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Improving Truthful Reporting of Polluting Firms by Rotating Inspectors: Experimental Evidence from a Bribery Game

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Abstract

We consider a two-layered review system of environmental regulation where a polluting firm periodically self-reports its emissions to a regulatory authority. The system typically requires a third party to verify the firm's report and, in addition, an official of the regulatory authority to spot-check. If there are potential gains from corruption, both the verifier and the official might be corruptible. Corruption is more likely in repeated-game situations, as suggested by the literature on corruption experiments. Our experimental design is motivated by the risk of under-reporting in emissions trading schemes where both the verifier and the official are corruptible and focuses on a situation with untruthful reporting and lax enforcement. Our test-bed is a three-player bribery game. We study how different types of rotation — a baseline of fixed matching, a complete rotation treatment, and two incomplete rotation treatments — affect untruthful reporting that requires collusion between three participants in a hierarchical structure. Our findings suggest that complete rotation improves significantly firms' truthful reporting and verifiers' truthful verification compared to situations where none is rotated, while incomplete rotation does not have such impact. In our experiment, none of the rotation treatments had a significant impact on the behavior of officials.

Keywords: truthful reporting, corruption, rotation, environmental regulation, experiment
JEL classification: *C92, D83, K42, Q58*

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1. Introduction

The truthfulness of self-reported information is essential for the credibility of systems such as tax compliance, food safety, health care, pollution control, and greenhouse gas reporting (McAllister, 2012; Hansen et al., 2014). Third-party verification and certification can help to increase the quality of reporting in such principal-agent situations. Numerous incidents suggest, however, that untruthful reporting can become systemic in such situations (Coates, 2007; Nyborg and Telle, 2006; Telle, 2013). For example, spot inspections by the relevant authority found evidence for non-compliance in verified emission reports in the European Union's Emissions Trading Scheme (EEA Technical Report 2015, EEA Report 2017). Moreover, incidents such as the collapse of Enron in 2001 or the Volkswagen scandal in 2016 demonstrate that deliberate manipulation of information can have severe consequences. The fact that third-party verifiers have enabled polluting firms to make money from fraudulent emission reductions has also been widely acknowledged in the academic literature (Russell, 1990; Bachram, 2004; Nyborg and Telle, 2006; Telle, 2013).

In the regulation of carbon emissions, under-reporting has been a longstanding concern (Betz and Sato, 2006; Neuhoff and Åhman et al., 2006; Neuhoff and Ferrario et al., 2006; Lohmann, 2009; and INTERPOL, 2013). Given that emissions trading schemes (henceforth "ETSs") either already include several jurisdictions, such as the European Union (EU) ETS, or are likely to link with others in the future (e.g., Switzerland, China, or Korea), there is an increasing risk that under-reporting is being strategically used across jurisdictions. When ETSs are in their initial stage of implementation in countries such as China or Korea, regulatory enforcement may be very lax and present a greater risk of untruthful reporting. Mechanisms that help ensure the principle of "a ton is a ton" are, therefore, even more important for climate change policies to be implemented with confidence.

Due to the characteristics of carbon emissions and asymmetric information regarding polluting firms' emissions, and because they are not suitable for continuous monitoring systems, climate policies rely heavily on third-party verifications and firms' self-reported information (Raymond and Cason, 2011).

Our study was motivated by the monitoring, reporting, and verification (henceforth "MRV") requirements[1] of ETSs. Like many other market-based regulations, ETSs pressure firms to internalize their external effects by setting a price on emissions. There are incentives for firms to reduce their compliance costs by legitimate reductions as well as by under-reporting emissions (McAllister, 2010).

To induce truthful reporting, the regulatory authority must carefully regulate the MRV process by qualifying verifiers, carrying out random checks on verified reports, and imposing penalties on violators. In this regard, MRV regulation in ETSs may involve inspections by both a third-party verifier and an official from the regulatory authority. We call this MRV regulation a two-layered review system, which - featuring three different players - typically requires the firm to report its emissions to a verifier, the verifier to check the report, and together with their findings to submit it to an official, and for the official randomly to check a firm's verified emissions report. Such two-layered review systems are widely used in many other situations, such as tax audits (Kleven et al., 2011).

However, several factors may undermine the effectiveness of the two-layered review system[2]. First of all, long-term interaction may foster corruption (Sobel, 1985; Tirole, 1986, 1988), since most ETSs allow the firm to choose and pay the verifier with no requirement to switch partners every few years, which is a common stipulation in accounting. Therefore, the verifier faces conflicting incentives either to reporting the actual emissions or, given the incentives to acquire long-term repeat business, to please the firm and conceal under-reporting when it occurs. Second, unlike the big four accounting

[1]Refers to "The Accreditation and Verification Regulation-Explanatory Guidance (AVR Explanatory Guidance (EGDI), version of 19 September 2012)," which is the European Commission's regulatory guidance document on the verification of greenhouse gas emission reports. Available at http://ec.europa.eu/clima/policies/ets/monitoring/docs/exp_guidance_1_en.pdf
[2]The polluting firm has, essentially, two ways of untruthful reporting, either over-reporting emissions (in the case of free allocation) to receive more allowances or under-reporting to avoid buying allowances or be allowed to sell permits. Our focus is on under-reporting because it is the primary concern of noncompliance in ETSs and it simplifies the situation in this paper.

firms (PricewaterhouseCoopers, Deloitte, Ernst & Young, and KPMG) that have high reputational costs to fear if they shirk, verifiers in most ETSs tend to be small firms. Third, enforcement may not be effective since the probability of an official's random check and the penalty for under-reporting are not clearly specified. Based on Becker's theory (1968), this may lead to situations where firms and verifiers are not able to estimate the risk and cost of their transgressions. The findings by Craswell and Calfee (1986) suggest that firms are likely to be untruthful if they believe there is a high potential of uncertainty. Fourth, the official usually has limited resources for compliance surveillance, can only levy a limited penalty on violators (Heyes, 2000), and may have been pressured or even bribed by polluting firm lobbyists to safeguard economic development (Peeters, 2006).

Although a corrupt official and verifier are not necessary preconditions for untruthful reporting, it may be a relevant ETS scenario in countries with high corruption perception index ratings (Transparency International 2019). It may be an even more relevant scenario in the case of ETSs which involve different countries/areas with competing industries (Van Asselt and Brewer, 2010). Indeed, there is some evidence of imperfect enforcement. For example, Nyborg and Telle (2004, 2006) observe that when enforcement is weak, violations will flourish. Ivanova (2010) shows empirically that the level of corruption in society influences the pollution data reported by polluting firms. Therefore, if both the official and the verifier are corrupt and inspections substandard, the under-reporting problem could be potentially severe, yet still remain undetected.

Despite the plausibility of these factors, it is unclear how the situation can be improved by changing the system. We use a laboratory experiment to study the incentives of firms, verifiers, and officials for truthful reporting to be elicited from a typical ETS with imperfect enforcement. The experiment allows us to identify behavioral incentives for untruthful reporting and to test the effectiveness of policies that might enhance truthful reporting while stripping away the hidden nature of corruption. The main objective of our experiment is to ascertain whether subjects report more truthfully in a complete rotation condition where subjects are randomly matched in each period compared to the baseline, where no rotation takes place and subjects remain in the same group throughout the experiment.

Job, or staff, rotation has been suggested and adopted as a precautionary measure in public administrations for situations where social relationships could have undesirable consequences. For example, the World Bank (2000) suggested using rotation to combat tax evasion and corruption in tax administration, the German federal government adopted staff rotation in public procurement (Abbink, 2004), and China practices staff rotation for higher-level government officials to curb factionalism and promote economic growth (Huang, 2002; Zhang and Gao, 2007). Nevertheless, with the exception of Abbink (2004), there is limited empirical evidence about the effectiveness of rotation and, in particular, no evidence concerning the effectiveness of rotation in two-layered review systems.

To explore the effects of rotation systematically, we implemented complete rotation (henceforth "CR") as our primary treatment, and two incomplete rotation treatments to explore whether unilateral rotation affects truthful reporting. In the CR treatment, we impose random matching on both verifiers and officials, and in the two incomplete rotation treatments we impose random matching on either verifiers (henceforth "RV") or officials (henceforth "RO"), separately.

We contribute to the literature on the monitoring and enforcement of environmental and other social regulations in three ways. First, we experimentally investigate the effect of rotation in a hierarchical two-layered review system while focusing on a situation with lax inspections by both the middle- and top-layer inspectors. Second, unlike previous studies that assume the top layer is strongly committed or that the probability of regulatory inspection is exogenous, random inspections by officials in our experiment are not predetermined but, instead, are reflections of officials' decisions. Third, we address the effectiveness of third-party verification in the context of ETSs, which has previously not been studied.

We find that CR treatment can significantly enhance truthful reporting even if both inspection layers are slack. Although our experiment to study the effect of staff rotation is motivated by ETSs, we use neutral language, so our findings are transferable to other systems such as environmental certification, tax administration, or financial regulation, where self-reporting is required and two-layered review systems are used.

The remainder of this paper is organized as follows. In Section 2, we describe the background of the MRV process of emissions trading schemes. In Section 3, we provide an overview of related literature. In Section 4, we describe the experimental design, present our hypotheses, and explain the implementation procedure. In Section 5, we present our experimental findings, and in Section 6 we offer some concluding remarks.

2. Background and Motivation for This Study

2.1 Compliance Processes in ETSs

ETSs typically involve three parties in their compliance processes. The first is a polluting firm that is under obligation to hold permits to cover its emissions. It has to submit one allowance for each unit of emissions. The second is an independent verifier that can be either an accredited legal entity or a certified natural person[3]. The third is an official, often referred to as a "competent authority," who is in charge of regulation enforcement[4]. Figure 1 shows a typical ETS compliance process[5].

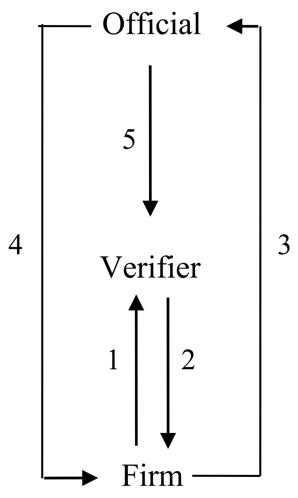

Figure 1: A typical ETS compliance process

The process starts with a firm drafting an emissions report and requesting a verifier to assess the report (Arrow 1). A verifier will assess the method and data sources used in the report, judge the report to be either satisfactory or unsatisfactory, and send the result to the firm (Arrow 2). Having received the verification result, the firm must submit both its original report and the verification result to the official and submit the corresponding amount of allowances to cover its emissions (Arrow 3). Finally, the official may carry out some random inspections on the firm (Arrow 4) and the verifier (Arrow 5) to ensure the truthfulness of the report and the credibility of the verification.

2.2 Reporting Requirements in Practice

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[3] A "natural person" is the opposite of a legal person who may be a legal organization, while the natural person is just a human being who is a capable and certified verifier.

[4] In the European Union ETS, there is an additional international accreditation body on the supranational level which acts as an additional official (European Commission, 2012). Our experimental setting is a representative of this compliance process. We assume the existence of one official only to make our model and experiment manageable.

[5] In Figure 1, we focus on the process of MRV. Elaborations of activities such as firms drafting monitoring plans, market trading of allowances - to name but a few - are omitted for simplicity. See "The accreditation and verification regulation-explanatory guidance (AVR Explanatory Guidance (EGD I), Version of 19 September 2012)" page 7-10, available at http://ec.europa.eu/clima/policies/ets/monitoring/docs/exp_guidance_1_en.pdf

Table 1 in online Appendix B summarizes the reporting requirements in the eight ETSs that are currently in place. Almost all schemes have established guidance for MRV. This reflects the widely perceived importance of stipulating the responsibility of each party in the compliance process. There are three common features. First, all schemes, except for the New Zealand and the Swiss ETS, require verification of a firm's self-reported emissions by a third-party verifier, and almost all schemes allow polluting firms to choose their own verifiers (Columns 4 and 5). Second, the majority of them require inspection by an official. However, if such an inspection is required, details such as how often and how long the inspection should be, tend to be under-specified. Third, the penalties for misreporting are ambiguous in some of the schemes and thus weaken the enforcement of the system.

As a result, the MRV guidance cannot guarantee that either the official's inspection or the verifier's assessment has been carried out in a satisfactory manner. If both the official and the verifier have a financial incentive to under-report, the ETS may end up with deficient inspections, the level of under-reporting may be high but remain undetected, and even the threat of severe penalties for under-reporting may be ineffective. Under-reporting will lower the market price of allowances compared to a situation with honest and comprehensive reporting.

3. Related Literature

Experimental studies about the enforcement of ETS have identified various ways to induce perfect compliance. These include economic incentives such as direct and indirect effects of intensive monitoring and high penalties, correlation between targeted enforcement and a firm's characteristics (Murphy and Stranlund, 2006, 2007), complementarities of the monitoring probability and the supply of permits (Caffera and Chávez, 2012), the distinct effects of penalties on misreporting and permit violations (Stranlund et al., 2011), the trade-off of banking in an emissions trading market (Cason and Gangadharan, 2006), and non-economic incentives such as the effect of perceived fairness on honest reporting (Raymond and Cason, 2011). Despite all of this, the problem of corruption-backed, dishonest reporting has been largely ignored. If the reviewers (verifiers and/or officials of the regulatory authority) are corruptible, the effects of factors that induce more truthful reporting might be weakened.

Corruption-backed untruthful reporting is based on trust and reciprocal cooperation between interacting parties. Experimental results based on laboratory data indicate that, in order to deter corruption, it is crucial to minimize interaction between the inspector and the inspected. For example, Wu and Abbink (2017) show that the anti-corruption effect of rewards is potent when future interaction is not expected. Along this line, rotation (or random matching) has been argued to undermine trust and prevent corruption since, in theory, it shortens interactions and increases uncertainty among participants (Tirole, 1986, 1988; Bac, 1996). A number of theoretical studies also support the hypothesis that a long-term relationship fosters trust, and rotation-induced uncertainty makes people less likely to engage in corruption (Sobel, 1985; Ryvkin and Serra, 2012).

Despite these insights, there is considerable evidence of cooperation (in our context, corruption is a form of cooperation) in anonymous one-shot games (see Abbink 2004 and references therein), which suggests that long-term repeated interaction might not be a necessary condition for cooperation. Gächter and Falk (2002), for example, find by way of a gift-exchange game that repeated interaction strengthens reciprocal cooperation. Moreover, Abbink (2004) on modified trust games, Duffy and Ochs (2009) on repeated prisoners' dilemma games, and Duflo et al. (2013) through a field experiment on pollution auditing, demonstrate that rotation has a significant negative effect on cooperation. Thus, we conjecture that staff rotation might weaken reciprocal collusion, even if it cannot eliminate corruption completely.

With respect to the two-layered review system, reciprocal cooperation or corruption involves three actors (from here on "subjects"). Unlike the experiments mentioned above, where the reciprocal cooperation is between two subjects and rotating either subject could produce complete anonymity, the effect of rotation in the two-layered review system might depend on the level of anonymity. To have the same level of anonymity as in the case of two players, we implemented "complete rotation" (rotating both verifiers and officials) as our primary treatment. Cooperation among three players is more challenging than between two players, as the former needs more coordination. Therefore, we conjecture that the complete-rotation treatment would be at least as effective as the rotation between two players, as demonstrated in the literature. In addition, we had two incomplete-rotation treatments in which only the verifiers or the officials were rotated from period to period. We designed these conditions to test whether the rotation effect still holds when only one of the inspectors is rotated.

Our work is most closely related to Abbink et al. (2002) and Abbink (2004). These authors explore the reciprocity relationships between briber and bribee in a trust game with fixed matching and random matching separately. In their experiment, the probability of corruption being discovered is fixed. Moreover, it seems that a fixed probability of regulatory inspection is commonly adopted for studies focusing on corruption, e.g., Armantier and Boly (2011). However, in reality the regulatory inspection is often endogenous, and the official has an incentive to relax enforcement due either to inadequate resources for inspection or an immediate benefit (bribe) from under-reporting. Therefore, we do not assume that officials have a strong commitment to fix the probability of inspection.

4. Design of Study

In Section 4, we first describe a three-staged sequential game (Subsection 4.1) and then develop the associated Perfect Bayesian Equilibrium (henceforth PBE) under the baseline treatment before deriving the relevant empirical hypotheses based on comparative statics of a typical PBE for the rotation treatments in Subsection 4.2. The implementation procedure is presented in Subsection 4.3.

4.1 The Game

Our design builds on Abbink et al. (2002) by adding a second layer of reviewer while capturing the essential aspects of the monitoring regulation in ETSs. Three types of players — assumed to be risk-neutral — are therefore involved: a polluting firm, a third-party verifier, and an official. Consider the following three-stage sequential game as illustrated in Figure 2.

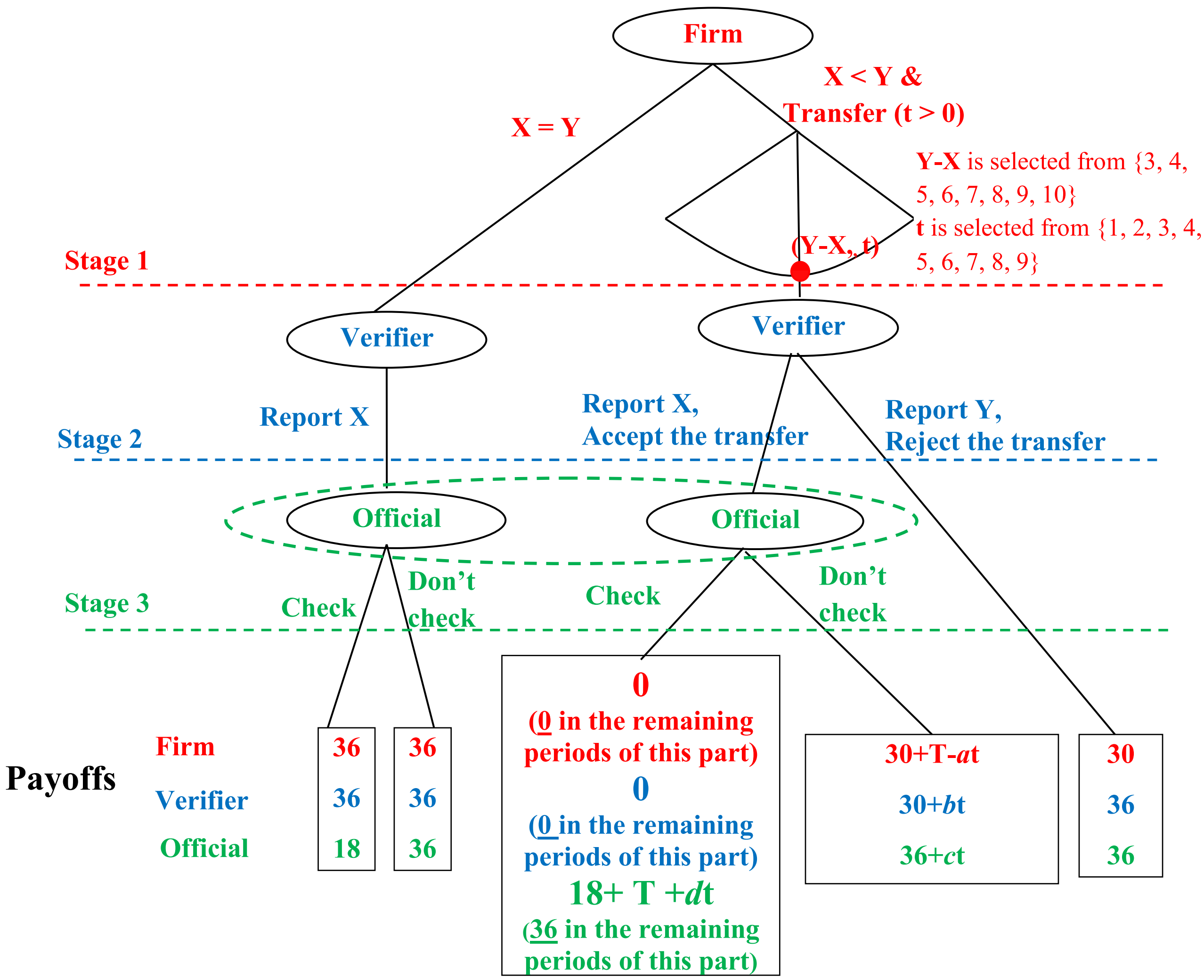

Figure 2: Decision situations in a period (price P>0)

Notes: P > 0 is the market price of allowances and T is the firm's earnings from under-reporting, i.e., $T = (Y\text{-}X) \times P$. The official's payoff for detecting under-reporting should not be larger than the group payoffs with successful under-reporting otherwise the official might end up over-checking. Therefore, we set the parameters to satisfy $T+dt-ct-18 \leq 60+T-at+bt$.

Stage 1. The firm first decides whether to report truthfully or untruthfully. The firm then selects two numbers from 1 to 100. The first number (Y) represents the number of emissions, and the second number (X) represents the number of reported emissions, which also equals the number of submitted allowances. Therefore, under-reporting is when X<Y. In addition, under-reporting generates additional earnings of T, which represents an opportunistic benefit: either reducing the number of allowances the firm needs to buy or increasing the number of excess allowances that the firm can sell at a uniform market price P. Therefore, T = (Y-X) × P. In our setting, the firms are identical in terms of additional earnings from under-reporting.

We divided each session into three identical parts, with six periods per part. If a firm's under-reporting is detected, the firm is not allowed to participate in the remaining periods of that part, although it can participate again in the next part.[6]

[6] In our experiment, we divided subjects into subpopulations of nine subjects, which were further sub-divided into three groups of three. The price P, which was initially set at 6, would be reduced by 2 in the following period if the total under-

Under-reporting is modelled as a precondition for corruption, so if an under-reporting decision is made, the firm must also specify a bribe t from 1 to 9. To influence both the verifier's and the official's decisions, it is expected that a higher level of under-reporting is associated with a higher level of bribe. Moreover, offering a bribe incurs a cost of 6 for the firm regardless of its consequences. As justified in Abbink et al. (2002), the transfer cost is considered as an initiation cost used to establish a relationship with potential bribees. If the verifier accepts the bribe, the firm's payoff is reduced by a×t, where the value of factor a here reflects the marginal disutility to the firm from the act of bribery. Therefore, untruthful-reporting could lead to a payoff of 30+T-at to the firm if under-reporting is not detected by verifier and official.

Stage 2. The verifier observes the firm's decision and reports his verification result to the official. To simulate the negotiation between the firm and the verifier, we allow free-form communication between them using a chat-box (60 seconds) before they make their decisions[7]. If the firm chooses to report truthfully, the verifier will report X which represents a satisfactory result. If the firm chooses under-reporting with a bribe, the verifier must choose either to accept the bribe and report X, or reject the bribe and report Y. Note that there are no other alternatives, such as the verifier accepting the bribe and reporting Y or rejecting the bribe and reporting X. This is because we simplified the game; if the verifier accepts the bribe, they are classified as "corrupt". In our experiment, a corrupt verifier will report X and conceal under-reporting, while a verifier that is not corrupt will report Y and reveal the under-reporting. To report X for under-reporting incurs a concealing cost of 6 for the verifier. Furthermore, if the verifier accepts the bribe, his payoff will be increased by b×t, where the factor b reflects the verifier's marginal utility of the bribe. Thus cooperation with an untruthful firm could lead to a payoff of 30+bt to the verifier if under-reporting is not detected by the official.

Stage 3. The official's decision depends on the verifier's report. If the verifier reports Y (unsatisfactory), the official is informed of the under-reporting problem, and the game ends. This is the main advantage of third-party verification as it saves the official's inspection costs. If the verifier reports X (satisfactory), the official has to decide whether to check the report further in order to determine whether the firm (and the verifier) have under-reported. As report X is associated with either truthful reporting or untruthful reporting, officials may base their decisions on experience. In this way, we endogenize the official's inspection decision.

We assume that if the official chooses to conduct a check, under-reporting and bribery will be detected. Conditional on under-reporting being detected, the official will forfeit the additional earnings T and d×t from the firm and the verifier, respectively. This parallels the actual situation where violators would be punished with a financial penalty at least as high as the market value of the shortfall in permits (Stranlund et al., 2011). The factor d reflects the official's marginal utility of forfeiting a transfer. Meanwhile, both the firm and the verifier receive a penalty such that their payoffs are reduced to zero.[8] We assume that checking is costly to the official and reduces her payoff by 18. As a result, checking might lead to a payoff of (36 – 18)+T+dt to the official if she detects

reporting of a subpopulation exceeded a certain level in a period (15 units in our setting). The subjects were informed that "P is initially set at 6, and may decrease over time. This depends on your group's decision and decisions of other groups. The larger the aggregate amount of Y-X, the more likely it is that P decreases in the following round." At the beginning of the next part, the price was reset to 6.

[7] We did not pre-structure the communication but insisted that subjects not reveal their identity. Communication between the players injects some realism, since in the MRV procedures of ETSs verifiers also talk to firms. Some schemes even require a timely communication between verifiers and firms in case of non-conformity and misstatement, but there is no such requirement between firms and officials (Mes et al., 2016). Therefore, we do not allow for communication other than that between verifiers and firms as it seems harder for either one of them legally to approach officials. The usage rate of chat-boxes did not differ much (with 75.6% in the baseline, 65.7% in the CR treatment, 65.6% in the RO treatment, and 78.5% in the RV treatment). Across all treatments, and conditional on not being caught by the official, subjects entered on average 2-3 lines per period in their chat-boxes to reach an agreement.

[8] Once under-reporting has been discovered by the official, both the firm and the verifier are prohibited from further participation in that particular part. This penalty is the most severe punishment in a lab experiment, which corresponds to the situation that corrupt stakeholders lose all their earnings and receive imprisonment if discovered (see Table 1 in online Appendix B; for instance, the ETS scheme in Canada imposes a prison term for misreporting). For the remaining periods of that part, the official will receive 36 per period as a salary.

under-reporting. If the report is found to be truthful, both the firm and the verifier receive 36 and the official receives 18. When the official chooses not to check, truthful-reporting gives 36 for all players, and under-reporting would lead to a higher payoff (i.e., 36+*c*t) for the official. Here *c* reflects the official's marginal utility of receiving a transfer, and $c\times t$ is an ex-post bribe to the official, for instance, because the official waits until the very last minute to accept a bribe so as to appear not too eager.

Given our interest in under-reporting (and the relative advantages of the different rotation schemes), we choose parameters that favor under-reporting relative to truthful reporting for all players. In summary, our main incentives in the sequential game in Figure 2 are as follows:

i. Firms and verifiers can develop direct reciprocal cooperation since the bribe is directly transferred from firms to verifiers.
ii. The official's checking decision is endogenous and is reflected by that official's individual decision in each period.
iii. Officials may cooperate with firms and verifiers only implicitly, as they do not observe a bribe transfer before making any decision.
iv. Officials have little to lose if they do not check, so there is not a strong incentive to do so. This results in the ineffective enforcement of the review system.

Matching conditions. In this study, the sequential game depicted in Figure 2 is common to all treatments. However, we differentiate between four types of matching conditions: fixed matching, complete rotation, rotation of verifiers, and rotation of officials. The fixed-matching condition is our baseline, where no rotation is applied, meaning that each subject remains in the same group throughout the whole experiment. The complete-rotation (CR) condition is our primary treatment where both the verifier and the official are simultaneously rotated and randomly matched in each period to a different group. The rotation of verifiers (RV) and the rotation of officials (RO) are two incomplete-rotation treatments where either only the verifiers or only the officials are subjected to random matching among different groups, separately. RV and RO treatments are supplements to the CR treatment meant to tease apart the relative importance of the actions of verifiers and officials.

We assume that rotation treatments introduce heterogeneous corruptibility and thus heterogeneous marginal (dis)utility of the bribe transfers of participants. Moreover, the variation and uncertainty of opponents' corruptibility introduced by rotation result in different corruptibility and different highest or lowest acceptable bribes of participants compared with their counterparts in the baseline. As a result, for the same numerical values of corruption-backed payoffs, participant-perceived values might no longer be the same. In other words, participants' marginal (dis)utility of bribe transfers might also vary with rotation.

To model the rotation procedure, we consider the variation of participants' (subjective) marginal (dis)utility pertaining to bribe transfers under different matching conditions. It is recognized that uncertainty about the corruptibility of a potential partner results in lower levels of corruption (Ryvkin and Serra, 2012). Therefore, the uncertainty introduced by rotation might result in less untruthful reporting. The intuition is that in an uncertain world, as long as the firm is unsure about the inspectors' (either the verifier's or the official's) corruptibility, or it is impossible for the firm to foster future cooperation with either the verifier or the official, the firm's perceived risk of being rejected by a verifier or checked by an official will be higher under a rotation treatment. As a result, the firm's corruptibility will be reduced, and at the same time its marginal disutility of corruption will be increased. In other words, the firm's marginal disutility of initiating a bribe transfer (*a*) becomes higher in both the complete and incomplete rotation treatments (CR, RV, and RO). This, conditional on the same level of under-reporting and bribe transfer, makes the firm perceive the value of corruption-backed payoff to be smaller than the numerical value. A firm's payoff decreases in *a* will then lead to more truthful reporting. Likewise, when the verifier is not expected to interact with the same firm again in the future, his lowest acceptable bribe will be higher, and his corruptibility is likely to be lower with rotation. Consequently, the verifier's (subjective) marginal utility of receiving a bribe transfer (*b*) becomes smaller with the RV and CR treatments when compared with the baseline. In the same way, the official's (subjective) marginal utility of receiving a bribe transfer (c) becomes smaller and her (subjective) marginal utility of forfeiting a transfer (d) becomes higher when the official is rotated and not expected to interact with the same firm in the future (RO and CR).

4.2 Theoretical Analysis

Considering the sequential game with imperfect information depicted in Figure 2, in this subsection, we first derive the Perfect Bayesian Equilibrium (PBE) of our baseline by using backward induction, and then derive the predictions for the effect of rotation treatments based on a comparative-statics analysis while taking into consideration the variation of participants' (subjective) marginal (dis)utility of bribe transfers under rotation treatments. For tractability, we compute the PBE on the assumption of risk neutrality as a first approximation and benchmark.[9] Since the game is finitely repeated with participants of different types moving sequentially in each round, the PBE of each round is the same as that for a one-shot game.

We assume that a polluting firm chooses truthful reporting with probability p and under-reporting with probability 1-p; a verifier chooses to cooperate and conceal under-reporting (Report X) with probability q, and to report Y with probability 1-q, and an official chooses to check with probability h, and not to check with probability 1-h. Therefore, on receiving Report X, the official's belief of truthful reporting is $\mu = p/(p+(1-p)q)$ if $p+(1-p)q>0$, and μ is equal to any $x \in [0,1]$ if $p+(1-p)q=0$. The latter implies $p = q = 0$, which means that a polluting firm chooses under-reporting, a verifier chooses to reject the transfer and report Y, and the game ends.

Furthermore, we use the subscripts "O", "V", and "F" to represent official, verifier, and firm, respectively, and obtain their expected utility as follows.

$$EU_{\boldsymbol{O}} = ((1-\mu)(T+dt-ct)-18)h+36+(1-\mu)ct, \quad (1)$$

$$EU_{\boldsymbol{V}} = (1-p)((1-h)(30+bt)-36)q+36, \quad (2)$$

and $EU_{\boldsymbol{F}} = (30qh-q(1-h)(T-at)+6)p+C, \quad (3)$

where $C := 30(1-q)+q(1-h)(30+T-at)$.

We derive the interior solution of the Perfect Bayesian Equilibrium of the baseline by using backward induction, which we define as $((p^*,1-p^*),(q^*,1-q^*),(h^*,1-h^*))$ and $(\mu^*,1-\mu^*)$ (see online Appendix A). In total, there are seven PBEs for this game if $T \geq \max\{18+ct-dt,\ at+bt\}$ is satisfied.[10] One typical PBE is given by

(i) $p^* = ((30+bt)(T+dt-ct-18))/((30+bt)(T+dt-ct-18)+18(6T-6at-5bt+30))$,

(ii) $q^* = (30+bt)/(30+bt+6(T-at-bt))$,

(iii) $h^* = (bt-6)/(30+bt)$,

---

[9] Note that the Perfect Bayesian Equilibrium of the sequential game is not easily computable without making an assumption regarding the participants' risk preferences. In theoretical studies on collusion and monitoring, it is common to start the analysis with risk neutrality (Laffont and Martinmort, 1997; Krasa and Villamil, 1992). Moreover, the risk preference will only affect the payoff function, and will not change the PBE substantially.

[10] We are particularly interested in the case where $T+dt-ct\geq 18$, as it is the most interesting situation where the additional earnings are high enough to make checking worthwhile. To ensure $0 \leq q^* \leq 1$, $T-at-bt \geq 0$ has to be satisfied, which means that the firm's payoff is at least as high as the payoff of the verifier if he accepts the bribe. As a consequence, it is necessary to have the condition $T \geq \max\{18+ct-dt,\ at+bt\}$ satisfied.

and (*iv*) $\mu^* = 1 - 18/(T + dt - ct)$.

We now examine the comparative statics of the typical PBE with respect to participants' (subjective) marginal (dis)utilities. The results are summarized as follows,

(i) $\frac{\partial p^*}{\partial a} > 0$ and $\frac{\partial p^*}{\partial b} > 0$; *(ii)* $\frac{\partial q^*}{\partial a} > 0$ and $\frac{\partial q^*}{\partial b} > 0$; *(iii)* $\frac{\partial p^*}{\partial c} < 0$ and $\frac{\partial p^*}{\partial d} > 0$; *(iv)* $\frac{\partial h^*}{\partial b} > 0$.
Formally, we obtain the following proposition.

Proposition 1. An increase in *a,* or an increase in *b*, or a decrease in *c*, or an increase in *d* leads to a higher probability of truthful reporting by firms. An increase in *a*, or an increase in *b,* leads to a higher probability of dishonest verifications by verifiers. Moreover, a decrease in *b* leads to a lower probability of checking by officials.

For proof of Proposition 1, refer to online Appendix A.

Proposition 1 suggests that, in the equilibrium, holding all else constant, (i) when the firm's marginal disutility of initiating a bribe transfer subjectively increases, its perceived value of the payoff from under-reporting will decrease, and it will in turn report truthfully more often. Moreover, conditional on a higher *a*, observing under-reporting might imply a stable collusion relationship between the firm and the official for the verifier, who will then verify dishonestly more often. (ii) When the verifier's marginal utility of receiving a bribe transfer decreases subjectively, his perceived value of the payoff for dishonest verification will decrease, and then he would verify honestly more often, which lowers the expected payoff for the firm, leading to less untruthful reporting and, at the same time, the official will check less. (iii) When the official's (subjective) marginal utility of receiving a bribe transfer decreases and her (subjective) marginal utility of forfeiting a transfer increases, her perceived value of the payoff for not checking will be smaller, and her perceived value of the payoff for checking will be higher, respectively. This results in a higher probability of checking. As a result, both the verifier and the firm will be more honest.

Next, based on the comparative statics, we summarize the effects of rotation treatments on a firm's probability of truthful reporting, a verifier's probability of honest verification, an official's probability of checking, as well as the rotation effect on the level of under-reporting and the level of selected transfer in all rotation treatments. For further details, please refer to all the proofs in online Appendix A.

(i) RV treatment induces an increase in a and a decrease in b. Therefore, the effect of RV on the probability of truthful reporting by firms is $\Delta p^*_{RV} = \frac{\partial p^*}{\partial a} - \frac{\partial p^*}{\partial b} \le 0$ since $T \ge at + bt$.

The effect of RV on the probability of dishonest verification by verifiers is $\Delta q^*_{RV} = \frac{\partial q^*}{\partial a} - \frac{\partial q^*}{\partial b} < 0$.

Additionally, the effect of RV on the probability of checking by the officials is $\Delta h^*_{RV} = \frac{\partial h^*}{\partial b} > 0$.

(ii) RO treatment induces a decrease in c and an increase in d. Thus, the effect of RO on the probability of truthful reporting by firms is $\Delta p^*_{RO} = \frac{\partial p^*}{\partial a} - \frac{\partial p^*}{\partial c} + \frac{\partial p^*}{\partial d} > 0$. Moreover, the effect of RO on the probability of dishonesty verification by verifiers is $\Delta q^*_{RO} = \frac{\partial q^*}{\partial a} > 0$.

(iii) Now we analyze the effects of CR treatment with which there is an increase in a, a decrease in b and c, and an increase in d.

First, the effect of the CR treatment on the probability of truthful reporting by firms is

$$\Delta p_{CR}^* = \frac{\partial p^*}{\partial a} - \frac{\partial p}{\partial b} - \frac{\partial p^*}{\partial c} + \frac{\partial p^*}{\partial d} > 0$$ since $T + dt - ct - 18 \leq 60 + T - at + bt$ and $T \geq at + bt$ hold.

Second, the effect of the CR treatment on the probability of dishonest verification by verifiers is

$$\Delta q_{CR}^* = \frac{\partial q^*}{\partial a} - \frac{\partial q^*}{\partial b} < 0 .$$

Third, the effect of CR treatment on the checking probability by officials is $\Delta h_{CR}^* = \frac{\partial h^*}{\partial b} > 0$.

(iv) We analyze the effects of rotation treatments on the level of under-reporting $\Delta x \equiv Y - X$ and the level of bribe transfers t on the PBE path. Given the official's belief of truthful reporting μ^*, we have $\Delta x^* = \frac{18}{(1-\mu^*)P} + \frac{ct - dt}{P}$ and $t^* = \frac{1}{d-c}[\frac{18}{(1-\mu^*)} - \Delta x P]$. Moreover, holding all else constant, we obtain $\frac{\partial \Delta x^*}{\partial c} > 0$, $\frac{\partial \Delta x^*}{\partial d} < 0$; and $\frac{\partial t^*}{\partial c} > 0$, $\frac{\partial t^*}{\partial d} < 0$. This suggests that, on average, both the level of under-reporting and the level of bribe transfers decrease with the CR and RO treatment compared with the baseline. Note that Δx^* and t^* are not affected by the changes in a and b suggesting that both the level of under-reporting and the level of bribe transfers, on average, would not change substantially with the RV treatment compared with the baseline. We now summarize all the testable empirical hypotheses, starting with our primary treatment, i.e., the complete rotation treatment.

H1 (CR treatment). *In the CR treatment, both the frequency of truthful reporting and the frequency of honest verification exceed their baseline counterparts, the frequency of checking is lower than its baseline counterpart, and both the level of under-reporting and the level of bribe transfers are lower than their baseline counterparts.*

H2 (RV treatment). *In the RV treatment, the frequency of truthful reporting does not exceed its baseline counterpart and there is no effect on the level of under-reporting and the level of bribe transfers. Moreover, the RV treatment induces a higher frequency of honest verification and a lower frequency of checking relative to the baseline.*

H3 (RO treatment). *In the RO treatment, the frequency of truthful reporting and the frequency of dishonest verification are both higher than their baseline counterparts, while there is no effect on the frequency of checking. In addition, both the level of under-reporting and the level of bribe transfers are lower than their baseline counterparts.*

Because the CR treatment leads to the highest level of uncertainty regarding other group members' corruptibility, by adding up all the possible variations of marginal (dis)utilities, the relevant comparative-statics analysis summarized in Proposition 1 indicates that the CR treatment has the most significant increase in the probability of truthful reporting. Furthermore, focusing on the probability of truthful reporting, Hypotheses H2-H3 state that it is possible that the effect of the RO treatment is greater than the effect of the RV treatment. This is possible because the incentive for corruption between the matched firm and the verifier relies on their perceived corruptibility of the official (Carrillo, 2000; Laffont and Martimort, 1997).[11] Moreover, the RO treatment introduces uncertainty on the corruptibility of the matched official, while the RV treatment does not have such uncertainty as long as the firm perceives the fixed official as corruptible.

[11] Carrillo (2000) and Laffont and Martimort (1997) show that in a hierarchical organization, corruptible senior managers encourage corruption at the lower levels.

4.3 Implementation

The experiment was conducted in the UNSW Business School's Experimental Research Laboratory[12] and programmed in zTree (Fischbacher, 2007). Overall, 198 participants took part and they were recruited from a pool of students registered on the Online Recruitment System for Economics Experiments (Greiner, 2015). Each participant participated in one treatment only. In total, 54 participants participated in each of the baseline and CR treatment, and 45 participants participated in each of RV and RO treatment.[13]

In the experiment, we fixed the values of participants' marginal (dis)utilities across all treatments. In particular, we set the parameters by following Abbink et al. (2002), i.e., a=2, b=3, c=3, and d=3. Even though with rotation treatments participants' (subjective) marginal (dis)utilities of bribe transfers vary with their expectations of their opponents' corruptibility, the values we set can be viewed as the objective values serving as a benchmark for participants to evaluate. In fact, even though the marginal (dis)utilities were fixed in the experiment, the subjective variation of them by participants is reflected by their decisions about bribe exchanges and whether to report truthfully or not. Moreover, fixing the values of marginal (dis)utility of bribe transfers gives us the advantage of focusing on participants' bribery decisions.

Each treatment had a multiple of nine participants, and each set of nine participants constituted a subpopulation divided equally into three groups. Each group consisted of one polluting firm, one verifier, and one official.[14] This partition was done to increase the number of genuinely independent observations and follows well-established precedent including the fact that participants were not informed about it. In rotation treatments, verifiers and officials were randomly matched in each period to a different group within the same subpopulation.[15] At the end of each period, participants were informed of their payoffs in that period as well as all the decisions made by the participants they were matched with in that period.

Each session included three identical parts with six periods per part. The first period of Parts 2 and 3 is a restart of the game. Figure 3 in the online Appendix B shows the implementation procedure. At the beginning of a session, the general instruction was read aloud by the same experimenter, and the written instructions were handed out to them. We asked participants to read the detailed instructions for themselves. We used context-free language in the instructions;[16] in particular, the terms "polluting firms", "verifiers", and "officials" were replaced by "Type-1", "Type-2", and "Type-3", respectively. Participants were told that Type-1 is a producer of a fictitious product and must buy a permit for each unit of production. In addition, "bribe" was replaced by "positive transfer". To facilitate participants' understanding of this complicated game, we guided them through two hypothetical examples and asked them to complete three comprehension questions before the experiment started. At the end of each period, payments were calculated based on their decisions and the decisions of their matched

[12] Now the BizLab of the UNSW Business School.

[13] We started the experiment by testing the anti-corruption effect of CR treatment first. Because CR treatment was expected to have the strongest effect, the RV and RO treatments would have made no sense if there had not been a difference between the baseline and CR treatments. Due to budget constraints, we recruited fewer students for the RV and RO treatment.

[14] We typically form an emissions trading market with three groups of the three types of participants. Huck et al. (2004) suggest that the degree of competitiveness in an experimental market increases with the number of firms. In particular, markets with two firms reveal some market powers and markets with at least four firms can be very competitive. But there is no clear evidence predicting the competitiveness of markets with three firms. Given studies of the windfall profits of firms with market power in EU ETS (Sijm et al., 2006), those firms' under-reported emissions may be large and they may influence the allowance price to a certain extent. Therefore, a market with three firms is suitable in this study. Note that even though there are three firms in one market, firms are not considered to be price makers because we require at least two firms to under-report their emissions at a relatively high level (Y-X>5) to reduce the permit price in the following round.

[15] In rotation treatments, if more than two firms under-reported and had been detected by the matched official, rotation is not applied. In fact, there were, respectively, 2.2%, 6.7%, and 4.6% of the observations in RV, RO, and CR treatments not applicable for rotation.

[16] We used context-free language in the instructions to avoid behaviour driven by environmental concerns, which seems unlikely for firms pursuing profit maximisation. For ease of exposition, we use loaded (non-neutral) language in this paper. All the payoffs are denoted in experimental dollars (E$). For more details, see the instructions in online Appendix D.

participants. Each participant's final payoff was the average of three randomly selected period-payoffs in addition to a five dollar show-up fee. On average, participants earned 26 Australian dollars, with earnings ranging from AUD 11 - 46.

5. Results

In this section, we use consolidated data in which we include all observations where participants were active in a period while excluding observations when participants were detected and therefore prohibited from participation in a particular part.[17] In Subsection 5.1 we examine the effects of rotation on firms' truthful reporting, and in Subsection 5.2 we examine the unrevealed untruthful reporting. In Subsection 5.3, we explore how rotation influences firms' reciprocity towards verifiers while in Subsections 5.4 and 5.5, we analyze the effects of rotation on both verifiers' and officials' inspection behavior, respectively. In Subsection 5.6, we compare the market prices that resulted from different treatments. Finally, in Subsection 5.7 we discuss how these observations relate to our hypotheses.

5.1 Firms' Truthful Reporting

Table 2 summarizes the mean values of firms' choices across all treatments, i.e., the frequency of truthful reporting, the selected level of under-reporting, and the level of transfer offered.

Table 2: Summary statistics of firms' choices

Variables	Number of participants	Obs.	Mean	Std. Dev.	Min	Max
Truthful reporting (Truthful dummy)						
Baseline	18	297	0.397	0.490	0	1
CR treatment	18	291	0.581	0.494	0	1
RV treatment	15	244	0.516	0.501	0	1
RO treatment	15	233	0.506	0.501	0	1
Selected level of under-reporting						
Baseline	18	297	4.993	4.654	0	10
CR treatment	18	291	3.340	4.512	0	10
RV treatment	15	244	4.184	4.709	0	10
RO treatment	15	233	4.099	4.720	0	10
Transfer Offered						
Baseline	18	297	3.636	3.688	0	9
CR treatment	18	291	2.567	3.573	0	9
RV treatment	15	244	3.020	3.615	0	9
RO treatment	15	233	3.232	3.889	0	9

Note: "Truthful reporting" is a dummy variable and is equal to 1 if the firm reported truthfully.

The mean values of the truthful reporting dummy indicate that the frequency of truthful reporting increased from roughly 40% in the baseline to 58% with the CR treatment, and around 51% with the incomplete rotation treatments (RV and RO). Using the means of the truthful reporting dummy over 18 periods on the subpopulation level, Wilcoxon rank-sum tests show the increment with the CR

[17] Note that we still keep the observations of the detected participants when they were active in the periods before being detected. In total, we excluded 10.4% of data. Specifically, 8.3% of the baseline, 9.6% of the RV treatment, 13.7% of the RO treatment, and 10.2% of the CR treatment observations were excluded because transgressions were detected by an official.

treatment (relative to the baseline) to be statistically significant at the 5% level and both the increment with the RV and RO treatments not statistically significant.

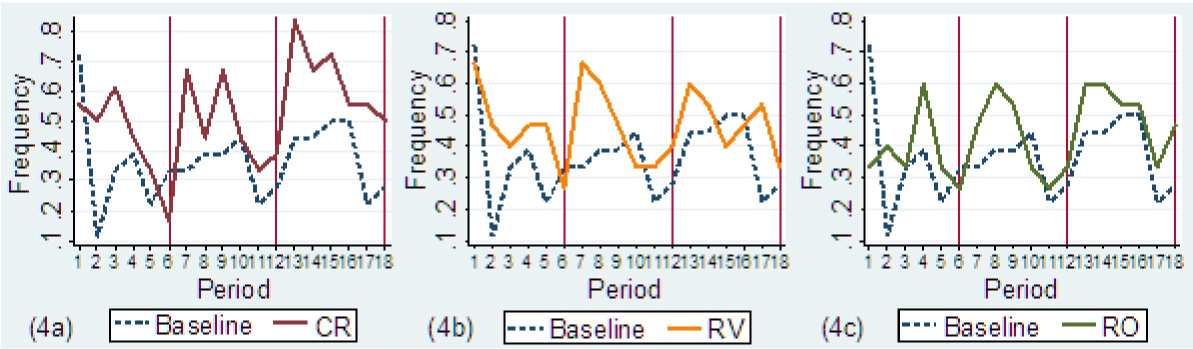

Figure 4: Frequency of truthful reporting over time

Note: The vertical lines indicate the three different parts featured in each treatment.

Figure 4 illustrates this finding (and also the nonsignificant increments with RV and RO treatments) for all truthful reporting choices for all 18 periods. The increment in truthful reporting is particularly pronounced in the last six periods (the last part), where the significance remains at the 5% level. In addition, there is a clear reduction pattern towards the end of each part across all treatments. This is likely to be an end-game effect.

Observation 1. *With the complete-rotation treatment (CR), there are significantly higher frequencies of truthful reporting than in the baseline with fixed matching.*

In Table 2, the average of selected under-reporting (i.e., the complement of truthful reporting) measures a firm's untruthfulness, and the average of transfer offered measures the extent to which firms are willing to influence the verifiers' decisions. Figure 5 shows the evolution of these two variables in each period of the baseline and the CR treatment.

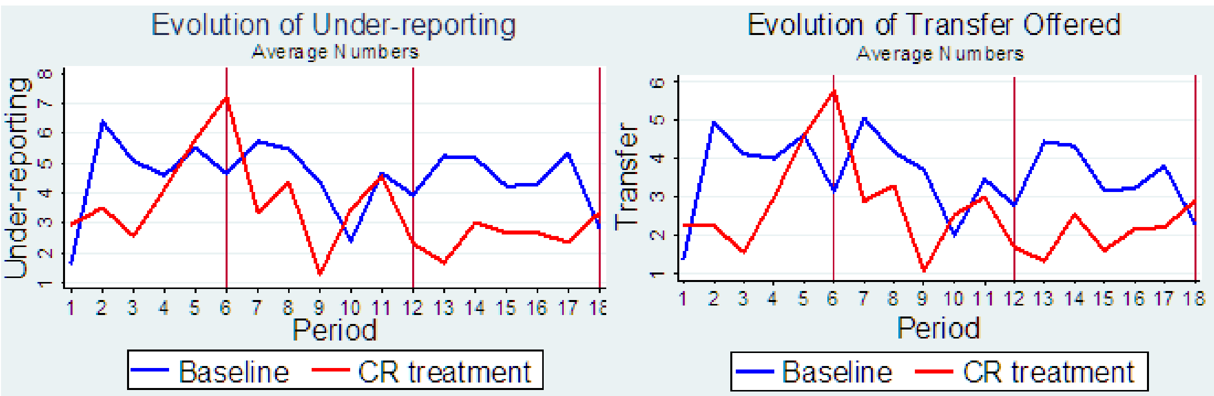

Figure 5: Evolution of selected under-reporting and transfer offered

Except for Period 6, the CR treatment seems to have a stable negative effect on firms' levels of under-reporting and transfers. As summarised in Table 2, the mean of selected under-reporting across all the observations, including truthful-reporting is reduced from almost 5 in the baseline to 3.34 with the CR treatment, a reduction of more than one third. Furthermore, the mean of transfers offered is reduced from 3.64 in the baseline to 2.57 with the CR treatment, a reduction of almost one third. The Wilcoxon rank-sum tests, applied to the average of observations for all periods of each subpopulation for the baseline and CR treatment, reject the null hypothesis of no significant differences with a significance at 5% for both the average of under-reporting and the average of transfer offered consisting with Hypothesis 1. The mean values of transfers offered on condition of under-reporting are 6.58 and 6.79 for the baseline and the CR treatment, respectively, while the frequencies of under-reporting are 164 and 110 for the baseline and the CR treatment, respectively. Therefore, the reduction in transfer offered with the CR treatment is the consequence of the increased frequency of truthful reporting compared to the baseline. With regard to the reductions induced by incomplete-rotation treatments, neither is statistically significant. The distribution of selected under-reporting and transfers offered in the baseline and with CR treatment is shown in Figure 6.

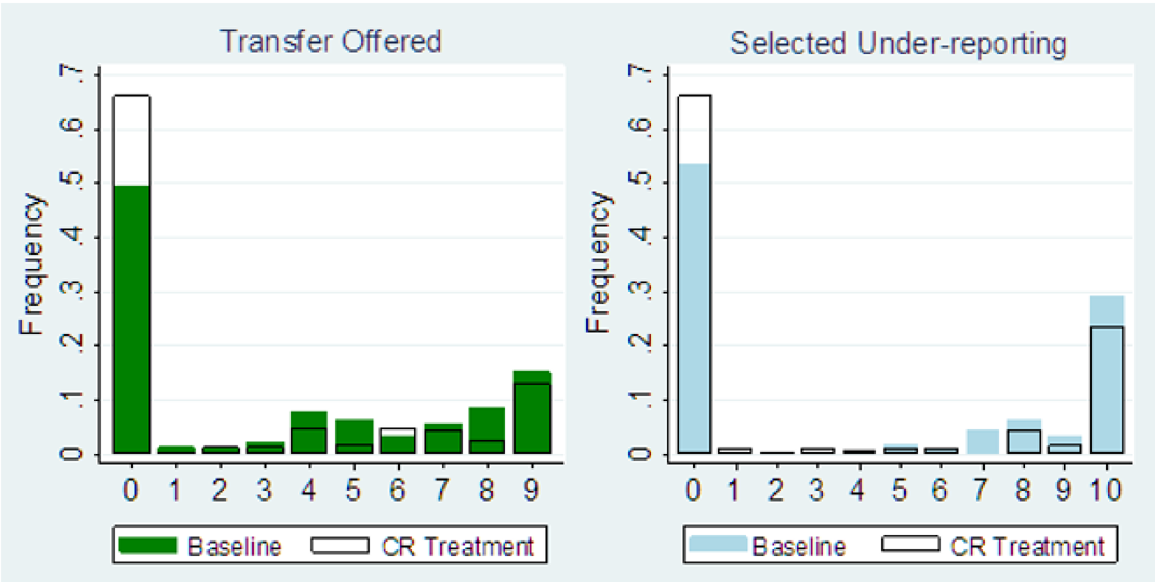

Figure 6: Distribution of selected under-reporting and transfer offered

Our main hypothesis of the positive impact of complete rotation on enhancing truthful reporting (Hypothesis H1) is confirmed since the frequency of truthful reporting (under-reporting = 0) is significantly higher in the CR treatment compared to the baseline. Moreover, if a firm decides to be untruthful, it tends to select the highest level of under-reporting (Y-X=10) and the highest transfer (transfer offered = 9). We can see that, for almost all levels, the CR treatment leads to lower under-reporting and lower transfers compared to the baseline.

It seems reasonable to expect the level of under-reporting to be positively correlated with the level of the transfer offered, as a firm with a higher degree of untruthfulness might show a greater propensity towards corruption by offering a higher bribe to a verifier. Using observations of individual firms' decisions in each period, the Spearman correlation coefficients confirm this conjecture. There are positive correlations between the level of selected under-reporting and the level of transfer offered in all treatment conditions (baseline: $\rho = 0.888$, $p = 0.000$; CR treatment: $\rho = 0.978$, $p = 0.000$; RV treatment: $\rho = 0.952$, $p = 0.000$; RO treatment: $\rho = 0.957$, $p = 0.000$). The results are illustrated in Figure 7 in online Appendix B, which shows the selected level of under-reporting for the corresponding transfer offered by firms.

Observation 2. *Although firms with a higher degree of untruthfulness tend to pay a higher transfer, complete rotation leads to significantly lower levels of under-reporting and significantly smaller transfers compared to the baseline.*

To better understand the firms' decisions and check the effectiveness of incomplete rotations, we run a series of OLS and Probit regressions (with standard errors clustered at the subpopulation level) that take the selected level of under-reporting, the transfer offered, and the decision of untruthful reporting as the dependent variables, separately.[18] The treatments are used as the key explanatory variable, and we include indicators of firms' experience with other matched participants as control variables: (i) period, (ii) part, (iii) price, (iv) whether there is a preceding reported Y, and (v) whether there is a preceding check. Table 3 reports the coefficients of the regressions and the corresponding marginal values.

Table 3: Effect of rotation on firms' behavior

Independent variables	Under-reporting	Transfer offered	Untruthful (dummy)	
	(1) Linear reg	(2) Linear reg	(3) Probit reg	(4) Marginal values
Panel A: Effect of complete rotation on Firms' behavior				
CR Treatment	-1.995**	-1.587**	-0.597***	-0.237***
	(0.770)	(0.654)	(0.220)	(0.087)
Period	0.121	0.176	0.032	0.013
	(0.168)	(0.147)	(0.050)	(0.020)
Part	-1.309	-1.321	-0.423	-0.168
	(1.059)	(0.872)	(0.309)	(0.123)
Price	0.341**	0.549***	0.126***	0.050***
	(0.147)	(0.137)	(0.045)	(0.018)
ReportY_lag	0.552	0.147	0.087	0.035
	(0.605)	(0.518)	(0.166)	(0.066)
Check_lag	-2.281***	-1.999***	-0.752***	-0.299***
	(0.587)	(0.430)	(0.229)	(0.092)
Intercept	5.264***	2.465***	0.226	
	(0.770)	(0.618)	(0.197)	
# of Observation	552	552	552	552
Panel B: Effect of RV on Firms' behavior				
RV Treatment	-0.892	-0.955	-0.298	-0.119
	(0.858)	(0.677)	(0.251)	(0.100)
Period	-0.024	0.012	-0.011	-0.004
	(0.192)	(0.167)	(0.052)	(0.021)
Part	0.087	0.027	-0.017	-0.007
	(1.145)	(0.951)	(0.317)	(0.126)
Price	0.199	0.481**	0.082	0.033
	(0.230)	(0.188)	(0.063)	(0.025)
ReportY_lag	1.080*	0.419	0.302*	0.120*
	(0.560)	(0.545)	(0.181)	(0.072)
Check_lag	-3.057***	-2.580***	-0.983***	-0.392***
	(0.797)	(0.632)	(0.231)	(0.090)
Intercept	4.422***	1.611***	-0.011	
	(0.581)	(0.415)	(0.142)	
# of Observation	508	508	508	508
Panel C: Effect of RO on Firms' behavior				
RO Treatment	-1.378	-0.952	-0.418	-0.167

[18] Standard Tobit regressions for the selected level of under-reporting and the transfer offered with clustered standard errors at the subpopulation level and dependent variables censored from below 0 does not change the main results qualitatively. Moreover, it yields a larger treatment effect of CR. Results are reported in Table C1 of online Appendix C. We thank an anonymous reviewer for pointing this out.

	(0.938)	(0.847)	(0.264)	(0.106)
Period	0.046	0.092	0.010	0.004
	(0.180)	(0.145)	(0.050)	(0.020)
Part	-0.512	-0.764	-0.225	-0.090
	(1.231)	(0.935)	(0.349)	(0.139)
Price	0.433*	0.607***	0.139**	0.055**
	(0.201)	(0.188)	(0.057)	(0.023)
ReportY_lag	-0.066	-0.116	-0.054	-0.022
	(0.599)	(0.522)	(0.172)	(0.069)
Check_lag	-2.689***	-2.159***	-0.802***	-0.320***
	(0.585)	(0.514)	(0.166)	(0.066)
Intercept	4.033***	1.930***	-0.018	
	(0.771)	(0.534)	(0.163)	
# of Observation	497	497	497	497

Notes: "Untruthful", "ReportY_lag" and "Check_lag" are all dummy variables. "Untruthful" = 1 if firms report untruthfully, "ReportY_lag" = 1 if the matched verifier in the preceding period chooses Report Y, "Check_lag" = 1 if the matched official in the preceding period chooses Check. Standard errors in parentheses clustered at the subpopulation level. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Based on Columns (1) - (4) of Panel A, we confirm that complete rotation (CR) has a significantly negative impact on the level of both selected under-reporting and transfer offered, as well as the incidence of under-reporting. Holding all else constant, the marginal value of CR in Column (4) show that a complete rotation would reduce the probability of untruthful reporting by 23.7%. This is consistent with Hypothesis H1. Focusing on the effects of RV and RO treatment in Panels B and C, although the treatment effects are negative, neither is statistically significant. This is consistent with Hypothesis H2, but not consistent with Hypothesis H3.

In Panel A, the coefficient of Check_lag shows that being checked by an official in the preceding period has a highly significant, negative effect. Note that the negative effect of an official's check in the preceding period is consistently significant at a 1% significance level under all rotation conditions, the effect of ReportY_lag is only marginally significant in the RV treatment with a smaller magnitude (see the coefficients of Check_lag and ReportY_lag in Panel A, Panel B, and Panel C). This might suggest that inspections by an official with more authority have a stronger deterrent impact in comparison with inspections by a verifier who does not have the same authority. In addition, the regressions demonstrate that the effect of being checked in the preceding period has a larger effect than the treatment effect under all rotation conditions (the coefficients of Check_lag are all significantly negative, and the size of them are larger than the coefficient of rotation treatment in Panel A, Panel B, and Panel C). This reflects a tendency of contagious non-cooperation by firms once they have been checked by an official. It is possible that truthful reporting might be further improved if the official's decision in a preceding period is revealed to the firm. The coefficients of price show significantly positive effects, suggesting that untruthful reporting problems could become serious when prices are relatively high. Because each part is a restart of the game, we applied the same estimation framework in each part separately to examine if there was any learning effect. The corresponding results can be found in Table C2 in online Appendix C. Focusing on the treatment effects, Table C2 shows that indeed there was a learning effect in the complete rotation treatment as the treatment effects became significantly larger in the later parts compared with the earlier parts. However, we do not observe such an effect with the incomplete rotation treatments.

Observation 3. *While complete-rotation treatment (CR) leads to more truthful reporting, incomplete-rotation treatments (RV and RO) do not lead to significant improvements in truthful reporting; nevertheless, the experience of being checked in the preceding period has a significantly negative effect on untruthful reporting, which is consistent in all rotation treatments.*

5.2 Unrevealed Untruthful Reporting

Since under-reporting relies crucially on the cooperation between firms and verifiers, we examine changes in unrevealed under-reporting with all treatments. In particular, we examine (i) the total number of untruthful reports that are not revealed by the matched verifiers, and (ii) the aggregate level of under-reporting of those unrevealed untruthful reports. Figure 8 illustrates the cumulative distribution function (CDF) of these two variables.

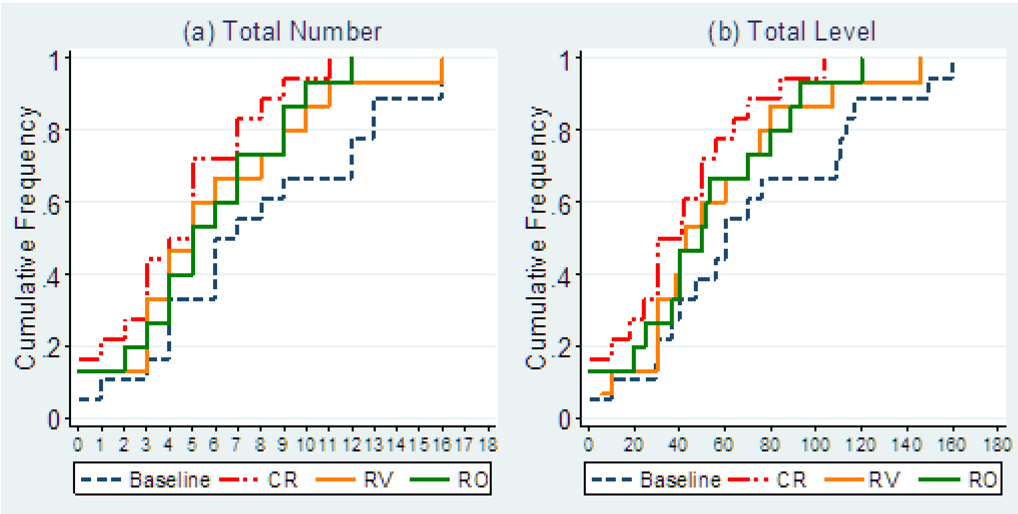

Figure 8: Cumulative distribution function of individuals' unrevealed under-reporting (total number and total level)

Notes: Figure 8 (a) used the total number of an untruthful report that is not revealed by a verifier for each firm. Figure 8 (b) used the aggregate level of under-reporting among those unrevealed untruthful reports for each firm.

Overall, the CR treatment leads to the most significant reduction in the number of unrevealed untruthful reports while the RV and RO treatments result in two moderate reductions; the difference between these two incomplete treatments (see Figure 8 (a)) is not significant. Complete rotation leads to a decrease of about 58% for firms who have more than five unrevealed untruthful reports, while incomplete rotation leads to a decrease of 30-40% (the percentage of firms who had at least five unrevealed untruthful reports is 66.7% in the baseline, 27.8% with the CR treatment, 40% with the RV treatment, and 46.7% with the RO treatment). In addition, similar patterns are observable in terms of the aggregate level of under-reporting in Figure 8 (b).

To substantiate these results, we report the results of Probit and OLS regressions (with standard errors clustered at the subpopulation level) in Table 4.

Table 4: Effect of rotation on unrevealed under-reporting

	CR		RV		RO	
Independent variables	(1) Under-reporting (Dummy)	(2) Under-reporting (Level)	(3) Under-reporting (Dummy)	(4) Under-reporting (Level)	(5) Under-reporting (Dummy)	(6) Under-reporting (Level)
Treatment	-0.245***	-2.179**	-0.129	-1.071	-0.175*	-1.507▲
	(0.081)	(0.776)	(0.087)	(0.755)	(0.091)	(0.848)
Period	0.013	0.081	0.004	0.028	0.009	0.077
	(0.022)	(0.187)	(0.023)	(0.208)	(0.021)	(0.195)

Part	-0.098	-0.506	-0.008	0.114	-0.068	-0.337
	(0.127)	(1.094)	(0.135)	(1.196)	(0.132)	(1.224)
Price	0.040**	0.267	0.041*	0.297	0.056***	0.449**
	(0.019)	(0.163)	(0.024)	(0.208)	(0.019)	(0.159)
ReportY_lag	-0.005	0.039	0.079	0.553	-0.135	-1.247
	(0.079)	(0.739)	(0.104)	(0.934)	(0.087)	(0.733)
Check_lag	-0.278***	-2.299***	-0.340***	-3.128***	-0.276***	-2.745***
	(0.044)	(0.511)	(0.066)	(0.705)	(0.066)	(0.643)
Intercept		3.688***		2.796***		2.722***
		(0.828)		(0.635)		(0.694)
# of Observation	552	552	508	508	497	497

Notes: Columns (1), (3), and (5) show marginal values of Probit estimates and Columns (2), (4), and (6) show coefficients of OLS estimates. The dependent variable Under-reporting Dummy is equal to 1 if the firm's report is untruthful and the verifier does not reveal it and chooses to report X. The dependent variable Under-reporting Level is each firm's aggregate level of under-reporting among all the unrevealed untruthful reports. The experience variables "ReportY_lag" and "Check_lag" are dummy variables for the firm's experience. "ReportY_lag" = 1 if the matched verifier revealed under-reporting and chooses Report Y in the preceding period. "Check_lag"=1 if the matched official checks in the preceding period. Standard errors in parentheses are clustered on subpopulation level. Significance level: * p< 0.1, ** p < 0.05, *** p < 0.01, ▲ p = 0.106.

Both the incidence and the level of unrevealed under-reporting are significantly reduced with the CR treatment compared with the baseline. The effects of the RO treatment are negative and marginally significant, while no significant effect is observed for the RV treatment. The coefficients of Check_lag are significantly negative while the coefficients of ReportY_lag are insignificant in all treatments. We conjecture that the cooperation between firms and verifiers is not easily hindered by a verifier's defection, but is easily influenced by an official's defection. Moreover, the coefficients of price are, in general, significantly positive, which indicates that firms' and verifiers' incentives in cooperation are strong as long as the price is sufficiently high.

Observation 4: *The incomplete-rotation treatments (RV and RO) lead to a moderate reduction in both the incidence and the level of unrevealed under-reporting compared to the baseline, while the complete-rotation treatment (CR) leads to statistically significant reductions.*

5.3 Reciprocity by Firms towards Verifiers

In our two-layered review setting, firms have a direct reciprocal relationship with verifiers. In this section, we explore how a firm's reciprocity towards a verifier varies with different levels of rotation. We adopt the same approach as Abbink (2004) when measuring firms' reciprocity towards verifiers' action:

$$\text{R(verifier)} = \frac{\sum transfer\_X}{\#ofReportX} - \frac{\sum transfer\_Y}{\#ofReportY}$$

where R indicates the excess transfer after Report X, the numerator transfer_X (or transfer_Y) denotes the aggregate transfer offered after a preceding Report X (or Report Y), and the denominator # of Report X (or # of Report Y) denotes the total number of Report X (or Report Y) in Periods 2-18[19]. We have an average R of –0.19 for the baseline, -0.17 for the CR treatment, and -0.65 for the RV treatment. Despite a negative average R-value, the Wilcoxon matched-pairs signed-ranks tests applied at the individual firm level show no significant difference between the average transfers after Report

[19] We only include cases where firms reported untruthfully and at the same time were not checked by officials in the preceding period. In total, 25 subjects were excluded from the analysis.

X and Report Y under each of these conditions. However, for the RO treatment, the average R-value is 2.68, which is significantly high among all treatments (a Wilcoxon rank-sum test on individual R between the baseline and the RO treatment shows a significance at 6%). Applying the same signed-ranks test on all observations with the RO treatment, we reject the null hypothesis that the average transfers after Report X is equal to the average transfers after Report Y at a significance level of 5% (one-sided). Therefore, when officials are only rotated, firms tend to strengthen their reciprocity towards the same verifier by offering higher transfers after Report X.

Observation 5. *In comparison to the baseline, firms' reciprocity towards verifiers' action is strengthened when only officials are rotated, where firms transfer more after Reporting X than after Reporting Y.*

The effects of rotations on truthful reporting by firms work through the inspections by both verifiers and officials. Therefore, we look next at the effect of rotations on the actions of both verifiers and officials.

5.4 Frequency of Rejections and Inspections by Verifiers

Figure 9 shows the relative frequency of verifiers' rejections that follow a particular transfer in both the baseline and the CR treatment, aggregated over all three parts for both treatments.

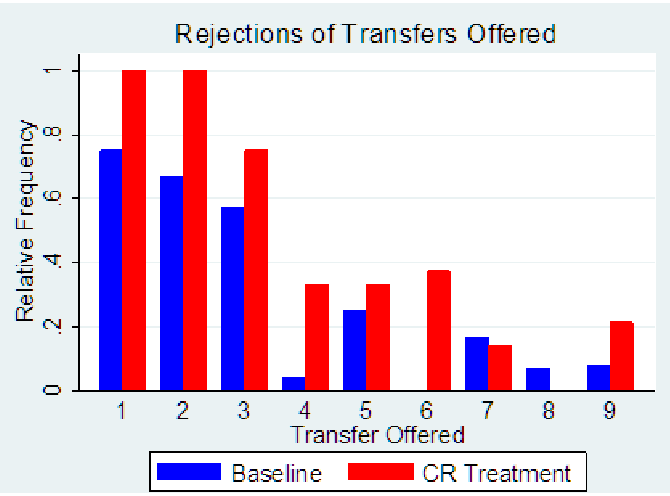

Figure 9: Relative frequency of rejections of transfers offered

Verifiers tend to reject small transfers and are less likely to reject larger transfers with both treatments. In addition, at almost all levels of transfer, rejection rates are relatively high in the CR treatment compared to the baseline. In Figure 10, we also depict the evolution of the frequency of rejections, one part at a time, using the observed choices of all verifiers per period in the baseline and the CR treatment.

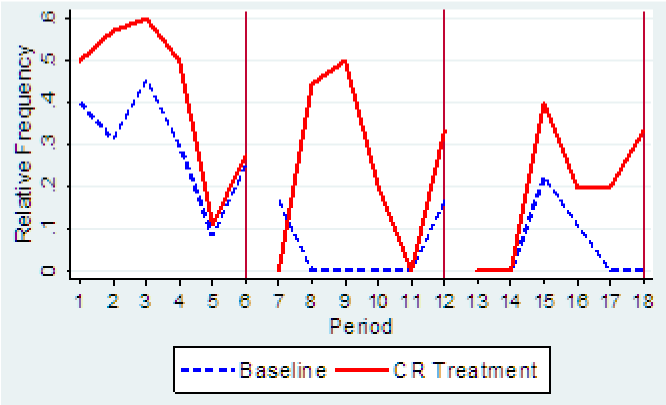

Figure 10: Evolution of frequency of rejections by verifiers

Figure 10 shows that the frequency of rejections is much higher for complete rotation than for fixed matching, which is consistent with Hypothesis H1. In total, there are 110 occurrences of transfers with CR treatment and the rejection rate is 29.1%. Compared to 14.6% of 164 occurrences of transfers being rejected in the baseline, this is an almost 100% increment, whereas the increment is about 50% with both the RV and RO treatments. Applying the Wilcoxon rank-sum test to the frequency of rejection on the subpopulation level, we find evidence of the CR treatment effect at a 5% significance level.

Observation 6. *Verifiers reject small transfers more frequently and reject larger transfers less frequently in both the baseline and the CR treatment. Rejection rates are overall significantly higher in the CR treatment than in the baseline.*

In Table 5, we estimate the treatment effects of rotations on verifiers' inspection behavior (with standard errors clustered at the subpopulation level), while taking into account the effect of their experience with other matched participants through Probit regressions and their marginal values.

Table 5: Effect of rotation on verifiers' Report Y

	CR Condition		RV Condition		RO Condition	
Independent variables	(1) Probit	(2) Marginal	(3) Probit	(4) Marginal	(5) Probit	(6) Marginal
Rotation treatment	0.543*	0.112§	0.431*	0.079**	0.641*	0.130$^{\Delta}$
	(0.280)	(0.068)	(0.225)	(0.038)	(0.330)	(0.080)
Period	0.063	0.012	-0.035	-0.006	-0.003	-0.001
	(0.081)	(0.015)	(0.069)	(0.012)	(0.115)	(0.021)
Part	-0.711$^{\blacktriangle}$	-0.137*	-0.033	-0.006	-0.320	-0.059
	(0.439)	(0.077)	(0.397)	(0.069)	(0.706)	(0.132)
Price	0.327**	0.063***	0.026	0.004	0.105	0.019
	(0.158)	(0.024)	(0.108)	(0.018)	(0.116)	(0.020)
Under-reporting level_Current	0.079	0.015*	0.008	0.001	-0.018	-0.003
	(0.048)	(0.009)	(0.040)	(0.007)	(0.039)	(0.007)
Transfer offered_Current	-0.231***	-0.045***	-0.193***	-0.033***	-0.135**	-0.025***
	(0.064)	(0.009)	(0.054)	(0.009)	(0.057)	(0.009)
Check_lag	0.889***	0.253**	0.758***	0.193***	0.388	0.089
	(0.281)	(0.107)	(0.149)	(0.051)	(0.600)	(0.157)
Intercept	-1.441		-0.004		-0.232	
	(1.072)		(0.624)		(0.629)	
# of Observation	249	249	254	254	241	241

Notes: We only include cases where firms report untruthfully. The dependent variable is "Report Y", which is a dummy. "Report Y" = 1 if a verifier reveals a report as untruthful. "Under-reporting level_Current" ("Transfer offered_Current") indicates the level of under-reporting (the transfer offered) in the current period. "Check_lag" (dummy) = 1 if the matched official in the preceding period chooses to check. Standard errors in parentheses are clustered on subpopulation level. Significance level: * p< 0.1, ** p <0.05, *** p < 0.01, § p=0.102, Δ p=0.106, ▲ p=0.105.

Focusing on the treatment effects, the effects of all rotation treatments are marginally significant at the 10% level; this is in line with Hypotheses H1 and H2, but not Hypothesis H3. Comparing the marginal value of all treatments, it is 11.2% with the CR treatment, 7.9% with the RV treatment, and 13% with the RO treatment (see coefficients of rotation treatment in Columns (2), (4), and (6) in Table 5). If the effect of the CR treatment is a convex combination of the effect of RV and RO treatment, then the effect of CR is mainly driven by rotating officials. Although the RO treatment leads to a higher frequency of honest inspection by verifiers, it also leads to a strengthened reciprocal relationship between the firms and verifiers (see Observation 5). As a result, we do not observe a significantly higher level of truthful reporting in the RO treatment compared to the baseline. Moreover, the modest effect of RV on the verifier's behavior is probably one of the reasons why RV does not lead to a significant improvement in truthful reporting.

Note that while the experience of being checked by the matched official (Check_lag) has a significant positive impact, the level of transfer offered (Transfer offered_Current) in the current period has a significant negative impact. The marginal value of the Check_lag under the CR treatment in Column (2) of Table 5 shows that a verifier being checked in the preceding period would have a 25.3% higher probability of honest verification than if not checked. This is a substantial effect, which outweighs the effect of CR treatment (11.2%). Likewise, the positive effect of Check_lag also holds with the RV treatment. Interestingly, the significance of the effect of Check_lag disappears with the RO treatment, which is probably due to the strengthened reciprocity between firms and verifiers, making verifiers careless of officials' decisions.

Moreover, a larger transfer would reduce the probability of reporting Y by 4.5% with CR treatment (see coefficients of Transfer offered_Current), which is a much smaller effect compared to the effect of being checked. In addition, the level of a firm's under-reporting (under-reporting level_Current) only has a marginal impact with CR treatment. This seems to suggest that the verifiers' inspections are motivated by their own interests, such as the amount of transfer and the matched official's decision, while their duty of overseeing untruthful reporting seems not to be a strong motive. The coefficient of part and price with CR treatment suggests that the treatment effect on verifiers' behavior is also dynamic; it is larger as they gain more experience or when the price is still high.

Observation 7. *The complete-rotation treatment has a significantly positive effect on verifiers' inspection, the RO treatment has a larger effect on verifiers' inspection compared to the RV treatment, although none of the treatment effects is significantly large. Verifiers' inspection behavior is mainly influenced by their experience of officials' inspections when verifiers are rotated.*

5.5 Frequency of Inspections by Officials

Table 6 depicts the treatment effect of rotation on officials' inspection behavior while clustering on the subpopulation level. Apparently, checking is not worthwhile if the level of under-reporting is relatively low; it is, however, too costly if the level of under-reporting is relatively high, since a higher level of under-reporting is likely to be associated with a larger transfer. Therefore, checking is most likely if under-reporting is in a mediate position. We, therefore, assume an inverted-U relationship between an official's decision to check and the selected level of under-reporting. In this regard, Figure 11 examines the relative frequency of checks that follow a particular level of under-reporting in the preceding period. We also use a histogram to show the number of occurrences of each level of under-reporting that officials have experienced. We use the level of under-reporting by matched firms in the preceding period as an official's conjecture for the level of under-reporting in a particular period.

Table 6: Rotation treatment effects on officials' check

	CR Condition		RV Condition		RO Condition	
Independent variables	(1) Probit	(2) Marginal	(3) Probit	(4) Marginal	(5) Probit	(6) Marginal
Rotation treatment	0.040	0.008	-0.094	-0.017	0.002	0.0003
	(0.291)	(0.058)	(0.285)	(0.050)	(0.227)	(0.042)
Period	0.062	0.012	0.156***	0.028***	0.132**	0.025**
	(0.071)	(0.014)	(0.043)	(0.007)	(0.055)	(0.010)
Part	-0.281	-0.056	-0.786***	-0.140***	-0.839***	-0.157***
	(0.356)	(0.068)	(0.233)	(0.038)	(0.240)	(0.039)
Price	0.032	0.006	0.209***	0.037**	0.073	0.014
	(0.084)	(0.017)	(0.076)	(0.015)	(0.063)	(0.013)
Untruthful dummy_lag	-0.070	-0.014	-3.396	-0.653	-1.199	-0.226
	(0.836)	(0.166)	(3.406)	(0.606)	(0.760)	(0.148)
Under-reporting level_lag	0.262	0.053	1.078	0.191	0.555**	0.104**
	(0.305)	(0.059)	(0.952)	(0.155)	(0.239)	(0.043)
Under-reporting level_lag^2	-0.021	-0.004	-0.071	-0.013	-0.045***	-0.009***
	(0.024)	(0.005)	(0.061)	(0.010)	(0.017)	(0.003)
Transfer offered_lag	-0.094**	-0.019*	-0.089	-0.016	-0.007	-0.001
	(0.041)	(0.010)	(0.072)	(0.012)	(0.067)	(0.013)
Intercept	-1.324***		-2.027***		-1.109**	
	(0.427)		(0.449)		(0.438)	
# of Observation	458	458	431	431	415	415

Notes: We only include cases where verifiers Report X. The dependent variable is "Check", which is a dummy. "Check" = 1 if an official chooses check. "Untruthful dummy_lag" (dummy) = 1 if the matched firm report untruthfully in the preceding period, "Under-reporting level_lag" indicates the level of under-reporting in the preceding period, and "Transfer offered_lag" indicates the level of transfer offered in the preceding period. Standard errors in parentheses are clustered on subpopulation level. Significance level: * p< 0.1, ** p <0.05, *** p < 0.01.

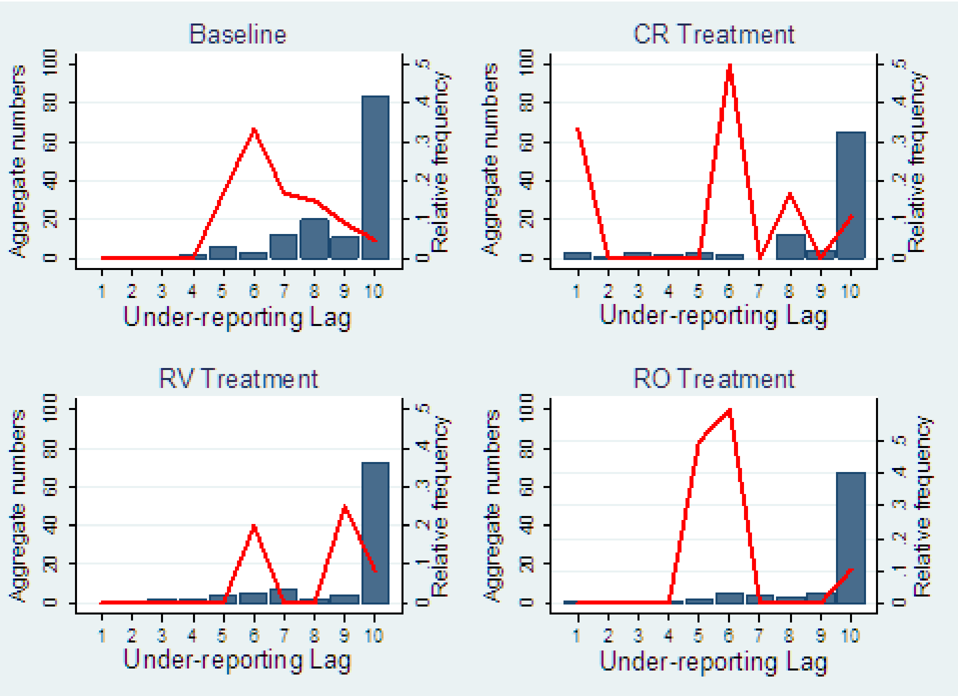

Figure 11: Frequency of firms' checks in case of under-reporting

Note: The histogram indicates the total occurrences of each under-reporting level that officials have experienced, and the red line indicates the firms' frequency of checks after experiencing a particular level of under-reporting.

No significant treatment effect can be observed regarding officials' inspection behavior with all rotation treatments (see treatment effects in Table 6). However, an important observation is that officials do respond to the magnitude of under-reporting they have experienced. In Figure 11, the apparent spike at 6 in all treatment conditions suggests that officials are most likely to conduct a check if they have experienced under-reporting at the level of 6[20]. From a closer look at the coefficients of Under-reporting level_lag and Under-reporting level_lag^2 in Table 6, we find that the highest probability of a check is when the under-reporting level is close to 6 or 7 in the preceding period[21], which is consistent with the observation in Figure 11. In addition, the experience of getting a transfer from a matched firm has a marginal negative effect on an official's inspection under the CR condition (see the coefficient of Untruthful dummy_lag in Column (1) and (2) in Table 6). The marginal value is -1.9%, which seems to suggest that officials tend to cooperate once they get an indication of untruthful reporting.

Moreover, since the experiment was divided into three identical parts with six repeated periods per part, there might be some learning effects on the officials as they gain more experience over the parts of the game. The coefficient of the marginal value of part in Table 6 is significantly negative in the incomplete rotation treatments. This supports the above argument concerning the learning effect on officials. In particular, holding all else constant, officials tend to check less frequently by 14% and 15.7% in the later parts with the RV and RO treatment, respectively.

Observation 8. *Officials' check is not influenced by staff rotation. Nevertheless, officials' inspection behavior is influenced by their learning of the game and their experience of firms' decisions. Officials check more frequently if the under-reporting level is around 6 in the preceding period.*

5.6 The Effect of Rotation on Market Price

Given the increase in truthful reporting with rotation treatments, we conjecture a higher market price of allowance with rotation treatments compared to the baseline. Treating each subpopulation's average price across all periods as one independent observation, the nonparametric Wilcoxon rank-sum tests show that the average market price with the CR treatment is higher than in the baseline with marginal significance ($p = 0.076$), while the average price with the incomplete rotation treatments are not significantly different from those in the baseline ($p = 0.359$ for RV and $p = 0.119$ for RO). In addition, Figure 12 illustrates the evolution of price over the 18 periods using the average prices in each period.

[20] The peak at 1 in the CR treatment and 9 in the RV treatment are probably due to interests and random variation.

[21] The level of under-reporting that is most likely to be checked is calculated as - (marginal value of under-reporting level_lag)/(2×margial value of under-reporting level_lag^2), which is 6.625 with CR treatment, 7.35 with RV treatment, and 5.78 with RO treatment.

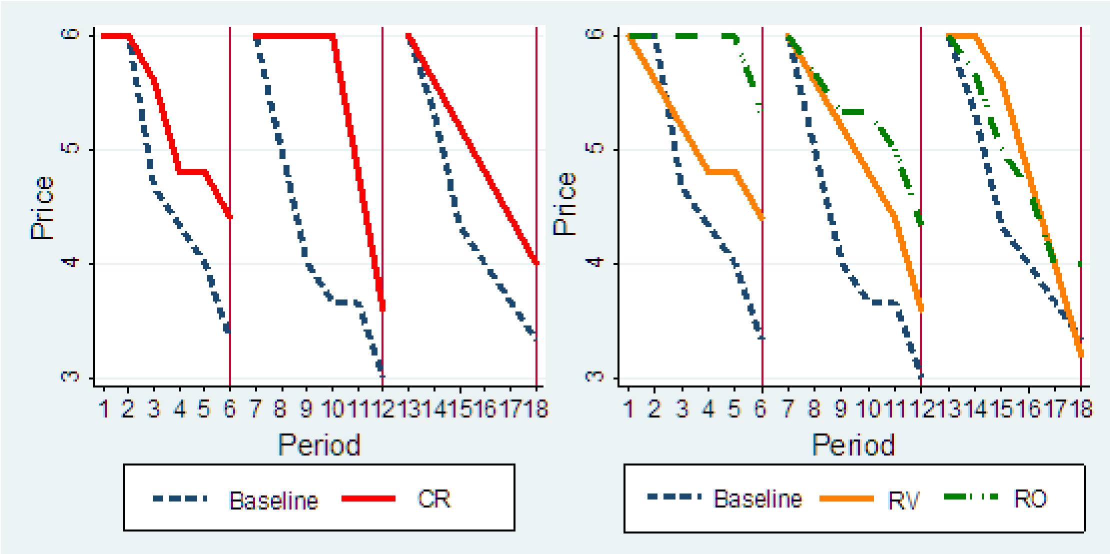

Figure 12: Evolution of price (average price)

In consistence with the conjecture, rotation treatments do lead to higher price levels in almost all periods, even though there is a declining tendency when getting to the last period of each part, which is consistent with the end-game effect in Figure 4.

However, the degree of price reduction at the end of each part might not be as high as at the beginning of each part, since firms have a stronger incentive for untruthful reporting when it is more beneficial, for example, when the price is 6 (see the coefficients of price in Tables 3 and 4). To explore whether this is true, we test to see if the difference between the price reduction in the last period and the second period of each part is statistically different from zero. The t-tests show that despite a lower price at the end of each part, the price reduction in the last period is higher than in the second period of each part in all rotation treatments (p =0.087 in CR, p = 0.089 in RV, p = 0.035 in RO)[22]. This suggests that the end-game effect might exacerbate untruthful reporting. There is evidence of a price collapse when approaching the end of the compliance period in the first and second phase of the EU ETS (Hintermann, 2010; Haita, 2013). Consequently, although rotation leads to higher prices to some extent, more rigorous inspections may be necessary when approaching the end of a compliance period, as untruthful reporting might increase during that time.

Observation 9. *With complete rotation, the market price of allowance is higher than in the baseline, and there is a pattern of price reduction at the end of each part with all treatments.*

5.7 Summary of Key Findings

We demonstrate that rotation tends to undermine trust and reciprocal cooperation among participants. We also show that the choice of rotation scheme affects the extent of untruthful reporting that requires cooperation between three participants in a hierarchical structure.

We experimentally tested three types of rotation against a baseline of no rotation. We observe that complete rotation leads to significantly lower frequencies of under-reporting, lower levels of under-reporting, and lower levels of transfers by firms, all of which is consistent with Hypothesis H1. However, regarding Hypothesis H3, we do not observe such effects with RO treatment (see Observations 1-4). Verifiers tend to reject transfers and reveal the under-reporting problem at a significantly higher frequency with the complete rotation treatment compared to the baseline, and such treatment effects are much weaker with the incomplete rotation treatments (see Observations 6

[22] We used the difference between the aggregate price reduction in Period 6, Period 12, and Period 18 and the aggregate price reduction in Period 2, Period 8, and Period 14 under each subpopulation as one independent observation.

and 7). However, none of the rotation treatments has a significant impact on decisions of officials (see Observation 8). This suggests that in our hierarchical structure, participants undergoing inspections (firms and verifiers) are more responsive to complete rotation, whereas officials in a regulating capacity, who are not monitored, demonstrate the least willingness to respond to any of the rotation treatments. Moreover, we observe a greater price reduction at the end of each part, suggesting that more rigorous inspections might be necessary towards the end of a compliance period.

6. Conclusion

We demonstrate experimentally the problem of untruthful reporting and the potential of rotation to enhance truthful reporting using different rotation schemes in a two-layered review system. In particular, inspections at both levels are very lax and liable to corrupting influences, while corruption is correlated with the problem of untruthful reporting. Our primary goal is to test the effect of complete rotation (CR treatment) on truthful reporting. We also explore the effects of two incomplete rotation schemes (RV and RO treatment). We find that implementing the complete rotation on both layers of inspectors has an anti-corruption effect and significantly increase the frequency of truthful reporting. Incomplete rotations, in contrast, do not have a significant impact on truthful reporting. While all rotation treatments have a weak impact on the verifiers' behavior, there is no treatment effect on inspection behavior of officials.

Our findings are particularly relevant for countries such as China, whose national ETS is currently under development. Although China has a strong administration, namely the National Development and Reform Commission (NDRC), in charge of the implementation of its national ETS, the MRV is left to provincial regulators, so the accuracy of the self-reported emissions is pivotal for the integrity of the whole system. Based on our findings, we argue that the establishment of independent third-party verifications and independent representatives of regulatory authorities is crucial for the effectiveness of the MRV system.

Some of our results may seem surprising. First and in particular, the behavior of officials was not influenced by any form of rotation. This seems to indicate that officials with the highest level of authority are hard to motivate. Therefore, further studies are needed to examine the effect of additional measures such as rewards to incentivise those at the top. Second, although the officials' checking behavior hardly changes in the CR treatment, firms tend to report truthfully more often compared to the baseline. This is an important finding which suggests that even without any significant enhancement in inspection rate, the induced uncertainty of both inspectors' corruptibility might lead to a higher level of truthfulness for individuals who are subject to inspection. Third, despite the effects of rotation, we find the level of under-reporting is always positive with all forms of rotation. Hence, we recommend that rotation needs to be complemented with other anti-corruption policies, such as punishments or rewards to reduce under-reporting further.

In line with our findings, future research could take several paths. One option would be to explore a situation where firms were allowed to select their verifiers. We would expect market competition to induce the verifier to be dishonest and collude with the firm and thus increase under-reporting. Second, an issue we have not considered in this paper is the cost of rotation. To explore whether rotation is more or less applicable at different frequencies might be worthwhile. Finally, although our experiment is modelled in a neutral language and therefore our findings should be applicable to other regulations, there is a specific feature related to the context ETSs, namely that under-reporting will have an impact on the corresponding market price. If under-reporting becomes more frequent, the price will fall and the incentive for under-reporting will be reduced as well. However, this may not affect other self-reporting systems such as taxation. Consequently, it is possible that the effectiveness of rotation might be dynamically different for systems other than ETSs.

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Online Appendix

A. Perfect Bayesian Equilibrium (PBE) of the game.

We assume that a polluting firm chooses truthful reporting with probability *p* and under-reporting with probability 1-*p*, the verifier decides to cooperate and conceal under-reporting (Report X) with probability *q* and to report Y with probability 1-*q*, and the official chooses to check with probability *h* and not to check with probability 1-*h*. Hence, we write $\beta_F = (p,\ 1-p)$, $\beta_V = (q,\ 1-q)$ and $\beta_O = (h,\ 1-h)$ as the firm's, the verifier's, and the official's behavioral strategies, respectively. On receiving a Report X, the official's belief of truthful reporting is $(\mu,\ 1-\mu)$,

where $\mu = \dfrac{p}{p+(1-p)q}$, if $p+(1-p)q > 0$ and μ is not defined and is equal to any $x \in [0,1]$ if $p+(1-p)q = 0$, that is $p = q = 0$, which means that a polluting firm chooses under-reporting Y, the verifier chooses to reject the transfer and report Y, and the game ends.

We use ((p^* ,1- p^*), (q^* , 1- q^*), (h^* , 1- h^*)) and (μ^* ,1- μ^*) to define the PBE of the game below.

By using backward induction, we solve the PBE of the game step-by-step, as depicted in Figure 2.

Step 1. We analyze the official's best response.

Given $\beta = (\beta_F, \beta_V, \beta_O)$ and $(\mu,\ 1-\mu)$, the expected utility of the official is

$$\begin{aligned} Eu_O(\beta|\mu) &= \mu(18h + 36(1-h)) + (1-\mu)(h(18+T+bt) + (1-h)(36+ct)) \\ &= ((1-\mu)(T+dt-ct) - 18)h + 36 + (1-\mu)ct, \end{aligned} \tag{1}$$

According to Equation (1), the official's best response is

$$h = \begin{cases} 1, & if\ \ \mu < A \\ any\ no. \in [0,1], & if\ \ \mu = A \\ 0, & if\ \ \mu > A \end{cases} \tag{2}$$

where $A := 1 - \dfrac{18}{T+dt-ct}$.

Step 2. We continue by discussing the verifier's best response.

Given (β_F, β_V) and $\beta_O(\beta_F, \beta_V)$, the expected utility of the verifier is

$$\begin{aligned} Eu_V(\beta_F, \beta_V, \beta_O(\beta_F, \beta_V)) &= 36p + (1-p)(q(1-h)(30+bt) + 36(1-q)) \\ &= (1-p)((1-h)(30+bt) - 36)q + 36 \end{aligned} \tag{3}$$

According to Equation (3), the best response of the verifier is

$$q = \begin{cases} 1, & if\ \ h < B \\ any\ no. \in [0,1], & if\ \ h = B \\ 0, & if\ \ h > B \end{cases}, \tag{4}$$

where $B := 1 - \dfrac{36}{(1-p)(30+bt)} = \dfrac{bt - 6 - p(30+bt)}{30 + bt - p(30+bt)}$.

Step 3. We continue by discussing the firm's best response.

Given β_F, $\beta_V(\beta_F,\beta_O)$ and $\beta_O(\beta_F,\beta_V)$, the expected utility of the firm is

$$Eu_F(\beta_F,\beta_V(\beta_F,\beta_O),\beta_O(\beta_F,\beta_V)) = 36p+(1-p)(30(1-q)+q(1-h)(30+T-at))$$
$$=(30qh-q(1-h)(T-at)+6)p+C, \qquad (5)$$

where $C := 30(1-q)+q(1-h)(30+T-at)$.

According to Equation (5), the best response of the firm is

$$p=\begin{cases}1, & \text{if } 30qh-q(1-h)(T-at)+6>0\\ \text{any no.}\in[0,1], & \text{if } 30qh-q(1-h)(T-at)+6=0\\ 0, & \text{if } 30qh-q(1-h)(T-at)+6<0\end{cases} \qquad (6)$$

Step 4. We solve for the PBE of this game.

Consider two cases as follows.

Case 1. When $T+dt-ct<18$, we have $(1-\mu)(T+dt-ct)-18<0$ and $h=0$, which implies that the best responses of the verifier and firm are

$$q=\begin{cases}1, & \text{if } (1-p)[(30+bt)-36]>0\\ \text{any no.}\in[0,1], & \text{if } (1-p)[(30+bt)-36]=0\\ 0, & \text{if } (1-p)[(30+bt)-36]<0\end{cases}, \qquad (7)$$

and $p=\begin{cases}1, & \text{if } -q(T-at)+6>0\\ \text{any no.}\in[0,1], & \text{if } -q(T-at)+6=0\\ 0, & \text{if } -q(T-at)+6<0\end{cases}.$ (8)

Based on the best response of the firm in Equation (8), we obtain the following results:

(1) If $-q(T-at)+6>0$, then $p=1$ and $(1-p)[(30+bt)-36]=0$,
we have $0\le q\le \frac{6}{T-at}$ and $h=0$. Therefore, ((1,0), (q , 1-q), (0, 1)) and (1,0) is a PBE, where $q\in[0,\frac{6}{T-at}]$ if $T+dt-ct<18$.

(2) If $-q(T-at)+6=0$, then we have

(i) $0<p<1$ and then we have $q=\frac{6}{T-at}$ if $bt=6$. This implies that ((p ,1- p), ($\frac{6}{T-at}$, 1- $\frac{6}{T-at}$), (0, 1)) and ($\frac{(T-at)p}{(T-at)p+6(1-p)}$,1- $\frac{(T-at)p}{(T-at)p+6(1-p)}$) is a PBE, where $p\in[0,1]$ if $T+dt-ct<18$.

(ii) $0<p<1$ and then we have $q=1$ if $bt>6$. This implies that ((p , 1- p), (0, 1), (0, 1)) and (1,0) is a PBE, where $p\in[0,1]$, if $T+dt-ct<18$.

(3) If $-q(T-at)+6<0$, then $p=0$, and we have the following results.

(i) $q=1$ if $bt>6$. Therefore, ((0, 1), (1, 0), (0, 1)) and (0, 1) is a PBE, if $T+dt-ct<18$.

(ii) $q \in [0,1]$ if $bt = 6$. Therefore, ((0, 1), (q , 1-q), (0, 1)) and (0, 1) is a PBE, if $T + dt - ct < 18$.
(iii) $q = 0$ if $bt < 6$. Therefore, ((0, 1), (0, 1), (0, 1)) and (μ , 1- μ) is a PBE, where $\mu \in [0,1]$, if $T + dt - ct < 18$.

Case 2. When $T + dt - ct \geq 18$, there exist three situations to discuss.

(1) If $\mu < A$, we have $h = 1$ according to Equation (2), $q = 0$ from Equation (4), and $p = 1$ from Equation (6), which implies that $\mu = 1$. This contradicts $\mu < A$. Therefore, there is no PBE for this situation.

(2) If $\mu > A$, we have $h = 0$ from Equation (2). For this situation, the best responses of verifier and firm based on Equations (4) and (6) become Equations (7) and (8), respectively.
According to Equation (8), we now consider three cases as follows.

Case (i) If $-q(T - at) + 6 = 0$ and $(1 - p)[(30 + bt) - 36] > 0$, then $\mu > A$, $0 < p < 1$, $q = 1$ and $h = 0$. Therefore, ((p , 1- p), (1, 0), (0, 1)) and (p , 1- p) is a PBE, if $T - at = 6$ and $bt > 6$. ***PBE(1)***

Case (ii) If $0 < -q(T - at) + 6 < 1$ ($p = 1$) and $(1 - p)[(30 + bt) - 36] = 0$ ($q \in [0,1]$), then $0 < q < \dfrac{6}{T - at}$, $p = 1$, and $h = 0$. In this case ((1, 0), (q , 1-q), (0, 1)) and (1, 0) is a PBE, if $T - at > 6$. ***PBE(2)***

Case (iii) If $-q(T - at) + 6 < 0$, then $p = 0$. In this case, we consider another three conditions for the best response of the verifier to derive the PBE of the game.

1) If $(30 + bt) - 36 > 0$, then $p = 0$, $q = 1$ and $h = 0$. Thus ((0, 1), (1, 0), (0, 1)) and (1, 0) is a PBE, if $T - at > 6$ and $bt > 6$. ***PBE(3)***
2) If $(30 + bt) - 36 < 0$, then we have $q = 0$ and $6 < 0$. This is a contradiction.
3) If $(30 + bt) - 36 = 0$, then $p = 0, q \in (\dfrac{6}{T - at}, 1]$, and $h = 0$. Therefore, ((0, 1), (q , 1-q), (0, 1)) and (0, 1) is a PBE, if $T - at > 6$ and $bt = 6$. ***PBE(4)***

(3) If $\mu = A$, recall $(1 - \mu)(T + dt - ct) - 18 = 0$, then $\mu = 1 - \dfrac{18}{T + dt - ct}$ and $h \in [0,1]$. Based on Equation (4), we discuss the best responses of the verifier by taking into account the following four cases.

Case (i) If $(1 - p)[(1 - h)(30 + bt) - 36] > 0$, then $q_5^* = 1, \mu_5^* = p_5^* = 1 - \dfrac{18}{T + dt - ct}$, $0 < h_5^* < \dfrac{bt - 6}{30 + bt}$. Therefore, ((p_5^* , 1- p_5^*), (1, 0), (h_5^* , 1- h_5^*)) and (μ_5^* , 1- μ_5^*) is a PBE, where $h_5^* \in (0, \dfrac{bt - 6}{30 + bt})$, if $T + dt - ct > 18$ and $bt > 6$. ***PBE(5)***

Case (ii) If $(1 - p)[(1 - h)(30 + bt) - 36] < 0$, then $q = 0$, $p = 1$, contradiction.

Case (iii) *If* $(1-p)[(1-h)(30+bt)-36]=0$, then $p=1$, $q\in[0,1]$ and $h\in[0,1]$, which satisfies $30qh-q(1-h)(T-at)+6>0$. Thus ((1, 0), (q, 1-q), (h, 1-h)) and (1, 0) is a PBE, where $30qh-q(1-h)(T-at)+6>0$ holds, if $T+dt-ct>18$. ***PBE(6)***

Case (iv) If $(1-p)[(1-h)(30+bt)-36]=0$ and $p\in[0,1)$, then we have $p_7^*=\dfrac{T+dt-ct-18}{T+dt-ct-18+18D}$, where $D:=1+\dfrac{6(T-at-bt)}{30+bt}$; $q_7^*=\dfrac{30+bt}{30+bt+6(T-at-bt)}$; $h_7^*=\dfrac{bt-6}{30+bt}$ and $\mu_7^*=1-\dfrac{18}{T+dt-ct}\in(0,1)$.

Therefore, ((p_7^*, 1-p_7^*), (q_7^*, 1-q_7^*), (h_7^*, 1-h_7^*)) and (μ_7^*, 1-μ_7^*) is a PBE, if $bt\geq 6$ and $T\geq\max\{18+ct-dt,\ at+bt\}$. ***PBE(7)***

Note that since $T\geq\max\{18+ct-dt,\ at+bt\}$ contains the following two conditions $T+dt-ct>18$ and $T-at-bt\geq 0$ that are necessary for $0\leq q_7^*\leq 1$ to be satisfied. When $T-at-bt=0$ is satisfied, ***PBE(7)*** becomes ***PBE(5)***; in other words, ***PBE(5)*** is a special case of ***PBE(7)***.

Although there are many cases to consider, we are particularly interested in the case where $T+dt-ct>18$ holds, which is also reflected by our experimental design. The intuition is that the additional earnings have to be high enough to make checking worthwhile for the official. To summarize the above analysis, there are multiple perfect Bayesian equilibria, including PBE(1)-PBE(7) above, when $T+dt-ct>18$ holds. The corresponding interior solutions, i.e., PBE(5) and PBE(7) are two typical perfect Bayesian equilibria of this game (identified by a superscript *).

Now we turn our attention to the effect of rotation treatments on the probability of truthful reporting by firms and the probability of honest verification by verifiers. By using a comparative-static analysis of the ***PBE(5)***, it is obvious that we obtain the following results:

$$\frac{\partial p_5^*}{\partial c}=\frac{-18t}{(T+dt-ct)^2}<0\text{ , and }\frac{\partial p_5^*}{\partial d}=\frac{18t}{(T+dt-ct)^2}>0.$$

Similarly, we have the comparative statics of the ***PBE(7)*** are as follows:

(i) $\dfrac{\partial p_7^*}{\partial a}>0$, since $\dfrac{\partial D}{\partial a}=\dfrac{-6t}{30+bt}<0$ and $\dfrac{\partial p_7^*}{\partial D}<0$; $\dfrac{\partial p_7^*}{\partial b}>0$, since $\dfrac{\partial D}{\partial b}=\dfrac{-6t(30+T-at)}{(30+bt)^2}<0$ and $\dfrac{\partial p_7^*}{\partial D}<0$. Since there is an increase in a and a decrease in b induced by RV, the treatment effect of RV on the probability of truthful reporting by firms is

$$\Delta p_{RV}^*=\frac{\partial p_7^*}{\partial a}-\frac{\partial p_7^*}{\partial b}=\frac{\partial p_7^*}{\partial D}\frac{6t(T-at-bt)}{(30+bt)^2}\leq 0\text{, since }T\geq at+bt.$$

(ii) $\dfrac{\partial q_7^*}{\partial a}=\dfrac{6t}{(30+bt+6(T-at-bt))^2}>0$; $\dfrac{\partial q_7^*}{\partial b}=\dfrac{6t(30+bt+(T-at-bt))}{(30+bt+6(T-at-bt))^2}>0$.

Therefore, the effect of the RV treatment on the probability of dishonest verification by verifiers is

$$\Delta q_{RV}^*=\frac{\partial q_7^*}{\partial a}-\frac{\partial q_7^*}{\partial b}=\frac{-6t(29+bt+(T-at-bt))}{(30+bt+6(T-at-bt))^2}<0.$$

(iii) $\Delta h_{RV}^{*} = \frac{\partial h_7^*}{\partial b} = \frac{36t}{(30+bt)^2} > 0$, which is the effect of the RV treatment on the probability of checking by the officials.

(iv) $\frac{\partial p_7^*}{\partial c} = \frac{-18Dt}{(T+dt-ct-18+18D)^2} < 0$; $\frac{\partial p_7^*}{\partial d} = \frac{18Dt}{(T+dt-ct-18+18D)^2} > 0$.

Since there is a decrease in c and an increase in d induced by the RO treatment, the treatment effect of RO on the probability of truthful reporting by firms is $\Delta p_{RO}^{*} = \frac{\partial p_7^*}{\partial a} - \frac{\partial p_7^*}{\partial c} + \frac{\partial p_7^*}{\partial d} > 0$.

(v) According to the result in ***(ii)*** above, one can see that the effect of the RO treatment on the probability of dishonesty verification by verifiers is $\Delta q_{RO}^{*} = \frac{\partial q_7^*}{\partial a} > 0$.

(vi) Now we analyze the effects of CR treatment under which there is an increase in a, a decrease in b and c, and an increase in d.

First, the effect of the CR treatment on the probability of truthful reporting by firms is $\Delta p_{CR}^{*} = \frac{\partial p_7^*}{\partial a} - \frac{\partial p_7^*}{\partial b} - \frac{\partial p_7^*}{\partial c} + \frac{\partial p_7^*}{\partial d} = \frac{(30+bt)^2 + 3(78+(b+c-d)t-T)(T-at-bt)}{(30+bt)^2(T+dt-ct-18+18D)^2} > 0$, since both $T+dt-ct-18 \le 60+T-at+bt$, and $T \ge at+bt$ hold. The former condition is an artifact of the design which ensures the official does not overly check.

Second, the effect of CR treatment on the probability of dishonest verification by verifiers is $\Delta q_{CR}^{*} = \frac{\partial q_7^*}{\partial a} - \frac{\partial q_7^*}{\partial b} < 0$.

Third, the effect of CR treatment on the check probability by officials is $\Delta h_{CR}^{*} = \frac{\partial h_7^*}{\partial b} > 0$.

(iv) We analyze the effects of rotation treatments on the level of under-reporting $\Delta x \equiv Y - X$ and the level of bribe transfers t on the PBE path. Given the official's belief of truthful-reporting μ^*, we have $\Delta x^* = \frac{18}{(1-\mu^*)P} + \frac{ct-dt}{P}$ and $t^* = \frac{1}{d-c}[\frac{18}{(1-\mu^*)} - \Delta x P]$. Moreover, holding all else constant, we obtain $\frac{\partial \Delta x^*}{\partial c} > 0$, $\frac{\partial \Delta x^*}{\partial d} < 0$ and $\frac{\partial t^*}{\partial c} > 0$, $\frac{\partial t^*}{\partial d} < 0$.

This suggests that both the average level of under-reporting and the average level of bribe transfers are decreased under the CR and RO treatment compared to the baseline. Because in the RV treatment both Δx^* and t^* are unaffected by the changes in a and b, from the theoretical perspective there is no treatment effect on the average level of under-reporting and the average level of bribe transfers in the RV treatment compared to that in the baseline

B. Table and Figures

Table 1: Reporting requirements across varied emissions trading schemes

	MRV guidance	Is verification required?	Who selects the verifier?	Who pays the verifier?	Is an official inspection required?	Penalty for misreporting?
Canada – Quebec Cap-and-Trade System[23]	Yes	Yes	Polluting firms	Polluting firms	No	Yes ($5,000-$500,000 in case of a natural person or imprisonment for less than 18 months; $15,000-$3,000,000 in other cases)
EU Emissions Trading System[24]	Yes	Yes	Polluting firms	Polluting firms	Yes (required in some member states, but not specified)	Yes (varies among member states and details are not specified)
Japan – Tokyo Cap-and-Trade Program[25]	Yes	Yes	Polluting firms	Polluting firms	Yes (not specified)	Yes (up to JPY500,000)
Korea Emissions Trading Scheme[26]	No	Yes	The Minister of Environment	Polluting firms	No	Yes (emitters with a fine up to KRW10 million)
New Zealand Emissions Trading Scheme[27]	Yes	No	N/A	N/A	Yes (not specified)	$25,000 in the case of an individual and $50,000 in the case of a body corporate
Swiss Emissions Trading Scheme[28]	Yes	No	Polluting firms	Polluting firms	Yes	No (CHF125/ton for non-compliance)
Turkey Carbon Market[29]	Yes (needs additional legislation)	Yes	Polluting firms	Polluting firms	No	No

Sources: https://icapcarbonaction.com/ets-topics/mvr-and-enforcement

[23]See http://www2.publicationsduquebec.gouv.qc.ca/dynamicSearch/telecharge.php?type=3&file=/Q_2/Q2R15_A.HTM

[24]See http://legisquebec.gouv.qc.ca/en/ShowDoc/cr/Q-2,%20r.%2015, and http://www.environmentportal.in/files/Art21-2009.pdf

[25]See https://www.kankyo.metro.tokyo.jp/en/climate/attachement/Tokyo_Cap-and-Trade_Program_detailed_version.pdf

[26]See http://www.law.go.kr/engLsInfoPWah.do?lsiSeq=104406 and http://unpan1.un.org/intradoc/groups/public/documents/apcity/unpan050317.pdf

[27]See http://www.mfe.govt.nz/publications/climate/seip-reporting-guidance/index.html, and http://www.legislation.govt.nz/act/public/2002/0040/latest/DLM158584.html, Subpart 4 of Part 4.

[28]Seehttp://www.bafu.admin.ch/klima/13877/14510/14719/index.html?lang=en

[29]See https://www.thepmr.org/system/files/documents/TUR-FINAL-MRP_2013-05-03%20Final.pdf. Page. 40-43

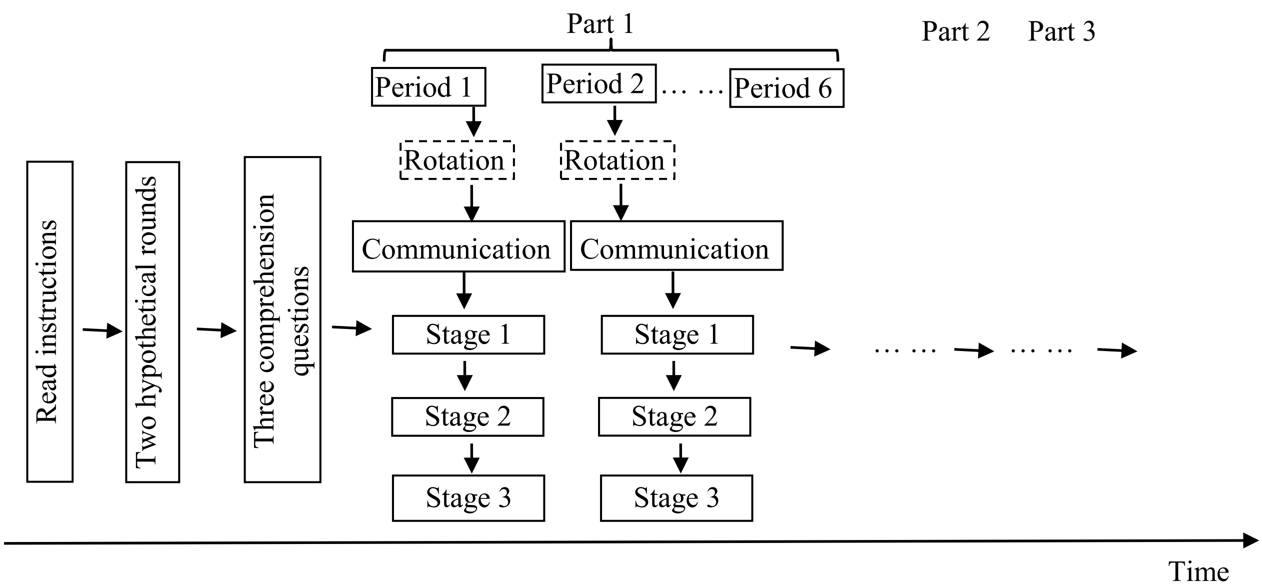


Figure 3: Experimental procedure

Figure 7: Selected level of under-reporting against transfer offered by firms

C. Additional Estimations

Table C1: Effect of rotation on firms' behavior (Tobit model)

Independent Variables	Under-reporting (1) Tobit linear reg	Transfer offered (2) Tobit linear reg
Panel A: Effect of complete rotation on Firms' behavior		
CR Treatment	-4.310***	-3.451**
	(1.672)	(1.383)
Period	0.254	0.301
	(0.358)	(0.297)
Part	-2.940	-2.680
	(2.277)	(1.832)
Price	0.846***	1.064***
	(0.285)	(0.237)
ReportY_lag	0.862	0.326
	(1.152)	(0.945)
Check_lag	-5.912***	-4.936***
	(1.858)	(1.362)
Intercept	2.626*	-0.181
	(1.438)	(1.086)
# of Observation	552	552
Panel B: Effect of RV on Firms' behavior		
RV Treatment	-1.823	-1.721
	(1.738)	(1.366)
Period	-0.043	0.010
	(0.368)	(0.300)
Part	-0.006	-0.090
	(2.194)	(1.736)
Price	0.463	0.780**
	(0.429)	(0.338)
ReportY_lag	1.905*	1.011
	(1.041)	(0.937)
Check_lag	-7.438***	-5.989***
	(1.602)	(1.091)
Intercept	1.341	-1.238*
	(0.887)	(0.639)
# of Observation	508	508
Panel C: Effect of RO on Firms' behavior		
RO Treatment	-2.800	-2.073
	(1.981)	(1.688)
Period	0.093	0.145
	(0.341)	(0.266)
Part	-1.256	-1.456
	(2.338)	(1.765)
Price	0.964***	1.130***
	(0.367)	(0.316)
ReportY_lag	-0.308	-0.345
	(1.131)	(0.928)
Check_lag	-6.056***	-4.804***
	(1.274)	(0.966)

Intercept	0.437	-1.342
	(1.387)	(0.944)
# of Observation	497	497

Notes: Tobit regressions include baseline and CR, baseline and RV, and baseline and RO observations for Panel A, Panel B, and Panel C, separately. Dependent variables are censored from below 0. As in Table 3, "Untruthful", "ReportY_lag" and "Check_lag" are all dummy variables. "Untruthful" = 1 if the firms report untruthfully, "ReportY_lag" = 1 if the matched verifier in the preceding period chooses Report Y, "Check_lag" = 1 if the matched official in the preceding period chooses Check. Standard errors in parentheses clustered at the subpopulation level. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C2: Effect of rotation on firms' behavior by parts

Panel A: Effect of complete rotation on Firms' behavior

Independent Variables	Under-reporting (1)			Transfer offered (2)			Untruthful (dummy) (3)			Untruthful (marginal values) (4)		
	Part 1	Part 2	Part 3	Part 1	Part 2	Part 3	Part 1	Part 2	Part 3	Part 1	Part 2	Part 3
CR Treatment	-2.17	-1.72*	-2.41**	-2.04	-1.44*	-1.74**	-0.75	-0.48**	-0.72***	-0.28	-0.19**	-0.27***
	(1.870)	(0.908)	(0.845)	(1.352)	(0.681)	(0.758)	(0.569)	(0.231)	(0.250)	(0.197)	(0.086)	(0.084)
Period	0.64*	-0.17	0.08	0.69**	-0.11	0.17	0.13	-0.03	0.04	0.05	-0.01	0.01
	(0.322)	(0.219)	(0.318)	(0.241)	(0.197)	(0.233)	(0.099)	(0.056)	(0.088)	(0.036)	(0.022)	(0.033)
Price	0.61	0.40*	0.18	0.91*	-.54***	0.44**	0.23	0.13**	0.08*	0.09	0.05**	0.03*
	(0.580)	(0.216)	(0.156)	(0.422)	(0.152)	(0.155)	(0.184)	(0.059)	(0.044)	(0.067)	(0.023)	(0.017)
ReportY_lag	0.80	-0.92	1.48	0.42	-1.07	0.91	0.12	-0.29	0.39	0.04	-0.11	0.15
	(1.131)	(1.372)	(1.058)	(0.793)	(1.079)	(0.974)	(0.322)	(0.428)	(0.270)	(0.121)	(0.158)	(0.106)
Check_lag	-1.87$^{\Delta}$	-1.93*	-2.93***	-1.76**	-1.62**	-2.44***	-0.77**	-0.42	-1.17***	-0.30**	-0.16	-0.33***
	(1.043)	(0.927)	(0.806)	(0.683)	(0.718)	(0.515)	(0.329)	(0.353)	(0.304)	(0.120)	(0.120)	(0.067)
Intercept	0.71	4.84*	3.11	-2.51	2.42	-0.74	-1.00	-0.20	-0.80			
	(2.840)	(2.478)	(5.047)	(2.105)	(2.156)	(3.800)	(0.918)	(0.644)	(1.412)			
# of Observation	159	184	209	159	184	209	159	184	209	159	184	209

Panel B: Effect of RV on Firms' behavior

Independent Variables	Under-reporting (1)			Transfer offered (2)			Untruthful (dummy) (3)			Untruthful (marginal values) (4)		
	Part 1	Part 2	Part 3	Part 1	Part 2	Part 3	Part 1	Part 2	Part 3	Part 1	Part 2	Part 3
RV Treatment	-1.91	-0.61	-0.46	-1.71*	-0.87	-0.50	-0.64*	-0.20	-0.16	-0.25*	-0.08	-0.06
	(1.147)	(1.366)	(0.803)	(0.794)	(0.970)	(0.685)	(0.353)	(0.376)	(0.221)	(0.129)	(0.149)	(0.088)
Period	-0.20	0.07	-0.06	-0.12	0.07	-0.01	-0.11	0.02	-0.01	-0.04	0.01	-0.003
	(0.238)	(0.262)	(0.294)	(0.255)	(0.219)	(0.227)	(0.081)	(0.071)	(0.074)	(0.031)	(0.028)	(0.030)
Price	0.10	0.26	0.19	0.36	0.51**	0.53**	0.08	0.08	0.07	0.03	0.03	0.03
	(0.321)	(0.355)	(0.264)	(0.258)	(0.209)	(0.214)	(0.100)	(0.092)	(0.067)	(0.038)	(0.037)	(0.027)
ReportY_lag	0.22	1.63	1.75	-0.07	0.40	1.14	0.06	0.31	0.55*	0.02	0.12	0.21*
	(0.528)	(1.252)	(1.262)	(0.470)	(1.037)	(1.04)	(0.132)	(0.332)	(0.331)	(0.051)	(0.128)	(0.118)
Check_lag	-1.15	-4.04**	-3.12**	-2.01*	-3.02**	-2.59***	-0.62	-1.38**	-0.97***	-0.24	-0.43***	-0.34***
	(1.619)	(1.426)	(1.01)	(0.959)	(1.236)	(0.756)	(0.443)	(0.666)	(0.281)	(0.167)	(0.133)	(0.090)
Intercept	6.31***	3.06	5.22	3.15	0.83	1.56	0.63	-0.43	-0.11			
	(1.69)	(2.970)	(5.088)	(1.753)	(2.347)	(4.058)	(0.588)	(0.822)	(1.295)			
# of Observation	146	175	187	146	175	187	146	175	187	146	175	187

Panel C: Effect of RO on Firms' behavior												
Independent Variables	Under-reporting (1)			Transfer offered (2)			Untruthful (dummy) (3)			Untruthful (marginal values) (4)		
	Part 1	Part 2	Part 3	Part 1	Part 2	Part 3	Part 1	Part 2	Part 3	Part 1	Part 2	Part 3
RO Treatment	-1.90	-1.25	-1.15	-0.63	-1.10	-1.07	-0.56	-0.38	-0.38▲	-0.21	-0.15	-0.15∆
	(1.488)	(1.023)	(0.921)	(1.204)	(0.786)	(0.794)	(0.427)	(0.286)	(0.238)	(0.162)	(0.110)	(0.092)
Period	-0.03	-0.06	0.14	0.04	-0.04	0.27	-0.03	-0.02	0.05	-0.01	-0.01	0.02
	(0.225)	(0.217)	(0.336)	(0.239)	(0.131)	(0.236)	(0.068)	(0.053)	(0.084)	(0.027)	(0.021)	(0.033)
Price	0.25	0.52***	0.41	0.48	0.59***	0.74***	0.12	0.16***	0.13**	0.04	0.06***	0.05**
	(0.457)	(0.156)	(0.246)	(0.392)	(0.149)	(0.202)	(0.140)	(0.045)	(0.060)	(0.052)	(0.018)	(0.024)
ReportY_lag	-0.24	-1.60	0.97	-0.30	-1.23	0.93	-0.18	-0.36	0.22	-0.07	-0.14	0.09
	(0.764)	(0.941)	(0.758)	(0.694)	(0.757)	(0.617)	(0.228)	(0.240)	(0.187)	(0.087)	(0.091)	(0.074)
Check_lag	-3.34***	0.13	-3.71***	-3.63***	1.06	-3.14***	-1.15***	0.02	-1.19***	-0.43***	0.01	-0.38***
	(1.046)	(1.672)	(1.174)	(0.760)	(1.512)	(0.774)	(0.381)	(0.425)	(0.367)	(0.110)	(0.169)	(0.091)
Intercept	5.11	3.26	1.16	2.10	1.50	-3.58	0.20	-0.45	-1.19			
	(2.35)	(2.147)	(5.884)	(2.255)	(1.401)	(4.261)	(0.710)	(0.509)	(1.477)			
# of Observation	136	176	185	136	176	185	136	176	185	136	176	185

Notes: Standard OLS and Probit framework are used for Column (1) – (2) and Column (3), separately. Column (4) reports the corresponding marginal values of Column (3). Part 1 includes observations from Period 1 – Period 6, Part 2 includes observations from Period 7 – Period 12, and Part 3 includes observations from Period 13 – Period 18. As in Table 3, "Untruthful", "ReportY_lag", and "Check_lag" are all dummy variables. "Untruthful" = 1 if the firms report untruthfully, "ReportY_lag" = 1 if the matched verifier in the preceding period chooses Report Y, "Check_lag" = 1 if the matched official in the preceding period chooses Check. Standard errors in parentheses clustered at the subpopulation level. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, ▲ p=0.108, ∆ p=0.101.

D. Instructions for the Rotation of the Official

(Other treatments are analogous)

General 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 0.5 AUS$. How much money you earn depends on your decisions and on the decisions of other participants.

In total, this experiment consists of 3 parts with 6 rounds per part. There are three types of participants: **Type-1** or **Type-2** or **Type-3**. 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.

<u>The Matching Rule</u>

At the beginning of the experiment, participants will be randomly assigned one of the three types and the assignment of type remains unchanged throughout the experiment. Types of participants will be anonymously matched to groups of three, i.e. one Type-1, one Type-2, and one Type-3 will be randomly assigned to one group. **As regards Type-1 and Type-2**, the groups remain unchanged. In every round, **a Type-3**, however, might **rotate out of the group** and might be substituted with another Type-3. Whether this happens, or not, is a random process. Thus, **both the Type-1 and Type-2 do not know which Type-3 they interact with in a particular round**.

<u>Motivation</u>

In each round, **Type-1** makes decisions for a company producing Y units of some unspecified good. To produce Y units a corresponding number of licenses (X) for each unit of good produced should be held by Type-1. Each unit of license is valued at a price, P. Depending on your decision and the decisions of others, P might be reduced in the following round.

Type-1 moves first and decides how many units (Y) s/he wants to produce and the number of licenses (X) s/he wants to report. S/he has the following options:

1. Licenses X= produced units Y
2. Licenses X< produced units Y and offer some of the benefits to Type-2 and Type-3 by choosing a positive transfer, t.

Type-2 and Type-3 examine whether Type-1 have complied with the rule of holding one license for each unit produced. In fact, **Type-2** is always informed about the discrepancies of the chosen X and Y, moves second and has the following choice:

1. Report X to Type-3
2. Report Y to Type-3

Type-3, in contrast, is not automatically informed about the discrepancies but can perform a (costly) check. Type-3 moves third and has the following two options:

1. Check and deduct E$ from the round-payoffs of Type-1 and Type-2 if s/he discovers discrepancies.
2. Do not check.

The detailed rules are explained later in the **Detailed Instructions**.

<u>Procedure (How the experiment unfolds.)</u>

Part 1 includes round 1 – 6, Part 2 includes round 7 –12, and Part 3 includes round 13 – 18 (see Figure 1 below).

After the first round, each round starts with the rotation of Type 3 and is followed by a communication stage and a decision-making stage.

Each decision-making stage may have up to 3 stages:

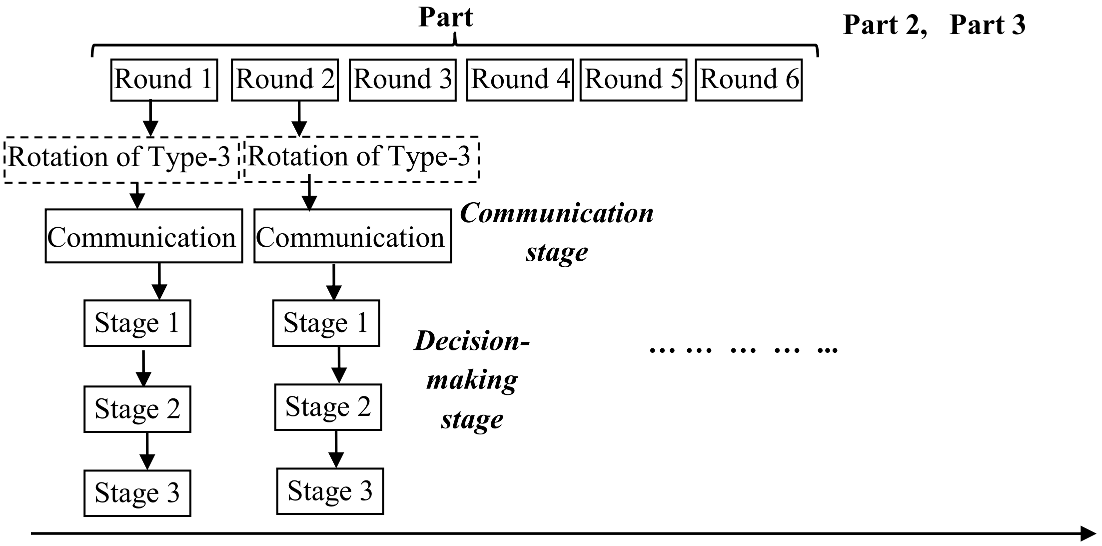


Figure 1 The experimental procedure

Communication stage:

In this stage, Type-1 and Type-2 within the same group will have the opportunity to communicate before making decisions. The communication is made possible by typing messages through a **Chat Box**. Type-1 and Type-2 may message each other in **the blue rectangular area** at the bottom. Once you are done, press "Enter" on your keyboard. Your message will subsequently be displayed in the Chat Box window.

Although we will record the messages you send to each other, your identity remains anonymous. We ask that you discuss your choices only.

You are expected to follow two basic rules: (1) Be civil to one another (no threats); and (2) Do not reveal your identity to others in any manner (like "I am participant no...." or "my name is…").

Each communication period lasts 60 seconds. When time runs out, you will be cut off and the decision-making stage will be triggered. If you press the "OK" button beneath the Chat Box entry area, you withdraw irreversibly from the communication period. The communication period will last less than 60 seconds only if all participants in a session have pressed the "OK" button.

Details of the decision-making stage will be provided in the **Detailed Instructions**.

(Figure 2, i.e. the decision tree, is distributed together with the Detailed Instructions)

Detailed Instructions

Stage 1*(Stage 1 in Figure 2 (in red) shows two possible choices that a Type-1 has.)*

At the beginning of a round, **a Type-1** determines Y the units of the good produced and X the number of licenses reported. A Type-1 has the following options:

1. Choose Y from **any integer number between 1 and 100**, where 1 and 100 are included.
2. Choose X whereas **X may be equal or smaller than Y** and **has to be chosen from [Y-10, Y]**.

Example 1: If Y = 50, X would be chosen from [40, 50]; if Y≤10, X would have to be chosen from [1, Y]. (See below)

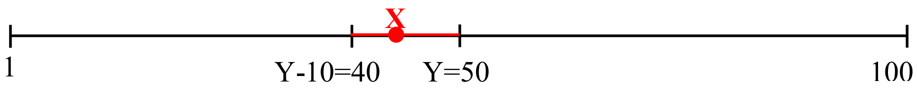


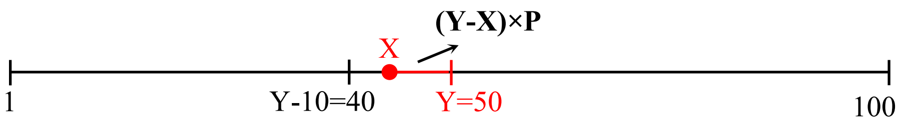
Table 1 Type-1's possible choices and outcomes

Stage 1: Type-1's choices

Choice 1	**Choose X=Y**	**Choose X < Y**
Choice 2	**No transfer**	**transfer, t (t>0)** • In order to **influence the actions of Type-2 and Type-3**, Type-1 **transfers t E$ to them.** • **Transfer, t,** has to be chosen from {1,2,3,4,5,6,7,8,9} (see Screenshot 1 below)
Outcomes	No additional earnings And continue to Stage 2	**Obtain potential additional earnings** by selling the difference of X and Y at a price P; i.e. (Y-X) × P. And continue to Stage 2. Additional earnings depend on: 1. Level of P. (P is initially set at 6, and may decrease over time. This depends on your group's decision and decisions of other groups. The larger the aggregate amount of Y-X, the more likely it is that P decreases in the following round.) 2. Decisions of Type-2 and Type-3 that Type-1 is matched with.

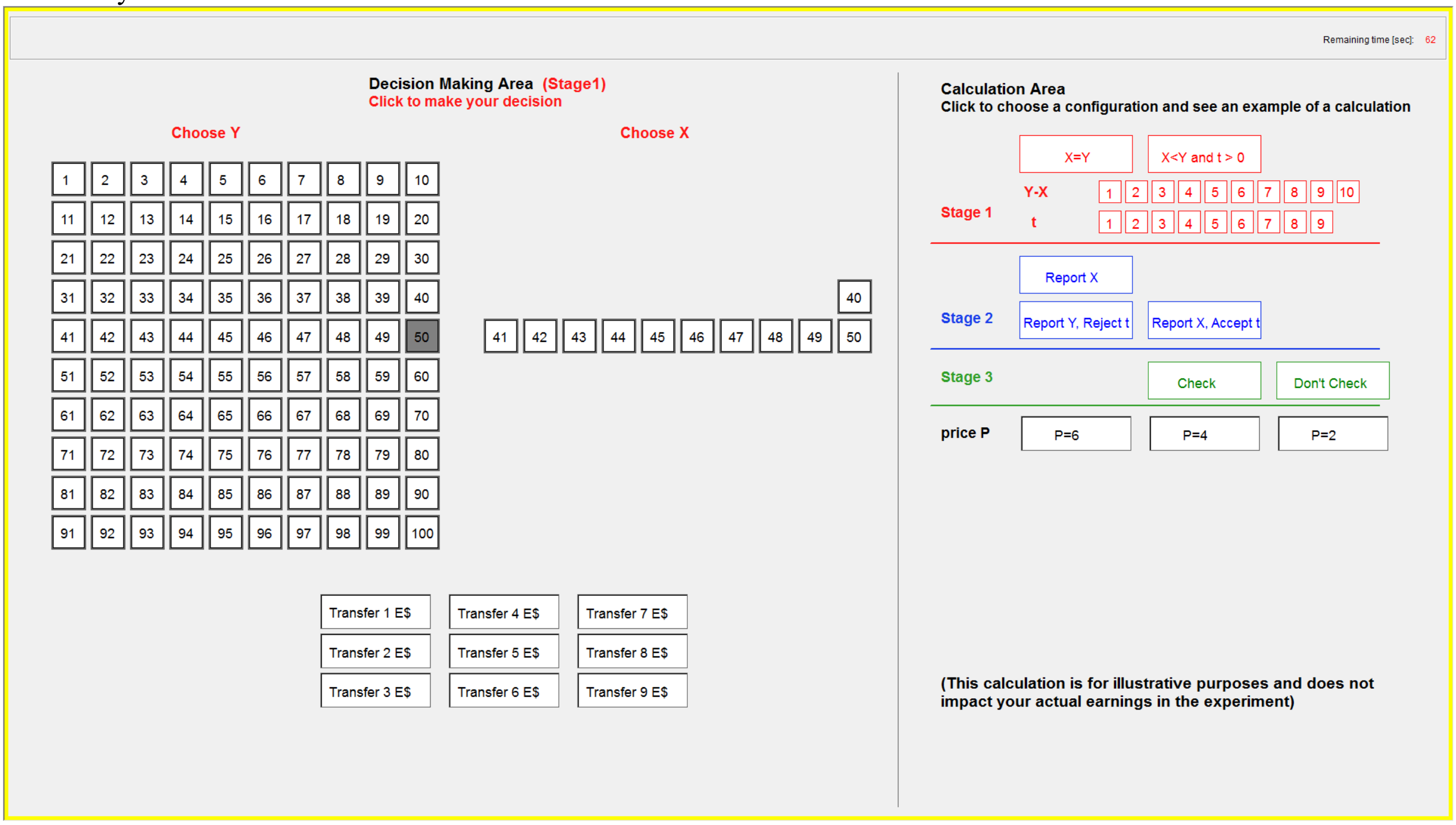
Note that as soon as P falls to zero, both the Type-1 and the Type-2 receives 30 E$ and the Type-3 receives 20 E$.

Example 2: If the Type-1 selected Y=50 and X= 42, and P = 6, then the additional earnings would be 48 E$, since (50-42) × 6 = 8×6 = 48.



<u>How to enter your decision in the system</u>

The following text explains Screenshot 1 (below) and use of Figure 2 in order to guide you to make your best decision.



Screenshot 1

The **Decision Making Area** is on the left (see Screenshot 1 below): Type-1 enters choice for Y first (on left) and then choice for X (on the right). The possible range for X will show up as soon as Y has been chosen. When X<Y, Type-1 should also choose transfer, t, which are displayed at the bottom.

A **Calculation Area** is shown on the right hand side. Participants can use it to make themselves familiar with their options and the decision-making process. You can find out the payoffs that come

with the various options by clicking on the button that will show up once you have made choices.

Decision Tree (Figure 2): Stage 1 shows the two possible choices and corresponding round-payoffs (in red) that a Type-1 participant has.

Stage 2*(Stage 2 in Figure 1 (in blue) shows possible choice(s) that a Type-2 has.)*

At the second stage of a round, **a Type-2** is informed of the chosen X and Y, and the amount of transfer, t, from the Type-1. **A Type-2 is required to report either X or Y to Type-3.** The following decision options are applied.

Table 2 Type-2's possible choices and outcomes

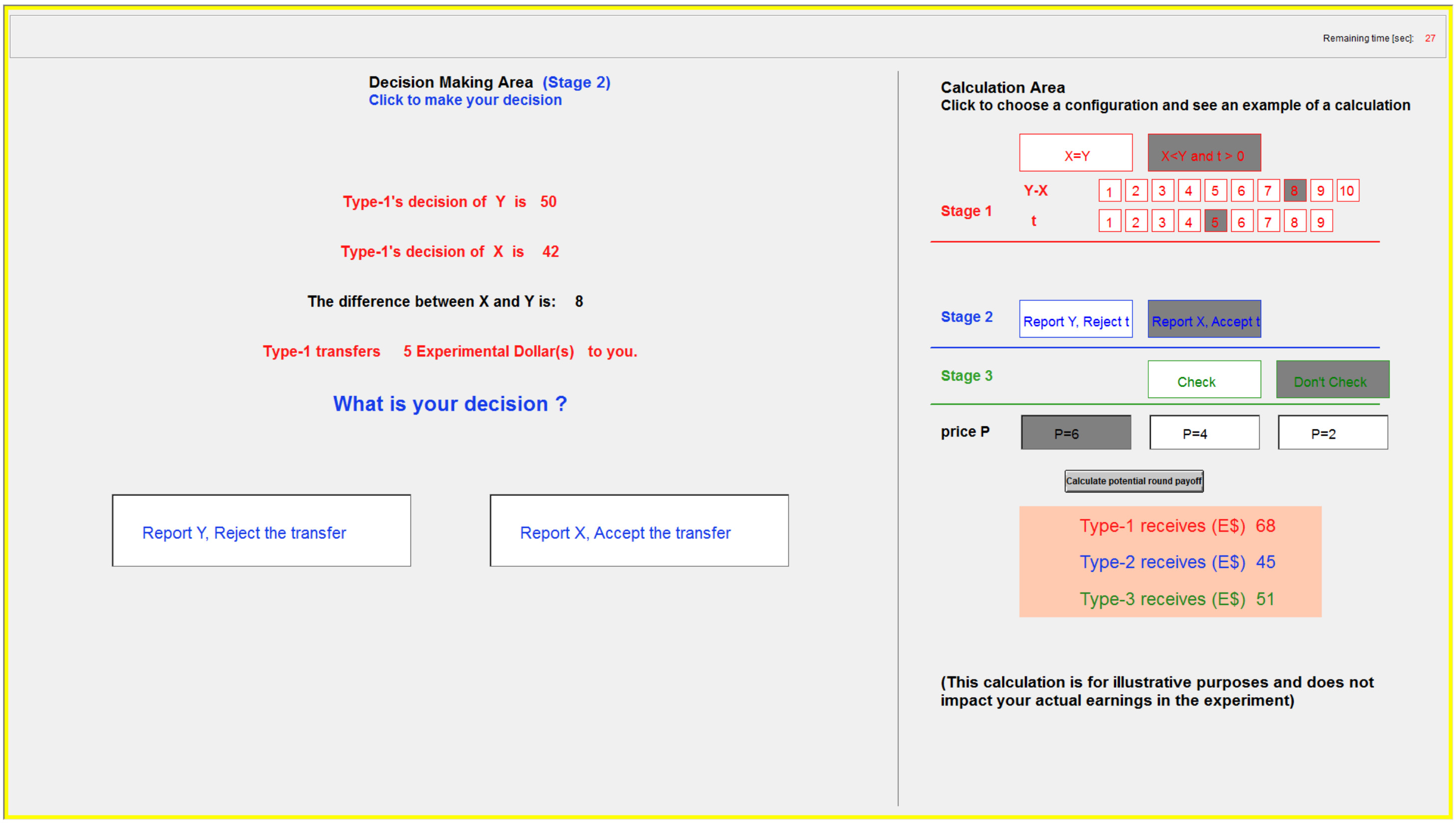
Stage 1: Type 1's choices	Choose X = Y & no transfer	Choose X<Y & transfer t>0

Stage 2: Type 2's choices	Report X	Report X and Accept the transfer	Report Y and Reject the transfer
Outcomes	Continue to Stage 3	Continue to Stage 3	Round ends

<u>How to enter your decision in the system</u>

Decision Making Area: On the left, information of choice of Type-1 is provided and Type-2 has to enter choice of reporting either X or Y.

Calculation Area: Type-2 can choose any configuration of decisions and explore the payoffs that come with various options (See Screenshot 2 below for an example).



Screenshot 2

Decision Tree (Figure 2)**:** Stage 2 shows the possible choice(s) and corresponding payoffs (in blue) that a Type-2 may have.

<u>Stage 3</u>*(Stage 3 in Figure 1 (in green) shows possible choice(s) that a Type-3 has.)*

At the third stage of a round, **a Type-3** is informed about the reported value X. Without knowing if there is a discrepancy between X and Y, the Type-3 has to choose between **"Check"** and **"Don't Check"** in the **Decision Making Area**. The following decision options are applied.

Table 3 Type-3's possible choices and outcomes

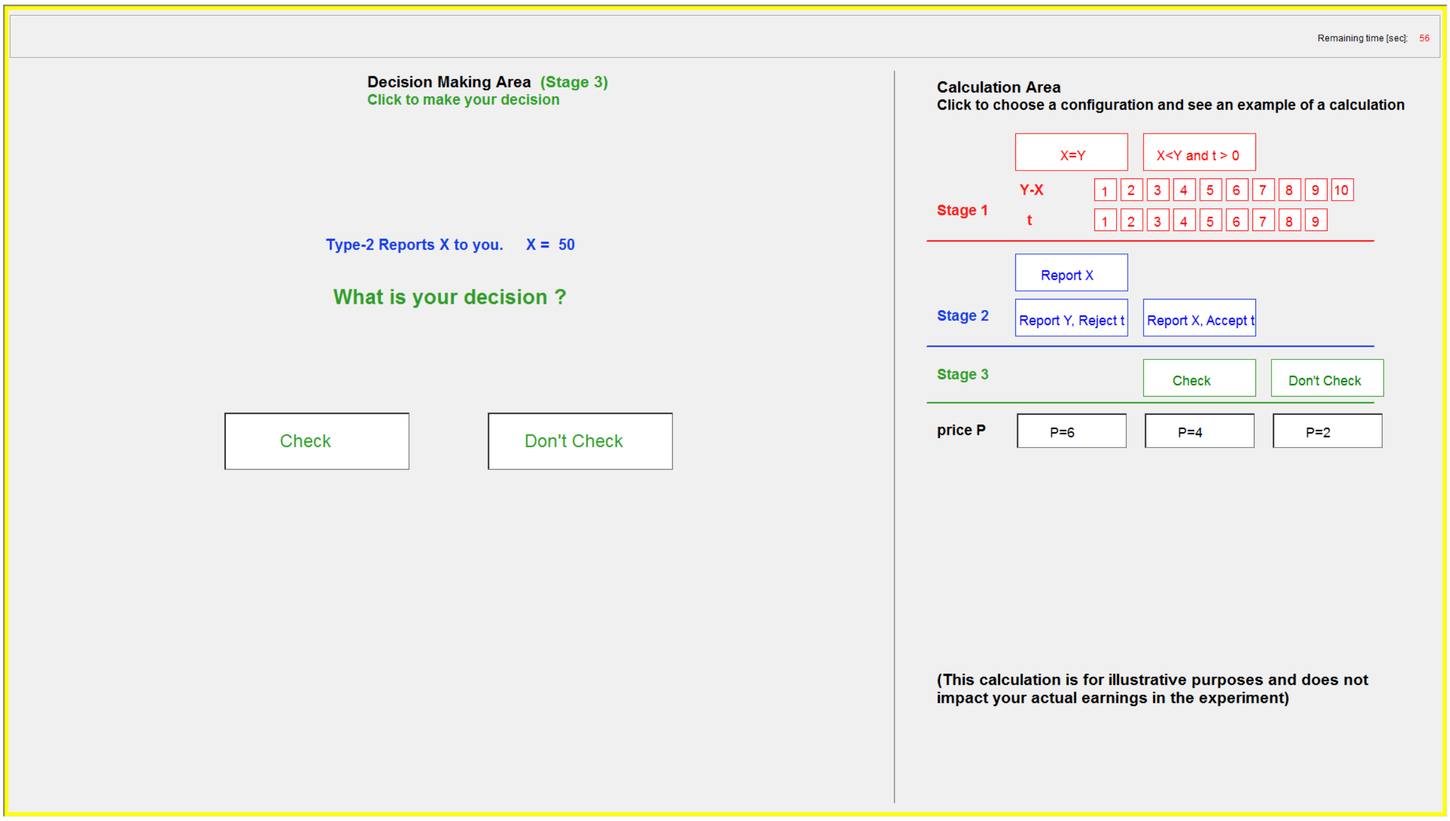
<table>
<tr><td>Stage 1: Type 1’s choices</td><td colspan="2">Choose X = Y & no transfer</td><td colspan="3">Choose X<Y & t>0</td></tr>
<tr><td>Stage 2: Type 2’s choices</td><td colspan="2">Report X</td><td colspan="2">Report X and Accept t</td><td>Report Y and
Reject t</td></tr>
<tr><td>Stage 3: Type 3’s choices</td><td>Check</td><td>Don’t Check</td><td>Check</td><td>Don’t Check</td><td>No action</td></tr>
<tr><td>Outcomes</td><td>Round payoffs</td><td>Round payoffs</td><td>• Type-1 and Type-2 receives 0
• Besides the Round Payoff, Type-3 receives the additional earnings and 3t in this round.
• In the remaining round of that part, all the three types in this situation are not allowed to participate.</td><td>Besides Round payoffs
• Type-1 receives the additional earnings and loses 2t
• Both Type-2 and Type-3 receives 3t</td><td>Round payoffs</td></tr>
</table>

Note that a Type-3 has a chance to make a choice only if Type-2 reports the value of X.

Also note that, when participants are not allowed to participate, both the Type-1 and Type-2 receive 0; and if the price P>0 the Type-3 receives 36 E$, if the price P=0 the Type-3 receives 20 E$.

How to enter your decision in the system

Decision Making Area: On the left, the Type-2’s decision is provided, and Type-3 has to choose between check and don’t check (See Screenshot 3 below).



Screenshot 3

Calculation Area: A Type-3 can also use the Calculation Area to explore her/his best decision.

Decision tree (Figure 2)**:** Stage 3 shows the possible choice(s) and corresponding payoffs (in green) that a Type-3 may have.

When you have made your decision, please press to allow everyone to move on to the next stage. If you have run out of time, the computer will remind you to make a decision by displaying **"Please make your decision! We need to move on."**

<u>End of round</u>

After stage 3, that round ends. Participants' round-payoffs are the results of all decisions made by Type-1, Type-2 and Type-3 within their group. At the end of each round, participants will be informed of all the decisions and their own payoff for that round.

<u>Payment</u>

To calculate your payments we will randomly **select one of your round-payoffs in each part**. Your final earnings will be the **average of the three randomly selected round-payoffs**.

<u>End of Experiment</u>

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, in addition to a show-up fee of 5 Australian dollars.