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Social Preferences and Self-Control*

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Abstract

We provide new evidence on the impact of diminished self-control on social preferences in the ultimatum game. In a sample of German university students ($N = 312$), depleted proposers made lower offers, and depleted responders rejected unfair offers as often as non-depleted ones. This agrees with previous evidence on the Dictator Game but stands in contrast with a previous study with a sample of Spanish university students. A possible explanation is that selfish motives are the default mode of behavior, but there is individual heterogeneity on whether strategic fairness (fear of rejection) can overcome them.

Keywords: Ultimatum game, self-control, ego depletion, social preferences

JEL-Classification: C72, C91

1 Introduction

Several recent contributions have examined the effects of exhausted self-control on social preferences. The general logic is that depleted self-control resources increase reliance on automatic processes of decision making, uncovering the "default" motives underlying behavior (e.g., Muraven et al., 1998). The basic hypothesis is that selfishness is implemented through

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more automatic processes on which participants rely more under self-control depletion, while prosocial motives are implemented in a controlled way. Consistent with this hypothesis, in the Dictator Game (DG) depleted participants give less than non-depleted ones (Achtziger et al., 2015). Also, in a small-sample study ($N = 29$) by Halali et al. (2013), proposers in the Ultimatum Game (UG) made lower offers. However, in the UG proposer behavior entails a strategic element, as the responder might reject (low) offers. Hence "fear of rejection" is an additional motive. The basic hypothesis only remains unchanged if this motive is implemented in a deliberative (controlled) way.

Halali et al. (2014) showed that depleted responders in the UG rejected more unfair offers than non-depleted ones. The interpretation is that responder decisions result from the interaction between monetary concerns and affective (impulsive) processes, the latter leading to the rejection of unfair offers and being implemented more automatically than monetary motives.

However, in a previous, large study ($N = 288$; Achtziger et al., 2016) we found that depleted proposers made on average *higher* offers and depleted responders accepted *more* unfair offers than non-depleted ones. The UG sessions in Achtziger et al. (2016) were run in Spain, on the same week and in the same laboratory as the DG sessions in Achtziger et al. (2015). Participant samples were disjoint but drawn from the same population. For the interpretation of the UG results, the DG study implies that "fear of rejection" is even more automatic than monetary concerns for proposers in the UG. That is, in that study, depleted proposers were motivated by fear of not getting the money if rejected and responders just wanted to get away with any positive amount of money in the game.

In addition to self-control depletion, a number of techniques and manipulations have been used to investigate the question of which is the default mode of behavior in social decision making. Piovesan and Wengström (2009) showed that more selfish decisions are associated with shorter response times in a variant of the DG, which is consistent with the view that monetary concerns are more automatic (hence generally faster) for proposers. However, Fischbacher et al. (2013) found a marked heterogeneity in response time patterns when subjects are classified according to their responder behavior in a series of mini-ultimatum games, implying that conclusions derived from response time measurement might be elusive in the domain of social preferences. Cappelen et al. (2016) found that fair decisions in a DG were faster than selfish ones and inferred that fair decisions are more intuitive, but Myrseth and Wollbrant (2016) argue that median response times are too large to support this conclusion (because all decisions might have entailed deliberation) and that the argument might suffer from a reverse-inference fallacy.

Sutter et al. (2003) investigated the effects of time pressure on responder behavior in the UG, the idea being that this manipulation impairs controlled processes and forces decision makers to rely on automatic processes more often. Responders were more likely to reject under time pressure, indicating that emotional reactions are more automatic and accepting every positive amount is the result of a more controlled, slower process. However, Sutter et al. (2003) also found that the effect disappears with repetition. Cappelletti et al. (2011) also investigated time pressure in the UG. The study employed the strategy method, that is, responders responded to each possible offer before knowing the offer, hence effectively stating a minimum threshold of acceptance (MTA). Every participant played both as a proposer and as a responder. As in Sutter et al. (2003), responders were more likely to reject under time pressure. However, proposers were found to offer more under time pressure in Cappelletti et al. (2011). The authors argued that this reflects more strategic considerations than other-regarding preferences: strategic proposers might make offers above their own MTA to avoid rejection, expecting the responder to behave as the proposer would.

Other studies have employed cognitive load, which is assumed to differentially impair controlled processes, hence leading to an increased reliance on automatic processes. Cappelletti et al. (2011) also tested cognitive load in the UG, but there was no significant effect neither for proposers nor for responders. Benjamin et al. (2006) and Hauge et al. (2009) also found no effects of cognitive load in dictator games. The exception is Schulz et al. (2014), where a particularly strong cognitive load manipulation was used. In this study, dictator mini-games were used, where participants chose between a fair and an unfair allocation. High-load condition subjects chose the fair allocation more often than low-load subjects. Cornelissen et al. (2011) also find no main effect of cognitive load in a dictator game; however, they do find an interaction effect in which participants classified as pro-social in a different task were more generous in the high-load condition.

The effects of manipulations as cognitive load or time pressure have also been studied in the framework of cooperation, employing, e.g., Prisoner's Dilemma or public good games. Although those games are strategically quite different from the UG and other games used to study social preferences, cooperative behavior in the face of a dominant, "egoistic" strategy (defecting in the Prisoner's Dilemma or refraining from contribution in public good games) is conceptually related to prosocial behavior in DG/UG games. Duffy and Smith (2014) found some evidence that subjects under (high) cognitive load behaved less strategically in a repeated multi-player Prisoner's Dilemma game, but, at the same time, the rates of cooperation were slightly lower among them compared to subjects under low load. In contrast, Døssing et al. (2017) find that subjects under higher cognitive load showed higher levels of initial cooperation in a repeated public goods game. Rand et al. (2012) (see also Rand et al., 2014) reported increased cooperation in social dilemmas for subjects under time pressure and put forward the hypothesis that cooperation is intuitive and grounded on a "social heuristic." However, this evidence is highly contested: Tinghög et al. (2013) failed to replicate the results in a series of five experiments; a registered multi-lab replication (Bouwmeester et al., 2017) found no causal impact of time pressure on cooperation; and Myrseth and Wollbrant (2017) contest the analysis in Rand et al. (2012, 2014) and argue that the data on those papers does not actually support the conclusion that cooperation is intuitive.

In view of the inconsistent findings regarding the default mode of behavior in social decision making, it is clear that previous evidence is insufficient to fully understand the impact of ego depletion and other manipulations in this field. We focus here on ego-depletion manipulations and present additional evidence on the effects of diminished self-control on social preferences, by using an almost identical design for the UG as in Achtziger et al. (2016), but a different subject pool (German university students).

In addition to the main question of interest, this work also contributes to the recent discussion on the effects of ego depletion on decision making, which has pointed out that effect sizes might have been overestimated due to publication bias (see, e.g., Carter and McCullough, 2014) and has raised doubts on the effects of certain manipulations (Hagger et al., 2016).

Vohs et al. (2008) showed that ego-depletion effects intensify when repeated decisions are made subsequently (as each subsequent decision requires self-control). Achtziger et al. (2015, 2016) used a framework with repeated decisions and confirmed that the effects of depletion manipulations do not vanish over the course of an experimental session with repeated decisions in strategic games. We follow those works and rely on a framework with repeated decisions.

Also as in Achtziger et al. (2015, 2016), we used monetary incentives in the manipulation task. This is of independent interest, as the vast majority of studies using depletion manipulations use non-incentivized tasks, but our previous studies showed that incentivizing the manipulation task does not counteract its effects on self-control.

2 Design and Procedures

The experiment was programmed in z-Tree (Fischbacher, 2007) and conducted at the University of Konstanz (Germany). It comprised 13 sessions of 12 proposers and 12 responders each ($N = 312$). Average earnings were 11.02 Euros. Subjects were recruited through ORSEE (Greiner, 2015), excluding students from economics or psychology.

The two-part setup followed Achtziger et al. (2016). Part 1 used a task from Baumeister et al. (1998) for 5 minutes. In the low-ego-depletion (LED) treatment, participants had to cross out all "e" letters in a series of paragraphs (blocks) from a physics textbook. In the high-ego-depletion (HED) treatment, participants had to cross out "e"s following a more demanding rule which required inhibition and hence depleted self-control resources. Specifically, "e"s had to be crossed unless either another vowel followed the letter or if there was another vowel exactly two letters away in either direction. Participants typed the number of crossed-out "e"s per block on-screen, and we checked the number of blocks actually worked out.

In part 2, participants played 12 Ultimatum Games (UGs) under perfect-stranger matching. In each UG, the proposer offered a share of a fixed monetary amount of 7 units (an integer from 0 to 7), and the responder decided whether to accept it or reject it (in which case both players received nothing). Participants were randomly allocated to the two roles. Participants were not aware of which version of the ego-depletion task their opponents had faced (they were also not aware of the fact that there were two versions).[1]

We used only two of the incentive treatments in Achtziger et al. (2016) (called LED-F and HED-P there). LED subjects received a flat-fee of 8 ECU (exchange rate 1 ECU = 0.25 Euros) for the depletion manipulation, whereas HED subjects received 4 further ECU for each correctly-solved block, and 2 ECU for each near miss ($+/-1$ crossed-out "e"s). Feedback on the number of correctly-solved blocks was given at the end of the part. This change was made to ensure that participants would work in the demanding task, but should be inconsequential because Achtziger et al. (2016) found no differences in UG behavior accruing to the payment method in part 1. Another difference was that, as previous studies, Achtziger et al. (2016) included an habituation phase where all participants had to cross out all "e"s. However, Baumeister et al. (1998) reported no habituation phase, and recent implementations of the task do not include it (Sripada et al., 2014; Alós-Ferrer et al., 2015). We will return to these

[1] In five sessions half of the proposers and half of the responders were in each treatment, as in Achtziger et al. (2016). The remaining sessions employed "pure matchings" between proposers and responders (HED/HED, HED/LED, LED/HED, and LED/LED). There were no matching effects.

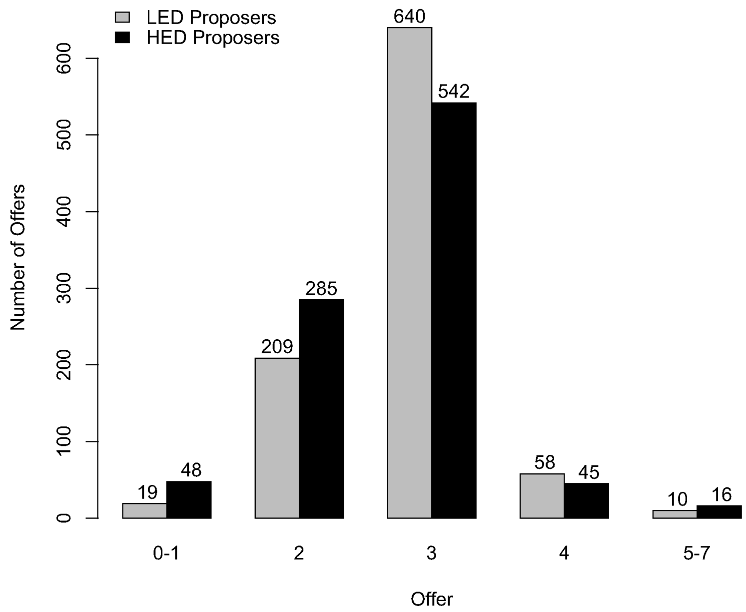


Figure 1: Histogram of proposers' offers.

differences in the discussion.

3 Results

The HED task was cognitively more demanding than the LED task. Compared to LED subjects, in part 1 HED subjects worked on significantly less blocks (HED, mean 1.564; LED, 2.673; Wilcoxon rank-sum (WRS) test, $z = 11.662$, $p < 0.001$) and solved significantly less blocks correctly (1 point per correct answer, 0.5 per near miss: HED, mean 0.183; LED, 0.403; WRS test, $z = 3.705$, $p < 0.001$).

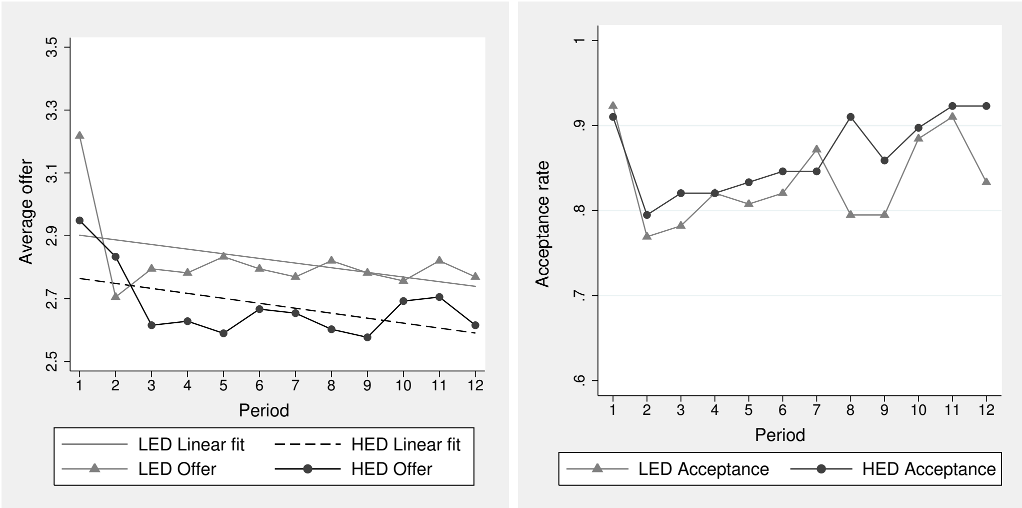
Depleted proposers offered *less* than non-depleted proposers with 36% of the offers made by HED proposers being 3 or less, compared to only 24% of offers made by LED proposers (Figure 1). In the first period (which is of particular interest as the decision is unaffected by learning effects), depleted proposers offered an average of 2.949, compared to 3.218 by non-depleted proposers. OLS regressions on first-period offers in Table 1 confirm that HED proposers offered less, revealing a significantly negative ego-depletion coefficient ($p = 0.042$ in Model 1, $p = 0.043$ in Model 2). Contrary to Achtziger et al. (2016), a gender dummy in Model 2 revealed no evidence for gender differences on offers.

	Model 1	Model 2
Ego depletion (HED=1)	-0.269^{**}	-0.275^{**}
	(0.131)	(0.134)
Male		0.030
		(0.134)
Constant	3.218^{***}	3.210^{***}
	(0.085)	(0.097)
Observations (subjects)	156	156
R^2	0.027	0.027

Table 1: OLS regressions on first-period proposer offers. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$.

Regarding repeated interactions, Figure 2(a) illustrates a weakly decreasing trend in average offers, with depleted proposers offering less than non-depleted proposers in 11 out of 12 periods (overall average 2.677 in HED, 2.821 in LED). Table 2 reports panel GLS regressions on proposer offers with standard errors clustered by session and subject random effects. Model 1 controls for acceptance rate and acceptance of the previous offer; Model 2 controls for period and gender. Both models confirm that HED proposers made significantly lower offers ($p = 0.021$ and $p = 0.034$, respectively). The effect of the acceptance rates of previous offers and whether the last round's offer was accepted are significantly negative. That is, offers decrease as offers are accepted (Roth et al., 1991). Unlike in Achtziger et al. (2016), depletion did not interact significantly with the number of blocks worked or the number of correctly-solved blocks, and including those variables did not improve model fit.

Regarding responder behavior, average acceptance rates are almost identical between treatments in the first period (HED, 0.910; LED, 0.923; $\chi^2 = 0.084$, $p = 0.772$). The average acceptance rate over all periods was 0.834 for LED and 0.865 for HED responders. In both treatments, the per-period acceptance rate increases slowly over time after an initial drop (Figure 2(b)). Table 3 reports probit panel regressions with subject random effects and standard errors clustered by session. Large offers increase the likelihood of acceptance in all models, and acceptance becomes significantly more likely as more unfair offers are observed. However, the ego-depletion coefficient is not significant ($p = 0.749$ in Models 1 and 2). Adding the number of blocks worked and the number of correctly-solved blocks as regressors did not change the results.



(a) Proposer offers

(b) Responder acceptance rates

Figure 2: Proposer and responder behavior over time.

	Model 1	Model 2
Accept rate until $t-1$	-0.314^{**}	-0.322^{**}
	(0.153)	(0.157)
Accept at $t-1$ (Yes=1)	-0.077^{**}	-0.074^{**}
	(0.035)	(0.037)
Ego depletion (HED=1)	-0.134^{**}	-0.131^{**}
	(0.058)	(0.062)
Male		-0.019
		(0.062)
Period		-0.002
		(0.007)
Constant	3.116^{***}	3.142^{***}
	(0.127)	(0.164)
Observations	1716	1716
Number of groups (subjects)	156	156
Wald χ^2	22.95	22.83
Prob $> \chi^2$	0.000	0.000

Table 2: Random-effects GLS regressions on proposer offers. Standard errors (clustered by session) in parentheses. *** $p < 0.01$, ** $p < 0.05$.

4 Discussion

Results for proposer behavior are in line with the hypothesis that monetary/egoistic concerns are implemented more automatically than fairness concerns. This agrees with Achtziger

	Model 1	Model 2
Offer at t	2.293***	2.322***
	(0.244)	(0.262)
Unfair offer accumulated	0.223***	0.272***
	(0.040)	(0.082)
Ego depletion (HED=1)	0.113	0.114
	(0.355)	(0.356)
Male		0.056
		(0.294)
Period		−0.022
		(0.031)
Constant	−4.581***	−4.627***
	(0.741)	(0.804)
Observations	1872	1872
Number of groups (subjects)	156	156
Wald χ^2	139.22	139.63
Prob > χ^2	0.000	0.000

Table 3: Random-effects probit regressions on responder acceptance. Standard errors (clustered by session) in parentheses. *** $p < 0.01$.

et al. (2015), in which depleted proposers made lower offers in a Dictator Game. Regarding responder behavior, ego depletion had no significant effect on acceptance decisions even after adding further controls.

The discrepancy with Achtziger et al. (2016), where depleted proposers made higher offers and depleted responders accepted more unfair offers, is particularly interesting. At this point, four possible factors contributing to an explanation need to be discussed.

The first factor concerns a minute difference in the experimental design, reported above. Contrary to Achtziger et al. (2016), and in the interest of speed, we did not include an "habituation phase" in the current experiment. In such a phase, all participants have to cross out all "e"s before moving on to the actual depletion task, the logic being that habitual behavior (to be inhibited later or not) is enhanced in this way. However, at the time of the experiment the psychological literature was unclear on whether such a phase was needed or not. The original paper introducing the "e" task did *not* report a habituation phase (Baumeister et al., 1998), and other experiments in the literature reported using the task without a habituation phase (e.g. Wan and Sternthal, 2008). A variant of the task was implemented by Sripada et al. (2014) in a computerized version which employs no habituation phase (also used in Alós-Ferrer et al., 2015). However, after our experiment, a multi-lab registered replication report (Hagger et al., 2016) failed to find significant effects using the computerized version of Sripada et al. (2014). Baumeister and Vohs (2016) responded raising doubts on whether the task was appropriate to induce ego depletion at all. Their main objection was the lack of an habituation phase in the computerized task used in Hagger et al. (2016). Hence, one could transfer the criticism to our experiment, as we did not use a habituation phase here (but we did in Achtziger et al., 2015, 2016).

Of course, we cannot discard this interpretation. However, it should be observed that the issue raised with the replication of Hagger et al. (2016) was that no significant effects were found. We did find clearly significant effects for proposer behavior in our study, which are conceptually in perfect alignment with previous results (Achtziger et al., 2015; Halali et al., 2013). Hence, it his hard to reconcile a view that our task might not have induced ego depletion with the results we do actually obtain.

The second factor concerns an even subtler difference in the experimental design, also reported above. In the experiment, the results of the incentivized depletion phase were given to participants after that phase, instead of at the end of the experiment as in Achtziger et al. (2015, 2016). This change, which helped presenting the tasks as completely separate, appeared unproblematic to us because the magnitude of the involved incentives is tiny. Recall that performance (number of blocks correctly solved) was identical to earnings in the depletion task in the HED but not in the LED condition. Participants in both conditions received a payment of 8 ECU for working on the task, but those in the HED condition received an additional piece rate of 4 ECU for each correctly solved block (or 2 ECU for an almost-correctly solved block). The mean piece-rate earnings due to the piece-rate incentives in the depletion task was .94 ECU ($SE = 1.82$) for proposers and 1.19 ECU ($SE = 1.99$) for responders. Since the exchange rate was 1 Euro = 4 ECU, the average piece-rate earnings were about 25 Eurocents, which is quite small in comparison to the total amount earned in the experiment (mean 11.02 Euros).

Still, it is not possible to completely rule out spillover effects from the depletion task to behavior in the UG. We therefore reran all regressions reported above including earnings in the depletion task as an additional regressor. This additional variable had no significant impact on proposer offers or responder acceptance decisions in any of the regressions. Its inclusion did not improve the overall fit of any of the models, and it did not change the effect of any of the previous regressors of interest.[2] Hence, there is no evidence that earnings in the

[2]For example, adding the earnings variable to Model 1 in Tables 1, 2, and 3 yields coefficients of $\beta = .006$ ($SE = .048$, $p = 0.894$), $\beta = -.006$ ($SE = .028$, $p = .844$), and $\beta = .026$ ($SE = .069$, $p = .703$), respectively.

depletion task influenced behavior in the UG.

The third factor and possible reason for the discrepancy with Achtziger et al. (2016) is sample heterogeneity. It is tempting to speculate that Spanish' subjects' intuition might be more altruistic than that of German subjects. However, this evidence is at odds with the results of Achtziger et al. (2015), where depletion resulted in clearly lower offers for Spanish' subjects playing the DG.[3]

The fourth factor is also related to sample heterogeneity. Although this is of course speculative, we hypothesize that the dominant responses for the UG uncovered in Achtziger et al. (2016) might be driven by local economic conditions. After the recent economic crisis, prospects for Spanish university students were rather bleak, with youth (under 25) unemployment rates in the 42-56% range for Spain (compared to, e.g., the 7-11% range for Germany) in the period 2009-13. On the face of gloomy economic prospects, strategic monetary motives might have become heavily automatized. That is, monetary concerns (selfishness) are implemented more automatically than prosocial ones, but for a population permanently exposed to dire economic prospects (University students in Spain), the need to secure the monetary payoff (by e.g. reducing the possibility of rejection through higher offers) might have become even more automatized. This is, however, a *post hoc* interpretation and, since we did not collect socioeconomic variables which could be used as controls, we cannot further explore it as a possible explanation.

In any case, the discrepancy in results between this paper and our previous work should not obscure the main point. The depletion manipulation we employ is rather short and subtle. Just a few minutes of crossing out letters on a piece of paper seems to alter the balance between motives underlying the decisions summarized by social preferences. The fact that such a mild intervention can affect decisions in strategic contexts indicates that the balance between selfish, prosocial, and other motives is a very fragile one, easily tilted by seemingly irrelevant factors.

The purpose of psychological manipulations as ego depletion, cognitive load, or time pressure in games as the UG or the DG is to uncover the "default" mode of behavior by investigating whether prosociality or selfishness are implemented more automatically/impulsively. Results in this field might be mixed because of individual and sample heterogeneity. That is, dominant/default responses might be more selfish for some decision makers, and more (ap-

[3]As pointed out by one reviewer, the depleted proposers in Achtziger et al. (2016) were offering more even though they faced responders who were willing to accept less, which could be construed as a strategic failure. However, the strategic behavior referred to here would assume that proposers hold correct beliefs on the responders' behavior and are also perfectly aware of the true effects of the manipulation on others.

parently or truly) prosocial for others. This view is natural if one recalls that, as pointed out by Bargh (1989), automatic decision processes are often those which have become "automatized," i.e., for context-dependent decisions what is more automatic depends on individual experience. We expect that, as long as no theory to identify (or experimental procedure to manipulate) the underlying dominant response (motivation) of *individual* participants is developed, the effect of psychological manipulations on social preferences in strategic settings can go either way.

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