On the Effects of Transparency on Labour Supply in Redistribution Systems

Marina Chugunova, Andreas Nicklisch & Kai-Uwe Schnapp

This paper analyzes whether there is a strategic incentive for redistribution systems to reveal information on the productivity of transfer recipients. The transparency of productivity is potentially able to stimulate both the work effort of taxpayers and transfer recipients. Experimental results do not support this claim. Rather than stimulating effort provision, transparency creates a negative reference point for many taxpayers. Thus, monitoring is negative for the overall productivity suggesting that there is an institutional advantage of deliberate ignorance for redistribution systems. Choosing not to reveal information yields superior outcomes and serves in the best interest of all members of the society.

A. Introduction*

In 2016, Christoph Engel and Ralph Hertwig wrote their seminal paper on deliberate ignorance, the decision to shrink from knowing information as this creates a strategic advantage. Their paper "Homo ignorans: Deliberately choosing not to know" represents a milestone in our understanding of human behavior as it departs from the traditional assumption that curiosity is a dominant human characteristic. They present examples of deliberate ignorance from a wide range of domains, demonstrating that not knowing sometimes yields superior outcomes.

A large body of literature refers to the approach by Hertwig and Engel (2016). Investors, for example, avoid looking at their financial portfolios when the stock market is down (Andries and Haddad, 2020; Sicherman et al., 2016). Consumers do not check whether the consumption of cheap products causes a negative externality to some third parties (Bartling et al., 2015). Kozyreva et al. (2020) argue that mental habits that help to

* Financial supports by the German Research Foundation (DFG) for Nicklisch (Ni 1610/1-1) and Schnapp (Schn 1282/4-1) are gratefully acknowledged.

avoid being tempted by attention-grabbing content are important components of modern digital information literacy. Finally, De Bruin et al. (2021) investigate the degree of news avoidance during the first months of the Covid-19 pandemic.

In this study, we discuss another facet of strategic ignorance, the advantage of a non-transparent institutional setting that support poor members of the society by means of redistribution mechanisms. That is, government institutions organize a transfer of income between rich or advantaged members of society and poor or disadvantaged members of society. The rich members provide the poor sufficient financial resources so that the transfer guarantees the sufficient income to participate in the normal economic, social, and political life of a society.

However, the willingness to contribute to the redistribution system for those who give depends on everybody's effort contributing fairly and according to his or her ability. In their study on redistribution in Denmark and the USA, Aarøe and Petersen (2014) show that the support of those who give depends on how much information on the recipients is available: if recipients are judged as being lazy, less support is granted in comparison to the situation in which the recipient is going through a streak of bad luck.

Increasing the transparency on the recipients' performance prior to the redistribution (i.e., monitoring the recipients' effort) would allow those who give to reciprocate: they would provide little support to those who are considered to be lazy and would provide a lot of support to those who are considered to be unlucky. Yet, a redistribution system seeks to guarantee a constant flow of transfers to all in need. Therefore, increasing the transparency could potentially lead to decreasing efficiency of the redistribution system.

To test this claim, we run a series of real-effort laboratory experiments with the provision of information on the performance of transfer recipients by means of unilateral monitoring. Each experiment consists of three phases with the game being played by pairs of players. In each phase participants solve math tasks for 30 minutes. Participants are paid for each correctly solved task and are free to choose when to work and when to be idle.

In the first phase all participants add or subtract pairs of three-digit numbers. In the second phase one player of each pair is randomly assigned high productivity while the other is assigned low productivity. Both players solve math exercises again for 30 minutes. The high productivity player adds or subtracts three-digit numbers as before, while the low productivity player adds or subtracts five-digit numbers. The third phase is equivalent to the first for both players in a group but players earn twice the amount of money for every correctly solved task. Only players who earned an income net of transfers beyond a threshold level in the second phase are allowed to take part in the high-income third phase.

Our baseline treatment (BA) follows the description above and has no interaction between players. The transfer treatment (TR) introduces a thirty percent income tax on the highly productive players. At the end of the second phase the tax revenue is transferred to the low productivity player within a group. The transfer increases the likelihood that disadvantaged players take part in the high-income third phase of the experiment despite their randomly assigned low productivity in the second phase.

The feedback treatment (FB) is identical to TR but introduces additionally unilateral monitoring. The highly productive players (taxpayers) can constantly observe the performance of the low productivity players (transfer recipients). However, there is no feedback in the opposite direction (from taxpayers to recipients), since we want to isolate the effect of this unilateral transparency on the overall productivity and avoid an interaction between taxpayers and transfer recipients. As such, in TR and FB transfers systematically target disadvantaged peers. What varies endogenously is recipients' own contribution to meeting the subsistence income threshold and to qualifying for the third phase.

We analyze the effects of transparency (i.e., provision of information) on the productivity for both taxpayers and transfer recipients. In our experimental societies players have heterogeneous productivity, but a homogeneous minimum income constraint (a subsistence income). In BA there is no redistribution, in TR redistribution is introduced, and, finally, in FB redistribution is coupled with the unilateral monitoring. Our results indicate that ignorance is a blessing. In TR both players in a pair increase their productivity significantly as compared to BA. The introduction of monitoring in FB does not, however, affect the productivity on the side of the taxpayers, and significantly reduces the productivity of transfer recipients. Hence, it seems that recipients try to stimulate large transfers by showing little productivity. Consequently, for our experimental economy as a whole, monitoring harms rather than promotes the total amount of labour supplied and thus efficiency.

These results relate to a small number of studies on circumstances in which transparency backfires and – against the traditional view in public economics – is not positive and desirable, but leads to lower production in a society. For instance, public monitoring of decision procedures in developing countries is generally considered to be usefull against corruption, but provides important to identify those who are clues recipients of bribes (Kolstad and Wiig, 2009; Lindstedt and Naurin, 2010). Likewise, if people have the opportunity to embezzle part of a common resource while others cannot sanction this behaviour, full transparency increases rather than prevents embezzlement (Khadjavi et al., 2017).

The rest of the paper proceeds as follows: In Section 2 we present our experimental design and procedure, in Section 3 we develop both our theoretical analysis and hypotheses, in Section 4 we present our results, and finally in Section 5 we draw conclusions based on our findings.

B. The Experimental Design

I. The Game

In our real-effort experiment, we asked participants to conduct simple computations and did so in three phases of 30 minutes each. In Phase I we measure the players' motivation and ability to perform the computing task, awarded them an income depending on how well they did, and advanced all participants to Phase II. In Phase II we applied our treatment but only those that passed an income threshold were advanced to Phase III. Phase III is a high income phase. Players need to reach Phase III in order to earn a decent amount of money in the experiment.

In all three phases all players are tasked with computing sums and differences without technical support, however, the concrete assignments, differ during the phases. In Phases I and III players compute the sums or the differences of two three-digit numbers. If a task is solved correctly, a new task appears on the screen, if not, the same task is presented again. Per correctly solved task players earn a piece rate ϕ. In all phases, the number of tasks is unlimited and thus players are free to decide how hard to work. By opting for a tedious and repetitive task across an extended period of time, we minimize the joy and intrinsic motivation of work and hence increase the importance of monetary incentives. To avoid an experimenter demand effect, players are not offered an alternative leisure activity.[1]

1 Research on productivity shows that not performing a dull task that requires a lot of concentration can also be considered as leisure as it saves cognitive bandwidth (e.g, Kool et al., 2010).

The design of Phase II, the treatment phase, differs in two respects: while players are still paired in groups of two each player is assigned a different computing task. And while one player solves the same task as in Phase I, that is, computing the sum or the difference of two three-digit numbers, the other player has to solve a more difficult and thus more time-consuming task, that is, computing the sum or the difference of two five-digit numbers. Irrespective of the task's difficulty, both types of players receive the same piece rate ϕ for a completed task.

As explained to the players between Phase I and II, advancing from Phase II to Phase III requires for all players to meet an an exogenously imposed income constraint. If a player earns less than S Euros in Phase II, she will not be advanced to the high income Phase III but is still required to wait in the laboratory until the other players have completed Phase III. We choose $\phi = 0.05$ Euros, $S = 3.20$ Euros. Both parameters are calibrated such that on average the threshold is slightly too high to be reached by players assigned the five-digit task, but easy to reach for players assigned with computing three-digit numbers.

In treatment conditions TR and FB, players who solve three-digit tasks in Phase II have to pay a tax rate $\tau = 0.3$ of their income. The tax revenue is transferred to the player of the other type in their group. We therefore refer to these players as "taxpayer" (T) and to the other players as "recipients" (R).[2] The types in each pair are randomly assigned to the players at the beginning of the experiment.

Our three treatment conditions Base (BA), Transfer (TR) and Feedback (FB)[3] introduce both different "production" conditions between taxpayers and different degrees of awareness about those production conditions by recipients: the treatment condition BA follows this outline without any change. In TR and FB, the transfer is meant to enable players R to reach the substance threshold (or make reaching the threshold substantially more likely). TR and FB differ on one dimension only: in FB throughout the second phase player T receives a real-time feedback on the performance of the player R, this is not the case in TR. Thus, on one hand, player T can exactly see how much effort the paired player R exercises to reach the threshold

2 Explanations given to the subjects in the lab use the labels "Type A" and "Type B" to avoid any unintended connotations. Instructions and example screens are available online at https://osf.io/bft6w

3 We reported the results from the former two treatment conditions already in Chugunova et al. (2022).

on her own. On the other hand, player T knows approximately how much player R still needs to pass the threshold. Therefore, in TR the willingness to assist the disadvantaged will rather follow a logic of unconditioned (or even unintended) solidarity, while FB relies on some sort of reciprocity (R extends effort, T supports via obligatory transfer).

All parameters of the game (i.e., difference in the complexity of tasks between player types, piece rates for both types, subsistence income, unilateral monitoring) are common knowledge. The implemented monitoring scheme is somewhat imperfect. While the taxpayer observes how many tasks the assigned partner solves, she has no information on player R's performance in the first phase. Hence, a taxpayer is not able to tell whether her counterpart performs to her true ability.

We would like to stress two important features of our design: (1) The accountability principle has been proven to play a role in redistribution decisions. That is, people are less willing to pay for redistribution if recipients are accountable for their situation (Cappelen et al., 2013). The type of disadvantage implemented in our experiment, however, cannot be ascribed to the accountability of players of type R. This interpretation is further supported by the random assignment of players to their types. Hence, we deliberately eliminate this rationale on the side of the taxpayers. (2) Previous studies on donations show that if donor incomes are earned (i.e., donors work for their income), donors are less likely to redistribute (e.g., Cherry et al., 2002; Ogawa et al., 2012). That is, if they feel entitled to their earnings, they are less likely to give. By employing a real effort design we create unfavorable conditions for redistribution by reinforcing the perception of being entitled to one's earnings. So in contrast to earlier experiments (e.g., Agranov and Palfrey, 2015), our design diminishes the easiness (acceptance) of giving.

II. Procedure

The experiment was conducted at the Wiso research Lab at the University of Hamburg using z-tree (Fischbacher, 2007) in spring and autumn 2016. Subjects were mostly students of various majors of the University of Hamburg, recruited online via hroot (Bock et al., 2014) and randomly assigned to treatments. No subject participated more than once.

Participants were seated in cubicles. Before the experiment started, they placed their cellphones into envelopes provided and sealed by the experimenters. The envelopes were returned to them only after they had been paid. They were also asked to keep their bags and personal belongings outside of their cubicles, ensuring that they could not use any electronic devices to help with the calculations. They were warned that the experimenter would monitor the use of forbidden devices throughout the experiment and that participants breaching the rules would be immediately removed from the experiment without being paid. The use of pen and paper, which were provided, was allowed.

After the participants were settled in, they received the instructions for Phase I in writing and by the experimenter reading them out loud. Participants were then randomly paired in sets of twos; those pairs stayed unchanged throughout the experiment, and they played simultaneously but independently of each other. The participants learned if they were player A or B right before the calculations in Phase I began, without, however, knowing the meaning attached to A and B. After Phase I was over the instructions for Phases II and III were distributed, and read out aloud, and the meaning attached to being player A or B was revealed. After each phase there was a three-minute break for each participant, without them having the opportunity to communicate with each other.

We conducted 10 sessions with 180 participants in total. Sessions run with 16–20 subjects per session. Overall, our data set consists of 32 pairs (i.e., independent observations of 31 players T and 32 players R) for BA, 30 (30 players T and 30 players R) for TR, and 28 (27 players T and 28 players R) for FB.[4] At the end of the experiment subjects were paid privately and in cash. Payments ranged from 3.75 to 52.40 Euros with an average of 24.30 Euros for approximately 140 minutes.

C. Hypothesis

I. Effort provision without redistribution

In the following, we assume that players hold a twice differentiable, additive utility function with concavely increasing benefits and convexly

4 We removed two participants (one participant in BA and one in FB) from the sample and did not use their data in the analysis, since individuals behaved nonsystematically different (their choices were more than 2.5 standard deviations away from the mean of relative performance measured as described below).

decreasing working costs. Both, benefits and costs depend on effort, while we expect players to differ with respect to their costs (i.e., their skills). That is, for two players i and j we label player i as more talented than player j if for every given work speed $\underline{v}$ it holds $c_j(\underline{v}) > c_i(\underline{v})$. In turn, players gain benefits from working which depends on the piece rate payment ϕ they receive and the work speed (per minute) v_i with which they choose to work (i.e., their endeavor). In sum, this yields

$$u(v_i) = w(\phi v_i) - c_i(v_i) \tag{1}$$

such that utility u_i of player i depends on the benefit w with $u'(w) > 0$ and $u''(w) < 0$, and the individual costs c_i with $u'(c_i) < 0$ and $u''(c_i) < 0$. It holds that $w'(v_i) > 0$ and $w''(v_i) < 0$, and $c'_i(v_i) > 0$ and $c''_i(v_i) > 0$.

In the first and – if possible – the third phase of the experiment, players maximize their utility by choosing v_i such that $\phi w'(\phi v_i) = c'_i(v_i)$ (first order condition). In the second phase, we have to consider that v_i may or may not be high enough to qualify i for the third phase. Thus, i's utility function changes to

$$u(v_i) = \begin{cases} w(\phi v_i) - c_i(v_i) & \text{if } 30 v_i \phi < S \\ w(\phi v_i) + U_i - c_i(v_i) & \text{otherwise.} \end{cases} \tag{2}$$

for U_i denoting i's expected utility in the third phase. This creates the incentives for players in the "medium effort cost range" to choose exactly a work speed v' that allows them to just reach the subsistence income. In turn, both players with very low or very high effort cost choose the same work speed as in phase I: their "initial" work in phase I is so low or high, so that the subsistence income requirement does not affect their decisions.

Hence, in phase II of BA, one can predict a partition of both advantaged and disadvantaged players into three groups. The first group chooses a work speed which yields an income below the subsistence income requirement. Within this group, people increase their work speed if they are better at solving the tasks (i.e., if they have lower effort costs). The second group chooses a work speed v' which yields an income at the subsistence income threshold. All players within this group choose this speed regardless how good they are at solving the tasks. Finally, players in the third group choose a work speed which yields an income beyond the subsistence income requirement. Within this group, people increase their work speed if they are better in solving tasks.

II. Effort provision with redistribution and monitoring

When analyzing the choice of a work speed in the treatments with redistribution, one has to consider the two types of players separately. For players of type T, the piece rate per solved task ϕ decreases to $(1 - \tau)\phi := \phi'$. For players of type R, the subsistence income requirement S decreases to $S - U' := S'$ with U' being the expected transfer they receive from their matched taxpayer.

Replacing ϕ with $\phi' < \phi$, it thus becomes "harder" for a taxpayer to meet the subsistence income requirement, as they earn only $(1 - \tau)\phi$ for each task. This implies that the range for the three groups of players T "moves to the right": a broader range of potential optimal work speeds now belong to the first group earning less than the subsistence income, while a smaller range of players choose a work speed leading to more than the subsistence income. Finally, taxpayers who were to choose a work speed leading to an income beyond the subsistence income now choose a work speed corresponding with the subsistence income.

In turn, replacing S with $S' < S$, it becomes "easier" for transfer recipients to meet the subsistence income requirement implying that the range for the three groups of players "moves to the left".

In other words, from a theoretical point of view, redistribution is nothing more than a wage cut for taxpayers, similar to Kessler and Norton (2016), and an expected decrease in the subsistence income threshold for transfer recipients. Since we calibrated the threshold, piece rate and tax rate such that many taxpayers belong to those who were to choose a work speed leading to an income beyond the subsistence income even with taxation, whereas recipients belong to those who choose a work speed close to the subsistence income even without taxation, we predict a positive effect of redistribution on productivity in phase II:

H_1: Redistribution imposes benefits since players produce in total more in TR than in BA.

With transparency as designed in our experiment a taxpayer can observe the effort of a recipient, but not vice versa. Earlier research has shown that monitoring among peers has enormous effects on behavior (e.g., Bandiera et al., 2005; Mas and Moretti, 2009). The reason for this is a human inclination to reciprocate and match others' behavior (e.g., Fischbacher et al., 2001): if two people work on a joint project or on similar tasks, they tend to harmonize their effort. In our context, this could mean that transfer recipients and taxpayers approximate their work speed with monitoring:

H_2: Transparency amplifies the connection between work speeds of players R and T in FB, while the connection is weaker in TR.

Yet, knowing that both work speeds by R and T become more similar while R being monitored offers R a strategic leverage: if R accelerates her work speed (at least in the beginning of phase II), she will stimulate the work speed of T as well and it increases her chances to meet the subsistence income. In other words, the unilateral monitoring of R by T may create an additional incentive to increase R's work speed. In this case, there would be a strategic benefit of transparency. Those considerations lead us to formulate a hypothesis that follows the former argumentation:

H_3: Transparency increases the benefits of redistribution, since particularly players R produce more in FB than in TR.

However, we are aware that also the opposite argumentation appears plausible: if R do not realize the strategic leverage, they may even create a negative reference point for T, slowing T down. In this case, transparency backfires: the unilateral monitoring yields a decrease in efficiency and a redistribution system that keeps taxpayers ignorant with respect to recipients work effort would provide superior efficiency.

D. Results

I. Does redistribution lead to more production on the side of the taxpayers?

To measure the performance of the participants controlling for their initial ability we divide the number of correctly solved tasks in the second phase by the number of correctly solved tasks in the first phase. We further on call this relation between stages "relative performance". The relative performance of 1 means that a player solves in the second phase exactly the same number of tasks as in the first one; a number less than 1 means that she reduces her effort and solves less, and the number more than 1 means respectively that she improves her performance. Thus, one can also interpret relative performance as a percentage of change as compared to the first phase.

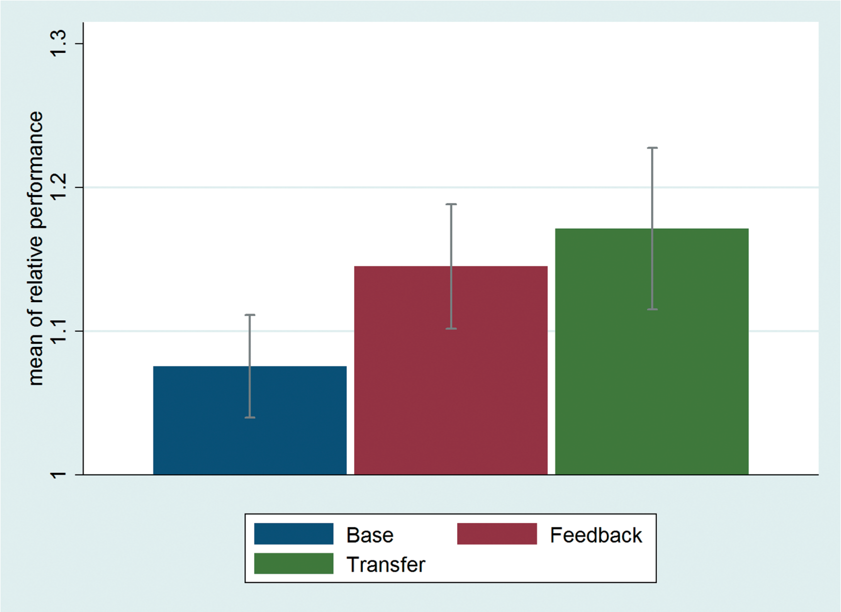

Figure 1: Average relative performance of players T across treatments. 95 % confidence interval.

Figure 1 displays the response to taxation on the aggregate level. Overall, there are no implicit costs of redistribution for players T, but rather benefits in terms of productivity. The relative performance of players T increased on average by 7,6 % in BA, by 17,1 % in TR, and by 14,5 % in FB. As shown in the Fig. 1, redistribution in TR and FB improves productivity as compared to BA. Mann-Whitney U-tests[5] confirm H_1 by showing that the relative performance of players T in FB (1.15) and TR (1.17) is significantly higher than in BA (1.08; p=0.03 with FB and p=0.005 with TR). However, there is no significant increase or decrease due to the introduction of the monitoring. In other words, the relative performance in TR and FB are not significantly different from each other (p=0.45). Thus, we cannot confirm our hypothesis H_3. The presence of monitoring leads to a slight, but insignificant decrease in relative performance.

5 All nonparametric statistical tests reported are two-tailed and take individuals as units of observations.

II. Does monitoring lead to more production on the side of the taxpayers?

We calibrate the subsistence income requirement such that it provides an extremely mild restriction for players T. Fig. 2 displays how restrictive the imposed subsistence income threshold is for every individual as well as individual response to the introduction of taxation. Blue bars show how players T need to adjust their performance in the second phase to meet the subsistence income requirement; the vast majority of players could lower their performance considerably and still meet this requirement. Red bars show the actually observed relative performance; only 3 (2) players T in FB (TR) are not able to meet the requirement and the majority of subjects boosted their performance.

To analyze how differences in initial skills influence the relative performance in TR and FB in greater detail, we divide players T into high and low "talent" groups according to the median of performance in the first phase of each treatment. We observe that it is the higher relative perfor-

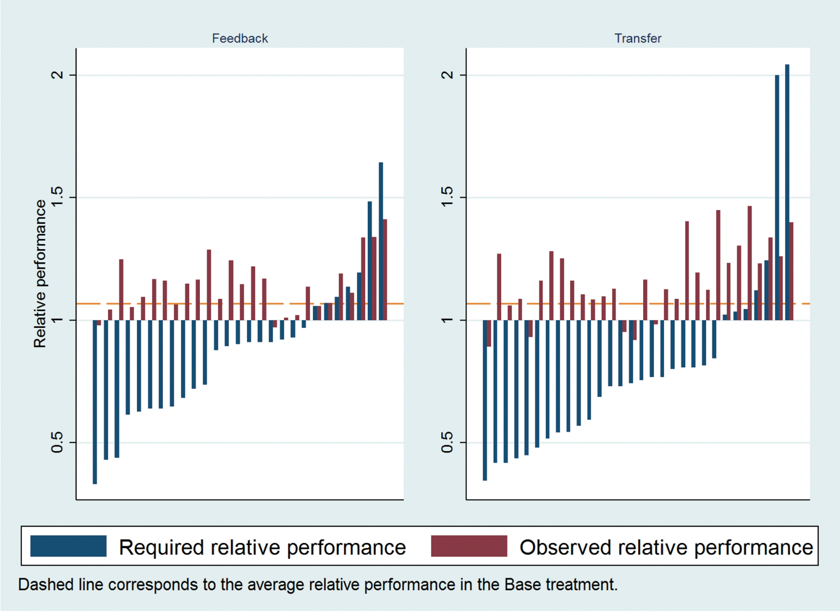

Figure 2: Relative performance required to reach the subsistence threshold and actual relative performance for each taxpayer. Dashed line depicts the median relative performance in the Base treatment.

mance among low talent taxpayers that predominantly drives the overall higher relative performance level in TR treatment (see Fig. 3). Among low talent taxpayers the highest performance can be observed in TR (1.25), which is significantly different from performance in BA (1.09; p=0.02) and in FB (1.16; p=0.07). The performance of low talent players T in FB is non-different than that in BA (p=0.24).

On the other hand, among high talent taxpayers, there is no significant difference between relative performance in TR (1.09) and FB (1.13; p=0.45) and TR and BA (1.06; p=0.35). The relative performance in FB is, however, significantly different from BA (p=0.04). Thus the introduction of monitoring causes some increase in effort among the high talent taxpayers, but at the same time decreases the effort of the less talented ones resulting on average in the slight but insignificant decrease in productivity due to the introduction of monitoring.

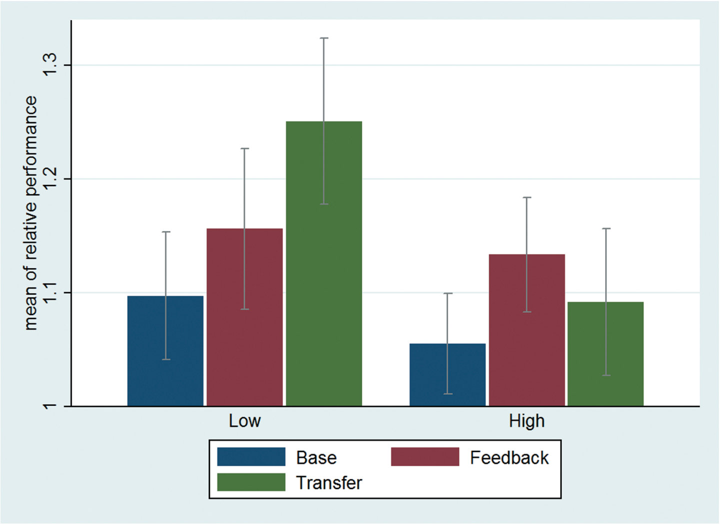

Figure 3: Relative performance of taxpayers: high versus low talent.

Summarizing all of the above:

Result 1: Redistribution without monitoring leads to a substantial increase in productivity for low talented players T; there is no such effect for high talent players T. Yet, monitoring of transfer recipients reverses the effect bringing about higher relative performance for high talent taxpayers and eliminating the enhancing effect for low talent players T. The magnitude of the effect is bigger for the former. On average productivity is enhanced by the introduction of the redistribution, but there is no additional boost due to transparency.

III. Does monitoring lead to more production on the side of the recipient?

Since transfer recipients faced a more difficult task in the second phase, they slowed down by approximately half, resulting in relative performance of 0.48 in BA, 0.55 in TR and 0.49 in FB (see Fig. 4). Mann-Whitney U-tests confirm that the relative performance of players R in the TR is significantly different than in BA (p=0.07), however, results reveal no significant differences in relative performance between FB and TR (p=0.40) or FB and BA (p=0.59). That is, against our H_3, recipients in the system of unilateral monitoring do not use their performances strategically to affect the behavior of the taxpayer by triggering reciprocity. Rather, it seems that players R are not aware of the strategic implication of their own work speed. Furthermore, it seems that monitoring demotivates players R as we observe that without monitoring more players attempt to reach the threshold: the correlation of performance in the first and second phase among players R is 0.40 (p=0.02) in BA, but 0.71 (p=0.000) in TR, while the correlation between performances during the two phases drops again in FB (0.39, p=0.04) (see Fig. 6).

To gain additional insights we divide players R into low and high talented subgroups according to the median of performance in each treatment. In Fig. 5 it can be seen that the higher relative performance in TR is predominantly driven by less talented players. Although the differences between high and low talent recipients are not significantly different across all treatment conditions, we find a substantially enhanced performance of the low talent transfer recipients in TR. In contrast, it seems that monitoring neither spurs the performance of high talent nor of low talent players R.

Fig. 7 sheds the light on the dynamics of the work speed of recipients over time. It shows the average time in seconds needed by players R to solve a task over the course of the second phase[6]. We observe a decrease

6 To adopt to the random fluctuation in the difficulty of the task, we take a moving average of 5 tasks: the current task and two tasks before and after all weighted equally.

in working speed around task 15, which is the most pronounced in the FB treatment. It can be connected to the understanding of how hard meeting the threshold is for disadvantaged players. The speed improves (i.e., decreases) over time as bad performers "drop out". The average work speed is the highest in TR, against opposing H_3. As a consequence of the higher speed the total number of tasks solved by the poorest performing quarter of players (i.e., the dashed lines) is significantly higher in TR (0.48) than in FB (0.24 p=0.005) and BA (0.28 p=0.02). Summarizing all of the above:

Result 2: Monitoring leads to a substantial decrease in productivity, particularly for low talent players R; there is little evidence that monitoring affects the performance of high talent recipients.

IV. Do taxpayers take the performance of the transfer recipients into account?

Answering the question of whether taxpayers indeed reciprocate or rather react to the current need of the recipients requires a deeper look into the performance of individual pairs. To do so we run an Arellano-Bond dynamic panel data estimation. Including a lagged dependent variable as an explanatory variable allows the model to partially adjust. We split the overall time of the second phase into one-minute sub-periods and run the panel regression with the number of correctly solved tasks of the taxpayer in the one-minute sub-period as a dependent variable. As the implemented monitoring system is unilateral, the direction of the effect is certain.

Table 1 displays two specifications (1–2)[7] of the estimation which differ only with respect of how quickly the taxpayer updates her behavior in response to the performance of the transfer recipient: in the column (1) we assume that the update is immediate (i.e., takes place at the same minute), and in the column (2) we allow for a one minute lag (i.e., taxpayers observe the performance and adopt in the next minute). Apart from the measure of performance of the recipient (i.e., number of tasks solved by the recipient in the respective one minute subperiod), we include in the estimation the number of the tasks the recipient solved up till the current period ("total solved by recipient")[8] and three dummy variables:"threshold with

7 Specifications 3 and 4 will be discussed in Section IV.
8 It is the actual number taxpayers observe on their screen in any respective minute.

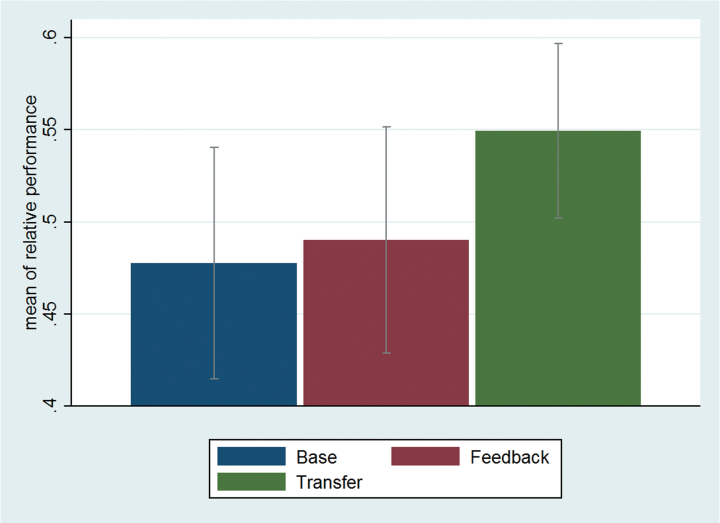

Figure 4: Average relative performance of recipients across treatments. 95 % confidence interval

transfer (recipient)" takes the value of one if the recipient is already above the threshold taking into account the transfer which is due to happen, respectively "threshold (taxpayer)" takes the value of one if the taxpayer is already above the threshold taking into account the transfer which is due to happen and finally, the control "faster" takes the value of one if the recipient happens to solve more tasks than taxpayer in the respective one minute period. Individual abilities are eliminated through fixed effects, standard errors clustered at the individual level.

Our results show that neither current nor lagged performance of the transfer recipients affects the performance of the taxpayer: recipient outperforming the taxpayer and the taxpayer reaching the threshold are the only significant coefficients. If the recipient outperforms the taxpayer, the taxpayer solves roughly 1.2 tasks per minute less. Considering the size of the constant it implies a slowdown of almost 30 %. The coefficient is negative indicating that if the recipient is so fast that she does not require any help. The positive effect of passing the threshold is somewhat more surprising. It means that after reaching the threshold, the taxpayer speeds up and gets more productive. It can be explained by the motivational spike of taxpayers from being able to reach the threshold and qualifying for further production. Fig. 8 looks deeper into the performance of the players three

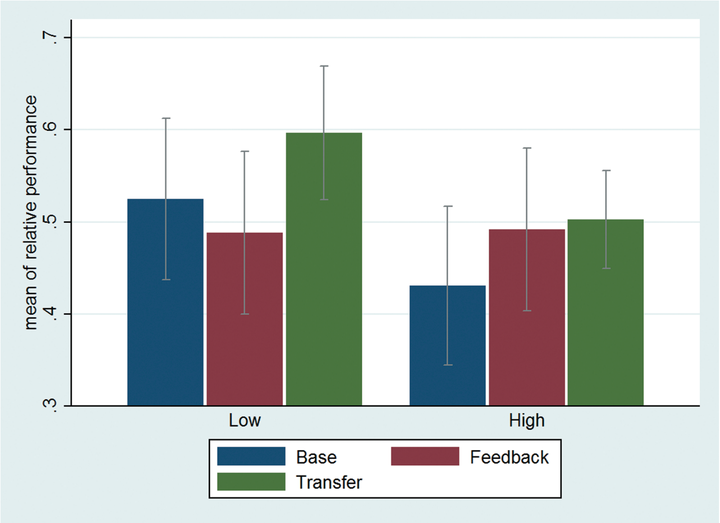

Figure 5: Relative performance of recipients: high versus low talent.

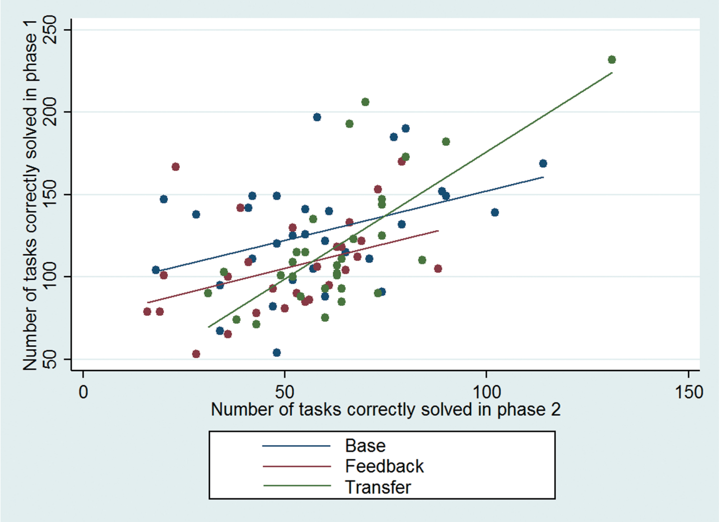

Figure 6: Correlation between correctly solved tasks of players R in the first and second phase.

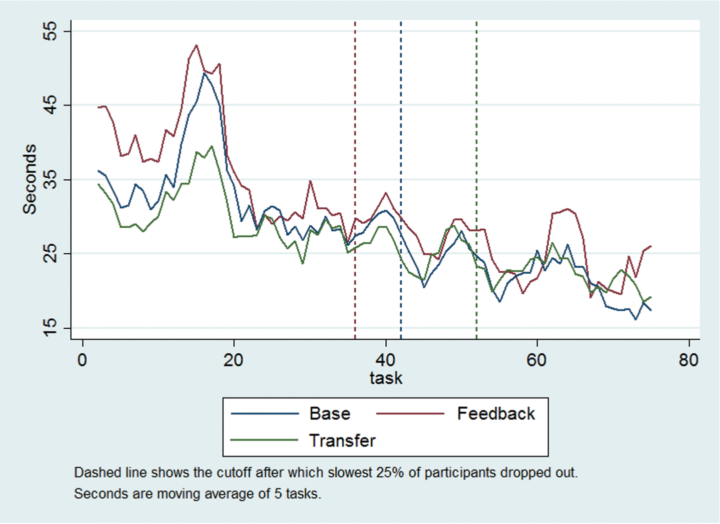

Figure 7: Work speed dynamics of recipients.

minutes (i.e., 10 % of the phase time) before and after the threshold. It is possible that some players did not reach the threshold at least 3 minutes before the end of the phase; consequently, the average number of tasks completed after the threshold may be affected by the attrition of slower players. In other words, fast players produced for 3 minutes before and 3 minutes after the threshold, while slowest players produced for 3 minutes before and – for example – 2 minutes after and thus the average may be influenced by the lower number of slower players after the threshold as such. Although we can not eliminate this bias completely, Fig. 8 shows how many minutes were taken into account for calculating the aggregate (white numbers at the bottom of each bar)[9]. That is, in the FB treatment some slower players had less than three minutes after the threshold: two players had one minute after the threshold only and three players had two minutes. Wilcoxon signed-ranks test confirms that the speed of production is not significantly different three minutes before and after the threshold (BA p=0.26, TR p=0.46, FB p=0.69). In BA and FB taxpayers slightly slow down in solving the exercises, while in TR they slightly sped up.

9 Time data are missing for some sessions in BA and TR, therefore the graph is based on the data for 27 taxpayers in BA, 20 taxpayers in TR and 27 taxpayers in FB.

Table 1: DPD Estimation: Determinants of Performance.

Variables	(1)	(2)	(3)	(4)
L.Correctly Solved Tasks by Taxpayer	−0.0192 (0.0333)	−0.0192 (0.0331)	−0.0193 (0.0331)	−0.0195 (0.0333)
Correctly Solved Tasks by Recipient		−0.00958 (0.0524)		0.0399 (0.0575)
L. Correctly Solved Tasks by Recipient	0.0139 (0.0647)		0.00810 (0.0637)	
Total Solved by Recipient	7.86e-05 (0.00753)	0.000259 (0.00758)	−1.54e-05 (0.00772)	−0.000168 (0.00777)
Threshold with Transfer (Recipient)	0.0999 (0.245)	0.0997 (0.244)	0.125 (0.251)	0.127 (0.254)
Threshold (Taxpayer)	0.613*** (0.183)	0.611*** (0.183)	0.596*** (0.183)	0.592*** (0.179)
Faster	−1.273* (0.708)	−1.262* (0.713)	−1.331* (0.752)	−1.366* (0.761)
Desperate			0.624* (0.319)	0.708** (0.352)
Constant	4.467*** (0.355)	4.501*** (0.362)	4.198*** (0.336)	4.112*** (0.343)
Observations	754	754	754	754
Number of id	27	27	27	27
Wald chi2	33.44	33.59	51.98	50.99

Robust standard errors in parentheses
** p<0.01, ** p<0.05, * p<0.1

V. When do taxpayers take the performance of the transfer recipients into account?

So far, there is little evidence supporting our H_2. In our argumentation in favour of this hypothesis, we stressed the reciprocal nature of redistribution: those recipients who work hard receive much help. However, the finding that taxpayers pay little attention to the performance of the recipients may indicate that their solidarity is simply an altruistic act of helping those incapable of helping themselves. If solidarity is motivated by unconditional help to those in need, it could explain the absence of changes in the labor supply of taxpayers.

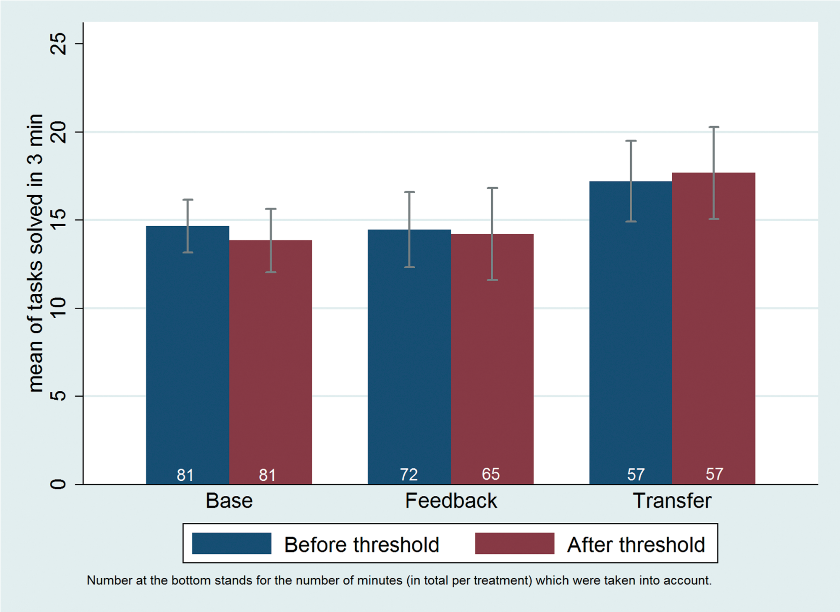

Figure 8: Mean of the number of correctly solved tasks three minutes before and after the threshold, 95 % CI.

Although our experiment does not allow to clearly separate these two logics of social exchange, we will attempt to gather some suggestive evidence in this regard. Fig. 9 shows non-linear relation between relative performance of the taxpayer and the overall performance of recipients in the second phase: if the recipient is far from the threshold, the taxpayer's effort is high, as well as in the case where the recipient is just below it or above. This U-shape can explained by the fact that the logics of reciprocity and solidarity may be complementary.

Very needy transfer recipients trigger taxpayers' solidarity which leads to higher relative performance. In turn, transfer recipients who make it almost on their own deserve support: taxpayers reciprocate and boost their relative performance as well. Players in between do not trigger solidarity, but do not perform well enough to trigger positive reciprocity. Thus, it seems that as soon as the recipient is good enough to be seen as "could have done better", taxpayers' relative performance is the lowest.

The shape of the curve suggests that due to solidarity desperate recipients are supported by taxpayers. It is possible that some players did not

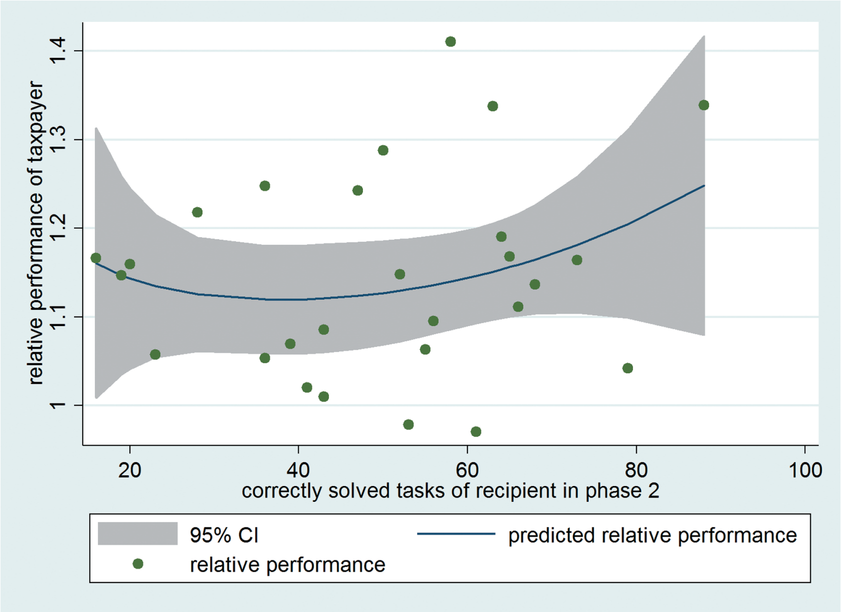

Figure 9: Relative performance of taxpayers depending on the performance of the recipient in Feedback.

reach the threshold at least 3 minutes before the end of the phase; consequently, the average number of tasks completed after the threshold may be affected by the attrition of slower players. We classify players as desperate if the total number of solved tasks and the number of tasks solved in the respective one minute sub-period are below median. Although such rule is rather arbitrary, it allows to single out those who perform poorly in general as well as in the respective sub-period. The estimation confirms that if a transfer recipient is desperate, a taxpayer speeds up by approx. 0.6 tasks per minute. This evidence is, however, suggestive, since we do not elicit the beliefs of taxpayers regarding the effort and ability of the transfer recipients.

Summarizing all of the above:

Result 3: There is some evidence suggesting that both recipients far off the threshold and recipients just below or at the threshold trigger high effort by the taxpayers. For those recipients, transparency increases efficiency in the redistribution system, while recipients between those two extreme cases would benefit from taxpayers' ignorance about their work speed.

E. Conclusion

Large-scale redistribution is often criticized or even opposed because of the common fear of taxpayers being exploited by transfer recipients. In this paper we deal with the widely expressed public opinion that transparency of recipients' efforts may be one mechanism to promote the acceptance of redistribution and its ability to create incentives for transfer recipients to provide extra effort. To study the effects of transparency, we conduct a series of real-effort experiments in which taxpayers can unilaterally observe transfer recipients' level of needs and level of efforts and thus adjust their performance according to the received information.

Transparency is expected to increase productivity by stimulating both the work speed of taxpayers and transfer recipients. Our results do not support this expectation. We find that monitoring does not enhance the performance of the taxpayers but negatively affects the productivity of the transfer recipients. It seems that recipients do not use their performance strategically to trigger positive reciprocity among taxpayers. In turn, only very poor and very good performing recipients stimulate higher effort by taxpayers. Thus, the effect of monitoring on the overall productivity is negative.

Therefore, our results indicate an institutional advantage of deliberate ignorance for redistribution. Rather than stimulating effort provision by taxpayers and transfer recipients, transparency creates a negative reference point for many taxpayers. Overall, transparency backfires and sealing off information increases efficiency. This result demonstrates the importance of Hertwig and Engel's approach: strategic ignorance, that is, deliberately choosing not to know or institutionally choosing not to reveal information, yields sometimes superior outcomes. Hence, in some circumstances, cautiously using strategic ignorance is in the best interest of all members of the society.

References

Aarøe, Lene and Michael Bang Petersen (2014). "Crowding out culture: Scan- dinavians and Americans agree on social welfare in the face of deservingness cues". In: *The Journal of Politics* 76.3, pp. 684–697.

Agranov, Marina and Thomas R Palfrey (2015). "Equilibrium tax rates and income redistribution: A laboratory study". In: *Journal of Public Economics* 130, pp. 45–58.

Andries, Marianne and Valentin Haddad (2020). "Information aversion". In: *Journal of Political Economy* 128.5, pp. 1901–1939.

Bandiera, Oriana, Iwan Barankay, and Imran Rasul (2005). "Social preferences and the response to incentives: Evidence from personnel data". In: *The Quarterly Journal of Economics* 120.3, pp. 917–962.

Bartling, Björn, Roberto A Weber, and Lan Yao (2015). "Do markets erode social responsibility?" In: *The Quarterly Journal of Economics* 130.1, pp. 219–266.

Bock, Olaf, Ingmar Baetge, and Andreas Nicklisch (2014). "hroot: Hamburg registration and organization online tool". In: *European Economic Review* 71, pp. 117–120.

Cappelen, Alexander W, James Konow, Erik Ø Sørensen, and Bertil Tungodden (2013). "Just luck: An experimental study of risk-taking and fairness". In: *The American Economic Review* 103.4, pp. 1398–1413.

Cherry, Todd L, Peter Frykblom, and Jason F Shogren (2002). "Hardnose the dictator". In: *The American Economic Review* 92.4, pp. 1218–1221.

Chugunova, M., Nicklisch, A., & Schnapp, K.- U. (2022). Redistribution and production with a subsistence income constraint: a real-effort experiment. FinanzArchiv, 78(1/2), 208–238.

De Bruin, Kiki, Yael De Haan, Rens Vliegenthart, Sanne Kruikemeier, and Mark Boukes (2021). "News avoidance during the COVID-19 crisis: Understanding information overload". In: *Digital Journalism* 9.9, pp. 1286– 1302.

Fischbacher, Urs (2007). "z-Tree: Zurich toolbox for ready-made economic experiments". In: *Experimental Economics* 10.2, pp. 171–178.

Fischbacher, Urs, Simon Gächter, and Ernst Fehr (2001). "Are people conditionally cooperative? Evidence from a public goods experiment". In: *Economics Letters* 71.3, pp. 397–404.

Hertwig, Ralph and Christoph Engel (2016). "Homo ignorans: Deliberately choosing not to know". In: *Perspectives on Psychological Science* 11.3, pp. 359–372.

Kessler, Judd B and Michael I Norton (2016). "Tax aversion in labor supply". In: *Journal of Economic Behavior & Organization* 124, pp. 15–28.

Khadjavi, Menusch, Andreas Lange, and Andreas Nicklisch (2017). "How transparency may corrupt – experimental evidence from asymmetric public goods games". In: *Journal of Economic Behavior and Organization*.

Kolstad, Ivar and Arne Wiig (2009). "Is transparency the key to reducing corruption in resource-rich countries?" In: *World Development* 37.3, pp. 521– 532.

Kool, Wouter, Joseph T McGuire, Zev B Rosen, and Matthew M Botvinick (2010). "Decision making and the avoidance of cognitive demand." In: *Journal of Experimental Psychology: General* 139.4, p. 665.

Kozyreva, Anastasia, Stephan Lewandowsky, and Ralph Hertwig (2020). “Citizens versus the internet: Confronting digital challenges with cognitive tools”. In: *Psychological Science in the Public Interest* 21.3, pp. 103– 156.

Lindstedt, Catharina and Daniel Naurin (2010). “Transparency is not enough: Making transparency effective in reducing corruption”. In: *International Political Science Review* 31.3, pp. 301–322.

Mas, Alexandre and Enrico Moretti (2009). “Peers at work”. In: *The American Economic Review* 99.1, pp. 112–145.

Ogawa, Kazuhito, Toru Takemoto, Hiromasa Takahashi, and Akihiro Suzuki (2012). “Income earning opportunity and work performance affect donating behavior: Evidence from dictator game experiments”. In: *The Journal of Socio-Economics* 41.6, pp. 816–826.

Sicherman, Nachum, George Loewenstein, Duane J Seppi, and Stephen P Utkus (2016). “Financial attention”. In: *The Review of Financial Studies* 29.4, pp. 863–897.