On the Dynamics of Altruistic Behavior

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Abstract

We study a series of dictator games repeated a number of times at considerably large time intervals. The experimental design is such that reputation and learning effects can be ruled out. Treatments differ with respect to the number of repetitions, the time span between repetitions and observability of behavior. We observe in all treatments a strong tendency towards more selfish behavior over the course of the repeated experiment. We argue that this behavior could be rationalized if the act of giving in dictator games is driven by social norms that approve repeated gifts over a single altruistic act. We report the results of the experiment using the norm elicitation method introduced by Krupka and Weber (2013).

Keywords: dictator game, repeated experiments, dynamics of behavior, norm elicitation.

JEL-Class.-No.: C91, C73

All Data files, Do files and instructions are available on x-econ.org – an online repository for experimental data: https://doi.org/10.23663/x2027

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1 Introduction

The study of other-regarding preferences is one of the most important topics for experimental economists that has emerged during the last three decades. The discovery that these kinds of preferences play a crucial role in many important economic situations is one of the most impressive findings deriving from economics laboratories. It has provoked the development of new theories and changed the way economists think about human decisions.[1]

Theoretical and experimental research on other-regarding preferences is nevertheless also subject to substantial criticism. Much attention has been given to a debate started by Levitt and List (2007), who highlight a number of methodological problems inherent in laboratory experiments and question the external validity of experimental findings.[2]

One particular concern expressed by Levitt and List, with regard to other-regarding preferences, is the generalizability of findings from one-shot experiments. They claim that prosocial behavior, rather than other-regarding preferences, in one-shot lab experiments might indicate concerns about reputation building because, "personal experiences may [effectively] cause subjects to play these one-shot games as if they have some repetition." They also point out that "social dilemmas are typically not one-time encounters, but rather repeated games" and raise the question whether an "effect observed in the lab manifests itself over a longer time period." DellaVigna (2009) adds to the criticism directed at theories of social preferences by pointing out that the theories based on the experimental findings regarding prosocial behavior seem to over predict giving in the field. Putting these two points together could lead to the hypothesis that the over prediction of "other-regarding behavior" is caused by the fact that laboratory experiments usually do not investigate repeated identical decisions within a longer time span, but rather behavior (one-shot or repeated) within a single experimental session.

Studying decisions over a long period is not possible with the standard procedure used in experimental economics. In fact, to create a repeated situation it is necessary to repeat experimental *sessions* and not to repeat decisions within a single session. If sessions are repeated, subjects become familiar with the experimental situation, and they can display well-developed

[1] See Cooper and Kagel (2016) for a selective overview.

[2] Besides general comments on the pros and cons of experiments conducted in the laboratory (Falk and Heckman (2009), Bardsley et al. (2010), Croson and Gächter (2010), Henrich et al. (2010)), one strand of the literature started to test the crucial methodological points empirically. For example, Barmettler et al. (2012) investigated the effect of the "experimenter-subject" interaction under laboratory conditions. Slonim and Roth (1998), Cameron (1999) and, more recently, Andersen et al. (2011) investigated whether social preferences disappear under high stakes and Fehr and List (2004) asked if student subjects show significantly higher degrees of social preferences than non-student subjects.

behavior. One obvious difference between the repetition of sessions and the repetition of decisions is that in the latter case the average opportunity costs[3] decline with every new round played in the experimental session, while they are constant over repeated sessions.[4]

In a recently published paper, Brosig-Koch et al. (2017) show that in a repeated experiment with modified dictator games, the amounts the dictators handed over to the recipients decreased sharply. When the experiment was conducted the third time, nearly any money was given to the recipients. This observation seems to be in line with the suspicion of DellaVigna (2009) and others that single session experiments are not able to reproduce situations containing the choice between selfish and altruistic behavior if these situations occur repeatedly.

In experimental economic research, we are interested in results that prove to be robust, i. e. which can be reliably reproduced and which do not depend on details of the experimental arrangement. Should it become apparent that the result of Brosig-Koch et al. is robust in this sense, it will have important implications for the experimental investigation of dictator games. It would then have to be assumed that the donations observed in non-repetitive experiments represent the maximum altruistic behavior that can be observed in the laboratory.

How such a result is to be interpreted depends mainly on the cause of the observed decline in altruistic behavior. Brosig-Koch et al. offer two explanations, which are not mutually exclusive. Firstly, a strong *experimenter demand effect* (EDE), which loses itself when the experiment is repeated, and secondly *moral self licensing* (MSL). The latter refers to the effect that when people perform a good deed, they derive the right to think more strongly about themselves in the future. Should it become clear that the EDE statement is correct, this would have a significant impact on the interpretation of dictator experiments and on the experimental exploration of social preferences in general. It would then have to be examined, for example, whether other experimental evidence of social preferences can be attributed, at least in part, to an EDE.

This paper is divided into two parts. In the first part, we examine the robustness of the result of Brosig-Koche et al. and examine the question of whether the decrease in handovers is related to the way in which the experiment is carried out. On the one hand, we will test the frequency of repetition and on the other hand the time span between repetitions. In addition, we compare a treatment in which the experiment is repeated four times at variable intervals. For example,

[3] This covers all the costs subjects have if they move to the laboratory and spend some time there.

[4] The argument also holds for multi-round experiments in which only one randomly chosen decision is payoff relevant, because in these kind of experiments subjects are forced to look at the decisions as "repeated one-shot decisions". Therefore, they do not explicitly focus on repeated decisions.

one of the intervals will be to conduct the experiment weekly on the same day within the experimental window. Checking the robustness also means that we will not use a modified dictator game (as Brosig-Koch et al. did), but the classic version.

The study of robustness also encompasses checking whether the repetition effect is still valid when combined with other influences on the dictator's behavior. From the literature, it is known that the observability of dictator behavior has a very positive effect on the donations in dictator experiments. The question is whether this positive effect cancels or at least weakens the repetition effect. We investigate this by performing all treatment, both single blind and double blind.

The first part of our study shows that, once again, the willingness to forego one's own income decreases over time: subjects became more selfish when they were repeatedly put in identical situations in which they could decide either to be selfish or to act altruistically. Moreover, it seems to be that the number of repetitions is more important than the time span between each repetition. The observability of dictator behavior only affects the level of the donations to the recipients but not the dynamics of behavior. Finally, we find that a regular pattern has a stabilizing effect on altruistic behavior and reduces the observability effect.[5]

In the second part of the paper, we look for indications that there is actually an MSL effect behind the repetition effect. A simple theoretical consideration shows that a reduction of the gifts to the recipients in a repeated dictator experiment is a rational reaction when the repeated gift of amount x leads to a higher level of social recognition than a one-off gift of x. We use the norm elicitation mechanism introduced by Krupka and Weber (2010) to test the hypothesis that repeated donations lead to higher social recognition. It shows that this cannot be derived from the data. Though we cannot rule out that MSL driven by higher social recognition plays a role, the experimental evidence for this is weak.

The rest of the paper is organized as follows. After a short review of the related literature (section 2) in section 3 we briefly discuss some methodological features that are of importance if experimental sessions are repeated. In section 4, we will present the design and the results of the experiments we conducted to get a deeper understanding of the "repetition-effect". In section 5, we investigate the moral self-licensing hypothesis. Section 6 concludes. All data files,

[5] A further question is whether the observation made by Brosig-Koch et al. (2017) also prevails in experiments employing games with strategic interactions between subjects. We do not deal with this question in this paper. However, we do this in a different paper (Sass et al. 2018), in which a public good game is repeated. There we show that, the repetition of the experiments also leads to a significant reduction of the contributions to the public good. Although a direct comparison with the dictator game is difficult, we have the impression that the repetition effect is a bit smaller in the public good setting.

do files and instructions are available on x-econ.org – an online repository for experimental data.

2 Related Literature

There are very few experiments in which dictator game *sessions* are repeated, but there are some papers in which the dictator game was repeated *within* one session. However, in most of these experiments only one randomly selected repetition was payoff relevant. Thus, these experiments are more repeated one-shot experiments than a repeated experiment in the narrow sense of the word.

Cason and Mui (1998) repeated the dictator game in a buyer-seller frame and investigated the impact the effect of different information about the other player had on dictators' offers. The information was either "relevant" (informed the dictator about what the other player did in a previous round) or "irrelevant" (information about the date of birth). Interestingly, subjects became more selfish after receiving irrelevant information but not after gaining relevant information.

Hamman et al. (2010) repeated the dictator game 12 times within a session, but, as was the case in Cason and Mui (1998), only one randomly selected round was paid. They found that the amount dictators offered fell in the course of the experiment. One year later they repeated the experiment with different subjects and found no decrease in the offers, but rather the amounts handed over to the recipients were very low from the start. In a meta-study covering 129 studies, Engel (2011) found that if the dictator game is repeated, contributions decay and the equal split is chosen less often. The results of Brañas-Garza et al. (2013) contradict these findings. They played 16 dictator games and varied the information dictators received about the recipients. They found no decay of the offers and no impact of the frame (the information given to the dictators). However, they found a strong moral self-licensing effect and a strong moral cleansing effect. Having given generously, the dictators became more selfish in the next round of the game, and after being selfish, they were willing to give more the next time. Achtziger et al. (2015) played 12 dictator games and observed that the amounts handed over to the recipients decreased. However, the incentive structure of their experiment differs from a standard dictator game inasmuch as within the 12 games, the players randomly and anonymously received either the dictator's share or the recipients share. Achtziger et al. conjecture that the decay of the amount gifted to the recipient is caused by the increasing experience the player gains during the experiment.

Summing up, the experiments with repetitions within a session deliver mixed evidence. In most experiments, the amounts handed over to the recipients decrease, but none of the experiments repeated the dictator game identically. This makes it hard to interpret the results with respect to the question of what the pure repetition effect in the dictator game is.

The dictator game sessions are repeated with the same subjects in three experiments[6]. Brañas-Garza et al. (2013b) repeated the experiment for 7 months with the same subjects and observed a dramatic decrease in the amounts given to the recipients. The experiment was run in a classroom wherein the first wave was conducted in the first week of the students' first academic year. The repetition was conducted at the end of the first term. Between the two repetitions, there was considerable scope for learning to take place; for example, students became familiar with each other and they learned basic economics. It is highly plausible that learning effects are the reason for the behavioral changes observed by Brañas-Garza et al. (2013). Schmitz (2014) conducted a charity donation experiment with one repetition either after 4 hours or after one week. He observed that the share of the endowment donated decreased in both cases.

Brosig-Koch et al. (2017) is the most closely related paper and we directly refer to it. The games investigated there are 8 modified dictator games and two sequential prisoner dilemma games. The modification to the dictator games consists of a variable price for the donations the dictator could make to the recipients. In a pure dictator game, one dollar donated by the dictator increases the recipient's payoff by one dollar. In the modified versions of the dictator game used by Brosig-Koch et al. (2017), this relation varied between ½ and 2. These games were played in either a "give" mode (the dictator owns the endowment and he can give money) or a "take" mode (the recipient owns the endowment and the dictator can take money). All these games were repeated twice with a span of four weeks between repetitions. In each wave, the recipients were newly recruited. The experiment was designed in a way to eliminate learning and reputation effects as far as possible and to ensure that subjects in each of the three waves were confronted with the same decision problem. The most important finding is that altruistic behavior in the first wave of the experiment was in the range of comparable one-shot experiments reported in the literature and was nearly completely absent in the last wave.

Brosig-Koch et al. (2017) offer two different explanations for the decay of altruism. The first starts with the assumption that in one-shot dictator games, a strong experimenter demand effect is at work inducing the impression that "giving" is the right thing to do. If the experiment is

[6] In the following, we will refer to each repetition of a session as a "wave".

repeated, this effect is watered down and thus the pressure to give decreases. The second explanation is that the reduction of gifts to the recipients is caused by a moral self-licensing effect. Donations made in the first wave justify being more selfish in following waves. Merrit et al. (2010) offer an overview of the literature on this effect. Gneezy et al. (2014) introduce a rather similar effect as an explanation for positive offers in dictator games. According to their theory, giving to a charity is the result of *conscience accounting* because past immoral actions can be neutralized by being altruistic in the present.

3 Methodological aspects of repeated sessions experiments

Situations in which social preferences play a role are typically not single events. On the contrary, they occur more or less as regularly repeated identical situations. The effect of these repetitions is that people who must decide whether to behave selfishly or altruistically become familiar with this kind of decision. Most of the social behavior in the real world happens to people who have already learned how to handle these situations. Most of the behavior we observe in the real world is well-developed behavior. If we want to investigate how decision makers behave after they have learned everything that can be learned about a particular decision problem, we have to design the experiment accordingly. This means that the experiment should provide scope for well-developed behavior and avoid any influences other than those arising purely from repeating the game. Therefore, the experimental design should take into account the following points:

- The repetitions must be *identical* in every aspect of the decision problem.
- The subjects must be able to learn everything about the decision situation rather quickly. This implies that the decision problem should be as simple as possible. After becoming familiar with the decision situations, they should be familiar with everything. No further learning should be necessary or possible.
- Reputation effects should also be completely excluded because they would alter the situation from wave to wave.
- The repetition of sessions is unavoidably accompanied by a loss of control because it is not possible to observe what the subjects do between the repetitions. Thus, the experimental design must compensate for this disadvantage and minimize the possibility of systematic biases.

In the following, we describe how the design of our experiment considers these methodological reflections.

Measuring social preferences and compensating for the loss of control

The dictator game allows us to measure prosocial behavior in a very direct and simple way. The share handed over to the recipient gives us a direct measure of the strength of the prosocial behavior revealed by the dictator. A downside of employing the dictator game is its well-established proneness to framing effects. For this reason, we use several different treatments and thus gain a high number of independent observations. Using multiple treatments also helps to compensate for the unavoidable loss of control that accompanies repeated experiments. If particular patterns of behavior can be observed in many, or all, of these treatments, we can rule out that unobservable events happening outside the laboratory cause such a pattern because these events are likely to randomly influence behavior in all directions. Further measures to compensate for the loss of control are design elements that ensure that subjects have no contact with each other. This reduces the probability that subjects talk with each other between the sessions.

Ensuring identical decisions

To ensure that all waves are identical, we use exactly the same procedure in each wave. Among other things, this ensures that the opportunity cost of coming to the laboratory and participating in the experiment is identical in each wave. This is an important difference to an experimental design in which an experiment is repeated identically within one session. In the latter kind of experiments, the average opportunity cost declines with each repetition.

Upon entering the laboratory, each subject was shown a live video transmission of another room in the laboratory in which the recipients of the dictator game were seated. The resolution of the video image was so low that subjects were unable to recognize the identity of the recipients. This was done to make sure that the dictators realized they were paired with real subjects. Each recipient could only take part once in the experiments conducted for this study and did not receive a show-up fee. The dictators were informed (in the written instructions, see Appendices A and B) that the recipients were recruited for each wave separately and that they would always be matched with someone new who only took part in one single session. This ensured that the dictators really were in identical situations in each wave: in each wave they were paired with a new recipient and received an endowment of 10 Euros.

The payoff was organized as follows. The dictators were given an endowment of EUR 10, split into ten single EUR 1 coins. They were asked to put the money they wished to give to the recipient into an envelope and keep the rest for themselves. After the dictators left the laboratory, the envelopes were randomly distributed amongst the recipients in the other room. One may argue that the fact that dictators in former waves could earn some money may cause an income effect and thus the repetitions are not strictly identical. We are aware of this point, but if the small amounts of money earned in previous waves have an income effect, it is very plausible that a donation to the other player is a *normal* good, which means that the donations will increase over the course of the experiment. Thus, if the income effect exists (which we cannot rule out) then it would work in the opposite direction of what we expect to observe, namely that donations decrease.

Learning about the decision situation quickly

Dictators have to make an extremely simple decision without any strategic consideration. If the dictator is allocated an amount of money X, nobody has to "learn" that X is greater than $X-y$ for any positive y given to the recipient. Therefore, we can rule out that learning to play the dominant strategy leads to dictators reducing their gifts to the recipients. The only thing left to be learned is the experimental situation itself – and this can be done in the first wave of the experiment.

Excluding reputation and further learning effects

We cannot observe what subjects do in the time passing between the two waves. This loss of control could be problematic, notably if subjects talk to each other about the experiment and learn from others what the "right" behavior in a dictator game is. This might change their own behavior in the next wave. At the same time, this could lead to strong reputation effects. We minimized the risks of such undesired influences by implementing an elaborate procedure of picking up each subject at an individual meeting point, escorting the subject to the lab and having the subject take a seat inside a single soundproof, opaque booth. After the experiment, each subject left the laboratory individually. Thus, in no waves and at no point in time did a subject learn of any other subject's involvement in the same experiment. The only information subjects gained from the experiment was their own decision. As already mentioned, the advantage of the dictator game is that it is immediately clear what the consequences of a decision are and nobody has to learn his dominant strategy. Weber (2003) shows that learning could also take place without any feedback except for one's own decision in a previous round. However,

Weber observed learning without feedback in a guessing game in which it was rather complicated to calculate the equilibrium choices, and k-level reasoning was necessary to find the best reply. It is plausible that a subject who behaved as a level-k player in *t* learns that she should optimize against level-k in *t*+1. In a simple dictator game, there is no room for learning at all.

4 Experiments part one: Investigation of the repetition effect

4.1 Treatments

The experimental procedure was already described in section 3. The design of the treatments follow two different research questions. The first concerns the behavioral dynamics over the repetitions of the sessions and the question of how these depend on parameters that describe the pattern of repetitions. The second aims at the interaction of the repetition effect with other elements of the experimental design. We decided to vary the observability of behavior since it is well known that in a dictator game it makes a substantial difference whether giving is observable or not. The question is whether the behavioral *dynamics* are also affected by the observability of behavior.

Behavioral dynamics

One purpose of our experimental design is to gain a more comprehensive picture of the forces that are at work if identical repetitions take place. To do this, it is helpful to vary the characteristic elements of the repeated situation: the *number of repetitions*, the *time span between repetitions* and the *regularity of repetitions*. In order to separate the effect of the length of the time span between repetitions, we ran treatments in which the initial experiment is only once repeated but with different spans of time between the start and the second wave: 2 hours (2H), 2 days (2D) and 2 weeks (2W). We compared these treatments with treatments in which the game was repeated three times: after 2 hours, 2 days and 2 weeks. We also ran a treatment with four waves and a constant time interval of one week between repetitions to study the effect of a regularly occurring event that forced the dictators to choose between selfish and non-selfish behavior.

Social distance, single-blind vs. double-blind

Studies by various authors[7] have shown that the observability of behavior is a crucial determinant of altruistic giving in the sense that double-blind treatments reveal lower donations in dic-

[7] See, e.g., Ariely et al. (2009), Harbaugh (1998), Hoffman et al. (1996).

tator games than single-blind treatments. The observability, or image-effect, should not be confused with the social norm effect detected by Krupka and Weber (2013). Social approval in their framework does not directly depend on the observability of a good deed, because approval and disapproval are intrinsically motivated. Social norms have an impact on behavior because they are internalized. Given this difference between the self-image and the social norm effect, it would be interesting to see how the observability of donations influences the dynamics of dictator behavior. To account for the impact of the observability of behavior, we conducted all the experiments described above in a single-blind and a double-blind treatment. Since there is a high probability that the observability of the dictator behavior will affect decisions, the comparison of these two treatments allows us to study how the repetition effect interacts with other determinants of prosocial behavior. Our conjecture was that this observability will have no direct effect on the dynamics of prosocial behavior, but a level effect on the amount given by the dictators to the recipients. The reason for this conjecture is that we assume that the decrease in altruism is driven by mechanisms that are independent of the "image effect" triggered by the observability of behavior. At least, this holds for the two explanations Brosig-Koch et al. (2017) offer for the decay of altruism.

As a side effect, running all the experiments double- and single-blind doubles the number of treatments. This is important because the unavoidable loss of control that goes along with the repeated-session method makes it necessary to obtain robust results. The high number of independent observations in the different treatments rules out the possibility that random influences outside the laboratory are responsible for the effects observed in the different waves.

The payoff procedure for the single-blind treatments was straightforward. Upon leaving the booth, the dictators were asked by the experimenter to sign a receipt for the money to be taken home. Dictator behavior was thus directly observable. The dictators knew from the instructions that they would have to sign a receipt immediately after the experiment.

In the double-blind treatments, we had all the dictators draw a secret fake identity for the course of the experiment before the start of the first wave. The dictators randomly picked a sealed envelope containing a number of identical paper strips on which the name of a city was printed. The number of paper strips in the envelope corresponded to the number of waves in which the dictator took part. In each wave, the dictators were required to put one of the paper strips into the envelope together with the money they wanted to give to the recipient. This procedure enabled us to track individual behavior without knowing the identity of the dictator. Between the waves, the dictators kept the paper strips with their private property. They were instructed not

to reveal their fake identity to any other person. All the envelopes were collected one by one by knocking on the door of the booth and having the dictators put them into a cardboard box held by the experimenter. This whole process was filmed by video camera and transmitted live to the dictators on monitor screens in their booths. Thus, they could satisfy themselves that the experimenter did not open the envelopes immediately after collecting them, making it impossible for the experimenter to identify individual behavior. Again, all information about this procedure was given to the dictators via the instructions before they made their decision.

A single session lasted 20 minutes. Subjects were recruited by ORSEE (Greiner (2015)). Table 1 summarizes all the treatments. The treatment names are constructed by the following rule: "number of waves (*2, 4*); time span between waves *H*(ours), *D*(ays), *W*(eeks); single-blind (*SB*), double-blind (*DB*)". In total 377 dictators participated.

Name	2HSB	2DSB	2WSB	2HDB	2DDB	2WDB	4FSB	4FDB	4VSB	4VDB
Number of waves	2	2	2	2	2	2	4	4	4	4
Time span between repetitions	2 hours	2 days	2 weeks	2 hours	2 days	2 weeks	1 week	1 week	2 hours 2 days 2 weeks	2 hours 2 days 2 weeks
Double-blind	no	no	no	yes	yes	yes	no	yes	no	yes
N	39	55	33	38	38	36	25	44	32	37

Table 1: Treatment overview and number of dictators (independent observations) in each treatment.

4.2 Results

Table 2 and Figure 1a to 1c present the descriptive results for the six treatments with one repetition of the dictator game grouped by the time span between the first and second game.

			Change	**Average**	**Diff**
	Start	**2 H**			
Single-blind	3.56	2.79	-21.6 %	3.18	0.81
Double-blind	2.55	2.18	-14.5 %	2.37	
	Start	**2 D**			
Single-blind	3.38	3.11	-8.0 %	3.25	1.45
Double-blind	2.05	1.55	-24.4 %	1.8	
	Start	**2 W**			
Single-blind	3.09	2.39	-22.7 %	2.74	0.14
Double-blind	2.81	2.39	-14.9 %	2.60	

Table 2: Average transfers in treatments with two waves with 2-hour, 2-day and 2-week time spans between waves. "Change" is the difference between the first and the second wave; "Average" the average transfer in both waves and "Diff" is the difference between single-blind and double-blind.

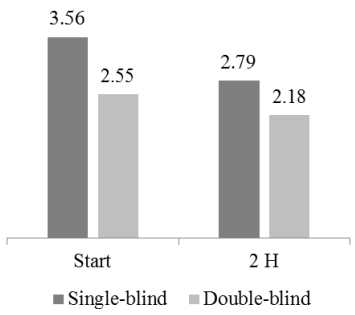

Figure 1a: Average transfers 2H

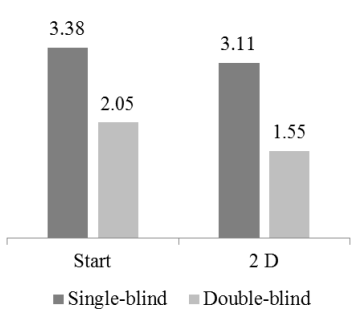

Figure 1b: Average transfers 2D

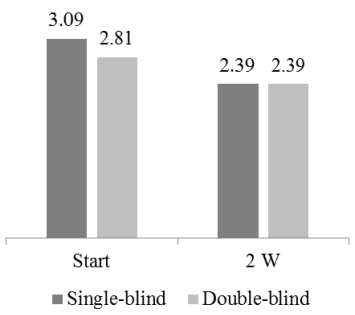

Figure 1c: Average transfers 2W

Two observations are worth mentioning. First, for all three time intervals, the second transfer to the recipient is smaller than the first transfer. The decay is statistically significant at the five-percent level (Wilcoxon signed-rank tests) with the exception of the single-blind treatment with a 2-day interval ($p = .10$). On average, the gifts to the recipients decrease by 16.9 percent. Second, for all three time intervals, subjects handed more money over to the recipient in the single-blind treatment than in the double-blind treatment. The difference between the single-blind and double-blind treatments is statistically significant at the five-percent level for the 2-hour and the 2-day treatments but not for the 2-week treatment.

Table 3, Figure 2 and Figure 3 display the results for the four treatments with three repetitions grouped by fixed and variable time intervals. Once again, we observe that the gifts to the recipients decrease over the course of the experiment.

	Start	**Diff**	**2 H**	**Diff**	**2 D**	**Diff**	**2 W**	**Diff**	**Change**	**Average**	**Diff**
Single-blind	3.28	.79	2.84	.95	2.53	.85	2.31	.88	-29.6 %	2.74	.87
Double blind	2.49		1.89		1.68		1.43		-42.6 %	1.87	
	Start	**Diff**	**1 W**	**Diff**	**2 W**	**Diff**	**3 W**	**Diff**	**Change**	**Average**	**Diff**
Single-blind	2.80	-.04	2.28	.03	2.12	.04	1.68	-.39	-40.0 %	2.22	-.11
Double-blind	2.84		2.25		2.16		2.07		-27.1 %	2.33	

Table 3: Average transfers in treatments with four waves and with fixed and varying time intervals. "Diff" is the difference between the single-blind and double-blind treatment, **H, D, W** stand for hours, days and weeks, "Change" is the change between the first and the last wave, "Average" is the average over all four waves.

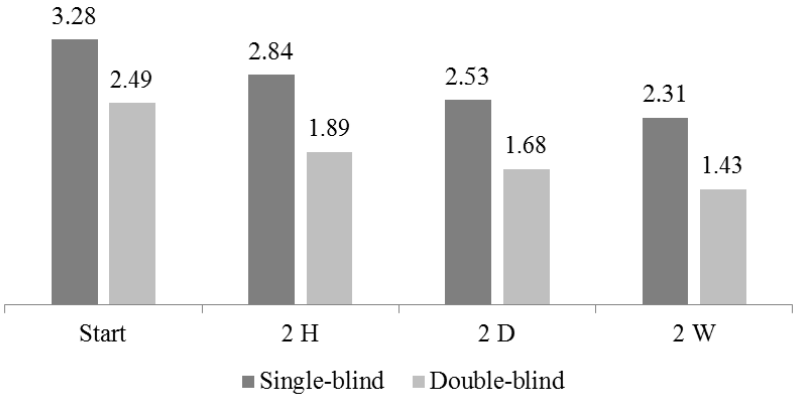

Figure 2: Average transfers three repetitions with varying time intervals

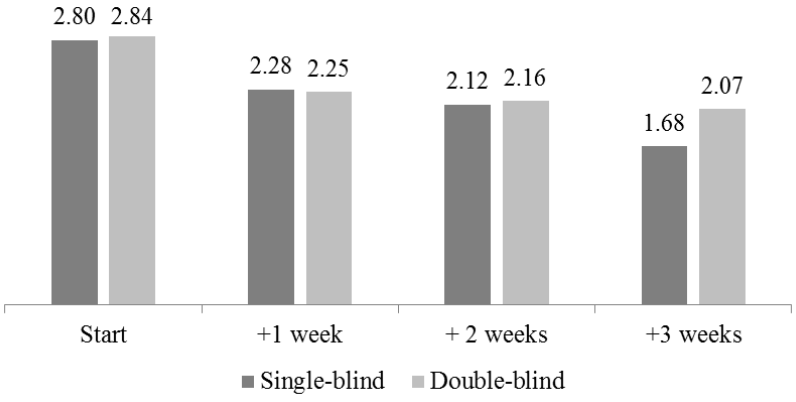

Figure 3: Average transfers three repetitions with fixed time intervals

In the treatment with varying intervals, the single-blind gifts are significantly higher than the double-blind gifts ($p < .05$ for the start and $p < .03$ for 2H, 2D, 2W, Mann-Whitney U-test). The difference between the single-blind and the double-blind treatments measured in euros is nearly the same throughout all four waves. This implies that the impact of the non-observability in the double-blind treatments is constant over the course of the experiment, which means that the image motivation for giving money to the recipient does not wash out but works in all repetitions. Furthermore, the transfers to the recipient decrease in both treatments (single-blind and double-blind) from wave to wave. The decay of the transfers is statistically significant at the five-percent level, except for the two middle waves (2H, 2D) and the last two waves in the double blind treatment (2D, 2W). From wave 1 to wave 4, we observe a reduction of 30 percent in the single-blind treatment and 42 percent in the double-blind treatment.

In the treatments with fixed time intervals, we do not find any significant differences between the single-blind and the double-blind treatments. In both treatments, we find a strong and statistically significant reduction of the transfers from the first to the second wave ($p < .01$ Wilcoxon signed-rank tests). After the second wave, however, the behavior is rather stable, particularly in the double-blind treatment. The small decreases in waves three and four are both insignificant ($p > .3$). In the single-blind treatment, the difference between the middle waves is also insignificant but the reduction from wave three to wave four is once again significant ($p < .03$). The overall decrease in the transfers amounts to 40 percent in the single-blind treatment and 27.1 percent in the double-blind treatment.

Two further observations are notable. First, the level of dictator transfers in the fixed time interval treatment differs substantially from all other treatments, including treatments with two waves. In the single-blind treatment dictator transfers is the lowest over all treatments and in the double-blind treatment, it is the highest of all treatments. Second, the transfer is stable after the first repetition. The overall decrease in the transfers is comparable to the experiments with varying time intervals but the dynamics of the behavior differs. Thus, we do not find an observability effect when the time span between repetitions is fixed. The difference-in-difference estimation displayed in Table 4 shows that the overall reduction in the transfers does not differ between the treatments with fixed and with varying time intervals.

We can only speculate about the reasons for both observations. It is possible that the fixation of the time intervals stabilizes behavior. Doing the same task at the same time of the week increases the chances of being in the same mindset. This could be because of the day of the week itself (e.g. Monday), or because of prior weekly activities, for example if the experiment is always after a math class. When the time interval is variable, prior activities are likely to differ between waves. For the same reason subjects could have chosen a relatively low (*high*) transfer in the single-blind (*double-blind*) treatment. All subjects agreed on the schedule of the experiments prior to their decisions. Therefore, subjects in the fixed time interval treatment knew about coming to the laboratory at the exact same time and weekday in the next weeks. It is possible; that this looming routine has decreased (*increased*) the transfers in the beginning of the single-blind (*double-blind*) treatment. While this is a speculative explanation, it is backed by the fact that the second lowest starting transfer in a single-blind treatment can be observed in the 2W-treatment, the only treatment were subjects did the same task at the same day and time of the week. The second highest starting transfer in a double-blind treatment can be found in the 2W-treatment as well.

Furthermore, the expectation of being in the laboratory regularly at the same time for four weeks obviously influenced the giving behavior from the beginning. Under these conditions, the question of whether one's own behavior was observable or not was obviously less important than in the other treatments. It is a pure speculation, but because of the regularity of the repetitions, it could be that the subjects expected at the latest from the second week that they would have the same task repeatedly to get. Under both single-blind and double-blind conditions, they probably made a decision about the regular giving in each week and not only about the current giving.

	Wave 1			Waves 2-4			
Outcome	var. interv.	fixed interv.	Diff	var. interv.	fixed interv.	Diff	diff-in-diff
Transfer	2.548	3.225	.677**	2.263	2.903	.640***	-.037
Std. error	.154	.244	.289	.101	.160	.189	.154

** p < .05; *** p < .01

Table 4: Difference-in-difference estimation: fixed and varying time intervals, means and standard error measured by linear regression.

In the next step we analyze the individual data to understand the reason for the reduction in the average transfers of the dictators. As an example, Figure 4 displays the individual transfers at the start (abscises) and in Wave 2 (ordinate) for the double blind 2-hour treatment (light dots) and the double blind 2-day treatment (dark dots)[8]. A point close to the 45°-line indicates an unchanged transfer by a particular dictator. Points below this line visualize a decrease in the transfer. Average data showed that the start values for the 2-day treatment are lower than they are for the 2-hour treatment. Individual data reveals that this is not due to more dictators giving nothing to the recipients (two in 2-hour treatment vs. three in 2-day treatment). Dictators in both treatments make a positive transfer at the start, but this transfer is less in the 2-day treatment. Therefore, the dark dots are more to left, than the light dots. Since the majority of transfers in both treatments is below the 45°-line, we observe a reduction of transfer giving in both treatments.

[8] Jitter-option is used for better visibility. Figures for other treatments are in Appendix E.

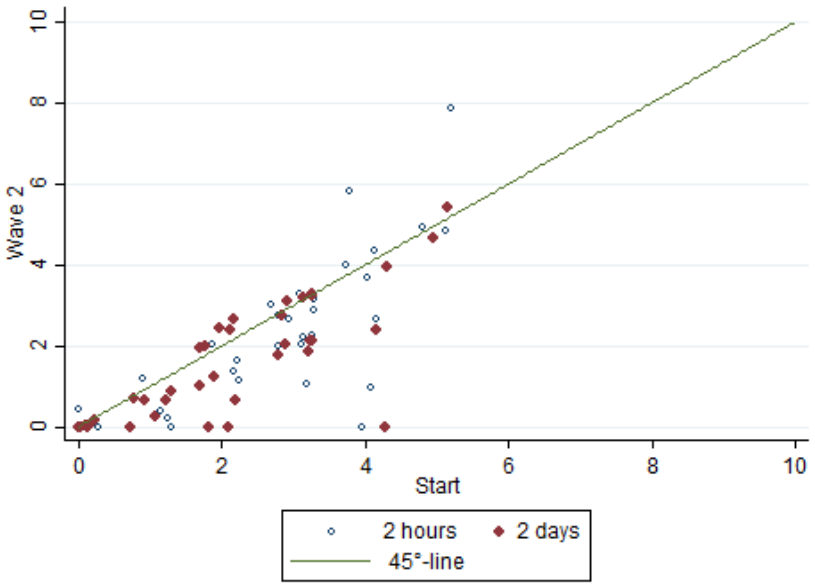

Figure 4: Individual transfer in Start and in Wave 2

With this knowledge and to obtain a deeper insight in the behavioral dynamics, we run a tobit regression with the following model:

$$Transfer = \begin{cases} \alpha + \beta 1D1 + \beta 2D2 + \gamma 1W1 + \gamma 3W3 + \gamma 4W4 & Transfer > 0 \\ 0 & Transfer = 0 \end{cases}$$

D_1 and D_2 are treatment variables. *Two waves* ($D_1 = 1$) indicates whether an observation is made in a treatment with two waves or with four waves ($D_1 = 0$), *Single* ($D_2 = 1$) stands for a single-blind treatment or a double-blind treatment ($D_2 = 0$). W_1 *to* W_4 are categorical variables. W_1, for example, becomes 1 if the decision made by the dictator is the first in a sequence (Wave 1), while W_3 indicates the third and W_4 the fourth experiment in a sequence. The second decision (W_2) serves as the baseline category. We use clustered error terms to control for multiple observations per subject. Table 5 shows the regression results in terms of log odds.

We do not find any significant impact of the *Two waves* variable in this model. This indicates that the likelihood of observing altruistic behavior in a particular situation does not depend on whether the dictator is scheduled for two or four waves. However, observability increases the likelihood of finding altruistic behavior, as indicated by an odds ratio greater than 1 for the *Single* variable, which is also significant at the five-percent level. This confirms our conjecture that the observability of behavior has a strong and constant impact on the altruistic behavior in all repetitions of the game.

Log likelihood:	-2003.79	Number of obs:	1030

Transfer	Coefficient	Std. Err.	t	P>|t|	[95% Conf. Interval]	
Two waves	.05	.15	.32	.75	-.25	.34
Single	.66***	.12	5.39	.00	.42	.91
Wave 1	.58***	.14	4.01	.00	.30	.86
Wave 3	-.22	.22	-.99	.33	-.65	.21
Wave 4	-.50***	.15	-2.25	.02	-.93	-.06
_cons	1.88	.15	12.6	.00	1.86	2.05

Table 5: Tobit regression: altruistic behavior, *** indicated significance at a 1% level, ** at a 5% level.

The coefficient of the first wave variable is also greater than 0 and highly significant. This confirms that the identical repetition of the dictator games changes the altruistic behavior of the dictators. The first time subjects are in a situation in which they either have to decide to behave altruistically or selfishly obviously differs from subsequent experiences. Thus, it seems fair to say that the altruism we observe in one-shot dictator experiments is the upper bound of altruism subjects are willing to exercise. The coefficients of waves three and four are also smaller than zero and the coefficient of wave 4 is significant. Compared to the second wave, the willingness to behave altruistically declines with each further repetition.

We run a second regression in order to investigate whether the length of the time span between the subsequent waves has any impact on the altruism shown by the dictators. Table 6 shows the results of the tobit regression using the time spans as independent categorical variables. *Interval_2nd* takes the value 0 if the second wave takes place after 2 hours; 1 ≙ 2 days; 2 ≙ 1 week and 3 ≙ 2 weeks; where "2 hours" is used as the baseline category. It turns out that the odds ratios are all close to one and not significant. Therefore, the time between two repetitions seems not to be of importance. While the gifts depend on the *number* of repetitions, they do not depend on the time that has gone by since the last dictator decision.

Log likelihood:	-742.77				Number of obs:	377
Transfer	Coefficient	Std. Err.	t	P>|t|	[95% Conf. Interval]	
Interval_2nd = 1	.18	.28	.06	.95	-.53	.57
Interval_2nd = 2	-.14	.30	-.46	.65	-.74	.46
Interval_2nd = 3	.07	.30	.24	.81	-.53	.68
_cons	2.23	.17	12.87	0.00	1.89	2.57

Table 6: Tobit regression: transfer and time span between repetitions

Summarizing, our results show that:

1. *The transfers to the recipients decline if the experiment is repeated.*

Our experiment not only confirms the results of Brosig-Koch et al. (2017) but also demonstrates that repetition effect is very robust. In all variants of the dictator game we used, which all differ from those used by Brosig-Koch et al., it turns out that identical repetitions of the experiment leads to a strong reduction of gifts to the recipient.

2. *The time span between repetitions is not important, but the number of repetitions is.*

This is a surprising result because it seems to be a plausible assumption that the effect of the experience of "giving in the past" is the stronger the shorter the time span between the actual decision and the experience.

3. *The image motivation for giving is constant over time and has no effect on the dynamics of altruistic behavior.*

Once again, our experiments have shown that the observability of giving increases the willingness to give money to the recipient. However, the important point here is that the repetition effect is independent of this image effect. Even if subjects can be observed, they reduce their gifts to the recipients. This is important for the moral self-licensing explanation we present in the next section. The hypothesis discussed there is that the reduction of giving is a rational reaction to a strong appreciation of repeated giving.

4. *Fixed time intervals of one week between repetitions reduce the observability effect.*

We do not have a comprehensive explanation for this observation. The effects of regular experimental episodes should be subject to further research.

The question remains: what is it that drives the behavioral dynamics we observe in all our treatments? We cannot rule out that also in our experiment a experimenter demand effect is at work which waters down in the course of the repetitions. This explanation for the repetition effect would be in line with the observation that the reduction of gifts depends on the number of repetitions.

An alternative explanation is the already mentioned moral self-licensing effect. In the next section, we refer to a simple model (see appendix D) that shows that moral self-licensing can be rationalized using relatively mild assumptions concerning the utility function that social approval for being altruistic is stronger when altruism is shown twice rather than as a single act. In a second experimental investigation employing the norm elicitation method of Krupka and Weber (2013), we investigate if this really is the case.

5 A moral self-licensing explanation of the dynamics of dictator behavior

5.1 Theory and experimental setting

Sachdeva et al. (2009) describe altruistic behavior as a "result from an internal balancing of moral self-worth and the cost inherent in altruistic behavior" (p. 523). Krupka and Weber (2013), who argue that altruistic giving can be interpreted as an expression of the willingness to pay for following a social norm, discuss a similar trade-off.

We adapt and combine these two lines of reasoning by assuming that the utility of a dictator in a dictator game experiment[9] at time t stems from his or her monetary payoff π_t and social appreciation A_t for sharing money with the recipient. In appendix D we introduce a simple model that shows that it might be is a rational choice to reduce the gift if the dictator experiment is repeated. Two assumptions are essential for this result. First, giving a gift of size *x* a second time leads to a higher approval than giving *x* the first time. Second, monetary income and social approval are perfect complements.

The first assumption necessary to rationalize the behavioral dynamics observed in our experiments can be tested experimentally. Krupka and Weber (2013) introduced an incentivized method for the elicitation of norms. We use this method in order to elicit the social norms relevant in the case of a repeated and a single shot dictator game

The underlying idea of Krupka and Weber (2013) is to elicit behavioral norms via an incentivized coordination game. Subjects are asked to state their belief with respect to what the majority of the other participants felt about the social appropriateness of certain behaviors. Since the true norm serves as a focal point for the coordination game, the majority decision uncovers the social norm actually at work. Subjects whose stated belief matches that of the majority are financially rewarded and thus the coordination game is properly incentivized.

Since we were interested in the behavioral norms for dictator behavior in the one-shot context and the repeated context, we invited two groups (A and B) of 50 subjects to take part in the elicitation experiments (between-subject design). All participants were recruited using hroot[10] (Bock et al. (2014)). The subjects were separately seated at simultaneously in a large lecture hall with more than 500 seats so that all data could be collected in a single session. Each subject

[9] The applicability of our model is not limited to decisions in the context of dictator game experiments. It can also be applied to any kind of social dilemma experiment in which a subject is forced to decide between payoff maximizing and altruistic behavior, such as the trust game (Berg et al. (1995)), public good game (Ledyard (1995)), mutual gift giving game (Güth et al. (2003)), etc.

[10] Hamburg registration and organization online tool

received written instructions (see Appendix D) in which a standard dictator experiment with $E = EUR10$ was described. The description of the base game was, in fact, identical to the instructions used for the single-blind treatments of the actual dictator games we also conducted for this study (see section 2, Appendix A). The instructions were numbered with an ID which served as a means to run the norm elicitation process anonymously. The subjects picked up the instructions by themselves in such a way that the ID was not observable for the experimenter.

The elicitation experiment was divided into two tasks. In the first task, subjects in group A were instructed to evaluate the social appropriateness of four different gifts G in the one-shot context (Table 7) and in group B ten different sequences of gifts G_t, G_{t+1} in the repeated context[11] respectively (Table 8). To do so, they could choose from four evaluations: 'very desirable' (++), 'somewhat desirable (+)', 'somewhat undesirable (-)' and 'very undesirable (--)'.

#	G in EUR	++	+	-	--
1	2				
2	4				
3	6				
4	8				

Table 7: Evaluated behaviors in the one-shot context

#	G_t in EUR	G_{t+1} in EUR	++	+	-	--
1	2	2				
2	4	4				
3	6	6				
4	8	8				
5	4	2				
6	6	2				
7	6	4				
8	8	2				
9	8	4				
10	8	6				

Table 8: Evaluated sequences of behaviors in the repeated context

After all the subjects had stated their belief about the assessments of the majority on a sheet of paper, one out of the four (ten) evaluations was randomly drawn to be payoff relevant and the

[11] The time interval between t and $t + 1$ was specified as 1 week

results for this particular evaluation were calculated on the spot. Those subjects who marked the assessment the majority had chosen received a payoff of EUR 10 whereas all other subjects received a show up fee of EUR 5 only.

The second task was identical in both treatments and required the subjects to state their belief as to whether the majority thought that a gift of G in a one-shot context deserved either the same, lower, or higher level of social appreciation as a sequence of G, G in a repeated context (Table 9). Once again, one of the four evaluations was randomly drawn for payoff calculation and those subjects who matched the majority's assessment received an additional payoff of EUR 2.

#	G in EUR of a player A in one-shot context	G_t, G_{t+1} in EUR of a player B in repeated context	Player A deserves more social appreciation	Both players deserve the same social appreciation	Player B deserves more social appreciation
1	2	2, 2			
2	4	4, 4			
3	6	6, 6			
4	8	8, 8			

Table 9: Evaluated cross-comparisons of dictator behaviors in both contexts

5.2 Results

Table 10 summarizes the results from the first task of the norm elicitation experiment in the one-shot context ($N = 50$). It includes a social appropriateness score A for each G that is based on all answers given in that treatment. We follow Krupka and Weber (2013), who aggregate the evaluations over the four possible steps by assigning numbers to the four evaluations (1 for each ++; 1/3 for each +; -1/3 for each -; -1 for each --). The total sum is then divided by$N$ in order to get a mean evaluation of social appropriateness for each G.

#	G in EUR	++	+	-	--	N	$A(G)$
1	2	4	5	18	23	50	-.47
2	4	14	29	7	0	50	.43
3	6	13	27	9	1	50	.36
4	8	13	7	17	13	50	-.07

Table 10: Results norm elicitation experiment task 1, one-shot context

Interpreting the appropriateness score as a proxy for the social appreciation available to the dictator, the results reveal that a dictator could actually do too much good. Social appreciation increases for small levels of$G$, but decreases for higher levels of G as more and more subjects find very high gifts socially inappropriate. While this is a notable result in itself, we are generally more interested in a cross-comparison between the social appropriateness scores of G in the one-shot context and a sequence of G, G in the repeated context.

#	G_t in EUR	G_{t+1} in EUR	++	+	-	--	N	$A(G)$
1	2	2	8	3	14	25	50	-.41
2	4	4	11	29	10	0	50	.35
3	6	6	22	18	8	2	50	.47
4	8	8	18	9	9	14	50	.08
5	4	2	6	8	24	12	50	-.23
6	6	2	6	7	30	7	50	-.17
7	6	4	18	30	2	0	50	.55
8	8	2	9	11	22	8	50	-.05
9	8	4	9	27	13	1	50	.25
10	8	6	19	12	14	4	49[12]	.29

Table 11: Results norm elicitation experiment task 1, repeated context

For this purpose, the first four evaluations shown in Table 11 are particularly important to us. While the results are generally similar to those of the one-shot context, there is a small and not significant[13] upward shift in social appropriateness for high levels of G, e.g. EUR 6 and EUR 8. This shift is graphically depicted in Figure 5. Gifts of more than 50% of the endowment (super-fair offers) are extremely rare. Therefore, the finding that the dashed line is above the solid line in this area is not strong evidence for a higher approval of repeated gifts.

[12] One subject accidentally gave two answers for this particular evaluation, so it is not included in the data.
[13] *p = 0.3 and 0.2 Chi Square test.*

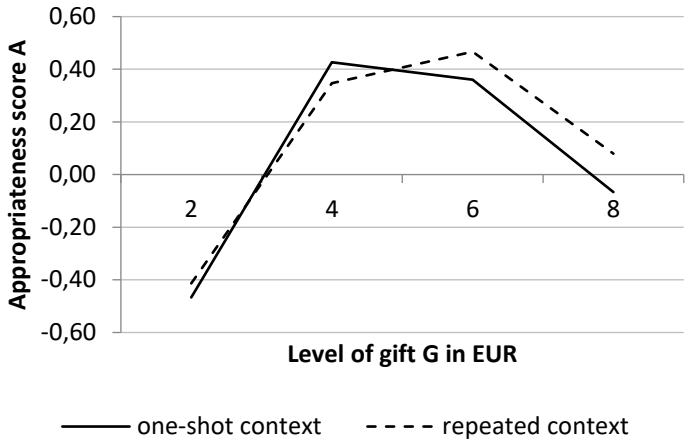

Figure 4: Social appropriateness scores one-shot context vs. repeated context

The last six observations in table 11 are also interesting. The combination (6, 4) was scored highest and, in particular, much higher than (6, 2) and (8, 2). This shows that it is appreciated if someone who has already done his duty in an acceptable way (by transferring 6 in the first wave) continues to be nice in the second wave by transferring 4. But the total transfer in the combination (6, 4) is higher than in (6, 2) and this may be responsible for the higher approval. Once again, super-fair offers of much more than 50% of the endowment were not appreciated and this explains why (8, 2) is less appreciated than (6, 4).

The results from the second task of our norm elicitation experiment confirms that there is only weak evidence for the hypothesis that giving the same amount a second time leads to higher social approval. The combined data from both groups of subjects show that a significant number of participants believe that a dictator giving the same amount to a recipient twice deserves a higher level of social appreciation than a dictator who only does it once in the one-shot context (see Table 12). Once again, this holds only for the super-fair offers of 6 and 8 respectively.

#	G in EUR of a player A in one-shot context	G_t, G_{t+1} in EUR of a player B in repeated context	Player A deserves more social appreciation	Both players deserve the same social appreciation	Player B deserves more social appreciation
1	2	2, 2	21	76	4
2	4	4, 4	9	83	9
3	6	6, 6	8	54	39
4	8	8, 8	7	40	54

Table 12: Results task 2 norm elicitation experiment (combined $N = 101$)[14]

[14] 51 subjects took part in the one-shot treatment, but one of them failed to fill out the task 1 data sheet correctly. That subject's data is not included in Table 10 but it is in Table 12; thus $N = 101$ for task 2.

Obviously, most subjects also believe that social appreciation should be the same in both the one-shot context and the repeated context, as long as the gifts are between 2 and 5 Euros. Very few subjects think that the one-shot context dictator deserves more social appreciation, therefore possibly increasing their gift in a repeated dictator game. In summary, the results of our norm elicitation experiment only weakly support the theoretical explanation suggested in section 5.1.

6 Conclusion

In the first experimental part of our investigation, we contributed to the small number of studies that have looked at repeated decisions between more or less selfish behavior. We could confirm the observation of Brosig-Koch et al. (2017) that the willingness to forego personal payoff for the sake of others decreases over time. Moreover, we show that this is a robust finding because we varied the game used in the experiment; specifically, the time span between repetitions, the number of repetitions and the regularity of the repetitions. The decay of gifts to the recipient could be observed in all variants of the experiment. We therefore believe that this "repetition effect" is a promising candidate to become a stylized fact of dictator game experiments.

A simple explanation for this behavioral dynamic would be that people need time and repetitions to learn their (selfish) best reply. We do not believe that this is a plausible explanation, because most of the games in which this dynamic has been observed are so simple that there is little room for learning effects. This is particularly true for the dictator game we employed in this study.

A second way to easily explain unstable behavior in repeated situations is that the preferences of subjects change over time. Again, this is not a convincing explanation since it is not clear *why* preferences should change and why they all change in the same direction. The observation that the degree of social behavior decreases in *all* our treatments is also a strong argument against the suspicion that events happening outside the laboratory between the different waves are responsible for the behavioral change.

As already mentioned by Brosig-Koch et al. (2017), two plausible explanations remain. Firstly, a diminishing experimenter demand effect and, secondly, a moral self-licensing effect. In the second part of this paper, we tested an explanation for the latter within the rational choice model. Given that subjects perceive the social approval they reap from doing something good in such a way that simply doing it more than once gains higher approval than doing it only once, this could lead to the rational decision to reduce the level of altruistic giving. If the relative

price of social approval declines, the necessary amount of approval to sustain this self-image can be obtained with a lower sacrifice in terms of personal income.

The results of our norm elicitation experiments show overwhelmingly weak evidence for such reasoning. Only differences between the social approval of one and two super-fair gifts, ($G = EUR6$ and of$G = EUR8$) which usually do not occur in dictator game experiments, go in the right direction but were not significant. Our conclusion is that a moral self-licensing effect, if any, is not the result of rational choice. It is obvious, however, that the second explanation for the decline of gifts - the experimenter demand effect - is the more likely one based on our findings.

Our findings have some implications for the interpretation of experimental results concerning social preferences. When not repeated experiments try to explain behavior in situations that keep recurring, it seems fair to say that the amount of altruism observed in laboratories represents the upper bound of altruism – repetitions tend to result in less altruism. This may be one reason why – as DellaVigna (2009) points out – theories and experimental findings seem to over-predict altruistic behavior.

An interesting question is whether the observation we have made in the repeated dictator game experiments can also be applied to other games in which social preferences play a role. Examples of this could be the trust game or the public good game. The main difference to the dictator game is that the players in these games are in a strategic interaction. It could be that the experimental demand effect in the dictator game experiment is particularly pronounced. This would suggest that the repetition effect is less pronounced in other games. Whether this is actually the case must be left to future research.

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Appendix A: Instructions single-blind treatments

The following instructions are the English translation of the original German instructions. The original instructions are available from the corresponding author.

These instructions were given to all subjects taking part in any of the single-blind treatments.

- You will now take part in an experiment within the context of experimental economics. In this experiment, you can earn money that will be paid out to you in cash at the end of the experiment.
- You and another subject are part of the following decision situation. The other subject's identity will not be revealed to you at any point in time. Likewise, your identity will not revealed to the other subject. Thus, the interaction is completely anonymous.
- You have been endowed with EUR 10, split up into 10 EUR 1 coins. You are asked to divide this amount of money between yourself and the other subject. Please decide on the amount of money (if any) that you want to give to the other subject by placing the equivalent number of coins into the envelope in front of you (please do not seal the envelope just yet, thank you!)

  Additional information on the other subject:

  - At this moment the other subject is sitting in the adjacent room. Upon entering the laboratory, you were shown a live video transmission from that room showing the other participants in this experiment. To ensure anonymity, we deliberately chose a low resolution.
  - The other subject was – just like you – invited randomly from the general MaXLab subject pool.
  - The other subject will take part in this experiment today for the first and only time.
  - The other subject will only receive the money that you give to him or her. There is no show-up fee or any other monetary compensation.
  - The other subject does not make a decision in the context of this experiment.

- Procedure of this experiment
    - Please wait inside your booth until you are picked up by an experimenter
    - Please hand over the envelope to the experimenter and sign a receipt for the amount of money you want to take home.
    - You will then leave the laboratory on your own without encountering any other participant of this experiment.
    - The experimenter will take your envelope and the envelopes of other subjects that had the same role as yourself and randomly distribute them among the participants in the adjacent room.

Appendix B: Instructions double-blind treatments

The following instructions are the English translation of the original German instructions. The original instructions are available from the corresponding author.

The instructions in the double-blind treatments slightly differed with respect to the number of repetitions and the time intervals between two repetitions. Differences in the instructions are highlighted in **bold print**.

- You will now take part in an experiment within the context of experimental economics. In this experiment, you can earn money that will be paid out to you in cash at the end of the experiment.
- The experiment **consists of two parts (2HDB, 2DDB, 2WDB) / consists of four parts (4VDB) / has duration of four weeks (4FDB)**. The peculiarities that result from this experimental setup are explained in detail in the following instructions. Please read them carefully. Thank you!
- You and another subject are part of the following decision situation. The other subject's identity will not be revealed to you at any point in time. Likewise, your identity will not revealed to the other subject. Thus, the interaction is completely anonymous.
- You have been endowed with EUR 10, split up into 10 EUR 1 coins. You are asked to divide this amount of money between yourself and the other subject. Please decide on the amount of money (if any) that you want to give to the other subject by placing the equivalent number of coins into the envelope in front of you. The experimenter will then take your envelope and the envelopes of other subjects that had the same role as you and randomly distribute them among the participants in the adjacent room.
- Upon entering the laboratory, you were asked to randomly choose an envelope containing strips of paper on which the name of a city are printed. The name of the city is your identity in the context of this experiment. Please treat this identity confidentially. Nobody but you should know the name of the city you drew. Please add ONE of the strips to the envelope with the money that you want to give to the other subject. Please keep the other strips and bring them with you for the **second experiment (2HDB, 2DDB, 2WDB) / the other experiments (4VDB, 4FDB).** The city identity allows us to monitor

your individual behavior without knowing your true identity. Thus, the decisions you make in this experiment are completely anonymous and not even the experimenter will know what you have decided. To ensure anonymity, we have set up a live video transmission that you can see on the screen in front of you. It allows you to monitor the process of us collecting all envelopes, so you can be sure that we do not open your envelope immediately after collection.

- Additional information on the other subject:
  - At this moment the other subject is sitting in the adjacent room. Upon entering the laboratory, you were shown a live video transmission from that room showing other participants in this experiment. To ensure anonymity, we deliberately chose a low resolution.
  - The other subject was – just like you – invited randomly from the general MaXLab subject pool.
  - The other subject will take part in this experiment today for the first and only time.
  - The other subject will only receive the money that you give to him or her. There is no show-up fee or any other monetary compensation.
  - The other subject does not make a decision in the context of this experiment.

Appendix C: Instructions norm elicitation experiment

The following instructions are the English translation of the original German instructions. The original instructions are available from the corresponding author.

While PAGE 1, PAGE 2 and Page 4 of the instructions below are identical for both norm elicitation experiments that we conducted, PAGE 3 (the data sheet) differs. Therefore both versions of the data sheets are included in this appendix. Subjects received PAGE 4 after they completed PAGE 3.

PAGE 1: INSTRUCTIONS ID: 1

Please read these instructions carefully. If you have any questions, please raise your hand and wait for an experimenter to come to your seat.

You will receive a show-up fee of EUR 5 for participating in this experiment. You might receive an additional monetary compensation depending on the decisions that you and other participants make in the context of this experiment.

Note: Please do not communicate with other subjects during this experiment verbally or in any other way. Subjects not obeying this rule will be excluded from the experiment and will not receive a payment. Thank you!

50 subjects will be taking part in this experiment. All of them are sitting in this lecture hall at the same time. Your task is to indicate what you estimate or believe the majority of the other subjects think is a "socially appropriate" or "socially desirable" behavior in a certain decision situation. If your estimation is identical with the estimation of the majority of other subjects, you will receive an additional EUR 5 on top of the show-up fee, thus EUR 10 in total. If not, you will only receive the show-up fee that every participant will be paid in any case.

The situation in question is given by an experiment that other subjects took part in or will take part in at a different point in time in Magdeburg. You'll find the description of this experiment on the next page. Questions you might have will be answered at your seat. Please raise your hand if you have any.

On the third page you will find the actual questions you are asked to answer. To answer the questions, just mark one of the given response options. This is not about what **you** personally think is the appropriate behavior but what the majority of the other subjects think.

Procedure of this experiment:

1. Please read the description of the base game on page 2 carefully.
2. Answer the question on page 3 (data sheet)
3. Separate page 3 from these instructions, fold it once and hand it to an experimenter when asked to do so.
4. The data sheets will be evaluated immediately after collection.
5. You will find an ID on each page in the top right corner. After the evaluation of the data sheets, we will list all IDs and the corresponding payment. Please line up at the payment desk when asked to do so.

PAGE 2: DESCRIPTION OF THE BASE GAME ID:1

The following box includes instructions of an experiment that other subjects took part in or will take part in at a different point in time in Magdeburg.

Please read these instructions carefully. Although you will not take part in the experiment described in these instructions, it is important that you are familiar with them.

- You will now take part in an experiment within the context of experimental economics. In this experiment, you can earn money that will be paid out to you in cash at the end of the experiment.
- You and another subject are part of the following decision situation. The other subject's identity will not be revealed to you at any point in time. Likewise, your identity will not revealed to the other subject. Thus, the interaction is completely anonymous.
- You have been endowed with EUR 10, split up into 10 EUR 1 coins. You are asked to divide this amount of money between yourself and the other subject. Please decide on the amount of money (if any) that you want to give to the other subject by placing the equivalent number of coins into the envelope in front of you (please do not seal the envelope just yet, thank you!)

  Additional information on the other subject:

  - At this moment the other subject is sitting in the adjacent room. Upon entering the laboratory, you were shown a live video transmission from that room showing other participants in this experiment. To ensure anonymity, we deliberately chose a low resolution.
  - The other subject was – just like you – invited randomly from the general MaXLab subject pool.
  - The other subject will take part in this experiment today for the first and only time.
  - The other subject will only receive the money that you give to him or her. There is no show-up fee or any other monetary compensation.
  - The other subject does not make a decision in the context of this experiment.

PAGE 3: DATA SHEET **(One Shot)** ID:1

The experiment described on page 2 is conducted in a laboratory.

The following table consists of 4 different possibilities on how a player could behave in the two experiments. You are asked to indicate for each possibility, what you believe a majority of your co-participants thinks of the "appropriateness" or "social desirability" of the different behaviors. Options range between "very desirable/very appropriate" to "somewhat desirable/somewhat appropriate" to "somewhat undesirable/inappropriate" to "very undesirable/very inappropriate".

Note: Only <u>one</u> of the 5 possibilities is chosen for evaluation. You will receive the additional EUR 5 if you match the choice made by the majority of participants in the randomly drawn row.

Behavior of the subject dividing the money	very desirable/ very appropriate	somewhat desirable/ somewhat appropriate	somewhat undesirable/ somewhat inappropriate	very undesirable/ very inappropriate
Amount given: 2 EUR				
Amount given: 4 EUR				
Amount given: 6 EUR				
Amount given: 8 EUR				

Please make ONE mark in each ROW!

PAGE 3: DATA SHEET **(repeated)** ID:1

The experiment described on page 2 is conducted in a laboratory. **One week** after the experiment, the players that were asked to divide the EUR 10 take part in an identical repetition of the experiment, though they are matched with freshly recruited new partners who only take part in the experiment once.

The following table consists of 10 different possibilities on how a player could behave in the two experiments. In the first column you find the behavior in the first experiment and in the second column you find the behavior in the second experiment. You are asked to indicate for each possibility, what you believe a majority of your co-participants thinks of the "appropriateness" or "social desirability" of the behavior in the second experiment. Options range between "very desirable/very appropriate" to "somewhat desirable/somewhat appropriate" to "somewhat undesirable/inappropriate" to "very undesirable/very inappropriate".

Note: Only <u>one</u> of the 10 possibilities is chosen for evaluation. You will receive the additional EUR 5 if you match the choice made by the majority of participants in the randomly drawn row.

Amount given in the first experiment	Amount given in the second experiment	very desirable/ very appropriate	somewhat desirable/ somewhat appropriate	somewhat undesirable/ somewhat inappropriate	very undesirable/ very inappropriate
2 EUR	2 EUR				
4 EUR	2 EUR				
	4 EUR				
6 EUR	2 EUR				
	4 EUR				
	6 EUR				
8 EUR	2 EUR				
	4 EUR				
	6 EUR				
	8 EUR				

Please make ONE mark in each ROW!

PAGE 4: ADDITIONAL SHEET ID (Copy from page 1/2) _____

Now you have the opportunity to earn 2 extra Euros. Therefore, answer the all questions at the bottom of this page. One of the 4 questions is chosen for evaluation. You will receive the additional EUR 2 if you match the choice made by the majority of participants in the randomly drawn row. Background: There are two groups: group A and group B. All players from both groups play the base game, which is described on page 2. After playing the game, the experiment ends for all players from group A. One week after the experiment, the players from group B take part in an identical repetition of the experiment, though they are matched with freshly recruited new partners who only take part in the experiment once.

The following table consists of 4 different possibilities on how a player of group A and a player of group B could behave in the experiments. In the first column you find the behavior of the player from group A and in the second column you find the behavior of the player from group B. You are asked to indicate for each possibility, what you believe a majority of your co-participants thinks of the "social desirability" of the two behaviors.

Behavior of a Player from group A	Behavior of a Player from group b	The behavior of the player from group A is socially more desirable	The behavior of both players is equally desirable	The behavior of the player from group B is socially more desirable
Amount given in the experiment: 2 EUR	Amount given in the 1st experiment: 2 EUR ---------- -- Amount given in the 2nd experiment: 2 EUR			
Amount given in the experiment: 4 EUR	Amount given in the 1st experiment: 4 EUR ---------- -- Amount given in the 2nd experiment: 4 EUR			
Amount given in the experiment: 6 EUR	Amount given in the 1st experiment: 6 EUR ---------- -- Amount given in the 2nd experiment: 6 EUR			
Amount given in the experiment: 8 EUR	Amount given in the 1st experiment: 8 EUR ---------- -- Amount given in the 2nd experiment: 8 EUR			

Please make ONE mark in each ROW!

Appendix D: A simple model

Let E_t denote the monetary endowment given to the dictator and G_t the gift made to the recipient by the dictator. Then $\pi_t = E_t - G_t$ is the monetary payoff of a dictator at time *t*. Furthermore, we assume $A_t = A_t(G_t)$, so the level of social appreciation A_t is a function of the gift made to the recipient. It follows that utility is solely dependent on G_t:

$$U_t = U_t(\pi_t, A_t) = U\big(\pi_t(G_t), A_t(G_t)\big) = U_t(G_t).$$

We further assume that the dictator treats monetary payoff and social appreciation as perfect complements, thus:

$$U_t = U_t(\pi_t, A_t) \ = min\{\pi_t(G_t); A_t(G_t)\}^{15}$$

It is reasonable to assume that there is an interval $[0, \bar{G}]$ and $\partial A/\partial G > 0$ for $G \in [0, \bar{G}]$. At least for low levels of G, social appreciation increases as gift size increases. Given $A_t(G_t)$, there is a unique G_t^* that maximizes U_t. This utility maximizing gift level can be found graphically at the intersection of $\pi_t(G_t)$ and $A_t(G_t)$ (see **Fehler! Verweisquelle konnte nicht gefunden werden.**).

To allow for changes in other-regarding behavior over time, thus $G_t^* \neq G_{t+1}^*$, we assume that the level of social appreciation available to the dictator at $t+1$ is different to that available at t. Giving the same level of gift G^0 at *t+1* may be accompanied by a higher (or lower) social approval than giving G^0 in *t*. To illustrate the idea behind this assumption, imagine students living in a shared flat. Having or not having done the dishes the day before will arguably make a difference to the moral appropriateness of altruistic or selfish behavior, i.e. whether a flatmate does the dishes today or not. This change in judgment is reflected by changes in $A(G)$ in our model. Having done one's duty the day before reduces the (social) pressure to do it again today. Thus, *if* someone does the dishes twice, this is even more approvable.[16]

Formally, this change in social approval being linked to a decision through repetition is expressed by a shift of $A(G)$ as demonstrated in **Fehler! Verweisquelle konnte nicht gefunden werden.**. In this figure, the utility function is graphically represented by the lower envelope of

[15] Obviously, the behavior of completely selfish dictators cannot be rationalized with this kind of utility function. Note though that we are interested in the dynamics of other-regarding behavior only, so our focus is purely on subjects who exhibit at least some degree of other-regarding behavior. On the issue of heterogeneity in people's preferences see, for example, Fischbacher and Gächter (2010).

[16] Conversely, repeated refusal to do the dishes might make the second refusal even less socially appropriate than the first one because with the initial refusal to fulfill one's duty comes social pressure to set the record straight at the next opportunity.

$\pi_t(G_t)$ and $A_t(G_t)$; thus, the utility maximizing gift level G_t^* can be found at the intersection of $A_t(G_t)$ and $\pi_t(G_t)$. An upward shift of $A_t(G_t)$ causes a decrease of the optimal gift at *t+1* as the intersection of $A_{t+1}(G_{t+1})$ and $\pi_{t+1}(G_{t+1})$ is necessarily to the left of G_t^* for $\pi_{t+1}(G_{t+1}) = \pi_t(G_t)$.

Note that the change in $A(G)$ over time is not assumed to be caused by a particular choice of G at a former point in time, but by the repetition of the decision itself. A subject at $t+1$ recognizes that he or she was in the same decision situation before and therefore forms a new perception of what behavior is socially appropriate, with this belief perhaps deviating from what he or she deemed socially adequate at t, when the decision was made in a one-shot context.[17]

If there exists a norm that rates something that has been done repeatedly differently from a single event, then a subject exhibiting stable altruistic behavior essentially foregoes his or her right to act more self-servingly and thus deserves a higher degree of social appreciation for that same level of altruistic behavior compared to the one-shot context. Such a shift of $A(G)$ implies a change in relative prices between π and A, which causes the dictator to become more selfish at $t+1$.

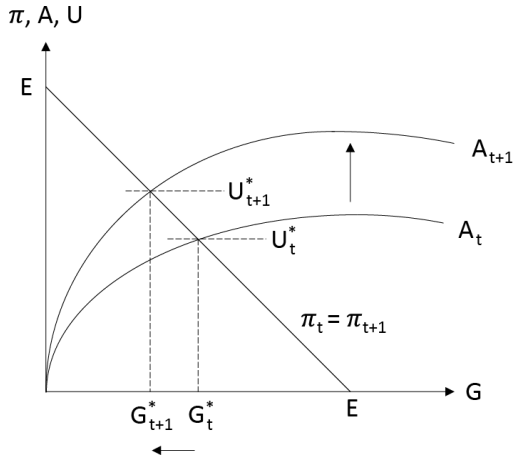


Figure 5: Change towards more selfish behavior caused by upward shift of $A(G)$

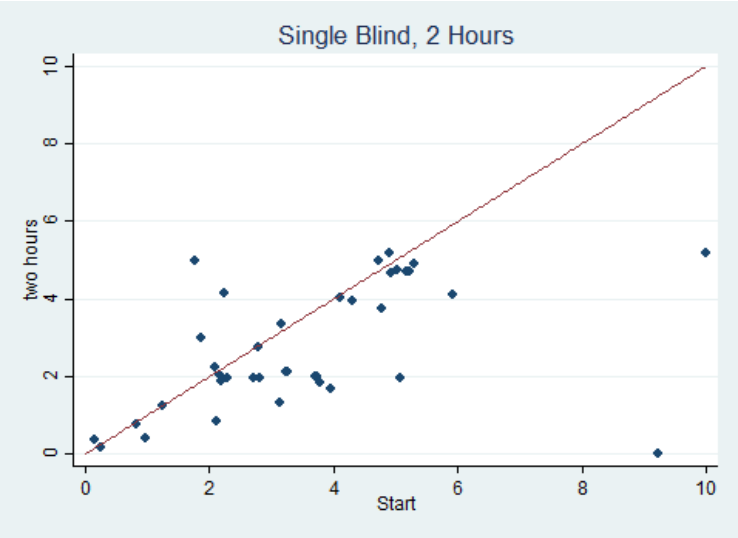
Our theoretical framework allows us to rationalize changes in other-regarding behavior by using the assumption of changes in the social appreciation function. In the example illustrated above, a change towards more selfish behavior is the consequence of *more* social appreciation available to the dictator. Note though that the direction of the effect would reverse if, for example, a dictator felt that social appreciation for a selfish decision at the second time of asking is

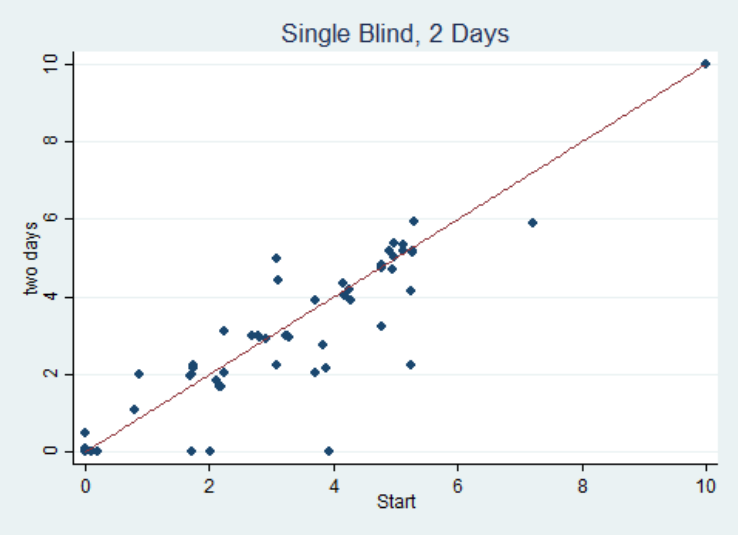
[17] We do not assume any other changes in the decision environment, so $E_t = E_{t+1}$ and more importantly $U_t = U_{t+1}$. Any change in other-regarding behavior is therefore assumed to be a consequence of a change in $A(G)$.

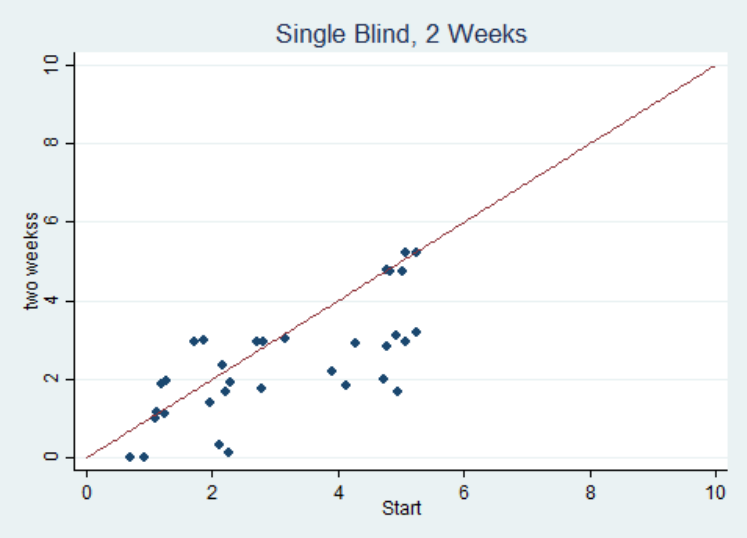
even *lower* than it was for the same selfish decision in the one-shot context. In such a scenario, $A_{t+1}(G_{t+1}^0) < A_t(G_t^0)$ might hold for sufficiently low levels of G and a dictator yearning for social appreciation might increase the gift accordingly. Furthermore, stable other-regarding behavior is also not ruled out by our framework. If a subject does not perceive a subjective change in regard to the available social appreciation, he or she might exhibit completely stable behavior over time.

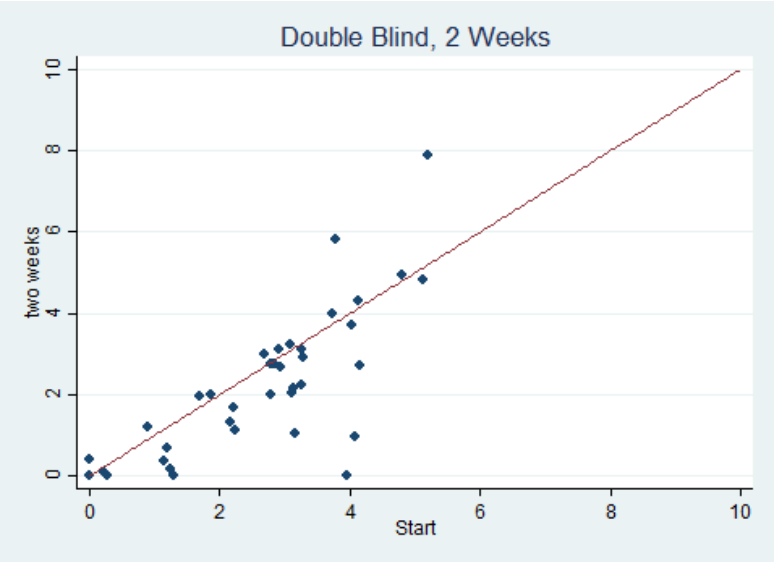
Appendix E: Individual Data

We compare dictator transfer at the start with the giving in the first repetition.








Double Blind, 2 Weeks
two weeks
Start
0
2
4
6
8
10