Mental Accounting of Public Funds – The Flypaper Effect in the Lab*

by

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Abstract

This paper reports evidence from a laboratory experiment that focusses on mental accounting of 'public funds'. Groups of three players decide upon how much to redistribute within the group. We find that the inclination to redistribute is significantly higher if it is financed out of a common budget. Since the common budget is otherwise used for the players' private consumption, its relative size should not affect the decision to redistribute. We interpret this finding as evidence for a flypaper effect due to mental accounting and discuss implications for tax policy and government spending.

JEL classification: C92, D72, H31

Keywords: mental accounting, flypaper effect, lab experiments

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1 Introduction

Empirical evidence suggests that voters are more likely to agree to government expenditures out of excess tax revenue than out of new taxes. Precisely speaking, a positive shock in available public funds will result in higher public expenditures than if an equivalent shock occured in the form of private income. In the literature, this effect is known as the 'flypaper effect' referring to the observation that the money sticks where it hits (Hines and Thaler 1995). The flypaper effect is hard to reconcile with standard models of policy-making. Optimal policy should be expected to equate the social marginal utility of public funds and that of private funds. It should not matter where these funds come from.

The literature provides a range of explanations for this phenomenon which we will review in section 2. In this paper, we report evidence from a lab experiment that allows focussing on one specific explanation for the flypaper effect: mental accounting (Thaler 1985). The concept of mental accounting assumes that households (or voters) assign different meanings to different kinds of funds. Public funds are 'meant' to be used for public expenditures. Hines and Thaler (1995) use this approach to theoretically explain the flypaper effect.

We use a standard redistribution experiment (as in Tyran and Sausgruber 2006, Höchtl et al. 2012) in which groups of three players are formed. Two of them ('the rich') receive an amount of money, the third ('the poor') does not. All three players vote on how much is given to the poor player and the intermediate vote (the median voter's proposal) is realized. As a novel element, we introduce a common budget for the two rich players the size of which is varied. We measure the impact of a variation of the common budget on the transfer proposals. Since the variation in the common budget is independent from the variation in the overall economy size, the common budget should, in principle, be completely irrelevant for the transfer proposal.

Since, however, the common budget may just be a reference point used for 'anchoring' the adequacy of proposals, we consider two different treatments that allow for precise identification of mental accounting. In the first treatment, the transfer is paid from the rich players' individual accounts. In the second treatment, the transfer is paid out of the common account. Since the money in the common account belongs to the rich players anyway, it should not matter in which way the money is taken from them. Our experiment, however, shows that the mode of representation, i.e. the framing of where the money comes from, matters.

We find that the transfers significantly increase in the size of the common budget (i.e. the public funds). If the common budget, measured in per cent of the economy size, increases by ten percentage points, the proposal increases by 0.5 to 1.2 percentage points. Especially the poor voters demand more redistribution if the common budget is large. Dividing the sample in different subgroups shows that those with below median math skills are prone to give more if the common account is relatively large. There are neither significant differences between male and female participants, though, nor between economics students and students from other disciplines. Using the proposals in the first part of the

game (without a common account), we divide the participants into four groups (very selfish, mildly selfish, mildly generous, very generous). Unsurprisingly, it is mainly the two intermediate ('mildly') groups that react to changes in the relative size of the common account.

Our results prove to be robust in a number of checks. A change in the order of treatments does not significantly change our results. We also change the framing by using 'account 1' and 'account 2' instead of 'individual account' and 'common account'. Surprisingly, the effects of the common account become even larger. Finally, we change the voting mechanism to a random dictator regime. Whereas the number of extreme proposals (give nothing and full redistribution) increases, the average effect remains unaffected.

The behavioral approach to the flypaper effect is rooted in the concepts of mental accouting, framing and fungibility. We review these concepts and the associated literature in more depth in the next section. Our paper adds to this literature by replicating a flypaper in the lab – in the absence of all confounding institutional influences. To the best of our knowledge, we are the first to experimentally show that the propensity to redistribute is larger if the transfer is funded out of a common budget instead of an individual budget – even though this should not make any difference.

The remainder of the paper is organized as follows. The next section reviews the literature on the flypaper effect and mental accounting. Section 3 describes the experimental setup. Section 4 reports the results of the experiment in the basic design and section 5 provides deeper insights by considering the results of two slight modifications. Section 6 concludes and discusses some implications.

2 Related literature and theory

The flypaper effect implies that governments are inclined to spend a large part (if not all) of an exogenous increase in public funds, e.g. due to a grant, but would not finance the spending increase via an increase in taxation. There are numerous studies using quasi-experimental designs to show the flypaper effect with real world data, e.g. Heyndels and van Driessche (1998, 2002), Dahlberg et al. (2008), Gennari and Messina (2014), Vegh and Vuletin (2015), Allers and Vermeulen (2016).[2]

As indicated above, there is a whole range of explanations for the flypaper effect. For our purpose, it is useful to differentiate between behavioral explanations and non-behavioral ones. With regard to the latter, there might be, in some cases, straightforward institutional reasons why public funds (e.g. grants from the central government) may not be distributed via tax cuts to households, see e.g. Brooks and Phillips (2008). Moreover, Dahlby (2011) argues that the flypaper effect may well be explained within the neoclassical model of second-

[2]The flypaper is not restricted to the realm of public finance. For instance, Jacoby (2002) analyzes the flypaper effect of a school feeding program on caloric intake within families. Choi et al. (2009) consider the household's portfolio choice and detect a flypaper effect which may be due to mental accounting.

best taxation. As long as there is an excess burden of taxation, there is an efficient low level of public goods provision if the planner is (close to) benevolent. If this efficient cost per unit of tax revenue is increasing at a sufficiently steep slope, an increase in funds may lead to an over-proportional increase in spending.

Apart from this, the flypaper effect may be explained by imperfect political decision-making. For instance, governments might shy away from the equity debate that preceeds tax reforms. Spending additional funds on public goods may be the politically less costly option. Moreover, a dysfunctional political system may be expansionary, i.e. uses additional funds to grow instead of maximizing household utility. The latter, however, would not explain why a negative shock on public funds is not responded to by tax hikes. Roemer and Silvestre (2002) build a model in which the flypaper effect occurs as an outcome of multi-party political competition.

A behavioral explanation for the flypaper effect has first been given by Hines and Thaler (1995, henceforth HT95) who presume that the flypaper effect may be due to one of (or the combination of) three behavioral irregularities. First, loss aversion[3] implies that taxpayers are "*much more sensitive to decreases in their welfare than to increases. This implies that the political cost of explicitly raising a tax is greater than the political benefit of an equivalent tax cut.*" (HT95, p. 223) This creates a policy discontinuity where additional need for public funds does not necessarily lead to tax hikes, and additional funds (grants) do not necessarily lead to tax cuts. However, this statement is based on the assumption that tax financed spending hikes are a welfare loss from the voters' point of view. This does not necessarily need to be the case.

Second, HT95 consider the possibility that the flypaper effect is due to framing[4]: "*The choices to the public are not framed as between spending the money or cutting taxes, but rather how should the money be spent.*" (HT95, p. 223). While this may be true, it remains an open question why political competition does not provide alternative framings if they are welfare-enhancing.

Third, mental accounting implies that the fungibility of public funds may be limited. "*We know that changes in housing wealth, pension wealth and future income have very different effects on consumption than equivalent present value changes in current liquid assets or income. So, it should be no great surprise that households violate fungibility in evaluating their political leaders.*" (HT95, p. 223) If public funds are mentally accounted for by voters in a different way than private funds, the propensity to spend on certain goods may differ across types of funds: Public funds are 'meant' to be spent on public goods, private funds should be spent on private consumption goods. In this sense, funds are not perfectly fungible anymore, as one Euro in private funds is treated differently

[3] A number of papers assumes loss aversion to rationalize various features of the tax system, see, e.g., Elffers and Hessing (1997), Yaniv (1999), Bernasconi and Zanardi (2004), Kanbur, Pirttila, and Tuomala (2008), or Dhami and al Nowaihi (2007, 2010).

[4] For the impact of tax framing see, e.g., Chang et al. (1987), Robben et al. (1990a), Robben et al. (1990b), Schepanski and Shearer (1995), Kirchler and Maciejovsky (2001) and Copeland and Cuccia (2002). For dissenting evidence see, e.g., Schadewald (1989).

from one Euro in public funds.

Our lab experiment is designed to exclusively focus on mental accounting of public funds which may limit their fungibility. In the lab economy, there are, obviously, neither institutional restrictions on how to spend the money nor an efficiency cost of taxation. We will not allow for public goods and, thus, eliminate the role of loss aversion which, according to HT95, may occur if individuals do not value public goods sufficiently (we will, however, discuss the potential role of loss aversion in a more subtle context below). Moreover, there will be no active framing by policy-makers. The only framing we admit is that part of the available funds is labelled as 'common budget' whereas the remaining part is 'individual budget'.

The basic idea of mental accounting as an explanation for non-standard behavior is established by Thaler (1985, 1990, 1999).[5] It has been applied to consumer choices[6], portfolio choice[7], development aid[8] and tax avoidance[9]. Abeler and Marklein (2017) report that participants in a field experiment change their consumption behavior depending on the way income is provided (in-kind vs. cash). Clingingsmith (2015) compares the amounts given in a dictator game when the endowment is earned versus when it is received as a windfall and finds that income from these sources is only partially fungible (see also Cherry 2001). Goerg et al. (2017) show that if income is provided by a party that is perceived as more similar to the recipient, senders give less in a dictator game. In contrast to these papers, we vary the type of account in which the endowment is provided. In order to mimick a situation in which a flypaper effect may occur, we have a common budget and individual accounts. By construction, individuals should be indifferent between the different accounts. Our main focus is to test whether money provided in the common budget is more likely to be spent on 'common purposes' (here: giving to the poor).[10]

For this purpose, we adopt the setting of a classical redistribution game for the experiment. Groups of three players are formed. Two of the players are 'rich' and have an individual endowment, one of them is 'poor' and has no endowment. Each player votes on the amount transferred to the poor player. In such a setting, it has been shown that, although any strategic incentive is

[5]The related theory of narrow bracketing is developed i.a. by Tversky and Kahneman (1981) and Rabin and Weizsäcker (2009).

[6]See, e.g., Heath and Soll (1996), Milkman and Beshears (2009), Hastings and Shapiro (2013), Abeler and Marklein (2017).

[7]See, e.g., Choi et al. (2009).

[8]Pack and Pack (1990), van de Walle and Mu (2007) and Van de Sijpe (2013) find that revenue from development aid is not fungible and therefore has the impact the donors intended. For contradicting evidence see Khilji and Zambelli (1991) and Pack and Pack (1993). Kooreman (2000), Edmonds (2002) and Schady and Rosero (2008) evaluate the effectiveness of child benefit by analysing whether sources of household income matters.

[9]See, e.g., Fochmann and Wolf (2016).

[10]A related approach is chosen by Fosgaard et al. (2014) who consider a public good game where participants can give to or take away from the public good. Although these two mechanisms are economically equivalent, the cooperation changes. The authors show that this is mainly due to changed beliefs about the other participants' behavior – an aspect which is of minor importance in our study.

absent, the average individual tends to vote for non-zero contributions to the poor player. For instance, Engel (2011) derives a giving ratio of 28.3% in a meta-study on dictator games. In ultimatum games the mean offer is about 40% of the total endowment (Tisserand 2014). As a novelty, we introduce a common budget which is distributed to the rich players at the end of each round (which makes it effectively the rich players' individual fund). The main question is whether voting behavior is affected by the relative sizes of the common budget and the individual budget. In the absence of mental accounting, only the total amount of money should be relevant. How the money is labelled should be completely irrelevant. The lab experiment allows us to provide identical circumstances for voting where only the relative sizes of the common budget and the individual budget is varied.

3 Experimental setup

The experiment took place at the University of Muenster in November 2015 and January 2016. 264 students (117 female and 147 male) from various disciplines (Business: 26.89%, Economics: 20.08%, Law: 14.77%, other: 38.26%) drawn from a pool of students signed up in the online recruitment system ORSEE (Greiner 2015) participated in 16 sessions. Each session was conducted with 15 or 18 participants each who had no opportunity to communicate during the experiment. Written instructions were handed out on paper and read aloud by the experimenter. Thereafter, participants had to do two control tasks (see appendices A.3 and A.4 for an English translation). Each session lasted approximately 80 minutes and finished with a short questionnaire. The questionnaire as well as the experiment were programmed in z-Tree (Fischbacher 2007). On average, participants earned 17.16 Euros.

Each session consists of 24 periods. Each period has five distinct stages.

Stage 1 (Group formation and role assignment) All participants are randomly and anonymously matched in groups of three players. Each player is randomly assigned a 'type'. Within each group, two players are assigned the type *Green*, the third one the type *Blue*. Types *Green* and *Blue* differ in endowments.

Stage 2 (Endowments) The two type *Green* players receive a random but identical endowment of lab currency ("points", 30 points equal 1€) on their *individual account*, the type *Blue* player receives an endowment of zero. Next to their *individual account*, the two *Green* types are endowed with a random amount in a *common account*. Randomization was realized as follows: We let the computer stochastically determine the size of the economy out of the interval $[30, 60]$. Then, the size of the *common account* is drawn from the interval $[0, 30]$. The two draws are independent and have an underlying uniform distribution. The difference between the size of the economy and the *common account* is then split in half and assigned the *Green* types' individual accounts. All group

members know each others' account balances as well as the common account balance.

Stage 3 (Transfer proposals, voting) At this stage, the group members vote on a transfer from the *Green* types to the *Blue* type player. All three players are asked to make a proposal how large the transfer is supposed to be. From the three proposals, the intermediate one is realized (which is equivalent to median voter rule). Only the realized transfer will, at stage 5, be revealed to the group members.

Stage 4 (Financing of transfer) The treatments differ in how the transfer is financed. In the INDIVIDUAL treatment, each of the type *Green* players pays half of the transfer out of her *individual account*. After that, the *common account* is dissolved and paid out to the type *Green* players' individual accounts. If the initial *individual account* is lower than half of the transfer, it turns negative; the deficit is later on compensated by the payment out of the *common account*. The maximum transfer cannot exceed the overall endowments by the type *Green* players. In the COMMON treatment, the transfer is paid out of the *common account*. Afterwards, the remainder of the *common account* is paid out to the *Green* types' *individual accounts*. If the *common account* turns negative due to a high transfer, the deficit is compensated out of the *individual accounts*.

Stage 5 (Payoffs) At the end of each period, the realized transfer and all group members' final account balances are revealed. Each player's period income (i.e. their final account balance) is stored. The aggregate income is paid out at the end of the experiment.

In both, the INDIVIDUAL and the COMMON treatment, the two type *Green* players receive half of all money in the economy after deducting the transfer to the type *Blue* player. If mental accounting is not relevant, both type *Green* players should realize that they pay half of the transfer out of their pockets. Then, the size of the *common account* should be completely irrelevant for the transfer proposals. However, in the presence of mental accounting, a large *common account* may be used to finance a large transfer since the *common account* is 'meant' to be used for group interests which is, here, redistribution. Then, all three players might be inclined to increase the transfer proposal if the *common account* is large.[11]

However, the literature suggests that individiduals sometimes anchor their decisions on some salient, though irrelevant number. Such an irrelevant number may be the common account. In order to rule out that this number drives the

[11] Loss aversion may also play a subtle role here. Suppose that a type Green player mentally distinguishes between both accounts and considers the individual account as her personal endowment. A transfer paid out of her individual account is then perceived as an actual loss, whereas a transfer paid out of the common account is perceived as a decrease in the gain the player obtains when the account is dissolved. A segregated valuation of payment streams, thus, induces the type Green player to propose differently in the COMMON treatment.

transfer proposals, we use the variation within treatments. Note that, in both treatments, there is a common account. The only difference is that the transfer is either taken out of the *individual account* (INDIVIDUAL treatment) or the *common account* (COMMON treatment). In terms of payoffs, this differentiation is meaningless; in terms of mental accounting, it may affect the transfer proposal.

In the first six periods, the common budget is endowed with zero points. We refer to this phase as the CONTROL treatment which allows us to collect information on the participants' propensity to give. In periods 7 to 15, participants are either in the INDIVIDUAL treatment or the COMMON treatment, in periods 16 to 24, the treatment is switched. We have thus intersubject and intrasubject variation.

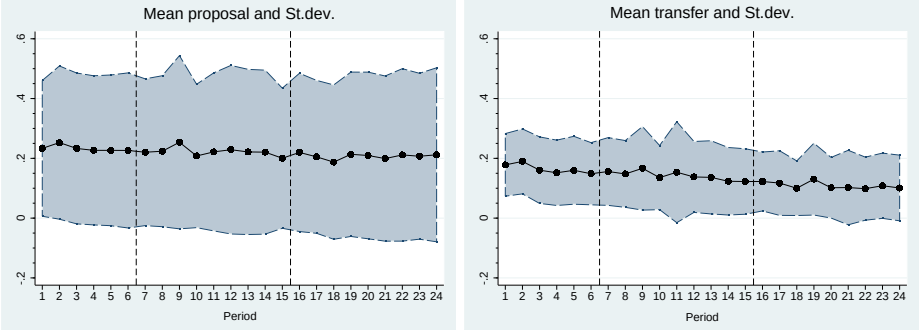
One concern with the experiment design is that participants might think that they cannot give more than the endowment in the common account. We therefore provided a control task which explicitly used contributions that exceed the size of the common account. In the Appendix, we provide evidence that there is mild bunching of proposals at the common account size, but is not of major importance. Moreover, the bunching occurs in both treatments and, thus, leaves our main identification strategy unaffected.

4 Results

4.1 Basic results

Fig. 1 shows the mean proposal ratio over periods (left hand side) and the mean realized transfer ratio (right hand side). In period 1, the average proposal is 23.3% of the economy size. There is a slight downward trend, but even in period 24, the players still propose on average 21.9% of the economy size to the poor player (the type *Blue* player). The realized transfer is somewhat smaller (since it is usually a type *Green* player who makes the median proposal) and the downward trend is more pronounced. However, transfers are on average still more than 10% of the economy in the last period, leaving the type *Green* player with a bit less than 45% of the economy.

Table 1 shows the proposal ratio, i.e. proposal as a fraction of the size of the economy, for the total sample and different subsamples. The total number of observations is 6336 and the average proposal ratio is 22% of the economy size. A quarter of participants have proposed zero to the type *Blue* player which is exclusively driven by type *Green* players' proposals and, interestingly, mostly by male participants. Five percent in total (14% of the type *Blue* players) have proposed the full economy size. Again, this kind of selfish proposing behavior is mostly found with male players. Around half of all observations (3024) have been made with a sequence of treatments where the COMMON treatment came first (periods 7-15) and the INDIVIDUAL treatment came last (periods 16-24). The other half (3312) had the opposite sequence. Voting behavior between sequences did not fundamentally differ.



(a) Mean proposal (b) Mean transfer

Figure 1: Mean proposals and mean transfers.

Table 1: Descriptive statistics

	N	Participants	Proposal ratio	St.dev.	Extreme proposals	
					zero	max
Total	6336	264	0.22	0.26	0.26	0.05
Female	2808	117	0.21	0.21	0.16	0.02
Male	3528	147	0.23	0.30	0.34	0.08
Green	4224	264	0.09	0.12	0.39	0
Blue	2112	264	0.47	0.29	0	0.14
COM-IND	3024	126	0.21	0.27	0.28	0.05
IND-COM	3312	138	0.23	0.26	0.24	0.05

Notes: Proposal ratio as share of proposed points of economy size.
Zero (max) proposal as share of proposals with zero (maximum) points of all proposals.

We now have a first look at the impact of a variation in the common account size. Fig. 2 shows the proposals (y-axis) for different common account sizes (x-axis), both measured as a fraction of the overall economy size. In the whole sample, the relative size of the common account is associated with larger amounts proposed to be given to the poor. A similar correlation is found for the realized transfers.

To rule out that this entirely due to anchoring on the size of the common account, we now turn to the two treatments, INDIVIDUAL and COMMON. Fig. 3 shows the same binned scatter plot for each treatment (controlled for individual specific demographics and periods). The different slopes suggest that the association between common account size and proposals is stronger in the COMMON treatment.

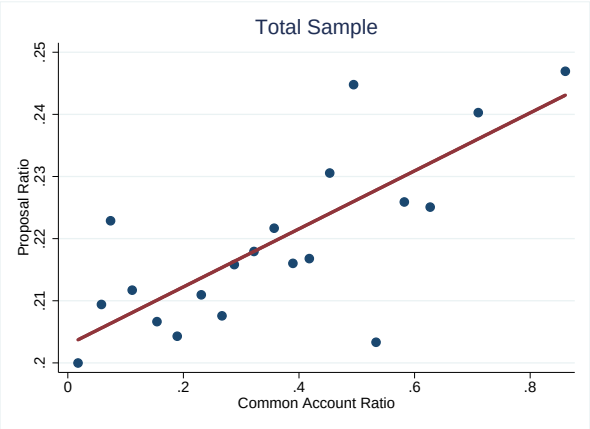


Figure 2: Proposals and common account size.

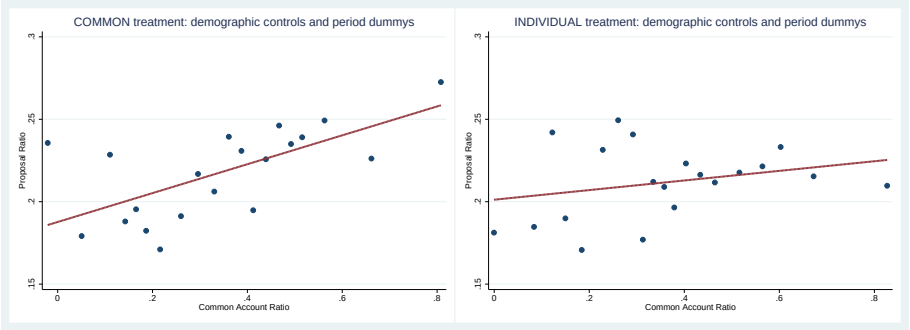


Figure 3: Binned scatter plot for each treatment.

Fig. 4 gives the estimated differences between mean proposals for common account sizes *below* and *above* median. Again, we control for periods. Whereas the difference is close to zero in the INDIVIDUAL treatment, it is significantly positive in the COMMON treatment. That is, only if the transfer to the poor is taken directly out of the common account, giving reacts to the common account size.

4.2 Analysis

To gain further insights, we now turn to regression analysis. Let i be the index of individuals, j the index of a group and t the time index. The proposal for a transfer to the *Blue* type is denoted by $Prop_{it}$. Since we assume that fairness is

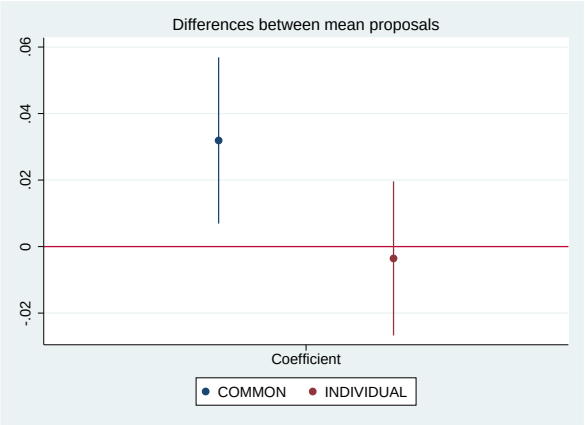


Figure 4: Differences between proposals for common account sizes below and above median.

a relative concept, i.e. others' endowments are considered fair relative to one's own endowment, we scale the proposal by the size of the economy, denoted by $Size_{jt}$, i.e. initial endowments of type *Green* players plus the common budget. Thus, the variable of interest is the proposal ratio, $\frac{Prop_{it}}{Size_{jt}}$.

In the first six periods, the common budget is fixed at zero. We use this phase to collect information on the participants' general willingness to give. Controlling for all observed and unobserved heterogeneity (by using fixed effects estimation) and using period dummies, the average proposal ratio is 25.8% on average. The realized proposal ratio is 17.9.

After period 6, the two type *Green* players have a common budget, denoted by $Common_{jt}$. As indicated above, the size of the common budget is determined independently from the size of the economy. We start by regressing the proposal ratio on the common budget ratio, allowing the size of the economy to have an impact:

$$\frac{Prop_{it}}{Size_{jt}} = a_0 + a_1 \frac{Common_{jt}}{Size_{jt}} + a_2 Size_{jt} + \mathbf{b}\mathbf{X}_i + \varepsilon$$

where $\mathbf{X}_i$ denotes the vector of player-specific socio-demographic variables.

Table 2 reports the regression results. It shows that the relative size of the common account has a significant impact on the relative size of the proposal. Starting with a simple OLS estimation in specification (1), an increase of the common account by 10 percentage points increases the proposal by 0.5 percentage points. Note that this is conditional on the size of the economy, i.e. a variation of the common account does not add to the potential consumption of the two type *Green* players.

– Table 2 here –

The fact that the relative size of the common budget has a significant impact is in line with the hypothesis that public funds are differently mentally accounted for than private funds. So far, however, we cannot rule out that this effect is due to simple anchoring. Players may take the common budget as some (arbitrary) reference at which their proposal is measured. To rule out anchoring as the driving force behind this, we now make use of the two treatments, COMMON and INDIVIDUAL. In both treatments, the common budget is a potential anchor, but only in the COMMON treatment, the transfer will be directly financed out of public funds, i.e. the common budget.

Let COMMON denote a dummy that equals 1 if the participant is in the COMMON treatment and 0 if not. Now, consider the following regression equation.

$$\begin{aligned}\frac{Prop_{it}}{Size_{jt}} &= a_0 + a_1\frac{Common_{jt}}{Size_{jt}} + a_2\frac{Common_{jt}}{Size_{jt}} \times COMMON + a_3 COMMON \\ &\quad + a_4 Size_{jt} + a_5 Size_{jt} \times COMMON + \mathbf{bX} + \varepsilon\end{aligned}$$

The results are reported in Table 3. The simple OLS estimation in column (1) shows that the sum of the coefficients $a_1 + a_2$ is larger than a_1 alone, but imprecisely measured. In the subsequent estimation, we add some obvious controls (2), period dummies (3), and all available controls (4), before eliminating all individual-specific time-invariant heterogeneity by the use of fixed effects (5) and period dummys (6). Including period dummies, the impact of common account ratio in the COMMON treatment is almost eight times as large as in the INDIVIDUAL treatment, and the difference is statistically significant on the 1 per cent level. Adding the two coefficients, this implies that an increase in the common account ratio by ten percentage points increases the proposal ratio by 0.9 percentage points.

– Table 3 here –

So far, we did not differentiate between the sequence of treatments. Table 4 reports the coefficient estimates of the common account ratio for different subsamples according to when the treatment took place. Independent in which sequence the treatments took place, a significant impact of the common account ratio on the proposal ratio only occurs in the COMMON treatment. The effect is, however, somewhat smaller, when the COMMON treatment comes after the INDIVIDUAL treatment.

– Table 4 here –

Table 5 measures the impact of the common account ratio on the proposal ratio across treatments. This mostly captures the results derived for the total sample with treatment interactions; it illustrates, though, the substantial difference between the treatments. The coefficient estimate on the common account ratio is five times as large in the COMMON treatment compared to the INDIVIDUAL treatment and it is significant on the 1 per cent level. The estimations

reported in Colums (3) and (4) reveal that the impact of the common account ratio in the COMMON treatment is clearly significant for both type *Green* and type *Blue* players. However, the impact is triple the size for the type *Blue* players. Column (5) shows the estimation results including the lag variable of the transfer economy size ratio. Past transfers may be an indicator of some kind of social norm that influences the participants' redistribution behavior. In fact, an increase of the lagged transfer ratio by 10 percentage points increases the proposal by 1.2 percentage points. The estimate of the interaction term stays robust, though.

– Table 5 here –

Additionally, we split the sample in those with an Abitur grade in math above the median and below. It turns out that the mental accounting effect only occurs with those with a below median Abitur grade in math. This finding is in line with other studies that show that some behavioral anomalies are related to skills (see e.g. Abeler and Marklein 2017). Then, we split the sample in female and male participants. Here, the results suggest that there is no difference between men and women with respect to the impact of the treatment.

– Table 6 here –

Table 1 shows, that one third of the proposals are either extremely low or extremely high, i.e. the participants propose zero points as a *Green* player and demand the total amount of points as a *Blue* player. These kind of very selfish participants show no treatment effect as they do not respond to a change in the common account balance. To distinguish different types of participants with respect to their proposal behavior, we split the sample into four parts. We assign types according to the mean proposals as *Green* players in the first six rounds, in which the common account balance was zero. We take this mean proposals to form four groups: very selfish (0.007), mildly selfish (0.039), mildly generous (0.12) and very generous players (0.17).[12] Table 7 shows, that the COMMON treatment does not have an impact on the extremely selfish and extremely generous participants. The treatment effect is mainly driven by the other half of the participants, who exhibit moderate redistribution preferences.

– Table 7 here –

5 Extensions

To get a deeper understanding of the main drivers of mental accounting in our experiment we slightly change the design. The first modification concerns the

[12]The numbers in brackets show the mean proposal ratio in the periods 7 to 24, when the participant was a *Green* player.

name of the accounts in such a way that we call the '*individual account*' '*account 1*' and the '*common account*' '*account 2*'. 51 students (26 female and 25 male) participated in 4 sessions. The results in Table 8 show that mental accounting is not driven by the names of the accounts. Therefore, we conclude that this behavior is caused by the transfer rules and the assignment of the accounts. Surprisingly, the fixed effects regression gives an coefficient for the treatment effect more than twice as high as in the basic design whereas the mean proposal ratio (0.20) corresponds almost exactly to the mean in the basic design (0.22).

– Table 8 here –

The second modification concerns the mechanism that determines the transfer to the type *Blue* players. In the basic design, the amount is chosen by the median voter. Literature provides some arguments that the voting mechanism may influence the results due to strategic behavior (see e.g. Strulovici 2010). For example participants might hide behind the median voter and therefore take extreme positions, i.e. they propose either zero or the whole economy depending on their type.[13] To check, whether the voting rule has an impact, we replace the median voter procedure by the random dictator procedure. Note that this design is identical to the basic design in all respects except for the determination of the transfer. 69 students (31 female and 38 male) participated in 4 sessions. The results in Table 9 show that our main findings are not affected by the voting mechanism, which is in line with Höchtl et al. (2012). While mental accounting can be identified in all designs the random dictator rule seems to lead to more extreme proposals (basic design: 30.1%; random dictator design: 53.3%). One possible explanation may be, that if the decisive individual is chosen randomly from the three group members, it is more likely that an extreme proposal is realized as a transfer.[14] Therefore, in contrast to the median voter rule the revelation of very selfish preferences is as likely as the revelation of more moderate preferences. As a consequence, participants with a moderate preference for redistribution may respond to the new information and adjust their behavior.[15]

– Table 9 here –

6 Discussion

Our experiment shows that the transfer to the poor player increases in the size of the public budget. We interpret this as public funds being mentally accounted

[13] Shayo and Harel (2012) and Kamenica and Brad (2014) find factors influencing the voters decision that go beyond someone's material motivation especially when the probability of being pivotal is very small.

[14] This is reflected by the share of extreme transfers of the total number of transfers (basic design: 18.0%; random dictator design: 55.6%).

[15] For a theoretical model of learning about other people's preferences by observing their voting behavior see Aytimur et al. (2014).

for in a different way than private funds. Mental accounting may thus explain why voters urge the government to take over new tasks if revenue is available, but are reluctant to agree to tax increases when revenue is not available.

It has to be conceded that the highly artificial situation in the lab somewhat restricts the scope for interpretation of real worlds spending decisions. However, if mental accounting can be observed in this transparent, often repeated situation, it may at least establish the argument that this kind of non-standard behavior may affect real-world decisions.

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Appendix

A.1 Tables

Table 2: Effect of Common Account Ratio on Proposal Ratio (total sample)

	Dependent variable: *Proposal Ratio*$_{it}$					
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) FE	(6) FE
Common Account Ratio	0.0499***	0.0483**	0.0455**	0.0436**	0.0516***	0.0488**
	(0.0192)	(0.0193)	(0.0198)	(0.0198)	(0.0144)	(0.0170)
Economy Size	-0.0011**	-0.0011**	-0.0011**	-0.0011**	-0.0013***	-0.0012***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0003)	(0.0003)
Abitur Grade		0.0001	0.0001	0.0002*		
		(0.0001)	(0.0001)	(0.0001)		
Female		-0.0240**	-0.0241**	-0.0164		
		(0.0116)	(0.0117)	(0.0120)		
Economics		0.0030	0.0030	0.0016		
		(0.0118)	(0.0118)	(0.0118)		
Constant	0.248***	0.243***	0.237***	0.266***	0.253***	0.248***
	(0.0243)	(0.0330)	(0.0360)	(0.0490)	(0.0157)	(0.0229)
Period Dummys	NO	NO	YES	YES	NO	YES
Controls	NO	NO	NO	YES	NO	NO
Observations	4752	4752	4752	4752	4752	4752
Cluster/Groups	264/-	264/-	264/-	264/-	16/264	16/264

Notes: Standard errors in parentheses. In OLS estimates, standard errors are clustered by subject id, in FE estimates by sessions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of Common Account Ratio across treatments (total sample)

	Dependent variable: $Proposal\ Ratio_{it}$					
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) FE	(6) FE
COMMON Dummy	0.0114	0.0151	0.0004	-0.0001	-0.0292	-0.0469*
	(0.0506)	(0.0505)	(0.0515)	(0.0514)	(0.0230)	(0.0254)
Common Account Ratio	0.0272	0.0241	0.0191	0.0185	0.0172	0.0109
	(0.0264)	(0.0261)	(0.0263)	(0.0264)	(0.0164)	(0.0182)
Common Account Ratio	0.0463	0.0448	0.05002	0.0520	0.0724***	0.0800***
× COMMON Dummy	(0.0378)	(0.0377)	(0.0382)	(0.0383)	(0.0145)	(0.0154)
Economy Size	-0.0009	-0.0009	-0.0010	-0.0010	-0.0013***	-0.0014***
	(0.0006)	(0.0006)	(0.0007)	(0.0006)	(0.0004)	(0.0005)
Economy Size	-0.0004	-0.0005	-0.0002	-0.0002	0.0003	0.0006
× COMMON Dummy	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0005)	(0.0005)
Abitur Grade		0.0001	0.0002	0.0002*		
		(0.0001)	(0.0001)	(0.0001)		
Subj. Math Ability		-0.0192***	-0.1920***	-0.0169***		
		(0.0062)	(0.0059)	(0.0063)		
Female		-0.0178	-0.0179	-0.0163		
		(0.0114)	(0.0114)	(0.0120)		
Economics		0.0003	0.0004	0.0014		
		(0.0115)	(0.0115)	(0.0118)		
Constant	0.242***	0.271***	0.271***	0.263***	0.266***	0.266***
	(0.0341)	(0.0419)	(0.0450)	(0.0526)	(0.0197)	(0.0254)
Period dummys	NO	NO	YES	YES	NO	YES
Controls	NO	NO	NO	YES	NO	NO
Observations	4752	4752	4752	4752	4752	4752
Cluster/Groups	264/-	264/-	264/-	264/-	16/264	16/264

Notes: Standard errors in parentheses. COMMON dummy takes the value of 1 in the COMMON treatment and 0 otherwise. Ability is a self-evaluation on math skill measured by a fivepoint Likert scale.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Comparison of sequences (FE)

Dependent variable: *Proposal Ratio*$_{it}$			
		(1) COMMON treatment	(2) INDIVIDUAL treatment
Common Account Ratio	Period 07 - 15	0.1116***	0.0199
		(0.0246)	(0.0253)
	Period 16 - 24	0.0888***	0.0253
		(0.0195)	(0.0197)

Notes: Standard errors in parentheses. Estimates include period dummies.
* p < 0.10, ** p < 0.05, *** p < 0.01.

Table 5: Comparison of subgroups and lag variables (FE)

	Dependent variable: *Proposal Ratio*$_{it}$				
	(1) INDIVIDUAL	(2) COMMON	(3) GREEN	(4) BLUE	(5) LAG
Common Account Ratio	0.0231	0.1000***	0.0097	0.0483	0.0140
	(0.0151)	(0.0148)	(0.0119)	(0.0452)	(0.0180)
Economy Size	-0.0016***	-0.0008	-0.0003	-0.0024***	-0.0014***
	(0.0004)	(0.0003)	(0.0004)	(0.0007)	(0.0005)
COMMON Dummy			-0.0515**	0.0491	-0.0437*
			(0.0210)	(0.0363)	(0.0234)
Common Account Ratio			0.0602***	0.1464***	0.0747***
× COMMON Dummy			(0.0166)	(0.0483)	(0.0005)
Economy Size			0.0006	-0.0018**	0.0006
× COMMON Dummy			(0.0005)	(0.0008)	(0.0005)
L.Transfer Ratio					0.1174**
					(0.0472)
Constant	0.291***	0.221***	0.0682***	0.599***	0.251***
	(0.0244)	(0.0213)	(0.0173)	(0.0408)	(0.0261)
Observations	2376	2376	3168	1584	4752
Cluster/Groups	16/264	16/264	16/264	16/264	16/264

Notes: Standard errors in parentheses. Estimates include period dummies.
* p < 0.10, ** p < 0.05, *** p < 0.01.

Table 6: Comparison of subgroups (cont.) (FE)

	Dependent variable: $Proposal\ Ratio_{it}$					
	(1) Math skills (> median)	(2) Math skills (< median)	(3) Female	(4) Male	(5) Economics	(6) Non-Economics
Common Account Ratio	0.0226	0.0005	0.0635**	-0.0284	-0.0222	0.0402
	(0.0316)	(0.0310)	(0.0231)	(0.0242)	(0.0259)	(0.0294)
Economy Size	-0.0016	-0.0013**	-0.0006	-0.0021**	-0.0024**	-0.0004
	(0.0010)	(0.0006)	(0.0005)	(0.0008)	(0.0008)	(0.0009)
COMMON Dummy	-0.0229	-0.0738	-0.0373	-0.0458	-0.0848	-0.0136
	(0.0420)	(0.0493)	(0.0314)	(0.0600)	(0.0503)	(0.0489)
Common Account Ratio	0.0044	0.1512***	0.0735*	0.0771**	0.1038***	0.0621**
× COMMON Dummy	(0.0385)	(0.0312)	(0.0368)	(0.0303)	(0.0341)	(0.0281)
Economy Size	0.0008	0.0005	0.0002	0.0008	0.0010	-0.0002
× COMMON Dummy	(0.0010)	(0.0010)	(0.0007)	(0.00011)	(0.0008)	(0.0010)
Constant	0.275***	0.259***	0.196***	0.320***	0.318***	0.218***
	(0.0435)	(0.0380)	(0.0276)	(0.0428)	(0.0440)	(0.0388)
Observations	2376	2376	2106	2646	2232	2520
Cluster/Groups	16/132	16/132	16/117	16/147	16/124	16/140

Notes: Standard errors in parentheses. Estimates include period dummies. Math skills measured by Abitur grade in math. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 7: Redistribution types

	Dependent variable: *Proposal Ratio*$_{it}$			
	(1) very selfish FE	(2) mildly selfish FE	(3) mildly generous FE	(4) very generous FE
COMMON Dummy	-0.0123	-0.1193	-0.0354	-0.0085
	(0.1230)	(0.0709)	(0.0506)	(0.0596)
Common Account Ratio	0.0451	-0.0440	-0.0292	0.0681**
	(0.0505)	(0.0313)	(0.0427)	(0.0277)
Common Account Ratio	-0.0423	0.1608***	0.1383**	0.0520
× COMMON Dummy	(0.0512)	(0.0472)	(0.0495)	(0.0608)
Economy Size	-0.0036**	-0.0023***	-0.0010	0.0005
	(0.0014)	(0.0007)	(0.0009)	(0.0008)
Economy Size	0.0013	0.0011	0.0000	0.0001
× COMMON Dummy	(0.0024)	(0.0013)	(0.0010)	(0.0011)
Constant	0.361***	0.279***	0.276***	0.169***
	(0.0669)	(0.0348)	(0.0525)	(0.0397)
Share of Females in %	18.2	50.0	54.5	54.5
Share of Economics in %	66.7	47.0	37.9	36.4
Observations	1188	1188	1188	1188
Cluster/Groups	15/66	16/66	14/66	15/66

Notes: Standard errors in parentheses. Estimates include period dummies. * p<0.10, ** p<0.05, *** p<0.01.

Table 8: Effect of Common Account Ratio in Framing design

	(1) INDIVIDUAL	(2) COMMON	(3) OLS	(4) FE
Common Account Ratio	0.0208	0.181**	-0.0493	-0.0003
	(0.0734)	(0.0354)	(0.0863)	(0.0741)
Economy Size	-0.0005	-0.0012	0.0003	-0.0003
	(0.0008)	(0.0006)	(0.0019)	(0.0009)
COMMON Dummy			-0.0488	-0.0801
			(0.148)	(0.0684)
Common Account Ratio			0.229*	0.173**
× COMMON Dummy			(0.117)	(0.0512)
Economy Size			-0.0019	-0.0007
× COMMON Dummy			(0.0022)	(0.0011)
Constant	0.255**	0.172***	0.237**	0.253**
	(0.0648)	(0.0294)	(0.114)	(0.0719)
Observations	459	459	918	918

Standard errors in parentheses. Estimates include period dummies.
Models in columns 1 and 2 are estimated with fixed effects regression.
* p<0.10, ** p<0.05, *** p<0.01

Table 9: Effect of Common Account Ratio in Random design

	(1) INDIVIDUAL	(2) COMMON	(3) OLS	(4) FE
Common Account Ratio	-0.0437	0.0833*	-0.0436	-0.0330
	(0.0237)	(0.0305)	(0.0876)	(0.0260)
Economy Size	0.0004	-0.0002	0.0004	0.0005
	(0.0005)	(0.0004)	(0.0021)	(0.0006)
COMMON Dummy			-0.0291	-0.0150
			(0.157)	(0.0596)
Common Account Ratio			0.131	0.111*
× COMMON Dummy			(0.130)	(0.0457)
Economy Size			-0.0005	-0.0006
× COMMON Dummy			(0.0031)	(0.0011)
Constant	0.304***	0.281***	0.305***	0.298***
	(0.0260)	(0.0162)	(0.112)	(0.0338)
Observations	621	621	1242	1242

Standard errors in parentheses. Models in columns 1 and 2 are estimated with fixed effects regression.
* p<0.10, ** p<0.05, *** p<0.01

A.2 Bunching

Fig. 5 plots the frequency of proposals measured as a fraction of the common account. 23 per cent of all proposals are larger than the size of the common account (i.e. the ratio is equal to or below unity). This fraction does not vary substantially across treatments. In the COMMON treatment the share of proposals exceeding the common account is 0.22, in the INDIVIDUAL treatment it is 0.23. Therefore, we are confident that our results are not driven by the misunderstanding that proposals must not exceed the common account.

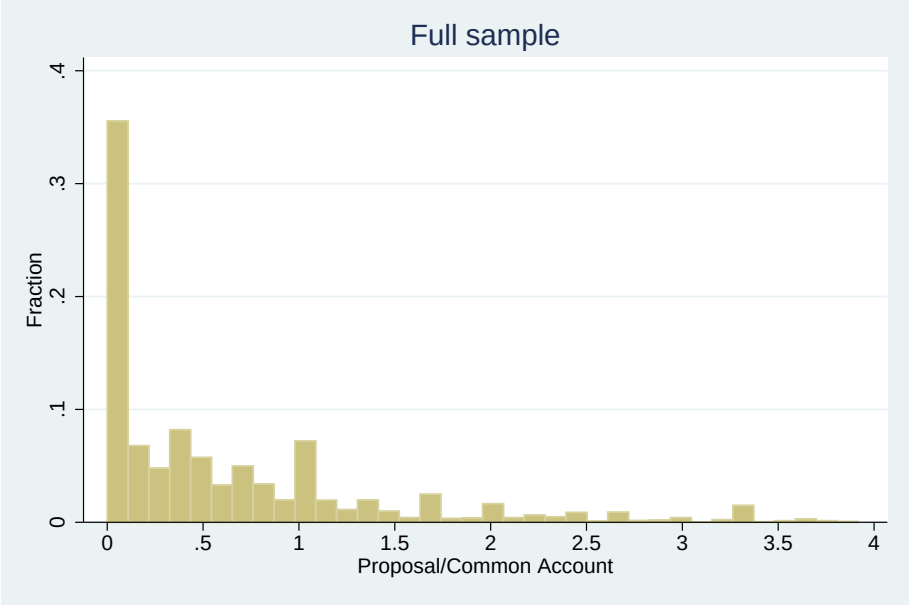

Figure 5: Full Sample.

A.3 Instructions

General instructions

You are about to participate in an economic experiment. This means that in the following you will make decisions that will lead to money payments; money that you and others will receive. Similarly, your final payoff depends also on decisions made by other participants. Please read our instructions carefully. At the end of the experiment you will get paid anonymously.

It is not allowed to communicate during the experiment. If you have any questions, please ask the experimenters. We will help you. Violations of this rule will exclude you from the experiment.

The experiment consists of 24 periods. At the beginning of each period you will be randomly matched in groups of three. It is possible that you will be matched with the same persons more than once. Since neither you nor your partner receive any personal information, you remain anonymous.

There are two types of players: GREEN and BLUE. Each group consists of two GREEN players and one BLUE player. At the beginning of each period your type will be determined randomly. Therefore, it is possible that you are a GREEN or a BLUE player several rounds in a row.

During the experiment, the currency is points. Therefore, all information on income will be in points. At the end of the experiment, we will convert all points on your individual account to Euro. 30 points equal 1 Euro.

Procedure

Every player has an individual account. Moreover, there is a common account for GREEN players (common account GREEN). At the beginning of each period every GREEN player is endowed with a random number of points. Additionally, the GREEN players obtain a random number on the common account. The BLUE players do not get any amount to their individual accounts.

[*If COMMON treatment from period 7 to 15*]
The group decides on the amount the BLUE player gets. This amount is financed by the common account GREEN. This is the unique opportunity for player BLUE to obtain points. For this purpose every player makes a proposal. The middle proposal of the three proposals is implemented. That means that the highest and the lowest proposals are ignored.

In the first 6 periods there does **not** exist a common account GREEN, that means that the amount to BLUE is financed by the individual accounts of GREEN in equal shares. From period 7 an endowment of the common account GREEN is determined. The rules of the game will be adjusted slightly after period 15.

[*If INDIVIDUAL treatment from period 7 to 15*]
The group decides on the amount the BLUE player gets. This amount is financed by the GREEN players' individual accounts in equal shares. This is the unique opportunity for player BLUE to obtain points. For this purpose every

player makes a proposal. The middle proposal of the three proposals is implemented. That means that the highest and the lowest proposals are ignored. Thereafter, the total amount on the common account GREEN is split among the GREEN players.

In the first 6 periods there does **not** exist a common account GREEN. From period 7 on, an endowment of the common account GREEN is determined. The rules of the game will be adjusted slightly after period 15.

Procedure of a period

To organize the procedure of a period clearly every period is divided into 6 stages.

Stage 1 It is randomly determined whether you are GREEN or BLUE.

Stage 2 (starting from period 7) GREEN players obtain a random amount to their common account GREEN.

Stage 3 GREEN players obtain a random amount to their individual account. The BLUE player does not obtain an amount, which means that there are 0 points on her account.

Stage 4 The group decides how many points are transferred to the BLUE player. For this purpose all players make anonymous proposals. The proposals are made anonymously. The proposal representing the middle amount of points is implemented which means that the highest and the lowest proposal remain unconsidered.

[*If Common treatment from period 7 to 15*]
Stage 5 The transfer to BLUE is financed by the common account GREEN. If there is a positive amount on the common account GREEN after the transfer, this amount will be transferred to the individual accounts of the GREEN players in equal shares. If there is a negative amount on the common account of GREEN after the transfer, the account will be cleared by transfers from the individual accounts of the GREEN players.

[*If INDIVIDUAL treatment from period 7 to 15*]
Stage 5 The transfer to BLUE is financed by the individual accounts of the GREEN players. Each GREEN player provides half of the transfer to BLUE. The common account GREEN is split among the GREEN players in equal shares.

Stage 6 On the last stage the account balances of all group members and the individual period income is shown.

If Common treatment from period 7 to 15

Example Calculation 1

Account balances

- Common account GREEN: 20
- GREEN I: 15
- GREEN II: 15
- BLUE: 0

Proposals: GREEN I: ~~10~~, GREEN II: **14**, BLUE: ~~15~~

New account balances

- Common account GREEN: 6
- GREEN I: 15
- GREEN II: 15
- BLUE: 14

Closure of common account GREEN (each GREEN player obtains 3)

Final account balances (period income)

- Common account GREEN: 0
- GREEN I: 18
- GREEN II: 18
- BLUE: 14

Example Calculation 2

Account balances

- Common account GREEN: 10
- GREEN I: 5
- GREEN II: 5
- BLUE: 0

Proposals: GREEN I: **12**, GREEN II: ~~5~~, BLUE: ~~20~~

New account balances

- Common account GREEN: -2
- GREEN I: 5
- GREEN II: 5
- BLUE: 12

Clearing of common account GREEN (each GREEN player provides 1)

Final account balances (period income)

- Common account GREEN: 0
- GREEN I: 4
- GREEN II: 4
- BLUE: 12

If INDIVIDUAL treatment from period 7 to 15

Example Calculation 1

Account balances

- Common account GREEN: 20
- GREEN I: 15
- GREEN II: 15
- BLUE: 0

Proposals: GREEN I: ~~10~~, GREEN II: **14**, BLUE: ~~15~~

New account balances

- Common account GREEN: 20
- GREEN I: 8
- GREEN II: 8
- BLUE: 14

Closure of common account GREEN (each GREEN player obtains 10)

Final account balances (period income)

- Common account GREEN: 0
- GREEN I: 18
- GREEN II: 18
- BLUE: 14

Example Calculation 2

Account balances

- Common account GREEN: 10
- GREEN I: 5
- GREEN II: 5
- BLUE: 0

Proposals: GREEN I: **12**, GREEN II: ~~5~~, BLUE: ~~20~~

New account balances

- Common account GREEN: 10
- GREEN I: -1
- GREEN II: -1
- BLUE: 12

Clearing of common account GREEN (each GREEN player provides 5)

Final account balances (period income)

- Common account of GREEN: 0
- GREEN I: 4
- GREEN II: 4
- BLUE: 12

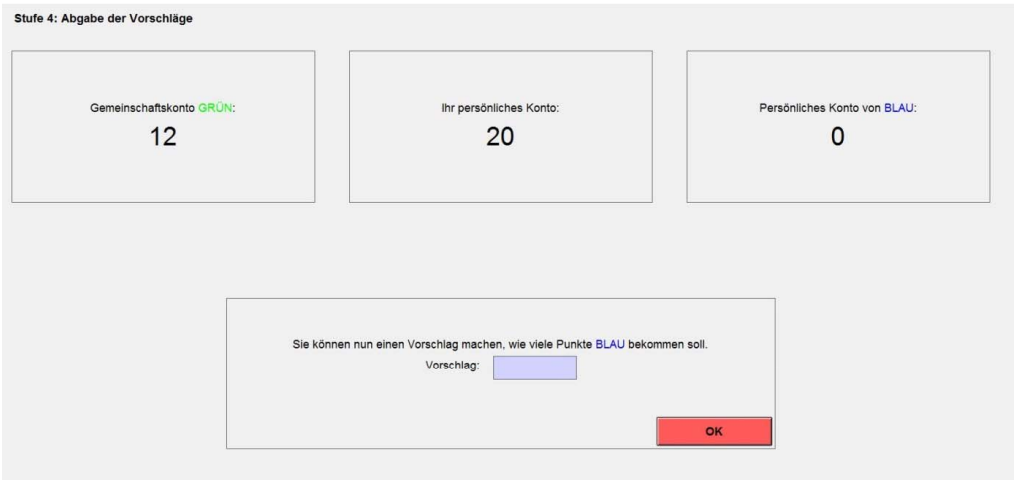

Figure 6: Screenshot (stage 4).

A.4 Control tasks

These control tasks were handed out to the participants to check whether they understand the transfer's funding procedure.

Account balances

- Common account GREEN: 30
- GREEN I: 12
- GREEN II: 12
- BLUE: 0

Proposals: GREEN I: 16, GREEN II: 20, BLUE: 8

New account balances

- Common account GREEN:
- GREEN I:
- GREEN II:
- BLUE:

Closure of common account GREEN

Final account balances (period income)

- Common account GREEN: 0
- GREEN I:
- GREEN II:
- BLUE:

Control task 2

Account balances

- Common account GREEN: 14
- GREEN I: 6
- GREEN II: 6
- BLUE: 0

Proposals: GREEN I: 21, GREEN II: 5, BLUE: 18

New account balances

- Common account GREEN:
- GREEN I:
- GREEN II:
- BLUE:

Clearing of common account GREEN

Final account balances (period income)

- Common account GREEN: 0
- GREEN I:
- GREEN II:
- BLUE: