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ABSTRACT

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Fungibility of money is a central principle in economics. It implies that any unit of money is substitutable for another and that the composition of income is irrelevant for consumption. We find in a field experiment that even in a simple, incentivized setup many subjects do not treat money as fungible. When a label is attached to a part of their budget, subjects change consumption according to the suggestion of the label. A controlled laboratory experiment confirms this result and further shows that subjects with lower mathematical abilities are more likely to violate fungibility. The findings lend support to behavioral models such as narrow bracketing or mental accounting. One implication of our results is that in-kind benefits distort consumption more than usually assumed.

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1 Introduction

Fungibility of money is a central principle in economics. It implies that any unit of money is substitutable for another. In the analysis of consumer choice, for example, fungibility prescribes that consumption decisions are based exclusively on the consumer's total wealth—its composition is irrelevant (Modigliani & Brumberg 1954). Fungibility is assumed throughout most of economic theory. Some empirical findings, however, cast doubt on the generality of the concept. Odean (1998), for example, analyzes stock market behavior of individual investors and finds that investors sell winning stocks too soon and keep losing stocks too long. He explains this finding by assuming that investors evaluate each stock holding separately (i.e., treat them as non-fungible) and are loss averse in each stock holding with respect to the buying price. Other examples come from the fields of asset pricing (Benartzi & Thaler 1995, Barberis et al. 2001), stock market participation (Barberis et al. 2006), stock trading (Barberis & Huang 2001), and life-cycle saving (Shefrin & Thaler 1988). Despite the fundamental importance of fungibility, surprisingly little is known about the degree to which individual decision-making is in line with the notion of fungibility.

In this paper, we investigate whether individuals treat different income sources as fungible using a combination of a natural field experiment and a controlled laboratory experiment. Both studies have the same general design: the consumer has a cash budget and an additional lump-sum subsidy of amount S at his disposal. In the *Cash treatment*, the subsidy S is given in cash. In the *Label treatment*, the subsidy is given as in-kind benefit, i.e., the subsidy has to be spent on the subsidized good. The crucial feature of our design is that the optimal consumption of the subsidized good exceeds the amount of the subsidy. By shifting the remaining budget, the consumer can reach the same first-best consumption level in both treatments, i.e. the subsidy is inframarginal and not distortionary. Therefore, treatments merely differ in the label attached to the subsidy; rational consumers should not be influenced by whether the subsidy is given as cash or in kind. Standard theory thus predicts consumption to be the same in both treatments. In contrast, a consumer who does not treat different income sources as fungible will spend the in-kind subsidy fully on the subsidized good and thus consume too much of this good. If consumption of the subsidized good is higher in the Label treatment, we can conclude that behavior is not in line with the principle of fungibility.

The field experiment was conducted in a wine restaurant. Guests received either a voucher for the whole bill (Cash treatment) or a voucher of the same amount that had to be used for beverage consumption (Label treatment). The value of the

voucher was lower than the usual beverage consumption of almost all guests. In a restaurant, guests consume at least a minimal amount of two distinct goods (eating and drinking). Thus, an in-kind voucher of a lower amount than this minimal amount does not distort the optimal decision. The field experiment allows us to collect natural decision data in a controlled way. In the terminology of Harrison & List (2004) the experiment is a "natural field experiment": participants acted in a natural, incentivized, and well-known environment. Additionally, they were not aware that they participated in an experiment and therefore did not feel observed. They could not self-select into treatments as these were exogenously assigned. Participants could not even self-select into the experiment in general since vouchers were not advertised and came as a surprise to participants after they had entered the restaurant.[2]

The laboratory experiment has the same general design as the field experiment and offers an even more controlled and well-defined setup. Subjects could consume two goods and had at their disposal a cash budget and either a cash subsidy (Cash treatment) or an in-kind subsidy (Label treatment).[3] We induced a standard microeconomic utility function by specifying monetary payoffs for the possible consumption bundles. Importantly, the parameters were chosen such that the in-kind subsidy is inframarginal: the amount of the subsidy is lower than the amount spent on the subsidized good in the (first-best) optimum for every subject. This is a major advantage compared to field data where the exact utility function cannot be known and the individual budget varies. Since we know the optimal decision in the laboratory, we can also compare actual decisions to the optimum and calculate an individual error. Additionally, it is possible to collect further information about subjects and to analyze the channels of a potential treatment effect. Field and laboratory experiment are thus methodological complements.

The results of both experiments show that fungibility is violated in the settings under investigation. In the field experiment, consumption of the subsidized good (beverages) is considerably higher in the Label treatment than in the Cash treatment. Guests in the Label treatment spend 3.90 euro or 25% more on the subsidized good than guests in the Cash treatment. This effect is also large compared to the

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[2]Self-selection because of word-of-mouth (i.e., early participants telling their neighbors) is very unlikely: we find that behavior is not different after the experiment compared to before. Since guests did not know when the experiment ended, a self-selection induced effect should show up also after the experiment.

[3]To have an additional intra-person measure of fungibility, subjects decided twice in the laboratory experiment. In a first stage, they allocated only the cash budget without the subsidy. Then the main treatment stage followed.

value of the voucher of 8 euro. The treatment difference is due to the choice of more expensive beverages: the average price per liter of consumed beverages is 3.52 euro higher in the Label treatment compared to the Cash treatment. In contrast, the treatment difference in consumed beverage *volume* is negligible. The total consumption (including meals) is not affected by the type of subsidy. But if we compare guests in the Cash and in the Label treatment with guests who did not receive either voucher, we find that spending is higher by 3.93 euro per person when a voucher is given.

The laboratory experiment confirms and extends the results of the field experiment. While average consumption of both goods is close to optimal in the Cash treatment, consumption of the subsidized good is significantly higher in the Label treatment. Subjects in the Label treatment consume on average 15% more of the subsidized good. Compared to a baseline stage without subsidy, subjects in the Label treatment spend 57% of the subsidy on the subsidized good compared to only 28% in the Cash treatment. One in five subjects in the Label treatment even spends every additional money unit on the subsidized good, while in the Cash treatment only one in fifty subjects does so. As we are able to collect more data on subjects in the lab than in the field experiment, we can analyze the underlying reasons for the treatment effect. We find a strong impact of subjects' mathematical abilities: subjects with higher mathematical skills (measured by their high school math grade) act consistently with standard economic theory. By contrast, in the group of subjects with lower mathematical skills, the treatment difference is large and significant. This supports the view that the violation of fungibility occurs for cognitive reasons and relates our study to recent work by Frederick (2005), Benjamin et al. (2006), and Casari et al. (2007). These studies show that people with higher mathematical and cognitive skills are more likely to behave in line with standard economic theory, whereas people with lower cognitive skills tend to act in accordance with theories of boundedly rational behavior.

Taken together, our results show that consumers do not always treat money as fungible. This has implications for several areas of economic research. First, our findings lend support to field studies that explain behavior of stock market investors by assuming that investors are loss averse in each portfolio position (e.g., Odean 1998, Benartzi & Thaler 1995). These studies assume that investors evaluate each component of their portfolio (e.g., stock vs. bond holdings) separately. We provide a direct test of this assumption. Second, our results are important for theories of life-cycle saving which usually rely on the assumption of fungibility. Since we find that even in a very simple setup fungibility does not hold for all

subjects, our evidence supports savings models in which the assumption of fungibility is relaxed (see, e.g., Shefrin & Thaler 1988, Barberis & Huang 2001). Furthermore, the specific design of our study allows us to give a rationale for the observed behavior of benefits recipients. Assuming a lack of fungibility could explain why housing benefits have such a strong effect on market rents as shown by Susin (2002), Gibbons & Manning (2006), and Fack (2006). Taking our results at face value, tenants receiving inframarginal housing benefits should be willing to pay a higher rent for a given apartment compared to tenants who get the same amount as cash grant. If landlords are aware of this behavior, they can exploit the existence of housing benefits to increase the rent. We discuss this issue in more detail in Section 5.

But why should people treat money as non-fungible? Tversky & Kahneman (1981) suggest that decision makers often do not decide globally but rather evaluate parts of a decision separately. This phenomenon has been called "narrow framing" (Kahneman & Lovallo 1993) or "narrow bracketing" (Read et al. 1999). For the allocation of a budget coming from different sources, making separate decisions implies a violation of fungibility. Since assessing the decisions separately is cognitively less demanding, our finding that subjects with lower mathematical skills are more likely to violate fungibility also points to narrow bracketing as a potential explanation for the treatment effect. More directly related to the consumption setup we analyze is the concept of "mental accounting" (Thaler 1980, 1985, 1999). Mental accounting proposes that consumers use a set of heuristics to deal with their day-to-day financial decisions. Mental accounting assumes that consumers have mental budgets for different expenditure categories or for different investment categories, thereby constraining the fungibility of money. In this framework, a label can influence consumption choice if it determines to which mental budget the consumer assigns the benefit payment.

Most empirical studies testing fungibility rely on non-incentivized surveys (e.g., Heath & Soll 1996, O'Curry 1997, Prelec & Loewenstein 1998, White 2006). Only few papers investigate fungibility in incentivized laboratory experiments or with field data. Levin (1998) finds in a large household survey that the marginal propensity to consume out of current income is higher than out of wealth; he can exclude liquidity constraints as explanation. Milkman et al. (2007) show that customers of an online grocer spend more when they redeem a $10-off coupon. $10 is negligibly small compared to life-time wealth and should not alter spending behavior if customers treat wealth and coupon as fungible. Arkes et al. (1994) find that people spend more when they just received a surprise gift compared to when they expected to get the gift (see also Epley et al. 2006). In these three studies, however, the treatment effect

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could potentially be influenced by a change in mood because of receiving a gift. In our study, this cannot have an impact. Finally, Gneezy & Potters (1997), Thaler et al. (1997), and Rabin & Weizsäcker (2007) show in experiments that subjects evaluate subsequent gambles separately, i.e., treat them as non-fungible. There are, however, also a couple of studies finding that subjects do treat different income sources as fungible: for example, in an experiment by Maciejovsky et al. (2001), subjects do not treat income differently that they receive from selling experimental assets or from the assets' dividends; and Moffitt (1989), studying the food stamps program in Puerto Rico, finds no difference in consumption between households who receive inframarginal in-kind benefits or cash grants.

The remainder of the paper is organized as follows: The design of both experiments is described in Section 2. Section 3 reports results of the field experiment. Section 4 presents results of the laboratory experiment and analyzes potential channels of the effect. Section 5 discusses an application of our results to benefit payments and concludes.

2 Experimental Design

2.1 General Setup

The goal of this paper is to experimentally test whether individual behavior is in line with fungibility. We examine this question in a simple two-goods consumption case where one good is subsidized in a particular way. Assume that a consumer has a cash budget of amount R and a subsidy of amount S at his disposal. In the *Cash treatment*, the subsidy S is given lump-sum in cash. In the *Label treatment*, the subsidy has the same amount and is also given lump-sum but it has to be spent on the subsidized good. The crucial detail of our design is that the optimal consumption of the subsidized good is larger than the amount of the subsidy, i.e., a rational consumer should not be influenced by whether the subsidy is given as cash or in kind.

Consider the indifference curve diagram in Figure 1, where the subsidized good (s) is on the horizontal axis and the other good (o) is on the vertical axis. For simplicity, the price of the subsidized good is normalized to $p_s = 1$. The dashed line is the budget constraint in the Cash treatment. Assume that the optimal consumption bundle is B. In the Label treatment, the subsidy is paid in kind; the consumer faces a kinked budget constraint (solid line). However, the kink does not

affect optimal decision making, as the amount of S is lower than the amount s^B spent optimally on the subsidized good. The first-best choice B is still feasible. Under the assumption that fungibility holds, consumption should therefore be identical across treatments.

Now consider a consumer who does not treat different income sources as fungible. A violation of fungibility implies that the consumer has some sort of cognitive or mental sub-budgets between which money cannot be shifted (Thaler 1985). We assume that a labeled payment is posted to the sub-budget the label corresponds to, whereas a cash budget is allocated optimally to the different sub-budgets. In the Cash treatment, we would still expect such a consumer to choose bundle B. As both income components are cash, the consumer can allocate the optimal amounts to the sub-budgets for the two goods and thus choose the optimal consumption bundle. The difference to the standard model occurs in the Label treatment. The consumer will still allocate the cash endowment optimally (assume that this corresponds to bundle A). The subsidy, however, will be allocated to the sub-budget for the subsidized good. Since the sub-budgets are non-fungible, the subsidy will thus be spent entirely on the subsidized good. In the extreme case of complete non-fungibility, this results in a consumption of bundle C where $s^C = s^A + S$ (see Figure 1). If both goods are normal, $s^C \geq s^B$.[4]

Therefore, if some subjects do not treat money as fungible, we should expect average consumption of the subsidized good in the Label treatment to be higher than in the Cash treatment. This does not exclude the possibility that some subjects act in line with fungibility or that others are only influenced to a certain extent by the label attached to the subsidy.

It could be that people consume differently when they receive a gift or subsidy, just because they are in a positive mood. Such a change in mood cannot influence our main treatment comparison as subjects in both treatments get a subsidy.

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[4]This reasoning depends on the order in which cash budget and subsidy are spent. If the consumer spent the subsidy first, he would be able to allocate the cash budget so as to reach bundle B. In the experiments, we are therefore testing the joint hypothesis of fungibility and order of spending. To minimize the influence of the order of spending as much as possible, subjects in the lab experiment decided twice: first, they allocated only the cash budget R. We will call this stage *reference stage*. In a second step, called *subsidy stage*, subjects allocated the combined budget $R + S$. The experience from the reference stage will probably lead most subjects to first allocate the cash endowment during the subsidy stage and then the subsidy, as they have already calculated how to allocate the cash budget R. Additionally, this setup allows us to calculate the consumption increase from reference to subsidy stage to get an intra-person measure of fungibility.
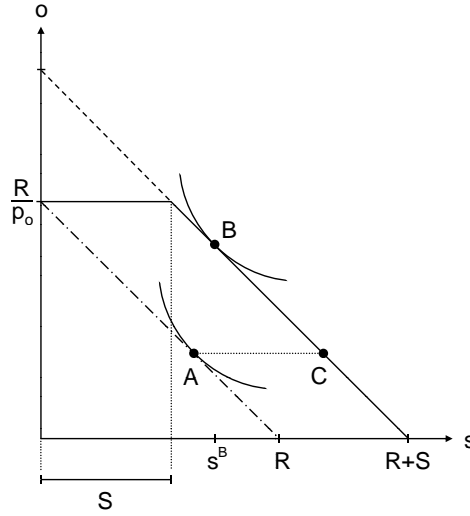
Figure 1: *Consumption decision with inframarginal in-kind benefit. The subsidized good (s) is on the horizontal axis, the other good (o) is on the vertical axis. The dashed line is the budget constraint when the subsidy is given in cash. The solid line is the budget constraint when the subsidy is given in kind. The dot-and-dash line is the budget constraint when no subsidy is given.*

2.2   Design of the Field Experiment

The field experiment was conducted in a wine restaurant situated in a wine-growing region of southern Germany. The restaurant itself is located in a winery. We chose this restaurant because almost all guests eat something *and* drink wine and other beverages; thus guests consume at least a minimal amount of two distinct goods.[5] Giving a beverage voucher that is smaller than the minimal amount spent for beverages will therefore not distort the optimal decision. Usual per-capita spending in this restaurant is about 40 euro ($\sim$54 USD).

Guests were not aware of participating in an experiment. Upon arrival at the restaurant, they learned that the restaurant was celebrating its fourth anniversary (which was indeed the case) and that they would receive an 8-euro voucher per person ($\sim$11 USD at the time of the experiment). The type of voucher differed by day. On days of the Cash treatment, vouchers were given as "gourmet voucher" that could be spent on either beverages or the meal. To avoid confusion with the laboratory experiment, we will call this treatment *Field Cash treatment* (FCT). The Field Cash treatment serves as our primary control treatment. On days of the Label

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[5]In Germany, it is very unusual to not consume beverages in a restaurant; water must also be purchased.

treatment, vouchers were given as a "gourmet beverage voucher". These vouchers could only be spent on beverages. We will refer to this treatment as *Field Label treatment*. From communication with the owner of the restaurant we knew that almost all guests consume beverages worth more than 8 euro, even without getting a voucher. Therefore, the beverage voucher should not distort the consumption decision of these guests; the only treatment difference should be the label on the voucher.[6] Both types of vouchers had to be redeemed the same evening.

The restaurant distributed a total of 196 vouchers, one per person, starting with beverage vouchers until half of the available vouchers were distributed. To avoid distributing vouchers from both treatments on the same evening, the restaurant continued with beverage vouchers during that evening. From the next day on, the remaining vouchers were issued as bill vouchers. Between treatments, observations are thus counter-balanced over days of the week. Overall, 107 vouchers were distributed in the FLT and 89 vouchers in the FCT. We consider each table in the restaurant as one independent observation and calculate all absolute values per capita. Since we distributed one voucher per person, we can relate per-capita consumption directly to the amount of a single voucher. This leaves us with 37 independent observations in the Field Label treatment and 34 in the Field Cash treatment. No guest participated in both FLT and FCT. During the experiment, the menu did not change and the same two waiters were present in the restaurant. Our data consist of the detailed bill per table showing all consumed items and an additional questionnaire filled in by a waiter stating how many persons correspond to this bill, the share of women at the table, the share of persons below wine-drinking age, whether guests paid separately and the amount of the tip.

Additionally, we collected information about the time directly before and after the experiments to compare behavior in the two main treatments described above to the behavior of guests who did not receive either voucher. We have data on 356 persons which corresponds to 116 independent observations (76 before and 40 after the two main treatments).[7]

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[6]We could not avoid that some participants consumed less than the amount of the subsidy because total consumption had a high variance, ranging from 18.95 euro to 84.25 euro. In the laboratory experiment, described in the in next section, we can ensure that the subsidy is infra-marginal for all subjects by choosing an appropriate payoff function and by endowing every subject with the same budget.

[7]We have the full information described above (bill and questionnaire) only for observations after the two main treatments. For the observations before, we know only the bill and the number of persons. Furthermore, it could be that some of these guests show up more than once in our data.

Results of the field experiment are presented in Section 3.

2.3 Design of the Laboratory Experiment

In the laboratory experiment, subjects had to make two subsequent consumption decisions. In each stage, subjects were endowed with a budget that they could allocate on two goods. For each good, we induced a standard microeconomic utility function by specifying monetary payoffs for the possible consumption levels. Total payoff was the sum of the payoffs for each of the two goods in both stages. The first decision stage, which we will call *reference stage*, serves to yield a reference transaction to which decisions in the second stage can be compared. The second stage, called *subsidy stage*, is our main treatment stage.

In the reference stage, subjects received a cash budget of 50 money units which they could allocate freely on the two goods; the reference stage was identical in both treatments. In the subsidy stage, subjects again had an endowment of 50 money units at their disposal and additionally received a subsidy of 30 money units. The only difference between the two treatments was the type of the subsidy. In the *Lab Cash treatment* (LCT), the subsidy was given as an unconditional cash grant. In the *Lab Label treatment* (LLT), the subsidy was given as an in-kind benefit, i.e., the money had to be spent entirely on the subsidized good. Parameters were chosen such that the in-kind benefit was inframarginal for all subjects and not distortionary. By shifting the remainder of their budget appropriately, subjects could reach the same optimal consumption bundle as in the LCT. The only treatment difference therefore was the label attached to the subsidy.

The exact specification of the payoff functions is presented in Table 1. For each good, the payoff is increasing in consumption and the marginal payoff is weakly decreasing. Prices per unit were $p_s = 3$ for the subsidized good and $p_o = 2$ for the other good. Payoff functions and prices were the same in both stages. Unspent budget could neither be saved nor did it yield any payoff. There was no time limit for decisions. For these parameters, the consumption bundles (s, o) displayed in Figure 1 are as follows: the optimal consumption bundle in the reference stage is $A = (12, 7)$; the optimal bundle in the subsidy stage is $B = (13, 20)$; the bundle C is $(22, 7)$.

In order to make the difference between the initial endowment and the subsidy payment more salient, subjects had to earn their endowment in a real-effort task. Before consumption decisions were taken, subjects had to count the number of zeros in large spreadsheets that consisted of zeros and ones. When they managed to count

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Consumed units	0	1	2	3	4	5	6	7	8	9	10	11	12
Payoff													
Subsidized good	0	36	70	102	132	160	186	210	232	252	270	286	299
Other good	0	30	57	81	102	120	135	147	157	166	175	184	192

Consumed units	13	14	15	16	17	18	19	20	21	22	23	24	25
Payoff													
Subsidized good	310	316	322	328	333	338	343	347	351	355	358	361	364
Other good	200	208	216	223	230	237	244	251	256	261	266	271	276

Table 1: *Payoff functions in the laboratory experiment. "Subsidized good" denotes the good that is subsidized in the second stage of the Lab Label treatment. Payoff points were converted into real money after the experiment.*

the correct number of zeros in a given amount of time they earned 100 money units that were later split in half for the two consumption decisions.[8] One subject failed to complete the task on time and is henceforth excluded from the analysis. We chose this rather boring activity to minimize the intrinsic motivation subjects could have for the task and thus to strengthen their perception of really having earned the money (cf. Cherry et al. 2002).

Subjects were students from the University of Bonn studying various majors except Economics. Treatments were assigned randomly and no subject participated in more than one treatment. At the beginning of the experiment, instructions were read aloud and subjects had to answer a number of control questions to ensure that they understood the task.[9] Detailed instructions for the two stages were given later on the computer screen. This allowed us to have subjects of both treatments in the same session and thus to align the delivery of the two treatments as much as possible. At the end of the experiment, subjects answered a questionnaire. The experiment was computerized using the software z-Tree (Fischbacher 2007). 92 subjects participated in the experiment, one of whom failed to complete the real-effort task. This leaves us with 45 independent observations in the Lab Cash treatment and 46 observations in the Lab Label treatment. Payoff points (cf. Table 1) were transformed into real money at a rate of 100 points = 1 euro. In addition to their earnings from

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[8]The precise rules were as follows: subjects got 8 large tables with 300 entries each. To complete the task, they had to count the correct number of zeros on four sheets within 15 minutes. An answer was also counted as correct if the number reported differed only by 1 from the true number. If subjects did not complete the task, they got an endowment of 10 money units only.

[9]See Appendix A for an English translation of the instructions.

the consumption decisions, subjects received a show-up fee of 2.50 euro. On average, subjects earned 12.20 euro (~14.80 USD at the time of the experiment). Sessions lasted between 60 and 70 minutes.

Results of the laboratory experiment are presented in Section 4.

3   Results of the Field Experiment

In this section, we report results of the field experiment. First we show that consumption of the subsidized good (beverages) is higher in the Field Label treatment than in the Field Cash treatment. Guests do not drink more in terms of volume but consume more expensive beverages. Total consumption, including meals, is not affected by the type of subsidy but spending is higher compared to when no voucher is given.

> **Result 1:** *Spending on the subsidized good (beverages) is significantly higher in the Field Label treatment than in the Field Cash treatment.*

Participants in the Cash treatment who receive a bill voucher spend on average 15.04 euro per capita on beverages (alcoholic and non-alcoholic). Participants in the Label treatment spend 18.94 euro on beverages. The treatment difference of 3.90 euro is very high considering the value of the subsidy (8 euro); the marginal effect is 25%. When we regress per-capita beverage consumption on a treatment dummy, the p-value is 0.052 (column 1 of Table 2). In a regression with controls for the share of women, share of persons below wine-drinking age, day of the week, and outside temperature, the treatment coefficient rises to 4.57 euro and the p-value goes down to 0.040 (column 2). The treatment difference is also significant in a non-parametric Mann-Whitney U-test ($p = 0.085$).[10]

Unfortunately, we don't have information on one important predictor of consumption: total wealth (in terms of Figure 1, we cannot observe R). We pursue two alternative approaches to address this problem. First, we take *meal consumption* as proxy for total wealth: if richer people consume more beverages they should also consume more meals. We regress absolute beverage consumption per capita on a treatment dummy and control for meal consumption per capita. The coefficient of the treatment dummy rises to 4.90 euro and has now a p-value of 0.006 (column 3 of Table 2). This result holds when we add the controls mentioned above to the regression (column 4). A second possibility is to regress the *share* of beverage con-

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[10]All tests reported in this paper are two sided.

sumption on a treatment dummy and controls (columns 5 and 6 of Table 2). These regressions also show a highly significant impact of the treatment: giving a beverage voucher instead of a bill voucher increases the share of beverage consumption by 7.9 percentage points (from 33.7 percent; $p = 0.004$). In a regression with the controls described above (excluding meal consumption), the treatment effect goes up to 8.7 percentage points ($p = 0.004$). The difference in beverage shares is also significant in a U-test ($p = 0.013$). All these results and the results in the following regressions continue to hold if we drop the assumption that all observations are independent and instead only assume independence across days (by calculating robust standard errors clustered on day).[11]

Since the restaurant first distributed all beverage vouchers and then all bill vouchers, it might be that the treatment difference in beverage consumption is driven by an overall (falling) time trend. We can test this potential confound with the data we collected before and after the two main treatments; here, guests did not receive either voucher. If a time trend existed, it should also show up in this data. This is, however, not the case. Participants after the two main treatments spend even a little bit more on beverages (16.27 vs. 15.38 euro) but this difference is not significant, neither in a U-test ($p = 0.521$), nor in regressions with or without the controls described above[12] ($p = 0.863$ and $p = 0.562$). There is also no significant time trend in total consumption or in the share of beverage consumption. [13]

Taken together, participants in our field experiment do not treat vouchers and wealth as completely fungible. Next, we analyze how the additional spending on the subsidized good in the Label treatment is used. Do participants consume a larger volume of beverages or do they consume more expensive beverages? Our data support the latter hypothesis.

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[11]The results also hold if we exclude tables that pay separately (only very few do so). The number of persons at a table also plays no role.

[12]Excluding share of women and share of kids, as we have this data only for observations *after* the two main treatments.

[13]This result also speaks against a potential self-selection into the experiment because of word-of-mouth. It could be that early participants tell their neighbors about the vouchers and that these neighbors come to the restaurant to benefit from the vouchers. If this were true and if the neighbors had different consumption patterns, this could influence our treatment comparison. As restaurant guests did not know when the experiment ended, this would imply that we should also find an effect in the comparison of consumption behavior before and after the experiment. As shown above, there is no such difference.

Table 2: Treatment Effect of Receiving a Beverage vs. a Bill Voucher

	Absolute beverage consumption				Share of bev. consumption		Total consumption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if Label treatment	3.903*	4.573**	4.897***	5.109***	0.079***	0.087***	2.089	3.579
	(1.976)	(2.182)	(1.710)	(1.889)	(0.027)	(0.029)	(3.363)	(3.711)
Share of women		-1.617		0.333		-0.018		-5.233
		(4.541)		(3.946)		(0.061)		(7.724)
Share of kids		-10.764		-0.540		0.105		-29.723
		(14.636)		(12.829)		(0.197)		(24.893)
Outside temperature (in °C)		-0.391		-0.391		-0.005		-0.391
		(0.517)		(0.447)		(0.007)		(0.880)
Meal consumption			0.548***	0.539***				
			(0.109)	(0.113)				
Controls for day of the week	No	Yes	No	Yes	No	Yes	No	Yes
Constant	15.035***	21.116*	-0.122	5.988	0.337***	0.413***	42.712***	49.163**
	(1.426)	(10.903)	(3.250)	(9.943)	(0.019)	(0.147)	(2.428)	(18.544)
N.Obs.	71	71	71	71	71	71	71	71
Prob > F	0.052	0.346	0.000	0.000	0.004	0.083	0.537	0.731
Adjusted R^2	0.04	0.01	0.29	0.26	0.10	0.08	-0.01	-0.04

Table 2: *OLS estimates of the treatment effect on absolute and relative spending for the subsidized good (beverages) and on total spending. Notes: Standard errors are in parentheses. The sample includes observations from Field Cash and Field Label treatment. Absolute consumption is measured in euro. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.*

Result 2: *The volume of the consumed beverages does not depend on the type of subsidy given. By contrast, price per volume of the subsidized good is higher in the Field Label treatment.*

Participants in the Cash treatment drink on average 0.807 ltr of beverages (alcoholic and non-alcoholic) while participants in the Label treatment drink 0.857 ltr. The difference of 0.050 ltr (about 2 fl oz) is neither significant in a non-parametric U-test ($p = 0.420$) nor in OLS regressions with or without controls (see columns 1 and 2 of Table 3). In contrast, participants in the Label treatment consume more expensive beverages. The average price per liter is 21.91 euro/ltr in the Field Label treatment, while it is 18.39 euro/ltr in the Field Cash treatment. This difference of 3.52 euro is statistically significant (U-test, $p = 0.045$). In an OLS regression without controls, the p-value is 0.026; in a regression with the controls described above, the coefficient rises to 3.89 euro and the p-value is 0.022 (see columns 3 and 4 of Table 3).[14] We find the same pattern if we analyze the most expensive beverage item ordered. This item cost on average 12.20 euro in the Cash treatment and 19.93 euro in the Label treatment (U-test, $p = 0.003$).

So far we have argued that receiving a beverage voucher compared to a bill voucher should not alter consumption behavior if guests treated different income sources as fungible. The same argument can also be applied to the comparison of guests who receive either voucher and guests who do not receive such a subsidy. The 8 euro increase in lifetime income (by receiving the voucher) can surely be neglected. According to standard theory, we should not expect consumption to be influenced by receiving a voucher. Our data shows, however, that also in this comparison guests do not treat income sources as completely fungible.[15]

Result 3: *Overall spending is higher in both subsidy treatments (FCT and FLT) compared to when no subsidy is given.*

Directly before and after the two main treatments we collected data of guests who did not receive a voucher. These guests spend on average 39.87 euro per capita. Guests in our two main treatments spend on average 3.93 euro more.[16] This difference is

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[14]One table in the FCT did not consume any beverages and is excluded from this analysis. All other results stay the same if we exclude this observation also for those analyses.

[15]Keep in mind that this effect could be influenced by a change in mood: when you get a gift you might be inclined to spend more, just because you are happy. The results so far and the results of the laboratory experiment cannot be influenced by mood.

[16]Average total consumption is not different between the two main treatments. In the Field Cash treatment, participants spend on average 42.71 euro per capita while participants in the Field Label

Table 3: Use of Increased Spending

	Consumed beverage volume		Price per liter	
	(1)	(2)	(3)	(4)
1 if Label treatment	0.050	0.086	3.520**	3.886**
	(0.072)	(0.081)	(1.541)	(1.653)
Share of women		-0.139		-0.167
		(0.169)		(3.424)
Share of kids		-0.025		-10.634
		(0.544)		(11.035)
Outside temperature (in °C)		0.005		-0.587
		(0.019)		(0.390)
Controls for day of the week	No	Yes	No	Yes
Constant	0.807***	0.710*	18.390***	27.903***
	(0.052)	(0.405)	(1.120)	(8.224)
N. Obs.	71	71	70	70
Prob > F	0.494	0.931	0.026	0.078
Adjusted R^2	-0.01	-0.06	0.06	0.08

Table 3: *OLS estimates of the treatment effect on volume (in liter) and price per volume (in euro/liter) of the subsidized good. Notes: Standard errors are in parentheses. The sample includes observations from Field Cash treatment and Field Label treatment. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.*

not significant in a U-test ($p = 0.157$) but in an OLS regression ($p = 0.059$, see Table 4, column 1). If we control for day of the week and outside temperature, the treatment effect rises to 4.92 euro and remains significant ($p = 0.080$, column 2).[17] Spending on beverages is also influenced by receiving a voucher. Since we expect a different impact on spending behavior according to the type of voucher, we regress absolute beverage consumption on two treatment dummies, one for the FLT and one for the FCT (Table 4, columns 3–6). Without controls, receiving a beverage voucher increases beverage consumption by 3.25 euro. This difference is significant ($p = 0.032$) and remains significant if we add the controls described above and/or if we control for meal consumption. The difference is also significant in a U-test ($p = 0.041$). In contrast, beverage consumption is not significantly higher when guests receive a bill voucher (see Table 4, columns 3–6; U-test, $p = 0.900$). The point estimate is even negative in some specifications. Apparently, guests who receive a bill voucher focus almost all additional spending on meals. This is also the reason why the share of beverage consumption is lower in the Cash treatment compared to guests without voucher (see Table 4, columns 7 and 8). In the Label treatment, the share of beverage consumption is higher compared to guests without voucher but not significantly. Our results on the comparison between guests with and without voucher are in line with findings of Milkman et al. (2007). They analyze data from an online grocer when customers redeem a \$10-off coupon compared to when they don't. Controlling for customer fixed effects, they find that customers spend on average \$1.87 more when they redeem such a subsidy.

Because of the high variance in total per-capita consumption, we could not exclude the possibility that some guests would like to consume less than the amount of the voucher, i.e., that their decisions are distorted. Indeed, in 16% of observations in the Field Cash treatment and of observations without voucher, absolute beverage consumption is lower than the value of the voucher. To avoid these distortions, we sort all observations by per-capita beverage consumption and exclude the lowest 16% in each treatment. In the resulting sample, the subsidy is inframarginal and

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treatment spend 44.80 euro. This difference is neither significant in a U-test ($p = 0.645$), nor in regressions with our without controls (see Table 2, columns 7 and 8). The amount of tip is also not different between the two main treatments and also not different compared to when no voucher is given (U-tests, FLT/FCT: $p = 0.416$, FLT/no voucher: $p = 0.327$, FCT/no voucher: $p = 0.777$). In Germany, there is no strong norm about how much tip to give; tipping in our data ranges from 0% to 12.5% of the amount of the bill.

[17]We do not have information about the share of women and the share of persons below wine-drinking age for observations before the experiment, thus we cannot control for these variables like we did in Table 2.

Table 4: Treatment Effect of Receiving Either Voucher

	Total consumption		Absolute beverage consumption				Share of bev. consumption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if Label or Cash treatment	3.930*	4.918*						
	(2.071)	(2.792)						
1 if Label treatment			3.251**	3.890**	2.507*	2.991*	0.025	0.033
			(1.509)	(1.842)	(1.353)	(1.650)	(0.023)	(0.027)
1 if Cash treatment			-0.652	0.092	-2.200	-1.521	-0.054**	-0.045
			(1.558)	(1.954)	(1.411)	(1.761)	(0.023)	(0.029)
Outside temperature (in °C)		-0.348		-0.239		-0.197		-0.002
		(0.358)		(0.209)		(0.187)		(0.003)
Meal consumption					0.443***	0.448***		
					(0.065)	(0.066)		
Controls for day of the week	No	Yes	No	Yes	No	Yes	No	Yes
Constant	39.871***	44.883***	15.687***	19.551***	4.971***	8.387**	0.390***	0.426***
	(1.276)	(5.772)	(0.742)	(3.380)	(1.695)	(3.434)	(0.011)	(0.050)
N. Obs.	187	187	187	187	187	187	187	187
Prob > F	0.059	0.417	0.065	0.403	0.000	0.000	0.018	0.095
Adjusted R^2	0.01	0.00	0.02	0.00	0.22	0.20	0.03	0.03

Table 4: *OLS estimates of the treatment effect compared to participants who do not receive a voucher. Notes: Standard errors are in parentheses. The sample includes observations from all three field treatments. Absolute consumption is measured in euro. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.*

non-distortionary for all participants. As shown in Tables 5 and 6, the distortions cause only a small part of the treatment differences and overall results remain very similar. The treatment difference between Label and Cash treatment in absolute beverage consumption is now between 3.40 and 4.53 euro depending on the exact specification (Table 5, columns 1–4). In the full sample, this effect was between 3.90 and 5.11 euro (see Table 2). The treatment effect remains significant (except for column 2). The share of beverage consumption increases by about 6.5 percentage points from Cash to Label treatment (full sample: 7.9 to 8.7 percentage points). We also obtain very similar results if we repeat the comparisons between guests who did or did not receive a voucher (see Tables 4 and 6): receiving a subsidy now increases consumption by 3.90 to 3.79 euro (full sample: 3.93 to 4.92 euro). The impact of a beverage voucher on beverage consumption compared to guest without voucher is now between 3.16 and 3.44 euro (full sample: 2.51 to 3.89 euro). Significance levels stay the same in most specifications.

Although it is reassuring to see that distorted participants do not drive the treatment difference in our field experiment, we would like to confirm the results in a setting where we can guarantee that the subsidy is inframarginal for all subjects. In a laboratory experiment, this is possible: we know budget and optimal decision and both are the same for all subjects.

4   Results of the Laboratory Experiment

In this section we report results from the laboratory experiment. First, we show that giving a labeled subsidy instead of a cash grant increases consumption of the subsidized good. Then we present evidence that this effect is stronger for subjects with lower mathematical abilities. Finally, we demonstrate that subjects' moral concerns cannot explain the treatment effect.

4.1   Consumption in the Experiment

The lab experiment consisted of two stages: in the *reference stage*, subjects allocated only a cash budget; in the subsequent *subsidy stage*, subjects had again the cash budget and an additional subsidy at their disposal. The total budget was identical for every subject. Before we turn to the subsidy stage, we analyze consumption decisions in the reference stage. The design of the reference stage was the same in both treatments. In particular, subjects were not aware of the fact that there

Table 5: Treatment Effect of Receiving a Beverage vs. a Bill Voucher for Non-distorted Participants

	Absolute beverage consumption				Share of bev. consumption		Total consumption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if Label treatment	3.398*	3.463	4.528**	4.216**	0.066***	0.064**	0.854	1.735
	(1.936)	(2.224)	(1.746)	(2.007)	(0.024)	(0.028)	(3.420)	(3.896)
Share of women		-2.357		-0.247		-0.009		-7.195
		(5.156)		(4.662)		(0.064)		(9.031)
Share of kids		-12.950		-3.558		0.089		-34.483
		(13.249)		(12.160)		(0.165)		(23.206)
Outside temperature (in °C)		-0.544		-0.513		-0.008		-0.613
		(0.537)		(0.482)		(0.007)		(0.940)
Meal consumption			0.444***	0.436***				
			(0.111)	(0.118)				
Controls for day of the week	No	Yes	No	Yes	No	Yes	No	Yes
Constant	17.421***	28.890**	4.481	14.963	0.372***	0.536***	46.552***	60.820***
	(1.403)	(11.577)	(3.473)	(11.056)	(0.018)	(0.144)	(2.479)	(20.279)
N.Obs.	59	59	59	59	59	59	59	59
Prob > F	0.085	0.528	0.000	0.014	0.008	0.144	0.804	0.775
Adjusted R^2	0.03	-0.01	0.24	0.18	0.10	0.07	-0.02	-0.05

Table 5: *OLS estimates of the treatment effect on absolute and relative spending for the subsidized good. The sample is restricted to observations of non-distorted participants: we sort all observations by per-capita beverage consumption and exclude the lowest 15% in each treatment (see text). Notes: Standard errors are in parentheses. The sample includes observations from Field Cash and Field Label treatment. Absolute consumption is measured in euro. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.*

Table 6: Treatment Effect of Receiving Either Voucher for Non-distorted Participants

	Total consumption		Absolute beverage consumption				Share of bev. consumption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if Label or Cash treatment	3.900*	3.792						
	(2.029)	(2.783)						
1 if Label treatment			3.434**	3.436*	3.156**	3.253*	0.037*	0.038
			(1.511)	(1.838)	(1.428)	(1.730)	(0.021)	(0.025)
1 if Cash treatment			0.036	0.490	-1.054	-0.495	-0.029	-0.023
			(1.571)	(1.994)	(1.503)	(1.888)	(0.022)	(0.028)
Outside temperature (in °C)		-0.117		-0.146		-0.157		-0.003
		(0.354)		(0.210)		(0.198)		(0.003)
Meal consumption					0.319***	0.326***		
					(0.072)	(0.072)		
Controls for day of the week	No	Yes	No	Yes	No	Yes	No	Yes
Constant	43.100***	46.489***	17.384***	21.441***	9.176***	13.308***	0.401***	0.468***
	(1.244)	(5.722)	(0.741)	(3.409)	(1.975)	(3.677)	(0.010)	(0.047)
N. Obs.	157	157	157	157	157	157	157	157
Prob > F	0.056	0.558	0.069	0.260	0.000	0.000	0.046	0.086
Adjusted R^2	0.02	-0.01	0.02	0.01	0.13	0.13	0.03	0.04

Table 6: *OLS estimates of the treatment effect compared to participants who do not receive a voucher. The sample is restricted to observations of non-distorted participants: we sort all observations by per-capita beverage consumption and exclude the lowest 15% in each treatment (see text). Notes: Standard errors are in parentheses. The sample includes observations from all three field treatments. Absolute consumption is measured in euro. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.*
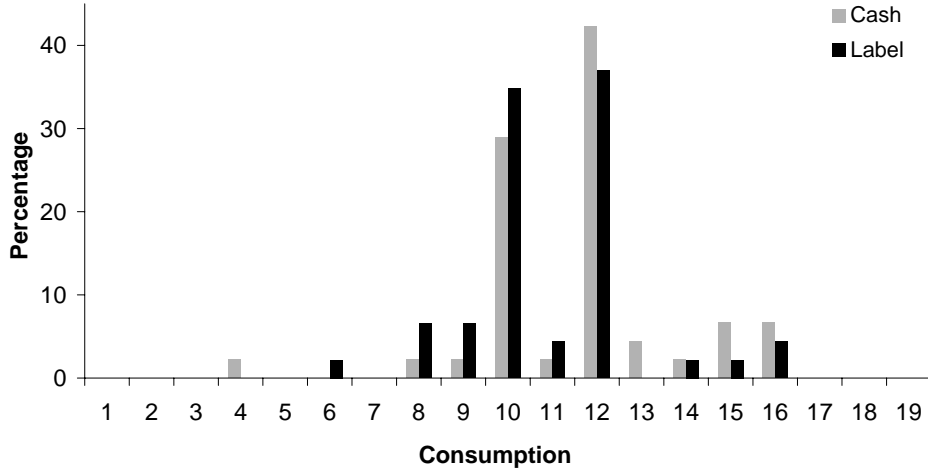
Figure 2: *Consumption of the subsidized good in the reference stage. A consumption of 12 units maximizes payoff.*

would be two different treatments in the subsidy stage. Figure 2 shows a histogram of consumption choices for the (later to be) subsidized good. Choices in the Lab Cash treatment are represented by grey bars; choices in the Lab Label treatment are represented by black bars. We find that decisions are very similar: the modal choice in both treatments is the optimum of 12 units. On average, subjects in the Lab Label treatment consume 11.0 units of the subsidized good, while subjects in the Lab Cash treatment consume 11.6 units. Subjects apparently have no problem understanding the decision and take the decision seriously. We are therefore confident that the experimental situation allows for meaningful interpretation and that the experimental incentives work. OLS regressions show that consumption is not different between treatments (see Table 7: column 1 without controls, column 2 with controls for age and gender).[18]

Next, we focus on outcomes in the subsidy stage. Our first result concerns the impact of the labeled subsidy on consumption choice and confirms the main finding

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[18]A U-test between treatments is, however, significant ($p = 0.066$). Still, this does not compromise our results for several reasons: first, in terms of profits, there is virtually no treatment difference (U-test, $p = 0.983$). Second, the absolute distance to the optimum is not different between treatments (U-test, $p = 0.577$). Finally, consumption is slightly *lower* in the Lab Label treatment. Thus, if there is any inertia in subjects' decisions, results from the reference stage work against a potential treatment effect in the subsidy stage, making our results even stronger.

of the field experiment.

> **Result 4:** *Consumption of the subsidized good is significantly higher in the Lab Label treatment. The marginal propensity to consume out of the subsidy is twice as large in the Lab Label treatment as in the Lab Cash treatment.*

A histogram of consumption choices in the subsidy stage is shown in Figure 3. Recall that the experiment is designed such that the same optimal consumption bundle can be reached in both treatments. If all subjects acted in line with fungibility, there should be no treatment difference. In the Cash treatment, we find that the modal choice is to consume the optimal amount of the subsidized good (13 units), and average consumption is 14.4 units.[19] In the Label treatment, by contrast, a consumption of an amount of 20 units is the most frequent choice and only a relatively small share of subjects choose the payoff-maximizing amount of 13 units. Overall, subjects in the Label treatment buy too much of the subsidized good, consuming 16.7 units on average; the marginal effect is 15%. The treatment difference is highly significant (U-test, $p = 0.006$). Moreover, subjects in the Label treatment leave money on the table, as their choices translate into significantly lower payoffs (U-test, $p = 0.014$). In Table 7 we provide OLS estimates of the treatment effect. The estimates in columns 3 and 4 show that the treatment dummy is positive and highly significant. The coefficient has a value of about 2.3 consumption units ($p = 0.006$) and remains unaffected when we control for age and gender.[20] To check the robustness of this finding we analyze an additional measure of performance: the absolute distance from the optimal consumption level. This measure also treats too low consumption as error. On average, subjects in the Cash treatment choose a consumption level that is 2.44 units away from the optimum; subjects in the Label treatment are on average 4.35 units away from the same optimum. The difference is significant (U-test, $p = 0.004$).

The two-stage design of our experiment enables us to compute an intra-person measure of fungibility by comparing decisions in the subsidy stage to decisions in the reference stage. A histogram of the intra-person change in consumption is shown in Figure 4. On average, the consumption increase in the Cash treatment is 2.8 units.

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[19]Very few subjects in the subsidy stage choose a consumption bundle that is not on the Pareto frontier (one in the LLT and two in the LCT). For ease of exposition, we report only the consumption of the subsidized good. Consumption of the other good can then be readily calculated. Our results do not change if we confine the analysis to the Pareto optimal choices.

[20]All regression results in Table 7 remain virtually unchanged if we perform tobit regressions instead of OLS, controlling for the fact that subjects could not consume more than 25 units of each good.
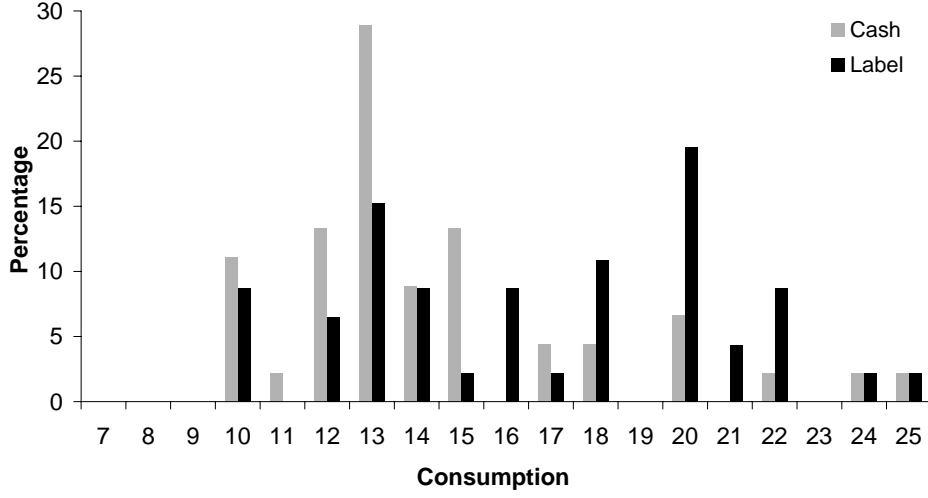
Figure 3: *Consumption of the subsidized good in the subsidy stage. A consumption of 13 units maximizes payoff.*

In contrast, the average consumption increase in the Label treatment is 5.7 units. This difference-in-difference is highly significant (U-test, $p = 0.001$).[21] We can also express the consumption increase in terms of marginal propensity to consume the subsidized good (MPC). As the subsidy payment has a value of 10 units of the subsidized good and as the additional subsidy is the only budget change between the reference and the subsidy stage, the resulting MPC out of the subsidy is 0.574 in the Label treatment and 0.280 in the Cash treatment. Attaching a label to the subsidy therefore doubles the MPC out of the subsidy.

Our next result documents the considerable heterogeneity we observe in behavior across individuals.

> **Result 5:** *The treatment difference is mainly driven by subjects who increase their consumption by the full amount of the subsidy.*

Figure 4 shows that the most frequent choice in the Cash treatment is a consumption increase by either 1 or 2 units. In contrast, the modal choice in the Label treatment is a consumption increase by 10 units, i.e., these subjects spend the entire subsidy on the subsidized good, on top of the consumption from the reference stage. Subjects who treat income sources as completely non-fungible will do exactly this (cf. bundle

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[21]The increase from reference to subsidy stage of absolute distance to the optimal consumption level is also higher in the Label treatment (U-test, $p = 0.034$).
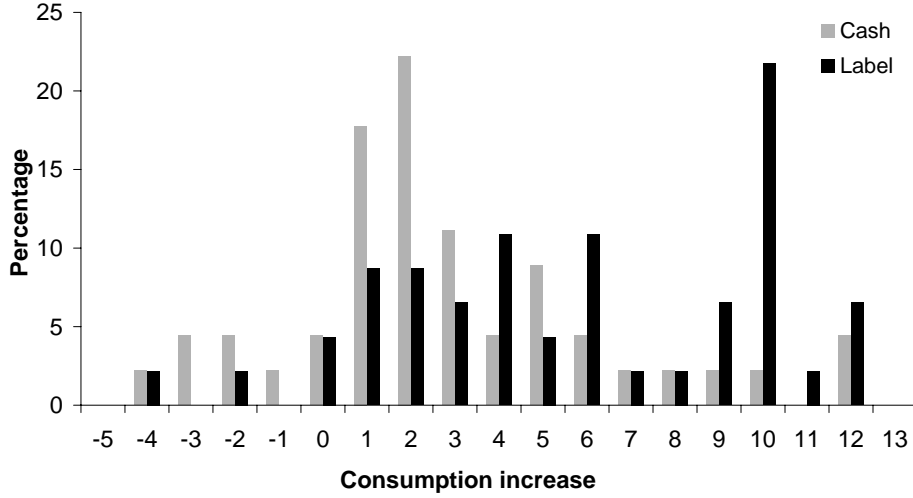
Figure 4: *Consumption increase of the subsidized good from reference stage to subsidy stage. The subsidy is worth 10 units of the subsidized good.*

C in Figure 1). In the Label treatment, 22% of subjects spend the whole subsidy on the subsidized good, while this is true for only 2% of subjects in the Cash treatment. These subjects drive most of the treatment effect, but not all of it. If we exclude these subjects from the analysis, the treatment difference in absolute consumption remains, although it is considerably smaller (1.4 units, previously 2.3 units; U-test, $p = 0.089$). The same is true for the treatment difference in consumption change (1.9 units, previously 2.9 units; U-test, $p = 0.020$).

Interestingly, subjects who spend the entire subsidy on the subsidized good decide much faster than the remaining subjects. They need on average 107 sec for their decision, whereas the other subjects need 234 sec, more than twice as long (U-test, $p = 0.001$). This difference suggests that spending the subsidy fully on the subsidized good is the result of a simple decision heuristic (like mental accounting) rather than extensive deliberations. As a consequence of their consumption decision, subjects who spend the entire subsidy on the subsidized good earn less than all other subjects (U-test: $p = 0.001$) and also less than the other subjects in the Label treatment ($p = 0.003$).

Table 7: Consumption in the Laboratory Experiment

	Consumption of the subsidized good							
	Reference stage		Subsidy stage					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if Label Treatment	-0.666	-0.652	2.273***	2.270***	0.911	0.869	2.091*	2.088*
	(0.438)	(0.439)	(0.808)	(0.818)	(1.081)	(1.103)	(1.069)	(1.081)
1 if Low-Math Group					-0.796	-0.861		
					(1.160)	(1.191)		
Label * Low-Math					2.883*	2.960*		
					(1.615)	(1.653)		
1 if High-MO Group							-0.970	-0.989
							(1.157)	(1.186)
Label * High-MO							0.240	0.254
							(1.655)	(1.676)
Age		-0.077		0.021		0.047		-0.010
		(0.075)		(0.140)		(0.141)		(0.144)
1 if Female		-0.402		0.132		0.068		0.115
		(0.458)		(0.852)		(0.843)		(0.858)
Constant	11.622***	13.605***	14.422***	13.870***	14.741***	13.663***	14.875***	15.045***
	(0.311)	(1.769)	(0.574)	(3.293)	(0.733)	(3.259)	(0.790)	(3.511)
N. Obs.	91	91	91	91	91	91	91	91
Prob > F	0.132	0.270	0.006	0.057	0.010	0.046	0.036	0.132
Adjusted R^2	0.01	0.01	0.07	0.05	0.09	0.07	0.06	0.04

Table 7: *OLS estimates of the treatment effect on consumption of the subsidized good in the laboratory experiment. Notes: Standard errors are in parentheses. The dependent variable is consumption of the subsidized good in the reference stage (columns 1 and 2) or in the subsidy stage (columns 3 to 8) measured in units. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.*

4.2 Impact of Mathematical Abilities

A consumer who does not treat different income components as fungible reduces the complexity of the consumption decision. In our setup, ignoring fungibility divides the rather complex two-good decision into two simple one-dimensional problems. Subjects with lower mathematical skills will have a larger gain from reducing the complexity of the decision. We therefore expect these subjects to violate fungibility more often and, as a consequence, to be more influenced by the treatment manipulation. We mentioned in Section 1 that a consumer who brackets his decisions narrowly, i.e., who does not decide globally, will violate fungibility. Read et al. (1999) conjecture that narrow bracketing is negatively correlated with cognitive and mathematical abilities.[22] Our next result supports their intuition. To analyze the interplay of cognitive abilities and the treatment effect, we use subjects' math grade in their final high school exam as a proxy for their cognitive and mathematical abilities.

> **Result 6:** *The treatment difference in consumption is larger for subjects with lower mathematical abilities.*

The grades were elicited in the post-experimental questionnaire. We split the sample at the median according to math grade, leading to a "High-Math" group (n= 50) and a "Low-Math" group (n=41). Since mathematical abilities might not only influence the decision in the Label treatment but also in the Cash treatment, we compare the treatment difference within the High-Math group to the difference within the Low-Math group.

Figure 5 presents cumulative percentages of consumption decisions in both treatments. In the High-Math group (left panel), cumulative distributions are very close to each other. In contrast, in the Low-Math group (right panel), there is a wide gap between the two distributions. The treatment effect in the High-Math group is 0.9 units (U-test, $p = 0.357$). The treatment effect in the Low-Math group of 3.8 units is considerably larger (U-test, $p = 0.004$). We test whether the difference between treatment effects is statistically significant in OLS regressions. Results are presented in Table 7, columns 5 and 6. The dependent variable is the consumption of the subsidized good. As explanatory variables we include a treatment dummy (=1 if Label treatment), a Math dummy (=1 if Low-Math) and the interaction

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[22]Thaler (1985) argues that mental accounting, a concept similar to narrow bracketing, serves as a heuristic to overcome problems of limited self-control. In our experiment, limited self-control plays no role. Subjects with lower cognitive abilities should be less likely to realize this fact and should thus be more likely to use this heuristic.
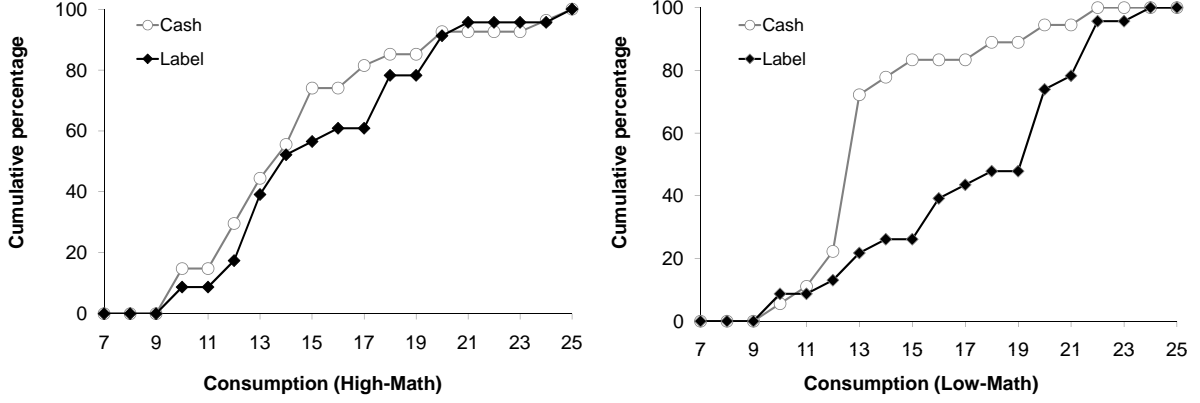
Figure 5: *Cumulative distribution of the consumption of the subsidized good for High-Math group (left panel) and Low-Math group (right panel). The grey line always depicts consumption in the Cash treatment, the black line consumption in the Label treatment. In order to form a High-Math group (n=50) and a Low-Math group (n=41) we elicited the math grades subjects obtained in their final high school exam and split the sample at the median.*

of the two. Compared to regressions without controls for mathematical abilities (columns 3 and 4), the treatment dummy is much smaller and insignificant. This indicates that subjects in the High-Math group (the reference category) do not act differently across treatments. In contrast, the coefficient of the interaction term is large (2.9 units) and significant. Controlling for age and gender leaves the significance level unaffected, and the coefficient increases slightly. Thus, the treatment effect is significantly larger for subjects who belong to the Low-Math group. We obtain the same result if we ignore the Cash treatment and directly compare consumption of High-Math and Low-Math subjects in the Label treatment. Low-Math subjects consume on average 2.1 units more of the subsidized good than High-Math subjects (U-test, $p = 0.066$). All these results hold if we take absolute distance to the optimal consumption as dependent variable.[23]

We have shown before that subjects who spend the whole subsidy on the subsidized good account for a large part of the treatment effect. The relation we have just shown between mathematical abilities and consumption in the subsidy stage holds also for these subjects. 90% of subjects who spend the entire subsidy on the subsidized good belong to the Low-Math group. Among all other subjects, this share is only 39.5%.

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[23]In the German high school system, there are two types of math course: intensive and basic course (*Leistungskurs* and *Grundkurs*). If we control for subjects' course results are similar.

4.3 Moral Obligation as an Alternative Explanation?

So far we have attributed the treatment difference to cognitive limitations that prevent subjects from treating the two income components as fungible. However, one could also imagine that receiving a benefit payment causes a feeling of moral obligation to spend the money in accordance with the benefit giver's intention. In response to the intention that is conveyed by the label, recipients might increase their consumption of the subsidized good above the level they would have chosen if they had received the same amount as an unconditional cash payment. This reasoning cannot explain the behavioral patterns according to mathematical ability that we presented in the last section (unless one makes the rather strong assumption that mathematical abilities and moral attitudes are negatively correlated). But perhaps a feeling of moral obligation could be an additional factor influencing behavior. The next result shows, however, that behavior in the experiment is not related to subjects' attitudes towards moral obligation.

> **Result 7:** *A feeling of moral obligation to comply with the label on the subsidy cannot explain the treatment difference in consumption.*

To measure subjects' attitude regarding moral obligation, we included a scenario in the post-experimental questionnaire in which subjects had to judge the behavior of a fictitious person. The scenario reads as follows (translated from German):

> Mr and Mrs Miller have two children (5 and 8 years old). They earn a total amount of 2000 euro per month, after taxes. Additionally, they receive 180 euro child benefit per child, i.e., a total of 360 euro per month. Usually, they spend about 300 euro per month for their children (child clothing, toys, etc.). They spend the rest of the child benefit on other things (e.g., their own hobbies).

Subjects had to indicate on a point scale from 1 to 6 how they judge the fictitious persons' behavior, 1 indicating "not appropriate at all" and 6 indicating "completely appropriate". Thus, a higher number indicates a *weaker* feeling of moral obligation. The decision situation described in the scenario above is very similar to the consumption decision in our experiment. In both situations, the intended use of the subsidy is obvious but the subsidy is not binding, i.e., a rational decision maker should not be influenced by the label attached to the subsidy.[24]

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[24]The questionnaire included two other scenarios concerning (i) a person claiming student support provided by the state in spite of not being entitled to it, and (ii) a person temporarily claiming

Analogous to our analysis of mathematical abilities, we split the sample at the median. This results in a group with strong moral obligation ("High-MO", n=38) and a group with weak moral obligation ("Low-MO", n=53). The cumulative percentages for both groups are shown in Figure 6. In both panels, the cumulative distributions are equally far from each other, indicating that the effect of the label does not depend on the moral concerns of the subjects. The treatment effect is essentially the same in both High-MO and Low-MO group (2.3 vs. 2.1 units); the significance levels of U-tests within each group are $p = 0.119$ and $p = 0.057$. This result is confirmed by a regression analysis. Columns 7 and 8 of Table 7 include a dummy equal to one if a subject is in the High-MO group and an interaction term between the treatment dummy and the High-MO dummy. We find that, in contrast to the math regressions in columns 5 and 6, the coefficient on the treatment dummy remains large (2.1 units) and significant. This indicates that also subjects with weak moral obligation are influenced by the label on the subsidy. Moreover, the interaction term is small (0.2 units) and far from being significant. Thus, the group of subjects with stronger moral obligation does not show a larger treatment effect.[25] This holds true if we focus on the subjects who spend the whole subsidy on the subsidized good in addition to their consumption in the reference stage. These subjects do not differ in their moral obligation from other subjects (U-test, $p = 0.298$). Taken together, these results indicate that moral obligation does not drive the treatment effect in the lab experiment.

5 Discussion and Conclusion

In this paper we pursued a dual research strategy by combining a natural field experiment and an incentivized laboratory experiment to test whether consumers treat different income sources as fungible. Both experiments yield the same results: even in a simple setup, many subjects do not act in line with fungibility. This effect is stronger for persons with lower mathematical skills. Differences in preferences, e.g, concerning the moral obligation to comply with the intention of the subsidy

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unemployment benefits although having a new job already on the horizon. The full texts of these two scenarios are reported in Appendix B. The results for all three scenarios are very similar.

[25]The answers to the two other vignettes are also not systematically related to the consumption of the subsidized good. In a similar regression as in column 7 of Table 7, the interaction terms are +0.91 units and -1.07 units for the student-support scenario and the unemployment-benefit scenario, respectively. Both are not significant. The same holds true if we take the average answer of each subject to the three vignettes as our proxy for moral obligation.
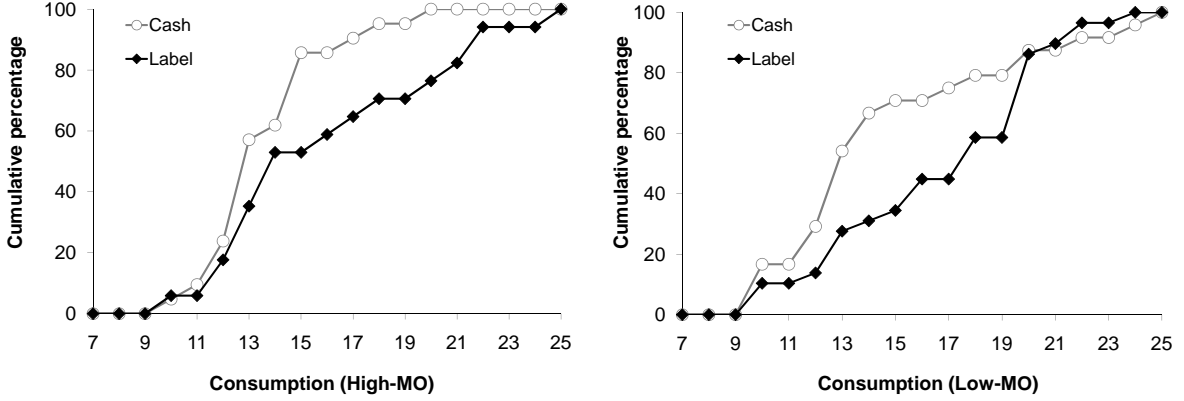
Figure 6: *Cumulative distribution of the consumption of the subsidized good for High-Moral-Obligation group (left panel) and Low-Moral-Obligation group (right panel). The grey line depicts consumption in the Cash treatment, the black line consumption in the Label treatment. In order to form a High-MO group (n=38) and a Low-MO group (n=53) we elicited moral attitudes via a vignette question concerning the use of child benefits and split the sample at the median.*

giver, do not drive our results.

As discussed in the introduction, our findings have implications for the study of stock market behavior and life-cycle savings. We furthermore suggest that fungibility plays an important role in a different setting where it has until now not been considered: the effect of in-kind benefits on consumption and market prices. Empirical studies for the U.S. and for France have shown that a rise in housing benefits for low-income tenants has lead to drastic rent increases (see, e.g., Susin 2002, Fack 2006).[26] For the U.S., where not all eligible households do receive the benefits, Susin (2002) finds that housing benefits even caused a *net loss* for low-income households. The standard explanation for this phenomenon is that the subsidy causes an increase in housing demand which is met by an inelastic supply. Our findings suggest that this is only part of the story. Taking our results at face value, tenants who receive housing benefits will have a higher willingness to pay for a given apartment, compared to tenants who receive the same amount as a cash grant. Landlords who anticipate this effect can increase the rent accordingly. Laferrère & Le Blanc (2004) present evidence from France that supports this view: controlling for apartment and neighborhood characteristics, landlords discriminate between non-assisted tenants and tenants who receive housing assistance, charging the latter group significantly higher rents. As a result, housing benefits do not necessarily make the recipients

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[26]Similarly, Gibbons & Manning (2006) show for the U.K. that a reduction in housing benefits has lead to lower rents.

better off, but may constitute a transfer payment from taxpayers to landlords. In our view, this problem can be mitigated by linking housing benefits less saliently to rent payments. The periodicity of the benefit payments, for instance, could be chosen such that it differs from the periodicity of the rent payments. Moreover, one could design the benefit system such that the exact amount of the subsidy depends on variables which the landlord cannot observe.

There are, however, other benefit payments for which it is good news if recipients violate fungibility. If recipients of child benefits do not treat money as fungible, they will spend more of the subsidy on child-related goods, compared to a situation in which they receive a cash grant of the same amount.[27] This effect has been documented with data from the Dutch child benefits system: Kooreman (2000) finds that the marginal propensity to consume child clothing out of child benefits is higher than it is out of other income.[28] For a similar benefit system, Munro (2005) finds that the unconditional "winter fuel allowance" in the UK has a positive effect on heating expenditures. These results suggest that public policy can influence consumers in a simple way by explicitly stating the intended use of the subsidy (Thaler & Sunstein 2003).

Our results do not imply that everybody in every situation will violate fungibility. In our laboratory experiment, treating money as non-fungible is linked to mathematical abilities and not to preferences, suggesting that this behavior is a mistake. Once the rational solution becomes obvious to subjects, e.g., by learning or by explanation, they will probably regret their decision and choose the optimal solution. The field experiment shows, however, that also experienced participants can be influenced by a label on the subsidy. In addition, coming back to our previous example of housing benefits, most people make only few housing decisions in their life; here, the scope for non-rational behavior should be especially large.

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[27]Recipients of child benefits are not restricted in their use of these funds and only the name of the benefit payment marks it as a separate income component. But the general argument of this paper applies if we assume that also merely labeled payments are posted to the mental sub-budget corresponding to the label.

[28]Blow et al. (2004), however, analyze data from the U.K. and find such a labeling effect only for some of their specifications.

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A   Instructions

Welcome to today's decision experiment.

To start, please read these instructions carefully. At the end of the instructions you will find some example questions. The experiment starts as soon as all participants have answered these questions correctly.

Please note that it is not allowed to communicate with other participants of the experiment from now on. If this should happen, the experiment loses its scientific value and we have to stop the experiment. If you have any questions, please hold your hand out of the cubicle; we will then come to you.

The experiment consists of two parts. They will be called **work phase** and **shopping phase**. During the work phase you have the possibility to earn talers. You can then use these talers for shopping during the shopping phase. The value your purchases have for you will be denoted in points during the experiment. Directly after the experiment, the points you achieved will be summed up and paid in cash to you according to an exchange rate of

1 point = 0.01 euro

In addition, you receive **2.50 euro** for having showed up on time. The 2.50 euro will be paid after the experiment independently of your decisions and **additionally** to the amount you earn during the experiment.

Work phase

During the **work phase** you have the opportunity to earn 100 talers. The work consists of counting the number of zeros in tables filled with zeros and ones. Below, you see an example table with 3 rows and 8 columns. The tables used in the experiment are larger, they contain 10 rows and 30 columns.

Example of work phase

1	1	1	0	0	0	1	0
1	0	1	0	1	1	0	1
1	0	0	0	1	0	1	1

You earn the 100 talers if you succeed in finding the correct number of zeros in four tables within 15 minutes. If you do not succeed in finding the correct number of zeros in four tables you earn 10 talers instead.

Work phase screen

During the work phase, you will receive eight sheets with zeros and ones. Please begin on sheet 1 and count the number of zeros on this sheet. Enter the number of zeros in the input box in the middle of the computer screen. After entering the number click on the OK-button. If you entered the correct number, you may continue with sheet 2. If you entered a number that is higher by 1 or lower by 1 than the correct number, your number will also be rated as correct. If you enter a number that deviates by more than plus/minus 1 from the correct number, your input will be rated as false. You then have another two tries to enter the correct number for this sheet. Thus, you have three tries in total for each sheet. In the top-right hand corner of the screen, you can see the remaining time in seconds. The time starts at 900 seconds = 15 minutes and counts backwards.

Please note: the **red number** above the OK-button indicates the number of the current sheet. If you enter three times a wrong number for a sheet, the counter for

the current sheet changes to the next sheet. If this occurs, please put the current sheet aside and start the next one.

You have a total of eight sheets at your disposal. As soon as you found the correct number of zeros on four sheets, the task is completed successfully and you receive 100 talers. **You then have finished the work phase.** If you do not succeed in completing the task within 15 minutes, you earn 10 talers instead.

Please note: Experience shows that is helpful to mark the 50th, 100th... counted zero. If you miscount in this case you do not have to start all over again but you can continue from the last marked zero.

Shopping phase

The **shopping phase** starts as soon as it has been determined for every participant if he or she completed the task of the work phase successfully. You will make **two** shopping decisions. Your credit balance is split equally between the two decisions. If you completed the task of the work phase successfully you have $100/2 = 50$ talers at your disposal per purchasing decision, otherwise you have $10/2 = 5$ talers.

During the shopping phase you can spend your money on two things that will be called **housing** and **clothing**. You decide which amount of housing and clothing you want to buy. Expenses for housing denote the rent of the apartment.

The value housing and clothing have for you are expressed in points that are exchanged into euro at the end of the experiment and paid out to you. How valuable a specific amount of housing or clothing is for you is denoted in two tables during the experiment. Below you see an example. In this example numbers of points and prices take on **different values** than in the experiment. The sole purpose of this example is to help you become familiar with the procedure of the purchasing decision.

Example of shopping phase

Housing			Clothing			
Units	Points		Units	Points		
0	0		0	0		
1	6		1	16		
2	11		2	24		
3	15		3	27		
4	18		4	29		
5	20		5	30		

Your credit balance
20 talers

Prices per unit
Housing: 4 talers
Clothing: 3 talers

In the left column of each table, the different amounts that are offered for sale are presented. The right column indicates how many points you get for the purchase of the corresponding amount. You can read from the table "Housing" that in this example 0 units of housing have a value of 0 points for you, 1 unit of housing has a value of 6 points, 2 units 11 points, and so on.

Your credit balance for the purchase is indicated in the top-right panel; in this example 20 talers. In the bottom-right panel you find the prices (in talers) for housing and clothing; prices are per unit. The **prices** for housing and clothing are **different**. The table "Prices per unit" shows that in this example a unit of housing costs 4 talers while clothing costs 3 talers per unit.

In the purchasing decision, you decide how many units of housing and how many units of clothing you want to buy. You can choose freely how many units to buy as long as the total price does not exceed your credit balance.

The **total price of your purchase** is calculated as follows:

Total price of purchase = (units of housing × price per unit of housing)   + (units of clothing × price per unit of clothing)

As soon as you have decided how many units of housing and how many units of clothing to buy, it is determined how many points you will get for this decision. If you do not spend your entire credit balance, **the talers not spent are forfeited**. Additionally, talers from the first purchasing decision cannot be kept for the second purchasing decision.

The **total number of points** is calculated as follows:

Total number of points =	points for purchased units of housing
	+ points for purchased units of clothing

Example of a purchase

In the example mentioned above, you have a credit balance of 20 talers. Imagine you wanted to buy 3 units of housing and 2 units of clothing. Then you have to pay $[(3 \times$ price per unit of housing$) + (2 \times$ price per unit of clothing$)]$ talers, i.e., $12+6 = 18$ talers. This purchase is possible with your credit balance.

In the tables, you find the number of points you get for this purchase. You get **15 points** for 3 units of housing and **24 points** for 2 units of clothing. Your purchase would thus earn you $15 + 24 = 39$ **points**

Please note: It is only possible to buy **one** amount of each good. For example, if you want to buy altogether 4 units of clothing, the point value that is noted next to the number 4 (29 points) matters for you. You cannot buy first one unit of clothing and then another 3 units of clothing, for example.

On the computer, you make your decisions on the input screen of the shopping phase. Below you see a screen shot of this input screen. The screen contains all information that you need for your decision: tables for the point values of housing and clothing, your credit balance and the prices per unit. The actual point values and prices used in the experiment have been replaced with "XXX".

Shopping phase screen

Housing	Points	Clothing	Points
0	XXX	0	XXX
1	XXX	1	XXX
2	XXX	2	XXX
3	XXX	3	XXX
4	XXX	4	XXX
5	XXX	5	XXX
6	XXX	6	XXX
7	XXX	7	XXX
8	XXX	8	XXX
9	XXX	9	XXX
10	XXX	10	XXX
11	XXX	11	XXX
12	XXX	12	XXX
13	XXX	13	XXX
14	XXX	14	XXX
15	XXX	15	XXX
16	XXX	16	XXX
17	XXX	17	XXX
18	XXX	18	XXX
19	XXX	19	XXX
20	XXX	20	XXX
21	XXX	21	XXX
22	XXX	22	XXX
23	XXX	23	XXX
24	XXX	24	XXX
25	XXX	25	XXX

Your budget for this decision:

XXX Taler

Price per unit:

Housing	Clothing
XXX Taler	XXX Taler

Your decision:

Housing	Clothing

OK

In the bottom-right hand corner of the screen, you can see two input fields. After having decided how many units of housing and of clothing to buy you enter your decision in these two fields and confirm your choice by clicking on the OK-button. **After having clicked on the OK-button you cannot change your decision anymore.** Your decision will be shown again on the screen. Please write your decision on the decision sheet that was handed out with these instructions. If you click on the OK-button although you would spend more talers than you have at your disposal, an error message is displayed and you have the possibility to correct your decision.

If you have any questions please hold your hand out of the cubicle; we will then come to you.

When all participants have answered the example questions correctly, the experiment starts with the working phase. When all participants have finished the working phase, you will be presented again short instructions for the first purchasing decision on the computer screen. Also for the second purchasing decision, the screen will show short instructions. As soon as all participants have taken the second purchasing decision the computer screen shows a questionnaire. After the questionnaire,

the experiment is over.

Please answer the example questions handed out with these instructions before the experiment starts.

On-screen Instructions

Before the Working Phase

The working phase is about to start now. If you succeed in counting the correct number of zeros on four sheets within 15 minutes, you have completed the task successfully and you get 100 talers. If you do not succeed in completing the task successfully you get 10 talers instead.

Please click on the OK-button to start the working phase.

Before the First Purchasing Decision

You completed the task successfully. Your credit balance per purchasing decision is thus 50 talers.

In the following shopping phase you will make **two** purchasing decisions.

You decide how many units of housing and how many units of clothing to buy. You can read from the tables on the screen how many points you will get for your decision. If you do not spend all your credit balance, the talers not spent will be forfeited.

Before the Second Purchasing Decision

Lab Label treatment

For the second purchasing decision, you get a **housing subsidy** of **30 talers** in addition to your credit balance of 50 talers. You can spend the housing subsidy **only on housing**.

If the amount you spend on housing is lower than the amount of the housing subsidy, i.e., lower than 30 talers, the part of the subsidy that is not spent is **forfeited**.

The **housing subsidy** is the **only difference** compared to the first purchasing decision. All prices and point values remain the same.

Please note: When entering your purchasing decision, please report the **total** number of units you buy, no matter whether you paid them out of your **own credit balance** or out of the **housing subsidy**.

Lab Cash treatment

For the second purchasing decision, you get a **subsidy** of **30 talers** in addition to your credit balance of 50 talers. You can spend the subsidy on housing, on clothing or on both.

If you do not spend the whole subsidy, the part of it that is not spent is **forfeited**.

The **subsidy** is the **only difference** compared to the first purchasing decision. All prices and point values remain the same.

Please note: When entering your purchase decision, please report the **total** number of units you buy, no matter whether you paid them out of your **own credit balance** or out of the **subsidy**.

B   Moral Obligation Vignettes

Student-Support Scenario

Mr Smith is a first-year Biology student who wants to apply for Bafög.[29] When he reads up on Bafög he notices that he has to specify the income of his parents and additionally his own wealth. He recently received part of his bequest, amounting to 32 000 euro. If he declares this amount his application will be rejected. He decides to not declare the bequest in his application in order to receive Bafög anyway.

What do you think about the behavior of Mr Smith on a scale from 1 to 6, where 1 means "Not appropriate at all" and 6 "Completely appropriate"?

Unemployment-Benefit Scenario

Ms Newman has finished her studies of Law and is looking for a job. She has already found one but this position is only available in three months. She knows that she is eligible for unemployment benefit. She could easily bridge the time until the job starts since she has savings of 10 000 euro. Additionally, her parents support her with 800 euro per month until the new job starts. Ms Newman decides to claim unemployment benefit in addition, amounting to 300 euro per month.

What do you think about the behavior of Ms Newman on a scale from 1 to 6, where 1 means "Not appropriate at all" and 6 "Completely appropriate"?

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[29] "Bafög" is the student support provided by the state in Germany. The amount depends on own income, own wealth and parents' income.