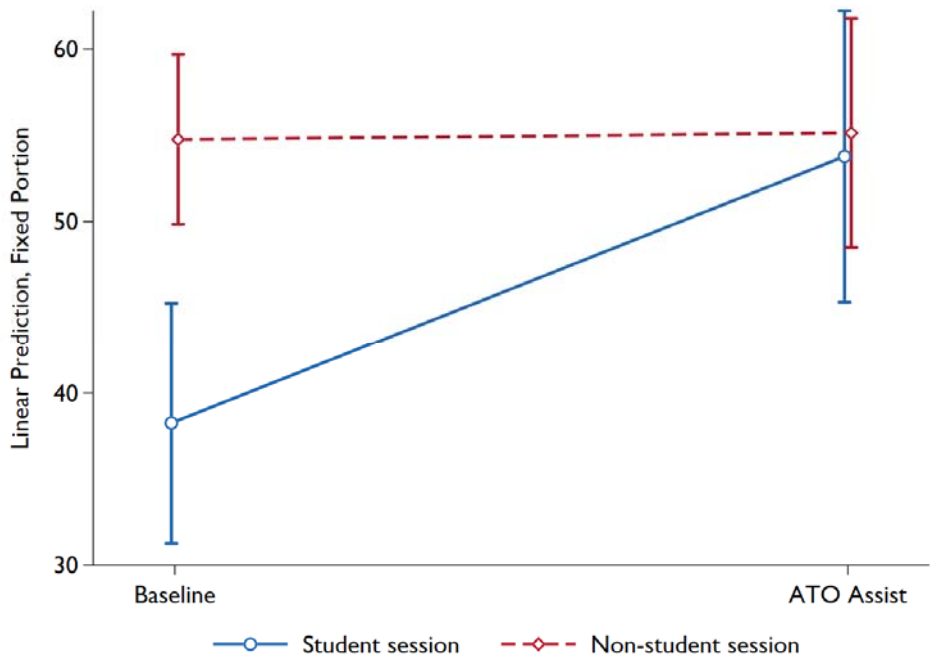
Notes: Difference is not statistically significant. All other treatments and the control group in the group labelled "*All others*". Error bars represent 95% confidence intervals of the mean proportion.

Figure A16: Estimated differences of declared amount to the joint control group (Student session)



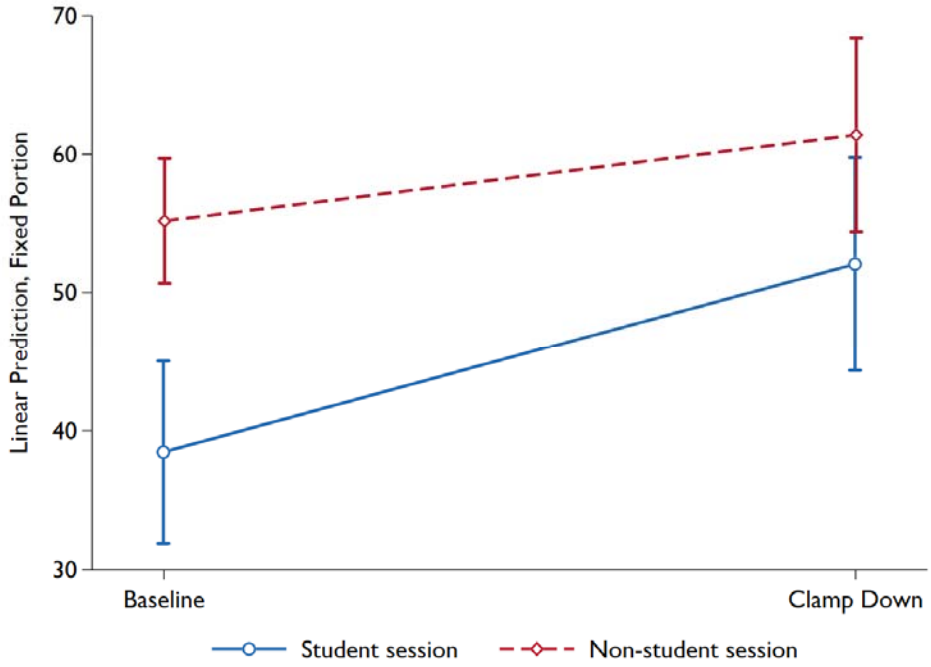
Note: Control group is equal to *Baseline* and *Peer Effect Buyer*. Error bars represent 95% confidence intervals.

Figure 17a: ATO assist



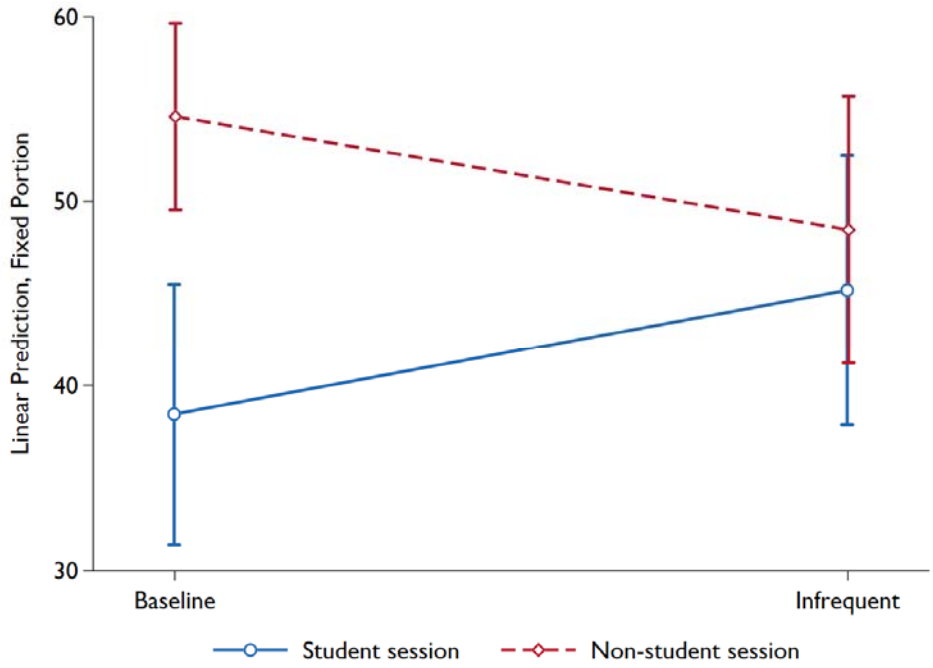
Note: Error bars represent 95% confidence intervals.

Figure 17b: Clamp down



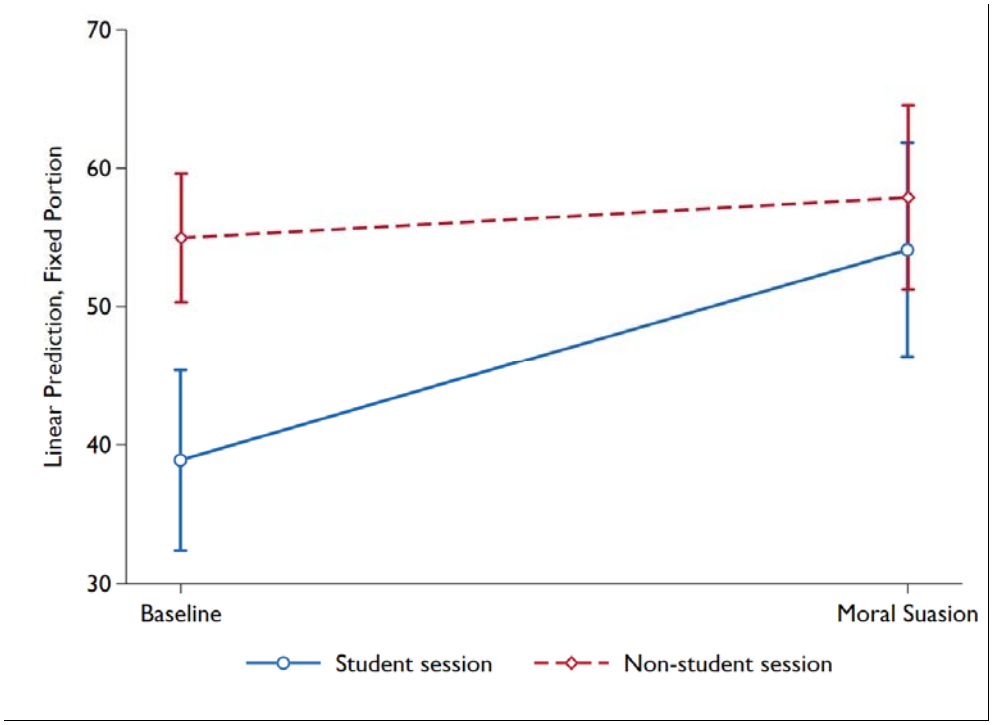
Note: Error bars represent 95% confidence intervals.

Figure 17c: Infrequent



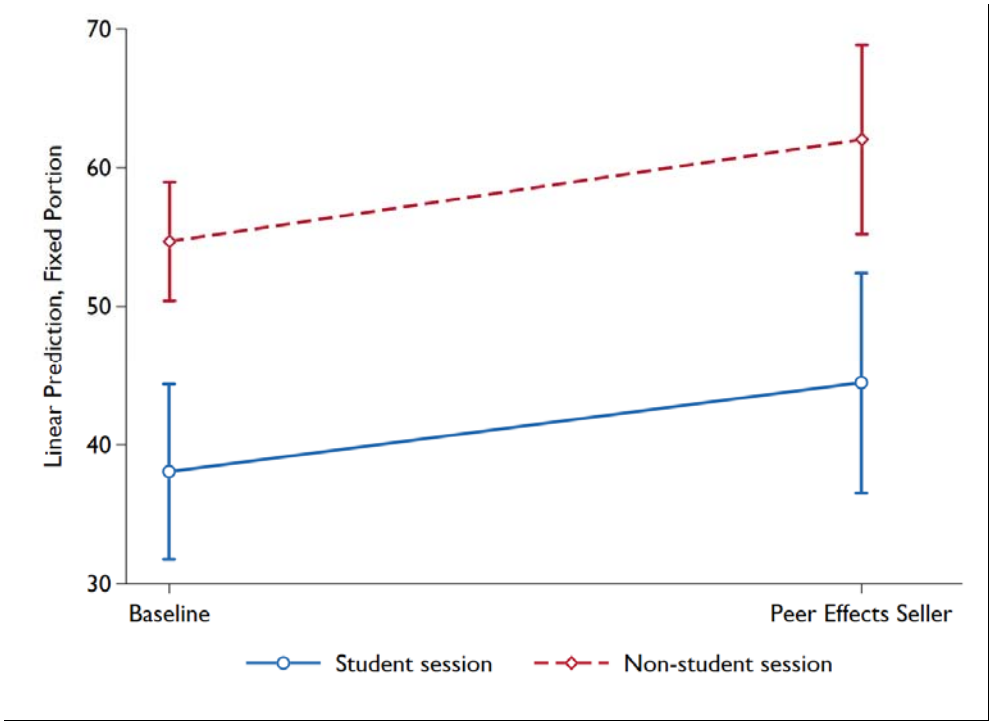
Note: Error bars represent 95% confidence intervals.

Figure 17d: Moral suasion



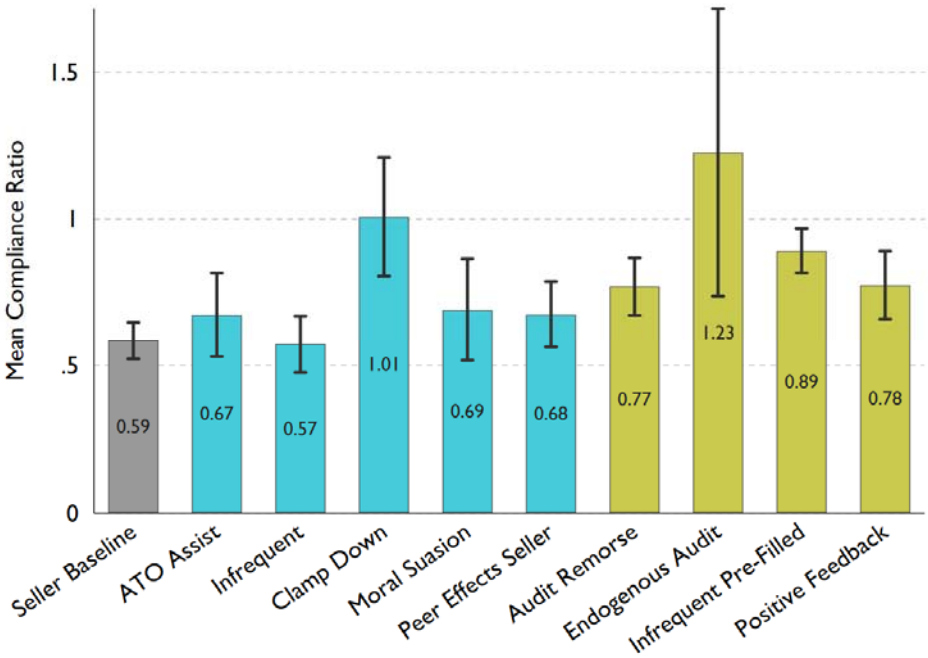
Note: Error bars represent 95% confidence intervals.

Figure 17e: Peer effects seller



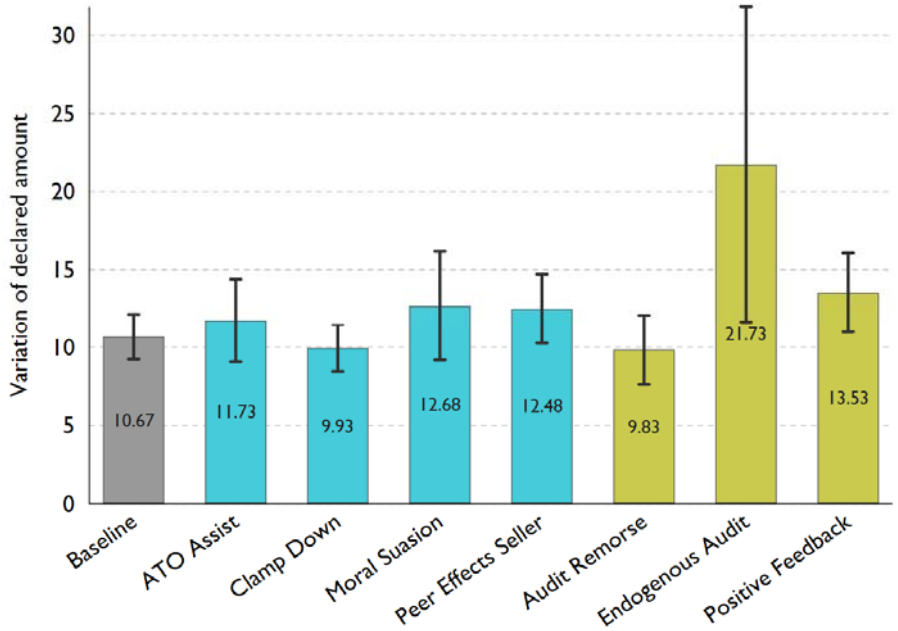
Note: Error bars represent 95% confidence intervals.

Figure A18: Tax compliance with the mixed reference group (Student session)



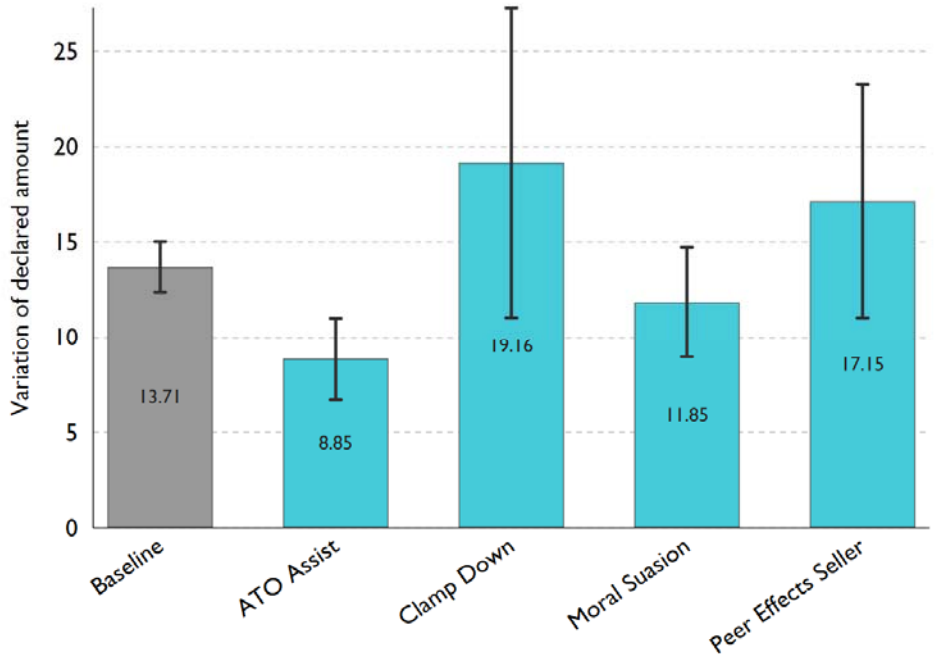
Note: Control group is equal to *Baseline* and *Peer Effect Buyer*. Error bars represent 95% confidence intervals.

Figure A19a: Variation of declared amount with the baseline (student session)



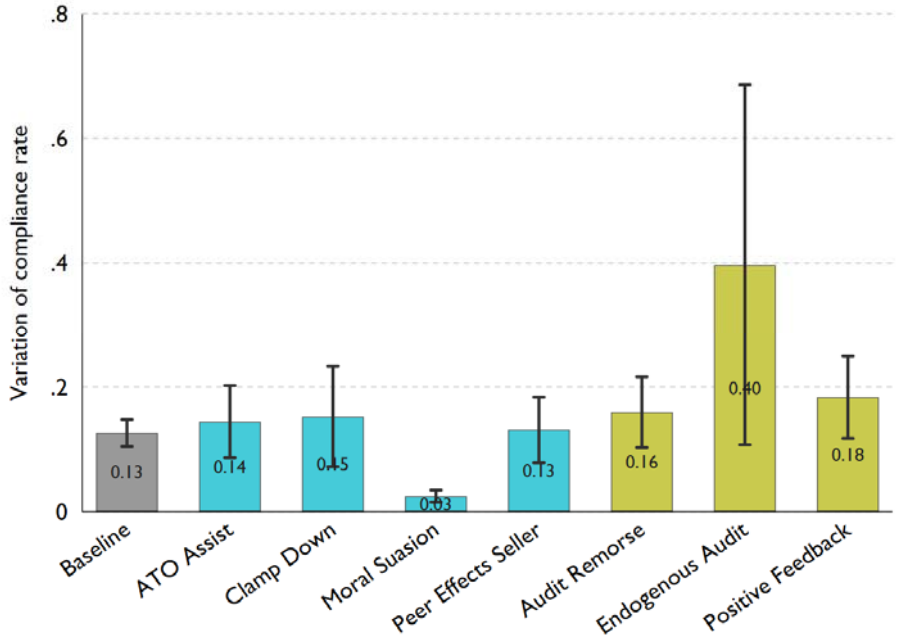
Notes: Variation of declared amount is calculated as the standard deviation of individual declared amount of all six rounds. Infrequent and peer effects buyer treatments are excluded. Error bars represent ±S.E. of mean.

Figure A19b: Variation of declared amount with the baseline (non-student session)



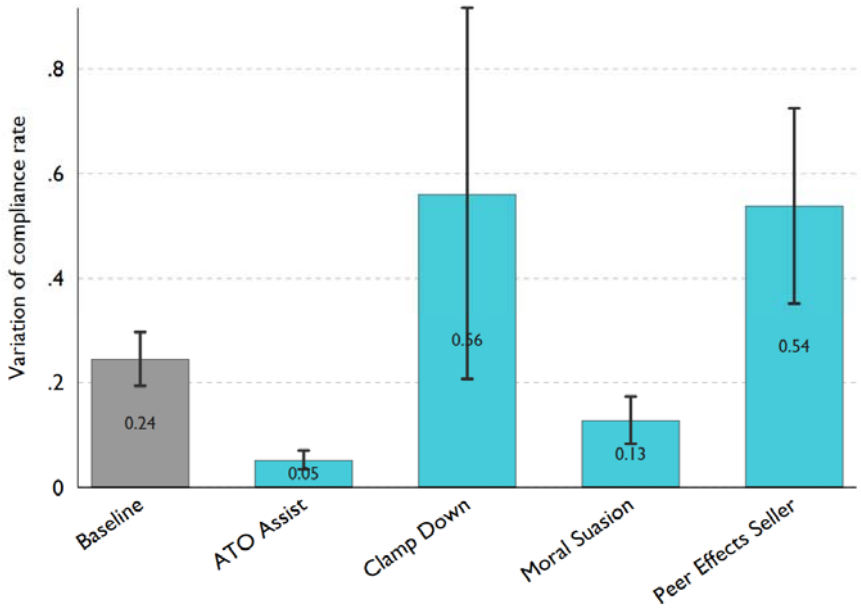
Notes: Error bars represent ±S.E. of mean.

Figure A20a: Variation of tax compliance ratio with the baseline (student session)



Notes: Variation of tax compliance ratio is calculated as the standard deviation of individual tax compliance ratio of all six rounds. Infrequent and peer effects buyer treatments are excluded. Error bars represent ±S.E. of mean.

Figure A20b: Variation of tax compliance ratio with the baseline (non-student session)



Notes: Error bars represent ±S.E. of mean.