

On the effects of transparency and reciprocity on
labor supply in the redistribution systems

Marina Chugunova, Andreas Nicklisch and Kai-Uwe Schnapp

Working Paper Nr. 2017-19

<http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-19.pdf>

Date: 2017-08-22



**FOR
2104**

On the effects of transparency and reciprocity on labor supply in the redistribution systems.*

Marina Chugunova [†] Andreas Nicklisch [‡]
Kai-Uwe Schnapp [§]

August 22, 2017

Abstract

Although taxation distorts work incentives both for taxpayers and transfer recipients, its net effect on labor provision is shown to be less severe than predicted by the theory. It is likely that the reciprocity between taxpayers and transfer recipients plays an important role in mitigating the negative consequences of redistribution and maintaining a high level of effort. To check it we run a series of real-effort experiments exploring the production effects of taxation in the environment with unilateral monitoring: Taxpayers can continuously monitor the effort of the transfer recipient, which is designed to trigger reciprocity. Surprisingly, we find that monitoring decreases the total labor provision: recipients produce significantly less under monitoring, while the production of the taxpayers remains unchanged.

Keywords: *redistribution, subsistence income, implicit costs of taxation, transparency, monitoring*

JEL-Classification: C91, H21, H24

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

[†]School of Economics and Social Sciences, University of Hamburg, Germany, and DFG Research Group “Need-Based Justice and Distributive Procedures” (FOR 2104).

[‡]Center for Economic Policy Research, HTW Chur, Switzerland, and FOR 2104.

[§]*Corresponding author:* School of Economics and Social Sciences, University of Hamburg, Germany, and FOR 2104; e-mail: kai-uwe.schnapp@wiso.uni-hamburg.de

1 Introduction

With the inequality rising even in the most equality-oriented countries the question of how to redistribute wealth without harming the economic growth gains practical importance. Redistribution through taxation is one of the most powerful tools to curb increasing inequality and research on taxation is well established in the field of economics. A number of papers study the effect of taxation on labor supply and finds that disincentive effect of taxation goes beyond the monetary losses associated with it. Taxation seems to lead to more reduction in the supply of labor than a wage cut of equal size (Kessler and Norton, 2016). Moreover, individuals tend to be willing to exert extra effort to avoid tax, as compared to receiving a discount of equal size (Sussman and Olivola, 2011). Kessler and Norton, (2016) refer to this phenomenon as tax aversion. There are physiological mechanisms that may cause such disincentive effects of taxation: for instance, the perception of a missing link between taxation and government spending (Mettler, 2011), or a lack of influence taxpayers have on the way tax revenues are spent (Lamberton, 2013). Yet, the overall effect of taxation on labor supply is at most moderate (e.g., Buch and Engel, 2014). One potential reason for this moderate effect is (indirect) reciprocity. On one hand, people do not want to be suckers, i.e., if somebody cheats or exploits the redistribution system, each taxpayer who contributed to the system may feel exploited. Similar problems have been studied extensively, for example, by Ostrom, (1990), Fehr and Gächter, (2000, 2002) with regard to the use of common resources. On the other hand, if transfers are not abused (in the sense that recipients do not free-ride on others' effort), but helps those who are in need, then positive reciprocity may even lead to increased labor supply in response to taxation.

Survey studies support the intuition about the reciprocal nature of redistribution system: Farkas et al., (1996) observe that survey participants expect recipients of welfare transfers to get spoiled by them and “adopt a wrong lifestyle and values”(p.9). Fong et al., (2005) use survey data as well and find that taxpayers approve of tax money to be spent on national defense while disapproving of welfare spending. The authors analyze the evidence of public support for redistribution and claim that the key to understanding this support or the lack thereof is the concept of strong reciprocity. They define strong reciprocity “as a propensity to cooperate and share with others similarly disposed, even at personal cost and a willingness to punish those who violate cooperative and other social norms” (ibid., p. 285). Results of a number of large scale surveys (e.g., Farkas et al., 1996; Fong, 2001; Gilens, 1999; Heclo et al., 1985) provide evidence that a connection between redistribution and reciprocity is common. If indeed people's support of a

redistribution system is driven by a strong reciprocity, then actions of the transfer recipients in that very system are of crucial importance for its functioning (Rabin, 1993). The results of Aarøe and Petersen, (2014) support the intuition that providing more information on the transfer recipients may be beneficial. They study support of redistribution in Denmark and the USA and find that differences in support for redistribution are due to different default stereotypes about whether welfare recipients are lazy or unlucky, which play a role when no clear information about the recipient is available. On the other hand, if the direct information is available, that is, when people base their judgment on the actual information about the recipients and not on the default stereotype, the support of redistribution in two countries levels out.

To deepen our understanding of the effects of reciprocity on labor supply within a redistribution system 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 a threshold level of income in the second phase are allowed to take part in the high-income third phase. The threshold structure of the experiment allows to justify redistribution within experimental societies.

Our baseline treatment (BA) follows the description above and has no interaction between players. The second treatment (TR–Transfer) introduces a thirty percent income tax on the highly productive players. At the end of the second phase collected money 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 third treatment (FB–Feedback) 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 are targeted at systematically disadvantaged peers. What varies endogenously is recipients' own contribution to meet the subsistence income and to qualify 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 uni-directional 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, looking at the experimental economy as a whole, monitoring seems to harm, rather than to promote the total amount of labor supplied and thus efficiency. Although transparency and monitoring in the public domain are normally regarded as positive and desirable, it appears that under some conditions they may lead to lower production in a society.

These results relate to earlier studies on transparency that show as well that transparency may backfire under certain conditions: for example if public information is used to identify those who are important receivers of bribes (Kolstad and Wiig, 2009; Lindstedt and Naurin, 2010) or if people have asymmetric action spaces where one player is in the position to embezzle part of a common resource while others can only contribute and have no means of sanctioning (Khadjavi et al., 2017).

The rest of the paper proceeds as follows: Section 2 presents our experimental design and procedure, Section 3 develops a theoretical analysis and hypotheses, Section 4 presents the results. Section 5 concludes the paper.

2 The Experimental Design

2.1 The Game

To capture and measure the effect of transparency and monitoring on the supply of labor we run a real-effort economic experiment. The experiment consists of three phases of 30 minutes each, during which participants can do simple calculation tasks. The game is played in random pairs, which remain unchanged throughout the experiment. In phase one and three both

players compute the sum or the difference of two three-digit numbers. If the task is solved correctly, a new task appears on the screen; otherwise, the same task is presented again. The number of tasks is not limited and, therefore, participants are free to decide how hard, to work. Payment differs between the first and third phase: in phase one participants earn 0.05 Euro per task and in phase three 0.10 Euro. During the first phase we elicit the motivation and initial ability of the players to perform the task. Note that instructions for phases two and three of the experiment are distributed after the end of the phase one. We deliberately opt for a tedious and repetitive task and extended time span to perform this task, so that we minimize the joy and intrinsic motivation of work, and increase the importance of monetary incentives.

The second phase differs in three treatment conditions: Base (BA), Transfer (TR) and Feedback (FB)¹. In all three treatments, from the very beginning of the experiment, two players are randomly matched into pairs: each pair consists of player R (recipient) and player T (taxpayer).² The types of players differ only with respect to the second phase: T solves tasks with two three-digit numbers, while R – with two five-digit numbers. Irrespectively of the type of a player, the piece rate is 0.05 Euro per task. Both T and R face a subsistence income threshold: if a player cannot earn the subsistence income (in our case modeled as an exogenously imposed threshold of 3.20 Euro), she can not take part in the subsequent phase three (but waits in the laboratory until the end of the experiment without additional payment). The subsistence threshold is calibrated such that it is on average just too high to be reached by R on her own, but easy to achieve for T. The existence of the subsistence threshold justifies redistribution within the society to those who truly need support: without reaching the third phase, players receive approximately 7.6 Euro for a 140-minute experiment, which is well below the expectations of the participants.

BA follows this outline without any change. In TR and FB, players T pay an exogenously imposed tax of 30%, which is transferred with no loss of value to assigned players R and enables them (or at least makes it substantially more likely) to reach the substance threshold. The only feature that differs between TR and FB is that in FB throughout the second phase player T receives a real-time feedback on the performance of the player R. Thus, on one hand, player T can exactly see how much effort the paired player R

¹ We reported the results from the former two treatment conditions already in Chugunova et al., (2017).

² Different types of players were denoted as type A and B during the experiment to avoid any associations. The meaning and difference between the types as well as why we refer to them as taxpayers and recipients will be explained below.

exercises to reach the threshold on her own. On the other hand, player T knows approximately how much subsidy player R still needs to pass the threshold. 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. In a way, the implemented monitoring scheme can be seen as imperfect in the sense that although a taxpayer observes how many tasks the assigned partner solves, no information on the player’s R performance in the first phase is available. That is, a taxpayer is not able to see if her counterpart performs to her true ability. Apart from that, transparency is not absolute since recipients cannot monitor their taxpayers.

Three important features are embedded in the design to address our research question. First of all, due to the predetermined direction of transfers, subjects know in advance whether they are taxpayers or transfer recipients. The choice of systematically disadvantaged player within the pair is random and therefore transfer recipients both deserve support without any doubt and can not be held accountable for their lower productivity. Accountability principle was proved to play a role in redistribution decision (Cappelen, Konow, et al., 2013), however, it is not applicable in this environment. Although accountability is questionable in many real-world situations, we deliberately eliminate this type of uncertainty as this is not the focus of our research.

Secondly, we create the least favorable conditions for redistribution by reinforcing the entitlement towards one’s earnings. As previous studies on donation show (e.g., Cherry et al., 2002; Ogawa et al., 2012), if donors earn incomes (i.e., they work for their incomes), they are less likely to redistribute. In our experiment we use a real-effort task making taxpayers feel entitled to their earnings and thus less willing to transfer. So in contrast to earlier experiments (e.g., Agranov and Palfrey, 2015), our design diminishes the easiness of giving.

Thirdly, the unilateral monitoring system reduces complexity of the real-life environment considerably. We do so, since we want to isolate the causal effect of the effort “signal” – perhaps – its strategic use by the recipient. Although it may be questionable in the majority of real-word cases, it allows us to isolate the direction of our effect.

2.2 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.

Before the beginning of the experiment, participants placed their cell-phones into the provided envelopes and sealed them. They had access to the envelopes only after the payment. In addition, participants were asked to keep their bags and personal belongings outside of their cubicles. Thus, participants had no phones or calculators to help them solving the tasks. Pen and paper were provided to be used for calculations. After that participants were seated in cubicles and received a copy of the instructions for the first phase. Additionally, the instructions were read aloud. Subjects were randomly matched in groups of two, which remained unchanged throughout the experiment and played simultaneously, but independently from other pairs. Before any action took place a type of the player within the pair was displayed on her screen.³ After the end of phase one, instructions for the rest of the experiment (second and third phases) were distributed and read aloud. After each phase players had a short break of three minutes.

We conducted 10 sessions with 180 participants in total. Sessions run with 16-20 subjects per session yielding roughly 30 independent observations per treatment. 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.

3 Hypothesis

3.1 Effort provision without redistribution

In the following, we assume that players hold a twice differentiable, additive utility function with concavely increasing benefits and convexly decreasing working costs. Both, benefits and costs depend on effort. Effort subsumes in our experiment a combination of endeavor and skills. Therefore, 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 \bar{v} it holds $c_j(\bar{v}) > c_i(\bar{v})$. 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

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

³ Although types were revealed, the actual difference between types was made clear only after the end of phase one.

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

In the first and – if possible – the third phase of the experiment, players maximize their utility by choosing v_i such that $\phi\dot{w}(\phi v_i) = \dot{c}_i(v_i)$ (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. To qualify $30v_i\phi \geq S$ with S being the subsistence income. Thus, i 's utility function changes to

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

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 a work speed which allows them to just reach the subsistence income. Observe that the subsistence income requirement creates a discontinuous first order derivative of the marginal benefit of income. Specifically, for v_i'' such that $\phi v_i'' \neq S$ we have $\dot{w}(v_i'') = \phi\dot{w}(\phi v_i'')$, whereas for v_i' such that $\phi v_i' = S$ we have $\dot{w}(v_i') = \phi\dot{w}(\phi v_i') + U_i$.

Therefore, player i chooses a work speed such that they just reach the subsistence income if (i) $\phi\dot{w}(\phi v_i') + U_i \geq \dot{c}_i(v_i')$, and $\phi v_i' = S$, but (ii) $\phi\dot{w}(\phi v_i'') < \dot{c}_i(v_i'')$ for all $v_i'' > v_i'$. That is, they will choose neither a higher work speed, since this violates the first order condition (ii), nor a lower work speed, since this violates first order condition (i).

On the other hand, player i chooses a work speed which leads to an income below the subsistence income if (iii) $\phi\dot{w}(\phi v_i') + U_i < \dot{c}_i(v_i')$, and $\phi v_i' = S$. Again, any choice of v_i' and beyond – due to the concavity of $w(\cdot)$ – violates the first order conditions (iii).

Finally, player i chooses a work speed which leads to an income higher than the subsistence income if (iv) $\phi\dot{w}(\phi v_i'') \geq \dot{c}_i(v_i'')$ for some $v_i'' > v_i'$ and $\phi v_i' = S$. Any choice below the largest v_i'' violates the first order conditions (iv).

Overall, both for players R and players T, one can predict a partition of 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 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.

3.2 Effort provision with redistribution and monitoring

When analyzing the choice of a work speed in the treatments with redistribution, one has to consider 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 $S - \Phi := S'$ with Φ 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 “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 introduce a one-to-one relation between taxpayers and transfer recipients in our game and the former are overall more productive than the latter, we can hypothesize:

H_1 : Redistribution imposes implicit costs since players produce in total less in TR than in BA.

With monitoring 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 may use their work speed strategically to trigger positive reciprocity and to avoid negative reciprocity. That is, they attempt to increase their work speed to trigger extra effort of the observing taxpayer, and to avoid a deliberate slow-down of taxpayers in response to the recipient’s laziness. This would lead to:

H_2 : Transparency reduces the implicit costs of redistribution since players R

produce more in FB than in TR.

Yet, the general effect of monitoring in our environment is rather ambiguous. The reason for this is that those who are observed are systematically disadvantaged. Thus, even if player R tries her best and chooses the maximum work speed, this may be perceived by player T as if she works very slowly. In other words, the unilateral monitoring of R by T may create a negative reference point for T, slowing her down. After all, transparency could backfire in the sense that the unilateral monitoring yields a negative matching of work speeds implying that players T will slow down. Those considerations lead to our hypothesis:

H_3 : Transparency increases the implicit costs of redistribution, since players T produce less in FB than in TR.

4 Results

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

4.1 Does monitoring 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.

We calibrate the subsistence income requirement such that it provides an extremely mild restriction for players T. Fig.1 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

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 non-systematically different (their choices were more than 2.5 standard deviations from the mean of relative performance measured as described below).

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. Therefore, in line with the H_1 we expect a negative response to taxation (i.e., relative performance less than 1): players T work less in the second phase in TR and FB. 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.

Figure 2 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.2, unlike predicted by the standard theory relative performance in the treatments with redistribution (i.e., in TR and FB) improves as compared to BA. Mann-Whitney U-tests⁵ confirms 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). This result clearly contradicts our H_1 . 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.

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 performance 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 player 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

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

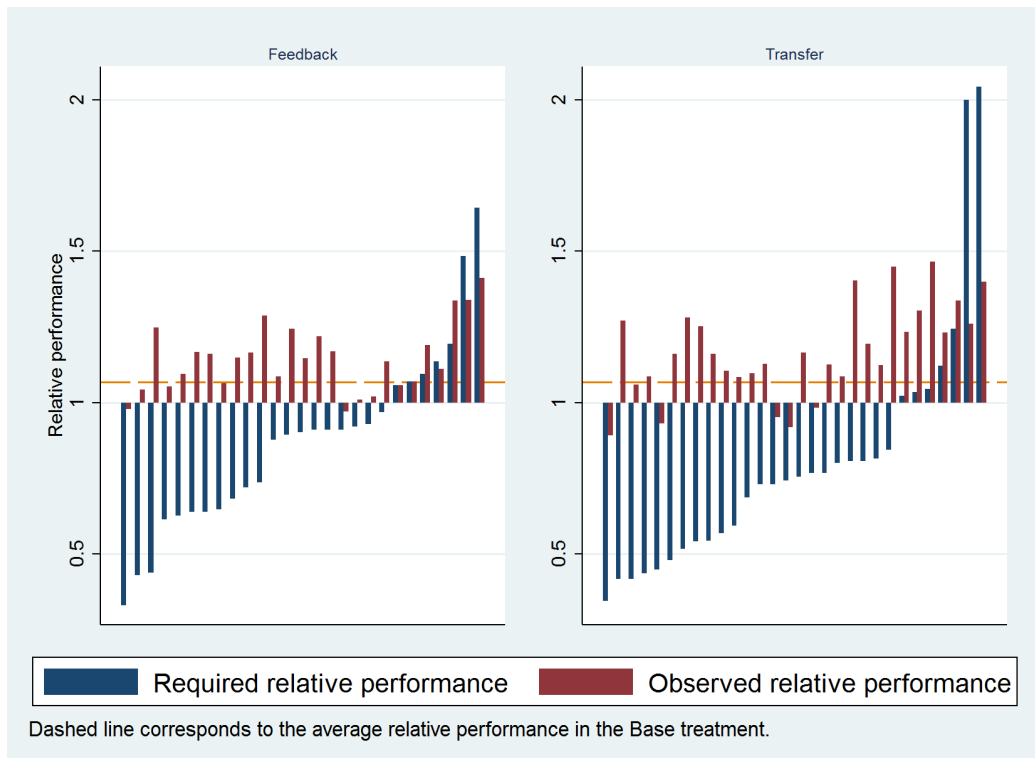


Figure 1: Relative performance required to reach the subsistence threshold and actual relative performance for each taxpayer. Dashed line depicts the median relative performance in the Base treatment.

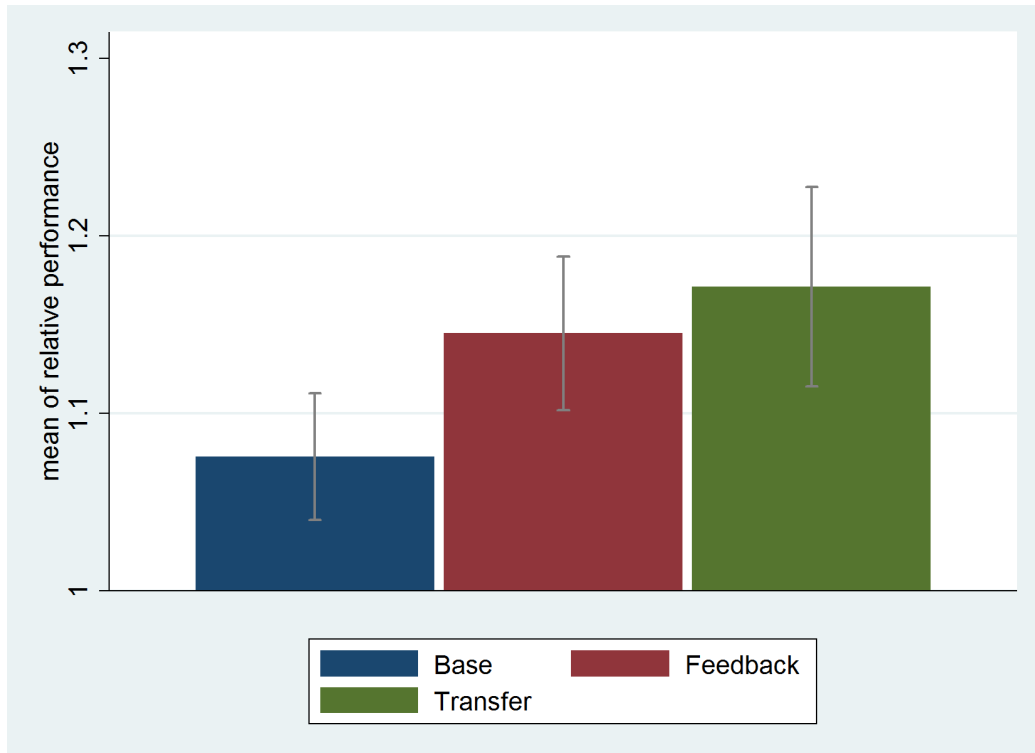


Figure 2: Average relative performance of players T across treatments. 95% confidence interval.

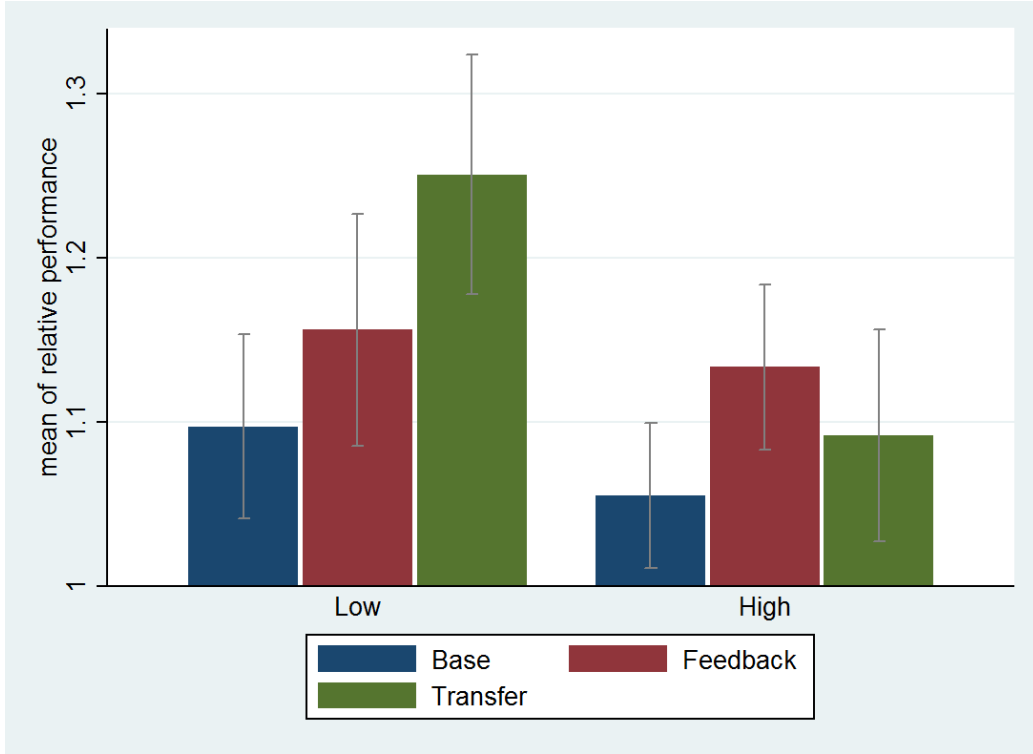


Figure 3: Relative performance of taxpayers: high versus low talent.

the slight but insignificant decrease in productivity due to the introduction of monitoring.

Summarizing all of the above:

Result₁: 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.

4.2 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 ca.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-test confirms that the relative performance of players R in the TR is significantly different than in BA ($p=0.07$), however, reveals no significant differences in relative performance between FB and TR ($p=0.40$) or FB and BA ($p=0.59$). In the system of unilateral monitoring a transfer recipient can use her performance strategically to affect the behavior of the taxpayer by triggering reciprocity. Although the performance of recipients in TR and FB is not significantly different, the fact that relative performance in FB is as well non-different from BA is puzzling and contradicts our H_2 . It seems that R players do not choose their work speed strategically in the second phase to trigger reciprocity. Rather, they anticipate that their working speed influences player T's performance negatively or not at all and lose their confidence in reaching the threshold. Along this line of argument, we observe in TR that the prospect of receiving an unconditional transfer motivates more players R to work harder in the second phase. With transfers (even of uncertain amount) but without monitoring more players attempt to reach the threshold. Supporting this logic 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. However, when the taxpayer monitors the performance of the transfer receiver, and thus the transfer may be somehow conditional on receiver's performance, the correlation in terms of performance between the two phases drops again (0.39 $p=0.04$) (see Fig. 6).

To gain additional insights we divide players R into low and high talented subgroups after 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. The differences between treatments within high and low groups are not significant. While there are no significant differences in performance of high and low talent groups within BA and FB treatments, enhanced performance of the low talent transfer recipients in TR leads to significantly higher relative performance of low talent group (0.6) as compared to the high talent group (0.5 $p=0.01$). Overall, 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 phase⁶. We observe a decrease 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 the threshold is for disadvantaged players. The speed improves (i.e., decreases) over time as bad

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

performers “drop out”. The average work speed is the highest in TR. 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:

Result₂: 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.

4.3 Do taxpayers take the performance of the transfer recipients into account?

Answering the question if 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 the 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 sub-period), 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 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

⁷ Specifications 3 and 4 will be discussed in Section 4.4.

⁸ It is the actual number taxpayers observe on their screen in any respective minute.

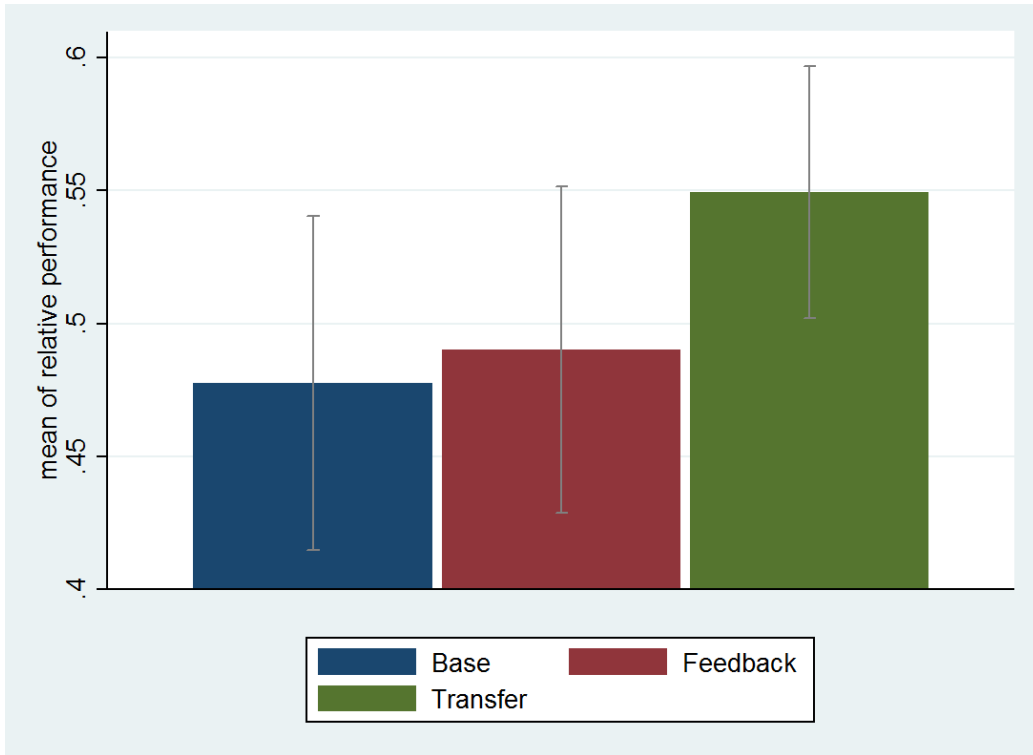


Figure 4: Average relative performance of recipients across treatments. 95% confidence interval

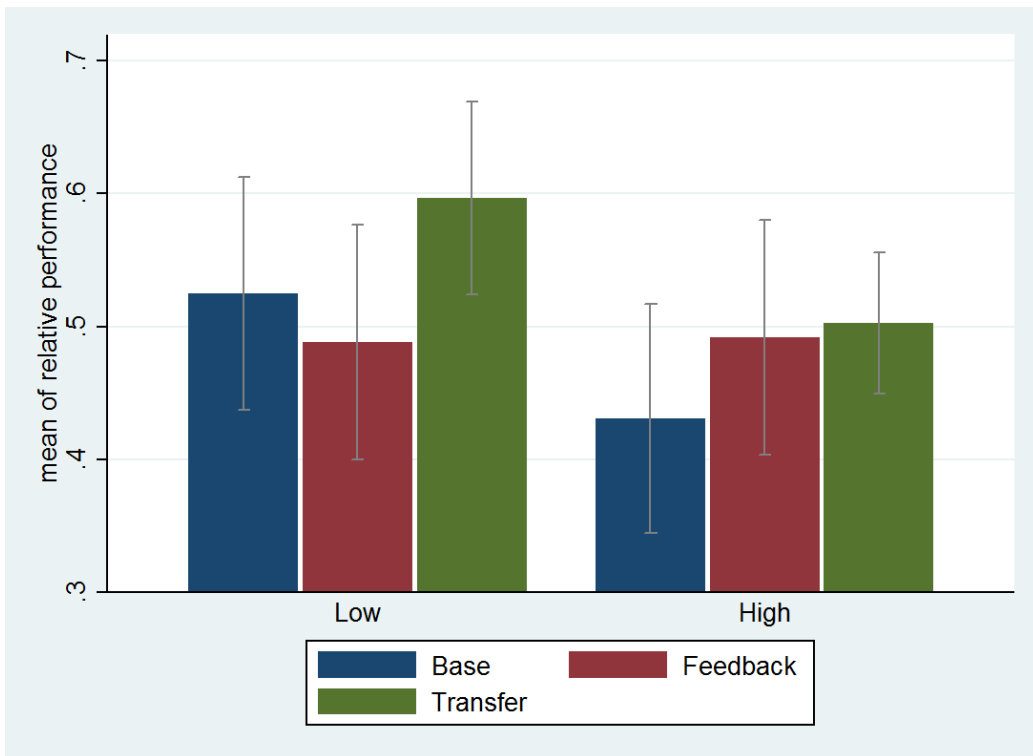


Figure 5: Relative performance of recipients: high versus low talent.

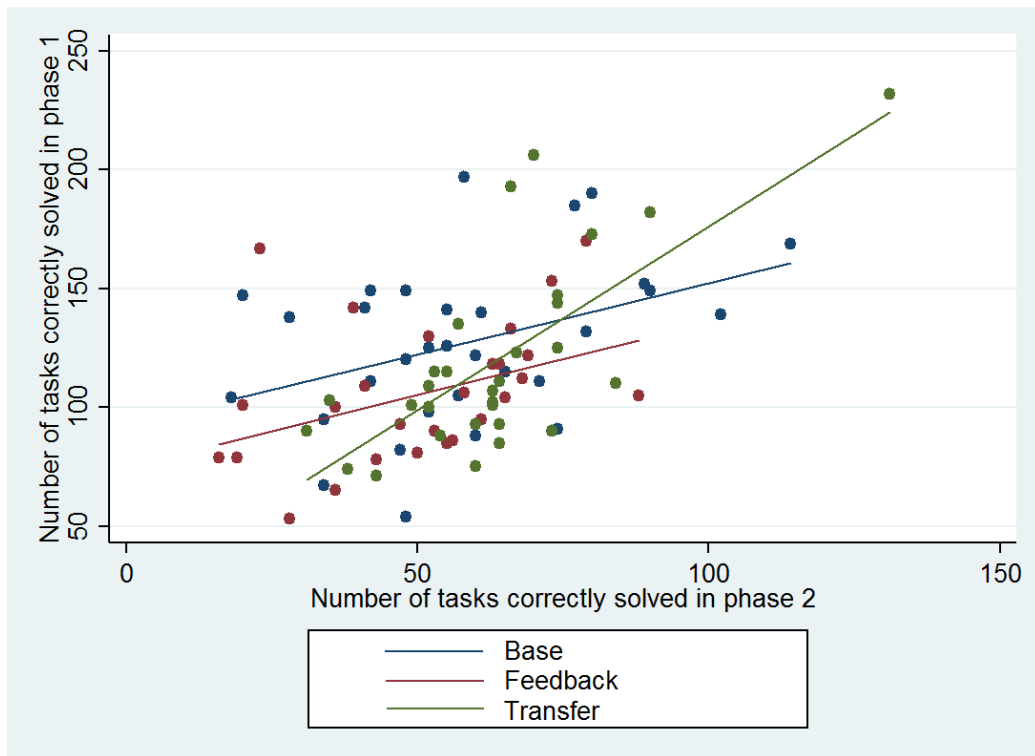


Figure 6: Correlation between correctly solved tasks of players R in the first and second phase.

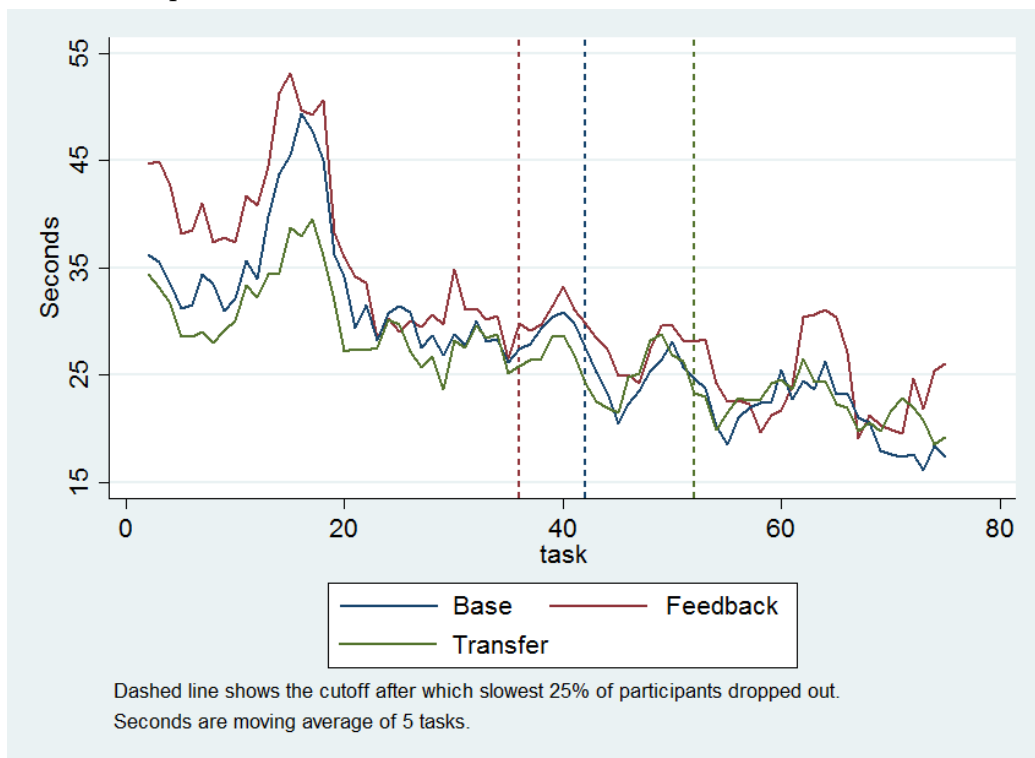


Figure 7: Work speed dynamics of recipients.

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 the slowdown of almost 30%. The coefficient is negative indicating that if the recipient is so fast than 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 motivation 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 minutes (i.e., 10% of the phase time) before and after the threshold. It is possible that not all players have reached the threshold at least 3 minutes before the end of the phase and thus the average number of task after the threshold may be affected by dropping out 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 e.g., 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 (in BA is $p=0.26$, 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.

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

In economics, redistribution is predominantly perceived and modeled as a reciprocal system. The finding that taxpayers do not take into account the performance of the recipients does not fit into such reciprocal framework. However, in other disciplines the conceptualization of a redistribution system

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

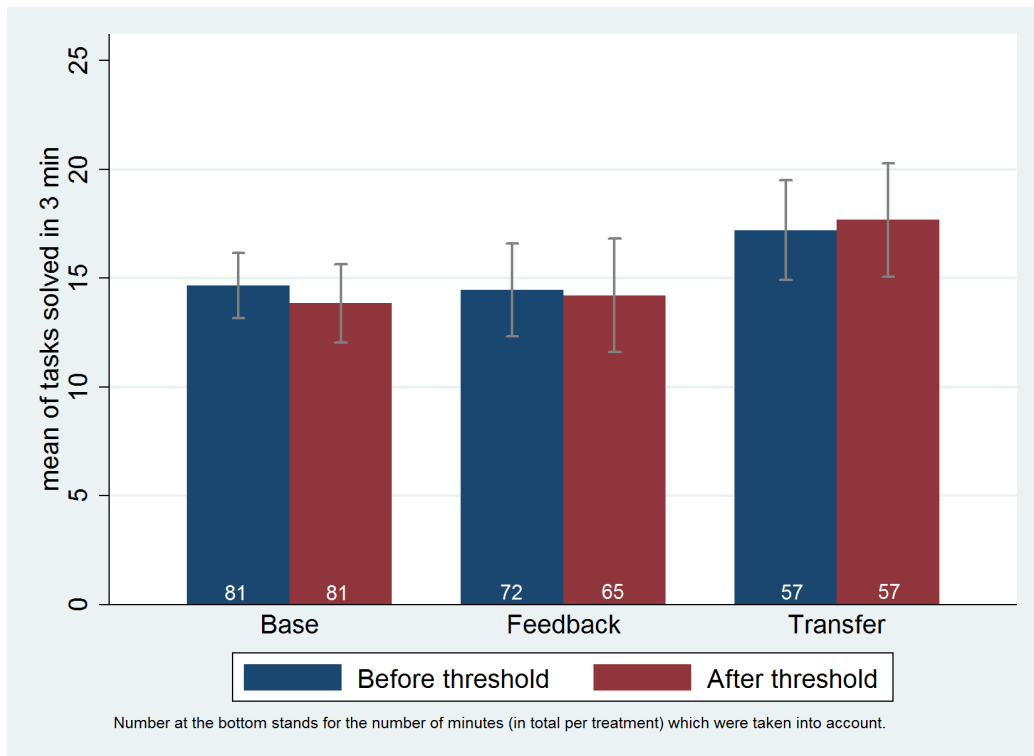


Figure 8: Mean of the number of correctly solved tasks three minutes before and after the threshold, 95% CI.

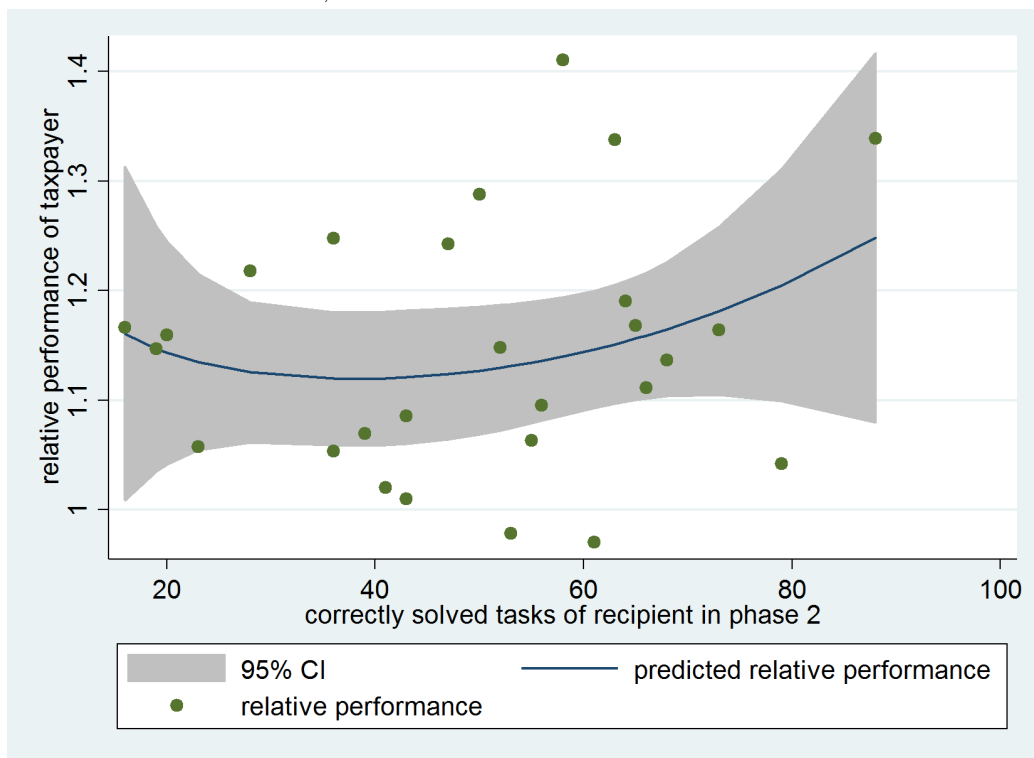


Figure 9: Relative performance of taxpayers depending on the performance of the recipient in Feedback.

is different and it can follow both the reciprocal logic as well as the logic of solidarity.¹⁰ If our redistribution system is based on solidarity, that is, on unconditional help to those in need, it could explain the absence of change in the labor supply of taxpayers.

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 be 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 subjects are supported by taxpayers. To check it, we come back to our estimation (Table 1:3-4) and run it again with the additional control of transfer recipients being desperate. 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.

5 Conclusion

The question of how to redistribute wealth within a society without damaging the economic growth is of major importance, particularly since the

¹⁰ "Solidarity is a matter of altruistic, one-sided transaction, of helping those incapable of helping themselves (and who at the extreme may never be able to give back and help others). ... Solidarity allows for a sustained one-way flow in favour of the have-nots, while reciprocity is characterised by an inability to tolerate a structurally unbalanced exchange" (Leitner and Lessenich, 2003, p. 329).

income inequality has increased in most countries over the last decades. Redistribution may be opposed or disliked for various reasons. One of them is the common fear of taxpayers to be exploited by transfer recipients. In this paper we deal with this widely expressed public opinion. Transparency of recipients' effort may be one mechanism to promote the acceptance of redistribution, as it reduces the incentives to abuse transfers for those who are supported by this system. To study the effects of transparency, we conduct a series of real-effort experiments where taxpayers can unilaterally observe both the level of needs and the level of effort of transfer recipients and thus adjust her performance according to the received information.

Higher transparency is expected to increase productivity by curbing the fear that the redistribution system is exploited. 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. Thus, the effect of monitoring on the overall productivity is negative.

The fact that we do not observe enhanced performance among transfer recipients suggests that they do not use their performance strategically to trigger positive reciprocity among taxpayers. On the contrary, it seems that recipients anticipate that taxpayers underestimate the effect of the brute bad luck of recipients and overweight the role of merit as implied by the merit primacy effect (Cappelen, Moene, et al., 2017). In other words, our findings suggest that recipients believe that taxpayers perceive them as able but lazy despite their disadvantage. In this case providing information on effort undermines recipients' hope for substantial support, and, as a consequence their productivity.

Yet, a redistribution system seems to be richer and more complex than a purely reciprocal system. A closer inspection of the interaction between taxpayers' and receivers' work speeds reveals a U-shape relation. That is, taxpayers relative performance increases when facing either high or low productivity recipients. Thus, the redistribution system seems to encompass both: the logic of social exchange based on reciprocity and based on solidarity. This implies that those who are completely unable, as well as those who are just not making it by themselves, will be helped.

Overall, the results of our paper are rather suggestive, but they show that due to the complexity of the redistribution system and to the interplay of incentives and motivations it is important to carefully consider the effects of monitoring. It is misleading to follow the public opinion that transparency is per se efficiency enhancing. Rather, it may enhance or diminish productivity, and its cautious use is in the best interest of all members of the society.

References

- Aarøe, Lene and Michael Bang Petersen (2014). “Crowding out culture: Scandinavians 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.
- 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.
- Bock, Olaf, Ingmar Baetge, and Andreas Nicklisch (2014). “hroot: Hamburg registration and organization online tool”. In: *European Economic Review* 71, pp. 117–120.
- Buch, Claudia M and Christoph Engel (2014). “Effort and redistribution: Better cousins than one might have thought”. In:
- 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.
- Cappelen, Alexander W, Karl O Moene, Siv-Elisabeth Skjeltbred, and Bertil Tungodden (2017). “The merit primacy effect”. In:
- Cherry, Todd L, Peter Frykblom, and Jason F Shogren (2002). “Hardnose the dictator”. In: *The American Economic Review* 92.4, pp. 1218–1221.
- Chugunova, Marina, Andreas Nicklisch, and Kai-Uwe Schnapp (2017). “Redistribution and Production with the Subsistence Income Constraint: a Real Effort Experiment”. In:
- Farkas, Steve, Jean Johnson, Will Friedman, and Ali Bers (1996). *The Values We Live by: What Americans Want from Welfare Reform: a Report from Public Agenda*. Public Agenda.
- Fehr, Ernst and Simon Gächter (2000). “Fairness and retaliation: The economics of reciprocity”. In: *The journal of economic perspectives* 14.3, pp. 159–181.
- (2002). “Altruistic punishment in humans”. In: *Nature* 415.6868, pp. 137–140.
- 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.
- Fong, Christina (2001). “Social preferences, self-interest, and the demand for redistribution”. In: *Journal of Public economics* 82.2, pp. 225–246.

- Fong, Christina, Samuel Bowles, and Herbert Gintis (2005). “10 Reciprocity and the Welfare State”. In: *Moral sentiments and material interests*, p. 277.
- Gilens, Martin (1999). *Why Americans hate welfare*.
- Hecl, Hugh, James Patterson, and John Logan Palmer (1985). *The political foundations of anti-poverty policy*. University of Wisconsin–Madison.
- 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.
- Lamberton, Cait (2013). “A spoonful of choice: How allocation increases satisfaction with tax payments”. In: *Journal of Public Policy & Marketing* 32.2, pp. 223–238.
- Leitner, Sigrid and Stephan Lessenich (2003). “Assessing welfare state change: the German social insurance state between reciprocity and solidarity”. In: *Journal of Public Policy* 23.3, pp. 325–347.
- 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.
- Mettler, Suzanne (2011). *The submerged state: How invisible government policies undermine American democracy*. University of Chicago Press.
- 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.
- Ostrom, Elinor (1990). *Governing the Commons: The Evolution of Institutions for Collective Action: The Political economy of institutions and decisions*. Cambridge: Cambridge University Press.
- Rabin, Matthew (1993). “Incorporating fairness into game theory and economics”. In: *The American economic review*, pp. 1281–1302.
- Sussman, Abigail B and Christopher Y Olivola (2011). “Axe the tax: Taxes are disliked more than equivalent costs”. In: *Journal of Marketing Research* 48.SPL, S91–S101.

A Appendix

A.1 Instructions: TR treatment¹¹

Instructions

General rules for participants

You are now going to participate in an economic experiment. At the end of the experiment you will receive a payment. How much you will earn depends on your activities and partly on the activities of other participants. Therefore, it is important that you read the following explanations carefully.

Please do not talk to each other after the start of the experiment. Please do not try to communicate in any other way, unless you are directly asked to during the experiment. If you have any questions, please let us know by a hand sign. An experimenter will come to you to answer your questions. Not following these rules will lead to the exclusion from the experiment and from all payments. Your decisions in the experiment and your answers in the following questionnaire are **anonymous**. Your identity is revealed only to the experimenter, but your answers can not be matched to your identity.

The experiment consists of **three** parts. **You will first receive the instructions for part one of the experiment.** The instructions for parts two and three will be distributed after completion of the first part.

For the duration of the experiment you are randomly assigned by the computer to a group of two players. Apart from you, your group has one more person. **In all three parts of the experiment you are together with the same person in a group.** In each group, there is a person A and person B. The computer assigns the roles randomly at the beginning of the experiment. The decisions of the other person in your group may have an effect on how much you earn. The decisions of people who are not in your group definitely have no influence on how much you earn.

After completing all three parts of the experiment, your payment for all three parts will be summed up and displayed on the screen in Euro. It will be paid in cash at the end of the experiment. After completing the experiment, please stay in your cubicle until we start paying off. During the payment procedure, please wait in your cabin until you are called to collect your payment individually. No other participant will see how much you have earned. Please bring along all the materials you have received from us to the payment.

Before we start the experiment, we kindly ask you to seal your mobile phone in the envelope with your cubicle number. We will collect the envelopes and will return them back together with your payment at the end of the experiment.

Part 1

In this part, you and the other person in your group can individually solve math problems. You have **30 minutes**. Your income in this part is completely independent of the other participant in your group and depends on how many tasks you solve correctly. The same applies to the other person in your group: The other person's income depends solely on the number of the tasks she will solve correctly and is independent of the number of tasks that you will solve. The tasks which you can solve are addition and subtraction calculations with two three-digit numbers. In subtraction tasks the result can never be negative. If you solve the task correctly, pressing the "Next" button will bring you to the next task. **Please do not use pocket calculators or similar tools.** You and the other person solve the same calculation tasks in the same order. **For each correctly solved task you get 0.05 Euro.**

If you have any questions, please show it with a hand sign.

¹¹ This is a translation of the original German instructions which can be obtained from the authors upon request.

Part 2

In this part of the experiment you can again individually solve computing tasks. You have **30 minutes**. If you solved the task correctly, then pressing the “Next” button will bring you to the next task. **Please do not use pocket calculators or similar tools.**

The tasks which you can solve are addition and subtraction calculations. However, Person A will add or subtract two three-digit numbers and Person B will add or subtract two five-digit numbers. Reminder: At the beginning of the experiment, the computer randomly assigned one participant in the group to be Person A and another participant to be Person B. The number of solved tasks in part 1 had no effect on this. **For each correctly solved task, you receive 0.05 Euro, regardless of whether it is three or five-digit numbers.**

At the end of Part 2, Person A must give away 30% of her earned income. This means that person A's income from Part 2 is 70% of the income from correctly solved tasks. The rest is transferred to Person B and increases the likelihood that she will be able to participate in Part 3 (more details below). That means that as income from Part 2 Person B receives, in addition to her earned income, 30% of the properly solved tasks of Person A.

Part 3

All participants who got more than € 3.20 in Part 2 can participate in Part 3 of the experiment. This means that person A (without the sum transferred to person B) must earn at least € 3.20 in Part 2 in order to participate in Part 3. For person B, this means: if the income from her solved tasks and the transfer payment received from person A together amounts to at least 3.20 Euro, person B can participate in Part 3. If a participant does not participate in Part 3, she must nevertheless stay in the laboratory until the end of Part 3. In Part 3, you can again individually solve calculation tasks. You have **30 minutes**. The tasks you can solve are addition and subtraction calculations of two three-digit numbers (both for Person A and Person B). In subtraction tasks the result can never be negative. If you solve the task correctly, pressing the “Next” button will bring you to the next task. **Please do not use pocket calculators or similar tools.** If both players in the pair participate, they get the same calculation tasks in the same order. **For each correctly solved task, you get 0.10 Euro.**

If you have any questions, please show it with a hand sign.

A.2 Feedback screen

Teil 2

Verbleibende Zeit (sec) 1071

Zur Erinnerung 50% ihrer gelösten Aufgaben geben Sie an das andere Gruppenmitglied ab. Wenn Sie 8.20 Euro oder mehr in Teil zwei verdienen, nehmen Sie am Teil drei teil.	Information zu Teil 2 Anzahl richtig eingegebener Resultate: 5 Anzahl richtig eingegebener Resultate des anderen Gruppenmitglieds: 2
--	---

Bitte lösen Sie die folgende Rechenaufgabe:

897

+

676

=

Eingabe bestätigen

Figure 10: Example of the screen of the taxpayer in the second phase, FB treatment.

DFG Research Group 2104

– Latest Contributions

2017:

Paetzel, Fabian, Lorenz, Jan and Tepe, Markus: Transparency diminishes framing-effects in voting on redistribution: Some experimental evidence. Working Paper Nr. 2017-06. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-06.pdf>

Schwaninger, Manuel, Neuhofer, Sabine and Kittel, Bernhard: Offers Beyond the Negotiating Dyad: Including the Excluded in a Network Exchange Experiment. Working Paper Nr. 2017-05. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-05.pdf>

Kittel, Bernhard, Neuhofer, Sabine and Schwaninger, Manuel: Need-based Justice in Social Exchange Networks. Working Paper Nr. 2017-04. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-04.pdf>

Diederich, Adele and Wyszynski, Marc: Need, framing, and time constraints in risky decision making. Working Paper Nr. 2017-03. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-03.pdf>

Kittel, Bernhard, Kanitsar, Georg and Traub, Stefan: Knowledge, Power, and Self-interest. Working Paper Nr. 2017-02. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-02.pdf>

Traub, Stefan and Krügel, Jan Philipp: Risk Taking and the Welfare State: Some Experimental Evidence. Working Paper Nr. 2017-01. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2017-01.pdf>

2016:

Guo, Lisa, Trueblood, Jennifer S. and Diederich, Adele: Thinking Fast Increases Framing Effects in Risky Decision-making. Working Paper Nr. 2016-04. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2016-04.pdf>

Paetzel, Fabian and Sausgruber, Rupert: Entitlements and loyalty in groups: An experimental study. Working Paper Nr. 2016-03. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2016-03.pdf>

Nicklisch, Andreas, Grechenig, Kristoffel and Thöni, Christian: Information-sensitive Leviathans. Working Paper Nr. 2016-02. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2016-02.pdf>

Greiff, Matthias and Paetzel, Fabian: Less sensitive reputation spurs cooperation: An experiment on noisy reputation systems. Working Paper Nr. 2016-01. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2016-01.pdf>

2015:

Schramme, Thomas: The metric and the threshold problem for theories of health justice: A comment on Venkatapuram. Working Paper Nr. 2015-05. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2015-05.pdf>

Nicklisch, Andreas, Grechenig, Kristoffel and Thöni, Christian: Information-sensitive Leviathans – the emergence of centralized punishment. Working Paper Nr. 2015-04. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2015-04.pdf>

Schramme, Thomas: Setting limits to public health efforts and the healthisation of society. Working Paper Nr. 2015-03. <http://bedarfsgerechtigkeit.hsu-hh.de/dropbox/wp/2015-03.pdf>



**FOR
2104**

DFG Research Group 2104 at Helmut Schmidt University Hamburg
<http://needs-based-justice.hsu-hh.de>