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How rewards enhance truthful reporting: an experimental analysis

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Abstract

We assess experimentally the problem of untruthful reporting when an entity self-reports information to a regulatory authority. To enhance truthful reporting, an independent third party is involved to review the report for truthfulness. In addition, a representative of the regulatory authority may check the verified report. Most of the time, the reporting entity selects and pays the third party, which is exposed to competition with other third parties. This may lead to conflicts of interests and promote untruthful reporting. In the present study, we show that providing rewards for successful detection, and thus turning the third party or the regulatory authority into "bounty-hunters," can significantly enhance truthful reporting. In our setting, rewarding the third-party tends to be more cost-effective than rewarding the regulatory authority as the latter induces a higher rate of unnecessary inspections on truthful reporting.

Key words: truthful reporting; self-reporting; rewards; experiment

JEL classification: *C92, D83, Q58*

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1. Introduction

Self-reporting by entities of their own activities commonly serves as an information-gathering tool, and is claimed to help saves enforcement costs (Kaplow and Shavell, 1994) and enhances law compliance (EPA, 1999; 2000). For example, industries self-report their air and water pollution discharges (Russell, 1990), individuals and organizations self-report their income taxes (Collins and Plumlee, 1991), among others. How do the regulatory agencies make sure that the self-reported information is truthful? Regulatory agencies are often not able to undertake adequate inspections due to the lack of capacities. Since there is an opposition to expand their size as a part of the public sector, verifications of self-reported information is often outsourced to private third-party verifiers, as the latter are more likely to enable innovation, efficiency, and quality (McAllister, 2012). The role of the regulatory agencies thus becomes: (1) to approve verifiers according to accreditation criteria, and (2) to perform control and random inspections. Incorporating the third-party verification into a random regulatory enforcement creates a two-layered review system[1]: one layer is the third-party verification, and the other layer is the regulator's random inspection.

The two-layered review system is applied in many Carbon Emissions Trading Schemes, such as the EU ETS. Nevertheless, evidence shows that the truthfulness of self-reported information might not be assured. For example, in Italy's Emissions Trading Scheme, an industry operator was able to replace the verifier which reported findings of violations for the reporting year 2008 – 2010, with a new verifier which did not report any findings for the year 2011 (European Court of Audit: Special Report, 2015). Likewise, as the regulator rely heavily on the performance of the verifiers in many other schemes, such as credit rating, food quality certification, organic farming certification and tax auditing (White, 2010; Ashcraft et al., 2011; Deaton, 2004; Hatanaka et al., 2005; Rohleder, 2005; Kleven et al. 2011; McAllister, 2012; Pomeranz, 2015), entity are typically allowed to select and pay the verifier who assess their reports (Kruger, 2008; Peeters, 2006; McAllister, 2012; Duflo et al., 2013a; and Duflo et al. 2013b; see also Shen et al., 2015).

[1]We take the application of the two-layered review system as a fact. However, there are advantages and disadvantages of this two-layered system compared with a one-layered system where the regulators and the verifiers are integrated. McAllister (2012) and Grossman and Hart (1986) provide useful discussions of the advantage of the two-layered system and the cost of integration.

This private contracting could lead to conflict of interest and increase the temptation for opportunism that results from repeated interactions between entities and third-party verifiers. In particular, competition may arise when firms are allowed to select verifiers, and several studies show that competition encourages dishonesty (Becker and Milbourn, 2011; Farhi et al., 2013; and Faravelli et al., 2015). Moreover, the size of the third parties may also have an impact since the reputation risk is higher for large verifying bodies than for small ones. Therefore, the effectiveness of third parties to disclose violations, and thus improve truthful reporting, cannot be taken for granted.

In addition, the regulator's random inspection, which requires regulatory authorities to oversee the performance of third parties by setting rules for certification and by conducting independent random inspections, might not be effective, too. For example, none of the Member States except for the UK, conducted on-the-spot inspections of installations in the context of EU ETS for Phase III from 2013 (European Court of Audit: Special Report, 2015). This could be due to their resource deficiencies, and their concerns for the entities' economic performance (McAllister, 2012; Peeters, 2006). Although according to the argument in Becker (1968), the amount of violations depends on the probability of detection and the level of the fine, given the weak enforcement the by regulator the effectiveness of fine might not be deterrent.

Moreover, the crux of the regulator's random inspection is that the regulator's enforcement effort is not exogenous, instead, it might be susceptible to the possibility of lobby activities. This is because the administrative success of regulatory authorities is measured by the compliance of polluting firms on paper rather than by the actual amount of emissions. The regulatory authorities might even be reluctant to penalise untruthful reporting in situations where entities are exposed to international competition (see discussion of the enforcement of China's environmental regulation in Ma and Ortolano, 2000).

Notwithstanding the prominence of third-party verification in many systems, misreporting can be prevalent given the pervasive dishonesty in Mazar and Ariely (2006) and Rosenbaum et al. (2014), the extensive under-reporting of violations in firms' self-audits (Telle, 2013). The conflict interests of the third-party verifiers and the weak enforcement efforts of the regulatory authorities give rise to the question at the heart of this paper: if untruthful reporting is prevalent and the third-party verification is fraudulent, how can a weak enforcement provide incentive for truth telling? To ensure the truthfulness of self-reported information, an optimal incentive scheme is essential.

Becker and Stigler (1974), Kofman and Lawarree (1993) and Bac (1998) have suggested rewarding enforcers for successful detections, thus turning them into “bounty-hunters”. Although rewarding regulators may seem a questionable strategy, Skladany (2009) argue that providing performance-based bonus for politicians in developing countries and high-level bureaucrats would reduce corruption when they are highly resistant to other anti-corruption methods. Motivated by this idea, and a lack of systematic research on the effects of rewards on the two-layered review system, we assess whether rewarding successful detection increases the probability of inspection and thus enhances truthful reporting. In particular, we investigate the following hypotheses using a laboratory experiment: First, we test if rewards will reduce the incidence of untruthful reporting. Second, we investigate if rewarding the verifier induces a higher frequency of truthful reporting than rewarding the regulator, while keeping the level of the reward constant. This could be achieved without adding additional enforcement cost to the government’s budget as the reward could be financed by fines. Third, we compare the Nash equilibrium of the stage-game with the findings in the finitely repeated settings of our experiment.

To the best of our knowledge, this is the first study that focuses on the effects of rewards in a two-layered review context, and especially whether the effects differ as a function of the layer at which they are introduced. We contribute to the existing body of enforcement literature by introducing new design elements to our experiment: First, regulators’ random checks are endogenous in our study. Second, we treat competition among verifiers as a factor that might facilitate firms’ untruthful reporting. Under competition, verifiers’ ability to attract firms varies with their reputation for cooperation. Third, we touch on the issue of third-party verification when dishonesty might prevail but remains undetected. The laboratory experiment is suitable as it allows us to disentangle confounding factors for misreporting, such as measurement noise and dishonesty, which are hard to identify in non-laboratory settings.

Our experiment was conducted in the context of an emissions trading scheme’s monitoring, reporting and verification activities, where a third party verifies the self-reported information of a polluting firm and the regulatory authority may randomly conduct checks. We introduce reward schemes for successful detection of untruthful reporting by third parties (called verifiers) and regulatory authorities (called regulators) separately. Consistent with our hypotheses, we find that both reward schemes significantly enhance truthful reporting of firms. Furthermore, rewarding regulators induces too many unnecessary inspections on truthful reporting and thus an efficiency loss to the society.

The remainder of the paper is organised as follows. In Section 2, we review the related literature on regulatory enforcement and the effect of rewards. In Section 3, we describe the experimental design and its implementation. In Section 4, we report and analyse the experimental results. We conclude in Section 5.

2. Literature review

There is a rich body of literature on regulatory enforcement, which is presented in the following section. Theoretical literature on regulatory enforcement suggests that requiring firms to self-report violations is effective in enhancing compliance by increasing fines for untruthful reporting and reducing fines for self-reported violations (Harford, 1987; Malik, 1993; Pfaff and Sanchirico, 2000; Hansen et al., 2014). The deterrence effect of a larger penalty imposed on untruthful reporting is based on the assumption that regulators are motivated to undertake inspections. However, Hiriart et al. (2011) show theoretically that, with asymmetric information of the firms' assets, the fines and the probability of inspections are too low compared to the optimal level with complete information. This is consistent with the observation of weak enforcement of environmental regulations in China in Ma and Ortolano (2000). Therefore, motivating regulators' inspection is critical for truthful self-reporting.

In addition, work on regulation enforcement is focussed on the "Harrington paradox", with the compliance rate being high while both the inspection rate and fine for violations are low (Harrington, 1988; Heyes and Rickman, 1999; Nyborg and Telle, 2006; and Hansen et al., 2014). In these studies, higher compliance rates result from a state-dependent enforcement regime where firms face different inspection rates based on their inspection histories and the threat of being moved to a group with a higher inspection rate. We argue that the high inspection rate might not be realistic as the regulator may lack the capacity and incentive to undertake inspections, and, under weak enforcement, regulators might be influenced by regulated firms. Therefore, the regulatory inspection may be better modelled as the regulator's own decision, which makes it an endogenous variable, as implemented in our experiment.

One critical task of regulating enforcement is to obtain accurate and continuous information on a firm's activities. While continuous monitoring is practical for some environmental pollutants, such as sulfur dioxide emissions (Joskow et al., 1998), other pollutants rely on firms' self-reported information and verifiers' verification (Kruger, 2008; Cason and Gangadharan, 2006; and McAllister, 2012). The difference in the monitoring, reporting and verification aspects results in either a centralized regulatory framework, where the regulator undertakes enforcement (possibly assigning verifiers to firms, either as "partners" or "strangers"[2]), or a decentralized regulatory framework, where both verifiers and regulators undertake inspections and enforcement. The latter framework is exactly the two-layered review system we study in this manuscript. We stress that the inspections of both layers may not be as rigorous as assumed in the literature.

Without changing the review structure, conventional policy instruments for improving the level of truthful reporting include either punishments or rewards. Although they seem to be two sides of the same coin, there are situations where punishments are ineffective but rewards might be effective (Kofman and Lawarree, 1993; Bac, 1998; Heyes, 2000; and Mishra, 2002).

The effect of rewards has been argued in different contexts. For example, rewarding socially desirable behaviour has been suggested (Becker and Stigler, 1974) and trialled through policies to combat cartel operations. Studies have shown that rewards are more effective than leniency (Motta and Polo, 2003; Harrington Jr., 2004; and Aubert et al., 2006). In the taxation literature, Alm et al. (1992), Torgler (2003), Bazart and Pickhardt (2011) and Kastlunger et al. (2011) find that external rewards for honest reports result in a higher compliance rate and a higher number of completely honest reports. Moreover, researchers have experimentally confirmed that rewards are effective for fighting against cheating behaviour (Pascual-Ezama et al., 2013), solving a cooperation problem (Choi and Ahn, 2013), deterring cartel formation (Bigoni et al., 2012), and reducing collusive bribery (Wu and Abbink, 2016).

However, some research suggests that rewards may crowd out individuals' intrinsic incentives and lead to negative side effects[3]. The underlying mechanism might be that people

[2]This distinction is routinely made in the experimental public-goods provision literature, which distinguishes fixed and random matching of participants (see Andreoni and Croson, 2008).

[3]Fiorillo (2011) empirically shows that rewards do not crowd out volunteers' intrinsic incentives to work. In contrast, Brink et al. (2013), in a field experiment on rewarding whistle blowers, show that rewards do crowd out people's intrinsic incentives. Although it seems that the effect of rewards is context-specific, Benabou and Tirole (2003) theoretically suggest two conditions for rewards to have a negative effect: first, the principal (regulator in this study) has to have private information about the agent (verifier in this study) or the task; second, rewards have to convey information of a low ability

perceive rewards as a means to control their behaviour (Deci et al., 1999; Ryan and Deci, 2000; but see Hertwig and Ortmann, 2001, section 4.1.2., for a critical assessment of that literature). Thus, the source and level of rewards need to be carefully designed to reduce the crowd-out effect.

In this study, we provide rewards for successful detection of untruthful reporting by either the verifier or the regulator. We attempt to minimize the crowd-out effect by making rewards a consequence of combined decisions of the firm, the verifier and the regulator. In this way, neither verifiers nor regulators feel controlled by the rewards. Also, inspections become less costly to both the verifiers and the regulators. Moreover, we endogenize the inspections of both the verifiers and regulators, respectively. Because the probability of detection is determined in reality by both the verifiers and regulators, we believe this to be an important feature that adds external validity to our study. In addition, we adopt a finitely repeated setting to capture the influence of reputation on inspection behaviour (see discussion in Selten, 1978, 1991). Our paper is related to Duflo et al. (2013a) in which verifiers were randomly assigned by the regulator and were rewarded for offering more accurate verification, i.e. random matching and rewards were implemented at the same time. We disentangle these effects.

3. Design and implementation

We examine the effect of two different reward schemes on enhancing truthful reporting[4]. In Section 3.1, we explain the stage game used in each treatment: the Baseline without rewards, the treatment of rewarding the verifier only (RV treatment henceforth), and the treatment of rewarding the regulator only (RO treatment henceforth). We formulate Hypothesis 1 and 2 at the end of this section. In Section 3.2 we explain the matching process that is used repeatedly to capture the competition among the verifiers and formulate Hypothesis 3 in the end. In Section 3.3 we describe the experimental procedure.

3.1 The stage game

or a boring task to the agent. Therefore, the crowd-out effect of rewards on intrinsic incentives might not eventuate if the above conditions are not met.

[4]The experiment instructions were free of the environmental regulation context, although for ease of reference loaded language is used in this manuscript.

Our stage game presents the two-layered review system as a three-player sequential game among a polluting firm (“it”), a verifier (“he”), and a regulator (“she”). Figure 1 illustrates the stage game with payoffs for all treatments at the end of each branch.

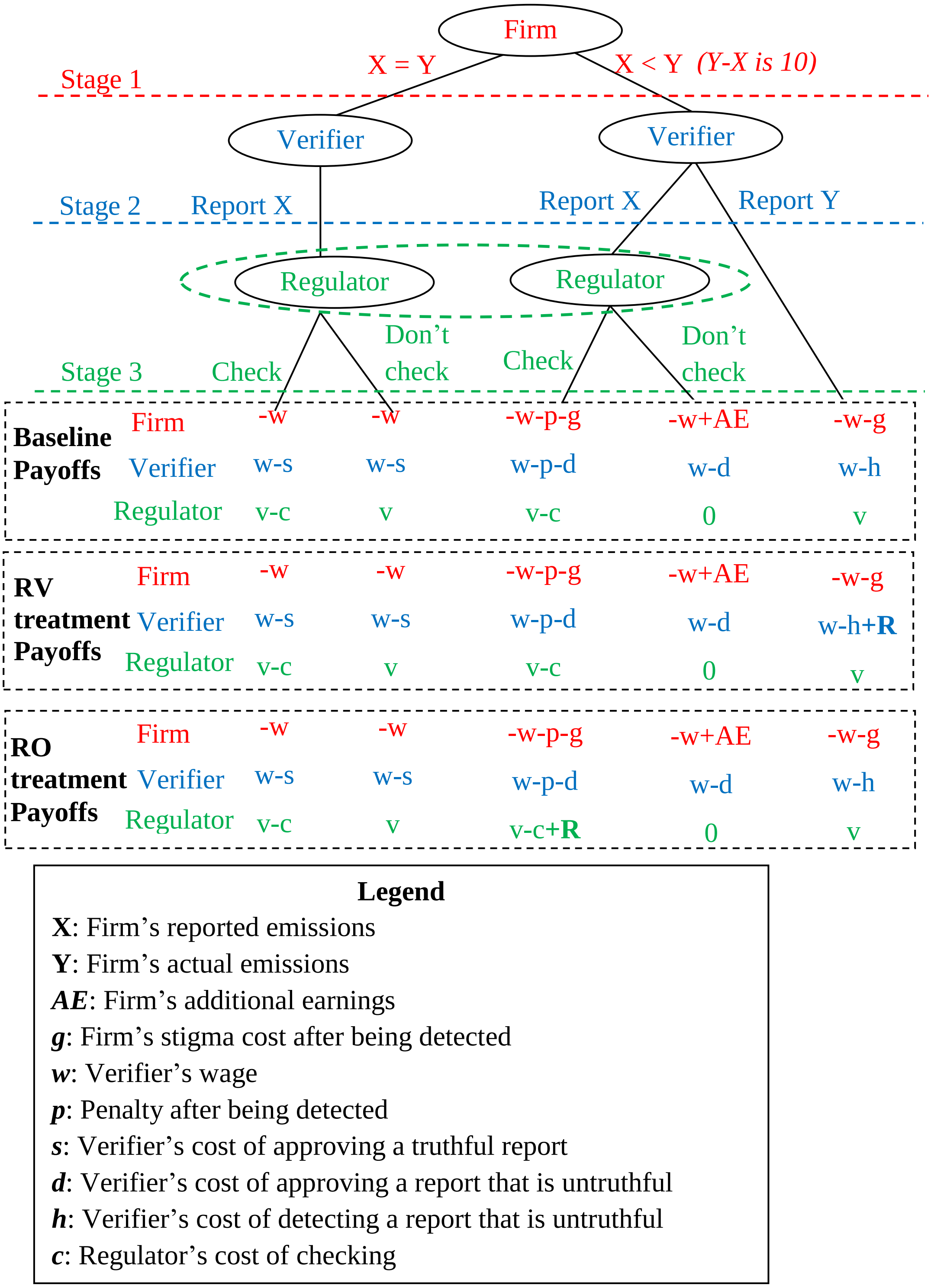

Figure 1. Stage game for all treatments

At Stage 1 of Figure 1, the polluting firm ("firm" from here on) makes its decisions. It chooses two integers: "Y" (the amount of emissions) from a closed interval [11, 100] and "X" (the amount of reported emissions) which, for sake of simplicity, we designate to be either Y-10 or Y. If X = Y, the firm reports truthfully; if X =Y-10, the firm reports untruthfully.[5] The firm knows that reporting untruthfully generates additional earnings, *AE*, which in our setting coincides with the emission fees it avoids. At Stage 2, the verifier observes the decision that the firm has made, and makes his decision. If X=Y, the verifier reports X to the regulator, and assesses the report as satisfactory. In this study, again for the sake of simplicity, we assume verifiers will not erroneously assess a truthful report as unsatisfactory. If X<Y, the verifier chooses between "Report X" (being dishonest, and verifying the report as satisfactory when it is not) and "Report Y" (being honest, and verifying the report as unsatisfactory when it is) to the regulator. Finally, at Stage 3, the regulator will have an opportunity to take an action if she receives a report of X implying by default a satisfactory result from the verifier. Without knowing the truthfulness of the report, the regulator then decides whether to check the verified report which is costly.[6]

The firm pays the verifier a wage of w. The verifier incurs three costs depending on his decision. First, the cost of verifying a truthful report as satisfactory is s (Report X when X=Y); second, the cost of being dishonest is d (Report X when X<Y); and third, the cost of being honest is h (Report Y when X<Y). We assume that $d<s<h$. For technological reasons $s<h$ because disapproving a report that is untruthful has a higher cost[7] than approving a truthful report, since the former is more likely to require a thorough inspection. We assume $d<s$ because the effort of a corrupted verifier will be relatively low. In fact, he might not take any inspection activity. Whenever untruthful reporting is detected, the firm bears a stigma cost g, which we assume to be small under weak enforcement. As for the regulator, checking is assumed to be costly with a level of c. In a case of truthful reporting, or when untruthful reporting is detected, the regulator collects a revenue of v, which represents the return of her successful regulation. We assume c is smaller than v. If the regulator detects an untruthful

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[5] We assume for simplicity that the level of under-reporting does not vary with the firm's characteristics, and the expected gains from under-reporting is homogenous among firms. Of course, the value of X signals the firm's production capacity and is related to the goals of environmental regulations, we leave the effect of X to future study.

[6] Note that all participants make decisions conditional on the decisions made in previous stage, i.e. the verifier makes decisions conditional on the firm's untruthful reporting, and the regulator makes decisions conditional on the verifier's Report X.

[7] There could also be reputational costs pertaining to firms and regulators but they clearly counteract and here we assume that they neutralize.

report, both the firm and the verifier bear a penalty of *p*. The payoffs resulting from the decisions of all three participants are shown at the terminal nodes of the decision tree in Figure 1.

The payoffs in the Baseline reflect the conflicting interests of the firm and the regulator. If the verifier, motivated by additional earnings, chooses to be dishonest (Report X when X<Y), then the firm's best response to not checking is untruthful reporting (X<Y), as $-w+AE>-w$, while the regulator's best response to untruthful reporting is checking; the firm's best response to checking is truthful reporting (X=Y), as $-w>-w-p-g$, while the regulator's best response to truthful reporting is not checking. In the stage game, the verifier and the regulator also have conflicting interests. When the firm reports untruthfully, the verifier's best response to not checking is to conceal the problem (Report X), while the regulator's best response under this situation is checking; the verifier's best response to checking is to reveal the problem (Report Y), while the regulator's best response to Report Y is not checking (no action). Thus, the Baseline stage game has a unique mixed-strategy Nash equilibrium: the regulator checks with probability $\sigma_{check} = (h-d)/p$, the verifier chooses to be honest (Report Y when X<Y) with probability $\sigma_{honest} = 1 - g/(AE + g - \sigma_{check}\ (p + g + AE))$, and the firm chooses to report truthfully with probability $\sigma_{truthful} = ((1-\sigma_{honest})(v - c))/((1-\sigma_{honest})(v-c)+c)$.

The other two treatments capture the two reward schemes we are interested in: rewarding the verifier for revealing an untruthful report, and rewarding the regulator for detecting an untruthful report that was verified as satisfactory. In these treatments, we test whether the respective reward scheme is able to enhance truthful reporting while undermining the verifier's desire to cooperate with the firm, and incentivising the regulator to check. We now describe the two reward schemes in more detail.

***Rewarding the verifier only (RV)*:** The verifier receives a reward *R* if he reveals a report that is untruthful, i.e. Report Y (see Figure 1). The stage game of RV is identical to the Baseline, except for the payoff to the verifier after choosing Report Y. Compared with the Baseline, the verifier now has an additional incentive to Report Y. To make the level of reward relevant to the verifier, we assume $R>h-d$, i.e. Report Y induces a higher payoff. The idea behind the RV treatment is to enhance the firm's expected cost of untruthful reporting by increasing the verifier's probability to Report Y, rather than making the detection of Report Y the most profitable decision to the verifier[8].

There is a unique pure-strategy Nash equilibrium for the stage game of the RV treatment: the firm reports truthfully, the verifier chooses Report Y if X<Y, and the regulator chooses Don't Check. An immediate implication of this equilibrium is that the RV treatment provides the verifier with sufficient incentives to be honest, which in turn discourages the firm from untruthful reporting. With our set of parameters, at least theoretically, a second-layer regulator is not needed under this reward structure and a one-layered review system (with only a verifier) would achieve the same outcome.

Rewarding the Regulator only (RO)**:** The regulator now has the opportunity to receive the reward *R* if she detects a report that is approved by a verifier as truthful when it is not (see Figure 1). Incentivised by the reward, the regulator might Check more often, and untruthful reporting becomes more risky to both the firm and the verifier. To make the level of reward relevant to the regulator, we assume $R>c$, i.e. Check now becomes not always costly. There is a mixed-strategy Nash equilibrium[9] for the stage game of the RO treatment: the regulator will Check with probability $\sigma_{check} = (h-d)/p$, the verifier will be honest (Report Y when X<Y) with probability $\sigma_{honest} = 1 - g/(AE + g - \sigma_{check}\ (p + g + AE))$, and the firm will report truthfully with probability $\sigma_{truthful} = ((1-\sigma_{honest})(v-c+R))/((1-\sigma_{honest})(v-c+R)+C)$. Relative to the Baseline, the RO treatment motivates the firm to choose truthful reporting more often as the value of $\sigma_{truthful}$ is larger. Moreover, comparing the mixed strategy of the verifier and the regulator in both the Baseline and the RO treatment, no behavioural change is expected. While this might seem counterintuitive at first sight, note that the regulator faces two opposing incentives under the RO treatment. On the one hand, the reward may incentivize the regulator to increase the frequency of checking. On the other hand, the enhanced incentive of truthful reporting by the firm may discourage the regulator from checking. In addition, the verifier's decisions are also based on the impact of the two opposing incentives on the part of the regulator. Overall, the effects of these two incentives

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[8]Note that, with our experimental parameters there are situations where verifiers' reporting Y and receiving a reward *R* in the RV treatment, are not as profitable (earning *w-h+R* but possibly only once because of lack of repeated business) as reporting X (concealing untruthful reporting) and repeatedly interacting with one or even more firms (earning *w-d* with each such interaction).

[9]The calculation of the Nash equilibria of the stage game used in the Baseline, RV, and RO treatments are are sketched out in Appendix.

offset each other at least theoretically in our experimental design. Figure 2 presents the actual parameters used in each treatment.

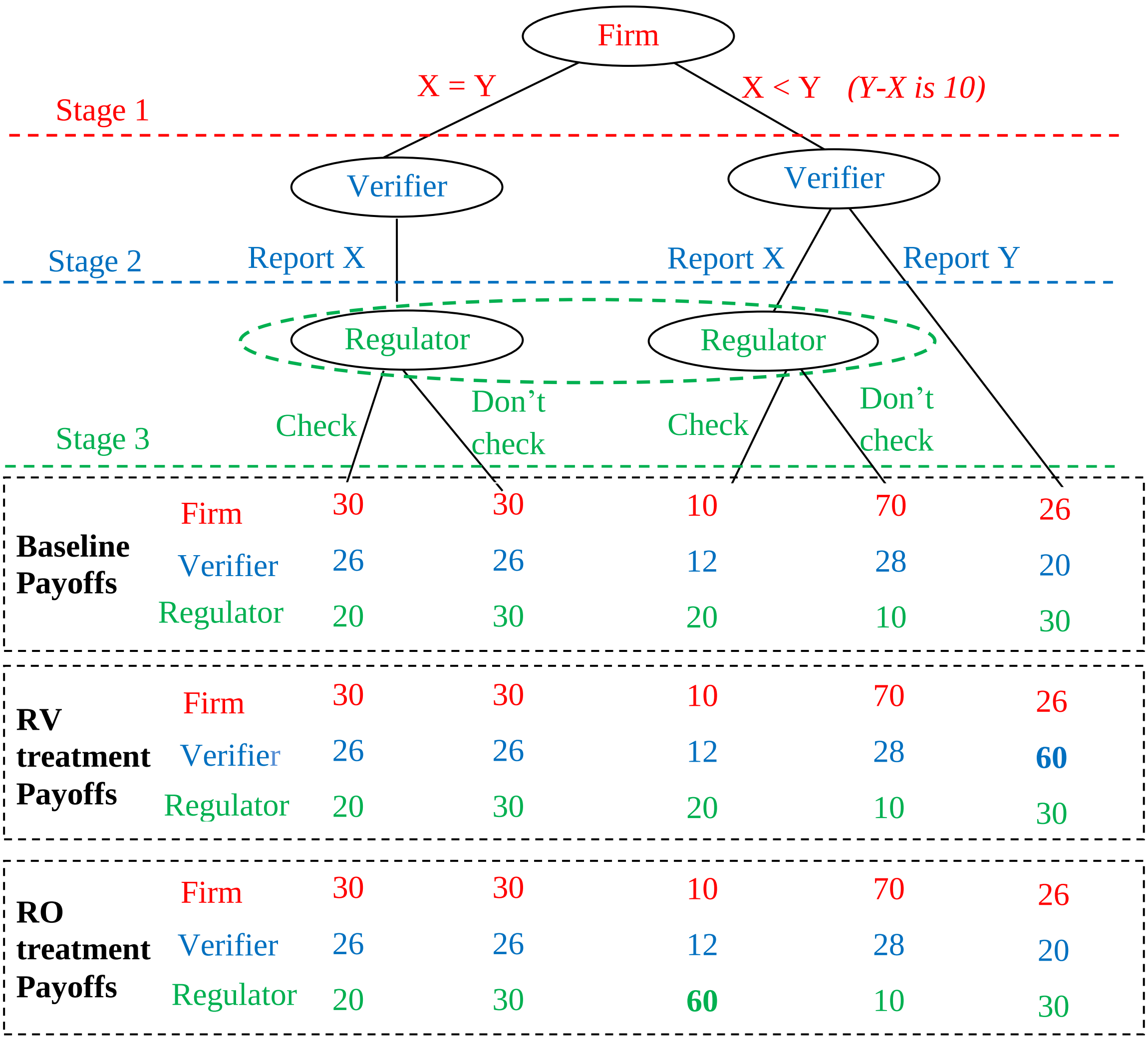


Figure 2. Parameterizations of the stage games

Note: All the payoffs were denoted in experimental dollars (E$), and 1 E$ was worth 0.4 Australian dollar in each session.

Note that the subgame-perfect Nash equilibrium of the repeated sequential game is not easily computable without making assumptions regarding the participants' beliefs, risk preferences, and their trust towards other participants. Because we are interested in the treatment effects, we compute the Nash equilibrium of each corresponding stage game under the assumption of risk neutrality as a first approximation and benchmark. In addition, as participants were informed at the beginning of each session that they would participate in fifteen rounds (with participants of different types moving sequentially in the stage game), the Nash equilibrium of each stage game would be the same as that for the fifteen repetitions. The Nash equilibria of all stage games are summarised in Table 1.

Table 1. Choice probability in Nash Equilibrium for each stage game

Treatments	Firm	Verifier	Regulator
	Probability of truthful reporting	Probability of revealing untruthful reporting (Report Y)	Probability of checking
Baseline	0.22	0.71	0.5
RV	1	1	0
RO	0.59	0.71	0.5

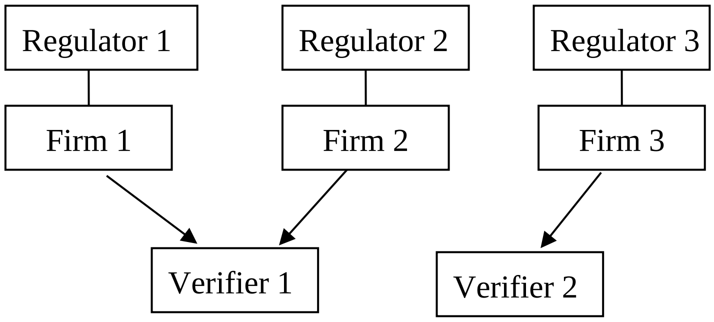
With our set of parameters, the Nash equilibria predict that the firm will report truthfully with a probability of 22% in the Baseline, 100% in the RV treatment, and 59% in the RO treatment. The verifier will reveal untruthful reporting (Report Y) with a probability of 71% in the Baseline and the RO treatment, and 100% in the RV treatment. The regulator will check with probability of 50% in the Baseline and the RO treatment, and with probability of 0 in the RV treatment. Thus, with the same level of reward *R*, both the RV and the RO treatment enhance the frequency of truthful reporting, while the RV treatment is predicted to have a larger enhancement than the RO treatment.

Hypothesis 1. *Firms anticipate that inspection is more worthwhile for either verifiers or regulators if successful detection is rewarded, and thus choose to report truthfully more often in the RV and the RO treatment than in the Baseline.*

Hypothesis 2. *With our set of experimental parameters, the frequency of truthful reporting is higher in the RV treatment than in the RO treatment.*

3.2 Repeating the stage game

In an attempt to increase the number of independent observations (and following standard procedure in experimental economics), participants were divided into subpopulations of eight. Each such subpopulation consisted of three firms, two verifiers, and three regulators. Each firm was matched with one regulator, and this matching remained unchanged throughout the experiment and was common knowledge. Verifiers were selected by firms at the beginning of each round. If a verifier was not selected by a firm, the verifier was excluded from participation in that round. Accordingly, a group consisted of one firm, one verifier, and one regulator (see the example of a matching in Figure 3).



Each firm was paired with one regulator.

Each firm selected one verifier (see example to the left).

Figure 3. Matching rule in a subpopulation

This matching was meant to capture a common situation in environmental regulation where a polluting firm was required to contract with one verifier, leading to competition among the verifiers. The uneven numbers of firms and verifiers (three firms and two verifiers) in a subpopulation was meant to capture that the number of firms is usually larger than the number of verifiers in real-life contexts.

In each session, it was common knowledge that each matched group played the stage game once in each round, and there were fifteen rounds in total. At the end of each round, participants were informed of their own payoffs in that round and the decisions of their matched group members (but no history on previous rounds) including whether the matched firm reported truthfully, which verifier the firm chose and what he decided, and whether the matched regulator checked.

We allow for learning through the fifteen repetitions of the stage game. Repetition is relevant given that tacit coordination is influenced by experience. However, the mixed-strategy in Table 1 might not hold due to participants' social preferences (Selten, 1978, 1991; Oechssler, 2013). And the competition among the verifier might induce them to focus on attracting more firms. For example, the verifier might choose to conceal untruthful reporting while signalling his willingness to cooperate to the firm that selects him in a particular round. In return, if the firm selects him again in the following round, it is likely that they will start to build a trusting relationship, which facilitates untruthful reporting. In addition, the participants' risk preference might also influence the results.

Indeed, it is a robust result in finitely repeated social-dilemma game experiments, that participants are more cooperative than predicted (see for example, Andreoni and Miller, 1993). Specifically, as asserted already in Selten (1978, 1991) there is little evidence for massive unravelling to non-cooperative behaviour except for the last couple of rounds (Kahn and Murnighan, 2008). An implication of this observation is that we might have fewer observations of verifiers disapproving a report that is untruthful in the repeated settings across all treatments[10].

Hypothesis 3. *Verifiers are more cooperative in finitely repeated settings, and therefore they choose to reveal untruthful reporting less often than the expected probability in the Nash equilibrium of the stage game across all treatments.*

3.3 Experimental procedure

Each session consisted of 16 or 24 participants. Role assignments were made randomly and anonymously. We had 48 students participated in each of the treatments, and overall 144 students participated. We therefore collected six independent observations on a subpopulation level in each treatment. Each session lasted for about one hour. The final payoff was based on decisions made in one randomly selected round, in addition to a 5 dollar show-up fee. The average payment was slightly above 20 Australian dollars, ranging from 6 to 54 dollars.

The experiment was conducted at the Australian School of Business Experimental Research Laboratory[11] at the University of New South Wales in November 2014. Participants were recruited through an Online Recruitment System (Greiner, 2015). Each participant only participated in one treatment. The experiment was programmed in zTree (Fischbacher, 2007). At the beginning of each session, participants were asked to answer some comprehension questions in order to ensure that the instructions were understood well. The experiment only started after all participants answered the questions correctly. During the experiment, participants were not allowed to communicate with each other. At the end of the experiment, participants were asked how risky they viewed themselves on a scale from 0 to 10, where 0 represents “not at all willing to take risks” and 10 represents the opposite. The experiment was conducted by a paid research assistant born in Australia, who was experienced at running experiments but blind to the research hypotheses.

4. Results

[10] Note that, given the verifiers’ decisions, we anticipate the probability of truthful reporting and the probability of checking to work in opposite directions, since the firms’ best response to not checking is to report untruthfully, while the regulators’ best response to untruthful reporting is to check.
[11] Now the Bizlab of the UNSW Australia Business School.

The overall level of truthful reporting by firms relies on decisions of both verifiers' and regulators'. Turning them into "bounty-hunters" enhances truthful reporting. In Table 2, we summarise the frequency of all participants' choices across all treatments.

Table 2. Summary statistics

Variables	# of participants	Obs.	Mean	Std. Dev.	Min	Max
Truthful reporting (dummy)						
Baseline	18	270	0.633	0.483	0	1
RV	18	270	0.770	0.421	0	1
RO	18	270	0.759	0.428	0	1
Report Y (dummy)						
Baseline	12	99	0.374	0.486	0	1
RV	12	62	0.855	0.355	0	1
RO	12	65	0.585	0.467	0	1
Check (dummy)						
Baseline	18	233	0.429	0.496	0	1
RV	18	217	0.147	0.355	0	1
RO	18	232	0.556	0.498	0	1

Notes: The "Truthful reporting" includes all observations, the observations of "Report Y" only include cases where firms select untruthful reporting, and the observations of "Check" only include cases where verifiers select Report X.

Table 2 shows that both the frequency of "Truthful reporting" and "Report Y" is increased in the RV and RO treatment compared to the Baseline, while the frequency of "Truthful reporting" is slightly higher in the RV treatment relative to the RO treatment. The frequency of "Check" is decreased in the RV treatment, but increased in the RO treatment relative to the Baseline.

In the remainder of this section, we examine the effects of the two reward schemes on truthful reporting in section 4.1 using consolidated data collected from the experiment, and discuss the impact of competition on the relationship between the firms and the verifiers. In section 4.2, we analyse the inspection behaviour of the verifiers and the regulators.

4.1 Truthful reporting

Rewarding successful detection by either verifiers or regulators, in general, enhances the frequency of truthful reporting by firms, consistent with predictions in Hypothesis 1 in Section 3.1. Figure 4 and Figure 5 illustrates this observation.

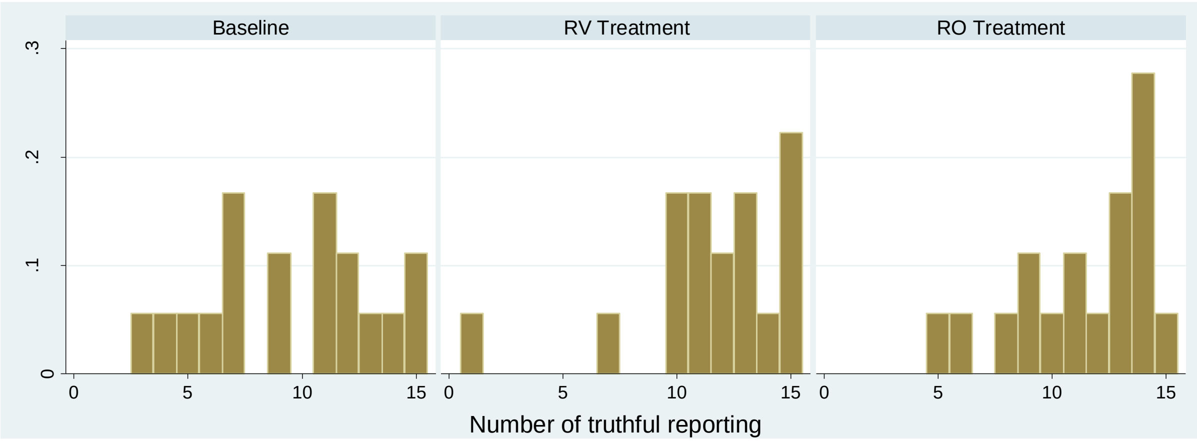


Figure 4. Distribution of number of truthful reporting across all rounds

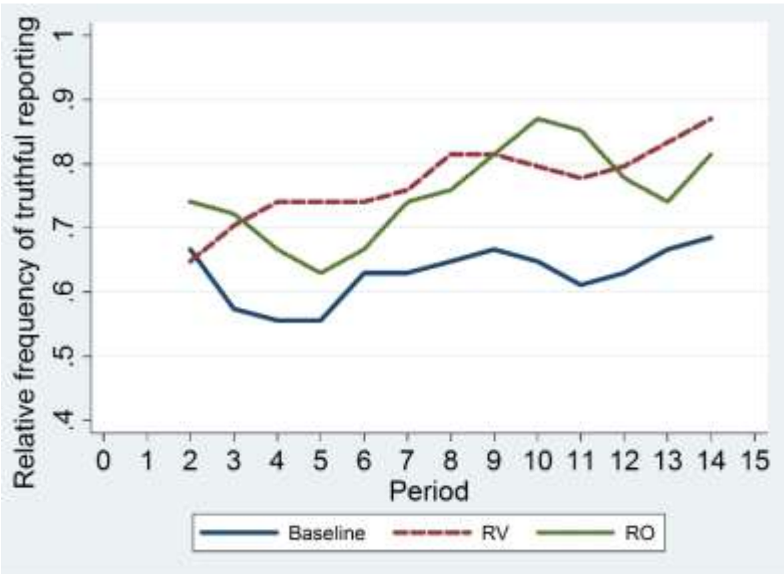


Figure 5. Frequency of firms' truthful reporting over time (smoothed by three-round moving average)

Figure 4 shows that the distribution of the number of truthful reporting across all rounds has shifted to the right in the RV and the RO treatment, suggesting that more firms select truthful reporting under the reward schemes and that they do so more often. Figure 5 displays the frequencies of truthful reporting over time in each treatment, smoothed by a three-round moving average to get rid of some idiosyncratic noise. It is clear that frequencies of truthful reporting are higher in both the RV and the RO treatment relative to the Baseline in almost all rounds. We use non-parametric Wilcoxon Rank Sum tests on a subpopulation level over fifteen rounds (i.e. 6 independent observations per treatment) to compare the frequency of truthful reporting across treatments. Test results confirm Hypothesis 1 as both the improvement in the RV and the RO treatment are statistically significant at $p = 0.065$. However, no significant difference in the frequency of truthful reporting between the RV treatment and the RO treatment is observed ($p = 1$). Recall that Hypothesis 2 predicts that with our set of experimental parameters the RV treatment would induce a higher frequency of truthful reporting than the RO treatment. This is not what we observe.

To explore the reason for this finding, we examine participants' risk preferences and whether there is any difference in the frequency of the most deterrent inspection between the two reward treatments. Presumably, a higher frequency of truthful reporting could result from risk aversion. Nonetheless, Wilcoxon Rank Sum tests based on individuals' risk preferences does not indicate any significant difference among all treatments. As for the deterrence of inspections, there are three types (depicted in Figure 2): (i) verifiers reveal untruthful reports, (ii) regulators choose to check when reports are truthful, (iii) regulators choose to check when reports are untruthful. With our set-up, the last case is the most deterrent to firms, as it results in the lowest payoff for them. The non-parametric Wilcoxon Rank Sum test on a subpopulation level does not reject the null hypothesis of no significant difference in the frequency of this inspection between the two reward treatments ($z = 1.538$, $p = 0.124$). We conclude that the slight difference in the frequency of truthful reporting between the RV and RO treatment is due to no significant difference in the regulators' checks when firms' untruthful reporting are verified as satisfactory.

To take a closer look at the determinants of firms' decisions on truthful reporting, we perform probit regressions while taking into account of time fix effect and the firms' experience with other matched participants, we cluster the standard error on the individual level.[12] Charness, Du and Yang (2011) show that the effect of a person's most recent experience can be different from the effect of his overall experience. Following this idea, we capture the firms' experience as follows. For the overall experience, we use the frequency that the firm has encountered Report Y (Check) by verifiers (regulators), and indicate it as "History of Report Y" ("History of Check"). For the most recent experience, we include dummies of having

[12] As a robustness check, we ran panel probit regressions and fund similar treatment effects.

encountered Report Y (Check) in last round, and indicate it as “Report Y_Lag” (“Check_Lag”).[13] The coefficients of the regressions and the marginal values of the coefficients are reported in Table 3.

Table 3. Effects of reward schemes on truthful reporting

	Truthful reporting (dummy)	
Independent variables	(1) Probit	(2) Marginal values
Treatment variables		
RV	0.855***	0.260***
	(0.293)	(0.085)
RO	0.644***	0.196***
	(0.193)	(0.062)
Experience variables		
History of Report Y	-4.215***	-1.283***
	(0.523)	(0.160)
History of Check	-0.703**	-0.214*
	(0.358)	(0.111)
Report Y_Lag	0.428**	0.130**
	(0.192)	(0.056)
Check_Lag	0.233**	0.071**
	(0.114)	(0.035)
Control for round dummies	Yes	Yes
Pseudo R^2	0.204	
Wald χ^2	151.06	
Number of observations	756	756

Notes: We pool all the firms’ observations across the treatments together in these regressions and exclude the observations in the first round.
“Truthful reporting” (dummy) = 1 if the firms report truthfully in a particular round. “History of Report Y” (“History of Check”) indicates the frequency that the firm has encountered Report Y (Check) up to that round. “Report Y_Lag” (“Check_Lag”) (dummy) = 1 if the firm encountered Report Y (Check) in its last round.
* p< 0.1, ** p < 0.05, *** p < 0.01.
Standard errors are in parentheses.

Looking at the treatment effects, we can see that both the RV and the RO treatment have a statistically significant positive impact on firms’ truthful reporting, confirming our prediction in Hypothesis 1. All else being equal, the marginal values of RV and RO are 0.260 and 0.196 in Column (2), showing that rewarding verifiers for successful detections would have a higher probability of truthful reporting than if the rewards were provided for regulators’

[13] Specifically, “History of Report Y” (or “History of Check”) is calculated according to $\frac{\text{the number of Report Y (or Check) a firm has encountered}}{\text{number of rounds that the stage game has been played}}$, “Report Y_Lag” (“Check_Lag”) is a dummy variable, which equals to 1 if the firm has encountered Report Y (Check) in last round.

successful detections. However, the Wald test does not reject the null hypothesis that there is no significant difference in the coefficients of RV and RO. We summarize the effect of the two reward schemes on truthful reporting as follows.

Observation 1: Effects of rewards on truthful reporting

Rewarding successful inspections by either verifiers or regulators induces statistically significant higher frequency of truthful reporting by firms. However, we observe no significant difference in the frequency of truthful reporting between the two reward treatments, even though the difference is in line with the direction predicted in Hypothesis 2.

Looking at the coefficients of firms' experience, a noteworthy observation is the opposite effect of the firms' most recent and overall experience of being detected. Both the firms' recent experience of being detected, i.e. "Report Y_Lag" and "Check_Lag", has a highly significant positive effect, which reflects the deterrent effect of inspection. But, in contrast, the firms' overall experience of being detected, i.e. "History of Report Y" and "History of Check", has a significant negative effect. This seems counterintuitive, because the more experience of dishonesty being detected previously, the more we would expect the firms to report truthfully. The observation seems to imply that, as the firms' experience of being detected accumulates, they increasingly speculate on collusion.

The matching rule opens the possibility that a firm's selection of a verifier is motivated by its intention to report untruthfully. In a competitive context, if a verifier has built a reputation for collusion, then we would expect that untruthful firms shun verifiers who show a tendency of choosing Report Y, and stick to the verifier who has concealed untruthful reporting. Therefore, we examine on an individual level: (1) whether there is any correlation between the firm's decision of sticking to the same verifier and the verifier's decision of concealing untruthful reporting in last round[14]; (2) whether there is any correlation between the number of a verifier's businesses and his frequency of Report Y in last round[15].

The Spearman correlation coefficients show no significant correlation between the firm's sticking to the same verifier and the verifier's concealing untruthful reporting in the Baseline

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[14] We use a dummy valuable "same" to indicate the firm's selection of verifiers, "same" equals to 1 if the firm's selection is the same as in its last round and 0 otherwise. "Report Y_Lag" indicates the matched verifier's decision in last round. We only use observations on untruthful reporting.

[15] The number of a verifier's businesses is based on the number of firms that select him, and the verifier's frequency of Report Y in a particular round is calculated according to $\frac{\text{the number of Report Y}}{\text{number of business}}$.

(ρ = 0.095, p = 0.364) and the RO treatment (ρ = 0.185, p = 0.157), but significantly positive correlation in the RV treatment (ρ = 0.380, p = 0.004). Moreover, the Spearman correlation coefficients between the verifiers' number of business and their frequency of Report Y in last round are ρ = -0.034 (p = 0.653) in the Baseline, ρ = -0.182 (p = 0.015) in the RV treatment and ρ = -0.038 (p = 0.609) in the RO treatment. A reasonable inference is that rewarding successful verifiers (RV) might strengthen the relationship between firms and verifiers that are devious, even though it would induce more honest reporting (see the coefficient of RV in Column (1) of Table 3). Untruthful firms gamble on verifiers not going for the short-term rewards and rather hope for payoffs from repeated business. We find indeed one firm in the RV treatment to have reported untruthfully fourteen times, and always selected the verifier who had concealed untruthful reporting in last round. Although the firm encountered Report Y eight times and Check four times, it was not discouraged from gambling on collusion. Its average payoff in the end is E$ 31.867, which is slightly above the payoff for truthful reporting (E$ 30).

4.2 Inspection behaviour

The estimation results in Table 3 indicate that both the RV and the RO treatment causes firms to choose truthful reporting more often, although with no significant difference between the two reward treatments. To better understand the firms' decisions, we analyse the treatment effect of RV and RO on the verifiers' and regulators' inspection. Figure 6 shows the relative frequencies of revealing untruthful reporting (Report Y) by verifiers and the relative frequencies of checking by regulators over all rounds, while they are again smoothed by a three-round moving average. Recall that Hypothesis 3 had implied that verifiers would be more cooperative, thus choosing Report Y less often, in a finitely repeated setting. This is observable in Figure 6.

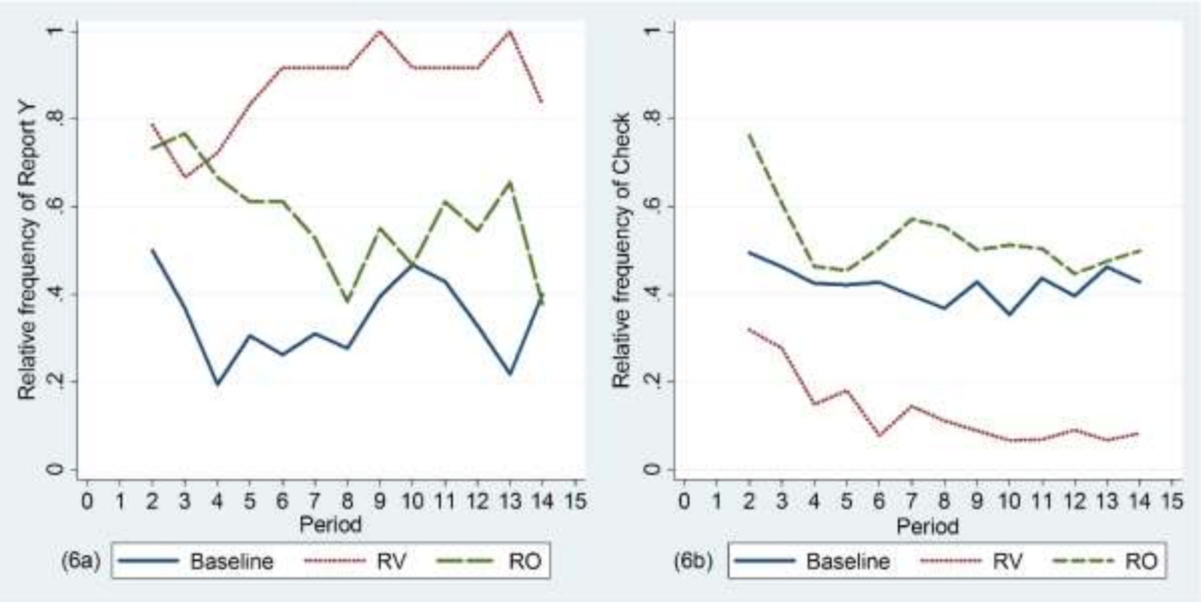

Figure 6. Frequencies of both Verifiers' Report Y and Regulators' Check over time (smoothed by three-round moving average)

Note: For calculating the frequency of Report Y, we only include cases when the verifier receives untruthful reporting from the matched firm; for calculating the frequency of Check, we only use cases where the regulator receives a Report X from the matched verifier.

Panel (6a) of Figure 6 shows that, in general, after the first few rounds of learning the frequencies of Report Y are between 20% and 40% in the Baseline. The RV treatment leads to remarkably high frequencies between 80% and 100%, more than doubling the frequencies in the Baseline. The RO treatment also leads to an increase to the range between 40% and 60%, which are lower than the frequencies in the RV treatment.[16] We compare the actual frequency of Report Y to the equilibrium prediction in Table 1 through binomial tests, and find significant difference across all treatments. Turning back to Hypothesis 3, our findings confirm the conjecture that verifiers are more cooperative in a finitely repeated setting than the expected mixed-strategy.

Observation 2:

In our finitely repeated setting, verifiers are more cooperative and choose Report Y less often than the predicted mixed-strategy of the stage game.

While the RV treatment leads to remarkably high frequencies of Report Y, it reduces the frequencies of Check (see Panel (6b) of Figure 6). In fact, starting in round six it is mostly

[16]Note that it is common knowledge the stage game would be played fifteen times at the beginning of the experiment. In Panel (6a) of Figure 6, the decline in the last round of the RV and the RO treatment might indicate an end-game effect.

well below 20 percent, which is less than half of the frequencies we observe in the Baseline and even less than what we observe in the RO treatment. The changes that the RV induces in the frequencies of Report Y and Check are both significant at the level of 1%, and the change that the RO induces in the frequency of Report Y is significant at p =0.065 according to the Wilcoxon Rank Sum test on a subpopulation level over fifteen rounds.

However, instead of reducing the frequency of Checking, the RO treatment enhances it and maintains it at a relative high frequency compared with the Baseline. In light of the increased frequency of firms' truthful reporting and verifiers' Report Y in the RO treatment relative to the Baseline (see Figure 5 and Penal (6a) of Figure 6), we would expect the regulators to check less in the RO treatment. This is obviously not what we observe. The unexpected high frequency of Check in the RO treatment implies that many of those checks are imposed on truthful reports, thus unnecessary. In fact, the number of unnecessary checks are higher in the RO treatment comparing with that in the Baseline and the RV treatment, and the Wilcoxon Rank Sum tests on a subpopulation level shows that both of the differences are statistically significant at p = 0.030 and p = 0.004, respectively.

We next look at the determinants of verifiers' and regulators' decisions through probit regressions while taking into account of time fixed effect and clustering at the individual level. Again, we include their recent and overall experience with matched participants in the regressions, and exclude the observations in the first round. We report the coefficients and the marginal values of the coefficients in Table 4.[17]

Table 4. Effects of reward schemes on inspection behaviour

	Verifiers Report Y		Regulators Check	
Independent variables	(1) Probit	(2) Marginal values	(3) Probit	(4) Marginal values
Treatment variables				
RV	1.326***	0.495***	-0.633**	-0.228**
	(0.386)	(0.143)	(0.301)	(0.102)
RO	0.760**	0.284**	0.660***	0.237***
	(0.366)	(0.132)	(0.212)	(0.081)
Experience Variables				
History of untruthful reporting	-1.370***	-0.511***	2.491***	0.896***
	(0.500)	(0.179)	(0.634)	(0.226)
Number of Client	0.126	0.047		
	(0.275)	(0.102)		

[17] We also ran panel probit regressions and fund similar treatment effects.

Untruthful Rate	1.041**	0.388**		
	(0.478)	(0.182)		
History of Check	-1.351***	-0.504***		
	(0.465)	(0.167)		
Check Rate_Lag	0.484	0.181		
	(0.377)	(0.140)		
History of Report Y			-1.389	-0.499
			(1.096)	(0.389)
Untruthful_Lag			0.504***	0.181***
			(0.186)	(0.068)
Report Y_Lag			-0.074	-0.027
			(0.289)	(0.104)
Control for round dummies	Yes	Yes	Yes	Yes
Pseudo R^2	0.269		0.255	
Wald χ^2	167.06		141.05	
Number of observations	179	179	640	640

Notes: Observations of "Report Y" only include cases where the verifier received untruthful report(s). Observations of "Check" only include cases where verifiers chose Report X.

"Verifiers Report Y" (dummy) = 1 if a verifier revealed at least one report that is untruthful in a particular round. "Regulators Check" (dummy) = 1 if a regulator checked in a particular round. "History of untruthful reporting" indicates the frequency that the verifier (or regulator) had experienced untruthful reporting up to that round. "Number of Client" indicates the number of firms who selected the verifier in that round. "Untruthful Rate" indicates the frequency of untruthful reporting that the verifier encountered in that round. "History of Check" ("Check_Rate_Lag") is the frequency that the verifier had experienced Check up to that round (in last round). "History of Report Y" indicate the frequency that the regulator had experienced Report Y up to that round. "Untruthful_Lag (Report Y_Last)" (dummy) =1 if the matched firm (verifier) chose untruthful reporting (Report Y) in last round.

* p< 0.1, ** p < 0.05, *** p < 0.01.

Standard errors are in parentheses.

One common observation is that both the reward treatments enhance the frequency of Report Y (see Column (1)). In Column (2), the marginal value of RV shows that holding other things constant verifiers would have 49.5% higher probability of revealing untruthful reporting if is rewarded for doing so. Since in the RO treatment, rewards are not directly imposed on successful verifiers, it is not surprising that the marginal value of RO (0.284) is smaller than RV. An interesting observation in Column (3) is that, while the RV treatment has a significantly negative effect on the regulators' behaviour, the RO treatment has a significantly positive effect. The negative effect of RV might be related to the enhanced frequency of Report Y by the verifiers, resulting in fewer chances for the regulators to check. The positive effect of RO on checking seems to imply that turning regulators into "bounty-hunters" indeed incentivises their checking. However, it seems that this enhanced incentive to detect untruthful reporting were more likely to be imposed on truthful reporting given the enhancement in firms' truthful reporting in the RO treatment relative to the Baseline.

Observation 3: Effects of rewards on inspection behaviour

Rewarding successful verifiers increases their frequency of revealing untruthful reporting, and reduces the frequency of checking by regulators. Rewarding successful regulators is also effective in incentivising both truthful verification and checking, however, it may incentivise checking to a degree beyond optimal.

Focusing on the category of experience variables, an observation is that the frequency of Check over time, i.e. "History of Check", has a significantly negative effect on the verifiers' decision (Column (2)). We propose that this result follows the same logic as our explanation for the negative effect of "History of Report Y" and "History of Check" on the firms' decision in section 4.1. The negative effect of "History of Check" in Column (2) might results from verifiers becoming more desperate and speculate on collusion as their experience of being checked increases. Moreover, as for checking, we find that the frequency of untruthful reporting over time, i.e. "History of untruthful reporting", and the encounter of untruthful reporting in the last round, i.e. "Untruthful_Lag", have significantly positive effect. This is indeed how the regulators are supposed to respond.

5. Conclusion

We report results from a laboratory experiment designed to investigate the effects of rewards on enhancing truthful reporting in a two-layered review system. Our experiment is motivated by the context of environmental regulation, where polluting firms are required to report the amount of emissions produced, and receive benefits from untruthful reporting. However, the results of our experiment speaks apply to any system where entities self-report information and select a third party to review their report. In addition, a regulatory authority may randomly check the verified report to ensure the truthfulness of the conveyed information.

Because of competition among the third parties and the resultant conflicting incentives as well as weak inspection by regulatory authorities, the probability of detection is too low to be effective and entities may not report truthfully. Incentives through rewards have been widely used in many other application areas, but there is no systematic analysis of the effect of rewards in the two-layered review system sketched above. In order to promote truthful reporting, we introduce two different reward schemes experimentally. In one, we reward the third party for revealing untruthful reporting (RV). In the other, we reward the representative of the regulatory authorities for successful detections of untruthful reporting that is verified as satisfactory (RO).

To summarise our findings, we report the observed choice probabilities and their corresponding probabilities in the Nash equilibria (NE, for short; previously reported in Table 1 and hence shaded below) in Table 5.

Table 5. Choice probabilities in stage games' NE and observation

	Baseline		RV		RO	
Probability	NE stage game	Observ.	NE stage game	Observ.	NE stage game	Observ.
Firm: Report truthfully	0.22	0.63	1	0.77	0.59	0.76
Verifier: Report Y	0.71	0.37	1	0.85	0.71	0.58
Regulator: Check	0.5	0.43	0	0.15	0.5	0.56

We find, both the RV and RO treatment have a positive effect on enhancing the frequency of truthful reporting, i.e. it is 77% in RV, 76% in RO and 63% in Baseline (see the first row of Table 5). Therefore, the positive effects of rewards on enhancing truthful reporting across treatments confirms the predictions in Hypothesis 1. We note that this effect comes about notwithstanding a very high base-rate of truthful reporting in the Baseline. This is most likely due to the well-documented pro-social preferences of lab participants (Cooper and Kagel, 2013). Inconsistent with Hypothesis 2, no significant difference in the frequency of truthful reporting is observed between the RV and the RO treatment. We attribute this to insignificant difference in the frequencies of the most deterrent detection, i.e. checking untruthful reports that were verified as satisfactory. We find that third parties in all treatments have a lower tendency to Report Y compared with the NE (conditional on having encountered an untruthful report: Baseline: 0.37 vs 0.71, RV: 0.85 vs 1, RO: 0.58 vs 0.71), which confirms our expectation of Hypothesis 3 that verifiers are more cooperative in finitely repeated settings.

A different consequence of the two reward schemes appears to be driven by the regulators' behaviour. Regulators tend to check less (more) in the RV (RO) treatment than in the Baseline (see the third row of Table 5). Given that the RO treatment incentivises honest behaviour on the part of entities and third-parties relative to the Baseline, the enhanced checking is probably too much of a good thing. This result suggests that from a hierarchical perspective, rewarding higher-layer inspectors (regulators) for successful enforcement might over-correct the enforcement problem by causing too many unnecessary checks on truthful reports, which is unnecessarily costly for the society.

Our study contributes to the literature on the effect of rewards on truth-telling in situations where compliance relies on self-reporting, and, simultaneously, enforcement is weak and inspection decisions are endogenous. First, we introduce rewards endogenously as a consequence of the combined decisions of the participants. Rewards do turn inspectors into “bounty-hunters” for untruthful reporting, as has been conceptually suggested in the literature. Second, we examine the effect of rewarding inspectors at different layers. We conclude that in situations where both layers’ inspections are lax and competition among the verifiers promotes untruthful reporting, rewarding the lower-layer inspectors (verifiers) for detecting untruthful reporting seems to be the more cost effective and efficient approach, which outperforms the approach of rewarding the higher-layer inspectors (officials). Third, we demonstrate that competition in a finitely repeated setting is likely to promote untruthful reporting, which is relevant to various forms of social regulation such as accounting, financial credit rating, etc.

Our present results suggest future research. First, although the level of reward used in the experiment is considerable, it does not make detection the most beneficial decision for the verifiers. Future research can vary the level of rewards in order to gain greater understanding of the reward effect in different levels. Second, untruthful reporting is also likely to be associated with corruption. Therefore, incorporating the reward schemes into a bribery game with the two-layered review system may provide insight for social regulation when the external legal system is not well established. Third, firms in reality are of different sizes, they may have varied capacities in untruthful reporting, thus varying the size of their additional earnings may add some additional insights on the effect of rewards with varied level of untruthful reporting.

Appendix. Detailed calculation of the Nash equilibria of the stage games (Baseline, RV, and RO)

We define the probability that Firm chooses to report truthfully is$\sigma_{truthful}$, the probability that Verifier chooses to be honest (Report Y when X<Y) is σ_{honest}, and the probability that Regulator chooses to check is σ_{check}. We derive the Nash equilibrium of each stage game by calculating participants' expected utility of each strategy, and then finding their best response to other matched participants' best decisions.

1. Calculation of the mixed-strategy Nash equilibrium of the Baseline stage game

Regulator:

Let us consider the Regulator's best response to other matched participants' best decisions.

If the Regulator chooses Check, then her expected utility is

$$U(Check) = \sigma_{truthful}(V - C) + (1 - \sigma_{truthful})(1 - \sigma_{honest})(V - C) + (1 - \sigma_{truthful})\sigma_{honest}V \quad \text{Eq. (1)}$$

If the Regulator chooses Don't Check, then her expected utility is

$$U(Don't\ Check) = \sigma_{truthful}V + (1 - \sigma_{truthful})(1 - \sigma_{honest}) * 0 + (1 - \sigma_{truthful})\sigma_{honest}V \quad \text{Eq. (2)}$$

The Regulator woud feel indifferent between choosing Check and Don't Check when $U(Check) = U(Don't\ Check)$, thus we get $\sigma_{truthful} = \frac{(1-\sigma_{honest})(V-C)}{(1-\sigma_{honest})(V-C)+C}$.

Verifier:

We consider the Verifier's best response to other matched participants' best decisions.

Whenever the Verifier encounters a report that is not truthful, if he chooses Report X , then his expected utility is

$$U(Report\ X) = (1 - \sigma_{truthful})\sigma_{check}(w - p - d) + (1 - \sigma_{truthful})(1 - \sigma_{check})(w - d) \quad \text{Eq. (3)}$$

If the Verifier chooses Report Y , then his expected utility is

$U(Report\ Y) = \left(1 - \sigma_{truthful}\right) * (w - h)$ Eq. (4)

The verifier woud feel indifferent between choosing Report X and Report Y when $U(Report\ X) = U(Report\ Y)$, thus we get $\sigma_{check} = \frac{h-d}{p}$.

Firm:

We consider the Firm's best response to other matched participants' best decisions.

If the Firm chooses to report truthfully (X=Y), then its expected utility is

$U(\mathrm{X} = \mathrm{Y}) = -w$ Eq. (5)

If the Firm chooses not to report truthfully (X<Y), then its expected utility is

$U(X < Y) = (1 - \sigma_{honest})\sigma_{check}(-w - p - g) + (1 - \sigma_{honest})(1 - \sigma_{check})(-w + AE) + \sigma_{honest}(-w - g)$ Eq. (6)

The firm woud feel indifferent between reporting truthfully and not truthfully when $U(X = Y) = U(X < Y)$, thus we get $\sigma_{honest} = 1 - \frac{g}{AE+g-\sigma_{check}(p+g+AE)}$

The stage game of the Baseline has a unique Nash equilibrium, whereFirms report truthfully with probability $\sigma_{truthful} = \frac{(1-\sigma_{honest})(V-C)}{(1-\sigma_{honest})(V-C)+C}$,Verifiers chooseto be honest (Report Y when X<Y) with probability $\sigma_{honest} = 1 - \frac{g}{AE+g-\sigma_{check}(p+g+AE)}$, and Officals Check with probability $\sigma_{check} = \frac{h-d}{p}$.

2. Calculation of the pure-strategy Nash equilibrium of the RV stage game

Verifier:

We consider the Verifier's best response to other matched participants' best decisions.

Whenever the Verifier encounters a report that is not truthful, if he chooses Report X , then his expected utility is

$U(Report\ X) = \left(1 - \sigma_{truthful}\right)\sigma_{check}(w - p - d) + \left(1 - \sigma_{truthful}\right)(1 - \sigma_{check})(w - d)$ Eq. (7)

If the Verifier chooses Report Y , then his expected utility is

$U(Report\ Y) = (1 - \sigma_{truthful}) * (w - h + R)$ Eq. (8)

Since $(w - h + R) > (w - d) > (w - p - d)$, the Verifier's dominant decision is Report Y, thus $\sigma_{honest} = 1$.

Firm:

We consider the Firm's best response to other matched participants' best decisions.

If the Firm chooses to report truthfully (X=Y), then its expected utility is

$U(\text{X} = \text{Y}) = -w$ Eq. (9)

If the Firm chooses not to report truthfully (X<Y), then its expected utility is

$U(X < Y) = (1 - \sigma_{honest})\sigma_{check}(-w - p - g) + (1 - \sigma_{honest})(1 - \sigma_{check})(-w + AE) + \sigma_{honest}(-w - g) = -w - g$(since $\sigma_{honest} = 1$) Eq. (10)

Thus $\sigma_{honest} = 1$, since $U(\text{X} = \text{Y}) = -w > -w - g = U(X < Y)$

Regulator:

We consider the Regulator's best response to other matched participants' best decisions.

If the Regulator chooses Check, then her expected utility is

$U(Check) = \sigma_{truthful}(V - C) + (1 - \sigma_{truthful})(1 - \sigma_{honest})(V - C) + (1 - \sigma_{truthful})\sigma_{honest}V = V - C$ Eq. (11)

If the Regulator chooses Don't Check, then her expected utility is

$U(Don't\ Check) = \sigma_{truthful}V + (1 - \sigma_{truthful})(1 - \sigma_{honest}) * 0 + (1 - \sigma_{truthful})\sigma_{honest}V = V$ Eq. (12)

Therefore, $\sigma_{check} = 0$,since $U(Check) < U(Don't\ Check)$.

The stage gameof the RV treatment has a unique Nash equilibrium, where Firm reports truthfully with probability 1, Verifier choosesto be honest (Report Y when X<Y) with probability 1, and Offical Check with probability 0.

3. Calculation of the mixed-strategy Nash equilibrium of the RO stage game

Regulator:

We consider the Regulator's best response to other matched participants' best decisions.

If the Regulator chooses Check, then her expected utility is

$$U(Check) = \sigma_{truthful}(V - C) + (1 - \sigma_{truthful})(1 - \sigma_{honest})(V - C + R) + (1 - \sigma_{truthful})\sigma_{honest}V \qquad \text{Eq. (13)}$$

If the Regulator chooses Don't Check, then her expected utility is

$$U(Don't\ Check) = \sigma_{truthful}V + (1 - \sigma_{truthful})(1 - \sigma_{honest}) * 0 + (1 - \sigma_{truthful})\sigma_{honest}V \qquad \text{Eq. (14)}$$

The Regulator woud feel indifferent between choosing Check and Don't Check when $U(Check) = U(Don't\ Check)$, thus we get $\sigma_{truthful} = \frac{(1-\sigma_{honest})(V-C+R)}{(1-\sigma_{honest})(V-C+R)+C}$.

Verifier:

We consider the Verifier's best response to other matched participants' best decisions.

Whenever the Verifier encounters a report that is not truthful, if he chooses Report X , then his expected utility is

$$U(Report\ X) = (1 - \sigma_{truthful})\sigma_{check}(w - p - d) + (1 - \sigma_{truthful})(1 - \sigma_{check})(w - d) \qquad \text{Eq. (15)}$$

If the Verifier chooses Report Y , then his expected utility is

$$U(Report\ Y) = (1 - \sigma_{truthful}) * (w - h) \qquad \text{Eq. (16)}$$

The verifier woud feel indifferent between choosing Report X and Report Y when Let $U(Report\ X) = U(Report\ Y)$, thus we get $\sigma_{check} = \frac{h-d}{p}$.

Firm:

We consider the Firm's best response to other matched participants' best decisions.

If the Firm chooses to report truthfully (X=Y), then its expected utility is

$$U(\mathrm{X} = \mathrm{Y}) = -w \qquad \text{Eq. (17)}$$

If the Firm chooses not to report truthfully (X<Y), then its expected utility is

$$U(X < Y) = (1 - \sigma_{honest})\sigma_{check}(-w - p - g) + (1 - \sigma_{honest})(1 - \sigma_{check})(-w + AE) + \sigma_{honest}(-w - g) \quad \text{Eq. (18)}$$

The firm woud feel indifferent between reporting truthfully and not truthfully when $U(X = Y) = U(X < Y)$, thus we get $\sigma_{honest} = 1 - \frac{g}{AE+g-\sigma_{check}(p+g+AE)}$

The stage game of the RO treatment has a unique Nash equilibrium, whereFirm reports truthfully with probability $\sigma_{truthful} = \frac{(1-\sigma_{honest})(V-C+R)}{(1-\sigma_{honest})(V-C+R)+C}$,Verifier chooses to be honest (Report Y when X<Y) with probability $\sigma_{honest} = 1 - \frac{g}{AE+g-\sigma_{check}(p+g+AE)}$, and Offical Check with probability $\sigma_{check} = \frac{h-d}{p}$.

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COMPLETE SET OF INSTRUCTIONS, FOR ONLINE PUBLICATION

Instructions

Welcome to this experiment!

Please turn off your cell phone and other electronic devices now. This is an experiment in economic decision making. In the experiment you can earn experimental dollars (E$). 1 E$ is worth 0.4 AUS$. How much money you earn depends on your decisions and on the decisions of other participants. All interactions among participants will take place through the computer network. Please do not talk with other participants during the experiment. If you have a question, raise your hand. One of the experimenters will then come to you and answer your question.

This experiment involves three types of participants: A-participant, B-participant and C-participant. At the beginning of the experiment, you will be randomly assigned one of the three types; your type will remain unchanged throughout the experiment. In addition, you are randomly assigned into a group of 8 participants, which contains three A-participants (A1, A2, and A3), two B-participants (B1 and B2) and three C-participants (C1, C2 and C3).

At the beginning of each round, a C-participant and an A-participant will be randomly paired with each other, and the A-participant will select one of the two B-participants to match with them. The matches between the C-participants and the A-participants remain unchanged throughout the experiment. Note that there are more A-participants than B-participants. **Therefore, B-participants may be selected more than once.**

Each round consists of three stages and different types of participants move sequentially, with A-participants moving before B-participants which in turn move before C-participants.

Stage 1: The first mover's decisions

A-participants move first. Each A-participant is a firm that produces Y units of some unspecified good. To produce, an A-participant should hold one license for each unit of good produced. After selecting a B-participant, the A-participant decides how many **units (Y) to produce** and **the number of licenses (X)** s/he wants to hold. S/he has two options:

1) X = Y;
2) X = Y-10.

The A-participant should first choose the number for Y and then choose the number for X in the "Decision Making Area". The two possible choices for X will show up as soon as Y has been chosen.

If the A-participant chooses X = Y, s/he will earn 30 E$ and the selected B-participant will earn 26 E$ from this interaction. If the A-participant chooses X = Y-10, their payoffs will depend on the choice of the selected B-participant.

Stage 2: The second mover's decisions

B-participants move second. B-participants examine whether the matched A-participant has complied with the rule of holding one license for each unit of product produced. The B-participant will be informed of the number of A-participants who have selected her/him, and their chosen X and Y.

If the matched A-participant chooses **X = Y**, then B-participant will **report X** to the C-participant. If the matched A-participant chooses **X = Y-10**, the B-participant then decides whether to **“report X”** or **“report Y”** for each A-participant who has selected her/him.

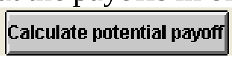
- If the B-participant reports Y, this interaction ends immediately and the three matched types receive payoffs according to Table 1 (see below).
- If the B-participant reports X, the payoffs in this interaction will depend on the choice of the matched C-participant.
- If a B-participant is not chosen by any A-participant, s/he does not make a decision and earns 0 E$ in that round.

A B-participant’s payoff in a round is the sum of earnings from all the interactions with A-participants in that round.

Stage 3: The third mover’s decisions

C-participants move third. C-participants are informed whether the B-participant they are matched with reported X or Y.

- If a C-participant receives **a report of Y**, s/he **does not take any action** in that round.
- If a C-participant receives **a report of X**, s/he chooses between “**Check**” and “**Don’t Check**”.(S/he does this without knowing if there is a discrepancy between the chosen X and Y.)

Please note that on your screen there will be a “Decision Making Area” on the left and a “Calculation Area” on the right. The Calculation Area is to help you to get familiar with your options and you can find out the payoffs **in one particular interaction** that comes with various options by clicking on the 

button, which will show up once you have made choices. **Only the decisions made in the “Decision Making Area” will impact your actual earnings.**

In total, the experiment has 15 rounds. At the end of each round, you will be informed of what happened and your payoff in that round. **Your final earnings will be one randomly selected round-payoff together with 5 dollar show-up fee**. After the experiment has concluded, please remain seated and do not communicate with others. We will call you individually by your seat number and pay you your final earnings in cash.

The following decision options and payoffs **per interaction** are applied in the experiment.

Table 1 The payoff table (**used in Baseline**)[18]

Stage 1: A's choices		Choose Y = X		Choose Y-X=10		
Stage 2: B's choices		Report X		Report X		Report Y
Stage 3: C's choices		Check	Don't Check	Check	Don't Check	No action
Payoffs	Type A	30	30	10	70	26
	Type B	26	26	12	28	20
	Type C	20	30	20	10	30

Table 1 The payoff table (**used in RV treatment**)

Stage 1: A's choices		Choose X = Y		Choose Y-X=10		
Stage 2: B's choices		Report X		Report X		Report Y
Stage 3: C's choices		Check	Don't Check	Check	Don't Check	No action
Payoffs	Type A	30	30	10	70	26
	Type B	26	26	12	28	60
	Type C	20	30	20	10	30

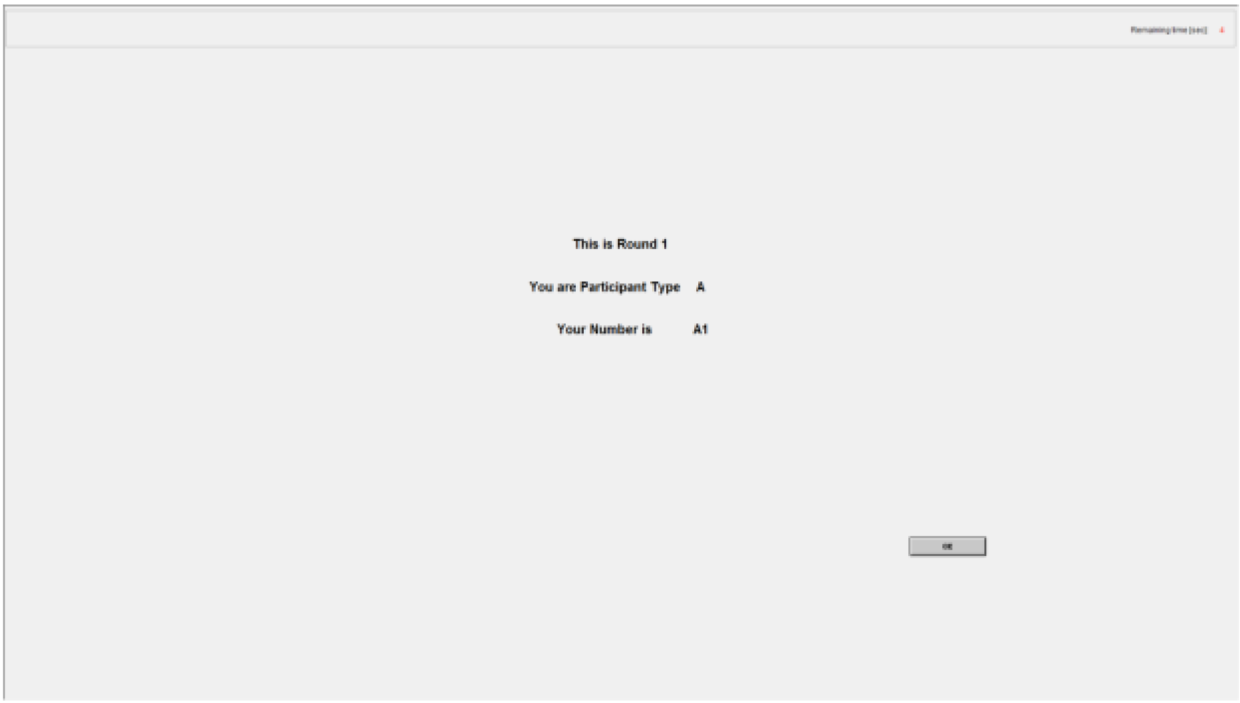
Table 1 The payoff table (**used in RO treatment**)

Stage 1: A's choices		Choose X = Y		Choose Y-X=10		
Stage 2: B's choices		Report X		Report X		Report Y
Stage 3: C's choices		Check	Don't Check	Check	Don't Check	No action
Payoffs	Type A	30	30	10	70	26
	Type B	26	26	12	28	20
	Type C	20	30	60	10	30

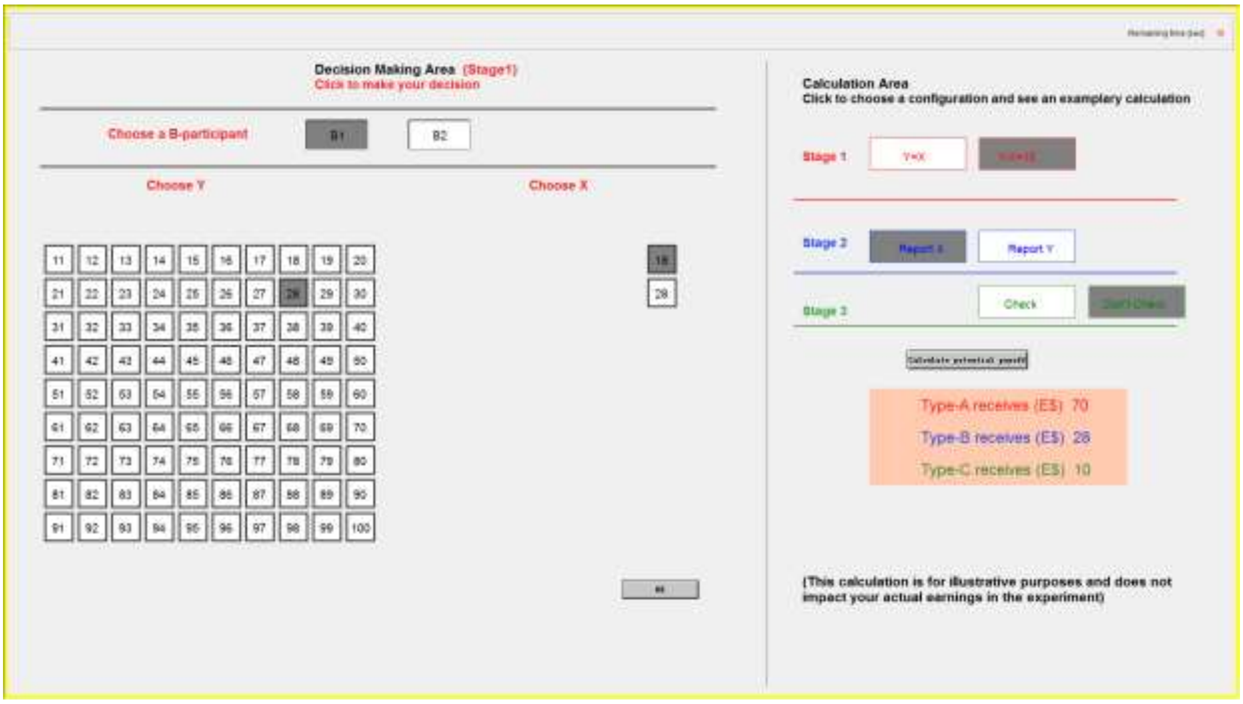
[18] Instructions in the parenthesis are to be deleted when use the payoff table in the corresponding treatment.

Screenshots

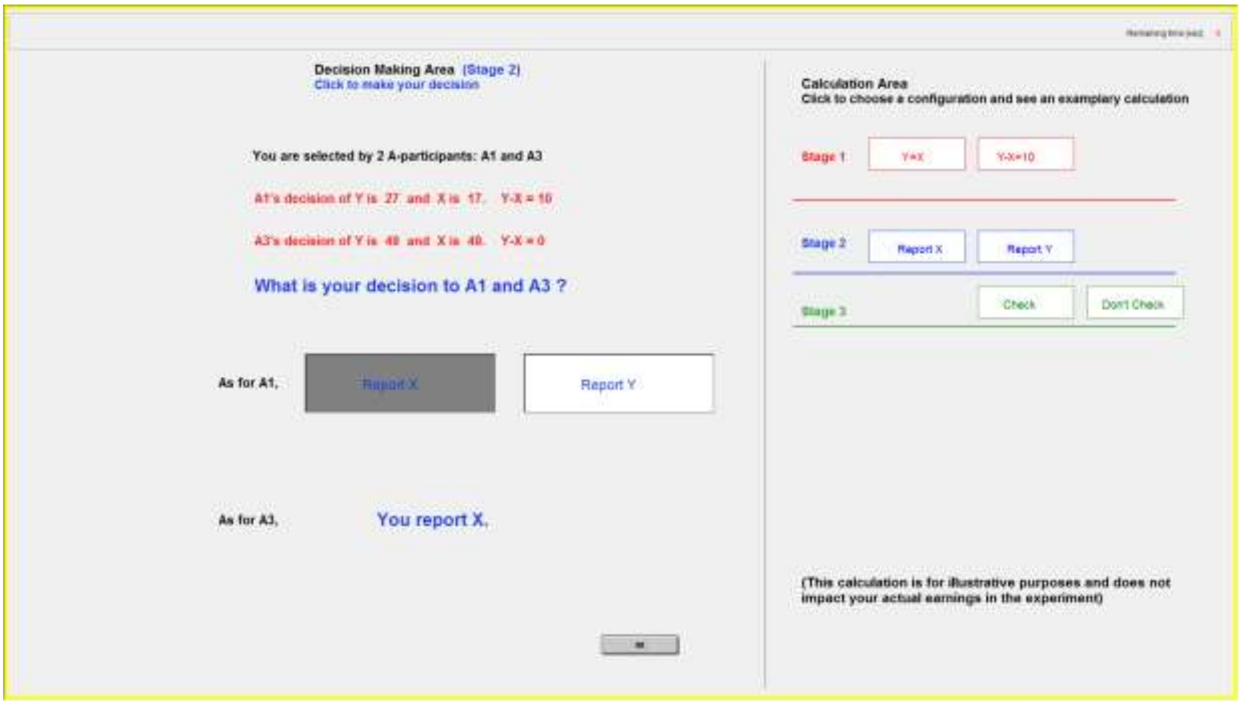
1. Information of Type and round number (see an example for A-participant).



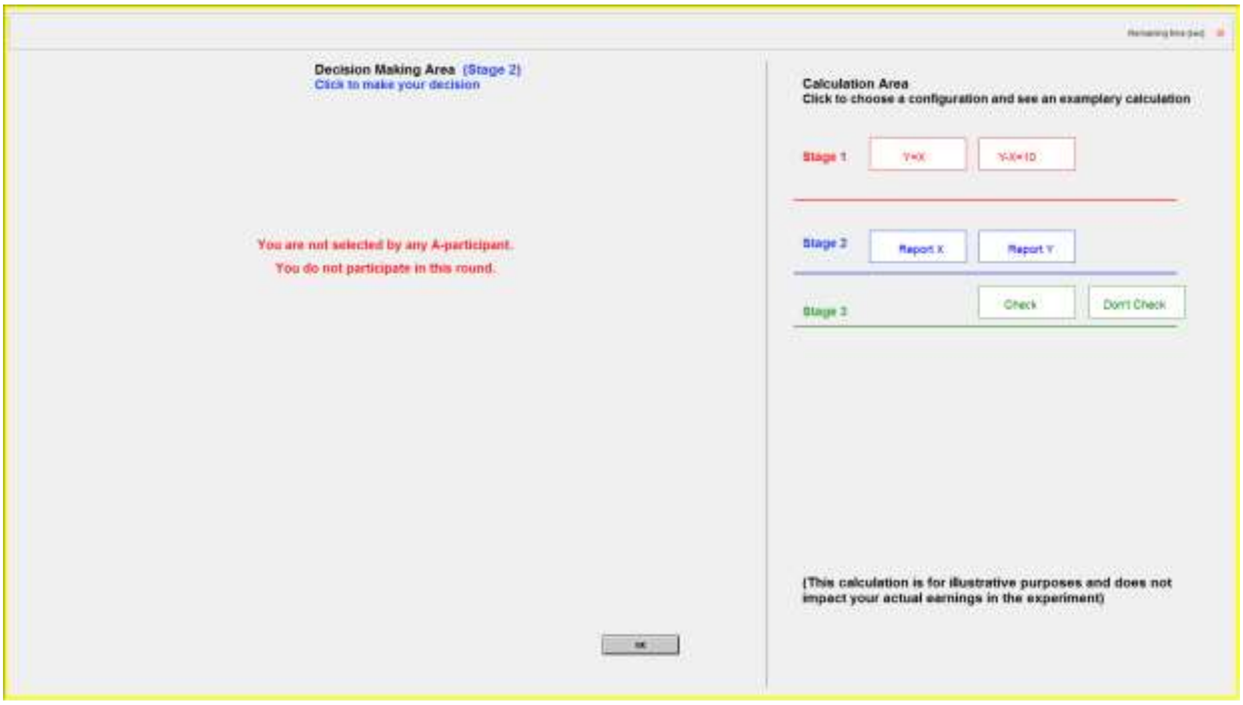
2. A-participant's decisions(see an example while the A-participant is using "Calculation Area").



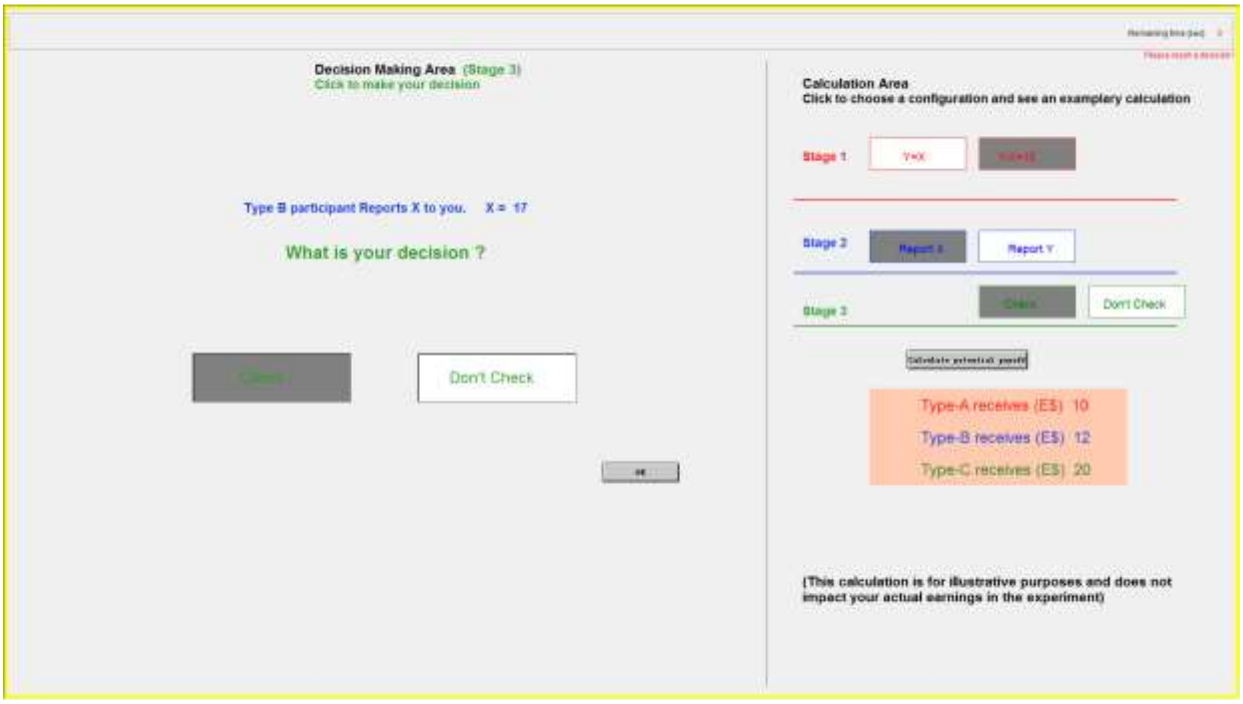
3. B-participant's decisions (see an example when the B-participant is selected by two A-participants).



4. A screenshot for B-participant if s/he is not selected.



5. C-participant's decisions if the matched B-participant reports X.



6. C-participant's decisions if the matched B-participant reports Y.

7. A summary of the decisions and payoffs in the involved interactions (e.g. B-participant).