Breaking bad:
Malfunctioning institutions erode good behavior

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Abstract

This paper studies whether malfunctioning institutions erode good behavior. We use a large-scale online experiment, in which participants play a repeated observed cheating game. When we ask participants to report honestly and promise no control, we find low cheating rates. When control of truthful reporting is introduced, low cheating rates remain. In our main treatment with a malfunctioning institution, participants do not know whether they are in the treatment with or without control. In this treatment, participants who do not face control for some rounds start cheating significantly more often, reaching highest cheating rates. That is, a malfunctioning institution leads to more cheating than no institution at all, which indicates that the development of cheating behavior is endogenous to the institutions. Our findings suggest a novel negative effect of unenforced laws.

Key Words: Lying, Cheating, Institutions, Control, Crowding-Out, Experiment.

JEL Classification: C90, D81, D82, D91.

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1 Introduction

The importance of institutions for the functioning of society is enormous. The economics literature has devoted considerable attention to institutions that help maintain desirable outcomes for society. For instance, punishment institutions have been found to be useful for maintaining cooperation (e.g., Fehr and Gächter, 2000; Gürerk et al., 2006; Nikiforakis and Normann, 2008; Dai et al., 2015). The other stream of research emphasizes the possible detrimental effect of audit/control institutions, because they crowd out intrinsic motivation and thus change people's behavior (e.g., Frey and Oberholzer-Gee, 1997; Gneezy and Rustichini, 2000; Frey and Jegen, 2001; Fehr and Rockenbach, 2003; Dickinson and Villeval, 2008). However in reality, especially in context of developing countries, some rules that are in place de facto are not enforced, i.e. institutions are malfunctioning. The unenforced laws are also common in developed countries, and their effects are debated among law scholars.[1] The focus of this paper is to study the effects of malfunctioning institutions, that is, institutions that are supposedly in place but are likely not functioning. We hypothesize that the potential positive impact of strong control institutions[2] on "good" behavior could be reversed if uncertainty exists regarding whether the institutions are functioning.

We study the effect of malfunctioning intitutions on good behavior in the context of a cheating game.[3] In an online experiment, we use a version of the observed cheating game

[1] The law scholars' views range widely. Some scholars argue that the unenforced laws suggest the norm of good behavior and thus have a positive effect despite no enforcement (e.g. Cooter, 1998). Other scholars claim that the effect of unenforced rules is detrimental, either because the violators might feel that they are being perceived as criminals (Leslie, 2000) or because unenforced rules impose an externality of discomfort for non-violators who observe the violations (Depoorter and Tontrup, 2016). Finally, some scholars argue that unenforced laws are problematic, because they undermine the power of law. This argument was captured by Justice Brandeis in Olmstead v. United States, 227 U.S. 438, 485, 48 S.Ct. 564, 575, 72 L.Ed. 944 (1928): "Our government… teaches the whole people by its example. If the government becomes the lawbreaker, it breeds contempt for law; it invites every man to become a law unto himself; it invites anarchy."

[2] We call a control institution "strong," when the control probability and the penalty are high enough to change the behavior of utility maximizers.

[3] Honesty is crucial for the functioning of societies and economies (e.g., Mauro, 1995; Pranab, 1997; Olken and Pande, 2012). From a homo-economicus perspective, a person will cheat for a monetary benefit, if both the possibility of being caught and the penalty are low enough (see Becker, 1968). However, the literature on lying

(Gneezy et al., 2018) repeated over 20 rounds. Experimental participants observe a number between 1 and 10 and are asked to report it to the experimenter. In the baseline treatment (NoControl), participants' monetary payoff is equal to the number they report in euros. Participants are told they are expected to report truthfully and they will not be controlled. By contrast, in the treatment with a strong institution (Control30), participants are informed that in each round, they have a 30% probability of being controlled. If they are controlled, participants receive the number of euros corresponding to the number they have reported, if they told the truth; and if they have misreported the number, they receive zero. Participants learn after each round whether a control was in place. We expect the strong control institution to work well in monitoring people's truthfulness and the controlling effect to outweigh possible crowding-out effects. These two treatments serve as important benchmarks for the malfunctioning institution treatment.

In many situations, strong institutions cannot be implemented. Despite the presence de jure of robust and modern institutions in many cases, they malfunction de facto. Examples of malfunctioning institutions range widely. For instance, in Indonesia, drug users could be punished by a prison sentence, but in practice, their punishment is often restricted to a bribe paid to a police officer. Corruption by public officials is one of the most widespread crimes in many developing countries, because the punishment of the officials is often not enforced in practice. Furthermore, even in developed economies, tax returns are often only checked superficially and waved through, if no noticeable irregularities are detected. Finally, due to the COVID-19 pandemic, many countries introduced quarantine laws and rules. Some of these

in economics shows people will often forego an opportunity to lie, even when there is no monetary penalty for lying (see Gneezy, 2005; Shalvi et al., 2011; Fischbacher and Föllmi-Heusi, 2013; Abeler et al., 2014; Gächter and Schulz, 2016; Kajackaite and Gneezy, 2017; Gneezy et al., 2018; Abeler et al., 2019, among many others).

rules, however, could not be enforced.[4] Taken together, in all these examples, even though institutions are used as a threat, they are not enforced.

What all these malfunctioning institutions have in common is that, when introduced, uncertainty exists regarding their enforcement. Over time, with experience, people develop endogenous beliefs about the probability that the law can be enforced. The longer they observe unpunished violations, the greater the likelihood that the institution is malfunctioning.[5] We introduce this uncertainty about the institution that is in place in the third treatment. In this treatment, the chance of being in the NoControl or Control30 treatment is 50-50. After each round, independently of their truthfulness, subjects learn whether they have been controlled. We distinguish between two sub-treatments: 50-50NoControl and 50-50Control30. Participants in the 50-50Control30 sub-treatment learn they are in this sub-treatment after the first round, in which control is implemented. Those in the 50-50NoControl sub-treatment never deterministically learn they are in this sub-treatment,[6] and we refer to this sub-treatment as a malfunctioning-institution treatment. Our central hypothesis is that over time, subjects will lie more in the malfunctioning-institution sub-treatment than in the treatment with no institution.[7]

The results from our online experiment are in line with our central hypothesis. Participants break bad[8] and cheat the most when the institution is malfunctioning—the 50-

[4] For instance, Switzerland required people coming from risk regions to isolate themselves from other household members within their place of residence; violations could lead to fines up to 10,000 CHF. Such a law is hard to implement, because verifying whether family members are remaining separate within the household is difficult.
[5] For example, when submitting a tax return in a new country, we do not know how thoroughly tax filings are being checked, but will find out over time. If we do not get audited or observe others not being audited for a long time, we might believe the audits are not being performed despite the promise. The same goes for the quarantine rules—we do not know whether we will actually be inspected, but we learn what kind of institution is in place over time.
[6] However, note that after 10 rounds without control, the probability that the sub-treatment is without control is 97.25%.
[7] An ideal test for our hypothesis would be running a treatment, in which we announce the Control30 treatment, but never control and allow subjects to communicate with others whether they were controlled. This design would involve deception, so we believe our design captures the main feature of the malfunctioning institution environment respecting the no-deception rule.
[8] By "breaking bad," we refer to increased lying rates over time.

50NoControl treatment has the lowest truthfulness rates. We also find that, contrary to our hypotheses, a strong control institution does not lead to more overall truthfulness than NoControl.

Further analyses of lying on the intensive and extensive margins show treatment effects that are masked by the overall truthfulness rates. Although Control30 does not lead to different truthfulness rates than NoControl, more subjects lie at least once in the course of the experiment in Control30 than in NoControl. This detrimental effect is counteracted by subjects in Control30 lying less on the intensive margin than in NoControl—conditional on lying at least once, paricipants in Control30 lie less often than in NoControl.

The malfunctioning institution, on the other hand, combines the worst of the two worlds and erodes good behavior. The proportion of subjects who lie at least once in this treatment is similar to the proportion of subjects who lied at least once in Control30, and, conditional on lying at least once, with experience, subjects in 50-50NoControl lie as often as subjects who lied at least once in NoControl. These results suggest that although the potential presence of a strong control institution crowds out some participants' honesty, learning over time that one is almost certainly in NoControl does not prompt honesty, but allows the individual to lie (almost) without the risk of a punisment.

What else can explain our main result that the malfunctioning institution leads to the lowest level of honesty? One possibility is that the result is driven by the, in expectation, very small control probability that crowds out intrinsic motivation for honesty while posing no risk of being controlled. This effect would be in line with Gneezy and Rustichini (2000), who show that very small incentives might negatively affect behavior. We run an additional treatment to control for this small chance of a fine effect: in this treatment, subjects know the probability of control is 0.03%, the exact control probability of the Bayesian updater in the last round of the

malfunctioning-institution treatment. However, we find no significant difference between NoControl and Control0.03 in the proportion of misreports. Thus, the effect of the malfunctioning institution cannot be explained simply by the crowding out due to the small probability of control. The effect goes beyond a "pay-enough-or-don't-pay-at-all" type of behavior. We conclude that the *threat* of the *strong* control institution is what crowds out honest behavior in the malfunctioning-institution sub-treatment. That is, threating individuals with a strong but not implementable institution leads to more overall lies than having a functioning weak institution in place (Control0.03) or no institution at all (NoControl).

Our results thus provide a new cause for a potential erosion of good behavior – a malfunctioning intitution. We believe that this detrimental effect goes beyond the context of cheating behavior.

Related literature. Our paper relates to several streams of research in economics and behavioral economics. First, it broadly relates to the above-discussed experiments on lying behavior (e.g., Gneezy et al., 2018; Abeler et al., 2019).

Second, it strongly relates to experiments on cheating, which include control institutions. To our knowledge, only two studies look at how control affects lying. In a deception-game setting (as in Gneezy, 2005), Laske et al. (2018) explore how the size of fines and the probability of being caught affect decisions to deceive by sending a wrong message to the receiver. In the repeated version of the experiment, they find that participants are sensitive to both the size of the fine and the probability of being monitored (5% vs. 50% chance). The higher and more likely the fine, the less participants cheated. Thus, unlike in our experiment, both weak and strong institutions worked to reduce lying.

The other experiment that looks at the effect of control on lying was conducted by Galeotti et al. (2021). The researchers were specifically interested in seeing the spillover effects of control. In the first stage of the quasi-experiment, some people had their ticket checked on public transportation and some did not. In the second stage, an actor followed the participants, then acted as if they were picking up a 5 euro banknote and asked the person whether it was theirs. The authors found that observing ticket checks on public transport led to more cheating in the form of individuals claiming the banknote was theirs. Interestingly, observing ticket checks led to more lying about the banknote for both groups—for those who cheated in the public transport and those who did not. Following Sliwka (2007), the authors argue the ticket checks signal a social norm of dishonesty in society and hence crowd out intrinsic motivation among some participants.

The other stream of research that relates to our paper is the tax-evasion literature. Most of the experiments show that increasing detection probabilities leads to more truthful reporting (see, e.g., Webley, 1997; Beck et al., 1991; Alm et al., 1992). Similar results have also been found when analyzing the effect of the probability of being caught stealing, with stealing decreasing with the probability of being caught (e.g., Harbaugh et al., 2013).

Finally, our paper relates to the intrinsic-motivation crowding-out literature in economics and psychology (e.g., Frey and Oberholzer-Gee, 1997; Deci et al., 1999; Gneezy and Rustichini, 2000; Frey and Jegen, 2001; Fehr and Rockenbach, 2003; Benabou and Tirole, 2003, 2006; Sliwka, 2007; Dickinson and Villeval, 2008; Mellström and Johannesson, 2008; Gneezy et al., 2011).

The remainder of the paper is organized as follows. In section 2, we introduce the experimental design and procedure. In section 3, we state the main hypotheses for our experiment. In section 4.1, we provide the overall effects of control institutions on truthfulness,

and in sections 4.2 and 4.3, we discuss lying behavior on extensive and intensive margins, respectively. Section 4.4 looks at how observing control affects lying, and section 4.5 discusses the weak-institution treatment. Section 5 concludes the paper.

2 Experimental design

2.1 Experimental procedure

In the experiment, subjects play a repeated-cheating game (see Appendix B for instructions). The setup for the experiment is an individual decision-making situation, with no interactions between subjects. We use the observed-cheating game (as in Gneezy et al., 2018). In each round, on their screens, participants see 10 boxes with hidden outcomes behind them. The outcomes behind the boxes are numbers between 1 and 10, placed in a random order, where each box has a different number. After clicking on one of the boxes and seeing the number, participants are asked to truthfully report the number to the experimenter. The higher the number reported, the higher the payoff, which creates an incentive to cheat by over-reporting. In the absence of a control, the participant's payoff for the round is the number she reports in euros. If a control is present, the payoff is equal to the number reported if the report is truthful, and 0 otherwise.

Each experimental session had 15–40 subjects, and each subject played the cheating game for 20 rounds. At the end of the experiment, one round was picked at random to be payoff relevant. The experiment was run online using oTree software (Chen et al., 2016). The participants logged in to the experiment online and were observed through Zoom until the cheating game started. The instructions were read aloud at the beginning of the session, and subjects could ask questions in private in a chat.

Before the start of the game, subjects had to pass a quiz about the game's rules. If the subjects gave a wrong answer to at least on question, they had to take the quiz again. A maximum of 10 attempts was allowed, and failing subjects (around 2%) were prohibited from participating in the experiment.[9]

Before the start of the first round, subjects had to turn off their cameras, so the subjects could not interact or observe the reactions of other participants. We treated each participant as one independent observation.

We ran experiments with members of the general population, whom the laboratory of the University of Valencia (LINEEX) recruited for the study using online advertisements. In total, we had 756 participants:143 in NoControl, 145 in Control30, 168 in 50-50NoControl, 159 in 50-50Control30, and 141 in Control0.03 (see section 4.4 for this condition) treatments.

The average duration of a session was about one hour, and the average payoff was 11.46 euros, including a show-up fee of 5 euros.

2.2 Treatments

To answer our research questions, we ran the following three treatments in a between-subjects design:

1. **No-institution treatment (NoControl).** This treatment served as a baseline and entailed no control institution. Subjects received a payoff equal to the number reported in euros. Subjects were told they were expected to report truthfully and would not be controlled.
2. **Strong-institution treatment (Control30).** In this treatment, subjects knew that in each round, they might be controlled with a 30% probability. In the case of control, the subjects received no payoff if they misreported the number, or they received the number reported

[9] We consider a failing rate of 2% to be high, likely because we use a general population and not a laboratory subject pool.

in euros if they were truthful. After each round, subjects learned whether they had been controlled in that round, independently of their truthfulness.

3. **50% chance of being in either No-institution or in Strong-institution treatment. (50-50).** Subjects received instructions for both NoControl and Control30 treatments and were told that with a 50% chance, they were in one of the treatments. After each round, subjects learned whether they had been controlled in that round, independently of their truthfulness.[10] We distinguish between two sub-treatments:
    a. **50-50Control30**. This sub-treatment consists of participants who were in the Control30 treatment. They had learned deterministically that they were in the Control30 treatment after the first round in which control was implemented.
    b. **Malfunctioning-institution treatment (50-50NoControl)**. This sub-treatment consists of participants who were in the NoControl treatment. These participants had never deterministically learned they were in the NoControl treatment. However, after 10 rounds of absent control, the probability that the treatment was NoControl was 97.25%, and after 19 rounds of absent control, the probability that the treatment was NoControl was 99.89%.

3 Hypotheses

We start with a simple theoretical framework to explain what theory would predict. We base our framework on Gneezy et al. (2018). Our theory differs from theirs in that we do not differentiate between the intrinsic and social-identity costs of lying, and instead consider the fixed psychological cost of lying. Furthermore, we introduce to the theoretical framework a

[10] This design feature is important. In all treatments, one finds out whether a control was in place, *independently* of their truthfulness. Thus, "experimenting" through lies in order to see whether a control was in place is not a rational strategy.

crowding out of intrinsic motivation for truth-telling, as a coefficient that reduces the psychological cost of lying, if a possibility of a control institution is announced.

An agent's type consists of a tuple (i, t, θ), where $i = \{1,2 \dots ,10\}, t \in [0, T]$, and $\theta \in [0,1]$. The value i represents the agent's actual observation of the random draw; t is the fixed psychological cost of lying; and θ is the parameter of crowding out due to the announcement of the control institution, with $\theta = 1$ in NoControl. The agent can report any number $j = \{1,2, \dots ,10\}$. Given the type (i, t, θ) and report j, the utility of the agent (assuming a linear utility of money) is

$$U = \begin{cases} j & if\ i = j \\ j - \theta t & if\ i \neq j, no\ control. \\ -\theta t & if\ i \neq j, control \end{cases}$$

Because we assume a fixed cost of lying and the cost therefore does not depend on the size of the lie, agents who lie, lie to the maximal extent and report a 10. We derive the optimal reporting in Proposition 1.

Proposition 1:

Under NoControl, $\theta = 1$. *Then, the optimal report is the following:* $j = \begin{cases} i & if\ i > 10 - t \\ 10 & otherwise \end{cases}$.

Under Control30, the optimal report is the following: $j = \begin{cases} i & if\ i > 7 - \theta t \\ 10 & otherwise \end{cases}$.

Under 50-50, the optimal report in round r *is the following:*

$$j = \begin{cases} i & if\ i > (10 - \theta t)p_r + (7 - \theta t)(1 - p_r) \\ 10 & otherwise \end{cases},$$

where p_r *is the probability of being in the 50-50NoControl sub-treatment in round* r.

Our central assumption is that crowding out happens based on the institution's announcement, even if the institution ends up being malfunctioning. That is, for those whose good behavior was crowded out, based on the assumption that the law might be enforced,

learning that the law is most likely unenforced does not crowd in their motivation to behave well.

We formulate our theoretical predictions of treatment differences assuming that the presence of a control institution crowds out intrinsic motivation for honesty, namely, $\theta < 1$.[11] We look at the truthfulness rates in the last 10 rounds. In these rounds, 50-50Control30 and Contro30 are virtually the same because the chance of not facing control in the first 10 rounds is only 2.82%, and thus, almost all agents in the 50-50Control30 treatment learn deterministically that they are in 50-50Control30. For 50-50NoControl, after 10 rounds of absent control, the probability of being in NoControl is $p_r > 97\%$ for a Bayesian updater. Therefore, 50-50NoControl and NoControl are virtually the same treatment, with respect to the control probability.

Proposition 2: In the last 10 rounds,
for any $\theta < \frac{t-0.09}{t}$*, truthful rates under* $50 - 50NoControl \leq NoControl.$[12]
for any θ *, truthful rates under* $50 - 50NoControl \leq Control30.$

Proposition 2 states the main predictions of the paper.

The comparison of NoControl and Control30 treatments depends on the level of crowding out, and whether the controlling effect outweighs the effect of crowding out. For $\theta >$

[11] In absence of crowding out the comparison of truthful rates in the last 10 rounds is predicted to be
$$Control30 \approx 50 - 50Control30 > 50 - 50NoControl \approx NoControl.$$

[12] In the eleventh round, more truthful reporting will occur in NoControl than in 50-50NoControl, if $10 - t < (10 - \theta t)0.97 + (7 - \theta t)0.03$, which leads to $\theta < \frac{t-0.09}{t}$. That is, truthful reporting will be lower in 50-50NoControl as long as there is *even very mild* crowding out.

$\frac{t-3}{t}$ (low crowding out),[13] Control30 leads to higher level of truthful reporting than NoControl, while for $\theta < \frac{t-3}{t}$ (high crowding out), the relation is reversed.

Before running the study, we based our hypotheses on assumption of relatively low crowding out effects, such that $\theta > \frac{t-3}{t}$. This resulted in the following pre-registered hypothesis concerning the order of the proportions of honest reports in the last 10 rounds between the (sub-)treatments: [14]

Control30=50-50Control30>NoControl>50-50NoControl.

As mentioned above, our hypothesis that the malfunctioning control institution—50-50NoControl—leads to the most substantial adverse effect on truthfulness rates is based on the assumption that crowding out of motivation happens at the announcement of the institution stage. Intrinsic motivation is not crowded in even when agents learn that the institution is malfunctioning.

4 Results

We test the hypothesis about the detrimental effect of the malfunctioning institution in an online experiment. In what follows, we first present the overall truthfulness rates over the (sub-

[13] In Control30, participants will report more truthfully than in NoControl if the crowding out is low enough. More truthful reporting will occur in Control30 than in NoControl, if $7 - \theta t < 10 - t$, which leads to $\theta > \frac{t-3}{t}$.

[14] We pre-registered the design and hypotheses at aspredicted.org: https://aspredicted.org/gy47f.pdf. Note we have changed the design of the weak-institution treatment (see section IV.VI) compared to the pre-registration. The previous design of decreasing the control probability in each round (see the pre-registration for more details) had a clear demand effect. The treatment was poorly designed, and we changed the design of this treatment immediately after the first session. Furthermore, we pre-registered too few observations for the Control30 treatment because we were expecting to pool the data from Control30 and 50-50Control30 treatments. We could not pool the data, due to treatment differences; thus, we collected additional observations for Control30 to achieve a similar number of observations in each (sub-)treatment. Finally, we pre-registered that we would run the treatments with a dictator game too, but we did not conduct the dictator-game experiments, because we felt adding an additional game is beyond the scope of one paper (however, we are open to running it for our next study). The Ethics Committee of LABEX, University of Lausanne, approved the experimental design.

)treatments. We then report the results regarding the extensive margin of lies, after which, we present the results regarding the intensive margin of lies. Then, we move to a further analysis of how observing control affects lying. Finally, we present the results from an additional treatment with a weak institution.

Throughout the results section, we call a result significant if it is significant at least at the 5% level. We use ">" to communicate "significantly more" at the 5% level, whereas we use "=" to communicate no significance at the 5% level. For all the results, we use the significance of the coefficient of interest in regression analyses with standard errors clustered at the individual level. Importantly, in all main analyses, as pre-registered, we analyze the last 10 rounds of the experiment.

4.1 Truthfulness rates

First, we test our main (pre-registered) hypothesis and compare the rates of truthful reporting in the last 10 rounds between treatments.

Result 1: *The comparison of truthful reporting between treatments in the last 10 rounds shows that overall, in each (sub-)treatment, truthfulness rates are significantly higher than in the malfunctioning-institution treatment.*[15] *We find no significant differences between truthfulness rates in the remaining (sub-)treatments:*

Control30=50-50Control30=NoControl>50-50NoControl.

Support: Figure 1 presents the proportion of truthful reports by rounds and treatments.[16] As can be seen in the figure, in the first rounds, all treatments lead to a similar rate of truthful

[15] The tests are based on regressions with controls for age, gender, draw, and rounds with the sample restricted to two treatments of interest for the comparison.
[16] See Figure A1 in the appendix for the truthful reporting by the actual observed draw.

reports.[17] However, over time, the rate of truthful reporting becomes significantly lower in 50-50NoControl than in other treatments. In the sub-sample of the first seven rounds, the truthfulness rate in 50-50NoControl is significantly lower than in NoControl, Control30, or 50-50Control30.[18]

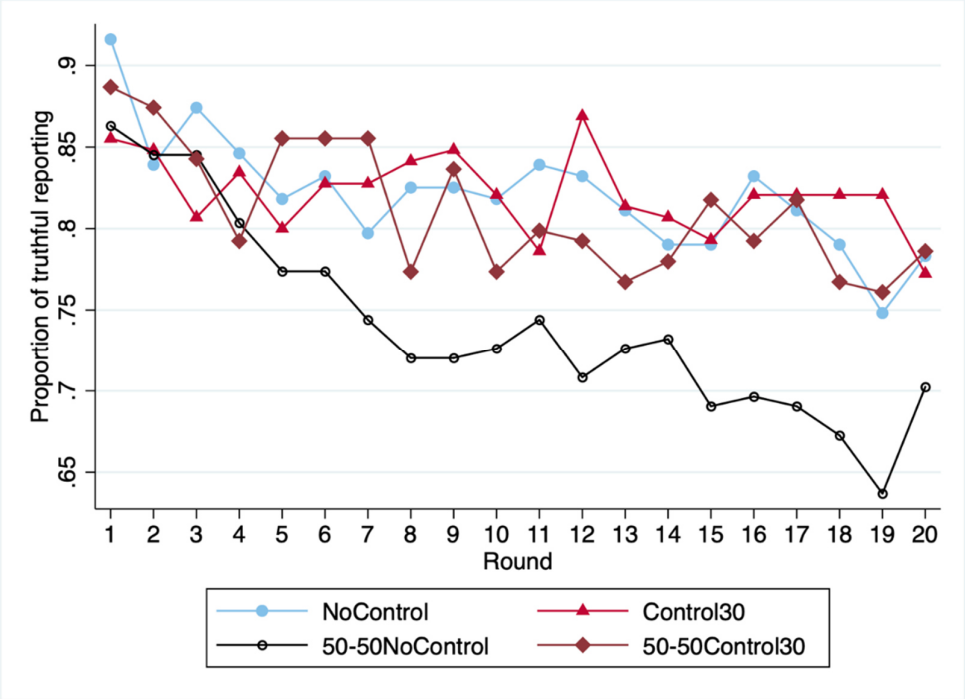


Figure 1. Proportion of truthful reporting by treatments and rounds

Table 1 presents the marginal effects of probit regressions of truthful reporting in the last 10 rounds. Model (1) contains only treatment dummies as explanatory variables, and Model (2) contains additional controls. Both models support the intuition from Figure 1 that in the later rounds, the 50-50NoControl treatment leads to significantly lower rates of truthful

[17]Furthermore, note the average truthful reporting in our experiment is higher than in the comparable experimental literature using the observed cheating game, such as Gneezy et al. (2018). For instance, in the NoControl condition, over all rounds, the average truthful reporting amounts to 82.1%. Such high truthfulness rates are likely driven by the subject pool, which consists of a general Spanish population (see Abeler et al., 2014, who show the general population lies less than a student subject pool). Furthermore, the majority of our subject pool—68%—are women, who tend to lie less than men (see Table 1). Finally, in the NoControl treatment, we explicitly state that participants will not be controlled, which signals trust, and therefore might lead to more truthfulness. In Gneezy et al. (2018), no such statement is made.

[18] These results are based on pairwise regression comparisons, including controls.

reporting than in the NoControl condition. In the last 10 rounds, the truthful reporting is around 11 percentage points lower in the 50-50NoControl than in the NoControl condition (see Model (2)). This detrimental effect of the malfunctioning institution is highly significant at the 1% level. Furthermore, we conduct pairwise comparisons using regression analyses and find that in the last 10 rounds, truthfulness in 50-50NoControl is significantly lower than in any other treatment.

	Truthful (1)	Truthful (2)
Control30	0.010	0.016
	(0.037)	(0.035)
50-50NoControl	-0.097***	-0.111***
	(0.036)	(0.035)
50-50Control30	-0.015	-0.008
	(0.035)	(0.033)
Female		0.073***
		(0.023)
Age		0.010***
		(0.002)
Draw		0.052***
		(0.002)
Round		-0.005***
		(0.001)
Observations	6150	6150
Number of clusters	615	615
Sample	Last 10 rounds	Last 10 rounds
Pseudo *R*-squared	0.039	0.173

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level.

Table 1. Marginal effects of probit regressions of truthful reporting in the last 10 rounds

Overall, our central hypothesis that the malfunctioning institution erodes honest behavior is strongly supported by the data. However, some of our additional predictions find no support in the data: we do not observe a positive overall effect of Control30 and 50-50Control30 on the lying rates. The results on the overall truthful reporting might be masking some underlying shifts in behavior on the extensive (lying at least once) and intensive margins (how often one lies conditional on lying at least once and what one reports conditional on misreporting). We

investigate the extensive and intensive margins separately in the next sections to detect the monitoring and crowding-out effects of our treatments.

4.2 Extensive margin: Lying at least once

Result 2: *Participants are more likely to lie at least once when they are in either the strong institution or the 50-50 condition than when no institution is in place. The comparison of proportions of subjects who lied at least once for the course of the experiment leads to the following result:*

Control30=50-50Control30=50-50NoControl>NoControl.

Support: Figure 2 presents the proportion of participants who lied at least once over all rounds by treatments and rounds.[19] As can be seen in the Figure, NoControl has the lowest proportion of those who lied at least once. Table 2 presents the results of probit regressions for the dummy of lying at least once over all rounds. The regressions show that on the extensive margin, Control30, 50-50Control30, and 50-50NoControl significantly crowd out honesty relative to the NoControl. We also conduct pairwise regressions, and they show no significant differences between Control30, 50-50Control30, and 50-50NoControl.

That is, the overall null effect of Control30 and 50-50Control30 on the truthfulness rates masks the crowding-out effect of Control30 and 50-50Control30 that is highly significant on the extensive margin.

[19] Unlike in other analyses, here we look not only at the last 10 rounds, but at all the rounds. The reason is that if somebody has not lied in the last 10 rounds but did in the first 10 rounds, categorizing him as a non-liar would be odd. All these results hold if we consider only the last 10 rounds of the experiment.

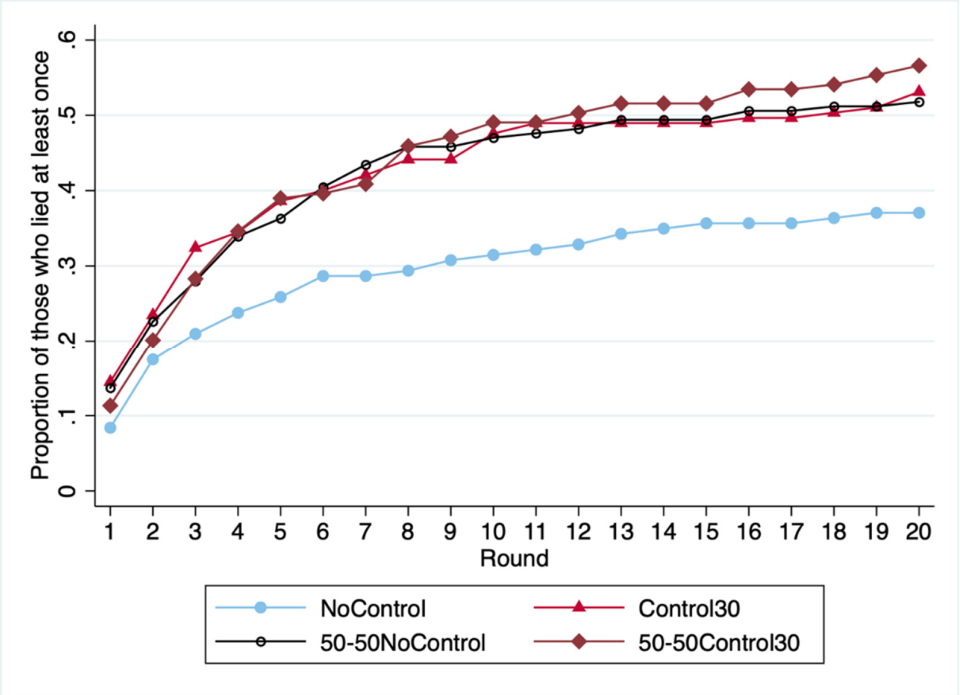


Figure 2. Proportion of participants who lied at least once from the start of the experiment

	Lied at least once (1)	Lied at least once (2)
Control30	0.160***	0.154***
	(0.057)	(0.055)
50-50NoControl	0.147***	0.169***
	(0.056)	(0.054)
50-50Control30	0.195***	0.197***
	(0.056)	(0.053)
Female		-0.176***
		(0.039)
Age		-0.019***
		(0.003)
Average draw		-0.044
		(0.028)
Observations	615	615
Sample	All rounds	All rounds
Pseudo *R*-squared	0.016	0.080

Notes: Standard errors in parentheses $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the individual level.

Table 2. Marginal effects of probit regressions of a dummy of lying at least once for the duration of the experiment

Finally, note the crowding out on the extensive margin starts early in the experiment. Using regression analyses, we find that in the sub-sample of the first four rounds, in Control30,

50-50Control30 and 50-50NoControl, lying at least once is significantly more observed than in NoControl.

Next, we analyze the effects of control institutions on the intensive margin of lying behavior.

4.3 Intensive margin: Lying pattern of those who lied at least once, and average reporting, conditional on lying

Result 3: *In the last 10 rounds, participants lie less often in Control30 and 50-50Control30 than in other conditions, conditional on lying at least once during the whole experiment. The comparison of the proportions of truthful reporting in the last 10 rounds for subjects who lied at least once in the experiment leads to the following result:*

Control30=50-50Control30>NoControl=50-50NoControl.

Support: Figure 3 presents the proportions of truthful reporting by round and treatment for those who lied at least once in the experiment.[20] The figure shows that participants in Control30 and 50-50Control30 lie less often than participants in other treatments, conditional on lying at least once. Probit models in Table 3 show that in the last 10 rounds, participants in Control30 and 50-50Control30 are highly significantly less likely to misreport, conditional on lying at least once in the experiment, than the participants in NoControl. We also conduct pairwise regressions and find Control30 and 50-50Control30 do not differ statistically from each other, conditional on lying at least once.

That is, the monitoring of a strong institution *does* work to prevent frequent lies. As a result, the negative effect of crowding out due to the presence of control (extensive margin) is

[20] See Figure A2 in the appendix for the truthful reporting, conditional on lying at least once, by the actual observed draw.

compensated by less frequent lies, conditional on lying at least once (intensive margin), which at the end leads to the aggregate absence of a difference in overall truthfulness between NoControl, Control30, and 50-50Control30.

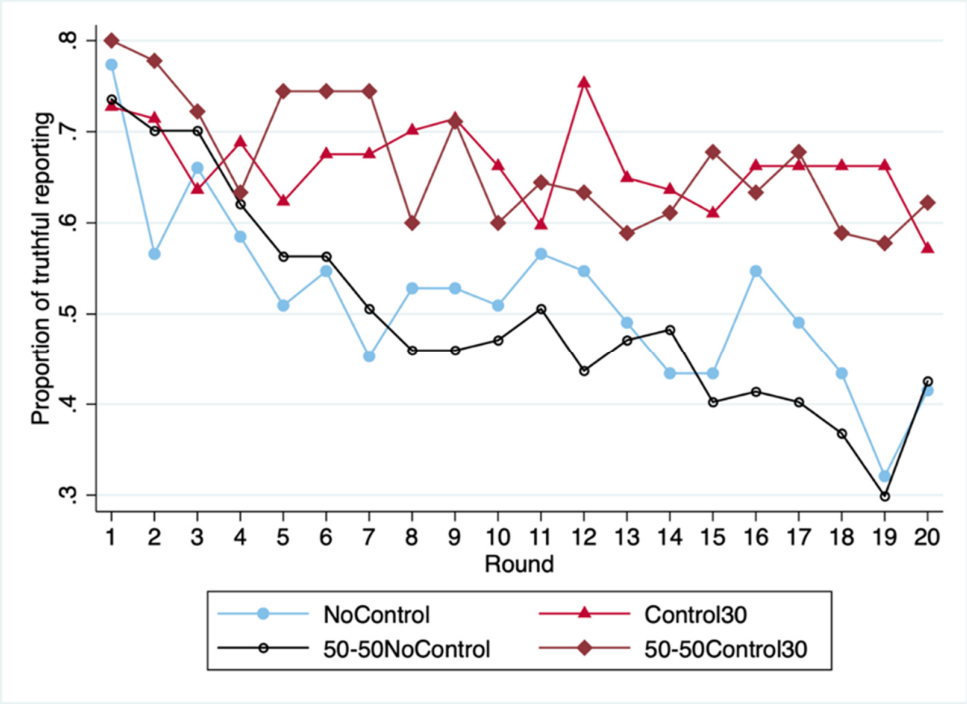


Figure 3. Proportion of truthful reporting by treatments and rounds for those who lied at least once during the experiment

In the next step, we turn to the dynamics of the breaking-bad effect on the intensive margin. Model (1) of Table 4 shows that in the first five rounds of the experiment, the rate of truthful reporting among those who lied at least once is significantly lower in NoControl than in all other treatments. Model (2) of Table 4 shows the pattern persists in Control30 and 50-50Control30 for the last five rounds of the experiment but not in 50-50NoControl. Thus, when learning that one is almost certainly in the 50-50NoControl sub-treatment, participants who lied at least once start to lie more often, which suggests they are aware no control will be in place.

	Truthful (1)	Truthful (2)
Control30	0.175***	0.174***
	(0.049)	(0.048)
50-50NoControl	-0.046	-0.051
	(0.049)	(0.049)
50-50Control30	0.154***	0.156***
	(0.046)	(0.045)
Female		0.015
		(0.029)
Age		0.003
		(0.004)
Draw		0.088***
		(0.002)
Round		-0.008***
		(0.002)
Observations	3070	3070
Number of clusters	307	307
Sample	Lied at least once in the experiment; last 10 rounds	Lied at least once in the experiment; last 10 rounds
Pseudo *R*-squared	0.029	0.341

Notes: Standard errors in parentheses $^{*}p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level.

Table 3. Marginal effects of probit regressions of truthful reporting in the last 10 rounds for subjects who lied at least once during the experiment

	Truthful (1)	Truthful (2)
Control30	0.194***	0.173***
	(0.053)	(0.050)
50-50NoControl	0.141***	-0.073
	(0.052)	(0.051)
50-50Control30	0.221***	0.165***
	(0.050)	(0.046)
Female	0.011	0.008
	(0.034)	(0.031)
Age	0.000	0.001
	(0.004)	(0.004)
Draw	0.089***	0.088***
	(0.002)	(0.002)
Observations	784	1491
Number of clusters	216	307
Sample	Those who lied at least once in rounds 1 to 5; first 5 rounds	Those who lied at least once in rounds 1 to 20; last 5 rounds
Pseudo *R*-squared	0.400	0.349

Notes: Standard errors in parentheses, $^{*}p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level.

Table 4. Marginal effects of probit regressions of truthful reporting in the first five and last five rounds for subjects who lied at least once

Together, the 50-50NoControl treatment combines the worst of the two worlds—it makes one more likely to lie at least once than in NoControl (extensive margin) and it makes one lie as frequently, conditional on lying at least once, as in the NoControl (intensive margin).

In further analyses on the lying patterns on the intensive margin, we compare the average numbers reported, given a lie, over the treatments.

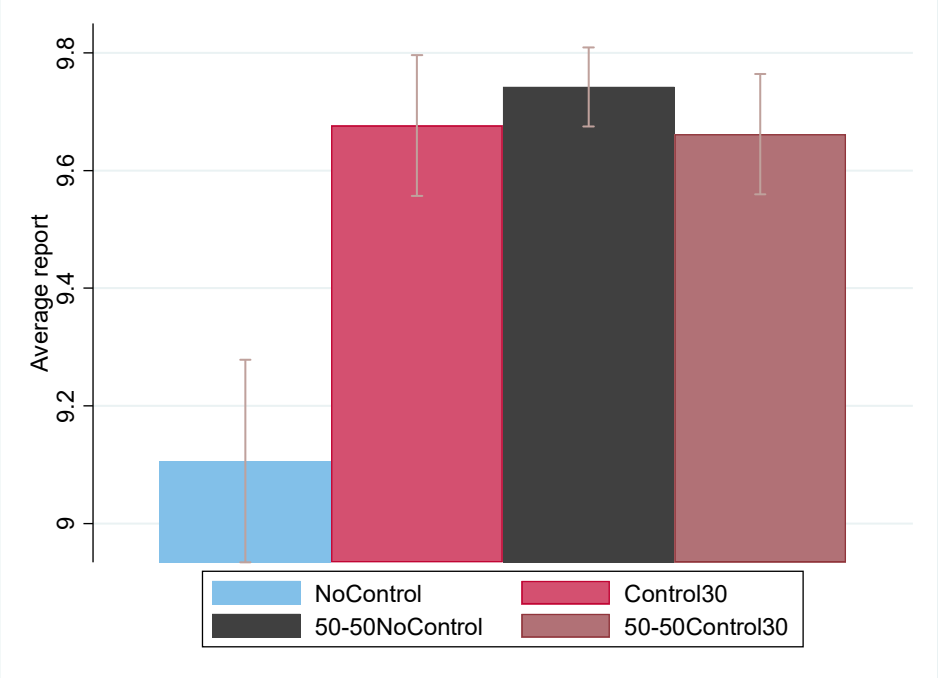
Result 4: *The average reporting, given a lie, is significantly higher in all (sub-)treatments than in the NoControl condition. The comparison of average reports in the last 10 rounds, given a lie, leads to the following result:*

Control30=50-50Control30=50-50NoControl>NoControl.

Support: Figure 4 presents the average reports by treatment in the last 10 rounds, conditional on lying. In NoControl, the average reported number given a lie is 9.11, which is significantly lower than in all other treatments. We find no significant difference in average reports between all other treatments. OLS regressions in Table 5 confirm that in all the treatments, the average number reported in the last 10 rounds, conditional on lying, is significantly higher than in NoControl.

A closer look at the distributions of numbers reported when lying reveals the lowest average reporting when lying in NoControl is driven by the lowest proportion of reporting a 10 for those who lie. In the last 10 rounds, only 62% of the participants who lie report a 10 in the NoControl condition, whereas the fraction amounts to 82% in Control30, 81% in 50-50NoControl, and 82% in 50-50Control30.[21]

[21] In the NoControl condition, lying to the full extent is similar to the one observed by Gneezy et al. (2018). In Gneezy et al. (2018), in the observed game (treatment "Numbers"), 68% of the participants who lie, lie to the full extent.



Notes: Gray bars represent 95% confidence intervals.

Figure 4. Average report, conditional on lying in the last 10 rounds

	Report (1)	Report (2)
Control30	0.570***	0.630***
	(0.187)	(0.184)
50-50NoControl	0.636***	0.648***
	(0.175)	(0.169)
50-50Control30	0.555***	0.631***
	(0.184)	(0.179)
Age		-0.002
		(0.010)
Female		-0.085
		(0.096)
Draw		0.075***
		(0.015)
Round		0.014
		(0.009)
Constant	9.106***	8.673***
	(0.163)	(0.311)
Observations	1395	1395
Number of clusters	289	289
Sample	Dishonest; last 10 rounds	Dishonest; last 10 rounds
R-squared	0.052	0.076

Notes: Standard errors in parentheses $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are clustered at the individual level.

Table 5. OLS of the reported number in the last 10 rounds, conditional on lying

4.4 Does observing control affect lying differently in Control30 and 50-50?

In Control30, observing control should not affect a classical agent, who knows the control probability is independent from the previous round. However, in the 50-50 treatment, observing control shows that one is in 50-50Control30, whereas not observing control yet makes one surer over time that one is in 50-50NoControl. Thus, not observing any control yet should lead to more lying in the 50-50treatment and should have no effect in Control30. We test in probit analyses whether participants use this reasoning.

	Truthful (1)	Truthful (2)	Truthful (3)	Truthful (4)
No control so far	-0.007	0.000	-0.048**	-0.053**
	(0.026)	(0.026)	(0.022)	(0.023)
Female	0.090**	0.091**	0.072***	0.067**
	(0.036)	(0.036)	(0.027)	(0.028)
Age	0.004	0.005	0.009***	0.010***
	(0.003)	(0.003)	(0.003)	(0.003)
Draw	0.049***	0.050***	0.054***	0.056***
	(0.005)	(0.005)	(0.003)	(0.003)
Observations	1450	2175	3270	4905
Number of clusters	145	145	327	327
Sample	First 10 Control30	First 15 Control30	First 10 50-50	First 15 50-50
Pseudo R-squared	0.20	0.20	0.21	0.20

Notes: Standard errors in parentheses, *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the individual level. Models (1) and (3) look at the first 10 rounds. Models (2) and (4) look at the first 15 rounds.

Table 6. Marginal effects of probit regressions of truthful reporting for 50-50 and Control30 treatments in the first 10 and 15 rounds

Table 6 presents marginal effects of Probit regressions of truthful reporting for the first 10 (Models (1) and (3)) and the first 15 (Models (2) and (4)) rounds, with the sample restricted to participants in Control30 and 50-50 treatments. The main goal of this analysis is to see the effect of the absence of control on truthfulness rates in Control30 and in 50-50 treatments. We find the variable "No control so far" is not significant for Control30, meaning the absence of control in Control30 does not influence the behavior of subjects. This behavior is rational, because the presence of control with a probability of 30% is deterministic in this treatment. The variable "No control so far" for the 50-50 treatment, on the other hand, is negative and significant, meaning subjects lie more if they do not observe control in the 50-50 treatment,

which is consistent with updating beliefs in favor of being in the environment without a control institution.

4.5 Weak institution

One might argue the detrimental effect of the 50-50NoControl treatment is driven by, in expectation, a low probability of control, and subjects perceiving it as a weak institution. We argue, however, that the detrimental effect of the malfunctioning institution goes beyond the effect of a small probability of control. We argue the *threat* of the *strong* control institution and no monitoring possibility are what drive the detrimental result.

In an additional treatment, we aim to distinguish the effect of weak institutions from malfunctioning ones. We introduce a treatment with a weak institution—Control0.03. In this treatment, subjects know that in each round, they might be controlled with a 0.03% probability. In the case of control, the subject receives no payoff if she misreported the number, or she receives the number reported in euros if she was truthful. After each round, subjects learn whether they were controlled in that round, independently of their truthfulness. Note the 0.03% probability corresponds to the Bayesian belief about the probability of control in the 50-50 treatment in round 20, after not being controlled in any previous round.

We expect Control 0.03 to lead to more cheating than NoControl, because of the crowding out of intrinsic motivation for truthtelling and no substantial possibility of being controlled.

From a rational perspective, the 0.03% probability of control does not affect the misreporting rate under risk neutrality, because misreporting under all draws is beneficial. Therefore, such a weak control institution should have no positive effect on truthfulness for payoff maximizers. At the same time, we expect the crowding-out effect from the presence of the control institution for the participants with a psychological cost of lying. However, we

expect this crowding out to be weaker with a weak institution than with the malfunctioning institution, because the threat of control is low under a weak institution. We hypothesize the following order of the proportions of honest reports in the last 10 rounds between the (sub-)treatments:

Control30=50-50Control30>NoControl>Control0.03>50-50NoControl.

In the experiment, however, we find no overall effect of weak institutions on truthfulness. Figure 5 shows that, in aggregate, lying in Control0.03 is similar to that in NoControl, 50-50Control30, and Control30, and shows significantly lower levels of lying than 50-50NoControl.

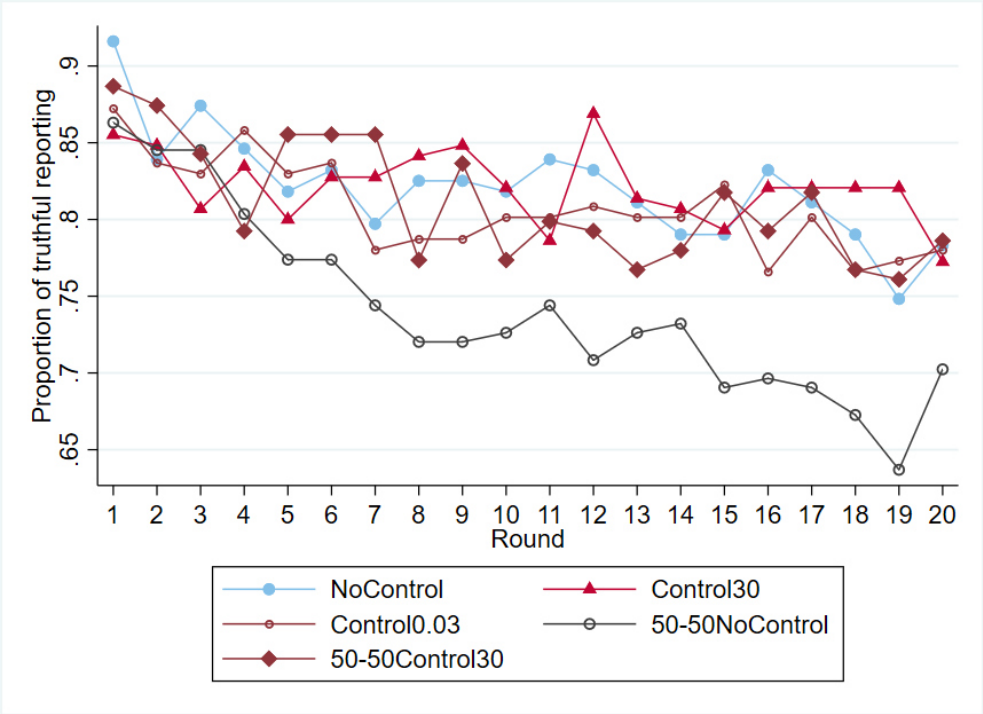


Figure 5. Average proportion of truthful reporting by treatments and rounds, for all treatments

Although we observe minimal evidence of crowding out on the intensive margin (in Control0.03, the average reported number given a lie is significantly higher than in NoControl), the aggregate effect is not significant either economically or statistically.

This result suggests θ might be endogenous to the probability of control. Although a threat of a strong institution crowds out intrinsic motivation for honesty, a certain weak instituion has no such effect. That is, having no institution or a weak institution is less detrimental than having a malfunctioning one.

5 Conclusion

The detrimental effect of malfunctioning institutions originates from the fact that the threat of a strong control institution crowds out intrinsic motivation for some individuals to tell the truth. Learning almost with certainty that no strong institution is in place does not crowd in intrinsic motivation, but rather leads to these individuals lying more on the intensive margin, because no monitoring exists, unlike in the strong institution.

The main contribution of this paper is the discovery of a new behavioral regularity. To our knowledge, none of the previous theories or experimental investigations on a crowding-out effect have analyzed uncertain incentives, which are common in real life. We show that an uncertain threat of punishment leads to severe detrimental effects and to the highest costs for an institution designer.

We are also the first to show the crowding-out effect exists in cheating games—an effect that has not been studied, even given the vast lying literature in behavioral economics.

We believe the insights of out paper generalize to other contexts, like altruistic behavior, public good contributions, and other contexts with where intrinsic motivation matters for behavior. A clear policy implication from our study is that having no institutions in place is better than threats of strong institutions that will not be implemented. For instance, it might be more efficient to let people quarantine away from their families on a trust basis than threatening them with large fines. It could also be more efficient to explicitly state to low

income tax payers (who are in any case not controlled extensively), that their reports will not be audited. Such trust institutions might work better than threatened with but not implemented ones. Beyond economics, we believe our paper sheds light on the potential effects of enforced laws. We provide empirical evidence that law might crowd out intrinsic motivation to behave as the law prescribes before it is evident that it is not enforced.

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Appendix A: Additional analyses

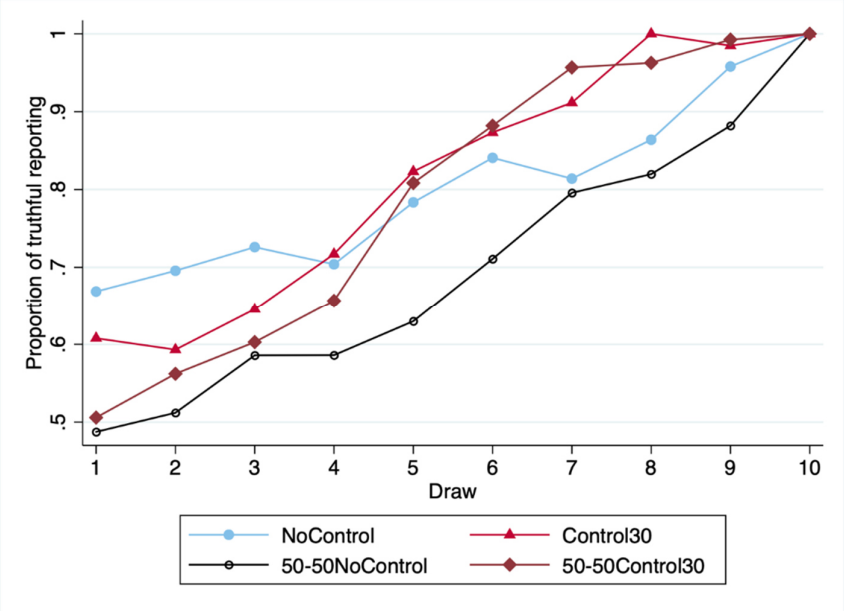


Figure A1. Average proportion of truthful reporting by draws and treatments in the last 10 rounds

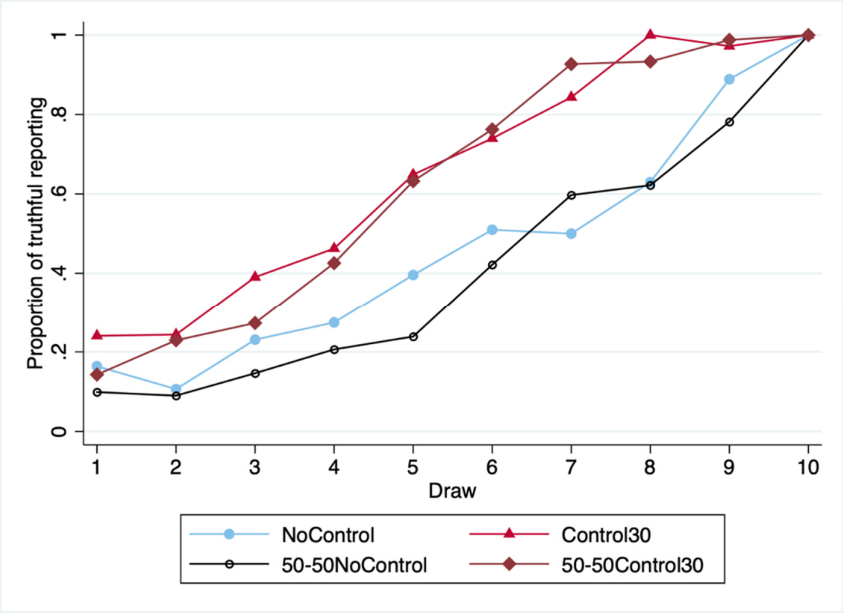


Figure A2. Proportion of truthful reporting by draws in the last 10 rounds for those who lied at least once during the experiment

Appendix B: Instructions

(The original instructions were in Spanish)

Instructions: NoControl

/Screen 1/

Welcome to our experiment. Please read the instructions carefully.

Each participant will receive five euros for attending, which will be paid out independently of the decisions made in the experiment.

Furthermore, you will be able to earn additional money, as we explain below. At the end of the experiment, you will receive the money you earned plus the five euros for attending.

Your decisions are private and no other participant will know about them.

/Screen 2/

On the screen you will see 10 boxes with numbers hidden behind them. The numbers in the boxes are 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10 and they are placed in a random order. We will ask you to click on one box.

Once you click on the box, you will see a number that we ask you to remember and later report to us. We ask you to report truthfully.

The number you report determines how much money you earn. You will be paid the equivalent in Euros to the number you report. In other words, if you write "1", you receive 1€, If you write "2", you receive 2€, if you write "3", you receive 3€ and so on.

There is no control of correctness of the reported number.

You will participate in this task for 20 rounds. Each time a new random configuration of boxes will be placed on the computer screen.

In each of the 20 rounds, the probability for implementing control is 0%.

At the end of the experiment, we will randomly choose one out of 20 rounds and will pay you the money that you have earned in that one particular round additional to the five Euros of participation fee. Each round can be picked with the same chance. Therefore, make your decisions such as if each round is payoff-relevant, because one of the rounds will be payoff-relevant.

Please click on "Next" to proceed.

/Screen 3/

Here is a questionnaire to check whether you understood the rules of the game.

Question 1:
What is the maximum number that could be behind a box? ___

Question 2:
Imagine you typed in number 3.

a) How many euros would you earn for this round? ___
b) What would be the total payoff for the experiment? ___

Question 3:
If I click on the same box in different rounds, I will see the same number:

- This is true
- This is false

Question 4:
One out of 20 rounds will be randomly picked to determine the payoff.

- This is true
- This is false

Click to submit your answers

/Screen 4/

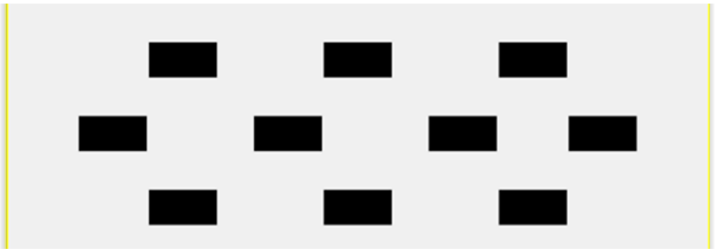
/If all answers are correct/ Your answers were correct.
/If not all answers are correct, then the previous screen appears again and it indicates which answers are not correct. It keeps appearing until all the answers are correct/

Click to proceed to the first round.

/Screen 5/

Round 1 out of 20

Please click on one of the boxes:



Please remember the number you saw.

/Screen 6/

Please report the number that you saw: ___ */answers between 1 and 10 are accepted, otherwise ask to correct/*

/Screen 7/

In this round, you reported: ...

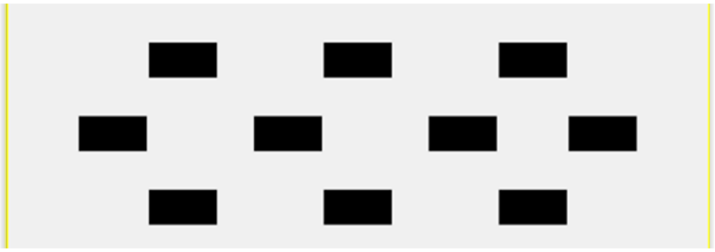
If this round will be selected you will receive the payoff of … Euros.

(…)

/Screen N-4/

Round 20 out of 20

Please click on one of the boxes:



Please remember the number you saw.

/Screen N-3/

Please report the number that you saw: ___

/Screen N-2/

In this round, you reported: ...

If this round will be selected you will receive the payoff of … Euros.

/Screen N-1/

Round … was randomly selected. In round …, you have earned …. Euros. You will receive this money plus the 5 euros for attending.

Please answer some demographic questions to finish the experiment.

/Screen N/
Please answer following questions.

Age: _____

Gender:
- ☐ Female
- ☐ Male
- ☐ Diverse

Please describe briefly how you made your decisions in this experiment:
_____
_____
_____

Instructions: Control30/ Control0.03

/Screen 1/

Welcome to our experiment. Please read the instructions carefully.

Each participant will receive five euros for attending, which will be paid out independently of the decisions made in the experiment.

Furthermore, you will be able to earn additional money, as we explain below. At the end of the experiment, you will receive the money you earned plus the five euros for attending.

Your decisions are private and no other participant will know about them.

/Screen 2/

On the screen you will see 10 boxes with numbers hidden behind them. The numbers in the boxes are 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10 and they are placed in a random order. We will ask you to click on one box.

Once you click on the box, you will see a number that we ask you to remember and later report to us. We ask you to report truthfully.

The number you report determines how much money you earn. You will be paid the equivalent in Euros to the number you report. In other words, if you write "1", you receive 1€, If you write "2", you receive 2€, if you write "3", you receive 3€ and so on.

In addition, with a chance of 30% */0.03% in Control0.03/*, control will be implemented. In the case of control, the number you reported will be compared with the number you observed. In the case of control: If the numbers match, you will receive the equivalent in Euros to the number you reported (as described in the previous paragraph); and if the numbers do not match, you will receive zero Euros.

You will participate in this task for 20 rounds. Each time a new random configuration of boxes will be placed on the computer screen.

In each of the 20 rounds, the probability for implementing control is 30% */0.03% in Control0.03/*.

At the end of the experiment, we will randomly choose one out of 20 rounds and will pay you the money that you have earned in that one particular round additional to the five Euros of participation fee. Each round can be picked with the same chance. Therefore, make your decisions such as if each round is payoff-relevant, because one of the rounds will be payoff-relevant.

Please click on "Next" to proceed.

Here is a questionnaire to check whether you understood the rules of the game.

Question 1:
What is the maximum number that could be behind a box? ___

Question 2:
Imagine you typed in number 3. There was no control in this round.

c) How many euros would you earn for this round? ___
d) What would be the total payoff for the experiment? ___

Question 3:
If I click on the same box in different rounds, I will see the same number:

- This is true
- This is false

Question 4:
One out of 20 rounds will be randomly picked to determine the payoff.

- This is true
- This is false

Click to submit your answers

/Screen 4/

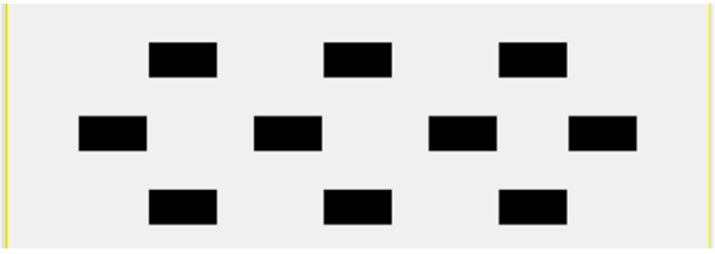
/If all answers are correct/ Your answers were correct.
/If not all answers are correct, then the previous screen appears again and it indicates which answers are not correct. It keeps appearing until all the answers are correct/

Click to proceed to the first round.

/Screen 5/

Round 1 out of 20

Please click on one of the boxes:



Please remember the number you saw.

/Screen 6/

Please report the number that you saw: __ */answers between 1 and 10 are accepted, otherwise ask to correct/*

/Screen 7/

In this round, you reported: ...

In this round, there was: control / no control.

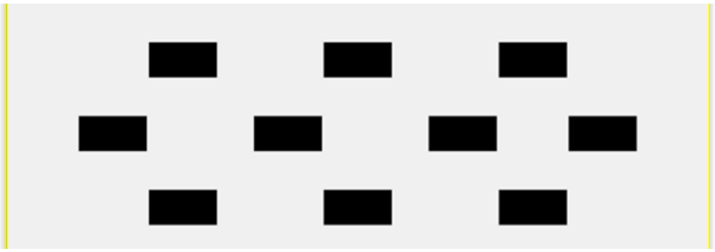
If this round will be selected you will receive the payoff of … Euros.

(…)

/Screen N-4/

Round 20 out of 20

Please click on one of the boxes:



Please remember the number you saw.

/Screen N-3/

Please report the number that you saw: ___

/Screen N-2/

In this round, you reported: ...

In this round, there was: control / no control.

If this round will be selected you will receive the payoff of … Euros.

/Screen N-1/

Round … was randomly selected. In round …, you have earned …. Euros. You will receive this money plus the 5 euros for attending.

Please answer some demographic questions to finish the experiment.

/Screen N/
Please answer following questions.

Age: _____

Gender:

- ☐ Female
- ☐ Male
- ☐ Diverse

Please describe briefly how you made your decisions in this experiment:

_____
_____
_____

Instructions: 50-50

/Screen 1/

Welcome to our experiment. Please read the instructions carefully.

Each participant will receive five euros for attending, which will be paid out independently of the decisions made in the experiment.

Furthermore, you will be able to earn additional money, as we explain below. At the end of the experiment, you will receive the money you earned plus the five euros for attending.

Your decisions are private and no other participant will know about them.

/Screen 2/

With 50% chance you are participating in Experiment 1, and with 50% chance you are participating in Experiment 2. We will not tell you explicitly in which – Experiment 1 or Experiment 2 – you have participated until the very end of the experiment.

On the next screens, you will find instructions for Experiment 1 and Experiment 2.

/Screen 3/

Experiment 1

On the screen you will see 10 boxes with numbers hidden behind them. The numbers in the boxes are 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10 and they are placed in a random order. We will ask you to click on one box.

Once you click on the box, you will see a number that we ask you to remember and later report to us. We ask you to report truthfully.

The number you report determines how much money you earn. You will be paid the equivalent in Euros to the number you report. In other words, if you write "1", you receive 1€, If you write "2", you receive 2€, if you write "3", you receive 3€ and so on.

There is no control of correctness of the reported number.

You will participate in this task for 20 rounds. Each time a new random configuration of boxes will be placed on the computer screen.

In each of the 20 rounds, the probability for implementing control is 0%.

At the end of the experiment, we will randomly choose one out of 20 rounds and will pay you the money that you have earned in that one particular round additional to the five Euros of participation fee. Each round can be picked with the same chance. Therefore, make your decisions such as if each round is payoff-relevant, because one of the rounds will be payoff-relevant.

/Screen 4/

Experiment 2

On the screen you will see 10 boxes with numbers hidden behind them. The numbers in the boxes are 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10 and they are placed in a random order. We will ask you to click on one box.

Once you click on the box, you will see a number that we ask you to remember and later report to us. We ask you to report truthfully.

The number you report determines how much money you earn. You will be paid the equivalent in Euros to the number you report. In other words, if you write "1", you receive 1€, If you write "2", you receive 2€, if you write "3", you receive 3€ and so on.

In addition, with a chance of 30%, control will be implemented. In the case of control, the number you reported will be compared with the number you observed. In the case of control: If the numbers match, you will receive the equivalent in Euros to the number you reported (as described in the previous paragraph); and if the numbers do not match, you will receive zero Euros.

You will participate in this task for 20 rounds. Each time a new random configuration of boxes will be placed on the computer screen.

In each of the 20 rounds, the probability for implementing control is 30%.

At the end of the experiment, we will randomly choose one out of 20 rounds and will pay you the money that you have earned in that one particular round additional to the five Euros of participation fee. Each round can be picked with the same chance. Therefore, make your decisions such as if each round is payoff-relevant, because one of the rounds will be payoff-relevant.

/Screen 5/

With 50% chance you are participating in Experiment 1, and with 50% chance you are participating in Experiment 2.

Please click on "Next" to proceed.

Here is a questionnaire to check whether you understood the rules of the game.

Question 1:
What is the maximum number that could be behind a box? ___

Question 2:
Imagine you typed in number 3. There was no control in this round.

e) How many euros would you earn for this round? ___
f) What would be the total payoff for the experiment? ___

Question 3:
If I click on the same box in different rounds, I will see the same number:

- This is true
- This is false

Question 4:
One out of 20 rounds will be randomly picked to determine the payoff.

- This is true
- This is false

Click to submit your answers

/Screen 4/

/If all answers are correct/ Your answers were correct.

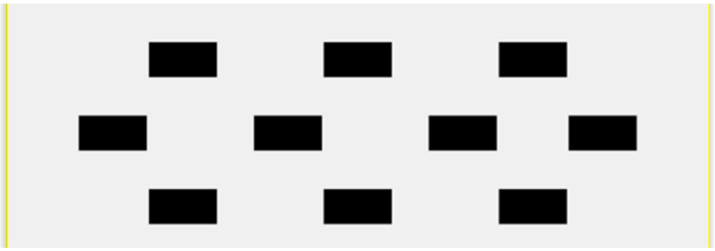
/If not all answers are correct, then the previous screen appears again and it indicates which answers are not correct. It keeps appearing until all the answers are correct/

Click to proceed to the first round.

/Screen 6/

Round 1 out of 20

Please click on one of the boxes:



Please remember the number you saw.

/Screen 7/

Please report the number that you saw: ___

/Screen 8/

In this round, you reported: ...

In this round, there was: control / no control.

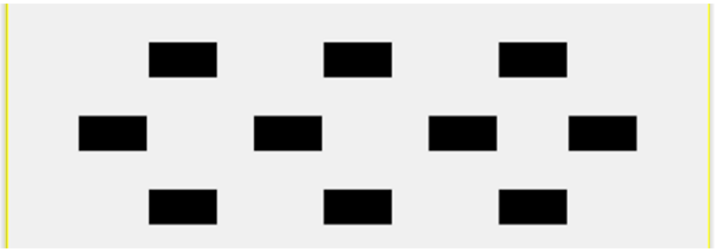
If this round will be selected you will receive the payoff of … Euros.

(…)

/Screen N-4/

Round 20 out of 20

Please click on one of the boxes:



Please remember the number you saw.

/Screen N-3/

Please report the number that you saw: ___

/Screen N-2/

In this round, you reported: ...

In this round, there was: control / no control.

If this round will be selected you will receive the payoff of … Euros.
/Screen N-1/

Round … was randomly selected. In round …, you have earned …. Euros. You will receive this money plus the X euros for attending.

Please answer some demographic questions to finish the experiment.

/Screen N/
Please answer following questions.

Age: _____

Gender:
- ☐ Female
- ☐ Male
- ☐ Diverse

Please describe briefly how you made your decisions in this experiment:
_____
_____
_____