December 2021

Fondazione Eni
Enrico Mattei

Working Paper

033.2021

Decreasing Incomes Increase Selfishness

Nickolas Gagnon, Riccardo D. Saulle, Henrik W. Zaunbrecher

Decreasing Incomes Increase Selfishness

By Nickolas Gagnon, Aarhus University
Riccardo D. Saulle, University of Padova
Henrik W. Zaunbrecher, Maastricht University

Summary

We use a controlled laboratory experiment to study the causal impact of income de-creases within a time period on redistribution decisions at the end of that period, in an environment where we keep fixed the sum of incomes over the period. First, we inves-tigate the effect of a negative income trend (intra-personal decrease), which means a decreasing income compared to one's recent past. Second, we investigate the effect of a negative income trend relative to the income trend of another person (inter-personal decrease). If intra-personal or inter-personal decreases create dissatisfaction for an individual, that person may become more selfish to obtain compensation. We formal-ize both effects in a multi-period model augmenting a standard model of inequality aversion. Overall, conditional on exhibiting sufficiently-strong social preferences, we find that individuals indeed behave more selfishly when they experience decreasing in-comes. While many studies examine the effect of income inequality on redistribution decisions, we delve into the history behind one's income to isolate the effect of income changes.

Address for correspondence:
Riccardo D. Saulle
Department of Economics and Management
University of Padova
Via Ugo Bassi 1, 35131 Padova, Italia
E-mail address: riccardo.saulle@unipd.it

Decreasing Incomes Increase Selfishness

Nickolas Gagnon ⓡ Riccardo D. Saulle ⓡ Henrik W. Zaunbrecher *

December 20, 2021

Abstract

We use a controlled laboratory experiment to study the causal impact of income decreases within a time period on redistribution decisions at the end of that period, in an environment where we keep fixed the sum of incomes over the period. First, we investigate the effect of a negative income trend (intra-personal decrease), which means a decreasing income compared to one's recent past. Second, we investigate the effect of a negative income trend relative to the income trend of another person (inter-personal decrease). If intra-personal or inter-personal decreases create dissatisfaction for an individual, that person may become more selfish to obtain compensation. We formalize both effects in a multi-period model augmenting a standard model of inequality aversion. Overall, conditional on exhibiting sufficiently-strong social preferences, we find that individuals indeed behave more selfishly when they experience decreasing incomes. While many studies examine the effect of income inequality on redistribution decisions, we delve into the history behind one's income to isolate the effect of income *changes*.

Keywords: Income Inequality; Income Change; Social Preferences; Social Comparison; Income Redistribution

JEL Classification: C91, D31, D63

---

1   Introduction

The carriers of value or utility are changes rather than final asset positions

– Kahneman and Tversky (1979), Prospect Theory: An Analysis of Decision under Risk

Heterogeneous income growth is a central issue of our times (Milanovic, 2016; Piketty, 2014). In the United States between 1980 and 2004, pre-tax real incomes increased by 121% for the richest 10%, 42% for the richest 10–50%, 7% for the richest 50–80%, and decreased by 25% for the poorest 20% (Piketty et al., 2018).[1] Concurrently, although economists make understanding the determinants of redistribution a priority (Alesina and Giuliano, 2009), the possible consequences of income changes for redistributive decisions have not been isolated. Loss aversion to past income and aversion to unequal income trends could both make an individual more selfish. Whether this is indeed the case is difficult to establish because income changes are intertwined with potential confounds. For instance, a person whose income decreased from $70,000$ USD per year to $50,000$ USD while his or her peer group's income stays at $70,000$ USD faces: a negative income trend ($-20,000$ USD), a negative relative income trend ($-20,000$ USD), a new income level ($50,000$ USD) and a new inequality level relative to the peer group ($-20,000$ USD). Drawing on the tradition of controlled laboratory experiments isolating the influence of income inequality on redistribution (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), our study offers the first evidence that income decreases indeed affect redistribution decisions, even after controlling for one's absolute income and for income inequality.

We designed a laboratory experiment, detailed in Section 2, in which individuals complete real-effort tasks in several distinct periods corresponding to our treatments. Every period contains two sub-periods, in each of which participants receive an exogenously-assigned income that we call "wage" for completing an individual task ("wage" denotes

---

[1]Comparable pictures emerge in other countries, more moderate in Europe and more extreme in Asia (Alvaredo et al., 2017). Some of this heterogeneity is spatial: several major industrial hubs experienced striking declines in average household incomes since the 1970s, e.g., Buffalo (-23%) and Detroit (-35%) (Hartley, 2013). Other differences in the evolution of incomes are based on education (Goldin and Katz, 2007), gender (Blau and Kahn, 2017), and ethnicity (Bayer and Charles, 2018).

a participant's income in a sub-period). The treatments that we implement vary the intra-personal and inter-personal income changes faced by two matched participants in a period by changing the wages between the first and second sub-periods. Furthermore, participants' income is taxed over the period and, at the end of the period, each of the two participants individually decides how the money deducted from both participants should be redistributed among them. We then implement one redistribution decisions per matched pair of participants. Crucially, each treatment manipulates income trends within a period while keeping constant the sum of incomes of each participant over that period before redistribution takes place at the end of the period.

Our contention that income decreases can influence redistribution decisions is rooted in the large number of studies documenting reference dependence, loss aversion, and inequality aversion. The first stream of literature behind our study is the literature on inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), which posits theoretically and shows empirically that individuals dislike that their income differs from the inter-personal reference point formed by the income of other individuals, especially falling behind the income of others.[2] They redistribute from themselves to others when their income is larger than others', and, especially, from others to themselves when their income is lower. It is possible that other inter-personal reference points exist, such as the change in income experienced by other individuals or, in other words, other individuals' income trend. Individuals would then take more from others to avoiding experiencing a disadvantageous change in their income relative to the change in others' income, i.e., to avoid facing a negative relative trend.

The literature on inequality aversion often studies consequences of this aversion in other domains where income changes and income inequality are weaved together. For instance,

---

[2]In this paper, we focus on disadvantageous inequality aversion for two reasons. First, disadvantageous inequality aversion is assumed to be stronger in these models and a large number of empirical studies have gathered evidence supporting its existence. Second, our research is closely linked to research on loss aversion, which considers that individuals are especially sensitive to losses. The literature on inequality aversion is quite rich, and includes empirical studies using observational methods (Clark and Oswald, 1996; Solnick and Hemenway, 1998; Luttmer, 2005), natural experiments (Kuhn et al., 2011; Card et al., 2012), field experiments (Cohn et al., 2014; Breza et al., 2017; Dube et al., 2019) as well as laboratory experiments (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) and experiments with the general population (Bellemare et al., 2008).

Card et al. (2012), Cohn et al. (2014), Breza et al. (2017), and Dube et al. (2019) all study the effect of learning about wage inequality on labor decisions, where a worker faces simultaneously a change in believed or actual income inequality and a new level of inequality.[3] Nevertheless, inequality aversion studies are not crafted to isolate consequences of individuals comparing themselves to others in terms of income changes, notably for redistribution decisions.

Not separating income inequality from the relative income trend can potentially bias the estimates of the effects of inequality aversion. Take a person whose previously equally-paid peer receives a salary increase of $3,000$ USD while his or her own income stays constant. In addition to a new income level, this person faces both (1) a higher inequality level relative to the peer ($-3,000$ USD) and (2) a negative relative income trend ($-3,000$ USD). Provided that the income level has a limited effect on altruism, if a person becomes more selfish both when facing disadvantageous inequality and when facing a negative relative income trend, this will positively bias our estimate of the effect of income inequality on altruism. The bias could also turn out to be negative: consider a person whose peer is paid $5,000$ USD less than he or she is and has to make a redistributive decision involving this peer. We might find that the person has a limited willingness to redistribute to the peer, but this decision potentially also captures a negative effect of a negative relative income trend on altruism.

The second stream of literature behind our study is the research on reference-dependent preferences, which has long modeled that individuals experience a disutility when falling behind an intra-personal reference point (Kahneman and Tversky, 1979; Tversky and Kahneman, 1991; Kőszegi and Rabin, 2006). Most relevant to our study is the empirical research suggesting that the behavior of workers is consistent with a dislike for falling behind their own past income (DellaVigna et al., 2017; Cohn et al., 2015).[4] Loewenstein and Sicherman (1991) also reports that individuals prefer increasing wage profiles to equivalent

---

[3]Another example is Kuhn et al. (2011), which analyzes changes in consumption after one's neighbor wins at a lottery. This situation entails a change as well as a new level of local income inequality.

[4]There is also evidence for other forms of reference dependency, such as workers disliking to fall behind their earnings goals (Camerer et al., 1997; Crawford and Meng, 2011) and behind their earnings expectations (Mas, 2006; Abeler et al., 2011).

decreasing ones.[5] In a similar vein, self-reported well-being is lower when one's living standard decreases over time (Clark et al., 2008; Senik, 2009). In terms of redistribution decisions, if individuals take their own past income as their intra-personal reference point, it is conceivable that they redistribute more from others to themselves to avoid as much as possible to fall behind their past income or, in other words, to avoid or decrease the extent of a negative income trend. Nevertheless, none of the current studies analyzes the impact of income decreases on redistribution decisions.

Similarly as for the relative income trend, failing to separate income inequality from the absolute income trend can potentially bias the estimates of the effects of inequality aversion. Take the case of a person whose income falls by $7,000$ USD, but whose previously equally-paid peer keeps the same income. He or she faces both (1) a higher inequality level relative to the peer ($-7,000$ USD) and (2) a negative relative income trend ($-7,000$ USD). The person becoming more selfish both when facing disadvantageous inequality and when facing a negative absolute income trend will then appear more selfish than if he or she had faced disadvantageous inequality alone.

Post experiment, we formalize in Section 3 our intuitions regarding the effect of changing income trends on redistribution. We combine two prominent models in a multi-period setting: (i) the inequity aversion of Bolton and Ockenfels (2000), which is a one-period model, and (ii) the reference dependence of Kőszegi and Rabin (2006), in the form employed by DellaVigna et al. (2017), among others, which is loss aversion relative to previous income. Our model considers two types of inequality aversions: inequality aversion in income and inequality aversion in income trends. That is, we assume that individuals not only care about relative incomes, but also about relative income trends. Furthermore, we assume that they are averse to a negative income trend, meaning that they suffer a specific utility loss when their incomes are lower than their past incomes. In addition to providing a more solid underpinning for our pre-experiment predictions (Section 4), the model offers an additional testable insight: decreasing incomes can only negatively affect the generosity

---

[5]The same effect has been shown for other contexts such as experiences (Ross and Simonson, 1991), environmental outcomes (Guyse et al., 2002), and health (Chapman, 1996).

of individuals who are at least minimally generous in the absence of income trends. This is important because most participants in our experiment never share anything. The reason is straightforward: fully selfish individuals have no scope for becoming more selfish (and almost fully selfish ones have little scope for doing so).

Key to our investigation is the causal relationship that we obtain between wage decreases and redistribution decisions, which is provided by the laboratory experiment. This methodology offers three main advantages. First, we randomly assign income trends, which precludes that individuals experiencing different trends do so because of different underling characteristics. Second, crucially, we can study absolute and relative income changes within a time period without changing the sum of incomes and income inequality over that period. That is, we separate the role of absolute and relative income decreases from the role of absolute income and the role of disadvantageous income inequality. This is usually impossible with observational data because income trends change the levels of income inequality, making it difficult to distinguish the effect of income inequality and income trends, e.g., individuals with the same income who experience different income trends also automatically experience a higher level of income inequality. Third, in each period of the experiment, we can provide well-defined and salient absolute income trends and relative income trends for each individual. That is, each period, a participant earns two wages in subsequent steps, providing a natural absolute income trend, and is matched with one other participant also earning two wages in the same subsequent steps, so that the difference between one's own income trend and the other's income trend provide a natural relative income trend. In contrast, with observational data, it is often not as obvious to determine the starting and ending points for calculating income trends and to select the relevant reference group. Thus, our experiment provides a controlled environment where we can isolate the effect of absolute and relative income decreases.

Our results, provided in Section 5, are as follows. For the sample as a whole (including selfish individuals), we report that, at a given absolute income and inequality level, there is a negative effect of decreasing incomes on generosity, but the effect is significant only

at marginal levels (10%). In line with our model, we find strong evidence of this negative effect for individuals with social preferences (i.e., those who exhibit sufficiently-strong disadvantageous inequality aversion). The overall effect is significant for all socially-inclined participants considered together. When we consider participants in the two roles of the experiment separately, the effect is significant for those who earn relatively more (High Earners), although, in our preferred robust specification, it is not for those who earn relatively less (Low Earners). Importantly, this is not dependent on the specific cutoff point that we use for defining participants with social preferences. When it comes to the types of decreases, inter-personal decreases have a significant effect on the combined participants as well as High and Low Earners considered separately, and the effect of strictly intra-personal decreases is generally not significant. There is evidence that inter-personal decreases have a stronger effect than strictly intra-personal decreases, but the difference is only significant at marginal levels.

Even though the time span of a period in our experiment is quite limited (8 minutes), the estimated average effect of income decreases is economically substantial for socially-inclined participants: they share approximately 12% (1/4 of a standard deviation) less with their matched participant.[6] Given that previous studies show that elicited social preferences map to political support for redistributive policies (e.g., Fisman et al., 2017; Kerschbamer and Müller, 2020; Epper et al., 2020; Almås et al., 2020), our results suggest that, at least for inequality-averse individuals, declining absolute or relative incomes could contribute to the support for or opposition to redistributive policies.[7] Income-trend inequality aversion can reinforce the positive effect of inequality aversion on working-class individuals' support for redistribution when the rich's income increases, but it can also work the other way, e.g., it can push the inequality averse rich to oppose redistribution to prevent that others' income

---

[6]While this is a modest effect, it is larger, for instance, than gender differences in generosity found in standard Dictator Games (1/10 of a standard deviation, meta-analysis of Bilén et al., 2021).

[7]While the degree of inequality aversion is moderate in our experiment, student samples provide a lower bound for the extent of inequality aversion in the population (Snowberg and Yariv, 2021). Social preferences elicited in experiments have also been shown to correlate with behavior outside of those experiments in other domains, such as loan repayments (Karlan, 2005), donations and other pro-social behaviors (Benz and Meier, 2008; Baran et al., 2010; Franzen and Pointner, 2013), work productivity (Cohn et al., 2015), and socially-responsible investments (Riedl and Smeets, 2017).

levels catch up to theirs. We conclude the paper with a brief discussion in Section 6.

2   Changing Income Trends and Measuring Redistribution

We designed an experiment consisting of five periods corresponding to five treatments. In a period, two participants are anonymously matched together. A period consists of two sub-periods in each of which participants perform a real-effort task for a wage and it ends with a redistribution decision. Note that we employ the term "wage" to refer to a participant's income from a sub-period.

The effort task in every sub-period is to reduce the size of four circles on the computer screen until they disappear. This is done by repeatedly clicking on a circle with the mouse while it moves across the screen. Only one circle appears at the time, and each click on it slightly decreases its size. A new circle appears once a circle completely disappears.[8] Participants have four minutes to complete the task, which can be completed easily by exerting a reasonable effort (most participants take approximately two minutes to finish the task). They are provided with a countdown and with a record of how many circles they have completed so far. Figure 1 provides a screenshot of the task as experienced by participants in sub-period 2. Note that, in the experiment, we call circles "balls" and that we provide participants with the reminder that they need to reach a "Ball Threshold" of four to indicate that they need to make four circles disappear in order to earn the wage.

At the start of a period, participants are informed of their wage for the first sub-period a few seconds before the first sub-period starts. During a period, they monitor how their own wage and the wage of their matched partner evolve over the two sub-periods. This information is visualized through one graph exhibiting one's own wages and the wages of the other participant over the period up to the current sub-period. That is, participants see the wages in the current sub-period, and if they are in the second sub-period, they also see the wages from the previous sub-period. In addition to providing a screenshot of the

---

[8]Participants are not paid more if they complete more than four circles. The task is a modified version of the one in Cacault and Grieder (2016).
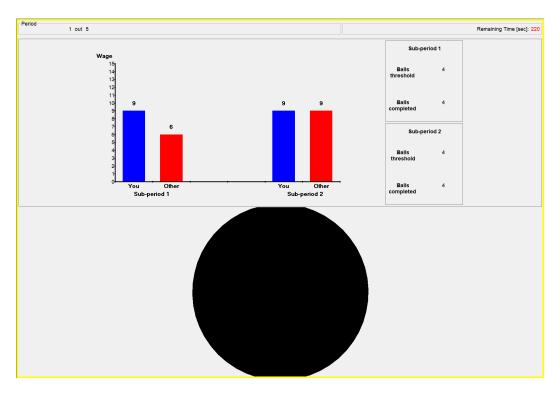
Figure 1: Screenshot of task in sub-period 2 (CATCHINGUP treatment)

task, Figure 1 also shows how participants observe the evolution of wages in sub-period 2. The screen presents the wage information from the first sub-period on the left part of the graph, and from the second sub-period on the right part of the graph. Participants have an additional minute to rest in between the two sub-periods. A few seconds before the second sub-period starts, they are informed about any wage changes that occur between the sub-periods.

The participants are paid the two wages of a period—one for each of the two sub-periods—if and only if they complete the task in both sub-periods.[9] A third of each wage that they earn during the two sub-periods is taken from them and placed in a joint account. That is, the joint account contains a third of the two wages of one participant, and a third of

---

[9] A participant is paid nothing for a period if the task in one of the two sub-periods is not completed. However, we set the wage high enough relative to the effort required for the task so that this only affected 2 out of 298 participants. We excluded these participants and their matched participants because the matched participants could see that those did not complete the task. Therefore, a total of four participants were dropped for the data analysis.

the two wages of the other participant.[10]

At the end of each period, the two participants individually propose how to distribute the money contained in the joint account, which has been collected from their wages for the two sub-periods. This is implemented through a dictator game with role uncertainty. That is, one of the two choices is randomly chosen to count. The money in the joint account always amounts to 11 Euro. Participants can keep the entire joint account for themselves, transfer its content to the other participant, or chose any in-between allocation in increments of 10 cents.[11] To further distinguish the different periods, participants then take a two-minute break before the next period starts. At the end of the experiment, one of the two participants' choices from one period is randomly chosen to count for payment.

Overall, the taxation and redistribution in the design boil down to a proportional income taxation followed by a redistribution decision and we use a flat income tax rate for convenience. This is similar to experiments where individuals allocate an jointly produced earnings (e.g., Konow, 2000; Cappelen et al., 2007) and related to experiments using proportional income taxation to determine what is to be redistributed (e.g., Sausgruber et al., 2021). In practice, this corresponds to situations where a public authority collects revenues in proportion to income and individuals decide how to redistribute those revenues.

We chose a within-subject design in order to increase statistical power (Bellemare et al., 2016). We employ five treatments that we implement for each participant over the five periods. The treatments vary the wage changes faced by participants between the first and second sub-period of a period. A participant is always in the role of either the High Earner or the Low Earner. Participants are not informed that they stay in their role. In the first role (High Earner), a participant always experiences advantageous income inequality over the period. That is, the sum of the two wages in a period is always 18 Euro. Similarly, the Low Earner always faces disadvantageous income inequality over the period—the sum of

---

[10]To ease the explanation for participants, we phrase the parts of the income taken as taxes and the distribution decisions as a redistribution of taxes collected. We collect a fix percentage of income to make it easier for participants to understand and calculate how much was taken from them.

[11]Figure B.1 in Appendix B provides a screenshot showing how the redistribution decision is presented to participants.

the two wages is always 15 Euro. This allows us to maintain the same income inequality over the period in all treatments, such that income inequality cannot explain any treatment differences.[12]

Table 1 details the wages of Low Earners and High Earners over the two sub-periods in the five treatments. The order of treatments is randomized.[13] As we are interested in studying individual responses to absolute and relative wage decreases, we designed the following treatments varying wages changes. While those treatments do not cover all possible wage changes, they do provide several types of declining wages, as well as one set of stable wage and one increasing wage profile.

In STABLE, the wage of each participant remains constant—9 Euro for the High Earner and 7.5 Euro for the Low Earner—in the two sub-periods. In INTRADECREASE and IN-TRAINCREASE, both participants face either an absolute wage increase or decrease. In INTRADECREASE, the wage decreases from 13.5 to 4.5 Euro for the High Earner and from 12 to 3 Euro for the Low Earner. INTRAINCREASE uses the same values but participants now receive the lower wage first. This allows us to study the effects of intra-personal wage changes. In INTRAINTERCHANGE, the Low Earner experiences a wage decrease while the High-Earner experiences a wage increase. The High Earner experiences the same increase as in INTRAINCREASE—from 4.5 to 13.5 Euro—whereas the Low Earner experiences the same decrease as in INTRADECREASE—from 12 to 3 Euro. The wage changes are therefore both absolute and relative for the two participants. In CATCHINGUP, the wage of the Low Earner increases—from 6 to 9 Euro—while the wage of the High Earner is constant at 9 Euro. That is, the High Earner encounters a relative wage decrease—the Low Earner is "catching up" with the High Earner.

Therefore, STABLE, INTRADECREASE, INTRAINCREASE provide environments in which

_____

[12]We found it natural to create income inequality over each period because income inequality is common outside of the laboratory. A possible alternative design is to impose income equality over the period in all treatments. The hypotheses presented in Section 4 would be the same under that alternative.

[13]There are 120 possible orders (5!). We overly sample from a random subset of the orders due to a software problem. That is, approximately 70% of the orders are randomly drawn from a random subset of 30 orders—the subset itself is a random selection from the 120 orders—and the remaining 30% is randomly drawn from the 90 other orders. Conducting the data analysis separately for each of those two sub-samples qualitatively provides the same results.
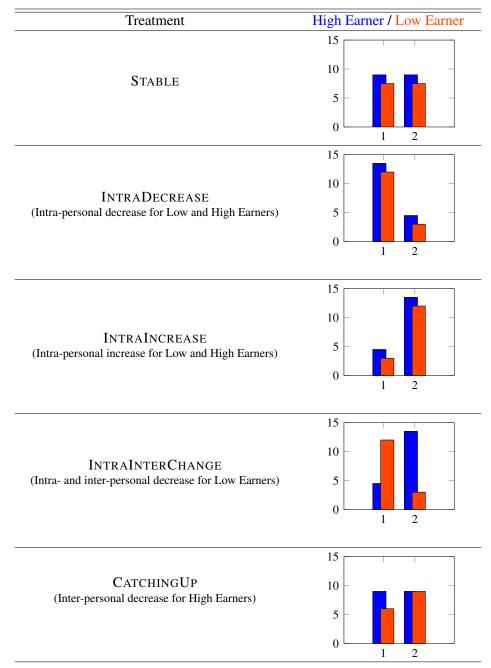
Table 1: Overview of Income Trends in Treatments

Treatment	High Earner / Low Earner
STABLE	
INTRADECREASE (Intra-personal decrease for Low and High Earners)	
INTRAINCREASE (Intra-personal increase for Low and High Earners)	
INTRAINTERCHANGE (Intra- and inter-personal decrease for Low Earners)	
CATCHINGUP (Inter-personal decrease for High Earners)	

Wages are indicated on the vertical axis in Euro, and the two sub-periods of a period are indicated on the horizontal axis. Income inequality is constant over the period: income is 15 Euro for the Low Earner and 18 Euro for the High Earner.

relative wages and income inequality are held constant, isolating the effect of absolute wage changes. INTRAINTERCHANGE still holds income inequality over the period constant, but, by interchanging the sub-period wages of the Low Earner compared to INTRAINCREASE, we obtain an environment with a large relative wage change—a decrease for the Low Earner and an increase for the High Earner—in the same direction as the absolute wage change. CATCHINGUP provides an environment where the High Earner only experiences a relative wage change—a decrease—while the absolute wage remains constant, thus allowing us to isolate the effect of a relative wage change in absence of an accompanying absolute wage change.

Before starting the experiment, the experimenter reads the instructions aloud and participants are provided with a written copy detailing all steps of the experiment.[14] After reading the instructions, participants complete comprehension questions, and help is provided if needed. They also go through a practice period, which is a shorter version of a real period, so that they become familiar with the proceedings of a period. This practice period includes the task and the redistribution decision, but does not count for payment. In it, each participant has the same wage, which stays constant of the two sub-periods. Participants are informed that they are paired with a participant in the same laboratory session in each period.

We designed the experiment using the software z-Tree (Fischbacher, 2007). It was conducted at the BEElab (Behavioural and Experimental Economics Laboratory) of Maastricht University in May-October 2019. Our sample consists of 294 participants recruited over 16 sessions via the online recruitment software ORSEE (Greiner, 2015).[15] The experiment lasted for about 90 minutes and participants earned 16.50 Euro on average.

---

[14]We provide the original instructions in Appendix A.

[15]See footnote 6 explaining that four additional participants are not counted in our sample.

3 The Model

We build our model by combining (i) the inequity aversion model of Bolton and Ockenfels (2000) (henceforth BO) and (ii) the reference dependence model of Kőszegi and Rabin (2006) (henceforth KR), in the form employed by DellaVigna et al. (2017), which is loss aversion to previous income. Before we describe the model, transparency requires us to mention that, although we sketched its foundations for advancing pre-experiment predictions in Section 4, we only formalized it after conducting the experiment.[16] Therefore, we do not test this model, but we rather employ it to better organize and enrich our ideas regarding the effect of income trends on redistribution decisions. Importantly, the model predicts that decreasing income trends should only affect the subset of individuals who are sufficiency altruistic, a feature that we subsequently use in Section 5.

We consider the following multi-period model with two individuals. In each period, each individual receives an income. We denote by $y_i \in \mathbb{R}_+$ the final income gained by individual i over all periods, and by $t_i \in \mathbb{R}_+$ the individual's final income trend generated by the streams of wages over the periods. In particular, t_i takes negative values when the trend is decreasing and positive values otherwise. We assume that the individual has personal concerns, specifically (i) final income over the periods and (ii) final income trend, and social concerns, specifically (iii) relative final income over the periods and (iv) relative final income trend. Formally, individual i's utility consists of four components: (i) *material payoff motivation function* $u_i(y_i)$, (ii) *trend gain-loss function* $\mu_i(t_i)$, (iii) *relative income motivation function* $\sigma_i(y_i, y_j)$, and (iv) *relative trend motivation function* $\tau_i(t_i, t_j)$. We describe the individual's utility as

$$U_i(u_i, \mu_i, \sigma_i, \tau_i). \tag{1}$$

We impose the restriction that (1) satisfies the following assumptions:

---

[16]See Bolton et al. (2005) for an example of post-hoc formalization.

A0: $u_i(x)$, $\sigma_i(x)$ and $\tau_i(x)$, *are continuous and twice differentiable for all x.*

A1: $\mu_i(t_i)$ *is continuous, differentiable for* $t_i \neq 0$ *with* $\mu_i(0) = 0$, *and strictly increasing.*

A2: *If* $z > x > 0$, *then* $\mu(z) + \mu(-z) < \mu(x) + \mu(-x)$.

A3: $\mu_i(t_i)$ *is concave for* $t_i > 0$ *and convex for* $t_i < 0$.

A4: $\lim_{t \to 0} \frac{\mu'(-|t_i|)}{\mu_i'(|t_i|)} \equiv \lambda > 1$.

A5: $u_i(y_i)$ *is increasing and concave in* y_i.

A6: σ_i *is increasing and concave in* y_i *with a maximum at* y_j.

A7: τ_i *is increasing and concave in* t_i *with a maximum at* t_j.

We make assumption **A0** for mathematical convenience. Assumptions **A1–4** recast *reference dependence* and *loss aversion* as in KR. Finally, assumption **A5–7** are in the spirit of *narrow self interests* and *comparative effect* in BO.

In our application, the material payoff motivation function is represented by

$$u_i(y_i) = y_i$$

which satisfies **A0** and **A5**. In line with KR and thus with assumption **A1–4**, we specify the trend gain-loss function as

$$\mu_i(t_i) = \begin{cases} \eta t_i & \text{if } t_i < 0 \\ 0 & \text{if } t_i \geq 0 \end{cases}$$

with $\eta \in [0,1)$. Next, we write the relative income and trends motivation functions as follows

$$\sigma_i(y_i, y_j) = \left(y_i - \frac{1}{2}(y_i + y_j) \right)^2$$

$$\tau_i(t_i, t_j) = \left(t_i - \frac{1}{2}(t_i + t_j) \right)^2$$

where we take $\frac{1}{2}(y_i + y_j)$ and $\frac{1}{2}(t_i + t_j)$ to be the reference points, which is similar in form to BO and satisfies assumptions **A0** and **A5–7**. Finally, we consider the following utility specification

$$U_i(y_i, y_j, t_i, t_j) = \begin{cases} a_i(y_i + \eta t_i) - b_i \left[\left(\frac{1}{2}(y_i - y_j) \right)^2 + \left(\frac{1}{2}(t_i - t_j) \right)^2 \right] & \text{if } t_i < 0 \\ a_i y_i - b_i \left[\left(\frac{1}{2}(y_i - y_j) \right)^2 + \left(\frac{1}{2}(t_i - t_j) \right)^2 \right] & \text{if } t_i \geq 0 \end{cases} \tag{2}$$

where $a_i, b_i > 0$ are the agent's sensitivity to personal concerns and social concerns, respectively. We interpret a_i/b_i as the agent's type, i.e., the ratio of weights attributed to the personal and social components of the motivation function.[17] Note that our model reduces to a BO-type motivation function when $t_i = t_j = 0$. Furthermore, the first two components of $U_i(u_i, \mu_i, \sigma_i, \tau_i)$ replicate the reference-dependence framework of KR. Specifically, the reference point in one's own trend is imposed to be zero in μ_i, i.e., the individual experiences an extra disutility from his/her own trend being negative, while there is no extra utility from one's trend being positive. This is similar to DellaVigna et al. (2017), which assumes loss aversion to previous income. Our utility function therefore reduces to a KR-type function when a participant does not care about social comparisons ($b_i = 0$)

The Dictator Game

Our experimental design boils down to a Dictator Game at the end of an economy with 2 sub-periods[18]. This is therefore the game that we study here. At end of each sub-period, each individual receives a wage. Let $W_i > 0$ be individual i's sum of wages over the two sub-periods. We denote by Δ_i the difference between the wage in the second sub-period

---

[17]BO's a_i/b_i is the ratio of weight placed on pecuniary and relative components, but the motivation function excludes our trend gain-loss function and our relative trend motivation function.

[18]For this specific application to the situation that we study with our experiment, we call each time period a sub-period instead of a period to be consistent with the wording in the rest of the paper. A period in the experiment consists of two sub-periods over which a treatment is administered and at the end of which a redistribution decision is made.

and the wage in the first sub-period. In the Dictator Game, individual i, who plays the role of the dictator, chooses the share $1 - s$ ($0 \leq s \leq 1$) of a tax account $T > 0$ to give to another individual j who is in the role of the recipient and does not make any decisions (T is independent of everything else). This additional amount modifies the wage in the second sub-period, which affects income levels as well as income trends. In light of our model, the final income of the dictator over the two periods is therefore given by $y_i = W_i + s$ and the final income trend by $t_i = \Delta_i + s$. Note that we use the term "final" because we are concerned about the income and the income trend at the very end of the timeframe, i.e., after the redistribution decision. For the recipient, the final income and final income trend are given by $y_j = W_j + 1 - s$ and $t_j = \Delta_j + 1 - s$. Therefore, the utility function $U_i^D(y_i, y_j, t_i, t_j)$ of the dictator takes the form

$$
U_i^D(.) = \begin{cases}
a_i(W_i + sT + \eta(\Delta_i + sT)) \\
\quad - b_i\left[\left(\tfrac{1}{2}(W_i - W_j + 2sT - T)\right)^2 + \left(\tfrac{1}{2}(\Delta_i - \Delta_j + 2sT - T)\right)^2\right] & \text{if } \Delta_i + sT < 0 \\
a_i(W_i + sT) \\
\quad - b_i\left[\left(\tfrac{1}{2}(W_i - W_j + 2sT - T)\right)^2 + \left(\tfrac{1}{2}(\Delta_i - \Delta_j + 2sT - T)\right)^2\right] & \text{if } \Delta_i + sT \geq 0
\end{cases}
$$

The thick red curve in Figure 2 panel (a) presents an example of the dictator's utility function, U_i^D, for our model. In contrast, the dashed black curve in the same panel illustrates the BO-style model, where we remove our trend gain-loss function and our relative trend motivation function. Figure C.1 in Appendix C provides additional examples of U_i^D for different agent types.

The following provides optimality conditions for $U_i^D(y_i, y_j, t_i, t_j)$.

Statement 1. *Define $\mathbb{H} \equiv W_i - W_j - 2T - 3\Delta_i - \Delta_j$. Then the function $U_i^D(y_i, y_j, t_i, t_j)$ is*

16

maximized at s such that:*

$$\arg\max U_i^D(y_i,y_j,t_i,t_j) = \begin{cases} s^* = \frac{a_i}{b_i}\frac{1}{4T} + \frac{1}{2} + \frac{(W_j-W_i)}{4T} + \frac{(\Delta_j-\Delta_i)}{4T} & if\ \frac{a_i}{b_i} > \mathbb{H} \\ s^* = \frac{a_i}{b_i}\frac{(1+\eta)}{4T} + \frac{1}{2} + \frac{(W_j-W_i)}{4T} + \frac{(\Delta_j-\Delta_i)}{4T} & if\ \frac{a_i}{b_i} < \frac{\mathbb{H}}{1+\eta} \\ s^* = -\frac{\Delta_i}{T} & if\ \frac{\mathbb{H}}{1+\eta} \le \frac{a_i}{b_i} \le \mathbb{H} \end{cases}$$

The proof, shown in Appendix C, relies on the fact that $U_i^D(y_i,y_j,t_i,t_j)$ is a piecewise function consisting of two concave functions.

The thick red curve in Figure 2 panel (b) depicts an example of the optimal taking choice s^* corresponding to $U_i^D(y_i,y_j,t_i,t_j)$ when $\Delta_j = -5$ and Δ_i varies from -10 to 10. Specifically, when $t_i = \Delta_i + sT$ is negative, the trend gain-loss component of the model bites and the consequent disutility induces the dictator to keep more for him/herself by choosing a higher s^*. As $t_i = \Delta_i + sT$ becomes positive, inequality aversion in trends $\sigma_i(t_i,t_j)$ decreases s^*. The dashed black curve represents the BO prediction, which is not affected by variations in trends.

(a) $\Delta_j = \Delta_i = -5$ (b) $\Delta_j = -5$
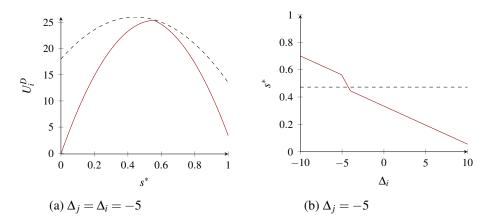
Figure 2: Examples of U_i^D and s^* for $a_i/b_i = 4$, $W_i = 10$, $W_j = 5$, $T = 9$, and $\eta = 0.8$. The thick line indicates our model predictions and the dashed line indicates BO predictions.

As outlined earlier, the functional form in (2) allows us to express individual heterogeneity through an individual type, which we take as the ratio a_i/b_i. The purely selfish individual is characterized by the limiting case $a_i/b_i \to \infty$, which implies $s^* = 1$. At the

other extreme, the purely altruistic individual is represented by $a_i/b_i = 0$. However, the value of s^* for the altruistic individual depends on relative incomes and trends. Since those two relative terms can be positive or negative, s^* can deviate from an equal share. A similar argument applies to any type of non fully selfish individual. Next, we characterize corner and interiors solutions in terms of individual types by providing an upper bound $\mathbb{U}$ and a lower bound $\mathbb{L}$.

Statement 2. *Define $\mathbb{U} \equiv W_i - W_j + \Delta_i - \Delta_j + 2T$ and $\mathbb{L} \equiv (W_i - W_j + \Delta_i - \Delta_j - 2T)(1 + \eta)^{-1}$. Then,*

$$\arg\max U_i(u_i, \mu_i, \sigma_i, \tau_i) = \begin{cases} s^* = 1 & \text{if } \frac{a_i}{b_i} \geq \mathbb{U} \\ s^* = 0 & \text{if } \frac{a_i}{b_i} \leq \mathbb{L} \\ s^* \in (0,1) & \text{if } \mathbb{L} < \frac{a_i}{b_i} < \mathbb{U} \end{cases}$$

In addition to income inequality aversion, two behavioral phenomena coexists in our model: loss aversion in one's own trend and inequality aversion in relative trends. The following two propositions disentangle the two separate effects.

Proposition 1. *Suppose $\Delta_i = \Delta_j$. Then s^* (weakly) increases in $[0,1]$ when Δ_i decreases.*

The thick red curve in Figure 3 panel (a) illustrates Proposition 1 with an example of how s^* varies when we change an individual's trend while keeping other factors constant. There is a region where the trend ($t_i = \Delta_i + sT$) becomes negative and s^* increases. As the trend becomes positive, the model reduces to the BO-type model, which is displayed by the dashed black curve.[19] We show in the next proposition that, given any individual trend, a lower relative trend decreases the dictator's sharing, i.e., s^* increases when Δ_j increases.

Proposition 2. *Suppose Δ_i is constant. Then s^* (weakly) increases in $[0,1]$ when Δ_j increases.*

The thick red curve in Figure 3 panel (b) illustrates Proposition 2 with an example of s^* when we vary the other individual's trend, keeping everything else constant. In this case,

---

[19]Recall that the trend being positive means that $t_i > 0$ (not $\Delta_i > 0$) because the choice of s affects the trend.

our prediction reduces to the BO prediction when the relative trend is zero. The difference is that the dictator shares less (more) to compensate a negative (positive) relative trend.

(a) $\Delta_i = \Delta_j$ (b) $\Delta_i = 0$
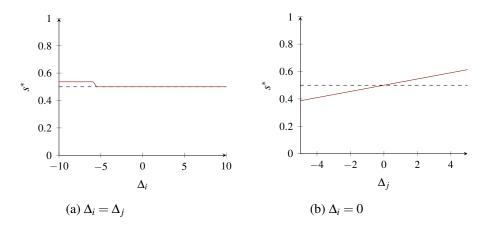
Figure 3: An example of s^* for $a_i/b_i = 2$ $W_i = 12$, $W_j = 10$, $T = 11$ and $\eta = 0.8$. The thick line indicates our model predictions and the dashed line indicates BO predictions.

4   Hypotheses

We posit that individuals with decreasing incomes experience disutility and take from others to be compensated. Specifically, we assume two forms of disutility: (i) intra-personal disutility created by a negative trend, and (ii) inter-personal disutility created by a negative relative trend. We formalized this idea in Section 3. In this section, we provide hypotheses for each role in the experiment: one for High Earners that combines the effect of a negative absolute and a relative trend and three for Low Earners that delineate the specific effects of a negative and of a relative income trend. In Section 5, whenever possible, we also look at aggregate effects for High and Low Earners together, but we did not specify an aggregate hypothesis beforehand. Keep in mind that, before redistribution takes places, High Earners face the same sum of incomes and income inequality over a period in every treatment, and that the same is true for Low Earners.

Hypothesis 1. *High Earners give less in* INTRADECREASE *and* CATCHINGUP *than in* STABLE, INTRAINCREASE, *and* INTRAINTERCHANGE.

Reason: High Earners experience a negative absolute trend INTRADECREASE and a negative relative trend in CATCHINGUP, which should both reduce giving compared to STABLE, INTRAINCREASE, and INTRAINTERCHANGE, where they experience neither type of negative trend. Giving in INTRADECREASE and CATCHINGUP is therefore jointly higher than in the other three treatments. The hypothesis is in line with Propositions 1 and 2 and combines the effects of absolute and relative negative trends.

We did not specify more detailed hypotheses for High Earners. This is because it was not clear-cut before the experiment which, if any, of the negative absolute or relative trend would have a larger effect on giving. That is, we could not predict any directional differences between the treatments CATCHINGUP and INTRADECREASE.[20]

Hypothesis 2. *Low Earners give less in* INTRAINTERCHANGE *and* INTRADECREASE *than in* STABLE, INTRAINCREASE, *and* CATCHINGUP.

Reason: Low Earners encounter a combined negative absolute and relative trend in INTRAINTERCHANGE and a negative absolute trend in INTRADECREASE, which should both reduce giving compared to STABLE, INTRAINCREASE, and CATCHINGUP where there is no negative trend of any sorts. Giving in INTRAINTERCHANGE and INTRADECREASE is therefore jointly higher than in the other three treatments. This is again in line with Propositions 1 and 2.

Hypothesis 3. *Low Earners give less in* INTRAINTERCHANGE *than in* INTRADECREASE, STABLE, INTRAINCREASE, *and* CATCHINGUP.

Reason: Low Earners face a combined negative absolute and relative trend in INTRAINTERCHANGE, which should reduce giving compared to INTRADECREASE, STABLE, INTRAINCREASE, and CATCHINGUP where there is either no negative trend or a negative absolute trend. Giving in INTRAINTERCHANGE is therefore higher than in the other four treatments. This is consistent with Proposition 2.

---

[20]In our model, whether one effect is larger than the other depends on an agent's sensitivity to personal concerns, a_i, to social concerns, b_i, and the parameter μ in the gain-loss function.

Hypothesis 4. *Low Earners give less in* INTRAINTERCHANGE *than in* INTRADECREASE.

Reason: Low Earners face a combined negative absolute and relative trend in INTRAIN-TERCHANGE, which should reduce giving compared to INTRADECREASE, where they only experience a negative absolute trend. Giving in INTRAINTERCHANGE is therefore higher than in INTRADECREASE. This is consistent with Proposition 2.

5   Estimating the Effect of Decreasing Incomes

We first provide summary statistics regarding the amounts shared in the different treatments. Then we proceed to test the effect of decreasing wages within a period on giving at the end of that period. We make a distinction between the full sample and participants with sufficiently-strong social preferences, whom our framework predicts to be the only ones to become less generous when experiencing decreasing wages (see Statements 1 and 2 in Section 3).

Summary Statistics

Table 2 presents the average amount from the 11-Euro joint account that participants give to the other participant. We provide the data for all participants and then for those participants with sufficiently-strong social preferences, i.e., who give a minimum amount in the baseline treatment with constant wages (STABLE). We use a minimum amount of 2 Euro, but the results are robust to using different cutoff points.[21] Overall, mean giving is 1.53 Euro (SD = 2.23 Euro; N = 1,470 observations, 294 participants) or 14% of the joint account for the entire sample, and 4.12 Euro (SD = 1.93 Euro; N = 455 observations, 91 participants) or 37% of the joint account for those with social preferences. High Earners and Low Earners exhibit similar mean giving (for all participants, High Earners give 1.57

---

[21]We chose a cutoff point that denotes a sufficiently-strong generosity in the absence of absolute or relative wage changes, but provide tests of our hypotheses using different cutoffs to show that this specific cutoff does not drive the results. Patterns in descriptive statistics are similar if we instead take cutoffs of 3, 1 and even 0.25 Euro (see Table D.1). Cutoffs higher than 2 Euro yield samples that are smaller and less suitable for separate analyses of the behavior of High Earners and Low Earners (e.g., 81 participants for 3 Euro (44 High Earners and 37 Low Earners) and 69 participants for 4 Euro (38 High Earners and 31 Low Earners)).

Table 2: Summary statistics
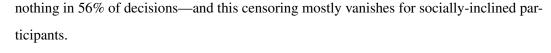
| Treatment | Amount given | | | |
| | All participants | | Participants with social preferences | |
	High Earners Mean (SD)	Low Earners Mean (SD)	High Earners Mean (SD)	Low Earners Mean (SD)
STABLE	1.62	1.49	4.74	4.54
	(2.35)	(2.18)	(1.46)	(1.60)
INTRADECREASE	1.57	1.48	3.86	4.14
	(2.42)	(2.15)	(2.17)	(1.99)
CATCHINGUP	1.41	1.58	3.61	4.19
(Inter decrease for High Earners)	(2.20)	(2.15)	(1.98)	(1.92)
INTRAINTERCHANGE	1.62	1.42	4.16	3.71
(Intra & inter decrease for Low Earners)	(2.34)	(2.03)	(2.01)	(1.99)
INTRAINCREASE	1.63	1.45	4.29	3.99
	(2.39)	(2.13)	(1.90)	(2.09)
N	147	147	48	43
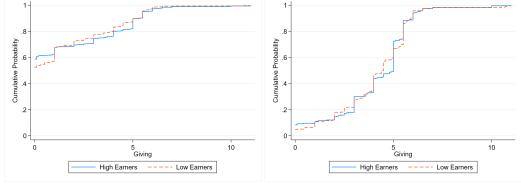
Note: Participants could give any amount between 0 and 11 Euro from the 11-Euro joint account. The remaining amount was credited to their own account. Participants with social preferences are those exhibiting sufficiently-strong advantageous inequality aversion, i.e., they give at least 2 Euro in the baseline treatment STABLE. Table D.1 shows that other cutoff points such as at least 3 Euro, 1 Euro, and 0.25 Euro in STABLE provide similar pictures.

Euro (SD = 2.13 Euro; N = 147) and Low Earners give 1.49 (SD = 2.02; N = 147); t-test and Mann-Whitney-Wilcoxon rank test p-values $\geq$ 0.599; the difference is smaller and p-values larger for socially-inclined participants). The giving patterns are more pronounced for participants with social preferences: High Earners are less generous in CATCHINGUP and in INTRADECREASE; Low Earners are less generous in INTRAINTERCHANGE.

Compared to dictator games in general, average giving in our experiment is on the lower side of the spectrum found in the literature (see meta-study by Engel (2011) and comment by Zhang and Ortmann (2014); average giving in the dictator game is 28.3%). Factors present in our study can reduce generosity, namely using a student sample, endowing recipients, repeating the game, dictators earning the money that they can redistribute, and the option of taking money from others, although having deserving recipients who earned the money that the dictator redistributes can increase giving. Moreover, role uncertainty about who will give and who will receive has also been shown to increase pro-sociality in dictator games (Iriberri and Rey-Biel, 2011).

Figure 4 shows the cumulative distribution of the amounts given, for High Earners and Low Earners, separately for the entire sample and for those participants with social preferences. There is extensive lower-bound censoring in the full sample—participants give

nothing in 56% of decisions—and this censoring mostly vanishes for socially-inclined participants.

(a) All participants

(b) Participants with social preferences

Figure 4: Cumulative distribution functions of giving

Tests

Table 3 and 5 shows the estimated effect of the wage decrease treatments on giving, keeping income and income inequality constant, for All Earners and separately for High Earners and Low Earners, respectively. The results are presented for all participants and for participants with sufficiently-strong social preferences. We leverage the statistical power of our within-subject design by using Tobit Random Effects regressions (Tobit RE) for the heavily-censored full sample, and Random Effects regressions (RE) for participants with sufficiently-strong social preferences—both with standard errors clustered at the individual level. Appendix D Table D.2 shows that results from Table 3 and 5 are very similar with Fixed Effects (FE) regressions for participants with sufficiently-strong social preferences, and Appendix Table D.3, D.4, and D.5 show that the RE results are also very similar if we use alternative cutoff points for defining participants with social preferences (giving at least 3 Euro, 1 Euro, and 0.25 Euro in STABLE).

The dependent variable is giving in Euro and the independent variables are as follows. Since we often test joint restrictions in our hypotheses (e.g., Hypothesis 1 is that

High Earners' giving in the two treatments with decreasing absolute or relative incomes is jointly higher than in the other three treatments), we create independent variables grouping the different types of decreasing incomes. First, the dummy variable *Any decrease* takes value 1 for all treatments with decreasing incomes for both High and Low Earners (INTRADECREASE and CATCHINGUP for Higher Earners and INTRADECREASE and IN-TRAINTERCHANGE for Low Earners) and 0 otherwise. Second, the dummy variable *Any strictly intra decrease* takes values 1 for all treatments with only intra-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. Third, the dummy variable *Any inter decrease* takes value 1 for all treatments with inter-personal decreasing incomes (CATCHINGUP for High Earners and INTRAIN-TERCHANGE for Low Earners[22]) and 0 otherwise. When we use individual treatments as independent variables, they appear as dummy variables with their name (e.g., CATCHIN-GUP) taking value 1 for observations in that treatment and 0 otherwise. We include period dummies, and a dummy for High Earners when we employ all Earners in the regression. Remember that a role (High or Low Earner) always has the same absolute income over a period and that all treatments create the same income inequality over a period, so that our analysis keeps those two factors constant.

Importantly, for socially-inclined participants, we present the results including and excluding the STABLE treatment. This matters because including STABLE in the regression could create a spurious negative relationship between the decreasing treatments and STA-BLE. For instance, suppose that participants' differences in giving choices across treatments simply consist of noise: they randomly give 0 or 2 Euro for every decision. Then, if we compare the other treatments to STABLE only for participants with social preferences (defined as those who give at least 2 Euro in STABLE where there is no wage trend), those participants will mechanically appear less generous in the other treatments. Reassuringly, our results show that, while excluding STABLE qualitatively affects the size of the estimates, it has minor impacts on our conclusions.

---

[22]INTRAINTERCHANGE combines intra- and inter-personal decreases for Low Earners.

Table 3: Regression tests of the effect of decreasing wages on giving—All Earners

	(1) Tobit RE	(2) Tobit RE	(3) RE	(4) RE	(5) RE	(6) RE
	All Participants		Participants with Social Preferences			
			All Earners			
Any decrease	−0.176*		−0.483***	−0.331***		
	(0.105)		(0.117)	(0.122)		
Any strictly intra decrease		−0.055			−0.301**	−0.159
		(0.133)			(0.118)	(0.134)
Any inter decrease		−0.295**			−0.660***	−0.502***
		(0.133)			(0.172)	(0.166)
Wald test *p*-value		0.142			0.050	0.054
Constant	−0.446	−0.441***	4.484***	4.199***	4.491***	4.208***
	(0.426)	(0.425)	(0.202)	(0.248)	(0.201)	(0.246)
Without STABLE				✓		✓
Period dummies	✓	✓	✓	✓	✓	✓
High Earners dummy	✓	✓	✓	✓	✓	✓
N	1470	1470	455	364	455	364

Note: Giving is indicated in Euro. Participants with social preferences are those exhibiting sufficiently-strong advantageous inequality aversion, i.e., they give at least 2 Euro in the baseline treatment STABLE. Appendix Table D.3, D.4, and D.5 show that the results are very similar if we use alternative cutoff points for defining participants with social preferences (giving at least 3 Euro, 1 Euro, and 0.25 Euro in STABLE). The dummy variable *Any decrease* takes value 1 for all treatments with decreasing incomes (INTRADECREASE and CATCHINGUP for Higher Earners and INTRADECREASE and INTRAINTERCHANGE for Low Earners) and 0 otherwise. The dummy variable *Any strictly intra decrease* takes values 1 for all treatments with only intra-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. Similarly, the dummy variable *Any inter decrease* takes value 1 for all treatments with inter-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. All 5 periods and therefore all treatments are included in the regression, except when we indicate that we remove the STABLE treatment (which leaves us with 4 periods). We indicate *p*-values from Wald test on the equality of the coefficients of *Any strictly intra decrease* and *Any inter decrease*. The coefficient of the High Earners dummy is always far from the 10% significance threshold. Appendix Table D.2 also shows that results are very similar with Fixed Effects regressions for participants with sufficiently-strong social preferences. Standard errors clustered at the individual level are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

To complement this analysis, Table 4 presents pairwise comparisons of treatments using non-parametric Wilcoxon signed-rank tests roughly equivalent to several tests in Table 3 and 5. We write "roughly" because they only use two periods of our data to make pairwise comparisons of treatments instead of using more periods and combining similar treatments, therefore not taking advantage of the five periods of observations, which reduces statistical power. Their advantage is that they do not assume normally distributed error terms. The conclusions from those tests are similar to those using regressions, but the results are not always significant. We indicate in footnotes whenever differences arise.

We make three sets of observations regarding the analyses reported in Table 3 and 5. First, in Table 3, when considering both types of Earners together, any decrease in income (*Any decrease*)—at a constant level of income and income inequality over a period—

Table 4: Wilcoxon signed-rank tests of the effect of decreasing wages on giving

		(1)	(3)	(4)	(5)
				All Participants	
		STABLE	INTRAINCREASE	OTHERNODECREASE	INTRADECREASE
All Earners	INTRADECREASE	0.042	0.268	0.050	
	OTHERDECREASE	0.058	0.147	0.026	
	N	294	294	294	
High Earners	INTRADECREASE	0.145	0.172	0.198	
	CATCHINGUP	0.045	0.064	0.053	0.455
	N	147	147	147	147
Low Earners	INTRADECREASE	0.173	0.813	0.140	
	INTRAINTERCHANGE	0.439	0.727	0.202	0.968
	N	147	147	147	147
		(6)	(7)	(8)	(9)
				Participants with Social Preferences	
		STABLE	INTRAINCREASE	OTHERNODECREASE	INTRADECREASE
All Earners	INTRADECREASE	<0.001	0.643	0.245	
	OTHERDECREASE	<0.001	0.014	0.008	
	N	91	91	91	
High Earners	INTRADECREASE	0.001	0.216	0.268	
	CATCHINGUP	<0.001	0.032	0.087	0.309
	N	48	48	48	48
Low Earners	INTRADECREASE	0.004	0.536	0.618	
	INTRAINTERCHANGE	0.001	0.176	0.037	0.241
	N	43	43	43	43

Note: We conduct pairwise comparisons of treatments. OTHERDECREASE denotes CATCHINGUP for High Earners and INTRAINTER-CHANGE for Low Earners; OTHERNODECREASE denotes INTRAINTERCHANGE for High Earners and CATCHINGUP for Low Earners.

reduces giving at marginally significant levels for the full sample and at highly significant levels for participants with social preferences (columns 1, 3, and 4).[23] For participants with social preferences, the effect amounts to approximately -0.5 Euro; relative to their mean giving of 4.12 (SD = 1.93) Euro, this corresponds to 1/4 of a standard deviation. Once we separate any strictly intra-personal decreases (*Any strictly intra decrease*) and any inter-personal decreases (*Any inter decrease*) (columns 2, 5, and 6), we observe that only inter-personal decreases have a significant impact in the full sample and in our robust specification excluding the STABLE treatment for participants with social preferences (the effect of *Any strictly intra-personal decreases* is significant under our basic specification). The table's Wald tests indicate that the difference between any strictly intra-personal decreases and any inter-personal decreases is not significant in the full sample, and marginally sig-

---

[23]The effect is significant for some pairwise comparisons using rank-based tests on the full sample in Table 4. If we separate Earners by their type, the effect of any decrease is not significant for High and Low Earners alone in the full sample (Table 5 columns 1-3). For socially-inclined participants (Table 5 columns 5-6 and 9-10), the effect is still highly significant for High Earners, and not significant for Low Earners in our preferred robust specification without STABLE.

nificant under our preferred robust specification for social participants (just below the 0.05 significance threshold under our basic specification; columns 2 and 5-6). At the aggregate level, we thus observe the following.

Result 1. ***Aggregate:*** *Among all Earners with social preferences, decreasing incomes significantly reduce average giving compared to non-decreasing incomes; when distinguishing decreases, inter-personal decreases have a significant effect, marginally stronger than that of strictly intra-personal decreases, which themselves have no significant effect. Decreasing incomes have a marginally significant impact in the full sample.*

Second, in Table 5, among socially-inclined High Earners (columns 5–8), the coefficient of the *Any decrease* variable is negative and significant under the specification with STABLE and our preferred robust specification without STABLE (column 5-6). This indicates that intra-personal (INTRADECREASE) and inter-personal (CATCHINGUP) decreases in income jointly lower average giving compared to the other treatments, which confirms Hypothesis 1. The intra-personal (INTRADECREASE) and the inter-personal (CATCHINGUP) decreases in income reduce average giving, at marginal and significant levels, in the robust specification without STABLE (column 8).[24] Although the effect of inter-personal decrease (approx. -0.7 Euro) is qualitatively larger than that of the intra-personal decrease (approx. -0.4 Euro), the Wald tests in the table show that the two are statistically indistinguishable (columns 7-8). Furthermore, with the full sample, and only the effect of inter-personal decrease is marginally significant (columns 1-2). The evidence is in line with Hypothesis 1 for High Earners with social preferences, but not for the full sample. We therefore write our second result as follows.

---

[24]Both effects are significant under our other specification with STABLE. If we use non-parametric tests of Table 4, only CATCHINGUP is significant or marginally significant in pairwise comparisons with the two non-decreasing income treatments other than STABLE.

Table 5: Regression tests of the effect of decreasing wages on giving—Low and High Earners

	(1) Tobit RE	(2) Tobit RE	(3) Tobit RE	(4) Tobit RE	(5) RE	(6) RE	(7) RE	(8) RE	(9) RE	(10) RE	(11) RE	(12) RE	(13) RE	(14) RE
	All Participants				Participants with Social Preferences									
	High Earners		Low Earners		High Earners				Low Earners					
Any decrease	−0.304 (0.187)		−0.104 (0.114)		−0.636*** (0.200)	−0.507** (0.210)			−0.322*** (0.115)	−0.151 (0.120)	−0.094 (0.135)	0.066 (0.167)		
Wald test *p*-value														
IntraDecrease		−0.170 (0.235)		0.012 (0.143)			−0.492*** (0.182)	−0.367* (0.203)			−0.094 (0.135)	0.066 (0.167)		
CatchingUp (Inter decrease for High Earners)		−0.437* (0.236)					−0.781** (0.308)	−0.653** (0.308)						
IntraInterChange (Intra & inter decrease for Low Earners)				−0.219 (0.144)							−0.551*** (0.181)	−0.376** (0.164)	−0.527*** (0.182)	−0.398** (0.168)
Wald test *p*-value		0.355		0.188			0.352	0.349			0.038	0.048		
Constant	−0.902* (0.520)	−0.909* (0.519)	0.194 (0.399)	0.208 (0.399)	4.556*** (0.213)	4.243*** (0.272)	4.547*** (0.215)	4.231*** (0.276)	4.361*** (0.247)	4.133*** (0.271)	4.398*** (0.246)	4.187*** (0.268)	4.378*** (0.248)	4.209*** (0.271)
Without STABLE						✓		✓		✓		✓		✓
Period dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	735	735	735	735	240	192	240	192	215	172	215	172	215	172

Note: Giving is indicated in Euro. Participants with social preferences are those exhibiting sufficiently-strong advantageous inequality aversion, i.e., they give at least 2 Euro in the baseline treatment STABLE. Appendix Table D.3, D.4, and D.5 show that the results are very similar if we use alternative cutoff points for defining participants with social preferences (giving at least 3 Euro, 1 Euro, and 0.25 Euro in STABLE). The dummy variable *Any decrease* takes value 1 for all treatments with decreasing incomes (INTRADECREASE and CATCHINGUP for Higher Earners and INTRADECREASE and INTRAINTERCHANGE for Low Earners) and 0 otherwise. The dummy variable *Any strictly intra decrease* takes values 1 for all treatments with only intra-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. The dummy variable *Any inter decrease* takes value 1 for all treatments with inter-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. All 5 periods and therefore all treatments are included in the regression, except when we indicate that we remove the STABLE treatment (which leaves us with 4 periods). We indicate *p*-values of *Any strictly intra decrease* and *Any inter decrease*, and on the equality of INTRAINTERCHANGE and INTRADECREASE or CATCHINGUP. The coefficient of the High Earners dummy is always far from the from Wald test on the equality of the coefficients of INTRAINTERCHANGE and INTRADECREASE or CATCHINGUP. The coefficient of the High Earners dummy is always far from the 10% significance threshold. Appendix Table D.2 also shows that results are very similar with Fixed Effects regressions for participants with sufficiently-strong social preferences. Standard errors clustered at the individual level are in parentheses. * *p*<0.10, ** *p*<0.05, *** *p*<0.01.

Result 2. *High Earners: Among High Earners with social preferences, decreasing incomes significantly reduce average giving compared to non-decreasing incomes; inter-(*CATCHINGUP*) and intra-personal (*INTRADECREASE*) decreases have significant and marginally significant effects, respectively, but the two are statistically indistinguishable. Only the effect of inter-personal decreases is marginally detectable in the full sample.*

Third, in Table 5, among Low Earners with social preferences (columns 9–14), the *Any decrease* variable is negative, significantly so under the specification with STABLE, but not under the robust specification without STABLE (columns 9-10). This means that absolute and relative decreasing incomes do not jointly reduce giving, which goes against Hypothesis 2. Individually, the joint intra- and inter-personal decrease in incomes (INTRAINTERCHANGE) significantly reduces average giving (approx. -0.4 Euro), but the point estimate of intra-personal decrease (INTRADECREASE) is close to 0 and not significant (columns 11-12). The joint intra- and inter-personal decrease (INTRAINTERCHANGE) also reduces giving significantly compared to all other schemes joined together (columns 13-14), which confirms Hypothesis 3. The accompanying Wald test (columns 11-12) shows that the effect of the joint intra- and inter-personal decrease is significantly larger than the the intra-personal effect, which confirms Hypothesis 4.[25] Moreover, similarly as for High Earners, no effect of any of the two types of income decreases has an effect on Low Earners in the full sample. Overall, for Low Earners with social preferences, the evidence goes against Hypothesis 2, which asserts that both types of decreases would jointly reduce giving. However, the evidence falls in line with Hypotheses 3 and 4, which predict that the joint intra- and inter-personal decrease would lower giving and that its effect would be larger than that of the intra-personal decrease. The full sample supports none of those three hypotheses (columns

---

[25]If we use the non-parametric tests of Table 4, only INTRAINTERCHANGE is significant for Low Earners in one of the pairwise comparisons with the two non-decreasing income treatments other than STABLE. Furthermore, INTRAINTERCHANGE is not significantly larger than INTRADECREASE. Overall, we primarily rely on the RE linear regressions for those tests because (i) a within-subject approach with several observations per individual is statistically much more powerful (Bellemare et al., 2016), (ii) although not strictly normally distributed, giving and regression residuals for participants with social preferences do not visually look too far off (e.g., see Figure 4 (b) for CDF of giving), and (3) as we show in the next subsection, order effects do not appear to influence our findings.

3-4). We thus write our third result as follows.

Result 3. *Low Earners: Among Low Earners with social preferences, decreasing incomes do not significantly reduce average giving compared to non-decreasing incomes. When distinguishing the types of decreases, the joint intra- and inter-personal income decrease (*INTRAINTERCHANGE*) significantly reduces average giving compared to non-decreasing incomes, and the intra-personal decrease (*INTRADECREASE*) does not; the effect of the first is significantly larger than that of the second. No effect is detectable in the full sample.*

To sum up, although income as well as income inequality over a period are the same in each treatment, we find strong evidence of a negative effect of decreasing incomes on giving among participants with social preferences. This is the case for all Earners joined together, and, when we consider the different roles in the experiment separately, for High and Low Earners evaluated separately. Overall, the evidence is strong for inter-personal decreases rendering participants more selfish and weak for intra-personal decreases. However, the difference between the two is itself limited: the inter-personal decrease is stronger than the intra-personal decrease at marginal significance levels for all Earners evaluated together, and right below significance levels for Low Earners.[26] When we consider the full sample—including participants without social preferences—there is limited (marginally significant) evidence for the negative effect of decreasing incomes on giving.

Evidence that an Increasing Relative Income Trend does not Increase Sharing

We did not make a hypothesis about how a positive relative income trend might affect generosity. Nevertheless, our formal model differs with our original intuition on this matter. Specifically, through Proposition 2, it predicts, if anything, a positive effect. We evaluate this possibility by using treatment INTRAINTERCHANGE, where High Earners experience an increasing relative trend. We compare their sharing behavior in INTRAINTERCHANGE relative to INTRAINCREASE and STABLE. Table 6 shows the analyses, first for all High Earners and second for socially-inclined High Earners, without and then with removing the

---

[26]More precisely, the effect of any inter decrease is stronger than that of any strictly intra decrease.

Table 6: Tests of the effect of increasing relative wages on giving for High Earners, excluding treatments with decreasing wages

	(1) RE	(2) RE	(3) RE
	All Participants	Participants with Social Preferences	
	High Earners	High Earners	
INTRAINTERCHANGE	−0.044	−0.365*	−0.150
(Inter increase for High Earners)	(0.108)	(0.191)	(0.198)
Constant	1.920	4.828	4.550
	(0.216)	(0.241)	(0.323)
Baseline is INTRADECREASE & STABLE	✓	✓	
Baseline is INTRADECREASE alone			✓
Period dummies	✓	✓	✓
N	441	144	96

Note: Giving is indicated in Euro. The treatments included in the regression are INTRAINTERCHANGE and the baseline treatment(s) indicated at the bottom of the table. Standard errors clustered at the individual level are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

STABLE treatment for robustness. Overall, we find no evidence that a positive relative trend increases sharing. This gives us an additional fourth result, which does not correspond to a hypothesis.

Result 4. *Relative Income Increase: Among all High Earners and among High Earners with social preferences, the relative increase in incomes (*INTRAINTERCHANGE*) does not significantly alter average giving compared to other non-decreasing incomes.*

Evidence that Order Effects Do not Drive our Results

We provide evidence that order effects do not drive our results by re-running the regressions from Table 3 and 5 concerning the effect of any decrease in income as well as any strictly intra decrease and any inter decrease for participants with social preferences, but this time only using the first two periods and including a dummy for facing a treatment with any income decrease in the first period. Table 7 shows the estimates for High and Low Earners together (columns 1–4), High Earners (column 5-6), and Low Earners (column 7-8). Keeping in mind the lower statistical power, the point estimates and significance levels are similar as for the corresponding Table 3 and 5 estimations; the dummy for starting with any decreasing income is never significant.[27]

---

[27]We do not present estimation of the effect of individual treatments because the number of observations is much reduced by using only 2 periods. Similarly, we use the first two periods instead of only the first period

Table 7: Tests of the effect of decreasing wages on giving, in first two periods while controlling for possible order effect of starting with a decreasing income treatment

	(1) RE	(2) RE	(3) RE	(4) RE	(5) RE	(6) RE	(7) RE	(8) RE
				Participants with Social Preferences				
		All Earners			High Earners		Low Earners	
Period(s)		1st-2nd			1st-2nd		1st-2nd	
Any decrease	−0.832***	−0.560**			−1.033**	−0.692**	−0.556*	−0.456
	(0.275)	(0.265)			(0.457)	(0.284)	(0.297)	(0.385)
Any strictly intra decrease			-0.340	-0.104				
			(0.309)	(0.381)				
Any inter decrease			−1.136***	−0.812***				
			(0.367)	(0.306)				
Wald test p-value			0.075	0.104				
Any decrease in 1st period dummy	0.324	0.250	0.293	0.197	0.470	0.333	0.155	0.206
	(0.348)	(0.432)	(0.350)	(0.439)	(0.471)	(0.562)	(0.513)	(0.629)
Constant	4.580***	4.245***	4.569***	4.229***	4.536***	4.263***	4.396***	4.156***
	(0.203)	(0.264)	(0.202)	(0.260)	(0.221)	(0.284)	(0.313)	(0.389)
Without STABLE		✓		✓		✓		✓
Period dummies	✓	✓	✓	✓	✓	✓	✓	✓
High Earners dummy	✓	✓	✓	✓				
N	182	132	182	132	96	69	86	63

Note: Note: Giving is indicated in Euro. Any decrease denotes INTRADECREASE and CATCHINGUP for High Earners, and INTRADECREASE and INTRAINTERCHANGE for Low Earners. Any strictly intra decrease denotes INTRADECREASE for High and Low Earners. Any inter decrease denotes CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners. All treatments are included in the regression, except when we indicate that we remove the STABLE treatment. We indicate p-values from Wald test on the equality of the coefficients of Any strictly intra decrease and Any inter decrease. Standard errors clustered at the individual level are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Additional Robustness Checks

We show in Appendix Section E that experience in the experiment, e.g., going through the first period, does not stop participants' reaction to decreasing wages in the following periods. We also provide evidence in Appendix Section F that our results are not due to participants misunderstanding the redistribution decisions.

6 Conclusion

In this study, we conducted an experiment designed to identify how income decreases causally affect redistribution behavior. Based on the literature on reference dependence, loss aversion, and inequality aversion, we hypothesized that individuals become more self-ish when they experience a negative absolute or relative income trend. In the full sample, we

because the sample size would otherwise become very small.

found evidence that individuals indeed share less with others after experiencing decreasing incomes, but the effect is only significant at marginal levels.

However, since we study the effect of aversion to decreasing income trends on generosity, we should only expect to observe effects for those individuals who are also inequity averse in outcomes. That is, those who are already fully selfish cannot be made more selfish by manipulating income trends. We indeed found that, for those socially-inclined individuals, a negative income trend reduces generosity, this time at highly significant levels, and independently of the specific cutoff that we employ to define those participants. When we conduct separate analyses by role in the experiment, the effect is significant for those participants who earn relatively more, but, in our robust specification, it is not for those who earn relatively less. Furthermore, inter-personal decreases significantly reduce the generosity of participants, and strictly intra-personal decreases generally do not lower generosity significantly. On the whole, inter-personal decreases appear to have a stronger effect than intra-personal decreases, but the difference is only marginally significant, so that we cannot clearly distinguish the two effects.

Our model can be seen as a first step towards a multi-period extension of Bolton and Ockenfels (2000), which, in the words of its authors "is a theory of "local behavior" in the sense that it explains stationary patterns [...] over a short time span in a constant frame." Specifically, we incorporate inequality aversion in income as well as inequality aversion in income trends. In the fashion of Kőszegi and Rabin (2006), we also assume that individuals are averse to their own trend being negative. Our model reduces to a Bolton and Ockenfels (2000) inequality-aversion model when individuals experience the same positive trends and to a Kőszegi and Rabin (2006) reference-dependence model when individuals experience the same negative trends and income levels. When income trends diverge, our theory differs from Bolton and Ockenfels (2000) in that it prescribes a behavior that we observe in the data: a negative relative trend induces more selfishness.[28] For the purpose of this paper,

---

[28]Conversely, since we model aversion to relative trend inequality similarly to aversion to relative income inequality, our theory also suggests that a positive relative trend induces generosity. This simplifies our theory, but is not supported by the data in our experiment.

we apply our theory to a dictator game, but natural extensions are to study how income trends may affect behavior in other settings such as bargaining problems, risky decisions, and consumer decisions.

The effect of decreasing incomes on generosity that we report also poses interesting possibilities for future research in domains where social preferences are at play. Redistributive policies aiming to close earnings gaps between groups—such as between ethnic minorities vs. whites, and men vs. women—can be met with more resistance by members of the traditionally better off group who are averse to relative income trends inequality and compare their trends to that of the other groups. Aversion to a negative absolute trend can increase the rich's opposition to and the poor's support for fiscal stimuli with redistributive aspects during economic downturns. In the workplace, trend-inequality averse workers might react badly—becoming less productive, less cooperative, engaging in sabotage—when their co-workers face relatively better income profiles over the years.

References

Abeler, J., Falk, A., Goette, L., and Huffman, D. (2011). Reference points and effort provision. *American Economic Review*, 101(2):470–92.

Alesina, A. F. and Giuliano, P. (2009). Preferences for redistribution. *NBER Working Paper No. 14825*.

Almås, I., Cappelen, A. W., and Tungodden, B. (2020). Cutthroat capitalism versus cuddly socialism: Are americans more meritocratic and efficiency-seeking than scandinavians? *Journal of Political Economy*, 128(5):1753–1788.

Alvaredo, F., Chancel, L., Piketty, T., Saez, E., and Zucman, G. (2017). World inequality report 2018. *World Inequality Lab*.

Baran, N. M., Sapienza, P., and Zingales, L. (2010). Can we infer social preferences from the lab? Evidence from the Trust Game. *NBER Working Paper No. 15654*.

Bayer, P. and Charles, K. K. (2018). Divergent paths: A new perspective on earnings differences between black and white men since 1940. *Quarterly Journal of Economics*, 133(3):1459–1501.

Bellemare, C., Bissonnette, L., and Kröger, S. (2016). Simulating power of economic experiments: the powerBBK package. *Journal of the Economic Science Association*, 2(2):157–168.

Bellemare, C., Kröger, S., and Van Soest, A. (2008). Measuring inequity aversion in a heterogeneous population using experimental decisions and subjective probabilities. *Econometrica*, 76(4):815–839.

Benz, M. and Meier, S. (2008). Do people behave in experiments as in the field?—Evidence from donations. *Experimental Economics*, 11(3):268–281.

Bilén, D., Dreber, A., and Johannesson, M. (2021). Are women more generous than men? a meta-analysis. *Journal of the Economic Science Association*, pages 1–18.

35

Blau, F. D. and Kahn, L. M. (2017). The gender wage gap: Extent, trends, and explanations. Journal of Economic Literature, 55(3):789–865.

Bolton, G. E., Brandts, J., and Ockenfels, A. (2005). Fair procedures: Evidence from games involving lotteries. Economic Journal, 115(506):1054–1076.

Bolton, G. E. and Ockenfels, A. (2000). ERC: A theory of equity, reciprocity, and competition. American Rconomic Review, 90(1):166–193.

Breza, E., Kaur, S., and Shamdasani, Y. (2017). The morale effects of pay inequality. Quarterly Journal of Economics, 133(2):611–663.

Cacault, M. P. and Grieder, M. (2016). How group identification distorts beliefs. SSRN Working Paper No. 2601712.

Camerer, C., Babcock, L., Loewenstein, G., and Thaler, R. (1997). Labor supply of New York City cabdrivers: One day at a time. Quarterly Journal of Economics, 112(2):407–441.

Cappelen, A. W., Hole, A. D., Sørensen, E. Ø., and Tungodden, B. (2007). The pluralism of fairness ideals: An experimental approach. American Economic Review, 97(3):818–827.

Card, D., Mas, A., Moretti, E., and Saez, E. (2012). Inequality at work: The effect of peer salaries on job satisfaction. American Economic Review, 102(6):2981–3003.

Chapman, G. B. (1996). Expectations and preferences for sequences of health and money. Organizational Behavior and Human Decision Processes, 67(1):59–75.

Clark, A. E., Frijters, P., and Shields, M. A. (2008). Relative income, happiness, and utility: An explanation for the easterlin paradox and other puzzles. Journal of Economic Literature, 46(1):95–144.

Clark, A. E. and Oswald, A. J. (1996). Satisfaction and comparison income. Journal of Public Economics, 61(3):359–381.

Cohn, A., Fehr, E., and Goette, L. (2015). Fair wages and effort provision: Combining evidence from a choice experiment and a field experiment. Management Science, 61(8):1777–1794.

Cohn, A., Fehr, E., Herrmann, B., and Schneider, F. (2014). Social comparison and effort provision: Evidence from a field experiment. Journal of the European Economic Association, 12(4):877–898.

Crawford, V. P. and Meng, J. (2011). New York City cab drivers' labor supply revisited: Reference-dependent preferences with rational expectations targets for hours and income. American Economic Review, 101(5):1912–1932.

DellaVigna, S., Lindner, A., Reizer, B., and Schmieder, J. F. (2017). Reference-dependent job search: evidence from Hungary. Quarterly Journal of Economics, 132(4):1969–2018.

Dube, A., Giuliano, L., and Leonard, J. (2019). Fairness and frictions: The impact of unequal raises on quit behavior. American Economic Review, 109(2):620–63.

Engel, C. (2011). Dictator games: A meta study. Experimental Economics, 14(4):583–610.

Epper, T., Fehr, E., and Senn, J. (2020). Other-regarding preferences and redistributive politics. University of Zurich Department of Economics Working Paper No. 399.

Fehr, E. and Schmidt, K. M. (1999). A theory of fairness, competition, and cooperation. Quarterly Journal of Economics, 114(3):817–868.

Fischbacher, U. (2007). z-tree: Zurich toolbox for ready-made economic experiments. Experimental economics, 10(2):171–178.

Fisman, R., Jakiela, P., and Kariv, S. (2017). Distributional preferences and political behavior. Journal of Public Economics, 155:1–10.

Franzen, A. and Pointner, S. (2013). The external validity of giving in the dictator game. Experimental Economics, 16(2):155–169.

Goldin, C. and Katz, L. F. (2007). Long-run changes in the wage structure: narrowing, widening, polarizing. NBER Working Paper No. 13568.

Greiner, B. (2015). Subject pool recruitment procedures: organizing experiments with orsee. Journal of the Economic Science Association, 1(1):114–125.

Guyse, J. L., Keller, L. R., and Eppel, T. (2002). Valuing environmental outcomes: Preferences for constant or improving sequences. Organizational Behavior and Human Decision Processes, 87(2):253–277.

Hartley, D. (2013). Urban decline in Rust-Belt cities. Federal Reserve Bank of Cleveland Economic Commentary, 2013-06.

Iriberri, N. and Rey-Biel, P. (2011). The role of role uncertainty in modified dictator games. Experimental Economics, 14(2):160–180.

Kahneman, D. and Tversky, A. (1979). Prospect theory: An analysis of decision under risk. Econometrica, 47(2):263–292.

Karlan, D. S. (2005). Using experimental economics to measure social capital and predict financial decisions. American Economic Review, 95(5):1688–1699.

Kerschbamer, R. and Müller, D. (2020). Social preferences and political attitudes: An online experiment on a large heterogeneous sample. Journal of Public Economics, 182:104076.

Konow, J. (2000). Fair shares: Accountability and cognitive dissonance in allocation decisions. American Economic Review, 90(4):1072–1091.

Kőszegi, B. and Rabin, M. (2006). A model of reference-dependent preferences. Quarterly Journal of Economics, 121(4):1133–1165.

Kuhn, P., Kooreman, P., Soetevent, A., and Kapteyn, A. (2011). The effects of lottery prizes on winners and their neighbors: Evidence from the Dutch postcode lottery. American Economic Review, 101(5):2226–47.

Loewenstein, G. and Sicherman, N. (1991). Do workers prefer increasing wage profiles? Journal of Labor Economics, 9(1):67–84.

Luttmer, E. F. (2005). Neighbors as negatives: relative earnings and well-being. Quarterly Journal of Economics, 120(3):963–1002.

Mas, A. (2006). Pay, reference points, and police performance. Quarterly Journal of Economics, 121(3):783–821.

Milanovic, B. (2016). Global Inequality: A New Approach for the Age of Globalization. Harvard University Press, Cambridge, MA.

Piketty, T. (2014). Capital in the Twenty-First Century Century. Harvard University Press, Cambridge, MA.

Piketty, T., Saez, E., and Zucman, G. (2018). Distributional national accounts: methods and estimates for the United States. Quarterly Journal of Economics, 133(2):553–609.

Riedl, A. and Smeets, P. (2017). Why do investors hold socially responsible mutual funds? Journal of Finance, 72(6):2505–2550.

Ross, W. T. and Simonson, I. (1991). Evaluations of pairs of experiences: A preference for happy endings. Journal of Behavioral Decision Making, 4(4):273–282.

Sausgruber, R., Sonntag, A., and Tyran, J.-R. (2021). Disincentives from redistribution: Evidence on a dividend of democracy. European Economic Review, page 103749.

Senik, C. (2009). Direct evidence on income comparisons and their welfare effects. Journal of Economic Behavior & Organization, 72(1):408–424.

Snowberg, E. and Yariv, L. (2021). Testing the waters: Behavior across participant pools. American Economic Review, 111(2):687–719.

Solnick, S. J. and Hemenway, D. (1998). Is more always better?: A survey on positional concerns. Journal of Economic Behavior & Organization, 37(3):373–383.

Tversky, A. and Kahneman, D. (1991). Loss aversion in riskless choice: A reference-dependent model. Quarterly Journal of Economics, 106(4):1039–1061.

Zhang, L. and Ortmann, A. (2014). The effects of the take-option in dictator-game experiments: a comment on Engel's (2011) meta-study. Experimental Economics, 17(3):414–420.

A Instructions

Instructions

Welcome to this economic experiment. Please read these instructions carefully. In the experiment, your decisions and the decisions of other participants will determine how much money you earn. You will be paid in cash at the end of the session, provided that you follow the rules. If you do not follow the rules, you will not be paid. You are forbidden from using your phone and from communicating with other participants at any time during the session. If you have any questions, please do not hesitate to raise your hand to ask the experimenter for help.

The **session lasts for up to 90 minutes**. You need to stay until the end to be paid. To complete the task in this experiment, you are also **required to be able to click many times with a computer mouse** (e.g., you need to have no injuries to your arms and fingers).

General Instructions

There are **5 periods, each lasting 8 minutes**. **Each period, you are matched with 1 anonymous other person**. This can be any participant in the session. **All participants work on the exact same task**. Each **period consists of 2 sub-periods of 4 minutes**. In each sub-period, you are asked to complete a **task to earn a wage**. You need to complete the task in each of the 2 sub-periods in a period to earn the 2 wages for the period. Over the 2 sub-periods, you observe your wage and the wage of the other participant. Your wage and the wage of the other participant may vary over the 2 sub-periods, but the task stays the same.

In each of the 2 sub-periods, a **tax is deducted from your wage and the wage of the other participant** for this period. The tax is 1/3 of the wage. You are left with the after-tax wages (wage minus tax). At the end of the period, the taxes that are taken from you and the other participant over the 2 sub-periods are joined together into the **Total Tax Collected**.

At the end of each of the 5 periods, you **decide how to distribute the Total Tax Collected between you and the other participant**. The **other participant also decides** how to distribute the Total Tax Collected. The decisions are anonymous. Either your distribution or the distribution of the other participant is randomly chosen to count for payment. If you do not complete the task for 1 of the 2 sub-periods for a period, then you do not earn anything in this period and you cannot choose a distribution in this period. The tax taken from the other participant is returned to him or her and there are no distribution decisions.

Until the end of the experiment, you do not receive feedback regarding what distribution the other participant chooses, and the other participant is not told what distribution you choose. Only at the end of the experiment, **1 of the 5 periods is randomly chosen** to count for payment by the computer.

You are paid the following two parts for the chosen period. First, you are **paid the 2 after-tax wages you earned** in that period. Second, either **your distribution of the Total Tax Collected or the distribution of the other participant** you were matched with for this period is **randomly chosen to be paid**.

You need to answer a few **comprehension questions** and go through a **Practice period** before you start the experiment. In the next two sections, you are given more details regarding specific components of

1

the experiment, and examples of how your wage and the wage of the other participant are presented during the experiment. Finally, a **timeline** summarizing the different steps is presented at the end of these instructions.

<div align="center">**Details**</div>

Other Participant: The other participant **is real and always works on the exact same task as you**. He or she is someone else in the session. At the start of each period, you are matched with a participant for this period. That is, you are matched with the same participant for the 2 sub-periods of this period.

Periods: There are 5 periods, each lasting 8 minutes. Between every period, you have a 2-minute break to relax.

Sub-periods: Within every period, there are 2 sub-periods of 4 minutes each.

Task: The **task is always exactly the same for every participant** in every sub-period. It is a simple task that can be completed by exerting a reasonable effort. The task is to repeatedly click on a ball that appears on your computer screen. Every time you click on the ball, it decreases in size until it disappears. Click on the ball, and wait for it to move until you click again. If you click multiple times before it moves, the ball does not disappear faster. The minimum number of balls that need to disappear (**Balls Threshold**) in each sub-period to earn your wage is **4 balls**. You need to reach this Ball Threshold in each of the 2 sub-periods of a period to earn the 2 wages for the period. This means that if you complete the task in only one of the 2 sub-periods of a period, you do not earn any of the 2 wages for the period. If you want, **you can make more than 4 balls disappear, but this will not change your wage**. That is, you cannot increase your wage by working more than meeting the Balls Threshold.

Wage and Tax: In a period, you earn two wages for completing the task: one wage in each of the two sub-periods. The wage may not be the same in the first sub-period and the second sub-period. **If you do not complete the task in each of the 2 sub-periods, you are not paid at all for this period.** That is, you neither earn the wage of the first sub-period nor the wage of the second sub-period. In each sub-period, a tax of 1/3 is removed from your wage and from the wage of the other participant. The tax always amounts to 1/3 of the wage, both for you and the other participant. You are left with the **after-tax wage** (wage minus tax).

Total Tax Collected: Taxes collected from you and from the other participant in the 2 sub-periods are added at the end of the period into the **Total Tax Collected**.

Distribution: At the end of each period, you choose how to distribute the money in the Total Tax Collected between yourself and the other participant. The other participant also decides how to distribute the Total Tax Collected. Either your distribution or the distribution chosen by the other participant is randomly chosen for payment, for the period randomly selected to be paid out. If you do not complete the task in sub-period 1 or 2 of a period, you cannot choose a distribution. In this case, the tax taken from the other participant is returned to him or her, and there are no distribution decisions. Similarly, if the other participant does not complete the task in one or both of the 2 sub-periods, you and the other

participant do not make a distribution choice, and the tax collected on your wages is returned to you. The distribution decisions are anonymous and you are not informed about any of them before the end of the session.

Payment: At the end of the session, the computer randomly selects 1 period for payment. There are 2 parts to your payment, provided that you have completed the task in the 2 sub-periods of the chosen period. First, you receive your after-tax wage for each of the 2 sub-periods. Second, your distribution or the distribution of the other participant you were matched with in this period is randomly chosen and paid out. The randomly selected distribution is the only one you are informed about during the experiment. **If you do not complete the task in each the 2 sub-periods of this period, you are paid nothing.**

Comprehension Questions: You are asked a few comprehension questions before the experiment starts to make sure that you understand the instructions. If you do not understand a comprehension question, please ask the experimenter for help by raising your hand.
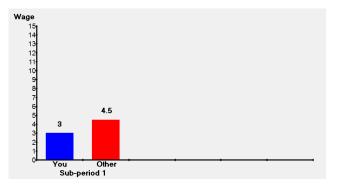
Practice Period: During the Practice period, you try the task and become familiar with the experiment, including the distribution decision. The **practice period is shorter than the regular periods, and does not count for payment**. The experiment starts after the practice period.

Questions: If anything is unclear, please do not hesitate to raise your hand to ask the experimenter.

3
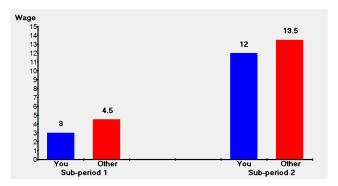
<center>**Examples**</center>
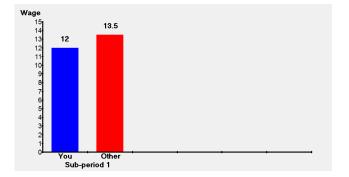
The following are three examples of how your wage and the wage of the other participant you are matched with for a period are indicated in each of the 2 sub-periods of a period.

Example 1: You are matched with a participant for the period. In sub-period 1, your wage is 3 EUR and the wage of the other participant is 4.5 EUR:
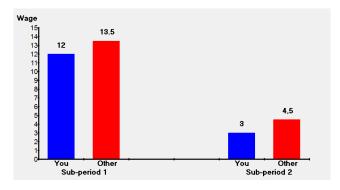
Then, in sub-period 2, your wage increases to 12 EUR and the other participant's wage increases to 13.5 EUR:

<center>4</center>
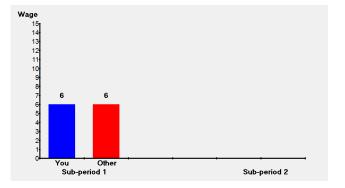
Example 2: You are matched with a participant for the period. In sub-period 1, your wage is 12 EUR and the wage of the other participant is 13.5 EUR:
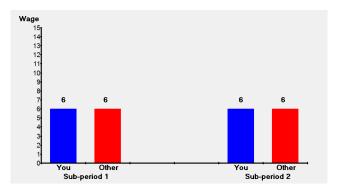
Then, in sub-period 2, your wage decreases to 3 EUR and the other participant's wage decreases to 4.5 EUR:

5
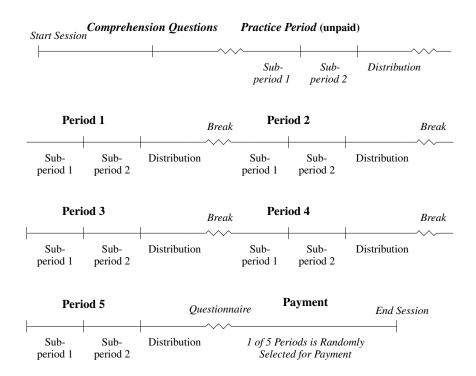
Example 3: You are matched with a participant for the period. In sub-period 1, your wage is 6 EUR and the wage of the other participant is also 6 EUR:

Then, in sub-period 2, your wage stays 6 EUR and the other participant's wage also stays 6 EUR:

6

Timeline

The following timeline summarizes the different steps in this experiment.

Comprehension Questions   *Practice Period* **(unpaid)**

Start Session

Sub-period 1   Sub-period 2   *Distribution*

Period 1   *Break*   **Period 2**   *Break*

Sub-period 1   Sub-period 2   Distribution   Sub-period 1   Sub-period 2   Distribution

Period 3   *Break*   **Period 4**   *Break*

Sub-period 1   Sub-period 2   Distribution   Sub-period 1   Sub-period 2   Distribution

Period 5   *Questionnaire*   **Payment**   *End Session*

Sub-period 1   Sub-period 2   Distribution   *1 of 5 Periods is Randomly Selected for Payment*

We call participants one by one at the end of the session to pay them. Please stay seated in your cubicle while you wait.

7

B Screenshots
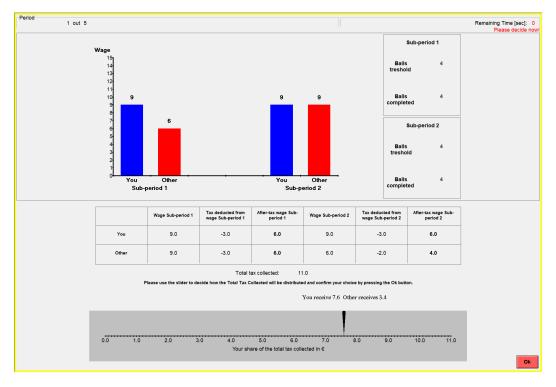
Figure B.1: Screenshot of redistribution stage (CATCHINGUP treatment)

C   Additional Information about the Model

Graphical examples of the dictator' utility function

Figure C.1 presents examples of a dictator's utility, U_i^D, for different agent types (a_i/b_i).
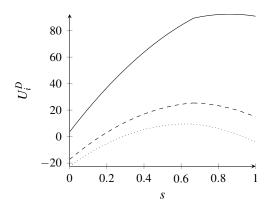
Figure C.1: Examples of U_i^D when $X_1 = 10, X_2 = 15, T = 9, \Delta_i = -6, \Delta_j = -10$, and $\eta = 0.8$. Thick, dashed, and dotted curves represent agent types (a_i/b_i) 6, 2 and 1.

Proof of Statement 1: Let $\overline{U}_i(y_i, y_j, t_i, t_j)$ and $\hat{U}_i(y_i, y_j, t_i, t_j)$ denote the two piece-wise components of the dictator's utility function $U_i^D(y_i, y_j, t_i, t_j)$. That is:

$$\hat{U}_i(.) = a_i(W_i + sT + \eta(\Delta_i + sT)) - b_i\left[\left(\frac{1}{2}(W_i - W_j + 2sT - T)\right)^2 + \left(\frac{1}{2}(\Delta_i - \Delta_j + 2sT - T)\right)^2\right]$$

$$\overline{U}_i(.) = a_i(W_i + sT) - b_i\left[\left(\frac{1}{2}(W_i - W_j + 2sT - T)\right)^2 + \left(\frac{1}{2}(\Delta_i - \Delta_j + 2sT - T)\right)^2\right]$$

Note that $\overline{U}_i(.)$ and $\hat{U}_i(.)$ are both continuous and concave with unique *maxima* at

$$\arg\max \overline{U}_i(y_i, y_j, t_i, t_j) = \frac{a_i}{b_i}\frac{1}{4T} + \frac{1}{2} + \frac{(W_j - W_i)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T}$$

49

and

$$\arg\max \hat{U}_i(y_i,y_j,t_i,t_j) = \frac{a_i}{b_i}\frac{(1+\eta)}{4T} + \frac{1}{2} + \frac{(W_j - W_i)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T}$$

Also note that the piecewise function $U_i^D(y_i,y_j,t_i,t_j)$ has a non-differentiable point at $\bar{s} \equiv -\frac{\Delta_i}{T}$. Because $\hat{U}_i'(\bar{s}) > \overline{U}_i'(\bar{s})$, it holds that $U_i^D(y_i,y_j,t_i,t_j)$ is a concave function as well and thus has a unique maximum. Furthermore, we have that $\overline{U}(s) > \hat{U}(s)$ whenever $s < \bar{s}$ and $\overline{U}(s) \leq \hat{U}(s)$ otherwise. To determine the maximum of U_i^D, we consider the following three cases:

Case (i): $\arg\max \overline{U}_i \geq \bar{s}$. First note that, under Case (i) it cannot be that $\arg\max U_i^D = \arg\max \hat{U}_i$. Indeed, $\overline{U}_i$ is increasing up to some $s^* \geq \bar{s}$ and $\overline{U}_i(s) > \hat{U}_i(s)$ for $s < \bar{s}$. Thus it has to be that either $\arg\max U_i^D = \bar{s}$ or $\arg\max U_i^D = s^*$ for some $s^* > \bar{s}$. The assumption of Case (i), together with the fact that $U_i^D = \overline{U}_i$ for $s \geq \bar{s}$, implies that $\overline{U}_i = \arg\max U_i^D$ whenever $\arg\max \overline{U}_i \geq \bar{s}$.

Finally, the condition $\arg\max \overline{U}_i \geq \bar{s}$ can be rewritten as

$$\frac{a_i}{b_i}\frac{1}{4T} + \frac{1}{2} + \frac{(W_j - W_i)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T} \geq -\frac{\Delta_i}{T} \iff$$
$$\frac{a_i}{b_i} \geq W_i - W_j - 2T - 3\Delta_i - \Delta_j.$$

Case (ii): $\arg\max \hat{U}_i \leq \bar{s}$. Similarly as before, note that under Case (ii) it cannot be that $\arg\max U_i^D = \arg\max \overline{U}_i$. Indeed, for $s > \bar{s}$, $\hat{U}_i$ is decreasing, but we still have that $\overline{U}_i(s) < \hat{U}_i(s)$. Therefore, it has to be that either $\arg\max U_i^D = \bar{s}$ or $\arg\max U_i^D = s^*$ for some $s^* < \bar{s}$. The assumption of Case (ii), together with the fact that $U_i^D = \hat{U}_i$ for $s \leq \bar{s}$, implies that $\arg\max U_i^D = \hat{U}_i$ whenever $\arg\max \hat{U}_i \leq \bar{s}$, where the latter can be rewritten as

$$\frac{a_i}{b_i}\frac{(1+\eta)}{4T} + \frac{1}{2} + \frac{(X_2 - X_1)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T} \leq -\frac{\Delta_i}{T} \iff$$
$$\frac{a_i}{b_i} \leq \frac{W_i - W_j - 2T - 3\Delta_i - \Delta_j}{1+\eta}.$$

Case (iii): $\arg\max \hat{U}_i > \bar{s}$ and $\arg\max \overline{U}_i < \bar{s}$. Since $\hat{U}_i$ and $\overline{U}_i$ are defined in U_i^D respectively for $s < \bar{s}$ and $s \geq \bar{s}$, it must be that $\arg\max U_i^D = \bar{s}$. Indeed, $\hat{U}_i$ is increasing up to $\bar{s}$ and $\overline{U}_i$ is decreasing for $s > \bar{s}$. Finally, the conditions $\arg\max \hat{U}_i > \bar{s}$ and $\arg\max \overline{U}_i < \bar{s}$ can be rewritten as follows:

$$\frac{a_i}{b_i}\frac{(1+\eta)}{4T} + \frac{1}{2} + \frac{(X_2 - X_1)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T} > -\frac{\Delta_i}{T} \iff$$
$$\frac{a_i}{b_i} > \frac{W_i - W_j - 2T - 3\Delta_i - \Delta_j}{1+\eta},$$

and

$$\frac{a_i}{b_i}\frac{1}{4T} + \frac{1}{2} + \frac{(X_2 - X_1)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T} < -\frac{\Delta_i}{T} \iff$$
$$\frac{a_i}{b_i} < W_i - W_j - 2T - 3\Delta_i - \Delta_j.$$

Cases (i), (ii), and (iii) together prove the statement. ∎

Proof of Statement 2: Assume that $\frac{a_i}{b_i} \geq \mathbb{U}$. Taking $\mathbb{U} > \mathbb{H}$ together with Statement 1, it holds that

$$\arg\max U_i^D = s^* = \frac{a_i}{b_i}\frac{1}{4T} + \frac{1}{2} + \frac{(W_j - W_i)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T}$$

To obtain a contradiction, suppose that $s^* < 1$. This implies that

$$\frac{a_i}{b_i}\frac{1}{4T} + \frac{1}{2} + \frac{(W_j - W_i)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T} < 1 \iff$$
$$\frac{a_i}{b_i} < W_i - W_j + \Delta_i - \Delta_j + 2T = \mathbb{U}$$

which is not possible because of the assumption. Thus, for $\frac{a_i}{b_i} \geq \mathbb{U}$, we know that $s^* = 1$.

Next, let's assume that $\frac{a_i}{b_i} \leq \mathbb{L}$. We consider two cases: $\Delta_1 \leq 0$ and $\Delta_i > 0$. In the former

51

case, it holds that $\mathbb{L} \leq \mathbb{H}(1+\eta)^{-1}$. Therefore, by Statement 1, it also holds that

$$\arg\max U_i^D = s^* = \frac{a_i}{b_i}\frac{1+\eta}{4T} + \frac{1}{2} + \frac{(W_j - W_i)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T}$$

To obtain a contradiction, let's assume that $s^* > 0$. It follows that

$$\frac{a_i}{b_i}\frac{1+\eta}{4T} + \frac{1}{2} + \frac{(W_j - W_i)}{4T} + \frac{(\Delta_j - \Delta_i)}{4T} > 0 \Longleftrightarrow$$

$$\frac{a_i}{b_i} > (W_i - W_j + \Delta_i - \Delta_j + 2T)(1+\eta)^{-1} = \mathbb{U} > \mathbb{L}$$

which is not possible because of the assumption.

In the latter case, it holds that $\mathbb{L} > \mathbb{H}(1+\eta)^{-1}$, which, by Statement 1, corresponds to

$$\arg\max U_i^D = s^* = \frac{-\Delta_i}{T}$$

Since $\Delta_i > 0$, s^* can never be strictly positive.

Finally, given the above, it is straightforward to show that the intermediate case yields $s^* \in (0,1)$. ∎

Proof of Proposition 1: Recall that, by Statement 1, s^* depends on the agent type a_i/b_i. For the high and low type ($a_i/b_i > \mathbb{H}, a_i/b_i < \mathbb{H}/(1+\eta)$), the value of s^* doe not depend on Δ_i whenever $\Delta_i = \Delta_j$. For the intermediate case, it holds that $\frac{\partial s^*}{\partial \Delta_i} < 0$. ∎

Proof of Proposition 2: For the high and low type ($a_i/b_i > \mathbb{H}, a_i/b_i < \mathbb{H}/(1+\eta)$), it holds that $\frac{\partial s^*}{\partial \Delta_i} > 0$. For the intermediate case, the value of s^* does not depends on Δ_j. ∎

D Additional Tables

Table D.1: Summary statistics with different cutoffs to determine participants with sufficiently-strong social preferences

| | Amount given, according to social preferences cutoff (minimum giving in STABLE) | | | | | |
| | ≥3 Euro | | ≥1 Euro | | ≥0.25 Euro | |
Treatment	High Earners Mean (SD)	Low Earners Mean (SD)	High Earners Mean (SD)	Low Earners Mean (SD)	High Earners Mean (SD)	Low Earners Mean (SD)
STABLE	4.98 (1.29)	4.91 (1.40)	4.16 (1.91)	3.48 (2.09)	4.09 (1.96)	3.21 (2.17)
INTRADECREASE	4.10 (2.08)	4.65 (1.62)	3.43 (2.33)	3.21 (2.25)	3.38 (2.34)	2.97 (2.28)
INTRAINTERCHANGE	4.36 (1.94)	4.10 (1.85)	3.65 (2.22)	2.97 (2.09)	3.60 (2.23)	2.75 (2.12)
CATCHINGUP	3.78 (1.94)	4.55 (1.75)	3.19 (2.10)	3.38 (2.10)	3.15 (2.10)	3.11 (2.18)
INTRAINCREASE	4.59 (1.66)	4.38 (1.92)	3.77 (2.17)	3.16 (2.23)	3.72 (2.18)	2.93 (2.26)
N	44	37	57	62	58	68

Note: Participants could give any amount between 0 and 11 Euro from the 11-Euro joint account. The remaining amount was credited to their own account. Participants with social preferences are those exhibiting sufficiently-strong advantageous inequality aversion, i.e., they give at least the amount of the cutoff in the baseline treatment STABLE. The main text uses 2 Euro as the cutoff and we present here descriptive statistics for the alternative cutoffs 3 Euro, 1 Euro, and 0.25 Euro.

Table D.2: Tests of the effect of decreasing wages on giving, using Fixed Effects instead of Random Effects for Participants with Social Preferences

	(1) FE	(2) FE	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE	(8) FE	(9) FE	(10) FE	(11) FE	(12) FE	(13) FE	(14) FE
							Participants with Social Preferences							
			All Earners				High Earners				Low Earners			
Any decrease	−0.483*** (0.117)	−0.332*** (0.122)			−0.636*** (0.200)	−0.509** (0.211)			−0.322*** (0.115)	−0.153 (0.120)				
Any strictly intra decrease			−0.301** (0.118)	−0.160 (0.134)										
Any inter decrease			−0.660*** (0.172)	−0.502*** (0.166)										
Wald test p-value			0.053	0.058										
INTRADECREASE							−0.492*** (0.182)	−0.367* (0.203)			−0.094 (0.135)	0.064 (0.167)		
CATCHINGUP							−0.781** (0.308)	−0.657** (0.310)						
INTRAINTERCHANGE											−0.551*** (0.181)	−0.377** (0.164)	−0.527*** (0.182)	−0.399** (0.169)
Wald test p-value							0.357	0.351			0.044	0.055		
Constant	4.475*** (0.144)	4.205*** (0.165)	4.482*** (0.141)	4.215*** (0.160)	4.556*** (0.227)	4.228*** (0.248)	4.547*** (0.232)	4.216*** (0.254)	4.361*** (0.163)	4.139*** (0.209)	4.398*** (0.158)	4.193*** (0.203)	4.378*** (0.159)	4.214*** (0.216)
Without STABLE		✓		✓		✓		✓		✓		✓		✓
Period dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	455	364	455	364	240	192	240	192	215	172	215	172	215	172

Note: Giving is indicated in Euro. Participants with social preferences are those exhibiting sufficiently-strong advantageous inequality aversion, i.e., they give at least 2 Euro in the baseline treatment STABLE. The dummy variable *Any decrease* takes value 1 for all treatments with decreasing incomes (INTRADECREASE and CATCHINGUP for Higher Earners and INTRADECREASE and INTRAINTERCHANGE for Low Earners) and 0 otherwise. The dummy variable *Any strictly intra decrease* takes values 1 for all treatments with only intra-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. Similarly, the dummy variable *Any inter decrease* takes value 1 for all treatments with inter-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. All 5 periods and therefore all treatments are included in the regression, except when we indicate that we remove the STABLE treatment (which leaves us with 4 periods). We indicate p-values from Wald test on the equality of the coefficients of Any strictly intra decrease and Any inter decrease, and on the equality of INTRAINTERCHANGE and INTRADECREASE or CATCHINGUP. There is no need for a High Earners dummy because FE does not incorporate time-invariant variables. Standard errors clustered at the individual level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table D.3: Tests of the effect of decreasing wages on giving, using the alternative definition that participants with social preferences are those giving at least 3 Euro in STABLE

	(1) RE	(2) RE	(3) RE	(4) RE	(5) RE	(6) RE	(7) RE	(8) RE	(9) RE	(10) RE	(11) RE	(12) RE	(13) RE	(14) RE
	Participants with Social Preferences													
	All Earners				High Earners				Low Earners					
Any decrease	−0.483*** (0.128)	−0.338** (0.134)			−0.681*** (0.217)	−0.563** (0.228)			−0.263** (0.114)	−0.069 (0.105)				
Any strictly intra decrease			−0.250** (0.126)	−0.116 (0.144)										
Any inter decrease			−0.707*** (0.189)	−0.553*** (0.180)										
Wald test p-value			0.019	0.021										
INTRADECREASE							−0.495** (0.195)	−0.388* (0.223)			0.028 (0.124)	0.202 (0.155)		
CATCHINGUP							−0.868*** (0.330)	−0.747** (0.326)						
INTRAINTERCHANGE											−0.553*** (0.200)	−0.350** (0.171)	−0.560*** (0.204)	−0.421** (0.186)
Wald test p-value							0.256	0.262			0.017	0.026		
Constant	4.702*** (0.174)	4.377*** (0.236)	4.711*** (0.173)	4.390*** (0.233)	4.791*** (0.181)	4.468*** (0.261)	4.778*** (0.185)	4.451*** (0.266)	4.710*** (0.226)	4.393*** (0.267)	4.765*** (0.219)	4.473*** (0.256)	4.771*** (0.216)	4.543*** (0.261)
Without STABLE		✓		✓		✓		✓		✓		✓		✓
Period dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
High Earners dummy	✓	✓	✓	✓										
N	405	324	405	324	220	176	220	176	185	148	185	148	185	148

Note: Giving is indicated in Euro. Participants with social preferences are those exhibiting sufficiently-strong advantageous inequality aversion, i.e., they give at least 3 Euro in the baseline treatment STABLE. The dummy variable *Any decrease* takes value 1 for all treatments with decreasing incomes (INTRADECREASE and CATCHINGUP for Higher Earners and INTRADECREASE and INTRAINTERCHANGE for Low Earners) and 0 otherwise. The dummy variable *Any strictly intra decrease* takes values 1 for all treatments with only intra-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. Similarly, the dummy variable *Any inter decrease* takes value 1 for all treatments with inter-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. All 5 periods and therefore all treatments are included in the regression, except when we indicate that we remove the STABLE treatment (which leaves us with 4 periods). We indicate p-values from Wald test on the equality of the coefficients of Any strictly intra decrease and Any inter decrease, and on the equality of INTRAINTERCHANGE and INTRADECREASE or CATCHINGUP. The coefficient of the High Earners dummy is always far from the 10% significance threshold. Standard errors are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table D.4: Tests of the effect of decreasing wages on giving, using the alternative definition that participants with social preferences are those giving at least 1 Euro in STABLE

	(1) RE	(2) RE	(3) RE	(4) RE	(5) RE	(6) RE	(7) RE	(8) RE	(9) RE	(10) RE	(11) RE	(12) RE	(13) RE	(14) RE
	Participants with Social Preferences													
	All Earners				High Earners				Low Earners					
Any decrease	−0.376*** (0.097)	−0.280*** (0.104)			−0.532*** (0.177)	−0.427** (0.191)			−0.243** (0.096)	−0.163 (0.106)				
Any strictly intra decrease			−0.237** (0.097)	−0.146 (0.108)										
Any inter decrease			−0.513*** (0.144)	−0.413*** (0.145)										
Wald test p-value			0.068	0.073										
INTRADECREASE							−0.387** (0.164)	−0.285 (0.177)			−0.097 (0.105)	−0.019 (0.129)		
CATCHINGUP							−0.676** (0.264)	−0.575** (0.274)						
INTRAINTERCHANGE											−0.388** (0.154)	−0.307** (0.151)	−0.364** (0.155)	−0.300** (0.151)
Wald test p-value							0.268	0.259			0.109	0.118		
Constant	3.974*** (0.243)	3.745*** (0.267)	3.979*** (0.243)	3.751*** (0.266)	3.982*** (0.258)	3.696*** (0.285)	3.971*** (0.260)	3.683*** (0.288)	3.550*** (0.244)	3.439*** (0.249)	3.569*** (0.246)	3.464*** (0.250)	3.548*** (0.248)	3.458*** (0.256)
Without STABLE		✓		✓		✓		✓		✓		✓		✓
Period dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
High Earners dummy	✓	✓	✓	✓										
N	595	476	595	476	285	228	285	228	310	248	310	248	310	248

Note: Giving is indicated in Euro. Participants with social preferences are those exhibiting sufficiently-strong advantageous inequality aversion, i.e., they give at least 1 Euro in the baseline treatment STABLE. The dummy variable *Any decrease* takes value 1 for all treatments with decreasing incomes (INTRADECREASE and CATCHINGUP for Higher Earners and INTRADECREASE and INTRAINTERCHANGE for Low Earners) and 0 otherwise. The dummy variable *Any strictly intra decrease* takes values 1 for all treatments with only intra-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. Similarly, the dummy variable *Any inter decrease* takes value 1 for all treatments with inter-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. All 5 periods and therefore all treatments are included in the regression, except when we indicate that we remove the STABLE treatment (which leaves us with 4 periods). We indicate p-values from Wald test on the equality of the coefficients of Any strictly intra decrease and Any inter decrease, and on the equality of INTRAINTERCHANGE and INTRADECREASE or CATCHINGUP. The coefficient of the High Earners dummy is always far from the 10% significance threshold. Standard errors clustered at the individual level are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table D.5: Tests of the effect of decreasing wages on giving, using the alternative definition that participants with social preferences are those giving at least 0.25 Euro in STABLE

	(1) RE	(2) RE	(3) RE	(4) RE	(5) RE	(6) RE	(7) RE	(8) RE	(9) RE	(10) RE	(11) RE	(12) RE	(13) RE	(14) RE
						Participants with Social Preferences								
		All Earners				High Earners					Low Earners			
Any decrease	−0.353*** (0.092)	−0.263*** (0.099)			−0.526*** (0.173)	−0.425** (0.185)			−0.214** (0.090)	−0.143 (0.099)				
Any strictly intra decrease			−0.220** (0.092)	−0.135 (0.102)										
Any inter decrease			−0.484*** (0.137)	−0.391*** (0.138)										
Wald test p-value			0.065	0.069										
INTRADECREASE							−0.389** (0.162)	−0.288* (0.172)			−0.074 (0.097)	−0.006 (0.118)		
CATCHINGUP							−0.663** (0.259)	−0.570** (0.269)						
INTRAINTERCHANGE											−0.353** (0.143)	−0.281** (0.140)	−0.334** (0.142)	−0.280** (0.138)
Wald test p-value							0.291	0.271			0.091	0.098		
High Earner dummy	−0.592* (0.354)	−0.527 (0.363)	−0.592* (0.355)	−0.528 (0.364)										
Constant	3.916*** (0.244)	3.697*** (0.265)	3.919*** (0.244)	3.702*** (0.265)	3.931*** (0.259)	3.648*** (0.284)	3.919*** (0.261)	3.633*** (0.288)	3.294*** (0.243)	3.198*** (0.246)	3.312*** (0.245)	3.223*** (0.247)	3.297*** (0.246)	3.221*** (0.252)
Without STABLE		✓		✓		✓		✓		✓		✓		✓
Period dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	630	504	630	504	290	232	290	232	340	272	340	272	340	272

Note: Giving is indicated in Euro. Participants with social preferences are those exhibiting sufficiently-strong advantageous inequality aversion, i.e., they give at least 0.25 Euro in the baseline treatment STABLE. The dummy variable *Any decrease* takes value 1 for all treatments with decreasing incomes (INTRADECREASE and CATCHINGUP for Higher Earners and INTRADECREASE and INTRAINTERCHANGE for Low Earners) and 0 otherwise. The dummy variable *Any strictly intra decrease* takes values 1 for all treatments with only intra-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. Similarly, the dummy variable *Any inter decrease* takes value 1 for all treatments with inter-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. All 5 periods and therefore all treatments are included in the regression, except when we indicate that we remove the STABLE treatment (which leaves us with 4 periods). We indicate p-values from Wald test on the equality of the coefficients of Any strictly intra decrease and Any inter decrease, and on the equality of INTRAINTERCHANGE and INTRADECREASE or CATCHINGUP. Standard errors clustered at the individual level are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

E   Evidence that First-Period Experience Does not Stop Effect of Decreasing Wages

We show that having some experience with the experiment, in the form of going through the first period, does not stop participants' reaction to decreasing wages in the next periods. Table E.6 reproduces basic regressions from Table **??** for social participants, but adding a term for the interaction between the first period and Any decrease. We do not distinguish between the two types of decreases to avoid diluting statistical power. The table shows that the negative effect of any decrease is not restricted to the first period as the coefficient of any decrease is significant. Furthermore, the interaction term between the first period and any decrease is negative—suggesting a possible greater impact in the first period—but not significant.

Table E.6: Tests of the effect of decreasing wages on giving, adding a dummy for the first encounter with decreasing wages

	(1) RE	(2) RE	(3) RE	(4) RE	(5) RE	(6) RE
	Participants with Social Preferences					
	All Earners		High Earners		Low Earners	
Any decrease	-0.365^{***}	-0.259^{**}	-0.511^{***}	-0.407^{**}	-0.217^{**}	-0.103
	(0.100)	(0.115)	(0.160)	(0.190)	(0.103)	(0.112)
Any decrease $\times$ 1$^{\text{st}}$ period	-0.610	-0.381	-0.745	-0.553	-0.497	-0.258
	(0.421)	(0.442)	(0.771)	(0.788)	(0.468)	(0.503)
Constant	4.647^{***}	4.334^{***}	4.723^{***}	4.402^{***}	4.534^{***}	4.257^{***}
	(0.224)	(0.276)	(0.250)	(0.311)	(0.305)	(0.349)
Without STABLE		✓		✓		✓
Period dummies	✓	✓	✓	✓	✓	✓
High Earners dummy	✓	✓				
N	455	364	240	192	215	172

Note: Giving is indicated in Euro. Any decrease denotes INTRADECREASE and CATCHINGUP for High Earners, and INTRADECREASE and INTRAINTERCHANGE for Low Earners. All treatments are included in the regression, except when we indicate that we remove the STABLE treatment. Standard errors clustered at the individual level are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

F   Evidence that Misunderstandings do not Drive our Results

Our results are robust to dropping the rare individuals who give almost everything during a period (i.e., ≥ 9 Euro from the 11-Euro joint account)—resulting in a greater income for the participant they are matched with than for themselves—because this might indicate that

they misunderstood the instructions (alternatively, those participants could simply be very generous). Appendix Table **??** reproduces some of the regressions from Table **??** for participants with social preferences, but excluding the 2 socially-inclined participants giving 9 Euro or more in at least one period. The results are very similar in terms of significance levels. The exception is that, for all Earners, Wald tests indicate that the difference between any inter decrease and any strict intra decrease is no longer marginally significant (columns 3-4).

Table F.7: Tests of the effect of decreasing wages on giving, excluding participants giving more than 9 Euro

	(1) RE	(2) RE	(3) RE	(4) RE	(5) RE	(6) RE	(7) RE	(8) RE	(9) RE	(10) RE	(11) RE	(12) RE
					Participants with Social Preferences							
	All Earners				High Earners				Low Earners			
Any decrease	−0.394*** (0.097)	−0.245** (0.105)			−0.504*** (0.152)	−0.372** (0.166)			−0.270*** (0.104)	−0.108 (0.113)		
Any strictly intra decrease			−0.297** (0.120)	−0.152 (0.135)								
Any inter decrease			−0.489*** (0.121)	−0.337*** (0.117)								
Wald test *p*-value			0.174	0.189								
INTRADECREASE							−0.478*** (0.184)	−0.343* (0.203)			−0.093 (0.137)	0.061 (0.167)
CATCHINGUP							−0.530*** (0.184)	−0.402** (0.190)				
INTRAINTERCHANGE											−0.447*** (0.152)	−0.282** (0.137)
Wald test *p*-value							0.803	0.780			0.076	0.094
Constant	4.503*** (0.202)	4.260*** (0.249)	4.505*** (0.202)	4.262*** (0.248)	4.605*** (0.211)	4.333*** (0.260)	4.603 (0.213)	4.330 (0.264)	4.302*** (0.245)	4.109*** (0.279)	4.327*** (0.241)	4.146*** (0.273)
Exclude giving > 9 Euro	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Without STABLE		✓		✓		✓		✓		✓		✓
Period dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
High Earners dummy	✓	✓	✓	✓								
N	445	356	445	356	235	188	235	188	210	168	210	168

Note: Giving is indicated in Euro. The dummy variable *Any decrease* takes value 1 for all treatments with decreasing incomes (INTRADECREASE and CATCHINGUP for Higher Earners and INTRADECREASE and INTRAINTERCHANGE for Low Earners) and 0 otherwise. The dummy variable *Any strictly intra decrease* takes values 1 for all treatments with only intra-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. Similarly, the dummy variable *Any inter decrease* takes value 1 for all treatments with inter-personal decreasing incomes (CATCHINGUP for High Earners and INTRAINTERCHANGE for Low Earners) and 0 otherwise. All 5 periods and therefore all treatments are included in the regression, except when we indicate that we remove the STABLE treatment (which leaves us with 4 periods). We indicate *p*-values from Wald test on the equality of the coefficients of Any strictly intra decrease and Any inter decrease, and on the equality of INTRAINTERCHANGE and INTRADECREASE or CATCHINGUP. Standard errors clustered at the individual level are in parentheses. * *p*<0.10, ** *p*<0.05, *** *p*<0.01.

"NOTE DI LAVORO" PUBLISHED IN 2021

1. 2021, Alberto Arcagni, Laura Cavalli, Marco Fattore, Partial order algorithms for the assessment of italian cities sustainability
2. 2021, Jean J. Gabszewicz, Marco A. Marini, Skerdilajda Zanaj, Random Encounters and Information Diffusion about Product Quality
3. 2021, Christian Gollier, The welfare cost of ignoring the beta
4. 2021, Richard S.J. Tol, The economic impact of weather and climate
5. 2021, Giacomo Falchetta, Nicolò Golinucci, Michel Noussan and Matteo Vincenzo Rocco, Environmental and energy implications of meat consumption pathways in sub-Saharan Africa
6. 2021, Carlo Andrea Bollino, Marzio Galeotti, On the water-energy-food nexus: Is there multivariate convergence?
7. 2021, Federica Cappelli, Gianni Guastella, Stefano Pareglio, Urban sprawl and air quality in European Cities: an empirical assessment
8. 2021, Paolo Maranzano, Joao Paulo Cerdeira Bento, Matteo Manera, The Role of Education and Income Inequality on Environmental Quality. A Panel Data Analysis of the EKC Hypothesis on OECD
9. 2021, Iwan Bos, Marco A. Marini, Riccardo D. Saulle, Myopic Oligopoly Pricing
10. 2021, Samir Cedic, Alwan Mahmoud, Matteo Manera, Gazi Salah Uddin, Information Diffusion and Spillover Dynamics in Renewable Energy Markets
11. 2021, Samir Cedic, Alwan Mahmoud, Matteo Manera, Gazi Salah Uddin, Uncertainty and Stock Returns in Energy Markets: A Quantile Regression Approach
12. 2021, Sergio Tavella, Michel Noussan, The potential role of hydrogen towards a low-carbon residential heating in Italy
13. 2021, Maryam Ahmadi, Matteo Manera, Oil Prices Shock and Economic Growth in Oil Exporting Countries
14. 2021, Antonin Pottier, Emmanuel Combet, Jean-Michel Cayla, Simona de Lauretis, Franck Nadaud, Who emits CO_2? Landscape of ecological inequalities in France from a critical perspective
15. 2021, Ville Korpela, Michele Lombardi, Riccardo D. Saulle, An Implementation Approach to Rotation Programs
16. 2021, Miguel Borrero, Santiago J. Rubio, An Adaptation-Mitigation Game: Does Adaptation Promote Participation in International Environmental Agreements?
17. 2021, Alan Finkelstein Shapiro, Gilbert E. Metcalf, The Macroeconomic Effects of a Carbon Tax to Meet the U.S. Paris Agreement Target: The Role of Firm Creation and Technology Adoption
18. 2021, Davide Bazzana, Jeremy Foltz, Ying Zhang, Impact of climate smart agriculture on food security: an agent-based analysis
19. 2021, Chiara Casoli, Riccardo (Jack) Lucchetti, Permanent-Transitory decomposition of cointegrated time series via Dynamic Factor Models, with an application to commodity prices
20. 2021, Pauline Pedehour, Lionel Richefort, Empowerment of social norms on water consumption
21. 2021, Carlo Drago, The Analysis and the Measurement of Poverty: An Interval-Based Composite Indicator Approach
22. 2021, Davide Bazzana, Francesco Menoncin, Sergio Veralli, The day after tomorrow: mitigation and adaptation policies to deal with uncertainty
23. 2021, Liang Nie, ZhongXiang Zhang, Is high-speed green? Evidence from a quasi-natural experiment in China
24. 2021, Michel Noussan, Manfred Hafner, Loyle Campbell, Xinqing Lu, Pier Paolo Raimondi, Erpu Zhu, Towards the decarbonization of the power sector – a comparison of China, the EU and the US based on historical data
25. 2021, Marta Castellini, Luca Di Corato, Michele Moretto, Sergio Vergalli, Energy exchange among heterogeneous prosumers under price uncertainty

26. 2021, Davide Bazzana, Michele Colturato, Roberto Savona, <u>Learning about Unprecedented Events: Agent-Based Modelling and the Stock Market Impact of COVID-19</u>
27. 2021, Nicola Comincioli, Paolo M. Panteghini, Sergio Vergalli, <u>The start-up decision under default risk</u>
28. 2021, Oleg Badunenko, Marzio Galeotti, Lester C. Hunt, <u>Better to grow or better to improve? Measuring environmental efficiency in OECD countries with a Stochastic Environmental Kuznets Frontier</u>
29. 2021, Matteo Mazzarano, Gianni Guastella, Stefano Pareglio, Anastasios Xepapadeas, <u>Carbon Boards and Transition Risk: Explicit and Implicit exposure implications for Total Stock Returns and Dividend Payouts</u>
30. 2021, Matteo Lanzafame, <u>Demography, growth and robots in advanced and emerging economies</u>
31. 2021, Shu Guo, ZhongXiang Zhanga, <u>Can the diligent governance increase subjective wellbeing? New evidence from environmental regulations in China</u>
32. 2021, Iwan Bos, Marco A. Marini, Riccardo D. Saulle, <u>Myopic Oligopoly Pricing</u>
33. 2021, Nickolas Gagnon, Riccardo D. Saulle, Henrik W. Zaunbrecher, <u>Decreasing Incomes Increase Selfishness</u>