Distributional Preferences and Ego Depletion

Loukas Balafoutas, Rudolf Kerschbamer, and Regine Oexl*

November 27, 2017

Abstract

By means of a laboratory experiment with 508 participants, we study the impact of ego depletion on revealed distributional preferences. Subjects are exposed to a social preference identification procedure in two consecutive weeks. In the treatment intervention they accomplish an ego depletion task before being exposed to the procedure in one of the two weeks, while in the control intervention they accomplish a control task. Half of the subjects are exposed to the intervention in week one and the other half in week two. Our design allows us to cleanly identify three separate effects on social preferences: i) the effect of exposing subjects to the social preference identification procedure a second time; ii) the effect of the intervention per se; and iii) the effect of ego depletion in particular. We find that only the intervention per se has an effect on social preferences for some types while the ego depletion task does not have a significant effect compared to the control task and preferences display a considerable degree of stability over time.

JEL: C9, C91, D01

Keywords: Ego Depletion, Distributional Preferences, Stability of Social Preferences

*All authors: University of Innsbruck, Universitätsstr. 15, 6020 Innsbruck, Austria – Balafoutas: Loukas.Balafoutas@uibk.ac.at; Kerschbamer: Rudolf.Kerschbamer@uibk.ac.at; Oexl: Regine.Oexl@uibk.ac.at

1 Introduction

To better understand human decision making, psychologists and economists are often concerned with the question which behaviors result out of controlled processes and which out of automatic processes (for a summary of dual-process theories and multiple-self models, see Alós-Ferrer and Strack, 2014). With respect to social or distributional preferences, two competing hypotheses have been suggested (see, for example, Rand et al., 2012, 2014; Halali et al., 2011, 2013; Hauge et al., 2016).[1] In a nutshell, H_1 suggests that egoism-based self-interest is the primary motive and that pro-social preferences result from – and are controlled by – a deliberative process. On the contrary, H_2 suggests that pro-social preferences are automatic, while self-interest considerations are controlled by the deliberative system. Obviously, it is also possible that what is automatic and what is controlled by the deliberative system is type-dependent: for some individuals, pro-social behavior may be the motive that is controlled by the deliberative system, while for others the controlled motive may be own-money maximization.

In this paper we present experimental evidence from a large laboratory experiment with more than 500 participants intended to investigate whether the deliberative system controls different basic motives for different distributional preference types: Subjects are exposed to a social preference identification procedure in two consecutive weeks. In the treatment intervention they accomplish an ego depletion task – meant to suppress the deliberative system – before being exposed to the procedure in one of the two weeks, while in the control intervention they accomplish a control task.[2] Half of the subjects are exposed to the

[1] By distributional or social preferences we mean that subjects may not only care for their own material payoff, but also for the material payoffs of others when making decisions.

[2] According to Baumeister et al. (1998, 2007), self-control is a limited resource which gets depleted from exertion. Hence, acts of self-control are predicted to cause short term impairments of self-control in subsequent tasks – even if subsequent tasks are unrelated. Another

intervention in week one and the other half in week two.

Exposing the subjects to the social preference identification procedure twice is essential for our research question since it allows us to see how social preferences *change* in response to the intervention. Exposing the subjects to the procedure at two different points in time also seems essential because performing the same task twice in the same experimental session is likely to induce experimenter demand effects and might also lead subjects to try to behave consistently across tasks. Finally, exposing half of the subjects to the intervention in week one and the other half in week two seems important to disentangle a possible retest effect from the intervention effect. Overall, our experimental design allows us to cleanly distinguish the effect of the suppression task from that of the control task, as well as to assess whether revealed social preferences are stable over time or whether recollecting the data one week later leads to different results.

A number of papers have studied the interaction of different social preference types and the deliberative system. Mischkowski and Glöckner (2016) expose subjects subsequently first to a social value orientation (SVO) elicitation procedure and then to a public goods game. Their main focus is on the question whether cooperative behavior in the public goods game is related to shorter decision times and they find a 'spontaneous cooperation effect' for pro-social but not for pro-self persons.[3] Halali et al. (2013) find that participants' SVO is unrelated to the effect of ego depletion in the ultimatum and in the dictator game. Cornelissen et al. (2011) combine data of SVO tasks with dictator exper-

method to uncover intuitive reasoning is by means of the concept of cognitive load (Swann et al., 1990; Gilbert et al., 1995), and yet another is to reduce response times (Rubinstein, 2007; Piovesan and Wengström, 2009).

[3]Other studies investigating the relation between response times and behavior are, among others, Piovesan and Wengström (2009), Cappelletti et al. (2011), Rand et al. (2012, 2014), Tinghög et al. (2013), and Ubeda (2014). While Cappelletti et al. (2011) and Rand et al. (2012, 2014) find a positive effect of time pressure on offers in the ultimatum game and on cooperation in a public goods game, Tinghög et al. (2013) find no effect, and Piovesan and Wengström (2009) and Ubeda (2014) report that faster responses are generally associated with more selfish and less cooperative behavior.

iments under cognitive load. While they do not detect a main effect of cognitive distraction, they find an interaction effect between SVO and cognitive load: Pro-socials become more generous under cognitive load, whereas pro-selves become (insignificantly) less generous.

Similarly inconclusive evidence is also provided by studies investigating the impact of the suppression of the deliberative system (ego depletion tasks as well as cognitive load tasks) on dictator or donation decisions. In contrast to the literature discussed in the previous paragraph those studies do not explicitly elicit distributional preference types – yet, the amount given in those kinds of tasks may be interpreted as a one-dimensional measure of altruism (or selfishness). While some studies find that the suppression of the deliberative system leads to increased selfishness, others find either no effect or evidence of increased pro-social behavior. For example, Xu et al. (2012) and DeWall et al. (2008) find less pro-social behavior in a donation (or helping) task among depleted subjects. Halali et al. (2013) find that depleted dictators choose the equal split significantly less often than non-depleted ones, and Achtziger et al. (2015) find that depleted dictators give less than non-depleted ones. On the contrary, dictators whose deliberative system is suppressed choose the equal split more often in Schulz et al. (2014). Kessler and Meier (2014) find that charitable giving increases after cognitive load (but only if the study is run later in a session of the overall experiment), while Hauge et al. (2016) find no effect of cognitive load on giving in dictator or donation games, and Grossman et al. (2014) find no effect on giving in the aggregate but opposing results for gender in a giving game.

There is also a literature investigating the impact of a suppression of the deliberative system on behavior in strategic games without explicitly eliciting distributional preferences (see, for instance, the literature cited in footnote 3). Since in

a strategic game a possible impact of the intervention on behavior might result from an impact on preferences or an impact on beliefs, we consider this literature as less related than the one discussed in the previous paragraphs.

Compared to the earlier literature on the interaction of different social preference types and the deliberative system we use a more systematic and more precise procedure for the elicitation of distributional preferences at the individual level – the Equality Equivalence Test (Kerschbamer 2015; henceforth EET). While the dictator game tasks and the SVO procedures employed in previous work yield only a one-dimensional index of pro- or anti-social preferences, the EET elicits non-strategic preferences in two domains of income allocations: In one – the domain of advantageous inequality – the decision maker is ahead of (i.e., receives a higher income than) another person; in the other – the domain of disadvantageous inequality – the decision maker is behind.[4] According to the revealed benevolence, neutrality or malevolence of the decision maker in the two domains, she or he is classified into one of nine archetypes of distributional preferences. For instance, a decision maker who reveals benevolence in both domains is classified as 'altruist', while a decision maker who reveals benevolence in the domain of advantageous inequality but neutrality in the domain of disadvantageous inequality is classified as 'maximin'. See Appendix A.1 for a more detailed description. Eliciting the social preferences of a subject in the two domains of inequality separately seems important because the predictions derived from H_1 and H_2 might differ across domains – see the discussion in the next paragraph.

As is evident from the literature discussion, different methods have been ap-

[4]The working paper version of Kerschbamer (2015) – cited as Kerschbamer (2013) – contains a thorough discussion of the advantages of the EET compared to other procedures for the elicitation of social preferences. See also the online appendix to Kerschbamer (2015). Here we focus on a single feature that seems especially relevant for the research question under investigation.

plied to suppress the deliberative system in order to uncover intuitive decision making processes. The method we use relies on the reduction of an individual's self-control by exposing it to an ego depletion task. Since self-control is the capacity of human beings to modify, change, or override their impulses, desires and habitual responses, a depletion task is predicted to enhance intuitive reasoning (Baumeister and Heatherton, 1996; Baumeister, 2002; Vohs, 2006). Hence, H_1 suggests that in the aggregate data ego depletion results in increased selfish behavior, while H_2 suggests that it increases pro-social behavior. Turning to the social preference types mentioned earlier, H_1 suggests that ego depletion has no effect on the measured social preferences of selfish subjects, but results in increased selfish behavior by altruists. For maximin subjects – who reveal benevolence in the domain of advantageous inequality but neutrality in the domain of disadvantageous inequality – the predicted effect is more subtle: they are predicted to decrease their benevolence in the domain of advantageous inequality but to remain unaffected in the domain of disadvantageous inequality. Turning to H_2, the predicted effect of ego depletion goes in exactly the opposite direction: selfish subjects are predicted to become more benevolent in response to ego depletion, while altruists should not react to it. Again the predicted response of maximin subjects is more subtle – they are expected to keep their benevolence in the domain of advantageous inequality and to become more benevolent in the domain of disadvantageous inequality.

Our main finding is that only the *intervention* per se has an effect on revealed social preferences of some types: While altruistic subjects reduce their other-regarding concerns significantly in the domain of disadvantageous inequality, maximin subjects increase their benevolence in this domain. Selfish subjects do not change their revealed pro-sociality in response to the intervention in either of the two domains of inequality, and altruistic and maximin subjects do not

respond to the intervention in the domain of advantageous inequality.

Overall, the results of our experiment can be summarized as follows: i) Only the intervention *per se* has an effect on the social preferences of some distributional preference types; ii) there is no significant difference between the treatment and the control intervention; and iii) there is no significant retest effect. While result iii) is in line with the literature on stability of distributional preferences – Balafoutas et al. (2014) also show that preference types remain predominantly constant over the two weeks at the individual level – the combination of results i) and ii) is surprising: one might have expected the ego depletion task to have a significantly different impact on behavior than the control task.

In the discussion section we address the question of which factors may help explain the pattern of our results, commenting on recent advances in the literature, our experimental design, the lack of a habituation task, and complex effects of ego depletion on self-control. This discussion leads to a number of suggestions for future research.

2 Experimental design

[[[[TABLE 1]]]]

We employ a two-by-two within-subjects experimental design, as summarized in Table 1: In one dimension we vary whether the subject undergoes the control (C) or the treatment (T) intervention; in the second dimension we vary in which week the subject is exposed to the intervention. To better explain the latter dimension, we distinguish between the 'classification week' and the 'intervention week'. In the classification week subjects are first exposed to the EET (as explained below), then they complete a risk aversion test (RAT) and a cognitive

reflection test. In the intervention week subjects first undergo the intervention, and then they are exposed to the EET. In the T condition the intervention is an ego depletion task (as explained below), while in the C condition it is a control task. For half of the subjects (subjects in group 1) the classification week is week one and the intervention week is week two, while for the second half (group 2) the intervention week is week one and the classification week is week two.

2.1 Distributional preferences: the EET

The EET (Kerschbamer, 2015) exposes subjects to a series of binary choices. Each choice involves two allocations, each specifying a payoff for the decision maker (DM) and a payoff for a randomly matched anonymous second subject, which we will call the passive subject (PS). In each choice, one of the two allocations is a symmetric reference allocation – that is, an allocation in which the DM and the PS receive the same income. The other allocation is asymmetric in the sense that it entails unequal payoffs for the two agents. In half of the choice tasks the asymmetric allocation is located in the domain of disadvantageous inequality (in which the DM receives a lower payoff than the PS), while in the other half it is located in the domain of advantageous inequality (where the DM receives a higher payoff than the PS). In both domains the EET systematically varies the price of giving (or taking) by increasing the material payoff of the DM in the asymmetric allocation while keeping all other payoffs constant.

For this study we used a test version with ten binary choices, five in each domain (see Appendix A.2). In the instructions subjects were informed that i) their payment for this part of the experiment (as the payments for all other parts) would be handed out at the end of the whole experiment (i.e., at the end

of the experiment in the second week); ii) they would receive two payments for this task, one as a DM and one as a PS; iii) for their payment as a DM one of the 10 binary choices would be selected randomly and the alternative chosen in this decision problem would be paid out; and iv) their payment as the PS would come from another participant (i.e., not from the PS of the DM under consideration).[5]

Given the design of the EET, in each of the two domains a rational DM switches at most once from the symmetric to the asymmetric allocation (and never in the other direction). As shown by Kerschbamer (2015), the switching points in the two domains can be used to construct a two-dimensional index representing the archetype of distributional concerns and preference intensity: The x-score (ranging from -2.5 to +2.5 in integer steps) measures pro-sociality in the domain of disadvantageous inequality, and the y-score (again ranging from -2.5 to +2.5 in integer steps) measures pro-sociality in the domain of advantageous inequality. In both domains a positive (negative) score means benevolence (malevolence) and a higher score means 'more benevolent' ('less malevolent').

2.2 The ego depletion task

Following standard procedures (Baumeister et al., 1998; Fischer et al., 2007; DeWall et al., 2007; Legault et al., 2009; Freeman and Muraven, 2010; Achtziger et al., 2015, 2016), we suppressed the subjects' deliberative system by means

[5] Kerschbamer (2015) calls this the double-role-assignment protocol and he mentions (in the online appendix to the paper) that a potential problem with it is that a subject's expectation about what she receives as a PS and about what her PS receives as the active DM might influence behavior. The fixed-role-assignment protocol (where roles are assigned ex ante and only half of the subjects make decisions as active DMs while the other half do nothing) and the role-uncertainty protocol (where each subject decides in the role of the active DM and only later subjects get to know whether they have been chosen as DM or as PS) seem to be cleaner in this respect but they suffer from other problems: With the fixed-role assignment the preferences of only half of the subjects are elicited; and with role-uncertainty, ex ante fairness concerns might affect behavior. See the appendix to Kerschbamer (2015) for a discussion of these issues.

of an ego depletion task. Specifically, we used a letter counting task, in which participants were asked to cross out a pattern of appearance of the letter 'e' in some paragraphs of text. Participants in the control condition were asked to cross out all letters 'e' , while participants in the depletion condition had to cross out a pattern of 'e' s that required the suppression of an impulse (for details, see the instructions in Appendix A.4). In both conditions, participants were provided with a total of six paragraphs, each containing seven rows of text from the novel 'Alice's Adventures in Wonderland' by Lewis Caroll. They had five minutes to complete as many paragraphs as possible and were then asked to enter the number of relevant counted 'e's for each paragraph into the computer. The correct counting of 'e's per paragraph was incentivized with three points for a correct number and one point if the number was almost correct – with almost correct interpreted as either one unit above or one unit below the correct number (1 point = 1 Euro). Feedback about the correct number of paragraphs was given at the end of the experiment in week two.

2.3 Procedures

The experiment was conducted between May 2014 and November 2015 at the Innsbruck EconLab and subjects were recruited via hroot (Bock et al., 2014).[6] Except for the paper and pencil task in the ego depletion stage, the experiment was computerized via z-Tree (Fischbacher, 2007). The general rules of participation were handed out and read out loud at the beginning of the experiment, while detailed instructions for the respective task followed immediately before the task. At the end of week two, all subjects received information about their earnings and their payment for both weeks. Sessions lasted approximately 45

[6] Our research ethics committee declared this study exempt from IRB approval since it is a standard experiment.

minutes and average earnings were 22.59 Euros.

We collected data from 621 subjects. Of those individuals, 48 (8%) subjects did not show up in the second week or could not be matched due to missing participation numbers. From the remaining 573 subjects, we excluded 65 from the analysis: Those participants made inconsistent choices in the RAT or in one of the two EETs.[7] We therefore remain with 508 subjects: 250 in the treatment and 258 in the control intervention. Approximately half of the subjects underwent the classification in week one (group 1: 224 individuals), the rest underwent the classification in week two (group 2: 284 individuals) – see Table 1.

2.4 Power calculations

[[[[TABLE 2]]]]

In search for an effect of ego depletion on distributional preference types, we started this project by running a pilot with four sessions and with all subjects undergoing the intervention in week two, collecting observations from 49 consistent subjects. As expected from previous research (Kerschbamer, 2015; Balafoutas et al., 2014, 2012), the three major distributional preference types appearing in the data were altruistic, selfish and maximin. Indeed, 46 of the 49 subjects in our pilot were classified into one of these types. Based on means and standard deviations from this sample, we then calculated the required sample size in order to detect a significant difference in the change from one week to the next between the treatment and the control intervention, using a two-sample

[7] The RAT and the EET rely on minimal assumptions regarding the rationality of a DM. In terms of axioms on preferences the assumptions are ordering (completeness and transitivity) and strict (own-money) monotonicity. By 'inconsistent choices' we mean choice patterns that are inconsistent with those basic axioms. From the 65 subjects that made inconsistent choices, 33 were in the treatment condition and 32 were in the control condition.

t-test. We aimed at collecting the number of observations required to have a statistical power of at least 90% to detect an effect at the significance level α of 10%.

Since we search for a significant difference between the treatment and the control condition 1) for all subjects, 2) for altruists, 3) for selfish subjects and 4) for maximin types, and since all tests are conducted for both domains (i.e., the domain of advantageous inequality and the domain of disadvantageous inequality), we are actually testing eight hypotheses simultaneously. Taking multiple hypotheses testing into account we decided to apply a Bonferroni-correction and adjusted the significance level α from 10% to 1.25%.

Column (1) of Table 2 reports the number of observations required to find a significant effect for the given effect size in our sample; column (2) reports the actual number of consistent observations we collected. As can be seen in column (3), we only collected enough observations for finding a significant effect for all types in the domain of disadvantageous inequality, and for finding a significant effect for the altruistic subjects in the domain of advantageous inequality. For selfish and maximin subjects, the differences between treatment and control turn out to be very small and therefore the number of observations required to find a significant effect in the domain of advantageous inequality would have been extremely high.

3 Results

There is clear evidence that the ego depletion task is more difficult to perform than the control task: Subjects solved significantly more paragraphs correctly in the control condition compared to the treatment condition (average number of correctly solved paragraphs: 0.07 in T and 0.36 in C; Mann-Whitney U-test

(MWU): $p < 0.01$); the same holds for the paragraphs almost correctly solved (0.16 in T and 0.43 in C; MWU: $p < 0.01$).

[[[[TABLE 3]]]]

In the classification week the empirically most relevant classes of distributional preferences – covering jointly about 90% of the subjects – are altruists (40%), maximin (20%) and selfish subjects (30%). The distribution of types in the population is in line with the literature (Balafoutas et al., 2012, 2014; Kerschbamer, 2015), and it does not differ between treatment and control condition (Fisher's exact test: $p = 0.56$), nor does it differ significantly between group 1 and group 2 (Fisher's exact test: $p = 0.20$). Hence, randomization has been successful. In the following, we investigate separately the three most relevant types of distributional preference: altruists, selfish and maximin.[8]

First, we examine whether revealed distributional preferences change over time. Comparing the classification week with the intervention week, we observe that 70% of the subjects remain of the same type.[9] There is also little change in the x- and the y-scores: Only 30% (29%) of the subjects change the x-(y)-score in the T condition and 33% (28%) in the C condition (two-sample test of proportions: $p = 0.48$ for the x-score and $p = 0.82$ for the y-score); and only 34% (31%) of the subjects change the x-(y)-score in group 1 and 30% (26%) in group 2 (two-sample test of proportions: $p = 0.29$ for the x-score and $p = 0.20$ for the y-score). We summarize these findings to:

Result 1: *Overall, distributional preference types and preference intensities are relatively stable over the two weeks.*

[8] For the other types, the number of observations is simply too small: equality averse (1 subject), kick-down (3), spiteful (5), envious subjects (11), kiss-up (13), inequality-averse (19).

[9] This result is also true if we compare each intervention dimension in isolation: Comparing the treatment to the control condition the fractions are 72% in T and 69% in C (two-sample test of proportions: $p = 0.40$), and comparing group 1 to group 2 the fractions are 68% in group 1 and 72% in group 2 (two-sample test of proportions: $p = 0.39$).

Next, we turn to the effect of the ego depletion task compared to the control task. On average, the x-score decreases by 0.07 in T, while it increases by 0.01 in C (Table 3). Similarly, the average y-score decreases by 0.07 in T, while it increases by 0.03 in C. The difference-in-difference between T and C is not significant for either score (MWU-tests: $p = 0.65$ for the x-score and $p = 0.17$ for the y-score). Also, when disaggregating the average change into the average change for each of the empirically most frequent distributional preference types, we do not detect a significant difference in the change between the treatment and the control intervention for any of the types ($p > 0.13$ for all comparisons).

The result is confirmed by an ordered logistic regression with robust standard errors clustered on the individual level (Table 4). The regression includes the following dummy variables: 'Week 2 (W2)', to control for a time or retest effect, 'Treatment (T)', to control for potential differences between the treatment and control group, 'Intervention (I)', to control for an effect of either the treatment or the control task, and the respective interaction terms. If the ego depletion task has a different effect than the control task, we would observe a significant coefficient on the 'I x T'-term, which is not the case – neither overall nor separately for any of the main distributional preference types.[10]

Result 2: *There is no significant difference in the effect of the ego depletion task compared to the control task.*

Furthermore, Table 4 confirms that there is no significant change in the scores from one week to the other. It also confirms that the treatment and the control group are not significantly different to each other. Yet, while we find that the intervention *per se* does not have a significant impact overall on the scores (columns (1) and (4) in Table 4), it has a significant impact on some types in

[10]The stars for the significance levels reported in Table 4 are already adjusted for simultaneous testing of eight hypotheses.

the domain of disadvantageous inequality: While subjects classified as *altruists* become significantly *less* benevolent (more selfish) under an intervention in this domain, subjects classified as *maximin* become significantly *more* benevolent (less selfish). These effects are statistically significant: Even when correcting for multiple hypotheses testing using a Bonferroni-correction for eight hypotheses, the 'intervention'-dummy is different from zero for the altruists and the maximin subjects at the 10% level.[11]

We summarize these findings to the following result:

Result 3: *There is a significant effect of the intervention on revealed social preferences for some of the major social preference types: In the domain of disadvantageous inequality, subjects classified as altruists in the classification week become less benevolent after the intervention, while subjects classified as maximin in the classification week become more benevolent after the intervention.*

[[[[TABLE 4]]]]

4 Discussion

We have found that there is no effect of recollecting the data, which points towards a considerable stability of preferences. Somewhat disturbingly, we have also found that the ego depletion and the control task have a similar impact on behavior. Hence, it appears that it is the intervention in itself that has an effect. Subjects classified as maximin – who per definition are benevolent in the domain of advantageous and neutral in the domain of disadvantageous

[11] Results are qualitatively confirmed by random effects regressions, see Table 5 in Appendix A.3; the effect of the intervention itself is still negative and significant for altruists, while the effect for maximin subjects is still positive but now insignificant.

inequality – increase their benevolence in the domain of disadvantageous inequality after the intervention; and subjects classified as altruists – who per definition are benevolent in both domains – decrease their benevolence in the domain of disadvantageous inequality after the intervention. In all other cases, we do not find a significant impact of the intervention on revealed distributional preferences.

What could drive our results? First, consider the question of why we find an intervention effect while the rest of the literature does not. Here, the discrepancy might simply be due to the fact that our experimental design allows for the identification of such an effect while the designs typically employed in the literature do not. Typically, experiments investigating the effects of ego depletion compare the behavior of subjects *with and without* being exposed to an ego depletion task before performing the task. Such an experimental design – often called the 'dual-task paradigm' – allows capturing the difference between the treatment and the control intervention but does not permit identifying the impact of the control task itself on the behavior of the experimental subjects.[12] Other studies investigating the impact of ego depletion elicit the behavior under study *before and after* an ego depletion intervention. Such studies cannot disentangle the intervention from the retest effect. By having the intervention once in week one and once in week two, our design allows for a clean separation of the two effects.[13]

[12] The 'dual-task paradigm' relies on two unrelated tasks – first the ego depletion task, then the task under study (Hagger et al., 2010; Baumeister et al., 1998; Finkel et al., 2006; Muraven et al., 1998).

[13] For 434 of the subjects we included the multidimensional mood questionnaire of Steyer et al. (1997) between the intervention and the EET. In line with the literature, we find that subjects are not significantly more tired in the treatment intervention compared to the control intervention (attention-tired-dimension, mean T: 11.49, C: 11.35, MWU: $p = 0.93$; $N_T = 213$, $N_C = 221$), they are not of a significantly worse mood (good-bad-mood-dimension, mean T: 13.80, C: 13.83, MWU: $p = 0.87$), and they are not significantly less calm or more nervous (calm-nervous-dimension, mean T: 13.87, C: 13.55, MWU: $p = 0.22$). While this does not imply that the two tasks *depleted* in the same way, it is further evidence suggesting that the control and depletion intervention worked in a similar way for the other dimensions.

Next consider the question of why maximin subjects increase their benevolence in the domain of disadvantageous inequality after the intervention while altruistic subjects decrease their benevolence in this domain. One possible explanation for this finding is that both the control and the treatment intervention cause an ego depletion effect and that this effect makes preferences more "moderate". An alternative explanation is that the intervention makes behavior more erratic, thus leading to a reversion to the mean for more extreme types.[14] This alternative explanation has some plausibility – yet, there is also evidence that speaks against this hypothesis: If behavior was more erratic under the intervention, we would expect more inconsistent choices after the intervention. We do not find evidence that points in that direction.[15]

Turning to the crucial question why the ego depletion task does not have a significantly different impact on behavior than the control task we note that our evidence is in line with recent findings in the psychology literature, where a number of papers failed to replicate the ego depletion effect. For instance, Xu et al. (2014) use the same depletion and control intervention as we do, collecting self-control measures before and after the intervention. They find no significant depletion effect on their measures of self-control. Using an only slightly different task adapted from Sripada et al. (2014), a pre-registered, multilab replication study by Hagger et al. (2016, registered in October 2014) finds no statistically significant ego depletion effect between the depletion and the control task.[16]

[14] Franco-Watkins et al. (2006, 2010) suggest a similar interpretation related to findings (by Hinson et al., 2003, for instance) that cognitive load results in greater impulsivity. Schulz et al. (2014) discuss the reversion-to-the-mean-hypothesis as possible alternative explanation for their findings in subsection 3.2, and conclude that it is unlikely to be responsible for their results.

[15] Details are available from the authors upon request.

[16] A subtle methodological issue concerns the question how monetary incentives associated with the depletion task influence behavior in the main task. Sripada et al. (2016) argue that in Hagger et al.'s multilab study, subjects were not paid, which may lead them to be less motivated and could drive the non-result. Yet, Achtziger et al. (2015, 2016) find no impact of the incentive scheme used in the depletion task on behavior in the ultimatum and in the dictator game. Our study was incentivized as well – and, yet, in line with Hagger et al. (2016), we do not find an effect.

Other authors note that the effect size of ego depletion might have been overestimated in parts of the literature as a result of publication bias – see, e.g. Carter and McCullough (2013) and Lurquin et al. (2016).

In light of the recent evidence, both economists and psychologists are currently struggling with the concept of ego depletion. As noted by Alós-Ferrer et al. (2015), a rapidly growing number of studies present evidence suggesting that the effects of ego depletion on self-control are more complex than initially assumed. The "need for tackling the conceptual crisis" of the ego depletion research has also be emphasized in a recent article by Lurquin and Miyake (2017). The authors state that one of the main problems of ego depletion research is the lack of a clear operational definition of self-control. This brings with it a lack of justification for using a specific depletion – and control – task, leading to the situation that in some studies "the same task (e.g., 3-digit by 3-digit multiplication) has been used as both the self-control (depletion task) [...] and the control (non-depletion) task" (Lurquin and Miyake, 2017, p. 2). This emphasizes the need for studying the (impact of) the control task more carefully, which is one of the methodological contributions of our study. Further research along these lines is required before conclusions can be drawn from existing research on the effects of ego depletion on social preferences or more generally on human motivation and behavior.

References

Achtziger, A., Alós-Ferrer, C., Wagner, A. K., 2015. Money, depletion, and prosociality in the dictator game. Journal of Neuroscience, Psychology, and Economics 8 (1), 1–14.

Achtziger, A., Alós-Ferrer, C., Wagner, A. K., 2016. The impact of self-control depletion on social preferences in the ultimatum game. Journal of Economic Psychology 53, 1–16.

Alós-Ferrer, C., Hügelschäfer, S., Li, J., 2015. Self-control depletion and decision making. Journal of Neuroscience, Psychology, and Economics 8 (4), 203 – 216.

Alós-Ferrer, C., Strack, F., 2014. From dual processes to multiple selves: Implications for economic behavior. Journal of Economic Psychology 41, 1 – 11.

Balafoutas, L., Kerschbamer, R., Kocher, M., Sutter, M., 2014. Revealed distributional preferences: Individuals vs. teams. Journal of Economic Behavior & Organization 108 (0), 319 – 330.

Balafoutas, L., Kerschbamer, R., Sutter, M., 2012. Distributional preferences and competitive behavior. Journal of Economic Behavior & Organization 83 (1), 125–135.

Baumeister, R. F., 2002. Ego depletion and self-control failure: An energy model of the self's executive function. Self and Identity 1 (2), 129–136.

Baumeister, R. F., Bratslavsky, E., Muraven, M., Tice, D. M., 1998. Ego depletion: Is the active self a limited resource? Journal of Personality and Social Psychology 74 (5), 1252–1265.

Baumeister, R. F., Heatherton, T. F., 1996. Self-regulation failure: An overview. Psychological Inquiry 7 (1), 1–15.

Baumeister, R. F., Vohs, K. D., Tice, D. M., 2007. The strength model of self-control. Current directions in psychological science 16 (6), 351–355.

Bock, O., Baetge, I., Nicklisch, A., 2014. hroot: Hamburg registration and organization online tool. European Economic Review 71, 117 – 120.

Cappelletti, D., Güth, W., Ploner, M., 2011. Being of two minds: An ultimatum experiment investigating affective processes. Journal of Economic Psychology 32, 940–950.

Carter, E. C., McCullough, M. E., 2013. Is ego depletion too incredible? evidence for the overestimation of the depletion effect. Behavioral and Brain Sciences 36 (6), 683–684.

Cornelissen, G., Dewitte, S., Warlop, L., 2011. Are social value orientations expressed automatically? Decision making in the dictator game. Personality and Social Psychology Bulletin 37 (8), 1080–1090.

DeWall, C. N., Baumeister, R. F., Gailliot, M. T., Maner, J. K., 2008. Depletion makes the heart grow less helpful: Helping as a function of self-regulatory energy and genetic relatedness. Personality and Social Psychology Bulletin 12 (34), 1653–1662.

DeWall, C. N., Baumeister, R. F., Stillman, T. F., Gailliot, M. T., 2007. Violence restrained: Effects of self-regulation and its depletion on aggression. Journal of Experimental Social Psychology 43 (1), 62 – 76.

Finkel, E. J., Dalton, A. N., Campbell, W. K., Brunell, A. B., Scarbeck, S. J., Chartrand, T. L., 2006. High-maintenance interaction: Inefficient social coordination impairs self-regulation. Journal of Personality and Social Psychology, 456–475.

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

Fischer, P., Greitemeyer, T., Frey, D., 2007. Ego depletion and positive illusions: Does the construction of positivity require regulatory resources? Personality and Social Psychology Bulletin 33 (9), 1306–1321.

Franco-Watkins, A., Pashler, H., Rickard, T. C., 2006. Does working memory load lead to greater impulsivity? Commentary on Hinson, Jameson, and Whitney's (2003). Journal of Experimental Psychology-Learning Memory and Cognition 32 (2), 443 – 447.

Franco-Watkins, A. M., Rickard, T. C., Pashler, H., 2010. Taxing executive processes does not necessarily increase impulsive decision making. Experimental psychology 57 (3), 193–201.

Freeman, N., Muraven, M., 2010. Self-control depletion leads to increased risk taking. Social Psychological and Personality Science 1 (2), 175–181.

Gilbert, D. T., Giesler, R. B., Morris, K. A., 1995. When comparisons arise. Journal of Personality and Social Psychology 69 (2), 227–236.

Grossman, Z., van der Weele, J., Andrijevik, A., 2014. A test of dual-process reasoning in charitable giving. University of california at santa barbara, economics working paper series, Department of Economics, UC Santa Barbara.

Hagger, M. S., Chatzisarantis, N. L. D., Alberts, H., Anggono, C. O., Batailler, C., Birt, A. R., Brand, R., Brandt, M. J., Brewer, G., Bruyneel, S., Calvillo, D. P., Campbell, W. K., Cannon, P. R., Carlucci, M., Carruth, N. P., Cheung, T., Crowell, A., Ridder, D. T. D. D., Dewitte, S., Elson, M., Evans, J. R., Fay, B. A., Fennis, B. M., Finley, A., Francis, Z., Heise, E., Hoemann, H., Inzlicht, M., Koole, S. L., Koppel, L., Kroese, F., Lange, F., Lau, K., Lynch, B. P.,

Martijn, C., Merckelbach, H., Mills, N. V., Michirev, A., Miyake, A., Mosser, A. E., Muise, M., Muller, D., Muzi, M., Nalis, D., Nurwanti, R., Otgaar, H., Philipp, M. C., Primoceri, P., Rentzsch, K., Ringos, L., Schlinkert, C., Schmeichel, B. J., Schoch, S. F., Schrama, M., Schtz, A., Stamos, A., Tinghg, G., Ullrich, J., vanDellen, M., Wimbarti, S., Wolff, W., Yusainy, C., Zerhouni, O., Zwienenberg, M., 2016. A multilab preregistered replication of the ego-depletion effect. Perspectives on Psychological Science 11 (4), 546–573.

Hagger, M. S., Wood, C., Stiff, C., Chatzisarantis, N. L. D., 2010. Ego Depletion and the Strength Model of Self-Control: A Meta-Analysis. Psychological Bulletin 136, 495–525.

Halali, E., Bereby-Meyer, Y., Meiran, N., 2011. When rationality and fairness conflict: The role of cognitive-control in the ultimatum game. Working Paper.

Halali, E., Bereby-Meyer, Y., Ockenfels, A., 2013. Is it all about the self? The effect of self-control depletion on ultimatum game proposers. Frontiers in Human Neuroscience 7 (240), 1–8.

Hauge, K. E., Brekke, K. A., Johansson, L.-O., Johansson-Stenman, O., Svedsäter, H., 2016. Keeping others in our mind or in our heart? Distribution games under cognitive load. Experimental Economics 19 (3), 562–576.

Hinson, J. M., Jameson, T. L., Whitney, P., 2003. Impulsive decision making and working memory. Journal of Experimental Psychology: Learning, Memory, and Cognition 29 (2), 298 – 306.

Kerschbamer, R., 2013. The geometry of distributional preferences and a non-parametric identification approach. Working Papers in Economics and Statistics 2013-25 (2013), University of Innsbruck.

Kerschbamer, R., 2015. The geometry of distributional preferences and a non-parametric identification approach: The equality equivalence test. European Economic Review 76, 85–103.

Kessler, J. B., Meier, S., 2014. Learning from (failed) replications: Cognitive load manipulations and charitable giving. Journal of Economic Behavior & Organization 102, 10 – 13.

Legault, L., Green-Demers, I., Eadie, A. L., 2009. When internalization leads to automatization: The role of self-determination in automatic stereotype suppression and implicit prejudice regulation. Motivation and Emotion 33 (1), 10–24.

Lurquin, J. H., Michaelson, L. E., Barker, J. E., Gustavson, D. E., von Bastian, C. C., Carruth, N. P., Miyake, A., 02 2016. No evidence of the ego-depletion effect across task characteristics and individual differences: A pre-registered study. PLoS ONE 11 (2), 1–20.

Lurquin, J. H., Miyake, A., 2017. Challenges to ego-depletion research go beyond the replication crisis: A need for tackling the conceptual crisis. Frontiers in Psychology 8, 568.

Mischkowski, D., Glöckner, A., 2016. Spontaneous cooperation for prosocials, but not for proselfs: Social value orientation moderates spontaneous cooperation behavior. Scientific Reports 6 (21555), 1–5.

Muraven, M., Tice, D. M., Baumeister, R. F., 1998. Self-control as a limited resource: Regulatory depletion patterns. Journal of Personality and Social Psychology 74 (3), 774–789.

Piovesan, M., Wengström, E., 2009. Fast or fair? A study of response times. Economics Letters 105 (2), 193–196.

Rand, D. G., Greene, J. D., Nowak, M. A., 2012. Spontaneous giving and calculated greed. Nature 489 (7416), 427–430.

Rand, D. G., Peysakhovich, A., Kraft-Todd, G. T., Newman, G. E., Wurzbacher, O., Nowak, M. A., Greene, J. D., 2014. Social heuristics shape intuitive cooperation. Nature Communications 5 (3677), 1–12.

Rubinstein, A., 2007. Instinctive and cognitive reasoning: A study of response times. The Economic Journal 117 (523), 1243–1259.

Schulz, J. F., Fischbacher, U., Thöni, C., Utikal, V., 2014. Affect and fairness: Dictator games under cognitive load. Journal of Economic Psychology 41, 77 – 87.

Sripada, C., Kessler, D., Jonides, J., 2014. Methylphenidate blocks effort-induced depletion of regulatory control in healthy volunteers. Psychological Science 25 (6), 1227–1234.

Sripada, C., Kessler, D., Jonides, J., 2016. Sifting signal from noise with replication science. Perspectives on Psychological Science 11 (4), 576–578.

Steyer, R. P., Schwenkmezger, P., Notz, P., Eid, M., 1997. Der Mehrdimensionale Befindlichkeitsfragebogen (MDBF). Hogrefe Verlag für Psychologie, Göttingen.

Swann, W. B., Hixon, G. J., Stein-Seroussi, A., Gilbert, D. T., 1990. The fleeting gleam of praise: Cognitive processes underlying behavioral reactions to self-relevant feedback. Journal of Personality and Social Psychology 59 (1), 17–26.

Tinghög, G., Andersson, D., Bonn, C., Böttiger, H., Josephson, C., Lundgren, G., Västfjäll, D., Kirchler, M., , Johannesson, M., 2013. Intuition and cooperation reconsidered. Nature 498 (7452), E1–E2.

Ubeda, P., 2014. The consistency of fairness rules: An experimental study. Journal of Economic Psychology 41, 88–100.

Vohs, K. D., 2006. Self-regulatory resources power the reflective system: Evidence from five domains. Journal of Consumer Psychology 16 (3), 217–223.

Xu, H., Bègue, L., Bushman, B. J., 2012. Too fatigued to care: Ego depletion, guilt, and prosocial behavior. Journal of Experimental Social Psychology 48 (5), 1183 – 1186.

Xu, X., Demos, K. E., Leahey, T. M., Hart, C. N., Trautvetter, J., Coward, P., Middleton, K. R., Wing, R. R., 2014. Failure to replicate depletion of self-control. PLoS One 9 (10).

Table 1: Treatment variations and number of observations

		Order of tasks		
		Group 1	Group 2	N
Intervention	Treatment (T)	T1 (108)	T2 (142)	250
	Control (C)	C1 (116)	C2 (142)	258
	N	224	284	508

For each individual we elicit the distributional preferences twice – first in week one and then in week two. Group 1: Classification week is week one, intervention week is week two. Group 2: Intervention week is week one, classification week is week two.

Table 2: Power calculations

			Pilot: first 4 sess. Mean (SD)	(1) Required N for α = 1.25%	(2) # of consistent obs. in total	(3) Sufficient # of obs. to detect an effect for α = 1.25%?
x-score *(domain of disadv. ineq.)*	Altruists	C	0.00 (0.63)	44	109	✓
		T	-0.58 (0.79)	44	92	✓
	Selfish	C	-0.11 (0.33)	27	69	✓
		T	0.25 (0.46)	27	82	✓
	Maximin	C	0.33 (0.58)	36	53	✓
		T	-1.00 (1.73)	36	51	✓
y-score *(domain of adv. ineq.)*	Altruists	C	-0.18 (0.40)	88	109	✓
		T	0.08 (0.51)	88	92	✓
	Selfish	C	-0.11 (0.33)	172	69	x
		T	0.00 (0.53)	172	82	x
	Maximin	C	0.00 (1.00)	460	53	x
		T	-0.33 (0.58)	460	51	x

Table 3: Average change of scores from 'classification week' to 'intervention week'

		All subjects	Altruists	Selfish	Maximin	Other
Changes in the *x-score (domain of disadv. inequality)*						
	T1+T2	-0.07	-0.28	-0.04	0.14	0.20
	C1+C2	0.01	-0.24	0.01	0.23	0.59
Changes in the *y-score (domain of adv. inequality)*						
	T1+T2	-0.07	-0.16	0.11	-0.22	-0.04
	C1+C2	0.03	-0.02	0.12	-0.19	0.41
Number of observations						
N	T1+T2	250	92	82	51	25
	C1+C2	258	109	69	53	27

Table 4: Ordered logistic regression on *scores*

	Dep. variable: x-score (*Domain of disadv. inequality*)				Dep. variable: y-score (*Domain of adv. inequality*)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All subjects	Altruists	Selfish	Maximin	All subjects	Altruists	Selfish	Maximin
Week 2 (W2)	0.37	-0.01	0.91	0.04	0.16	0.12	1.08	0.56
	(1.63)	(-0.05)	(1.83)	(0.30)	(0.74)	(0.30)	(2.42)	(1.32)
Treatment (T)	0.20	0.91	0.63	-0.26	0.12	0.34	0.29	0.33
	(0.74)	(2.45)	(1.20)	(-0.64)	(0.51)	(0.82)	(0.52)	(0.82)
Intervention (I)	0.13	-1.05*	0.53	1.66*	0.11	0.10	1.24	-0.36
	(0.59)	(-2.62)	(1.00)	(2.61)	(0.46)	(0.25)	(2.09)	(-0.53)
I x T	-0.57	-0.27	-1.01	-0.44	-0.42	-0.75	-0.45	-0.36
	(-1.62)	(-0.42)	(-1.30)	(-0.38)	(-1.28)	(-1.33)	(-0.45)	(-0.40)
I x W2	-0.30	0.65	-0.48	0.02	-0.16	-0.21	-1.04	-0.39
	(-0.74)	(0.96)	(-0.46)	(0.02)	(-0.36)	(-0.30)	(-0.97)	(-0.37)
T x W2	-0.67	-0.76	-0.88	0.41	-0.38	-0.26	-0.39	-0.82
	(-1.97)	(-1.57)	(-1.24)	(0.87)	(-1.18)	(-0.48)	(-0.54)	(-1.45)
I x T x W2	1.02	0.18	1.85	0.70	0.51	0.59	0.87	0.45
	(1.65)	(0.16)	(1.26)	(0.52)	(0.84)	(0.57)	(0.52)	(0.33)
Observations	1016	402	302	208	1016	402	302	208
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors clustered on the individual level. The stars in the table stand for the following p-values: * $p < 0.0125$, ** $p < 0.00625$, *** $p < 0.00125$. Since we are simultaneously testing eight hypotheses, these levels correspond to the Bonferroni corrected, standard significance levels of 10%, 5% and 1%, respectively. 'Week two' = 1 in the second week and zero otherwise; 'Treatment' = 1 in treatment condition and zero otherwise; 'Intervention' = 1 if the intervention takes place; the other variables are interaction terms of these dummies. Controls include gender, age, whether the subject studies economics or not, the cognitive reflection test and the RAT. z statistics in parentheses.

A Appendix

A.1 The nine archetypes, characterized by the slopes of their indifference curves

Preference type	$m > o$	$m < o$
Selfish (own money maximizing)	$\frac{\partial u}{\partial o} = 0$	$\frac{\partial u}{\partial o} = 0$
Altruistic (efficiency loving, surplus maximizing)	$\frac{\partial u}{\partial o} > 0$	$\frac{\partial u}{\partial o} > 0$
Spiteful (competitive, relative income maximizer)	$\frac{\partial u}{\partial o} < 0$	$\frac{\partial u}{\partial o} < 0$
Envious (grudging)	$\frac{\partial u}{\partial o} = 0$	$\frac{\partial u}{\partial o} < 0$
Maximin (Rawlsian, Leontief)	$\frac{\partial u}{\partial o} > 0$	$\frac{\partial u}{\partial o} = 0$
Inequality averse (inequitv averse, egalitarian)	$\frac{\partial u}{\partial o} > 0$	$\frac{\partial u}{\partial o} < 0$
Equality averse (equitvy averse)	$\frac{\partial u}{\partial o} < 0$	$\frac{\partial u}{\partial o} > 0$
Kick-down (bully-the-underlings)	$\frac{\partial u}{\partial o} < 0$	$\frac{\partial u}{\partial o} = 0$
Kiss-up (crawl-to-the-bigwigs)	$\frac{\partial u}{\partial o} = 0$	$\frac{\partial u}{\partial o} > 0$

A.2 Version of the Equality Equivalence Test (Kerschbamer, 2015) used in this experiment

Choices in the disadvantageous inequality block:

Alternative: Left		Your Choice		Alternative: Right	
You receive	*The other person receives*	*Cross here your choice*		*You receive*	*The other person receives*
3 points	6 points	☐	☐	4 points	4 points
3.8 points	6 points	☐	☐	4 points	4 points
4 points	6 points	☐	☐	4 points	4 points
4.2 points	6 points	☐	☐	4 points	4 points
5 points	6 points	☐	☐	4 points	4 points

Choices in the advantageous inequality block:

Alternative: Left		Your Choice		Alternative: Right	
You receive	*The other person receives*	*Cross here your choice*		*You receive*	*The other person receives*
3 points	2 points	☐	☐	4 points	4 points
3.8 points	2 points	☐	☐	4 points	4 points
4 points	2 points	☐	☐	4 points	4 points
4.2 points	2 points	☐	☐	4 points	4 points
5 points	2 points	☐	☐	4 points	4 points

A.3 Additional results

Table 5: Random Effects GLS regression

	Dep. variable: x-score (*Domain of disadv. inequality*)				Dep. variable: y-score (*Domain of adv. inequality*)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All subjects	Altruists	Selfish	Maximin	All subjects	Altruists	Selfish	Maximin
Week two (W2)	0.23	-0.00	0.18	0.02	0.10	0.03	0.17	0.16
	(1.83)	(-0.06)	(1.71)	(0.71)	(0.81)	(0.31)	(2.10)	(1.28)
Treatment (T)	0.18	0.25	0.13	-0.03	0.08	0.09	0.05	0.10
	(1.26)	(2.49)	(1.13)	(-0.41)	(0.63)	(0.85)	(0.46)	(0.80)
Intervention (I)	0.12	-0.32**	0.07	0.24	0.08	0.02	0.22	-0.13
	(0.93)	(-2.96)	(0.59)	(1.72)	(0.64)	(0.15)	(2.16)	(-0.76)
I x T	-0.42	-0.12	-0.24	-0.17	-0.23	-0.20	-0.07	-0.10
	(-2.20)	(-0.64)	(-1.32)	(-0.70)	(-1.32)	(-1.36)	(-0.43)	(-0.42)
I x T x W2	0.67	0.11	0.39	0.17	0.24	0.11	0.13	0.14
	(1.95)	(0.36)	(1.39)	(0.50)	(0.76)	(0.42)	(0.51)	(0.35)
I x W2	-0.19	0.19	-0.08	-0.03	-0.09	-0.07	-0.19	-0.10
	(-0.83)	(1.08)	(-0.40)	(-0.14)	(-0.42)	(-0.37)	(-1.17)	(-0.36)
T x W2	-0.44	-0.21	-0.17	0.05	-0.19	-0.08	-0.06	-0.24
	(-2.33)	(-1.59)	(-1.25)	(0.71)	(-1.12)	(-0.54)	(-0.49)	(-1.41)
Observations	1016	402	302	208	1016	402	302	208
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors clustered on the individual level. The stars in the table stand for the following p-values: * $p < 0.0125$, ** $p < 0.00625$, *** $p < 0.00125$. Since we are simultaneously testing eight hypotheses, these levels correspond to the Bonferroni corrected, standard significance levels of 10%, 5% and 1%, respectively. 'Week two' = 1 in the second week and zero otherwise; 'Treatment' = 1 in treatment condition and zero otherwise; 'Intervention' = 1 if the intervention takes place; the other variables are interaction terms of these dummies. Controls include gender, age, whether the subject studies economics or not, the cognitive reflection test and the RAT. z statistics in parentheses.

A.4 Instructions – not intended for publication

[*Instructions were originally in German.*]

Instructions [WEEK 1]

Welcome to an experiment over decision making!
Thank you for your participation!

Please, from now on, do not talk anymore to the other participants. For a better understanding, in the following we will only use male designations. Those should be understood as gender neutral.

During the experiment, you and the other participants are asked to take decisions. All decisions are to be taken on the computer. During the experiment, you are connected to other participants, but you will not know with which participants you are connected respectively. Your own as well as the decisions of the participants that are connected with you will determine your payments, according to the rules that follow below. Each participant will only be informed about his / her payment, but will not know how much other participants earn.

The experiment consists of five parts in total. Parts 1, 2 and 3 will take place now and will last for around 50 Minutes. Parts 4 and 5 will take place next Tuesday, 03.02.2015 at 10:00, and will last for around 60 Minutes.

For group 2, instructions were adapted as follows: [The experiment consists of five parts in total. Parts 1 and 2 will take place now and will last for around 60 Minutes. Parts 3, 4 and 5 will take place next Tuesday, 03.02.2015 at 10:00, and will last for around 50 Minutes.]

The five parts of the experiment are completely independent from each other. That is, your payment for part x only depends on decisions that you take in part x, and does not depend on decisions you take in other parts of the experi-

ment.

At the beginning of each part you receive the specific instructions. We will read the instructions out loud and will give you time for questions.

Your earnings are given in points. At the end of the experiment, the points will be converted in Euros and the amount will be paid to you in cash. The exchange rate is the following:

$$1 \text{ point} = 1 \text{ Euro}.$$

Please note that the total amount you may earn in the five parts of the experiment will be paid out at the end of part 5 in the next week. Please note also that you will receive the amount you earn today only if you participate in the parts 4 and 5 in the next week. If you are impeded to come next week, your earnings of part 1, 2 and 3 will lapse.

For group 2, instructions were adapted as follows: [Please note also that you will receive the amount you earn today only if you participate in the parts 3, 4 and 5 in the next week. If you are impeded to come next week, your earnings of part 1 and 2 will lapse.]

Whenever you have questions – also during the experiment – please raise your hand or attract attention to yourself. We will then come to your seat. Please do not ask questions in public.

Instructions to part 1 of the experiment [Part 3 for group 2]

Part 1 of the experiment consists of 10 decisions. In each of these 10 decisions, you will be paired with another participant of the experiment at random. We will denote the participant paired with you in the following 'your passive person'. You will see later on, why we denote this person 'passive person'. You will not know at any point in time the identity of your passive person. Your passive person will not know at any point in time your identity.

Each of your 10 decisions is a choice between the alternatives LEFT and RIGHT. Each alternative has consequences for your own payment and for the payment to your passive person.

Example: You may be asked if you prefer alternative LEFT, in which you receive 3.8 points and your passive person 6 points, or alternative RIGHT, in which you receive 4 points and your passive person receives 4 points as well. You then have to decide which of the two alternatives to choose. This decision problem is presented on the screen as follows:

Alternative LINKS	Alternative LINKS	IHRE WAHL	Alternative RECHTS	Alternative RECHTS
Sie erhalten Euro	Passive Person erhält Euro	Hier Ihre Wahl anklicken	Sie erhalten Euro	Passive Person erhält Euro
3.8	6.0	LINKS ○ ○ RECHTS	4.0	4.0

You will take 10 of such decisions in total. After you have taken all decisions, you will see all 10 decisions on one screen. You may then correct them once.

Your payment of part 1 of the experiment is determined as follows:

Payment as active person: For each participant one of the 10 decision situations is selected separately and at random, and the alternative chosen in the respective situation will then be paid out. If, for example, the situation above would be

selected and if you had chosen in the above situation the alternative RIGHT, then you would receive 4 points as active person, while your passive person would receive 4 points as passive person.

Payment as passive person: As your passive person receives points from your decision, without doing anything for it, you also receive points from another participant in the experiment, without doing anything for it; that is, you are for another participant the passive person. We will ensure that we will not draw your active and passive person being the same person. That is, if person X is your passive person, then for sure you will not be the passive person of person X.

Do you have questions?

Instructions to part 2 of the experiment [Part 4 for group 2]

Part 2 of the experiment consists of 10 decisions. Each of the decisions is the choice between alternative A and alternative B. Each decision only has consequences for your own payment, not for the payment of other participants in the experiment.

Alternative A always presents itself as an uncertain payout: With 50% probability you will receive 5 points, with 50% probability, 0 points.

Alternative B always presents itself as a certain payout: With 100% probability you will receive the amount that varies from decision to decision.

In total you take 10 of these decisions. After you have taken all decisions, you will see again all 10 decisions on one screen. You may then correct them once.

Your payment of part 2 of the experiment is determined as follows:

One of the 10 situations will be selected at random and used for payment. All situations have the same probability to being selected. Your payment of part 2 is then determined as follows:

- If you have chosen alternative A in the selected situation, then, with 50% probability you will receive 5 points and with 50% probability you will receive 0 points.
- If you have chosen alternative B in the selected situation, then you will receive the certain amount in the selected decision.

Do you have questions?

Instructions to part 3 of the experiment [Part 5 for group 2]

In part 3 of the experiment you will be invited to answer some questions.

Your payment of part 3 of the experiment is determined as follows:

For each correct answer you will receive 1 point.

[**On the computer screen:**]

- A bat and a ball cost 1.10 Euro in total. The bat costs 1.00 Euro more than the ball. How many cents does the ball cost? __ cents
- If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? __minutes
- In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half the lake? __days

Instructions [WEEK 2] [WEEK 1 for group 2]

Welcome to an experiment over decision making!

Thank you for your participation!

Please, from now on, do not talk anymore to the other participants. For a better understanding, in the following we will only use male designations. Those should be understood gender neutral.

During the experiment, you and the other participants are asked to take decisions. All decisions are to be taken on the computer. During the experiment you are connected to other participants, but you will not know with which participants you are connected respectively. Both your own as the decisions of the participants that are connected with you will determine your payments, according to the rules that follow below. Each participant will only be informed about his / her payment, but will not know how much other participants earn.

The experiment consists of five parts in total. Today, parts 4 and 5 will take place. They will last for approximately 50 minutes.

For group 2, instructions were adapted as follows: [Today, parts 3, 4 and 5 will take place. They will last for approximately 60 minutes.]

The five parts of the experiment are completely independent from each other. That is, your payment for part x only depends on decisions that you take in part x, and does not depend on decisions you take in other parts of the experiment.

At the beginning of each part you receive the specific instructions. We will read the instructions out loud and will give you time for questions.

Your earnings are given in points. At the end of the experiment, the points will be converted in Euros and the amount will be paid to you in cash.

The exchange rate is the following:

1 point = 1 Euro.

Whenever you have questions – also during the experiment – please raise your hand or attract attention to yourself. We will then come to your seat. Please do not ask questions in public.

Instructions to part 4 of the experiment [Part 1 for group 2]

In this part of the experiment, the participants in the experiment have to accomplish different tasks. The task you have to accomplish depends on the number you have been assigned. Your number is: 301 [*401 for the treatment group*].

Please enter this number in the computer in the correspondent field.

Your task in this part of the experiment consists of identifying letter sequences in the text on the enclosed sheet of paper (title: task sheet), in which the letter 'e' appears according to a specific pattern.

The exact instructions after which pattern you have to search you will find at the beginning of the task on your computer screen.

Please cross out the identified relevant 'e's, and subsequently write down the number of occurrences below the respective paragraph in the corresponding field.

You have 5 minutes for this task. Time will be measured from the experiment instructor. It does not play a role which paragraphs you work on. After 5 minutes, the instructor of the experiment will ask you to stop. Please write down then the number of relevant 'e's for each paragraph you have been working on in the corresponding field in the computer; you have 1 minute for this task.

At the end of this part of the experiment

We will collect all sheets and check if you have crossed out all relevant 'e's in the paragraphs for which you have entered a number unequal zero in the computer.

Your payment for part 4 of the experiment is determined as follows:

You will receive 3 points for each paragraph in which you have identified the number of relevant ‘e’s correctly. For paragraphs for which your answer is almost correct you will receive 1 point. As almost correct we count all answers, for which the given number is by 1 larger or smaller than the correct number.

Please write down your 6-digit participation number on the following sheet of paper with the title ‘task sheet’.

Do you have questions?

Task sheet

Please write down your participant number: _____

Paragraph 1:

Down, down, down. There was nothing else to do, so Alice soon began talking again. 'Dinah'll miss me very much to-night, I should think!' (Dinah was the cat.) 'I hope they'll remember her saucer of milk at tea-time. Dinah my dear! I wish you were down here with me! There are no mice in the air, I'm afraid, but you might catch a bat, and that's very like a mouse, you know. But do cats eat bats, I wonder?' And here Alice began to get rather sleepy, and went on saying to herself, in a dreamy sort of way, 'Do cats eat bats? Do cats eat bats?' and sometimes, 'Do bats eat cats?' for, you see,

This paragraph contains __ relevant 'e's.

Paragraph 2:

as she couldn't answer either question, it didn't much matter which way she put it. She felt that she was dozing off, and had just begun to dream that she was walking hand in hand with Dinah, and saying to her very earnestly, 'Now, Dinah, tell me the truth: did you ever eat a bat?' when suddenly, thump! thump! down she came upon a heap of sticks and dry leaves, and the fall was over. Alice was not a bit hurt, and she jumped up on to her feet in a moment: she looked up, but it was all dark overhead; before her was another long passage, and the White Rabbit was still in sight, hurrying

This paragraph contains __ relevant 'e's.

Paragraph 3:

down it. There was not a moment to be lost: away went Alice like the wind, and was just in time to hear it say, as it turned a corner, 'Oh my ears and whiskers,

how late it's getting!' She was close behind it when she turned the corner, but the Rabbit was no longer to be seen: she found herself in a long, low hall, which was lit up by a row of lamps hanging from the roof. There were doors all round the hall, but they were all locked; and when Alice had been all the way down one side and up the other, trying every door, she walked sadly down the middle, wondering how she was ever to get

This paragraph contains __ relevant 'e's.

Paragraph 4:

out again. Suddenly she came upon a little three legged table, all made of solid glass; there was nothing on it except a tiny golden key, and Alice's first thought was that it might belong to one of the doors of the hall; but, alas! either the locks were too large, or the key was too small, but at any rate it would not open any of them. However, on the second time round, she came upon a low curtain she had not noticed before, and behind it was a little door about fifteen inches high: she tried the little golden key in the lock, and to her great delight it fitted! Alice opened the door

This paragraph contains __ relevant 'e's.

Paragraph 5:

and found that it led into a small passage, not much larger than a rat-hole: she knelt down and looked along the passage into the loveliest garden you ever saw. How she longed to get out of that dark hall, and wander about among those beds of bright flowers and those cool fountains, but she could not even get her head through the doorway; 'and even if my head would go through,' thought poor Alice, 'it would be of very little use without my shoulders. Oh, how I wish I could shut up like a telescope! I think I could, if I only know how to begin.' For, you see, so many out of the way

This paragraph contains __ relevant ‘e’s.

Paragraph 6:

things had happened lately, that Alice had begun to think that very few things indeed were really impossible. There seemed to be no use in waiting by the little door, so she went back to the table, half hoping she might find another key on it, or at any rate a book of rules for shutting people up like telescopes: this time she found a little bottle on it, (’which certainly was not here before,’ said Alice,) and round the neck of the bottle was a paper label, with the words ’DRINK ME’ beautifully printed on it in large letters. It was all very well to say ’Drink me,’ but the wise little Alice was not

This paragraph contains __ relevant ‘e’s.

Instructions to part 5 of the experiment [Part 2 for group 2]

Part 5 of the experiment consists of 10 decisions. In each of these 10 decisions, you will be paired with another participant of the experiment at random. We will denote the participant paired with you in the following 'your passive person' . You will see later on, why we denote this person 'passive person'. You will not know at any point in time the identity of your passive person. Your passive person will not know at any point in time your identity.

Each of your 10 decisions is a choice between the alternatives LEFT and RIGHT. Each alternative has consequences for your own payment and for the payment to your passive person.

Example: You may be asked, if you prefer alternative LEFT, in which you receive 3.8 points and your passive person 6 points, or alternative RIGHT, in which you receive 4 points and your passive person receives 4 points as well. You then have to decide which of the two alternatives to choose. This decision problem is presented on the screen as follows:

Alternative LINKS	Alternative LINKS	IHRE WAHL	Alternative RECHTS	Alternative RECHTS
Sie erhalten Euro	Passive Person erhält Euro	Hier Ihre Wahl anklicken	Sie erhalten Euro	Passive Person erhält Euro
3.8	6.0	LINKS ○ ○ RECHTS	4.0	4.0

You will take 10 of such decisions in total. After you have taken all decisions, you will see all 10 decisions on one screen. You may then correct them once.

Your payment of part 5 of the experiment is determined as follows:

Payment as active person: For each participant one of the 10 decision situations is selected separately and at random, and the alternative chosen in the respective situation will then be paid out. If for example the situation above would be

selected and if you had chosen in the above situation the alternative RIGHT, then you would receive 4 points as active person, while your passive person would receive 4 points as passive person.

Payment as passive person: As your passive person receives points from your decision, without doing anything for it, you also receive points from another participant in the experiment, without doing anything for it; that is, you are for another participant the passive person. We will ensure that we will not draw your active and passive person as the same person. That is, if person X is your passive person, then for sure you will not be the passive person of person X.

Do you have questions?