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Market Design and Moral Behavior

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In an experiment with 739 subjects, we study whether and how different interventions might have an influence on the degree of moral behavior when subjects make decisions that can generate negative externalities on uninvolved parties. Particularly, subjects can either take money for themselves or donate it to UNICEF for measles vaccines. By considering two fairly different institutional regimes—one with individual decision making, one with a double-auction market—we expose the different interventions to a kind of robustness check. We find that the threat of monetary punishment promotes moral behavior in both regimes. Getting subjects more involved with the traded good has no effect, though, in both regimes. Only the removal of anonymity, thus making subjects identifiable, has different effects across regimes, which we explain by different perceptions of responsibility.

Keywords: morals; market design; experiment; behavioral economics; punishment
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1. Introduction

The question whether markets erode moral values is one of the most controversially discussed issues in philosophy and in the social sciences (Marx 1904, Weber 1978, Simmel 1990, Shleifer 2004, Sandel 2012). Throughout history, the perception has swung back and forth between seeing markets as (i) civilizing and (ii) destructive. Proponents of the first view see markets as a civilizing force because they increase integrity, honest behavior, cooperation, and trust among individuals. McCloskey (2006) argues that markets nurture "bourgeois virtues," and Friedman (1962) states that economic freedom is necessary for political freedom. The second view, that markets destruct moral values, strongly builds on the ideas of Marx (1904) about the alienation and exploitation of the workforce in the capitalist production process. Sandel (2012) argues that market norms might crowd out (prosocial) nonmarket norms, and therefore a society should think carefully about the scope and limits of markets to allocate resources.

Pioneering experimental work by Falk and Szech (2013) has taken a big step to increase our understanding of how markets may affect human behavior. They have investigated whether markets erode moral values by imposing negative externalities on uninvolved third parties. Falk and Szech (2013) have shown that experimental participants are more likely to accept the killing of a mouse in exchange for receiving a small monetary payment when participants interact on double-auction markets than when they make decisions individually, implying that people seem to act against their own moral standards when they engage in market interactions. Such evidence fuels the debate about the pros and cons of using markets for the allocation of goods. In particular, it raises the questions whether interventions to promote moral behavior in markets are possible and whether market design has an influence on these interventions (Roth 2002, Bolton and Ockenfels 2012, Bartling et al. 2015). This paper addresses exactly these two questions.

In an experiment with 739 participants we study, first, how four different interventions affect moral behavior; second, we study how different institutional regimes interact with the different interventions,[1] thereby addressing the issue of how robust the various interventions are. The first regime has its

[1] Bartling et al. (2015) have pointed out the relevance of market settings other than the double-auction market to consider moral behavior of subjects.

analogy in everyday consumption decisions, where individuals are price takers. Subjects decide about receiving a monetary payment (which mimics the utility gain from buying a good) at the cost of creating a negative externality to an uninvolved third party *outside the laboratory* in a choice list setting. If they refuse the monetary payment, they can avoid the negative externality. The second regime is a double-auction market. It is motivated by financial markets where individuals trade with each other and where trading can create negative externalities on uninvolved third parties.

In both regimes subjects can choose between taking money for themselves or donating a potentially larger sum of money to UNICEF for measles vaccines. Even today, the annual death toll from measles is considerable, with about 150,000 people estimated to die each year from measles infection, mostly children under the age of five.[2] Hence, donating money to UNICEF is a potentially lifesaving act for a third party. We have consciously chosen this setting because presumably immoral behavior in our experiment involves negative consequences for people *outside the laboratory*. Refusing to donate for measles vaccine has potentially real-world consequences and does not only imply fairness concerns among participants inside the lab.

The experimental interventions that we test in both regimes are motivated by properties of markets that are most prominently considered to be the reason for (im)moral behavior: (i) noninvolvement with the traded good (Sandel 2012), (ii) anonymity of trading (Ariely et al. 2009), and (iii) absence of punishment of a norm violation (Gintis et al. 2003, Fehr and Fischbacher 2004). Holding the institutional regime constant, we examine whether (i) information campaigns to get traders involved with the traded good, (ii) removing anonymity of traders, or (iii) the opportunity to punish immoral traders affect moral behavior on markets. In addition, we test one technical intervention, which only applies to the double-auction regime. We investigate whether reminding traders of their responsibility during trading may avoid diluting responsibility on markets and hence make behavior more moral. We conjecture that the latter is specific to financial markets because traders' moral perception of what they trade might be easily diluted because of the technical and monetary characteristics of trading.

We find that interventions can affect moral behavior (i.e., the amount of money donated to UNICEF), but not all of them do. In both regimes, the potential threat of monetary punishment promotes more moral behavior, whereas in both regimes behavior does not change when subjects get more involved with the traded good through supplying detailed information on measles. Both types of interventions are robust to the regime under which they are implemented. Removing anonymity of trading has different effects across regimes, though. In fact, it only promotes more moral behavior in the choice list-regime but not in the double-auction market. We try to explain this difference through different extents of diffusion of responsibility in both regimes. Finally, one treatment that is only implemented in the double-auction regime—reminding subjects of their responsibility before trading—does not affect moral behavior. In a post-experimental questionnaire, we show that donating money to UNICEF is, indeed, what subjects consider as the moral action that ought to be taken in our experiment.

2. The Experiment

2.1. Design of Baseline Treatments

In the first regime, subjects act in a choice list framework. This regime has its analogy in everyday consumption choices with possible negative externalities (e.g., child labor or inhumane working conditions). Individuals do not bargain with others but are price takers. They can either take a monetary payment (which mimics the utility gain from consumption) or make a donation to UNICEF for the measles vaccine. Taking the money creates a negative externality of no donation to UNICEF (which could have saved human lives), whereas refusing the money avoids such a negative externality. Because subjects make such decisions for a variety of different amounts of money to be taken instead of donating a fixed amount, we call this regime the choice list-regime, abbreviated as CL.

The second regime is implemented as a double-auction market, abbreviated as DA. Subjects bargain over splitting an amount of money between themselves or donating to UNICEF for measles vaccine. The analogy for this regime comes from financial markets such as soft commodity markets. Here production and trade of goods occur that can have negative externalities for third parties, e.g., child labor, environmental pollution, or food scarcity.

Participants in treatments of regime CL are facing a list of 22 choices between a payment to themselves ranging from 0.40 euro to 21.40 euros (increasing in steps of one euro) and the donation of 10.70 euros to UNICEF in each of 10 periods. The amount donated to UNICEF is worth half a package with 100 doses of measles vaccine. The procedure of determining which choice pair is selected for payment is as follows. First, subjects have to make decisions in the price list for 10 periods and then one period is chosen randomly, of

[2] WHO, http://www.who.int/mediacentre/factsheets/fs286/en/ (accessed November 2015).

which again one of the 22 choice pairs is selected randomly for payment. We let subjects repeat the price list 10 times to keep conditions similar to the treatments in Falk and Szech (2013). Furthermore, in the treatments with interventions outlined below, the repetition of the price-list procedure allows for learning from feedback after each period. Therefore, to compare these interventions treatments to the baseline treatment, we keep the conditions identical. The baseline treatment in regime CL, as described here, is called CL_BASE.

Participants in treatments of regime DA trade in a double-auction market where they can post limit orders and accept them by posting market orders. To mimic the multilateral markets of Falk and Szech (2013) as closely as possible, we implement a market surplus of two sellers by assigning six subjects as sellers and four subjects as buyers. Each seller holds one package with 100 doses of measles vaccine. For every seller that does not trade, these 100 units of measles vaccine are donated to UNICEF for 21.40 euros. If, however, a seller and a buyer agree on a price P, the seller receives P as payoff, and the buyer gets 21.40 euros minus P. In this case there is no donation. The market runs for 10 trading periods of three minutes each. Orders are executed according to price and then time priority. Market orders have priority over limit orders and are always executed instantaneously. The trading screen provides real-time information about the current price in a chart and about the number of transactions in the period (see the instructions in the online appendix, available as supplemental material at http://dx.doi.org/10.1287/mnsc.2015.2246). Once a subject traded in a market, her remaining open limit orders are removed from the order book, because each subject can conclude only one trade per period. At the end of the experiment one period is drawn randomly and is implemented with all monetary consequences. The baseline double-auction market treatment is labelled DA_BASE.

2.2. Design of Treatments with Interventions and Their Motivation

Treatments CL_PHYSICIAN and DA_PHYSICIAN are identical to the respective baseline treatments, but before trading starts a physician who works for Doctors Without Borders (Médecins Sans Frontières—MSF) gives a 10-minute presentation on measles and measles vaccination. This presentation provides subjects with detailed information on the effects of measles, such as death tolls and possible prevention, and how the vaccination works. The slides of the presentation and the script of the physician are intended to get subjects more strongly involved with the traded good (the vaccine)—see §§5.5 and 5.6 in the online appendix. This intervention has its analogy in the real world in the use of information campaigns that should increase subjects' involvement with an issue and should change behavior by educating people. We conjecture similar effects of this intervention in both regimes.

Treatments CL_IDENTIFIABILITY and DA_IDENTIFIABILITY are identical to the respective baseline treatments with the sole difference that anonymity is removed in the following way. After each period in CL_IDENTIFIABILITY, all subjects in a session are informed about whether the other subjects have taken money for themselves or donated to UNICEF in the randomly selected choice pair of the preceding period. Subjects can be identified by seat number but not by name. Subjects also get a history of all periods up to the current one. Similarly, after each period all subjects in DA_IDENTIFIABILITY are informed about whether and how often a particular subject has taken money (through a trade) or donated to UNICEF (by refusing to trade) in the preceding period and throughout all periods. Again, identification is possible by a subject's seat number only. Before the final questionnaire of the experiment is administered, subjects in both treatments are called to the front desk in public to receive payment in order of their seat number. Thereby, each participant can identify how often a subject took money for herself instead of donating it, and so strict anonymity is removed. This procedure was announced in detail at the beginning of the experiment. We conjecture a stronger effect in CL_IDENTIFIABILITY, because diffusion of responsibility is not possible here. Whereas subjects are solely responsible for their actions in CL_IDENTIFIABILITY, there is always a counterpart to trade in DA_IDENTIFIABILITY, which leads to a diffusion of responsibility in double-auction markets. Each trading partner can always excuse herself by arguing that without the second party's agreement she would not have traded and thus not have obstructed the donation. Such a line of reasoning is impossible in CL_IDENTIFIABILITY. Therefore, we expect that being solely identifiable for immoral actions by the public is more strongly avoided by the subjects in regime CL. Both treatments are related to and motivated by a field setting in which, e.g., nongovernmental organizations (NGOs) use lists of socially irresponsible companies to identify them for a broader public, i.e., for so-called "name and shame" campaigns.[3]

Treatments CL_PUNISHMENT and DA_PUNISHMENT are identical to the respective baseline treatments with the exception that two additional subjects

[3] We test the impact of this intervention in a single-shot environment, where we cannot control for reputation concerns. If we find effects in a one-shot setting, it is likely to find even stronger effects in a repeated setting.

act in each cohort/market as observers. Although observers are not materially affected by the actions chosen by the others, they can punish behavior if they wish. Each observer is endowed with 21.40 euros per period and is linked to five other participants in CL_PUNISHMENT or to three sellers and two buyers in DA_PUNISHMENT throughout the entire experiment. After each period an observer in CL_PUNISHMENT gets informed about the choice pair at which a particular subject switched from donating to taking the individual payment, which choice pair was chosen and would be paid if the preceding period were randomly selected for actual payment. Furthermore, each observer also gets the information how often an assigned subject took money for herself (or refused to do so) in all periods so far.[4] In treatment DA_PUNISHMENT observers can watch the market during trading, and so they observe trading activity such as posted limit orders and market prices. At the end of each period an observer receives information about which of his assigned subjects traded in the preceding period. Furthermore, an observer also receives information how often the assigned subjects traded—or did not trade—in all periods so far. This identification in both treatments is done by a subject-ID, which is not identical to a subject's seat number, though. Each observer can assign up to three euros of his endowment per period to each of the five assigned participants. For each euro spent on punishment by the observer, a particular subject loses three euros. Therefore, punishment is costly for the observer but even more so for the punished subject. We would like to note that punishment is designed to be as similar as possible across regimes.[5] To mimic the observation of trading prices in the double auction regime, we display the choice pair at which subjects switch to the individual payout in the choice list regime. Hence, we expect similar effects of punishment in both regimes. Analogies in the field to our treatments with a punishment opportunity would be costly NGO actions against norm violations or law enforcement by governments.

In addition to the above-described treatments, we run one more double-auction treatment, which is much less reasonable to test in the CL framework. Treatment DA_RESPONSIBILITY is identical to DA_BASE, with the only difference being that every time a limit or a market order is posted, a pop-up appears, reminding subjects that trading may have the effect that no donation will be made. The pop-up window for market orders reads as follows: "If you do trade, NO donation for the measles vaccine can be made on your behalf for this period. Do you want to continue?" The pop-up window for limit orders reads as follows: "If you post an offer, this can lead to a trade. In the latter case NO donation for the measles vaccine can be made on your behalf for this period. Do you want to continue?" This treatment is intended to test whether reminding traders of their responsibility may avoid a loss of focus on the moral dimension of trading and the dilution of responsibility on double-auction markets and hence make behavior more moral. This intervention is relevant for trading on financial markets because in this environment traders might lose attention about what they trade because of the technical and monetary characteristics of trading. By drawing their focus on the moral and normative dimension of the traded good, they might change their behavior accordingly (Krupka and Weber 2009).

2.3. Experimental Procedure

In the CL regime, 60 subjects participated in each treatment, except in CL_PUNISHMENT where 72 subjects were tested (60 decision makers and 12 observers). These sessions were run in May and October 2014, yielding one independent observation per participant. In the DA regime, we conducted eight markets (as independent observations) with 10 subjects each in each treatment, except for DA_PUNISHMENT with eight markets of 12 subjects (10 market traders and two observers) each. These sessions were run from May to July 2013.[6] In total, 739 bachelor and master students participated in the experiment.[7] Each subject participated in only one session of this study, and the show-up fee was 10 euros.[8] All sessions

[4] We also ran an alternative version of the punishment treatment, called CL_PUNISHMENT_MILD. Here, we tested the impact of a "mild" version of punishment, because the observers received little information on the subjects' actions. The observers only got information about whether a donation was made in the preceding period and how often money was donated up to the preceding period by each subject. Subjects' decisions for each single choice pair were not disclosed. Results are reported in the online appendix.

[5] We would like to thank an anonymous referee for suggesting treatment CL_PUNISHMENT as a more suitable comparison treatment to DA_PUNISHMENT than the original treatment CL_PUNISHMENT_MILD was.

[6] Initially, we had also run CL_BASE in early 2013. Yet, when extending the CL regime upon the useful suggestions of a referee, we decided to rerun CL_BASE again in May 2014 to keep the conditions (in particular the time of execution) within the CL regime identical. It is reassuring to note that the data for CL_BASE, which are presented in the following, are not significantly different from those in 2013, which we do not report here (Wilcoxon signed-ranks test, $p = 0.164$, $N = 108$).

[7] In one session of treatment DA_PHYSICIAN we tested 19 instead of 20 participants, since subjects (in all treatments) were free to leave, if they felt that they had a moral conflict. In this particular session of DA_PHYSICIAN more participants chose to leave than reserve participants attended.

[8] The show-up fee was chosen to be rather high to ensure that subjects would receive at least an amount of money in the experiment

were run at Innsbruck EconLab at the University of Innsbruck.

In total, each experimental session lasted between 60 and 90 minutes. At the beginning of each session, subjects had 15 minutes to read the instructions on their own and to have their questions answered privately. Then the trading or choice list screen was explained, followed by a nonincentivized trial period of three minutes to allow subjects to become familiar with the interface.

After subjects read the instructions they had the option to leave the experiment if they felt they had a moral conflict. Subjects that chose to leave the experiment received the show-up fee of 10 euros and were replaced with reserve candidates. The latter were assigned the roles of reserves and were present from the beginning of the experiment. In sum, 34 participants left the sessions, with all except one being replaced by reserve candidates. There are no significant differences across treatments in the number of subjects leaving.

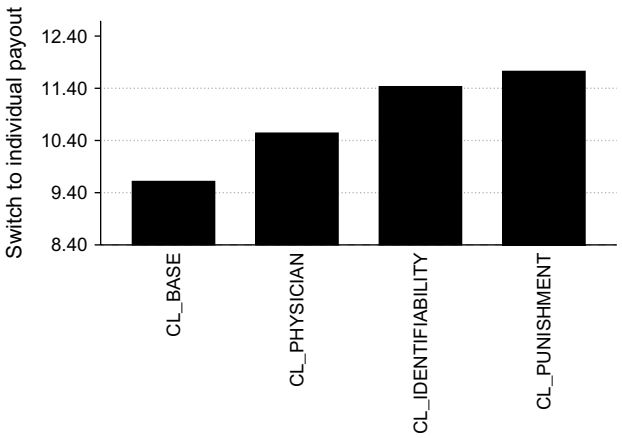
At the end of the 10 trading periods, subjects had to answer a questionnaire on their current emotional state. Participants also answered a questionnaire with several demographic questions and questions about their moral judgement of trading. Subjects received the payment in private and anonymously by another researcher who was not in the room during the experiment. The experiments were programmed and conducted with z-Tree 3.3.6 by Fischbacher (2007). Subjects were recruited using ORSEE (Online Recruitment System for Economic Experiments) by Greiner (2015) and hroot (Hamburg registration and organizational online tool) by Bock et al. (2012).

3. Results

3.1. Descriptive Overview

Figures 1 and 2 show descriptive results for the CL-treatments. In Figure 1 we show the average amount in euros at which subjects switch from donating to UNICEF (which they did for lower amounts) to taking the individual payment (for amounts at the average or higher). We observe that the average subject in CL_BASE already prefers 9.40 euros for herself over donating to UNICEF. The average amount for switching is larger in the other CL-treatments, reaching around 11.40 euros in CL_PUNISHMENT. Figure 2 shows the development of switching points

Figure 1 **Average Amount of Money at Which Subjects Decide to Take the Payment for Themselves Rather Than Donate Money**

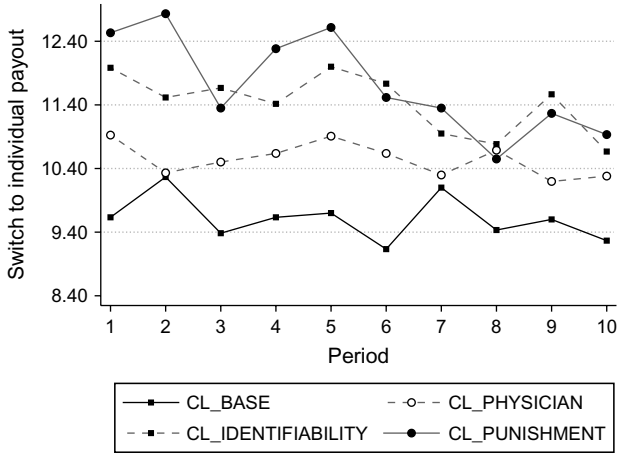


(translated into the respective amount of euros for the subject) over time for each treatment. We observe slightly negative time trends in most treatments, most pronounced in CL_PUNISHMENT.

Figures 3, 4, and 5 show descriptive results for the DA treatments. In Figure 3 we normalize the absolute number of trades by the maximum number of possible trades, i.e., by the number of buyers in the market. We observe that three intervention treatments—DA_RESPONSIBILITY, DA_PHYSICIAN, and DA_IDENTIFIABILITY—are practically at par with DA_BASE. In fact, those four treatments are all in a very narrow range from 82% to 84%. Only when the possibility of monetary sanctions through observers is introduced in DA_PUNISHMENT does the relative frequency of trade drop markedly to 69%.

Figure 4 shows the development of the relative trading frequency over time for each treatment, indicating no clear time trends. Figure 5 presents the mean trading prices over time across all markets in

Figure 2 **Average Amount of Money at Which Subjects Decide to Take the Payment for Themselves Over Time**



that is comparable to other experiments run in Innsbruck irrespective of their donation behavior. Furthermore, subjects were told that they would receive a receipt about the amount donated in the sessions two months after the experiment, which was actually done. The total amount of money donated to UNICEF was 3,445.40 euros, allowing for 16,100 measles vaccinations.

Figure 3 **Trading Frequency in Each Treatment of Regime DA**

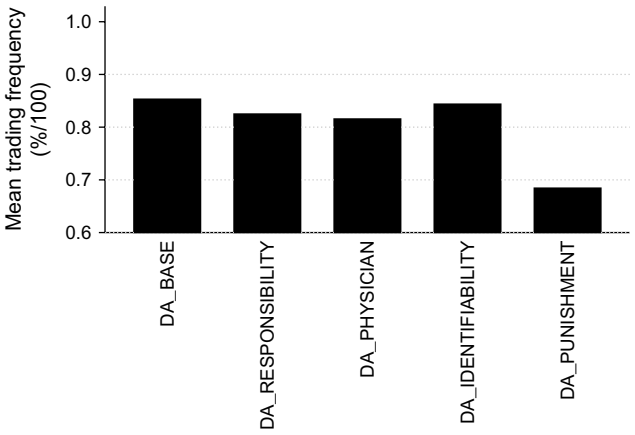
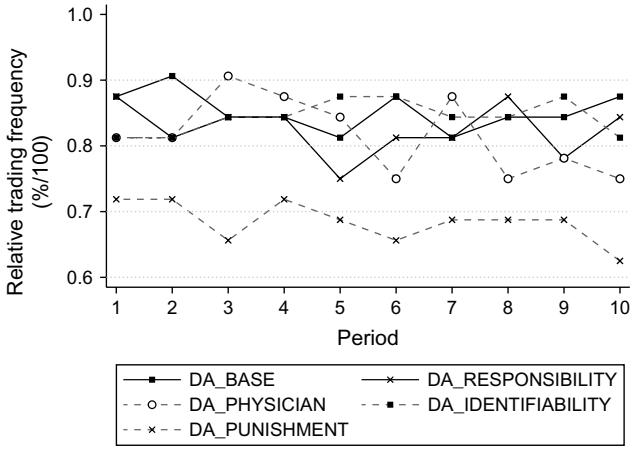


Figure 4 **Average Trading Frequency Over Time**



a particular treatment. Identical to Falk and Szech (2013), we observe that prices decline over time in all treatments, except for DA_PHYSICIAN.

Figure 5 **Mean Prices Over Time**

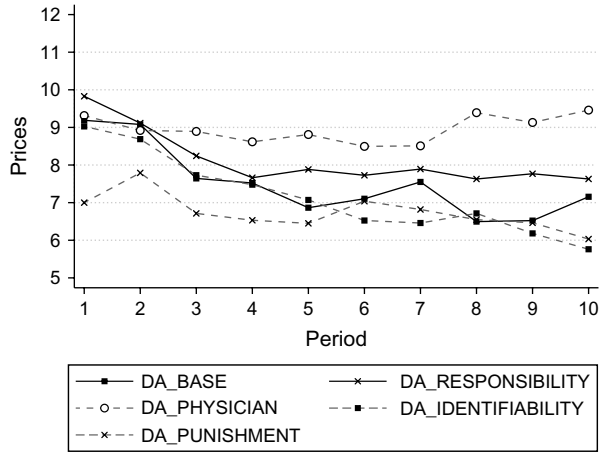


Table 1 **GLS Random Effects Panel Regression for Subjects Switching from Donation to Individual Payment in the Individual Treatments**

	Switching Point	
	Model 1	Model 2
CL_PHYSICIAN	0.926	0.933
	(1.171)	(1.275)
CL_IDENTIFIABILITY	1.813**	2.170***
	(2.128)	(2.701)
CL_PUNISHMENT	2.108**	2.988***
	(2.279)	(3.557)
Period		−0.042
		(−0.785)
Period × CL_PHYSICIAN		−0.001
		(−0.019)
Period × CL_IDENTIFIABILITY		−0.065
		(−0.924)
Period × CL_PUNISHMENT		−0.160**
		(−2.373)
α	9.215***	9.444***
	(13.969)	(17.525)
N	2,390	2,390
p-value of *F*-test	0.073	0.000

Notes. Dependent variable: number of the choice pair where subject *i* switches from donating to UNICEF to preferring the individual payment (*Switching Point*). The variable ranges from 0 (the subject always takes the individual payment) to 22 (the subject always donates). Binary dummy variables for each treatment serve as independent variables. Treatment CL_BASE is the benchmark and is therefore captured with the intercept α. Standard errors on a cohort level (i.e., the cohort of subjects that constitute an experiment session) are clustered to account for correlations between subjects. *z*-values are given in parentheses.

** and *** represent the 5% and 1% significance levels, respectively, of a two-sided test.

3.2. Statistical Tests

To test for statistical differences between the treatments of regime CL, we run generalized least squares (GLS) random effects panel regressions with 239 cross sections *s* (subjects) and 10 observations over time *p* (periods). To account for correlation across periods, we apply clustered standard errors on a cohort level of 10 subjects (White 1980).[9] In Table 1 the number of the choice pair where subject *i* switches from making a donation to preferring the individual payment (*Switching Point*) serves as the dependent variable.[10] In Model 1 we use binary treatment dummy variables

[9] In treatment CL_IDENTIFIABILITY a cohort of 10 subjects is connected by seeing each other's decisions, and in treatment CL_PUNISHMENT a cohort of 10 subjects is connected by two observers, who see their decisions and can punish them. Therefore, we take into account possible correlations between these subjects. For consistency, we applied clustered standard errors for all CL treatments alike, including CL_BASE.

[10] Since the dependent variable is integer and discrete, one should use a GLS random effects panel regression cautiously. For this reason, we additionally conducted ordered probit as well as negative binomial regressions for Model 1. We find the same treatments to be

Table 2 GLS Random Effects Panel Regression for Relative Trading Frequency and Market Prices in the Market Treatments

	Trade		*Price*	
	Model 1	Model 2	Model 1	Model 2
DA_RESPONSIBILITY	−0.028	−0.031	0.535	0.406
	(−0.323)	(−0.364)	(0.474)	(0.464)
DA_PHYSICIAN	−0.038	0.002	1.596	0.514
	(−0.453)	(0.024)	(0.950)	(0.422)
DA_IDENTIFIABILITY	−0.009	−0.040	−0.385	0.140
	(−0.138)	(−0.589)	(−0.286)	(0.141)
DA_PUNISHMENT	−0.169**	−0.150**	−0.776	−1.473
	(−2.430)	(−1.963)	(−0.545)	(−1.219)
Period		−0.003		−0.241***
		(−1.473)		(−2.616)
Period × DA_RESPONSIBILITY		0.001		0.023
		(0.164)		(0.215)
Period × DA_PHYSICIAN		−0.007		0.197
		(−1.284)		(1.483)
Period × DA_IDENTIFIABILITY		0.005		−0.095
		(1.107)		(−0.738)
Period × DA_PUNISHMENT		−0.003		0.127
		(−0.543)		(1.012)
α	0.853***	0.869***	7.410***	8.735***
	(14.835)	(16.030)	(7.321)	(10.850)
N	400	400	400	400
p-value of *F*-test	0.024	0.010	0.566	0.000

Notes. Dependent variables: relative trade frequency (*Trade*) and mean market prices (*Price*). *Trade* measures the number of trades as a ratio of the maximum possible number of trades in period p of market m. Binary dummy variables for each treatment serve as independent variables. Treatment DA_BASE is the benchmark and is therefore captured with the intercept α. Standard errors on a market level are clustered to account for correlations between subjects. z-values are given in parentheses.

** and *** represent the 5% and 1% significance levels, respectively, of a two-sided test.

as predictor variables. Treatment CL_BASE serves as benchmark and is captured with the intercept α. In Model 2 we additionally investigate the time trend in each treatment by adding a variable *Period* that runs from 1 to 10 and indicates the time trend of CL_BASE. The interaction of *Period* with the different treatments measures the differences in time trends with respect to CL_BASE.

We find that subjects switch from the donation to the individual payment at significantly higher individual payments in treatments CL_IDENTIFIABILITY and CL_PUNISHMENT, compared with the baseline CL_BASE. Furthermore, we find no time trend in the baseline treatment and no differences in time trends except for treatment CL_PUNISHMENT. It is important to mention that the coefficients of CL_IDENTIFIABILITY and CL_PUNISHMENT remain significant in Model 2. This means that the effect of removing anonymity and the threat of monetary punishment is already present at the beginning of the experiment and that both interventions have an effect even before subjects start observing the actions of others or can be punished.[11]

To test for statistical differences between the treatments of regime DA, we run similar GLS random effects panel regressions with clustered standard errors on a session level (White 1980). In Table 2 the relative frequency of trade (*Trade*)—i.e., the number of trades as a ratio of the maximum possible number of trades in period p of market m—and the respective average trading price (*Price*) serve as dependent variables.

We find that the relative trading frequency is only significantly reduced in treatment DA_PUNISHMENT compared with DA_BASE (see column *Trade*). We further find that there are no differences in time trends between DA_BASE and the other treatments with respect to the trading frequency. Again, it is important to mention that the coefficient of the treatment dummy of DA_PUNISHMENT remains significant in Model 2. This indicates a significantly lower

significant in both regressions compared to the GLS random effects regression as shown in Table 1. The two additional regressions are reported in Table A3 in the online appendix.

[11] Table A1 in the online appendix gives pairwise coefficient tests of the explanatory variables of Table 1. None of the tests is significant.

trading frequency already in period 1 before any monetary punishment is possible. This is evidence for the effect that already the *threat* of potential punishment increases moral behavior on markets in treatment DA_PUNISHMENT.

In the two right-most columns we show that price levels are indifferent between treatments. In addition, we find the typical negative price trend of treatment DA_BASE—as also found in Falk and Szech (2013)—but no treatment differences with respect to the time trend.[12]

We can summarize the two major results of our experiment as follows:

Result 1. In the CL regime, removing participants' anonymity and the threat of punishment lead to more moral behavior. Increasing the involvement of participants, however, does not improve moral behavior.

Result 2. In the DA regime, all nonmonetary interventions—i.e., reminding participants of their responsibility, increasing the involvement of participants with the traded good, and removing participants' anonymity—fail to lead to more moral behavior. Only the threat of punishment improves moral behavior in this framework.

3.3. Discussion of Results

The findings of the previous subsection raise some important questions. The first addresses why we do not provide statistical comparisons between regimes for the same intervention (treatment). We believe that making these comparisons in a controlled way is almost impossible, because both regimes differ in more than just one dimension, which makes it very difficult to attribute particular treatment differences to specific causes. Some of the most important differences between CL and DA are the following: individual decision making in CL versus bargaining decision making (on markets) in DA; no feedback during the decision process in CL versus continuous feedback about prices and other subjects' offers in DA; relatively quick decisions in CL (by clicking through the 22 choice pairs) versus three minutes of trading in each period in DA. Therefore, we abstain from directly comparing the same interventions across regimes. We would like to recall that the aim of having the two different regimes was to investigate how various interventions work in different environments—that share some features with respect to trading off money and a potentially life-saving donation, but that differ in many others. So, the two regimes are mainly motivated to check the robustness of interventions.

[12] Table A2 in the online appendix gives pairwise coefficient tests of the explanatory variable *Trade* of Table 2. We observe significantly fewer trades and hence more moral behavior in treatment DA_PUNISHMENT compared with all other treatments.

The second major question is why interventions in both regimes do not always yield identical results. Note first that the additional information provided by the physicianand the opportunity of monetary punishment do lead to the same outcomes across regimes (with the physician having no effect, but the monetary punishment having one). Note, however, that since we measure different variables in both regimes, monetary punishment affects the switching point from the individual payout to the donation (which is comparable to a price) in CL and trading frequency in DA. The intervention of removing anonymity in treatments DA_IDENTIFIABILITY and CL_IDENTIFIABILITY has different effects across regimes. We conjecture that the following explanations might account for this difference. In the double-auction markets subjects share responsibility, because there is always a counterpart when trading occurs. In regime CL, subjects do not share the responsibility of refusing to donate but are solely responsible for their actions. This difference between regimes can likely be attributed to the diffusion of responsibility (Darley and Latane 1968, Ciccarelli and White 2009). In the choice list regime, each subject determines the final outcome herself, and there is no other subject that can be blamed for it. In the double-auction setting, in contrast, a trade can only be conducted when two subjects agree on it. Therefore, in this setting the actions of two subjects determine the final outcome, where each one can excuse herself by blaming the other for concluding the trade. Since such diffusion of responsibility does not exist in regime CL, being individually identifiable for immoral actions by the public might be more strongly avoided by the subjects in treatment CL_IDENTIFIABILITY than in treatment DA_IDENTIFIABILITY. Another reason why we do not observe an effect in DA_IDENTIFIABILITY might be that in this treatment subjects can immediately see when and that other subjects in the market trade. Given the high frequency of trading, observing others trade might create a perception that trading—and thus taking the money—is socially acceptable. In treatment CL_IDENTIFIABILITY it is less obvious that others take the money instead of donating, because that depends upon which choice is randomly selected for implementation. This difference might contribute to the different effects of identifiability across regimes. Of course, the two explanations offered here do not rule out other causes and are in line with the findings of Cappelen et al. (2013) that both intrinsic and extrinsic motivation can explain prosocial behavior. In our setting, subjects might need to be intrinsically motivated to choose a donation, but this motivation might be reinforced by the extrinsic motivation of being solely identifiable for one's actions. Because of the

above-mentioned reasons, these social motives might be stronger in regime CL.

The third major question is concerned with the real-world analogy of our CL treatments. Our choice-list regime is comparable to the individual treatment of Falk and Szech (2013). Although they interpret it as an individual decision frame and contrast it to double-auction markets, we believe that this individual decision making in the CL regime resembles many everyday life situations of decisions in market settings as well. As customers, individuals are often price takers without an opportunity to bargain about prices. They decide about buying or not buying a product (often at varying prices), and in case of buying it they are obviously willing to accept the potential harm done to uninvolved third parties.

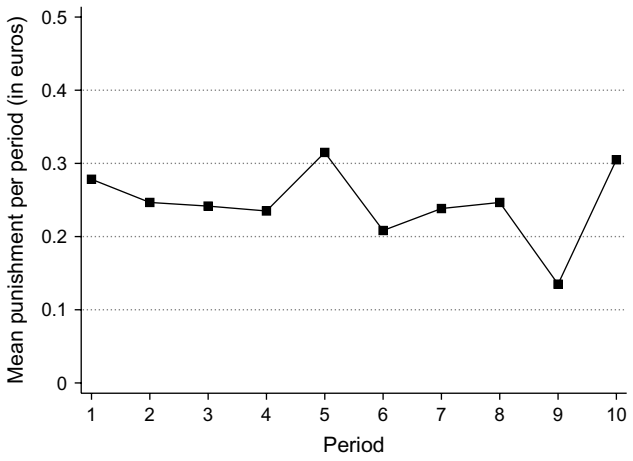
3.4. Analysis of Observer Behavior

Figure 6 shows the average amount of euros spent for punishment per subject and period in CL_PUNISHMENT. We find that the amount used for punishment is low and on average amounts to 0.25 euro per participant and period. To investigate whether there is a relationship between a subject's frequency of taking the payment for herself and the total amount of euros she is punished with, we run GLS panel regressions (60 subjects, 10 observations over time each). We first normalize the amount of punishment on a specific subject by the total punishment that the respective observer implements on all five assigned subjects (thus controlling for individual differences across observers) and then regress this relative amount of punishment on the variable *Switching Point*, furthermore on a dummy variable indicating whether the subject received an individual payment in the preceding period (*Payout*) and on a period variable (*Period*). We cluster standard errors on a cohort level of 10 subjects each (White 1980).[13] We find a significantly negative coefficient for *Switching Point* and a significantly positive coefficient for *Payout*. This indicates that observers punish subjects more severely when they have taken the individual payout already at lower amounts and when they have more often taken money in the past instead of donating to UNICEF. *Period* is significantly negative, indicating that observers punish less over time.[14]

Across all markets in treatment DA_PUNISHMENT, 90.1% of all euros spent for punishment are allocated to subjects who traded in the current period. Only 9.9% were spent on subjects who did not trade in the current period but typically had traded in a preceding period. On average, an observer spends 0.33 euro on each market participant (irrespective of trading activity) in each period. The average deduction from subjects who traded in the current period is 1.61 euros, and thus fairly small, compared to the potential gains from trade. The left panel of Figure 7 shows the average amount of euros spent for punishment per market participant and period. We find that the amount used for punishment decreases over time to a value below 0.20 euro in the last period. The right panel of Figure 7 shows the amount spent for punishment for sellers and buyers separately over time. We see that both roles are punished equally over time by the observers. We run an analogous GLS regression for regime DA as for CL.We use as explanatory variables a period variable (*Period*) and a binary dummy variable (*Trading*) indicating whether a subject traded in the preceding period. The coefficient for *Trading* is significantly positive, indicating that less moral behavior is punished more frequently.[15]

Note that the results on punishment in CL_PUNISHMENT and DA_PUNISHMENT suggest that the mere possibility of punishment has a positive effect on moral behavior in both regimes. As outlined in Models 2 of Tables 1 and 2, the differences in trading frequency between both punishment treatments and the corresponding base treatments are significant already in the first period when no prior punishment is possible.[16]

Figure 6 **Average Amount of Euros Spent for Punishment per Subject and Period in Treatment CL_PUNISHMENT**



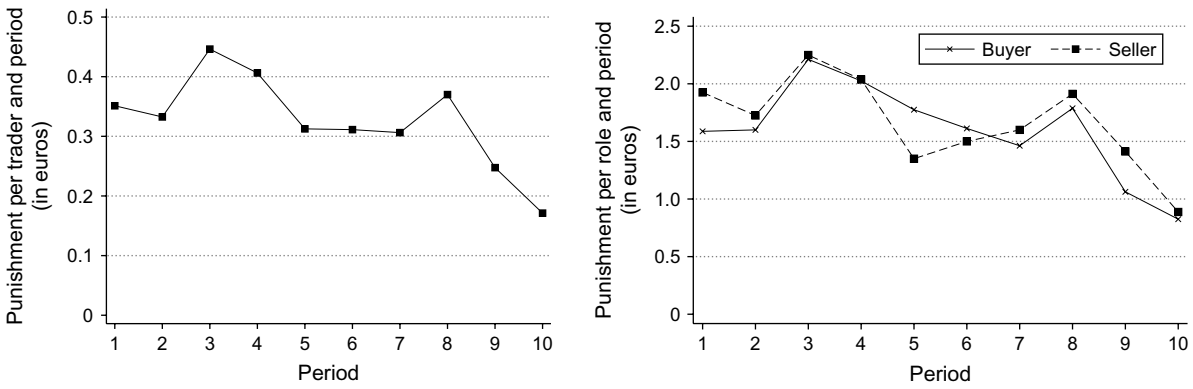
[13] As mentioned above, for treatment CL_PUNISHMENT we have to take into account possible correlations within the cohort of 10 subjects across periods. For consistency reasons, we applied clustered standard errors for all CL treatments alike.

[14] This regression is reported in Table A4 in the online appendix.

[15] This regression is reported in Table A5 in the online appendix.

[16] Previous work by Masclet et al. (2003) has found that nonmonetary sanctions can make subjects more cooperative in the framework of public goods provision. One major difference is that the Masclet et al. (2003) study relies on second-party punishment, whereas we rely on third-party punishment. Given that third-party punishment seems to have weaker effects than second-party

Figure 7 **Average Amount of Euros Spent for Punishment per Subject and Period (Left); Average Amount of Euros Spent for Punishment Over Time, Separated for Sellers and Buyers (Right)**



Note. Both panels show average values across all markets in treatment DA_PUNISHMENT.

3.5. Final Questionnaire

As a final, and important, point, we examine whether trading or taking money individually in our experiment is, indeed, regarded as unethical. Post-experimental survey evidence from 115 participants in regime CL and from 255 participants in regime DA reveals that participants judge others who take money for themselves as significantly less moral than those who refrain from trading.[17] The questionnaire was handed out at the end of the experiment and had not been announced at the beginning of the experiment. The two questions on the assessment of the moral dimension of trading read as follows:

"On a scale from 0 (very immoral) to 6 (very moral):

- How moral do you see people who have traded in this experiment? (DA)
- How moral do you see people who have NOT donated in this experiment? (CL)"

"On a scale from 0 (very immoral) to 6 (very moral):

- How moral do you see people who have NOT traded in this experiment? (DA)
- How moral do you see people who have donated in this experiment? (CL)"

In regime CL the average scores are 2.53 for the first question and 4.41 for the second question. The reported difference between both questions is highly significant (Wilcoxon signed-ranks test, $p = 0.000$, $N = 115$).[18] We obtain similar results for regime DA, where average scores are 3.05 for the first question and 4.62 for the second question (Wilcoxon signed-ranks test, $p = 0.000$, $N = 255$). The results from the questionnaire show that participants have a concise view on what is the moral action to take and what is not.

4. Conclusion

By testing two different institutional regimes we have investigated whether several different interventions could prevent or reduce moral decay in laboratory experiments. We have shown that specific interventions can affect the extent of moral behavior, yet not all of them do, and not in all regimes studied here.

The common ground that we found across both regimes is that the threat of monetary punishment increases the level of donations, and subjects become more moral by donating money to UNICEF more often than they take it for themselves. Moreover, both regimes also share the feature that getting subjects more involved with the traded good—here by letting a physician explain the risks, the actual death toll of measles, and how vaccination can help to prevent measles—does *not* change behavior. Relating this finding to the real world outside the laboratory suggests that information campaigns might have limited effects, if any.

Both regimes differ with respect to the influence of removing a subject's anonymity. Whereas this intervention had no influence on behavior in the DA market, it did have one in the CL regime. We believe that removed anonymity in our choice list regime works better than in the double-auction regime, because there is no diffusion of responsibility in the former and because in the DA regime subjects observe a large

punishment (Leibbrandt and López-Pérez 2012), this difference might explain why we did not find significant effects of nonmonetary interventions.

[17] We only administered the questionnaire for three treatments of regime DA—DA_PHYSICIAN, DA_IDENTIFIABILITY, and DA_PUNISHMENT—and, because of time constraints, not for sessions of treatment CL_PHYSICIAN and CL_PUNISHMENT, but only for treatments CL_IDENTIFIABILITY and CL_BASE.

[18] Including the answers for CL_PUNISHMENT_MILD, scores are 2.54 and 4.33, respectively. The Wilcoxon signed-ranks test shows similar results: $p = 0.000$, $N = 185$.

degree of trading, thus making it somewhat socially more acceptable.

Overall, our paper had the goal of studying whether and how different interventions might influence the degree of morality when subjects can make decisions that can generate negative externalities on uninvolved parties. By considering two fairly different regimes, we have exposed the various interventions to a kind of robustness check, finding common ground, respectively effects, for two interventions, but different effects for a third one. We consider these important steps for a better understanding of how institutional rules—governing markets or individual decision making—affect moral behavior. Both society on a more general level and companies in particular can benefit from such knowledge that may help to design rules that promote compliance with ethical norms and standards.

Supplemental Material

Supplemental material to this paper is available at http://dx.doi.org/10.1287/mnsc.2015.2246.

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