Macroeconomics in the Laboratory

Three Essays about Intertemporal Decisions And One Essay about Sunspots

Dissertation

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Dedicated to my parents, my wife, and my son.

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Chapter 1

Introduction

> "Like astronomers studying the evolution of stars or biologists studying the evolution of species, macroeconomists cannot conduct controlled experiments in a laboratory." Mankiw (2007, p.3)

Laboratory experiments do not belong to the universe of macroeconomics. That was the message of Gregory Mankiw for his readers in 2007. In his textbook, which is a popular introduction into macroeconomics, Mankiw negated the existence of experimental macroeconomics right at the beginning. At first glance, his thesis seems plausible. It is impossible to put an economy into a laboratory. Generations of macroeconomics students learned little about experimental macroeconomics, if at all.

However, other textbook authors see experiments as a part of macroeconomics. Weimann and Brosig-Koch (2019) explain that experiments now belong to every field of economics, including macroeconomics. Kagel and Roth (2016) also note an awakening of experimental macroeconomics. "There has been a lot of activity since the 1995 Handbook, including significant growth in macroeconomic experiments". Even the latest version of Mankiw's textbook now says at least: "Like astronomers studying the evolution of stars or biologists studying the evolution of species, macroeconomists *usually* cannot conduct controlled experiments in a laboratory." Mankiw (2019, p.2). Thus,

according to Mankiw, macroeconomic experiments are no longer impossible, but *unusual*.

Experimental macroeconomics evolved from absent to a rarity. Duffy (2016) sees the beginning of experimental macroeconomics here: "However, I will place the origins [...] with Lucas's 1986 invitation to macroeconomists to conduct laboratory experiments to resolve macro-coordination problems that were unresolved by theory. Lucas's invitation was followed up on by Aliprantis and Plott (1992), Lim et al. (1994) and Marimon and Sunder (1993, 1994, 1995), and perhaps as the result of their interesting and influential work, over the past two decades there has been a great blossoming of research testing macroeconomic theories in the laboratory."

The emergence of experimental macroeconomics is closely linked to the changes in macroeconomic modelling. The condition for the bust of macroeconomic experiments is the micro foundation of modern macro models. Most macro models assume that individuals do not behave differently from the representative agent. This is the point of attack for experimental economists. That individual behaviour, which is supposed to be decisive for the representative agent, can be well investigated in the laboratory. "Experimentalists testing nonstrategic macroeconomic models have sometimes taken this representativeness assumption at face value, and conducted individual decision-making experiments with a macroeconomic flavor. But, as we shall see, experimentalists have also considered whether small groups of subjects interacting with one another via markets or by observing or communicating with one another might outperform individuals in tasks that macroeconomic models assign to representative agents" (Duffy, 2016).

Duffy (2016) describes the possibilities for macroeconomic experiments as follows: "1) an assessment of the micro-assumptions underlying macroeconomic models, 2) a better understanding of the dynamics of forward-looking expectations which play a critical role in macroeconomic models, 3) a means of resolving equilibrium selection (coordination) problems in environments

with multiple equilibria, 4) validation of macroeconomic model predictions for which the relevant field data are not available and 5) the impact of various macroeconomic institutions and policy interventions on individual behavior."
This dissertation does not have the aim to present a survey of experimental macroeconomics (for a good survey see Duffy (2016)). Instead, it will present four experiments, which have in common to be macroeconomic. Three experiments deal with individual decisions which are decisive for the aggregated level. They deal with the intertemporal consumption decision. The intertemporal consumption decision is the heart of many macroeconomic models. Macroeconomic fundamentals depend on how individual households save or consume their income. For example, the growth path of an economy, the economic effects of supply-side or demand-side shocks, and the impact of economic stimulus packages depend on the saving behaviour of individuals. The understanding of intertemporal preferences is a key requirement for monetary and fiscal policy. The experiments belong to the first category listed by Duffy ("an assessment of the micro-assumptions underlying macroeconomic models"). The experiment "The More You Know? – Consumption Behavior and the Communication of Economic Information" also belongs to the second category ("dynamics of forward-looking expectations"). The experiment "Preferences over Wealth: Experimental Evidence" also fits into the fourth category ("validation of macroeconomic model predictions for which the relevant field data are not available") and the experiment "Are the Poor more Impatient than the Rich? Experimental Evidence on the Effect of (Lab) Wealth on Intertemporal Preferences" also examines "the impact of [...] macroeconomic [...] policy interventions on individual behavior" (category 5).
Within the experiments "The More You Know? – Consumption Behavior and the Communication of Economic Information" and "Preferences over Wealth: Experimental Evidence", participants do not make real decisions about future consumption. These experiments last less than two hours and

are completed afterwards. Instead of making a decision over a long horizon, participants have to decide over consecutive periods. Whereat, consumption is only virtual by spending an artificial currency. To achieve external validity, the virtual consumption of artificial currency generates real monetary payments for the participants. The transformation rate of virtual consumption into real monetary payment simulates a utility function. The experimental payment corresponds to the utility which would have been realised by the chosen consumption path given the utility function. This experimental setting does not test the intertemporal preferences, but the behaviour, given the intertemporal preferences. However, the behaviour can give insights about preferences, which are not (or wrong) modelled in the utility function (as in "Preferences over Wealth: Experimental Evidence"). Hey and Dardanoni (1988) are pioneers in that kind of experimental settings.

In contrast, however, the experiment "Are the Poor more Impatient than the Rich? Experimental Evidence on the Effect of (Lab) Wealth on Intertemporal Preferences" is a real intertemporal decision. The participants have to decide on real monetary payments in the future. After leaving the laboratory, the experiment was not over. Some of the participants got money transfers months after the experimental session. A pioneering experiment doing real intertemporal decisions is presented by Thaler (1991).

The experiment "Coordination Problems Triggered by Sunspots in the Laboratory" is a coordination game. It belongs to the third of Duffy's categories ("a means of resolving equilibrium selection (coordination) problems in environments with multiple equilibria"). Many macroeconomic phenomena appear to be coordination games. Price finding, valuation of shares, trust into banks or government bonds are only a few examples. A pioneering experiment which was a coordination game with a macro interpretation is described by Ochs (1995).

This dissertation is structured as follows. Chapter 2 presents the experiment "The More You Know? – Consumption Behavior and the Communication

of Economic Information". It deals with the question of how information about future income affects intertemporal consumption decisions. There is widespread public criticism of economic forecasts because only one point estimates are communicated. Forecasters hide other useful information that would illustrate the uncertainty of the estimate, such as confidence intervals. We want to find out whether such information is used by individuals. Our research question is whether information improves decision making. Moreover, we wonder whether the framing of the source of income uncertainty will affect decision making. We vary the framing of the source of income uncertainty. We test whether that affects the decisions, even though the economic situation is unchanged. Thus, we vary the conditions in that experiment along two dimensions: information and framing. The experiment is based on the experimental design by Brown et al. (2009). As described above, participants have to split virtual money between different periods. The real income of the participants is based on a special utility function (see Deaton (1991)). Utility depends not only positively on the current consumption, but also negatively on the consumption of previous periods. The utility function is called a habit-stock utility function. It makes planning more complex and, thus, information about future income more important.

Chapter 3 is about the experiment "Preferences over Wealth: Experimental Evidence". The experiment aims to reveal preferences over wealth. Some macroeconomic models assume that people save more than needed. This means that people do not save to finance future consumption but to enjoy the savings themselves. That assumption is a possibility to explain, e.g., huge savings of the very rich (Carroll, 1998). It is also a possibility to explain the so-called forward guidance puzzle. The forward guidance puzzle refers to the phenomenon that households do not spend more, even though the central banks promise to keep interest rates low for a long time (Michaillat and Saez, 2018; Rannenberg, 2019). Whether people save intentionally more than needed, could not yet be proven in the laboratory. Our experi-

ment is kept as simple as possible, to reveal preferences and not the ability to understand a complicated experiment. In contrast to the experiment "The More You Know? – Consumption Behavior and the Communication of Economic Information", the utility function is quite simple. The transformation rate decreases by each period with the same factor. Therefore, there is no monetary incentive to save. The optimal solution is to spend every income in each period.

Chapter 4 presents the experiment "Are the Poor more Impatient than the Rich? Experimental Evidence on the Effect of (Lab) Wealth on Intertemporal Preferences". Some macroeconomic models assume that poor households have a higher intertemporal discount rate than rich households (Deaton, 1992). That helps to explain why poor households have a higher propensity to consume. The economic theorist Fisher (1930) formulated this assumption, "A poor man is more impatient than a rich man of the same personal characteristics." While the assumption is widespread, there is no evidence from the laboratory. The experiment "Are the Poor more Impatient than the Rich? Experimental Evidence on the Effect of (Lab) Wealth on Intertemporal Preferences" aims to change this. To measure the intertemporal preferences, the participants play an intertemporal multiple price list task. They have to decide ten times between a sooner lower payment and a later higher payment. The decisions give a good approximation for individual patience. To distinguish between rich and poor households, the participants get different initial amounts of show-up fees. Thus, the real wealth of participants is microscopically affected. The participants with a lower show-up fee are the poor, the participants with a higher show-up fee are the rich. Following Fisher (1930) and assuming that the poor are more impatient, they opt for the earlier payment.

Chapter 5 presents the experiment "Coordination Problems Triggered by Sunspots in the Laboratory". It aims to reveal how external signals affect a coordination process. During the Euro Crisis, the rating agencies were criti-

cized. With their forecasts, they frighten politicians and the financial market. Some speculated whether the fear triggered by rating agencies is sufficient to worsen economic conditions such that the forecasts are self-fulfilling. Can a publicly announced forecast be self-fulfilling? To test this, we let groups of three participants play a stag-hunt game. The participants have to decide between two alternatives. The first alternative brings a high payment, if (and only if) all participants choose this alternative. It brings a low payment if any of the other participants choose another alternative. The other alternative brings a medium payment regardless of what the other participants do. The similarities to the financial market are as follows. If someone trusts the financial markets, they can make a lot of money, if everyone trusts the financial markets. If someone trusts the financial markets, but not enough other people trust them, they can lose a lot of money. If someone hoards their money at home, there is no return, but they don't have to fear losing it either. In that environment, we install a mechanism that randomly publishes forecasts. The forecast is either "the majority will choose alternative A" or "the majority will choose alternative B". Rational agents should not be affected by the announcements. Earlier experiments show that announcements can help to improve and accelerate coordination. However, we do wonder whether announcements may worsen the coordination.

Chapter 6 is the last chapter. It draws the conclusions of all presented experiments.

Chapter 2

The More You Know? – Consumption Behavior and the Communication of Economic Information

Working Paper

The more you know? Consumption behavior and the communication of economic information

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Abstract

Splitting available income between saving and consumption is one of the most central economic decisions households have to make. We use a laboratory experiment to shed more light on this decision. Based on a buffer stock saving model, we study the impact of information about the likelihood of future income shocks and different framings of the source of this shock on consumption. Overall, the model performs quite well in predicting the average consumption level in our experiment. Surprisingly, providing additional information about the likelihood of future income shocks tends to even increase the deviation from the optimal consumption path, if at all. Looking at individual behavioral patterns reveals that there is a significant share of participants, who show a high correlation between their individual consumption and current income, which contradicts the theoretical prediction. Providing additional information about the likelihood of future income shocks reduces the share of rule-of-thumb consumers.

Keywords: consumption, information, framing, individual behavior, laboratory experiment

JEL classifications: C91, D91, E21

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1 Introduction

Saving money for the future is among the most central economic decisions private households have to make. Consequently, the decision how to split available income between saving and consumption has always been of major interest in economic research. Early theoretical work on consumption and saving faced the problem that a realistic representation of the decision to save (e.g., through realistic preference models or stochastic income models) is very difficult to solve analytically. One way to deal with this is to use a more tractable utility function. Following this approach and implementing quadratic utility functions, the certainty equivalent model was developed (Deaton (1992)). The results obtained with this model are not able to explain several empirical observations regarding savings, though (see, e.g., Carroll (2001)). Another way is to solve the complicated model numerically and not analytically. This approach resulted in the buffer stock savings model (Deaton (1991), Zeldes (1989)), which predicts that consumers build up a "buffer stock" of savings to prepare themselves for future negative income shocks. With regard to this model, Deaton (1991, p. 1223) particularly notes that "the behavior of saving and asset accumulation is quite sensitive to what consumers believe about the stochastic process generating their incomes". The buffer stock savings model forms the basis for our study. We particularly aim to investigate how the type of information subjects receive about the stochastic process generating their income and the framing of this process influence the individual consumption–saving decision.[1]

Our research is based on a series of laboratory experiments. Using the experimental method has the advantage that it allows for a high degree of control over the decision environment. In the laboratory ceteris paribus changes of parameters can be implemented and their effects on individual behavior can be directly observed. External aspects like habit stock formation can be isolated and, if behavior changes, this variation can be attributed to the modified parameter (e.g., the information about stochastic income). Accordingly, laboratory experiments provide a suitable tool to test the predictions of economic models on optimal consumption. In a pioneering experiment, Hey and Dardanoni (1988) find that subjects were unable to consume and save optimally, but that the comparative static predictions of the tested model on optimal consumption under uncertainty still holds. This result is confirmed by Carbone and Hey (2004), who also report significant overreactions to changes in risk. The experiment by Brown et al. (2009) introduces the possibility of learning on how to save optimally. They observe that both, social learning (by seeing the decisions of other participants) and individual learning (by repeating the sequence of decisions seven times), improve the quality of decision making. Carbone and Duffy (2014) and Carbone and Infante (2015) analyze whether participants benefit from the information about other people's decisions or joint work. In contrast to Brown et al. (2009), they find that social information can even worsen the

[1] While the comprehensive survey by Browning and Lusardi (1996) lists a total of nine possible motives on why people save, including the precautionary, the life–cycle, the intertemporal substitution, and the enterprise motive, it is generally agreed that the uncertainty surrounding income (and expenditures) is one of the main determinants.

consumption decision. Carbone and Infante (2014) disentangle the role of certainty, risk, and ambiguity in intertemporal consumption. In all of their treatments they observe that participants do not consume optimally, even when faced with an unusually short lifecycle. Still, deviations in the ambiguity treatment are slightly greater than those in the other treatments. Geiger et al. (2016) experimentally test how information about current and future fiscal policies affects intertemporal consumption. They find that fiscal consolidations that occur in an unsustainable fiscal environment exert less contractionary effects on consumption. But participants appear to be reluctant to reduce savings sufficiently after the consolidation occurs, even when the fiscal situation becomes sustainable. While the link between information about future income and consumption is rather indirect in Geiger et al. (2016), the information provided in our study directly relates to the stochastic process generating participants' income.

Despite research on the consumption-saving decision, there are economic experiments focusing more generally on the role of information in expectation formation. Hommes (2011) surveys the literature on so-called learning-how-to-forecast-experiments, which provide a highly controlled setting to test different theories of expectations and learning. He concludes that the rational expectation forming individual is rather the exception than the norm. In line with this conclusion we observe a significant share of participants in our experiment, who do not smooth their consumption, but whose consumption is influenced by their current income. In reference to the theoretical work of Mankiw (2000) and Gali et al. (2009), we label these participants as "rule-of-thumb" consumers.[2] Despite the erratic and costly changes in individual consumption decisions, we observe that these participants' average level of consumption is close to, but higher than the optimum. In contrast, those participants, who are able to smooth their consumption, tend to over-save. Introducing additional information about the income generating process significantly reduces the share of rule-of-thumb consumers and increases the share of those who smooth their consumption. As both groups of participants reveal a similar absolute deviation from the optimal consumption path, the overall effect of providing additional information on this deviation is rather little, though.

2 Experimental Design

2.1 Model

We use a buffer stock saving model, which largely follows the design in Brown et al. (2009).[3] They in turn base their parameters on empirical evidence reported by Carroll et al. (2000). In our design as well as in Brown et al. (2009), consumers live for 30 periods. In each of these periods, consumers have to

[2] In his intertemporal consumption experiment on debt aversion, Meissner (2016) also identifies rules of thumb in subjects consumption decisions, but his classifications differ from ours.

[3] Meissner (2016) and Meissner and Rostam-Afschar (2017) use a similar design. In contrast to our study they allow for borrowing (to analyze the role of debt) and skip the habit stock in the utility function.

allocate their cash-on-hand (i.e., the sum of previous cash-on-hand and new income) between saving and consumption. The income in period t is the product of the fixed income received in this period and the stochastic income shock η_t. The stochastic income shock is chosen to be lognormally distributed with a standard deviation of $\sigma^2 = 1$: $log\eta \backsim N(-0.5, 1)$. This results in a mean income shock of $E[\eta] = 1$. The growth rate of the fixed income is set to be constant at 5% per period. Note that there is no interest rate and discount factor, and no borrowing or investment.

Consumers are assumed to have a per-period utility function, which is a CRRA (constant relative risk aversion) one:

$$U(C_t, H_{t-1}) = \frac{(\frac{C_t}{H_{t-1}^{\gamma}})^{1-\rho}}{1-\rho}. \tag{1}$$

Per-period utility $U(C_t, H_{t-1})$ depends on consumption in this period (C_t) and the habit stock from period $t-1$ (H_{t-1}). ρ is the coefficient of relative risk aversion and γ is a parameter indicating the importance of the habit stock variable. Following the parameter specification used by Brown et al. (2009), the utility function is adapted to include some scaling parameters, and parameters were chosen such that $\rho = 3$ and $\gamma = 0.6$. The following per-period utility function is therefore used in the experiment:

$$U(C_t, H_{t-1}) = 40 + 750\frac{(\frac{C_t+2.7}{H_{t-1}^{0.6}})^{1-3}}{1-3}. \tag{2}$$

A special feature of the model is the habit stock (H_t), which is a function of earlier consumption and which decreases the utility of current consumption. The inclusion of a habit stock into the calculation makes early consumption less attractive. As a consequence, early saving becomes more attractive even above the need to insure oneself against future negative income shocks. The habit stock develops according to $H_t = (1-\sigma)H_{t-1} + C_t$, where σ is the habit stock's depreciation rate. This rate indicates how persistent the habit stock is. The higher σ, the slower the habit stock decreases back to zero and the more important is it to incorporate it into one's calculation. As in Brown et al. (2009) σ is set to 0.3, which signifies a slow decrease of the habit stock over time. The starting habit H_0 is set to 10.

Consumers are assumed to maximize their expected lifetime utility by choosing the stream of consumption $\tilde{C_S}$ in each period t:

$$E_t[\sum_{s=t}^{T} U(\tilde{C_s}, \tilde{H_{s-1}})] \tag{3}$$

In our experiment, subjects are paid according to their lifetime utility. The specific details of the experimental procedure are provided below.

2.2 Experimental Procedure

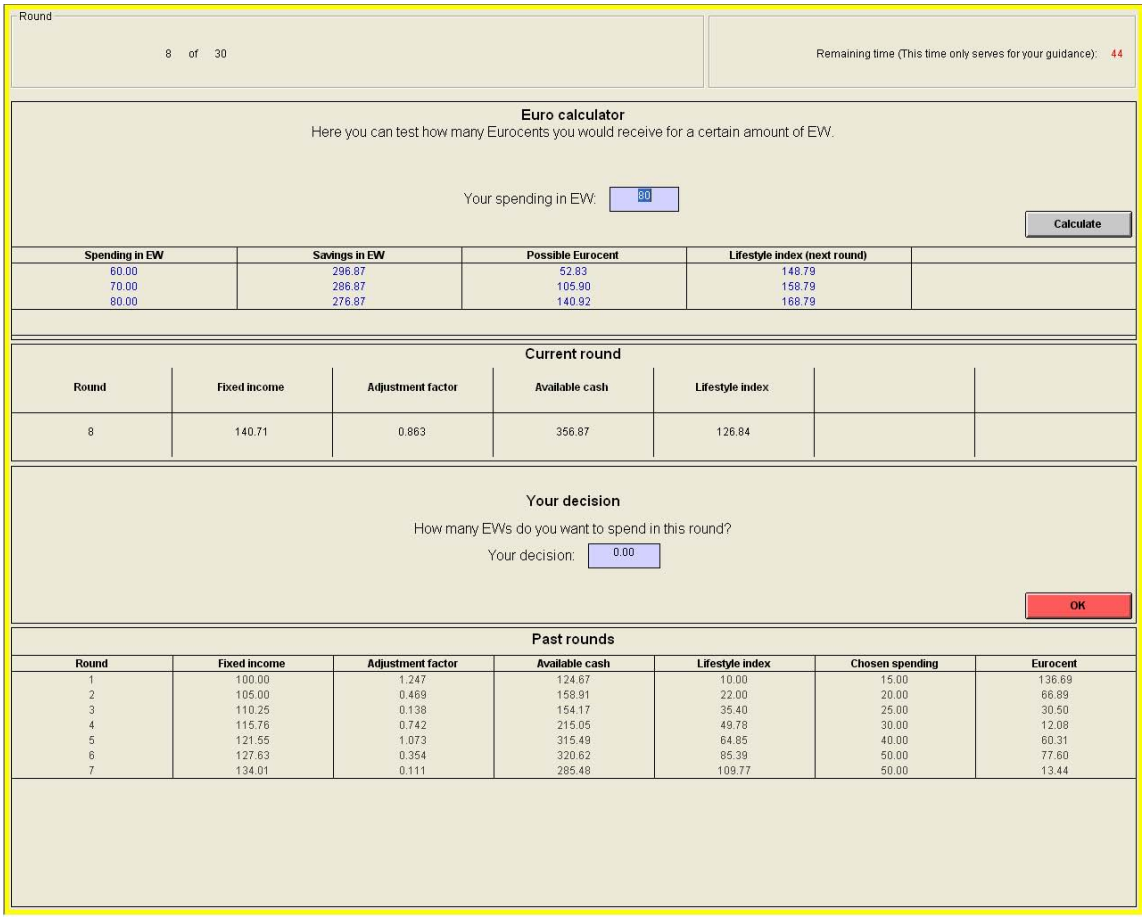
At the beginning of the experiment, subjects received detailed instructions. After reading the instructions, they had to answer eight comprehension questions about the decision task.[4] In total, subjects played 30 periods. The first period only started after all subjects had correctly answered all comprehension questions. During the experiment, possible questions asked by participants were always answered in private.

To avoid potential influences that could result from different previous knowledge of participants, technical terms from economics were avoided in the description of the decision task. Consequently, the income shock was labeled as "adjustment factor"and the habit stock was labeled as "lifestyle index". The money in the experiment was given in experimental currency units (called "EW" in the experiment due to the German word "Experimentwährung"). The amount of EW they chose to spend in a period was converted to actual Eurocents. The conversion factor corresponded to the utility function presented above. The instructions included a large table showing possible results of different spending decisions. Subjects also received the formula and a table indicating the development of the habit stock. In each period, participants were informed about the current income, their available cash-on-hand, and their habit stock. They further received the information that their income was subject to a random adjustment factor and that this adjustment factor was drawn independently in each period. To illustrate this, participants were provided with three example draws of the income shock over all 30 periods. Subjects could use a Euro calculator to compute what a certain amount of spending would mean in terms of Euro payoff. The Euro calculator was implemented in the decision screen (see Figure 2). Before each decision, subjects had to state a belief about this period's adjustment factor.

Our treatment variations include the specifics of information subjects receive about the random process determining their income (Info treatments) and the framing of the source of income shocks, i.e., the framing of the random adjustment factor (Frame treatments). The *Base* treatment was already described above and followed the design used by Brown et al. (2009). In addition to the *Base* treatment we implemented four Info treatments. In treatment *LowInfo*, subjects did not receive any example draws of the income shock. Treatments *PointInfo* and *IntervallInfo* added a point estimate and an intervall estimate of the random variable to the three example draws, respectively. These two treatments should lighten the discussion about the best presentation of economic forecasts. The *HighInfo* treatment implemented the most detailed information concerning the income shock. It added the density function of the random variable, the 90 percent confidence interval for the random variable, and a statement that the income shock is always larger than zero, but will be smaller than one most of the time. The three framings used for the source of income shocks were changes in tax policy (*TaxFrame*), changes in unemployment

[4]See the Appendix for a full set of instructions.

Figure 1: Screenshot



(*JobFrame*), or the general economic development (*EconFrame*). The Frame treatments had identical information as the *Base* treatment. See Table 1 for an overview of the different treatments.

The experiment was conducted computer-based using the software z-Tree (Fischbacher, 2007). It took place at the "Essen Laboratory for Experimental Economics" (elfe) at the University of Duisburg-Essen, Germany. Participants were undergraduate students from different departments of the University of Duisburg-Essen. They were recruited via the web-based online recruitment system ORSEE (Greiner, 2015) and the attached subject pool. In total, 8 sessions with up to 23 subjects each were conducted, leading to a total of 151 participants. The experiment lasted about 150 minutes and average payoff including the show-up fee of EUR 5.00 was EUR 27.40 (minimum EUR 0.10, maximum EUR 38.80).[5] Subjects were paid out one after the other to preserve anonymity.

[5]If the decisions made by a participant would lead to a negative payoff which exceeds the show-up fee, the participant was warned by a pop-up screen and could change his or her decision. 59 participants saw the pop-up warning and changed the decision in at least one period. 45 participants saw the pop-up warning and rejected to change the decision in at least one period.

Table 1: Experimental Treatments

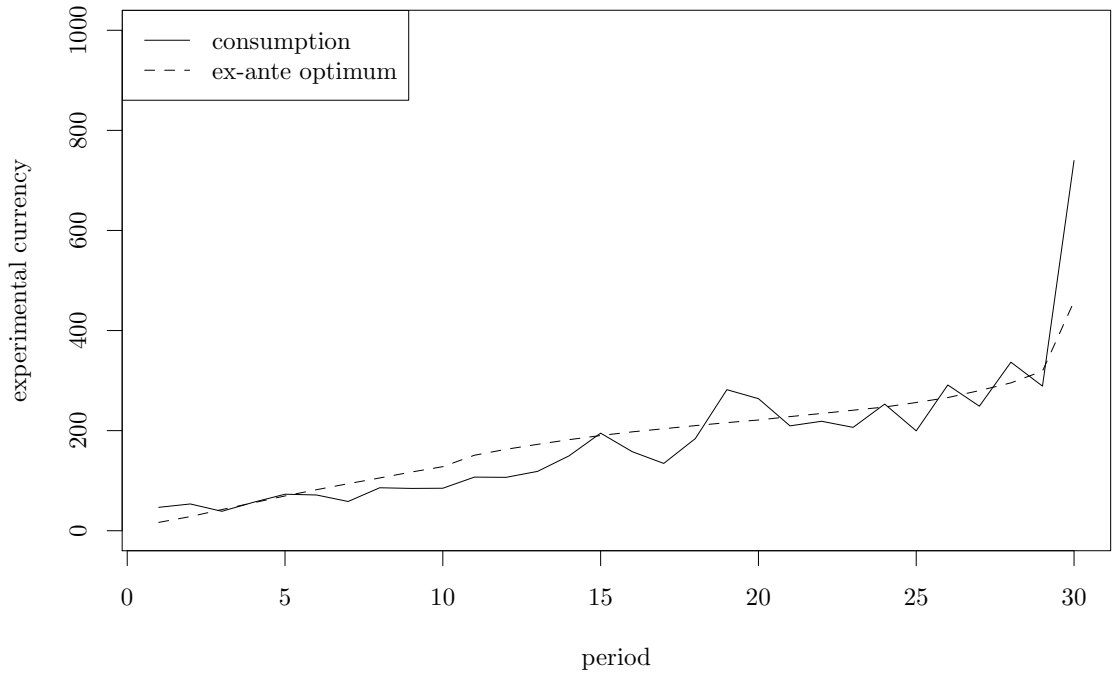
Treatment	# of participants (# of ind. obs.)
LowInfo	17
Base	23
PointInfo	18
IntervallInfo	20
HighInfo	16
EconFrame	18
TaxFrame	20
JobFrame	19

3 Results

On average, the participants' consumption path is surprisingly close to the optimum. We still observe small deviations from the optimum in all treatments. Figure 2 illustrates the average consumption path of the *Base* treatment and the ex-ante optimal consumption path (i.e., the path that would be chosen by a rational and income-maximizing participant who is ex ante fully informed about the specific income shocks drawn in each of the 30 periods). While participants' average consumption is too high at the beginning, it is too low in later periods. Only at the end of the experiment, most participants start to spend everything they have for consumption. The time trend revealed by participants in our experiment is similar to those observed by Carbone and Hey (2004), Ballinger et al. (2003), and Brown et al. (2009).

In order to compare consumption decisions between treatments, we calculate the **i**ndividual **ave**rage **abs**olute **d**eviation from the individual dynamic conditional optimum divided by fixed income (**i-ave-abs-d**) following Brown et al. (2009). For this, we compute the individual dynamic conditional optimum, given future income shocks, cash on hand, and the current habit stock. Then, we calculate the individual absolute deviation of the actually chosen consumption level from this conditional optimum. Finally, we divide the result by the fixed income to eliminate the given growth trend and take the average over all 30 periods. We find that the i-ave-abs-d is significantly lower in the *LowInfo* treatment than in the *HighInfo* treatment ($p = 0.049$, two-sided exact Mann-Whitney-U test). Apparently, participants deviate more from the optimal path when receiving more specific information about the random process determining their income (see Figure 3). There are no significant differences between the Frame treatments and the *LowInfo* treatment (as well as between the Frame treatments and the *Base* treatment), however. That more information rather tends to induce a higher than a lower deviation from the optimal path, is also supported when the comparisons between treatments are based on the per-period **i**ndividual **abs**olute **d**eviation from the individual dynamic conditional optimum divided by fixed income (**i-abs-d**). Except

Figure 2: Average Consumption Base Treatment



for three comparisons in period 2, the i-abs-d is (weakly) significantly lower in the *LowInfo* treatment than in other treatments, if at all.

3.1 Individual Behavior

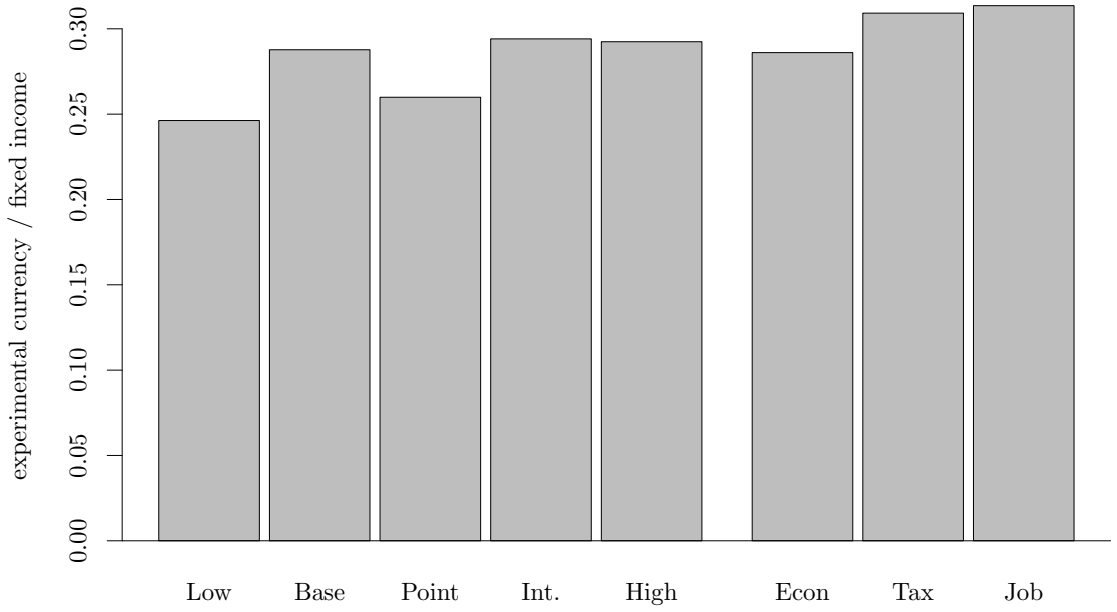
Using the i-abs-d as dependent variable, we also run linear mixed-effects regressions fitted by restricted maximum likelihood and clustered on an individual level. The results of our three models are summarized in Table 2. In model (1), we estimate the effects of the different treatments controlling for time trends only. As our previous analyzes revealed noticeable differences between *LowInfo* and the other treatments only, the treatment variables are dummies compared to *LowInfo*. Model (2) ads individual characteristics as independent variables: male (a dummy for male participants), age (the age in years), semester (the number of semesters), school (the final grade of high school), econ (a dummy for students of economics and business studies). In model (3) we additionally include the self-reported personality traits, patience, impulse, and risk.[6] Since the variables risk and impulse are significantly correlated with gender, we also include the interaction terms male:impulse and male:risk into model (3).

[6]The values correspond to the answers provided to the following questions: How do you personally assess yourself: Are you generally an impatient person or do you always have a lot of patience? (from 1="very impatient" to 11="very patient"); How do you assess yourself personally: Are you generally a person who thinks and thinks long before he acts, i.e. is not impulsive at all? Or are you a person who acts without much thought, i.e. is very impulsive? (from 1="not impulsive at all" to 11="very impulsive"); How do you personally assess yourself: Are you generally a person who is willing to take risks or do you try to avoid risks? (from 1="not willing to take risks at all" to 11="very willing to take risks"). We took these

Table 2: Linear Mixed-Effects Model

	Dependent variable:		
	Individual absolute deviation from conditional optimum		
	(1)	(2)	(3)
Base	0.041	0.041	0.046
	(0.035)	(0.036)	(0.036)
PointInfo	0.014	0.007	0.018
	(0.037)	(0.037)	(0.037)
IntervallInfo	0.048	0.043	0.055
	(0.036)	(0.036)	(0.036)
HighInfo	0.046	0.043	0.059
	(0.038)	(0.039)	(0.038)
EconFrame	0.040	0.033	0.060
	(0.037)	(0.037)	(0.037)
TaxFrame	0.063^{*}	0.057	0.057
	(0.036)	(0.037)	(0.036)
JobFrame	0.067^{*}	0.062^{*}	0.073^{**}
	(0.037)	(0.037)	(0.037)
period	-0.012^{***}	-0.012^{***}	-0.012^{***}
	(0.002)	(0.002)	(0.002)
$period^2$	0.001^{***}	0.001^{***}	0.001^{***}
	(0.0001)	(0.0001)	(0.0001)
male		0.014	−0.046
		(0.019)	(0.057)
age		−0.002	−0.002
		(0.002)	(0.002)
semester		0.001	0.002
		(0.002)	(0.002)
school		0.002	0.001
		(0.001)	(0.001)
econ		−0.020	−0.020
		(0.018)	(0.018)
patience			-0.006^{*}
			(0.003)
impulse			-0.012^{*}
			(0.006)
risk			0.006
			(0.006)
male:impulse			0.025^{***}
			(0.009)
male:risk			-0.019^{**}
			(0.008)
Constant	0.225^{***}	0.215^{***}	0.291^{***}
	(0.031)	(0.044)	(0.069)
Observations	4,530	4,530	4,530
Log Likelihood	−1,726.524	−1,746.836	−1,761.603
Akaike Inf. Crit.	3,477.049	3,527.672	3,567.207
Bayesian Inf. Crit.	3,554.044	3,636.730	3,708.316
Note:			*p<0.1; **p<0.05; ***p<0.01

Figure 3: Average Absolute Deviation from the Individual Dynamic Conditional Optimum Divided by Fixed Income



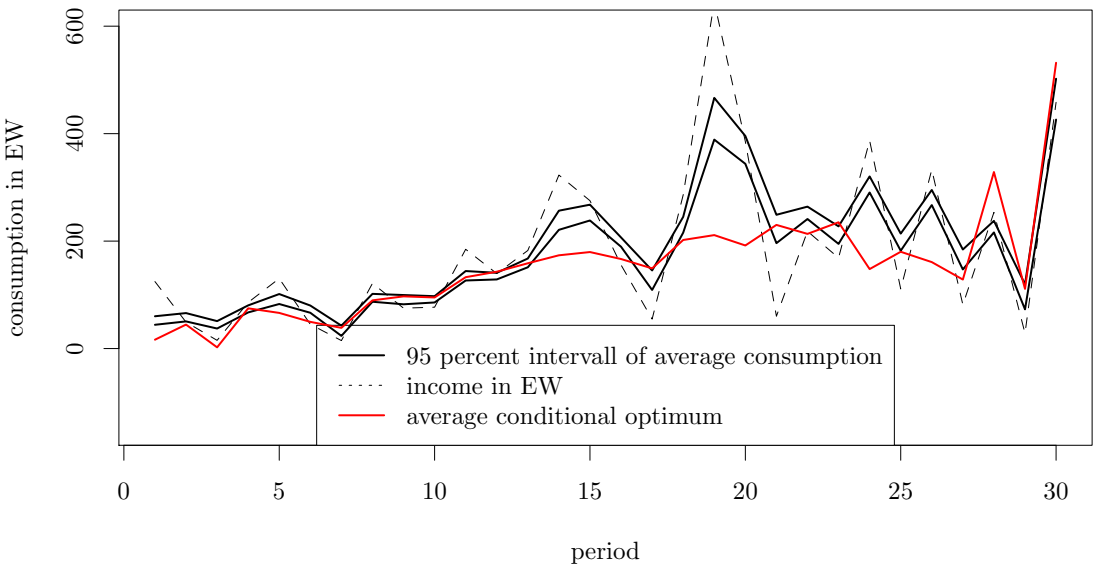
Model (1) reveals that the *TaxFrame* treatment and the *JobFrame* treatment slightly increase the i-abs-d compared to the *LowInfo* treatment. Controlling for individual characteristics in models (2) and (3) eliminates the slightly significant effect of the *TaxFrame* treatment. In model (3) we find some slightly negative effects of patience as well as impulsiveness. Regarding the latter observation, the interaction term demonstrates that for males more impulsive behavior has a significantly positive effect on i-abs-d, however. Moreover, for males more risk aversion leads to a significantly lower i-abs-d. Note that the effects observed in models (2) and (3) should be interpreted with caution. Including more variables in these models does not increase the informative value of the models, as the Akaike Information Criterion as well as the Bayesian Information Criterion are higher in models (2) and (3) as in model (1).

3.2 Rule-of-thumb Consumers

In order to explain the observation that more information or a more specific frame has rather weak and positive effects, if it has an effect at all, we look at individual behavioral patterns in more detail. We find that a significant share of participants responds strongly to the income shock, although it should not affect consumption decisions. In contrast, other participants show no such a response, i.e. they tend to smooth their consumption. Referring to the theoretical work of Mankiw (2000) and Gali et al. (2009), we label

questions from the German Socio-Economic Panel (SOEP).

Figure 4: Average Rule-of-Thumb Consumption in EW

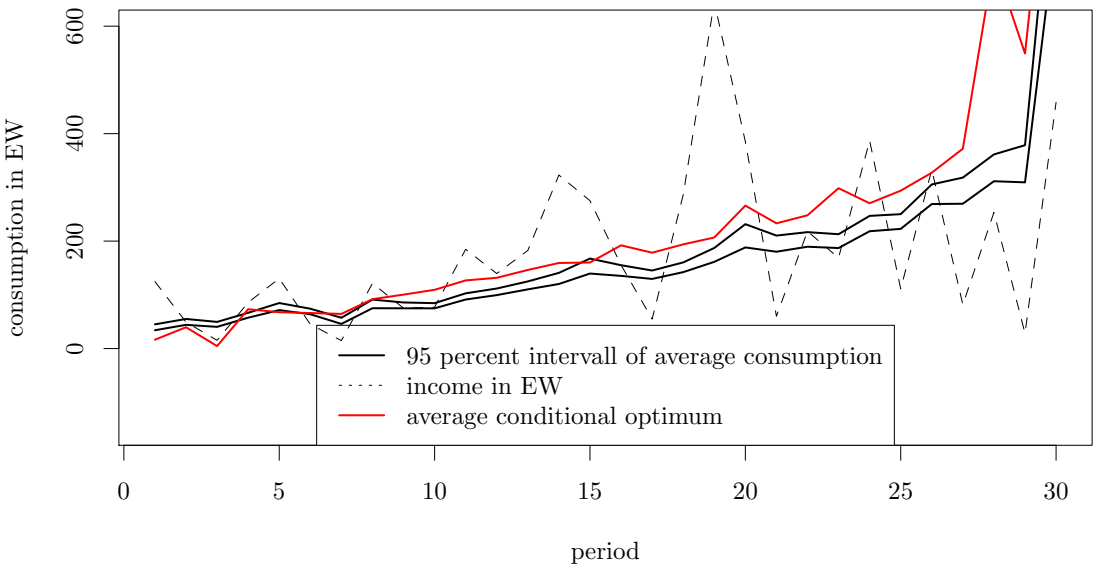


all those participants as "rule-of-thumb" consumers, whose correlation coefficient between consumption and income shock is above 0.5. Overall, 32.98 percent of our participants are classified as rule-of-thumb consumers. Figure 4 illustrates the 95 percent interval of the average consumption paths of the "rule-of-thumb" consumers. Figure 5 shows the 95 percent interval of the average consumption paths of the other participants. As an orientation, the actual income participants receive in each period is marked with the thin dotted line. Note that this income is the same for all participants.

Comparing rule-of-thumb consumers with those who rather smooth their consumption over time reveals that the former earn, on average, significantly less than the latter ($p = 0.005$, two-sided exact Mann-Whitney-U test). This is not surprising as the abrupt shifting of the consumption level is costly. Figures 4 and 5 further reveal that non-rule-of-thumb-consumers still consume too little in many periods, while rule-of-thumb consumers tend to consume too much. In particular, the **i**ndividual **ave**rage **d**eviation from the individual dynamic conditional optimum divided by fixed income (**i-ave-d**) is below zero for most of the"smoothers"(83%) and it is above zero for most of the rule-of-thumb consumers (88%). Accordingly, comparing the i-ave-d revealed by the two groups we find a significant difference ($p = 0.00$, two-sided exact Mann-Whitney-U test). The difference with regard to the i-ave-abs-d is not significant between the two groups, though ($p = 0.14$, two-sided exact Mann-Whitney-U test).

When comparing the share of rule-of-thumb consumers between our treatments (see Table 3), we

Figure 5: Average Not-Rule-of-Thumb Consumption in EW



observe differences that are consistent with the weak treatment differences regarding the i-ave-abs-d reported in the last section. In particular, the share of rule-of-thumb consumers seems to decrease with a high amount of information or with a job frame of the income shock. Applying a two-tailed Fisher-test reveals slightly significant differences between the *LowInfo* treatment and the *HighInfo* treatment ($p = 0.07$) and significant differences between the *LowInfo* treatment and the *JobFrame* treatment ($p = 0.03$). Similarly, comparing the individual correlation coefficients between income and consumption between treatments reveals significant differences between the *LowInfo* treatment and the *HighInfo* treatment ($p = 0.028$, two-sided exact Mann-Whitney-U test) and the *LowInfo* treatment and the *JobFrame* treatment ($p = 0.029$), and slightly significant differences between the *LowInfo* treatment and the *EconFrame* treatment ($p = 0.096$).

These observations are supported by a logit estimation using a dummy-variable for being a rule-of-thumb consumer as dependent variable (see Table 4). The independent variables are the same as in the linear-mixed-effects regressions presented in the last section. In all three models we find that a high level of information and a job frame (slightly) significantly decreases the likelihood of being a rule-of-thumb consumer. Models (2) and (3) additionally reveal that male participants are (somewhat) less likely to be rule-of-thumb consumers. Surprisingly, in model (3) we find that self-reported patience slightly significantly increases the likelihood of being a rule-of-thumb-consumer. However, according to the Akaike

Table 3: Share of Rule-of-Thumb Consumers

Treatment	Rule-of-thumb consumers	Other consumers	Total
LowInfo	9 (53 %)	8 (47 %)	17
Base treatment	8 (35 %)	15 (65 %)	23
PointInfo	5 (28 %)	13 (72 %)	18
IntervallInfo	6 (30 %)	14 (70 %)	20
HighInfo	3 (19 %)	13 (81 %)	16
EconFrame	5 (28 %)	13 (72 %)	18
TaxFrame	6 (30 %)	14 (70 %)	20
JobFrame	3 (16 %)	16 (84 %)	19

Information Criterion model (2), which does not include this control variable, has the highest informative value.

4 Conclusion

We use a laboratory experiment to analyze the effect of different amounts of information about the likelihood of future income shocks and different framings of the source of this shock on consumption behavior. Overall, more information tends to increase the absolute deviation from the optimal consumption path, if it has an effect at all. In order to explain the rather little effect of information, we look at individual behavioral patterns. In all treatments, we find a significant share of participants who respond strongly to the income shock, although this shock should not affect behavior. We label these participants as rule-of-thumb consumers. The share of rule-of-thumb consumers is significantly reduced with a high amount of information as well as with using a job market frame for the source of the income shock. As such, our experiment reveals that certain kinds of information help to reduce a behavioral pattern which is costly for participants. Still, since both non-rule-of-thumb consumers as well as rule-of-thumb consumers deviate from the optimal consumption path (though in different directions), the overall effect of information on the absolute deviation from optimal consumption is rather weak.

One interpretation of our results is that providing additional information increases participants' caution. While this increased caution might help people to smooth their consumption path, it also tends to increase over-saving. Note that Carbone and Infante (2014) find that ambiguity increases over-saving. Given our interpretation this might be due to the fact that people are more cautious when being faced with ambiguity.

Table 4: Logit Regression

	Dependent variable:		
	Dummy for rule-of-thumb-consumption		
	(1)	(2)	(3)
Base	−0.746	−1.046	−0.920
	(0.654)	(0.743)	(0.774)
PointInfo	−1.073	−1.101	−0.774
	(0.716)	(0.783)	(0.804)
IntervallInfo	−0.965	−0.966	−0.723
	(0.689)	(0.760)	(0.775)
HighInfo	−1.584**	−1.719**	−1.707*
	(0.804)	(0.857)	(0.895)
EconFrame	−1.073	−1.207	−1.085
	(0.716)	(0.791)	(0.827)
TaxFrame	−0.965	−1.019	−0.909
	(0.689)	(0.752)	(0.767)
JobFrame	−1.792**	−1.969**	−1.721**
	(0.795)	(0.857)	(0.873)
male		−1.773***	−2.770*
		(0.444)	(1.423)
age		−0.035	−0.013
		(0.045)	(0.047)
semester		−0.056	−0.054
		(0.049)	(0.052)
school		−0.005	−0.019
		(0.033)	(0.034)
econ		−0.062	0.007
		(0.407)	(0.419)
patience			0.147*
			(0.083)
impulse			−0.046
			(0.132)
risk			0.108
			(0.126)
male:impulse			0.282
			(0.233)
male:risk			−0.137
			(0.205)
Constant	0.118	1.704*	0.460
	(0.486)	(0.900)	(1.481)
Observations	151	151	151
Log Likelihood	−88.327	−77.963	−75.556
Akaike Inf. Crit.	192.655	181.927	187.111
Bayesian Inf. Crit.	216.793	221.1512	241.4224
Note:			*p<0.1; **p<0.05; ***p<0.01

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A Instructions[7]

Parts of the instructions that were exclusively available for one or more treatments are marked with square brackets and the corresponding treatment names. [for example]^{example}

Welcome to the experiment!

You are participating at a study of decision making behavior in the context of experimental economics. During the study you and the other participants will be asked to make decisions. You can earn money with this study. How much money you earn is dependent on the course of the experiment. You will receive detailed instructions about this in the following. All participants are paid in cash directly after the experiment one by one. To assure this, please remain seated after the experiment until your cabin number is called.

Should you have any questions before the start of the experiment, please ask an employee of the laboratory. He will come to your place and help you. **Any communication with the other participants during the experiment is only allowed when explicitly prompted; breaking this rule will lead to an immediate exclusion from the experiment.**

[7] These instructions are translated from the original German instructions.

Instructions

The experiment consists of **thirty rounds**. In each of these rounds you have to decide how to split your available money between **saving** and **spending**. The current round is displayed at the top of the screen. Your money is denoted in the unit **experimental currency (EW)**.

Available money

Your **available money** consists of two parts: Your **savings** from earlier rounds and your **current income**. It is denoted in EW.

Available money = Savings + current income

Current income

In each round, you receive a **current income**. This current income also consists of two parts: a **fixed income** and a **variable adjustment factor**.

Current income = Fixed income x variable adjustment factor

The fixed income is 100 EW in the first round and increases by 5% in each round afterwards. In the second round your fixed income is therefore 105 EW, in the third round 110.25 EW etc.

Variable adjustment factor

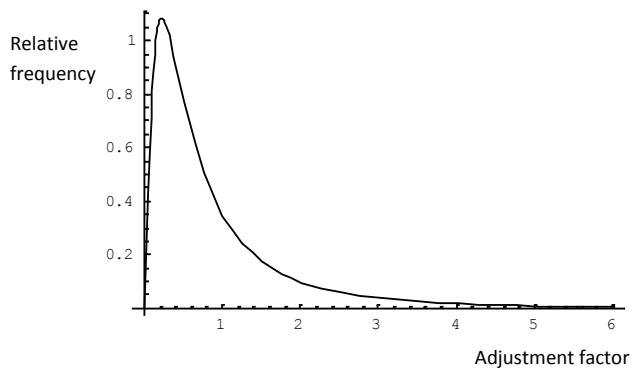
The adjustment factor is determined using a probability distribution, therefore it is determined **randomly**. The adjustment factors are determined independently in each round. A certain round's adjustment factor therefore has no influence on the next round's adjustment factor.

[The adjustment factor is variable, because in each round there is a chance to become **unemployed**. A low adjustment factor - and the resulting low current income - therefore represents high unemployment.]^{JobFrame}

[The adjustment factor is variable, because the **overall economic situation** is taken into account in each round. A low adjustment factor - and the resulting low current income - therefore represents a bad economic situation.]^{EconFrame}

[The adjustment factor is variable, because in each round the **tax policy** can be changed. A low adjustment factor - and the resulting low current income - therefore represents tax policy which puts high taxes on income.]^{TaxFrame}

[The probability distribution is represented by the following density function:



The x-axis shows all possible adjustment factors; the y-axis shows the relative frequency of each value. The adjustment factor is therefore always larger than 0, but will be smaller that 1 in most cases. In about 10% of all cases the adjustment factor will be smaller than 0.168. In about 10% of all cases it will be larger than 2.185. In ca. 80% of all cases the adjustment factor will be between 0.168 and 2.185.]^{HighInfo} [The following table shows three possible sequences of adjustment factors, which have been generated with this probability distribution. These possible courses are not used in the experiment and only serve as examples.

	Sequence A	**Sequence B**	**Sequence C**
Round	Adjustment factor	Adjustment factor	Adjustment factor
1	1.364	0.845	0.624
2	0.461	2.464	2.660
3	0.498	0.403	2.643
4	0.223	0.199	1.298
5	0.323	0.413	0.840
6	0.108	0.296	0.389
7	0.283	0.199	0.530
8	0.588	0.926	2.592
9	4.793	1.989	0.599
10	0.780	1.601	1.246
11	2.721	0.230	0.674
12	0.334	1.270	0.159
13	2.203	0.715	1.586
14	1.363	0.404	0.129
15	0.289	0.100	0.471
16	0.194	0.170	0.309
17	0.369	0.426	0.364
18	1.296	0.604	0.703
19	0.256	0.248	1.120
20	0.308	1.033	0.219
21	0.767	1.441	0.780
22	0.671	0.910	0.049
23	0.578	0.198	0.486
24	0.956	1.665	0.446
25	2.000	1.636	0.265
26	1.782	0.174	0.549
27	0.140	0.482	0.276
28	0.384	0.342	0.406
29	0.087	0.929	0.457
30	1.692	1.625	0.367

]Base, JobFrame, EconFrame, TaxFrame, PointInfo, IntervallInfo, and HighInfo

[The expected value of the adjustment factor is 1. When many adjustment factors are determined randomly, the average of their values will be 1.][PointInfo, IntervallInfo, and HighInfo]

[In addition, in about 10% of all cases the adjustment factor will be smaller than 0.168. In about 10% of all cases it will be larger than 2.185. In ca. 80% of all cases the adjustment factor will be between 0.168 and 2.185.][IntervallInfo and HighInfo]

Your estimation of the adjustment factor

Before your decision about saving and spending you have to estimate in every round, which adjustment factor will be **determined in the current round**. Please enter your estimation in the field "Estimation" on the screen.

Information after each round

In each round, a table shows savings, fixed income, adjustment factor, and the resulting available money. The values of the past rounds are also displayed in the table.

Your decision

Please enter your **decision** how much of your available money you want to spend in the current round (and with it indirectly how much you want to save) in the field "Your decision". The amount of EW which you **spend** in each round is converted to **Eurocent**.

The lifestyle index

For **testing purposes**, you can check how many Eurocents you receive for a certain amount of EW with the Euro calculator on the screen. The conversion from EW to Eurocents is influenced by your **lifestyle index**. The **higher** the lifestyle index, **the less** Eurocent you receive for a certain amount of EW you spend. The appendix of these instructions contains a **Eurocent conversion table (Table 1)** which shows how spending is converted to Eurocent. The following table is an excerpt of this conversion table:

Spending (in EW) \ Lifestyle index	10	20	50	100	150	200	250	300
5	-391.57	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00
10	20.48	-290.27	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00
20	185.03	87.76	-257.20	-400.00	-400.00	-400.00	-400.00	-400.00
40	238.81	211.32	113.83	-75.81	-286.26	-400.00	-400.00	-400.00
60	250.17	237.42	192.21	104.26	6.65	-97.80	-207.67	-322.04
80	254.35	247.02	221.03	170.48	114.37	54.33	-8.82	-74.56
100	256.34	251.59	234.73	201.95	165.57	126.64	85.69	43.06
120	257.43	254.10	242.30	219.33	193.84	166.57	137.88	108.01
140	258.10	255.64	246.91	229.93	211.09	190.92	169.71	147.63
160	258.54	256.65	249.93	236.87	222.37	206.86	190.55	173.56
180	258.84	257.34	252.02	241.66	230.16	217.86	204.92	191.45
200	259.06	257.84	253.51	245.10	235.76	225.76	215.25	204.31
220	259.22	258.21	254.63	247.65	239.92	231.64	222.93	213.86
240	259.34	258.49	255.48	249.61	243.09	236.12	228.79	221.15
260	259.44	258.71	256.14	251.13	245.57	239.62	233.36	226.84

For example, if you decide to spend 100 EW in a round, you will receive 256.34 Eurocent if your lifestyle-index is 10, but only 201.95 Eurocent if your lifestyle-index is 100.

Development of the lifestyle index

In each round, the lifestyle index is calculated as follows:

Lifestyle index (current round) = 0.7 x lifestyle index (last round) + Spending (last round)

Therefore, the **more** you spend in each round, the **higher** the lifestyle index gets. At the end of these instructions you find a **lifestyle table (Table 2)** which shows the development of the lifestyle index for a number of spending amounts. The following table is an excerpt of this table:

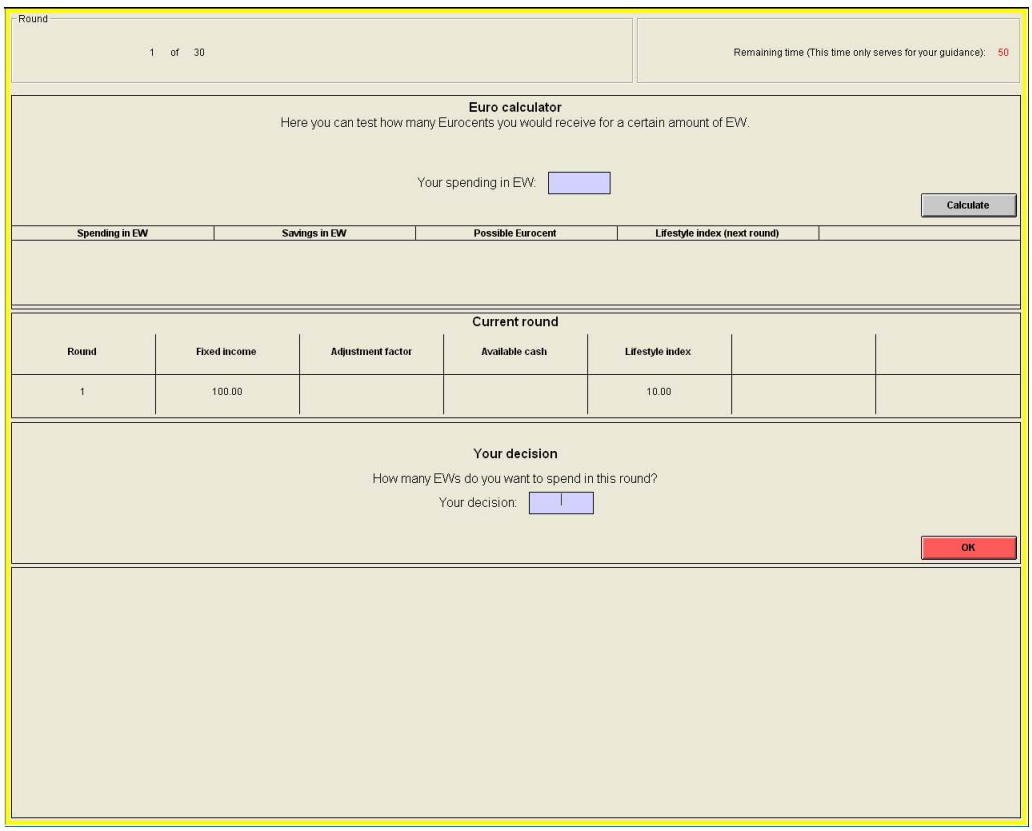
Spending, current round (in EW)	Lifestyle index, current round						
	10	**20**	**50**	**100**	**150**	**200**	**250**
10	17	24	45	80	115	150	185
20	27	34	55	90	125	160	195
40	47	54	75	110	145	180	215
60	67	74	95	130	165	200	235
80	87	94	115	150	185	220	255
100	107	114	135	170	205	240	275
120	127	134	155	190	225	260	295
140	147	154	175	210	245	280	315
160	167	174	195	230	265	300	335
180	187	194	215	250	285	320	355
200	207	214	235	270	305	340	375
220	227	234	255	290	325	360	395
240	247	254	275	310	345	380	415
260	267	274	295	330	365	400	435
280	287	294	315	350	385	420	455
300	307	314	335	370	405	440	475
320	327	334	355	390	425	460	495

If for example you choose to spend an amount of 60 EW with a lifestyle index of 10, your lifestyle index in the **next round** will be 67.

The lifestyle index in the **first round is 10**.

Course of the rounds

As already described, you have to decide in each of the 30 rounds how to split your available money between spending and saving. Once you have decided upon a split and left the round, you **cannot change** this decision any more. I.e. you cannot return to past rounds. The following picture shows your decision screen in the experiment.



Your payoff

After the 30 rounds, **all Eurocents** which you received through your spending are **added up**. You are payed this **sum** in cash at the end of the experiment. In addition, you receive a **show-up fee of EUR 5** for your participation at the experiment.

Comprehension questions

Before the experiment starts, you answer some comprehension questions on the screen. The experiment

will only start, when you have answered all questions correctly. These questions do not influence your payoff. **If you have questions regarding the instructions, please raise your hand.** An employee of the laboratory will come to you and answer your questions.

Eurocent conversion table (Table 1)

Spending (in EW) \ Lifestyle index	10	20	50	100	150	200	250	300	350	400	500	600	700	...
5	-391.57	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	
10	20.48	-290.27	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	
20	185.03	87.76	-257.20	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	
40	238.81	211.32	113.83	-75.81	-286.26	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	-400.00	
60	250.17	237.42	192.21	104.26	6.65	-97.80	-207.67	-322.04	-400.00	-400.00	-400.00	-400.00	-400.00	
80	254.35	247.02	221.03	170.48	114.37	54.33	-8.82	-74.56	-142.55	-212.50	-357.59	-400.00	-400.00	
100	256.34	251.59	234.73	201.95	165.57	126.64	85.69	43.06	-1.03	-46.39	-140.47	-238.41	-339.68	
120	257.43	254.10	242.30	219.33	193.84	166.57	137.88	108.01	77.13	45.35	-20.56	-89.17	-160.12	
140	258.10	255.64	246.91	229.93	211.09	190.92	169.71	147.63	124.80	101.30	52.58	1.85	-50.61	
160	258.54	256.65	249.93	236.87	222.37	206.86	190.55	173.56	156.00	137.92	100.44	61.41	21.06	
180	258.84	257.34	252.02	241.66	230.16	217.86	204.92	191.45	177.52	163.19	133.46	102.51	70.51	
200	259.06	257.84	253.51	245.10	235.76	225.76	215.25	204.31	192.99	181.35	157.20	132.06	106.06	
220	259.22	258.21	254.63	247.65	239.92	231.64	222.93	213.86	204.49	194.84	174.83	154.00	132.47	
240	259.34	258.49	255.48	249.61	243.09	236.12	228.79	221.15	213.26	205.14	188.29	170.75	152.62	...
260	259.44	258.71	256.14	251.13	245.57	239.62	233.36	226.84	220.11	213.17	198.79	183.83	168.35	
280	259.52	258.89	256.67	252.34	247.54	242.40	237.00	231.37	225.55	219.56	207.15	194.22	180.86	
300	259.58	259.03	257.09	253.32	249.13	244.65	239.93	235.03	229.95	224.73	213.90	202.63	190.97	
325	259.64	259.17	257.52	254.30	250.73	246.90	242.88	238.69	234.36	229.91	220.67	211.05	201.10	
350	259.69	259.29	257.86	255.08	251.99	248.69	245.22	241.61	237.87	234.02	226.05	217.74	209.15	
375	259.73	259.38	258.13	255.71	253.02	250.14	247.11	243.96	240.70	237.35	230.39	223.15	215.66	
400	259.76	259.45	258.36	256.22	253.86	251.33	248.66	245.89	243.02	240.07	233.95	227.58	221.00	
425	259.79	259.51	258.54	256.65	254.56	252.31	249.95	247.49	244.95	242.33	236.91	231.26	225.42	
450	259.81	259.57	258.70	257.01	255.14	253.14	251.03	248.83	246.57	244.23	239.39	234.35	229.14	
475	259.83	259.61	258.83	257.32	255.64	253.84	251.94	249.97	247.94	245.84	241.49	236.96	232.28	
500	259.85	259.65	258.95	257.58	256.06	254.43	252.72	250.95	249.11	247.21	243.29	239.20	234.97	
550	259.87	259.71	259.13	258.00	256.74	255.40	253.98	252.51	250.99	249.42	246.17	242.79	239.29	
600	259.89	259.76	259.27	258.31	257.26	256.13	254.94	253.70	252.42	251.10	248.37	245.53	242.59	
650	259.91	259.79	259.37	258.56	257.66	256.70	255.68	254.63	253.54	252.41	250.09	247.66	245.15	
700	259.92	259.82	259.46	258.76	257.98	257.15	256.28	255.37	254.42	253.46	251.45	249.35	247.20	
⋮							⋮							⋱

Lifestyle table (Table 2)

Lifestyle index, current round

Spending amount, current round (in EW)	10	20	50	100	150	200	250	300	350	400	500	600	700	800	900	•••
10	17	24	45	80	115	150	185	220	255	290	360	430	500	570	640	
20	27	34	55	90	125	160	195	230	265	300	370	440	510	580	650	
40	47	54	75	110	145	180	215	250	285	320	390	460	530	600	670	
60	67	74	95	130	165	200	235	270	305	340	410	480	550	620	690	
80	87	94	115	150	185	220	255	290	325	360	430	500	570	640	710	
100	107	114	135	170	205	240	275	310	345	380	450	520	590	660	730	
120	127	134	155	190	225	260	295	330	365	400	470	540	610	680	750	
140	147	154	175	210	245	280	315	350	385	420	490	560	630	700	770	
160	167	174	195	230	265	300	335	370	405	440	510	580	650	720	790	
180	187	194	215	250	285	320	355	390	425	460	530	600	670	740	810	
200	207	214	235	270	305	340	375	410	445	480	550	620	690	760	830	
220	227	234	255	290	325	360	395	430	465	500	570	640	710	780	850	
240	247	254	275	310	345	380	415	450	485	520	590	660	730	800	870	
260	267	274	295	330	365	400	435	470	505	540	610	680	750	820	890	
280	287	294	315	350	385	420	455	490	525	560	630	700	770	840	910	•••
300	307	314	335	370	405	440	475	510	545	580	650	720	790	860	930	
320	327	334	355	390	425	460	495	530	565	600	670	740	810	880	950	
340	347	354	375	410	445	480	515	550	585	620	690	760	830	900	970	
360	367	374	395	430	465	500	535	570	605	640	710	780	850	920	990	
380	387	394	415	450	485	520	555	590	625	660	730	800	870	940	1010	
400	407	414	435	470	505	540	575	610	645	680	750	820	890	960	1030	
420	427	434	455	490	525	560	595	630	665	700	770	840	910	980	1050	
440	447	454	475	510	545	580	615	650	685	720	790	860	930	1000	1070	
460	467	474	495	530	565	600	635	670	705	740	810	880	950	1020	1090	
480	487	494	515	550	585	620	655	690	725	760	830	900	970	1040	1110	
500	507	514	535	570	605	640	675	710	745	780	850	920	990	1060	1130	
520	527	534	555	590	625	660	695	730	765	800	870	940	1010	1080	1150	
540	547	554	575	610	645	680	715	750	785	820	890	960	1030	1100	1170	
560	567	574	595	630	665	700	735	770	805	840	910	980	1050	1120	1190	
580	587	594	615	650	685	720	755	790	825	860	930	1000	1070	1140	1210	
600	607	614	635	670	705	740	775	810	845	880	950	1020	1090	1160	1230	
620	627	634	655	690	725	760	795	830	865	900	970	1040	1110	1180	1250	
⋮								⋮								⋱

Chapter 3

Preferences over Wealth: Experimental Evidence

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Preferences over Wealth: Experimental Evidence

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Abstract. Preferences over wealth can explain why households do not spend more when real interest rates fall, because they save more than optimal under a standard model. However, little is known about preferences over wealth empirically. We run an intentionally simple lab experiment on intertemporal spending and saving decisions with 180 students. Under a positive discount factor, zero interest and linear utility, maximizing behaviour would imply spending any funds instantaneously. While half of the participants behave optimally, we find a robust pattern where participants on average form and maintain a stock of wealth, consistent with wealth entering the utility function directly.
Keywords. consumption; saving motives; wealth; experiment
JEL classification. D12, E21, E62, H23

1 Introduction

A good understanding of consumption and saving behaviour is indispensable for conducting monetary and fiscal policy. Recent New Keynesian models (Michaillat and Saez 2018; Rannenberg 2018) presume households to have preferences over wealth in order to solve the so-called forward guidance puzzle. The latter raises the question of why households do not consume much more of their savings when interest rates are close to or below zero (and should stay there for a long time). Standard preferences would predict a strong increase in consumption, which is, however, at odds with the evidence

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after the financial crisis. Preferences over wealth could be a possible explanation for the sluggish reaction to low interest rates.[1]

However, while preferences over wealth are increasingly being presumed as a useful tool in macro models, little is known about them empirically. From a behavioural perspective preferences over wealth essentially cover several saving motivations beyond intertemporal optimization. From the list of Browning and Lusardi (1996), these include precaution, the enjoyment of independence and being avaricious.

This paper investigates the details of possible preferences over wealth via a lab experiment among 180 students of various disciplines. We construct an intentionally simple consumption-saving experiment, where agents have to make periodical saving and spending decisions.

Just as in a situation with negative real interest rates, the value of savings decreases in each period. The underlying payoff function is designed such that the optimal decision would be to spend any periodical income or initial wealth instantaneously. Keeping any savings would be suboptimal for a rational agent as it would lower the payoff. The payoff function does not promote any saving motives. Nevertheless, agents are free to deviate from optimizing behaviour and can practice excess saving at the cost of a lower payoff.

The participants solve this easy optimization task quite well. However, we find systematic deviations from the optimal behaviour. While about half of the participants are optimizers, there is a robust pattern where participants on average tend to form and maintain a stock of wealth. The full sample of participants on average sacrifices about 2% of their maximum payout in order to hold wealth in the experiment – with rather similar figures among different treatments. For the subgroup of non-optimizers alone, the sacrifice is about 4.5%. Referring to a benchmark where the participants would at

[1] The forward guidance puzzle is only one field of application of preferences over wealth. Carroll (1998) has formulated a "Capitalist Spirit" model where direct utility from wealth explains the saving behaviour of the very rich (see also Francis 2009). Kurz (1968) and Zou (1994) study the implications of preferences over wealth for the economic growth path. Bakshi and Chen (1996) and Gong and Zou (2002) examine its implications for asset pricing, while Saez and Stantcheva (2016) analyzes wealth taxes in a model featuring preferences over wealth.

least spend their remaining cash on hand in the final period, the sacrifice of suboptimal intertemporal allocation is 7% for the full sample and 16% for non-optimizers.

We interpret these findings as evidence for preferences over wealth with utility from both consumption and wealth.

One might argue that small deviations from optimal behaviour can simply be interpreted as near-rationality (Cochrane 1989). However, further patterns imply a meaningful structure of the participants' behaviour: first, subjects starting from very different initial wealth conditions tend to approach the same stock after a smooth trajectory phase; second, agents that face higher discount factors plausibly approach a smaller stock of wealth, pointing to optimizing behaviour at the margin, consistent with Hey and Dardanoni (1987); third, participants tend to run down their stock in the final period. Fourth, saving dynamics are such that holding savings is mean-reverting around a certain level and the marginal propensity to save increases with higher wealth. Both results are very much in line with implications from the buffer stock model of precautionary saving which is akin to a model featuring preferences over wealth (Carroll 1998). Finally, personal characteristics of participants provide plausible explanatory factors: people that characterize themselves as more impulsive are more likely non-optimizers. On the other hand, if the own patience is perceived to be high, people save more, obviously discounting the foregone payout more strongly, again in line with the buffer stock model.

The paper is structured as follows. Section 2 discusses some theoretical underpinnings of our model in use. Section 3 lays out our experimental design in detail. Section 4 presents the results and provides some explanations. The final section concludes.

2 Model

We start from a simple model of consumption choice featuring additive utility over a finite horizon T. The household i maximizes the utility function

$$u_i = E\{\sum_{t=1}^{T} \beta^{(t-1)} v(c_{it})\} \tag{1}$$

where u_i is lifetime utility; $0 \leq \beta < 1$ is a positive intertemporal discount factor and $v(c_{it})$ is the instantaneous (sub)utility function, assumed to be increasing in consumption in period t, c_{it}. The process is subject to the intertemporal budget constraint

$$w_{it} = (1+r)w_{i,t-1} + y_{it} - c_{it} = x_{it} - c_{it}, \qquad \sum_{t=1}^{T} w_{it} \geq 0 \tag{2}$$

where w is wealth at the end of the respective period t, r the interest rate, y is income, and x cash on hand at the beginning of period.

In order to enforce the intertemporal budget constraint in the experiment [in line with][](Carbone and Hey 2004), we include a borrowing constraint $w_{it} \geq 0, \quad \forall t = 1..T$ (Deaton 1991). Under an uncertain income process $y_{t..T}$, subject to i.i.d. shocks, this might trigger a precautionary saving motive to insure against bad future income draws.

Moreover, if we would assume $v(c_t)$ to be CRRA, such that the consumption Euler equation would equal $c_{i,t+1}/c_{it} = [\beta(1+r)]^{\sigma}$, the household would have a saving motive in smoothing consumption, which could lead to $w_{it} > 0$.

However, we do not want to trigger a certain saving motive, neither precaution nor intertemporal substitution. Moreover, we want to rule out systematic error, which could stem from the complicated calculation of the optimal consumption path in such a framework. Furthermore, the macroeconomic literature since Campbell and Mankiw (1989) has often referred to two groups of consumers: forward-looking optimizers and hand-to-mouth consumers, where the latter are assumed to be so impatient that they consume

any cash on hand right away. Such behaviour would produce very similar patterns as credit constraints and precautionary saving motives.

For these reasons, we construct the optimal payoff utility function such that smoothing creates no rewards ($\sigma = 0$) while impatience dominates ($\beta < 1, r = 0$). Zero interest combined with a discount factor less than one is similar to a situation of negative real interest rates or even a situation with negative nominal interest rates. Under such conditions, the optimal rule boils down to consuming any cash on hand instantaneously: $c_{it} = x_{it}$. This should lead to $w_t = 0$ for all $t = 1...T$. Notice, that this result is independent of initial wealth w_0, expected income $Ey_{t..T}$ and β (as long as $\beta < 1$).

In the experiment, we endow half of the participants with a substantial initial stock of wealth. This gives us control over the impact of the borrowing constraint. Endowed households are *a priori* unconstrained by the borrowing limit if they would desire to smooth their consumption (even if smoothing would not maximize payout).

Observed saving should thus not be due to a desire to maximize payout or any enforced saving motive. Instead, saving is costly. Moreover, since the decision rule is rather simple, observed saving should not be driven by systematic miscalculation. As hand-to-mouth consumption and rational choice coincide in our case, deviations from optimality cannot simply be interpreted as mere signs of (partial) hand-to-mouth behaviour.

3 Experimental Design

The experiment took place at the "Essen Laboratory for Experimental Economics" (elfe) in November 2017. We used the software ORSEE (Greiner 2015) for participant recruitment and zTree (Fischbacher 2007) to conduct the experiment.

We processed nine sessions, with at least two different treatments in each session to avoid bias by date or time. The subjects are 180 students of various fields of interest, including both natural and social sciences. They are drawn from a pool of registered students at the University of Duisburg-Essen.

Participants perform the task independently at single PCs in the laboratory. They receive instructions (see Appendix C) and can ask questions, which are answered privately by the experimenter. Once all subjects indicated that they understood the instructions, they have to answer eight comprehension questions (see Appendix C). After all subjects have answered the questions correctly, the training part starts, which is identical to the main part, yet irrelevant for payoff. The main part follows. Participants then are cashed out according to their results.

The experimental design is kept as simple as possible in order to lower the risk of systematic mismatch between the intention of the experimenters and the understanding of the participants. Subjects have to make periodical spending and saving decisions under a well-known lifetime of 20 periods. They receive a periodical income y_{it} with known mean of 100 experimental currency units (ECU), subject to i.i.d. shocks. This gives them an expected stationary lifetime income of 2,000 ECU. Fluctuations of the periodical income can be observed by the participants, but the standard deviation (of 25.9 ECU) is not communicated. Periodical incomes and their fluctuations are identical for all participants.

Half of the participants start with an endowment of w_{i0} = 1,000 ECU and are instructed to represent "rich" households. The other half start with zero endowments and are instructed to represent "poor" households. Participants however do not get any further information about other participants' income, spending, wealth, etc.

In each round participants have to decide how much of their current cash on hand (x_{it}) they want to spend (c_{it}). The residual is saved as a risk-free, non-interest bearing and liquid asset (bank account), fully available for the next period. We assume people gain utility from consumption. This is constructed by rewarding participants' spending decisions with a cash-out after the experiment according to an underlying utility function, which is kept most simple: it is linearly increasing in consumption, there is no gain from smoothing and the linear transformation rate from consumption into payout after the

Table 1: Treatment Overview

	w0	w1000	
$\beta = .99$	30	29	59
$\beta = .95$	30	30	60
$\beta = .8$	30	31	61
	90	90	180

experiment is decreasing by a constant discount factor β_i:

$$U_i = \sum_{t=1}^{20} c_{it}\rho_i * \beta_i^{t-1} \tag{3}$$

where ρ_i is a transformation factor of consumption into utility U_i represented by money rewards after the experiment. The transformation factor is chosen relative to the β factor such that different β treatments still lead to comparable payout when participants behave optimally.

Due to the irrelevance of smoothing, zero interest and positive discounting, the optimal decision would be to spend any periodical income or initial wealth instantaneously. The utility function does not promote any saving. Nevertheless, participants are free to deviate from optimizing behaviour and can practice excess saving at the cost of a lower payoff.

We consider six different treatments: agents are randomly assigned the initial wealth stock $w_{i0} = [0; 1000]$; additionally, agents are randomly assigned one of three different discount factors $\beta_i = [99\%, 95\%, 80\%]$. The discount factors correspond to a situation with negative real interest rates. Assuming that the nominal interest rates would be zero and the intertemporal discount factor would be one, the chosen treatments would correspond to inflation rates of 1%, 5% and even 20%. See Table 1 for an overview of the different treatments and the number of (independent) participants.

We attempt to make sure that participants understand the task correctly by several means. The students are not confronted with equation (3) directly, but they receive a conversion Table of spending-payout pairs for all periods and can also consult a payout calculator implemented at their workplace. Participants have to answer comprehension questions and play a full trial run.

Our experiment is subject to the general critique of laboratory experiments regarding the relatively short time span and the poor context of the experiment as compared to real-life decisions of spending and saving. On the other hand, this has the advantage that we can abstract from a number of confounding factors that usually make it hard to identify behaviour from survey data and other more natural experiments.

Other simplifying attempts to intertemporal consumption choice experiments have been made by e.g. Carbone and Hey (2004), who also use a finite known lifetime, a simple expected income process (high when employed, low when unemployed), smoothing incentives and a no-borrowing constraint. They show excess sensitivity to current income changes. Another comparable avenue has been taken by Meissner (2016), whose experiment features a finite horizon, borrowing facilities, smoothing incentives, zero interest and zero discount rate, and treatments with an either increasing or decreasing stationary trend income path. He observes more optimal behaviour for the decreasing income trend and a reluctance to perform optimal early-period borrowing in the case of an upward-sloping income trend. Meissner interprets his findings as evidence for debt aversion. As a critical difference to our approach, both Carbone and Hey (2004) and Meissner (2016) impose an optimal consumption path that rewards smoothing and that is not straightforward, but has to be solved via mathematical software, which participants do not have at hand.

4 Results

4.1 Descriptive Statistics

Table 2 presents descriptive statistics, subdivided by treatments regarding initial wealth and discount factors. The parentheses contain the upper and lower limits of a 95% confidence interval determined by a simple bootstrap method with original sample size and 10.000 repetitions. On average, the participants are close to the optimizing rule. About 50% of participants (86/180) act completely in accordance with the payout-maximizing consumption path which would imply consuming any wealth and income instantaneously and thus saving nothing at all. The other half deviates from this by holding savings for at least some rounds. Of this latter group, another 16 subjects behave close to optimal with a savings rate (ratio between end-of-period wealth and beginning-of-period cash on hand $\varphi_{it} = (x_{it} - c_{it})/x_{it} = w_{it}/x_{it}$) that has a mean below 2%. That is, there are 78 non-optimizers with a relevant mean savings rate.

Panel A in Table 2 gives figures for the full sample of students, Panel B focuses on non-optimizers. We report (i) the number of students belonging in each group, (ii) the average of the end-of-period wealth $\overline{w_{it}}$ in ECU, (iii) the average savings rate $\overline{\varphi_{it}}$ in %, (iv) the mean loss of payout in Eurocents, (v) efficiency (eff_1) of payout in % measured in a range between zero (worst case) and maximum payout (p_{max}) and (vi) efficiency (eff_2) measured in a range between the payout that would emerge if agents would accumulate wealth until the very last round and then consume everything (second worst case) (p_{min}), and the maximum payout: $eff_1 = \frac{p}{p_{max}} > eff_2 = \frac{p-p_{min}}{p_{max}-p_{min}}$.[2]

[2]Since almost all participants (except for four cases) did not keep relevant savings after the very last round, eff_2 provides an intuitive reference point for the loss that agents may maximally incur due to suboptimal intertemporal allocation, while at least not foregoing any consumption possibilities over the experimental lifespan. Moreover, differences among the β treatments in the Table between eff_1 and eff_2 should be driven by the varying costs of saving alone, since postponing consumption is much more costly with e.g. $\beta = 80\%$ than with $\beta = 99\%$. In the latter case, postponing all consumption until the very last round would, in the case of $w_{i0} = 0$ ($w_{i0} = 1000$), still result in a payout of about 91% (88%) of the maximum, while it would give only 6% (3%) of the maximum when $\beta = 80\%$. The distinction can thus inform us about the way participants deviate from the optimal rule in terms of absolute or relative payout loss.

Table 2: Descriptive Statistics

Panel A: all

	all	w0	w1000	$\beta = .99$	$\beta = .95$	$\beta = .8$
obs	179	89	90	58	60	61
savings	47.67	27.76	67.35	76.2	52.82	15.47
$\overline{w_{it}}$ (ECU)	(43.36; 52.15)	(23.89; 32)	(59.57; 75.28)	(66.49; 86.46)	(44.66; 61.44)	(12.82; 18.35)
sav. rate	14.11	11.81	16.38	19.59	14.97	8.03
$\overline{\varphi_{it}}$ (%)	(10.97; 17.23)	(7.73; 16.2)	(11.68; 21.4)	(13.08; 26.5)	(9.46; 20.97)	(4.78; 11.71)
loss	-40.45	-40.45	-40.44	-30.52	-40.17	-50.16
(Eurocents)	(-56.76; -26.31)	(-67.53; -18.76)	(-59.78; -24)	(-58.62; -10.52)	(-64.5; -20)	(-85.41; -23.61)
eff_1	98.07	97.91	98.23	98.56	97.97	97.71
(%)	(97.32; 98.73)	(96.64; 98.97)	(97.4; 98.94)	(97.21; 99.51)	(96.88; 98.92)	(96.11; 98.92)
eff_2	93.16	90.9	95.4	86.03	95.54	97.6
(%)	(88.48; 96.55)	(81.53; 97.35)	(93.65; 96.92)	(72; 95.76)	(93.2; 97.6)	(95.91; 98.87)

Panel B: non-optimizers

	all	w0	w1000	$\beta = .99$	$\beta = .95$	$\beta = .8$
obs	77	37	40	29	25	23
savings	110.65	66.66	151.34	152.37	126.54	40.77
$\overline{w_{it}}$ (ECU)	(101.05; 120.3)	(58.04; 75.83)	(135.79; 167.28)	(135; 170.7)	(109.07; 145.45)	(34.07; 47.76)
sav. rate	32.7	28.3	36.77	39.17	35.79	21.2
$\overline{\varphi_{it}}$ (%)	(27.66; 37.85)	(21.14; 35.91)	(30.26; 43.26)	(30.27; 47.94)	(27.08; 44.94)	(15.44; 27.41)
loss	-93.51	-96.76	-90.5	-60.69	-96.4	-131.74
(Eurocents)	(-127.53; -64.03)	(-155.14; -49.46)	(-128.25; -58.5)	(-113.79; -23.45)	(-144.8; -56)	(-210.43; -72.61)
eff_1	95.54	95	96.05	97.13	95.13	93.99
(%)	(93.98; 96.91)	(92.25; 97.25)	(94.43; 97.43)	(94.58; 98.91)	(93.02; 96.98)	(90.41; 96.68)
eff_2	84.18	78.26	89.67	72.22	89.31	93.7
(%)	(73.81; 91.68)	(56.6; 93.13)	(86.85; 92.24)	(45.45; 90.7)	(84.87; 93.32)	(89.93; 96.53)

Students have been allocated to the different treatments by equal proportions. The calculations exclude one outlying participant (with treatments $w_{i0} = 0, \beta = 99\%$), who kept substantial savings after the final round and thus ended up with an extremely low payout of EUR 3.70 as compared to the optimum of about EUR 20. Excluding this agent does not alter the mean figures by much, but strongly deflates some confidence bounds.[3]

Non-optimizers (Panel B) are also rather equally distributed among the treatments, yet there is a higher probability of behaving optimal when discounting increases. Since optimizers would have zero-entries in each column and non-optimizers are distributed rather equally, the reported means in Panel B are scaled up versions of those in Panel A. The confidence bounds, on the other hand, are not inflated as much in Panel B even though the sample size is strongly reduced. This is because excluding optimizers eliminates between-group heterogeneity.

On average, participants hold a stock of savings of about 50 ECU over the periods, with an average saving rate of about 14%. In terms of payout this translates into a loss of about EUR 0.40 of the maximum average payout of about EUR 20. The general efficiency eff_1 is therefore rather high (98%). Relative to the benchmark where agents would at least spend everything over their lifetime and only intertemporal allocation is suboptimal, efficiency eff_2 equals 93%. When focussing on non-optimizers, average losses more than double to almost EUR 1. eff_2 is below 85% in this group.

Concerning the role of endowments, saving rates do not differ too much, but the average level of end-of-period savings is 2.5 times as large for agents with initial wealth compared to those without. The drivers of these deviations will be discussed below when looking at period-wise dynamics. There are no relevant differences in losses and efficiency, though.

[3]Note that our findings would be even reinforced when including the participant in the sample. Additionally, due to one absence at one day of the experiments that was compensated the other day, there are generally only 59 observations with $\beta = 99\%$, but 61 with $\beta = 80\%$. We are confident that this small deviation does not affect our results.

Zooming in to the treatments regarding discounting, there are significant differences in holding end-of-period savings between the groups. The treatment with very high costs of saving ($\beta = 80\%$) stands out also with a strongly significantly lower saving rate. These findings are plausible and show that participants rationally react to increasing costs of saving. From Panel B one can derive two distinct channels at play. First, with lower β there is a higher probability that participants behave optimally. Second, even within the group of non-optimizers, wealth holdings and saving rates are significantly reduced when saving is very costly. Most strikingly, both in Panel A and B, differences are small in terms of losses and eff_1. Irrespective of β, participants do accept rather similar absolute losses, while relative losses are significantly different (eff_2) and therefore do not seem to guide average behaviour.

Our interpretation of these first results is that participants do understand the rules and incentives of the game quite well. They react plausibly to higher discount rates and (with a few exceptions) eventually spend their entire lifetime income. Nevertheless, they readily sacrifice a constant portion of their payout of about 2% in order to hold wealth. When benchmarking with intertemporal (in-)efficiency (eff_2), the sacrifice rises to even 7%. Excluding optimizers, both figures are about double in size.

One might argue that small deviations from optimal behaviour can simply be interpreted as near-rationality (Cochrane 1989), stemming from small errors or due to weighing losses against costs of optimization. Since the optimal decision rule is quite simple, rational inattention is rather unlikely in our case. We try to make sense of the findings by looking at the data in more detail in the following sections.

4.2 Dynamic Saving Behaviour

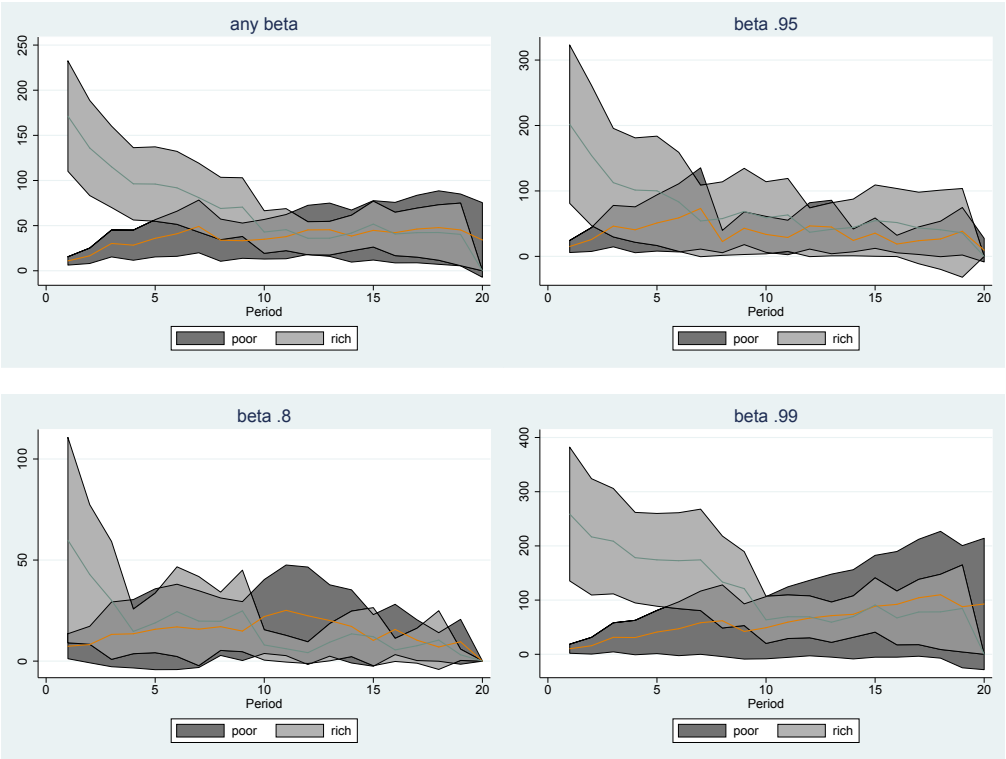
In order to understand the descriptive statistics in more detail, it is beneficial to see the development of the end-of-period wealth holdings over time. Figure 1 shows these patterns for the full sample and the single treatments. Several patterns stand out: "rich"

agents in the early periods tend to smoothly run down their endowment to a positive stock of wealth that they maintain until the penultimate round. "Poor" agents in the early periods tend to build up a stock of wealth of similar size which they also maintain. For each discount factor, "rich" and "poor" agents desire a stock of very similar size. The higher the costs of saving, the lower the desired stock of wealth, in line with Table 2, and the steeper the "rich" agents' trajectory towards the desired stock. On average, participants tend to form and maintain a stock of wealth of about 40-50% of the mean of the uncertain periodical income (equal to 2 standard deviations or about 2.5% of the expected lifetime income). If we interpret our experimental time horizon as a lifetime, the figures are very close to estimates by Lusardi (1998) on precautionary savings based on Health and Retirement Study data for the US (1-4.5% of permanent income). They also coincide with guidelines for financial planning suggesting to holding emergency funds of 33-50% of annual earnings under income uncertainty and with outcomes of a plausible parameterized buffer-stock saving model, where financial wealth is on average about 40% of annual income for agents that are still far from retirement age (Carroll 1997), like our participants.

The smooth run-down of endowed wealth in the first periods can explain why "rich" agents have significantly higher average saving rates in Table 2. This behaviour is consistent with the theory of the endowment effect (Thaler 1980): initial wealth may be valued higher than a purely rational choice would imply. Nevertheless, agents are responsive to smaller β and the endowment effect becomes relatively less important. Eventually, the desire to hold a significant basic stock of savings dominates for all treatments.

How can these patterns be rationalized? Keynes (1936) developed a comprehensive list of saving motives that has been reconsidered and amended by Browning and Lusardi (1996): (1) the precautionary motive, (2) the life-cycle motive, (3) the intertemporal substitution motive, (4) the improvement motive, (5) the independence motive, (6) the enterprise motive, (7) the bequest motive, (8) the avarice motive, and (9) the down-

Figure 1: Mean wealth stock at end-of-period of "poor" vs. "rich" agents for varying costs of saving β



payment motive. Given the structure of our experiment and the observed patterns, we can exclude the life-cycle, improvement, enterprise, bequest and downpayment motive. There is also no extrinsic reward for smoothing consumption. However, the observed pattern is generally consistent with precautionary buffer stock saving, even if in our experiment there is no need, but only costs of precaution. However, people seem to behave *as if* there were a need for precaution. At the same time, the dynamic patterns are consistent with an independence and an avarice motive. Both avarice, independence and precautionary motives would be consistent with a general preference over wealth.

One can test further implications of these theoretical saving concepts by looking at behaviour of agents over time, in order to discriminate between them. We can exploit the fact that in our experiment incomes vary by an i.i.d. process with a known mean, so there is transitory income uncertainty.

There are two relevant differences between the precautionary, avarice and independence motives: first, how saving is expected to be influenced by changes in the wealth level; second, the prediction of the motives of the reaction of saving behaviour after an unexpected increment in income (i.e. the marginal propensity to save, MPS) depending on the level of wealth. Regarding the direct impact of the wealth level on saving, both the precautionary and independence motive would predict that there is a positive target wealth level. Existing wealth would therefore ceteris paribus lead to less saving. The opposite would occur if people were driven by avarice motives.

Regarding the MPS, both the precautionary and the avarice motives would predict the MPS to be increasing in the level of wealth for very different reasons: precautionary savers will save more than optimal on average and will target a buffer stock, but on the edge of precautionary fear they will consume a higher share of a positive income shock the lower their wealth holdings are (Jappelli and Pistaferri 2014; Carroll 1997). Likewise, agents with less wealth will cut down consumption more strongly after a negative income shock. Avarice would imply that in particular rich persons want to become even

richer and therefore have a higher MPS. Independence would imply the opposite: the independence motive should be increasingly satisfied by rising wealth. Thus, given a certain wealth stock, additional income can be safely consumed completely.

Since the path of income y_{it} is stationary and has a known mean, but is subject to i.i.d. shocks, we can exploit the variation in income, consumption and cash on hand (normalized to its sample mean) in order to estimate the marginal propensity to save out of changes in income as compared to the previous round Δy_{it}:

$$\Delta s_{it} = \alpha_0 + \alpha_1 \Delta y_{it} + \varepsilon_{it} \tag{4}$$

where α_1 gives an estimate of the MPS. In order to investigate the impact of the level of wealth on saving directly and on the MPS, we append (4) by $w_{i,t-1}$, the wealth level at the end of the previous period, and by an interaction term $\Delta y_{it} * w_{i,t-1}$ to allow for a possible heterogeneity of the MPS:

$$\Delta s_{it} = \alpha_0 + \alpha_1 \Delta y_{it} + \alpha_2 w_{i,t-1} + \gamma \Delta y_{it} * w_{i,t-1} + \varepsilon_{it} \tag{5}$$

α_2 will signal the direct impact of the wealth level on saving behaviour, whereas γ indicates the impact of wealth on the MPS. Results (referring to the sample means of income and wealth) are shown in Table 3.

Column (1) gives a plain estimate assuming a homogeneous MPS, which turns out to be 16% on average (notably including all optimizers with a zero MPS). Agents in general do respond to an income increase by saving more, but the much larger part goes to additional consumption, which is plausible, given the incentives of the experiment. Column (2) then allows for heterogeneity with respect to wealth holdings. The baseline MPS of 20% is calculated at the mean level of wealth. The coefficient α_2 on $w_{i,t-1}$ itself shows that having a higher wealth level negatively affects saving (holding income constant). This reflects the general tendency to reducing wealth holdings when being

Table 3: Marginal Propensity to Save and the Level of Wealth

	Dep. Var. Δs_{it}				
	(1)	(2)	(3)	(4)	(5)
	plain	prime	FE	r9-19	nonopt
Δy_{it}	0.161***	0.204***	0.194***	0.185***	0.296***
	(0.0371)	(0.0357)	(0.0717)	(0.0375)	(0.0817)
$w_{i,t-1}$		-0.237***	-0.639***	-0.262***	-0.282***
		(0.0122)	(0.120)	(0.0176)	(0.0202)
$\Delta y_{it} * w_{i,t-1}$		0.00188***	0.00197**	0.00153***	0.00161***
		(0.000284)	(0.000842)	(0.000332)	(0.000472)
Const	-0.674	-6.455***	-16.47***	-8.179***	9.298**
	(1.713)	(1.640)	(2.987)	(1.992)	(3.782)
Obs	3,222	3,222	3,222	1,969	1,386
R^2	0.006	0.121	0.268	0.113	0.141

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

above the average and increasing wealth holdings when being below. This is in line with precautionary savings (or buffer stock behaviour as described by Carroll (1992)) and desired independence, but does not fit to avarice.

Regarding our second testable implication, the MPS increases in $w_{i,t-1}$, as shown by the interaction term γ. That is, a participant with 100 ECU more wealth than a reference participant will save about 19% more of a positive income shock. A poorer agent on the other hand, will consume a larger fraction of such shocks. This finding is consistent with precautionary saving motives and avarice. It does not support the independence motive that would project a lower MPS given an already high wealth level. Combining both tests, there is no evidence against precautionary saving, but the first test speaks against avarice and the second result does not square well with an independence motive.

The buffer-stock interpretation is reinforced when using a fixed-effects specification with clustering of standard errors at the participant level (column (3)) where we effectively discard level variation between agents' MPS. The mean reversion (reducing saving when wealth is above average and increasing saving when below) is even stronger. The

magnitude of heterogeneity of the MPS is slightly increased.

Might these effects be driven by the trajectories observed over the first rounds or the special circumstances in the last round? Given the figures in column (4), when only looking at observations from round 9 to 19, heterogeneity of MPS is somewhat lower, but still strongly statistically and economically significant. Could the results be driven by the difference of optimizers vs. non-optimizers? Since optimizers would have an MPS of zero, between-group variation may have a strong influence. However, when focussing on non-optimizers in column (5) results hardly change. From this analysis, the preference to hold costly non-interest bearing wealth might be best explained by a buffer-stock precautionary motive.

4.3 Personal Characteristics and Learning

As an additional exercise, we relate the mean end-of-period wealth to the respective treatment categories and self-reported characteristics of agents that we collected from a questionnaire after to the experiment. Results are shown in Table 5 in Appendix B. In line with Table 2, increasing costs of saving (lower β) have a negative impact on average saving stocks while the endowment is positively related to average savings. These findings do not change much when keeping the sample constant but controlling for a couple of personal characteristics in column (2).

The impact of the endowment becomes less clear when focusing on non-optimizers and is plausibly much less relevant in later periods. When looking at the dynamic specification in columns (5) and (6), there is strong inertia with respect to x_{it} in line with Figure 1. Cash on hand at the beginning of the period strongly predicts wealth at the end of the period, in particular during the later periods where the average savings stock is kept quite stable. When controlling for such an autoregressive process, the endowment w_0 ceases to be a relevant explanatory factor since x_{it} can track the different trajectories of "rich" and "poor" savers quite well. The discount factor is always highly statistically

significant, but less relevant in later periods and when looking at the dynamics instead of average levels. Regarding personal factors, male students tend to accumulate more savings on average, but this is only statistically significant and quantitatively more relevant when looking at the subgroup of non-optimizers. At the same time, the share of optimizers is larger among male participants.

The time it took participants to answer the comprehension questions is a significant watershed for optimal vs. non-optimal behaviour, since the coefficient is significant and positive for the full sample, but close to zero and insignificant when looking at non-optimizers or the late rounds (with lower average savings) only.

Self-perceptions do have some correlation with saving decisions as well: Higher patience is related to higher savings, which can be rationalized in that patient participants might have a lower intrinsic discount rate. In line with this interpretation, patience ceases to be a significant factor in later rounds when discounting becomes less important for the payout by design. Impulsiveness may be a good predictor for non-optimal behaviour as can be seen from the difference between column (2) and (3).[4] Likewise, impulsiveness can well explain some wealth level differences in later rounds (column (4)), but when controlling for level inertia in the dynamic specification via x_{it} (column (6)), it ceases to be significant. Perceptions of attitudes towards risk do not seem to be relevant for explaining saving behaviour in the experiment.

Table 6 in Appendix B shows similar regression output, when the participants' loss in Eurocents is considered as left-hand-side variable instead of average wealth holdings. Differences in losses are generally hard to explain – neither by experimental treatments nor by personal characteristics. In line with what has been found above, patience and impulsiveness do seem to have an impact, increasing the absolute value of losses.

Descriptive statistics from the training round (see Table 4 in Appendix A) show that there are much more (7) cases with a substantial loss (if there were a payout from

[4] A simple probit regression shows that a one-step increase on the scale of impulsiveness (0-10) makes it 10% less likely (and highly significantly so) to be an optimizer (results not shown).

the training round). There are much more non-optimizers (140 out of 173), which are again rather randomly distributed among the treatments. Furthermore, there are on average much higher savings, (theoretical) losses and lower efficiency. Therefore, there seems to be a substantial learning process towards the payout-relevant game or stronger compliance due to the payout incentive. We cannot discriminate one from the other.

Nevertheless, there is a strikingly similar dynamic of end-of-period wealth holdings over time (see Figure 2 in Appendix A). The average buffer is considerably higher and there are some level differences between agents with endowment and those without. However, the pattern of a smooth trajectory towards a preferred buffer stock that is maintained throughout the second half of the game seems quite robust. Likewise, higher costs of saving lead to a lower buffer stock and a steeper trajectory for endowed agents, very much in line with our central findings above.

5 Conclusions

We evaluate an intentionally simple lab experiment of intertemporal consumption-saving decisions. About half of the participants behave in line with the underlying optimal rule: they consume any cash on hand instantaneously. The behaviour of the other half, however, is consistent with general preferences over wealth. The dynamic patterns show that participants tend to smoothly form and maintain a buffer stock of wealth, quite consistent with a precautionary saving motive (as if there were a need for precaution), or some other form of wealth providing direct utility. In particular, participants seem ready to sacrifice a rather similar payout for these wealth holdings in the experiment. When costs of saving are higher, agents tend to hold less cash on hand. These findings can be rationalized by a simple model with a direct ingredient of wealth in the utility function where utility from consumption (the payout) is weighed against holding wealth

in the experiment:

$$u_i = E\{\sum_{t=1}^{T} \beta^t v(c_{it}, w_{it})\}. \tag{6}$$

In the optimum, the marginal utilities from consumption and wealth should be equal:

$$\frac{\partial v}{\partial c_{it}} = \frac{\partial v}{\partial w_{it}}. \tag{7}$$

In the face of the budget constraint, participants would trade off impatient consumption against wealth holdings. Plausibly, the utility from wealth should fall to zero after the final round.

Personal characteristics of participants provide some plausible explanatory factors: There is some evidence that a higher financial literacy leads to payout-maximizing behaviour. People that characterize themselves as more impulsive are more likely non-optimizers. When participants see themselves as more patient, they are also inclined to hold higher savings, likely employing a lower intrinsic discounting of future consumption, in line with a buffer stock model (Carroll 1997). In any way "[...]the implications for saving behaviour are [...] virtually indistinguishable from the idea that wealth enters the utility function directly" (Carroll 1998). Our experimental findings therefore may provide some micro-evidence for recent New-Keynesian models that use preferences over wealth as a channel to rationalize macroeconomic patterns (Michaillat and Saez 2018; Rannenberg 2018).

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Appendix A Statistics of the trial run

Table 4: Descriptive Statistics – Trial Run

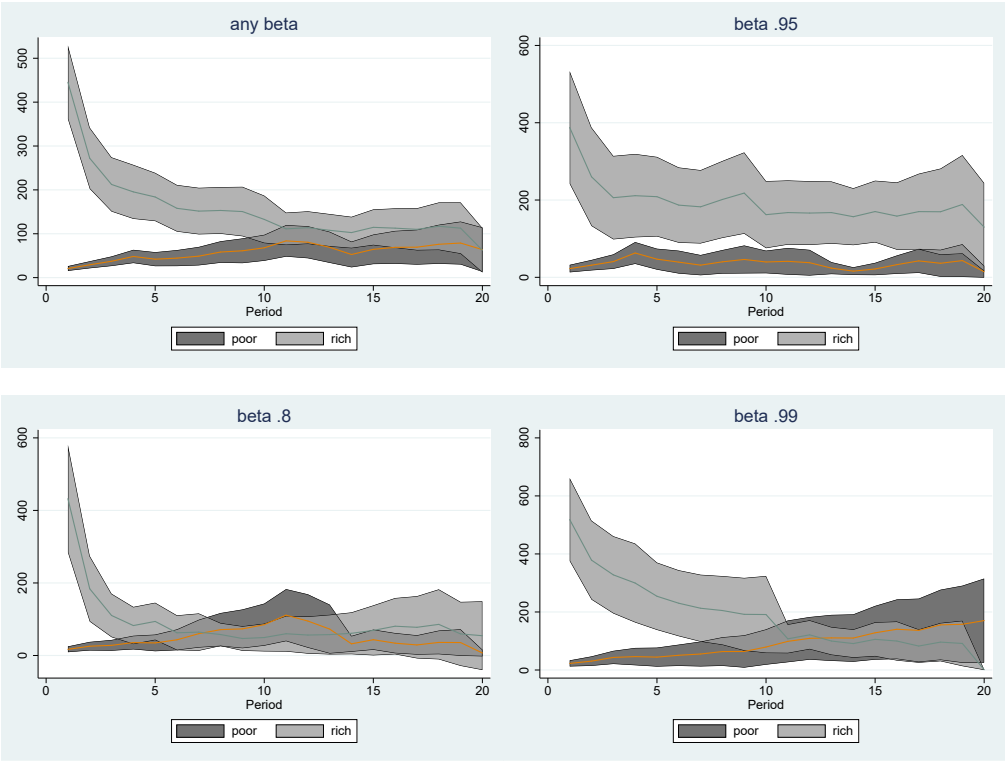
Panel A: all

	all	w0	w1000	$\beta = .99$	$\beta = .95$	$\beta = .8$
obs	179	89	90	58	60	61
savings	103.86	51.12	156.02	127.58	114.52	70.83
$\overline{w_{it}}$ (ECU)	(96.81; 111.07)	(45.31; 57.28)	(143.9; 168.6)	(113.34; 142.23)	(102.24; 127.72)	(61.15; 80.96)
sav. rate	24.82	19.27	30.31	27.56	26.36	20.7
$\overline{\varphi_{it}}$ (%)	(21.17; 28.48)	(14.82; 23.88)	(24.81; 35.96)	(20.67; 34.7)	(19.89; 33.05)	(15.55; 26.25)
loss	-140.34	-118.99	-161.44	-88.28	-113.17	-216.56
(Eurocents)	(-174.02; -109.11)	(-172.36; -73.48)	(-203.22; -121.67)	(-156.9; -34.31)	(-157.17; -73.33)	(-277.54; -162.3)
eff_1	93.52	94.18	92.87	95.79	94.87	90.03
(%)	(91.97; 94.95)	(91.67; 96.31)	(91; 94.64)	(92.49; 98.39)	(93.09; 96.52)	(87.23; 92.53)
eff_2	78.93	71.49	86.28	57.05	89.24	89.59
(%)	(66.89; 88.29)	(47.65; 89.67)	(83.63; 88.87)	(21.89; 84.97)	(85.68; 92.55)	(86.65; 92.2)

Panel B: non-optimizers

	all	w0	w1000	$\beta = .99$	$\beta = .95$	$\beta = .8$
obs	140	67	73	46	45	49
savings	132.48	67.83	191.83	160.65	152.06	88.07
$\overline{w_{it}}$ (ECU)	(123.5; 141.42)	(60.1; 75.92)	(177.15; 206.9)	(143.39; 178.6)	(136.36; 168.58)	(76.33; 100.28)
sav. rate	31.67	25.52	37.32	34.67	35.07	25.74
$\overline{\varphi_{it}}$ (%)	(27.72; 35.68)	(20.4; 30.98)	(31.67; 43.08)	(27.15; 42.46)	(28.08; 42.31)	(20.11; 31.7)
loss	-176.36	-154.78	-196.16	-108.7	-148.89	-265.1
(Eurocents)	(-217.5; -139)	(-222.99; -95.97)	(-245.48;-150)	(-195; -40.43)	(-203.78; -100.89)	(-330.61; -202.45)
eff_1	91.85	92.42	91.34	94.81	93.25	87.8
(%)	(89.97; 93.57)	(89.2; 95.16)	(89.14; 93.39)	(90.66; 98.1)	(91.05; 95.18)	(84.78; 90.69)
eff_2	73.58	62.78	83.5	47.04	85.82	87.25
(%)	(58.83; 85.28)	(31.91; 86.93)	(80.53; 86.36)	(2.87; 82.31)	(81.43; 89.7)	(84.08; 90.26)

Figure 2: Trial Run: Mean wealth stock at end-of-period of "poor" vs. "rich"



Appendix B Influence of personal characteristics

The characteristics are constant over the periods such that we can only exploit cross-sectional variation of the participants in this regard. When looking at the full sample in the final two columns, standard errors are clustered at the participant level. Column (1) is a plain specification featuring only the experimental treatments. Columns (2)-(4) look at the impact of personal characteristics for the full sample (2), a sample excluding optimizers (3) and a sample taking only the information from periods 9 to 19 into account (4). Columns (5) and (6) are rather similar to (2) and (4), but include the longitudinal dimension from single rounds and control for the impact of cash on hand x_{it} as a representation of an autoregressive process of savings. $1 - \beta$ are the costs of

Table 5: Wealth Holdings and Personal Characteristics

	Dep. Var. w_{it}					
	(1)	(2)	(3)	(4)	(5)	(6)
	plain	full	noopt	r9-19	w_{it}	w_{it} r9-19
$(1-\beta)$	-3.014***	-2.654***	-4.484**	-2.189**	-1.561***	-0.465***
	(0.924)	(0.928)	(1.841)	(0.850)	(0.378)	(0.140)
$w_0 = 1000$	39.96***	31.42**	47.72	10.24	-2.714	0.315
	(15.17)	(14.91)	(29.79)	(13.65)	(6.598)	(2.544)
x_{it}					0.416***	0.785***
					(0.0489)	(0.0550)
male		24.81	67.80**	16.39	14.37*	2.395
		(16.00)	(31.20)	(14.65)	(8.005)	(2.598)
age		2.197	12.70**	3.639	1.308	0.961**
		(2.539)	(5.223)	(2.325)	(1.253)	(0.428)
natsci		-4.117	11.39	-4.651	-2.696	-2.068
		(15.72)	(29.34)	(14.39)	(9.577)	(3.393)
school		10.84	6.658	8.446	6.476	1.854
		(12.93)	(23.23)	(11.84)	(6.172)	(1.736)
tquest		0.150**	0.00500	0.0528	0.0878**	0.00459
		(0.0679)	(0.102)	(0.0622)	(0.0401)	(0.0154)
patience		6.991**	10.29*	4.259	4.096***	0.841*
		(2.953)	(5.794)	(2.703)	(1.356)	(0.438)
impulse		8.501**	10.19	6.355**	4.986***	1.026
		(3.332)	(6.932)	(3.051)	(1.780)	(0.699)
risk		0.167	-3.409	-0.0598	0.0362	-0.141
		(3.419)	(7.284)	(3.130)	(1.670)	(0.599)
conc_econ		-10.81	-22.18	-9.292	-6.209	-1.898
		(12.46)	(25.12)	(11.41)	(6.272)	(1.990)
conc_fin		-22.94*	-7.461	-21.98*	-13.67*	-3.031
		(12.72)	(27.03)	(11.64)	(7.173)	(2.414)
conc_health		17.84	43.47*	22.11**	10.67	4.493**
		(12.18)	(21.95)	(11.15)	(6.848)	(2.269)
inc_real		7.289	17.36	6.495	3.952	1.153
		(12.63)	(27.01)	(11.56)	(8.463)	(3.351)
sav_real		0.171	-10.24	1.951	0.237	0.859
		(5.213)	(10.31)	(4.773)	(2.763)	(0.906)
Const	54.14***	-122.9	-349.3**	-125.0*	-115.8***	-114.7***
	(13.46)	(76.68)	(158.9)	(70.21)	(41.43)	(14.79)
Obs	179	179	77	179	3,580	1,969
R^2	0.090	0.241	0.443	0.165	0.476	0.707

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

saving $[1\%, 5\%, 20\%]$; $w_0 = 1000$ is 1, if the initial wealth is 1000, or 0, if the initial wealth is 0; x_{it} is cash-on-hand before the consumption decision; *natsci* is a dummy, which is 1, if the participant studies natural sciences or a technical subject, or 0 else; *male* is a dummy equal to one for male participants; *age* gives the age of the participant in years; *school* is the average high school exam grade (German "Abiturnote"); *tquest* measures the time in seconds until the participant has answered the comprehension questions; *patience* is the self reported patience (between 0 and 10, where 10 is largest); *impulse* is the self reported impulsiveness (same scale); *risk* is the self reported attitude towards risk taking (same scale); *conc_econ* is the answer to the question "Do you have concerns about the general economic development?" (between 1 and 3, where 3 means strong concerns); *conc_fin* is the answer to the question "Do you have concerns about your own financial situation?" (same scale); *concerns_health* is the answer to the question "Do you have concerns about your health?" (same scale); *inc_real* is the answer to the question "What is the amount of money, that you can use for spending per month on average?" categorized into seven classes; *sav_real* is the answer to the question "Are you able to save in a normal month? And if so, how much is that?" categorized into seven classes.

Table 6: Average Loss and Personal Characteristics

Dep. Var.	Average Loss (negative sign)[5]		
	(1)	(2)	(3)
	plain	full	noopt
$(1-\beta)$	-0.929	-1.260	-4.436*
	(0.953)	(1.000)	(2.313)
$w_0 = 1000$	0.121	3.882	14.60
	(15.64)	(16.06)	(37.42)
male		-1.301	-23.43
		(17.24)	(39.19)
age		2.190	-2.871
		(2.736)	(6.561)
natsci		4.096	-10.45
		(16.94)	(36.86)
school		-26.10*	-24.60
		(13.94)	(29.18)
tquest		-0.0713	0.0133
		(0.0732)	(0.128)
patience		-6.312**	-8.817
		(3.181)	(7.279)
impulse		-8.391**	-12.78
		(3.590)	(8.708)
risk		2.928	10.69
		(3.684)	(9.149)
conc_econ		-0.781	-9.895
		(13.43)	(31.56)
conc_fin		10.01	6.133
		(13.70)	(33.95)
conc_health		-14.78	-47.51*
		(13.13)	(27.58)
inc_real		3.616	24.08
		(13.60)	(33.93)
sav_real		-1.400	7.250
		(5.616)	(12.95)
Const	-32.32**	63.46	181.6
	(13.88)	(82.62)	(199.6)
Obs	179	179	77
R^2	0.005	0.094	0.178

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix C Instructions

The original instructions are in German language (see below). Here you see a translated version. For brevity, we only present the instructions for one treatment (wealth=0 [wealth=1000], $\beta = 0.95$, $\rho = 1.1$). Note that in the other treatments only the content of the conversion table changes.

Welcome to the experiment!

You will take part in a study of decision-making behaviour within the framework of experimental economic research. During the investigation you will be asked to make decisions. You can earn money. How much money this will be depends on the course of the experiment. Detailed instructions are given below. All participants will be paid individually and in cash immediately after the experiment. Therefore, please remain seated at your seat after the experiment until your seat number is called. Before the actual experiment, there will be a trial run. During the trial run you will not be able to make any real money. The rehearsal is only for the sake of understanding. The decisions you make in the trial do not affect the main part of the experiment. If you have any questions before the start of the experiment, please contact one of the laboratory staff members by hand signal. They will then come to your place and help you. No more questions can be answered during the experiment. After the main part of the experiment, a questionnaire will open. Only after all participants have answered the questionnaire completely the payout begins. **Any communication with the other participants is not allowed during the experiment. Violation of this rule will result in immediate exclusion from the experiment.**

The trial run and the main part of the experiment consist of **20 rounds** each. In each of these rounds you will have to make a decision on how to split your available money between **saving** and **spending**. You can see which round you are in at the top of the screen. Your money will be displayed in the unit Experimental Currency (ECU).

Money available

Your available money consists of several parts: Your savings from the previous rounds and your current income. From the first round of the trial run as well as from the first round of the main part, you also have a starting capital at your disposal. The available money is expressed in ECU. Attention: You cannot transfer any savings from the trial round to the main part of the experiment. Available Money = Savings + Current Income

Current income

In each round you receive a current income. The amount of the current income is determined randomly and varies in each round. On average, the current income is approximately 100 ECU. The exact amount of current income in each round is shown at the beginning of each round.

Starting wealth

From the first round of the trial run and from the first round of the main part, you have a starting wealth at your disposal. Your starting wealth is part of your available money. So you can spend or save it just like the rest of your available money. Since you represent a poor [rich] household, your wealth is 0 [1000] ECU.

Information in each round

In each round, savings, current income and the resulting available money are displayed in a table. You can also see the values of the previous rounds in the table.

Your decision

Your decision as to how much of your available money you want to spend in the round (and thus indirectly how much you want to save) is entered in the "Your decision" field. The ECU amount you spend in each round is converted into Eurocents. The sum of the ECU amounts of the main part is paid to you at the end of the experiment.

Conversion table

You can use the conversion calculator on the screen to calculate on a trial basis how many Eurocents you will receive for a certain amount in Eurocents. The more ECU you spend, the more Eurocents you receive. The conversion of expenses into Eurocents is also influenced by the number of rounds. The higher the number of rounds, the less Eurocents you will receive for a certain amount of expenditure. Attention! The values in the conversion table are rounded to whole numbers for clarity. In the game, however, the values are not rounded. Only at the very end is the amount in Eurocents that you have earned rounded to the next higher multiple of 10 Eurocents. The conversion calculator displays the values exactly with four decimal places.

Conversion Table Round 1 to 10

	Round:									
	1	2	3	4	5	6	7	8	9	10
Expenditure in ECU:	Earnings in Eurocents									
0	0	0	0	0	0	0	0	0	0	0
20	21	20	19	18	17	16	15	15	14	13
40	42	40	38	36	34	32	31	29	28	26
60	63	60	57	54	51	49	46	44	42	40
80	84	79	75	72	68	65	61	58	55	53
100	105	99	94	90	85	81	77	73	69	66
120	125	119	113	108	102	97	92	88	83	79
140	146	139	132	125	119	113	108	102	97	92
160	167	159	151	143	136	129	123	117	111	105
180	188	179	170	161	153	146	138	131	125	119
200	209	199	189	179	170	162	154	146	139	132
220	230	218	207	197	187	178	169	161	153	145
240	251	238	226	215	204	194	184	175	166	158
260	272	258	245	233	221	210	200	190	180	171
280	293	278	264	251	238	226	215	204	194	184
300	314	298	283	269	255	243	230	219	208	198
320	334	318	302	287	272	259	246	234	222	211
340	355	338	321	305	289	275	261	248	236	224
360	376	357	340	323	306	291	277	263	250	237
380	397	377	358	340	323	307	292	277	263	250
400	418	397	377	358	340	323	307	292	277	263
420	439	417	396	376	357	340	323	307	291	277
440	460	437	415	394	375	356	338	321	305	290
460	481	457	434	412	392	372	353	336	319	303
480	502	477	453	430	409	388	369	350	333	316
500	522	496	472	448	426	404	384	365	347	329
1000	1045	993	943	896	851	809	768	730	693	659
1500	1568	1489	1415	1344	1277	1213	1152	1095	1040	988
2000	2090	1986	1886	1792	1702	1617	1536	1460	1387	1317
3000	3135	2978	2829	2688	2553	2426	2305	2189	2080	1976
...	...	...	...	...	...	...	...	...	...	...

If, for example, you decide to spend 100 ECU in round 7, you will receive 77 Eurocents.

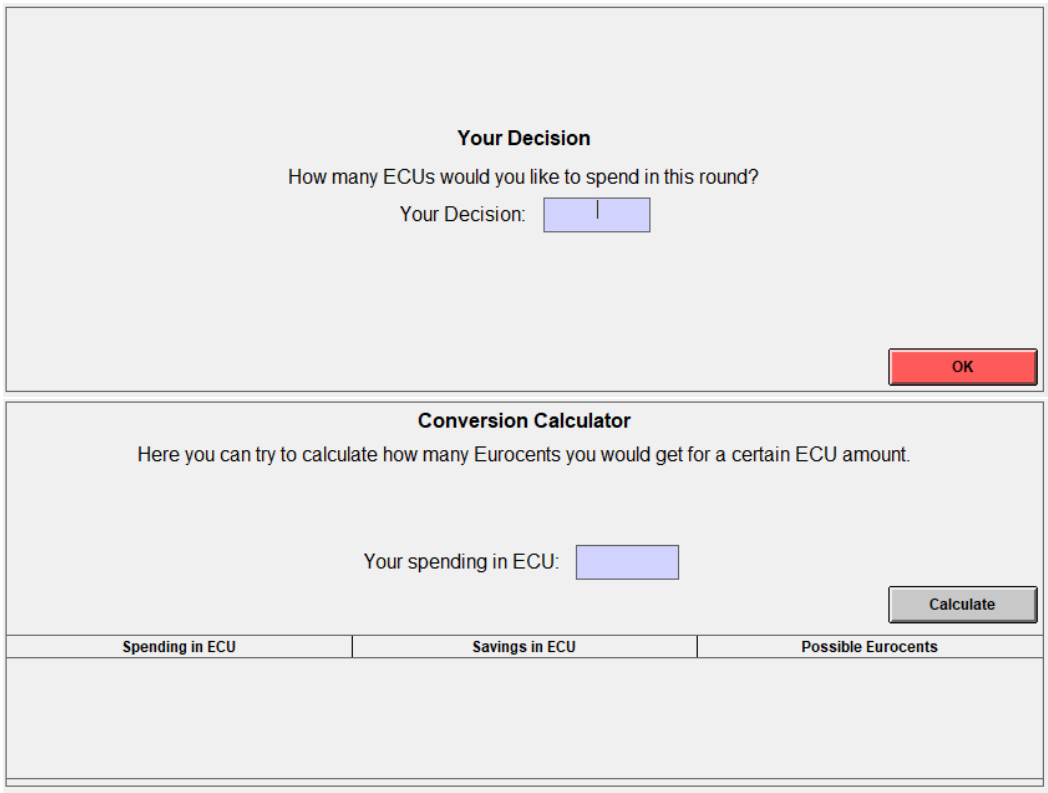
Conversion Table Round 11 to 20

	Round:									
	11	12	13	14	15	16	17	18	19	20
Expenditure in ECU:	Earnings in Eurocents:									
0	0	0	0	0	0	0	0	0	0	0
20	13	12	11	11	10	10	9	9	8	8
40	25	24	23	21	20	19	18	17	17	16
60	38	36	34	32	31	29	28	26	25	24
80	50	48	45	43	41	39	37	35	33	32
100	63	59	56	54	51	48	46	44	42	39
120	75	71	68	64	61	58	55	52	50	47
140	88	83	79	75	71	68	64	61	58	55
160	100	95	90	86	82	77	74	70	66	63
180	113	107	102	97	92	87	83	79	75	71
200	125	119	113	107	102	97	92	87	83	79
220	138	131	124	118	112	107	101	96	91	87
240	150	143	136	129	122	116	110	105	100	95
260	163	155	147	139	133	126	120	114	108	103
280	175	166	158	150	143	136	129	122	116	110
300	188	178	169	161	153	145	138	131	125	118
320	200	190	181	172	163	155	147	140	133	126
340	213	202	192	182	173	165	156	149	141	134
360	225	214	203	193	183	174	166	157	149	142
380	238	226	215	204	194	184	175	166	158	150
400	250	238	226	215	204	194	184	175	166	158
420	263	250	237	225	214	203	193	184	174	166
440	275	262	248	236	224	213	202	192	183	174
460	288	273	260	247	234	223	212	201	191	181
480	300	285	271	257	245	232	221	210	199	189
500	313	297	282	268	255	242	230	218	208	197
1000	626	594	565	536	510	484	460	437	415	394
1500	939	892	847	805	764	726	690	655	623	592
2000	1251	1189	1129	1073	1019	968	920	874	830	789
3000	1877	1783	1694	1609	1529	1452	1380	1311	1245	1183
...	...	...	...	...	...	...	...	...	...	...

If, for example, you decide to spend 320 ECU in round 18, you will receive 140 Eurocents.

Course of the rounds

As described above, in each of the 20 rounds you have to make a decision on how to spend and save your available money. Once you've decided to split and left the round, you can't change that decision. So you can't go back to past rounds. The following picture shows what your decision screen looks like in the experiment.



Your payment

The rounds of the trial round are not relevant for disbursement. Only the decisions in the main part of the experiment determine the payout. At the end of the 20 rounds of the main part, all Eurocents that you have collected through your expenses in the individual rounds of the main part are added. You will receive this sum in cash at the end of the experiment.

Comprehension questions

Before the trial run begins, you have to answer a series of on-screen comprehension questions. The experiment does not begin until you have answered all the questions correctly. These questions do not affect your payout. If you have any questions about the instructions, please contact us by hand signal. An employee will then come to you and answer your questions.

Comprehension Questions

- You are in round 1. How many Eurocents do you get if you spend 40 ECU?
- You are in round 2. How many Eurocents do you get if you spend 40 ECU?
- You are in round 3. How many Eurocents do you get if you spend 400 ECU?
- You are in round 16. How many Eurocents do you get if you spend 400 ECU?
- You spend 100 ECU. How many Eurocents do you get more by spending in round 2 instead of round 12?
- You spend 200 ECU. How much Eurocents do you get less if you spend in round 16 instead of round 3?
- In one round you will have a current income of 100 ECU plus savings from the preliminary rounds of 200 ECU. What is your disposable income in ECU in this round?
- In one round your disposable income is 250 ECU You spend 120 ECU. How high are your savings in the next round?

Original Instructions

In the following, you see the original instructions in German language. For brevity, we only present the instructions for one treatment (wealth=0, $\beta = 0.95$, $\rho = 1.1$).

Willkommen zum Experiment!

Sie nehmen an einer Untersuchung des Entscheidungsverhaltens im Rahmen der experimentellen Wirtschaftsforschung teil. Während der Untersuchung werden Sie gebeten, Entscheidungen zu treffen. Dabei können Sie Geld verdienen. Wie viel Geld das sein wird, hängt vom Experimentverlauf ab. Im Folgenden erhalten Sie hierzu detaillierte Instruktionen. Alle Teilnehmer werden direkt im Anschluss an das Experiment einzeln und in bar ausgezahlt. Bitte bleiben Sie daher nach dem Experiment so lange an Ihrem Platz sitzen, bis Ihre Platznummer aufgerufen wird. Vor dem eigentlichen Experiment findet ein Probedurchgang statt. In dem Probedurchgang können Sie kein echtes Geld verdienen. Der Probedurchgang dient lediglich dem Verständnis. Die Entscheidungen, die Sie in dem Probedurchgang treffen, haben keine Auswirkungen auf den Hauptteil des Experiments. Sollten Sie vor dem Start des Experiments Fragen haben, wenden Sie sich bitte per Handzeichen an einen Mitarbeiter des Labors. Er wird dann zu Ihnen an den Platz kommen und Ihnen weiterhelfen. Während des Experiments können keine Fragen mehr beantwortet werden. Nach dem Hauptdurchgang des Experiments, öffnet sich noch ein Fragebogen. Erst nachdem alle Teilnehmer den Fragebogen vollständig beantwortet haben, beginnt die Auszahlung. **Jegliche Kommunikation mit den anderen Teilnehmern ist während des Experiments nicht gestattet. Ein Verstoß gegen diese Regel führt zum sofortigen Ausschluss vom Experiment.**

Der Probedurchgang und der Hauptteil des Experiments bestehen aus je **20 Runden**. In jeder dieser Runden müssen Sie eine Entscheidung treffen, wie Sie Ihr verfügbares Geld zwischen **sparen** und **ausgeben** aufteilen wollen. In welcher Runde Sie sich befinden, sehen Sie am oberen Bildschirmrand. Ihr Geld wir dabei in der Einheit Experimentwährung (EW) angezeigt.

Verfügbares Geld

Ihr verfügbares Geld setzt sich aus mehreren Teilen zusammen: Ihr Erspartes aus den vorherigen Runden und Ihr aktuelles Einkommen. Ab der ersten Runde des Probedurchgangs sowie ab der ersten Runde des Haupteils steht Ihnen zusätzlich ein Startvermögen zur Verfügung. Das verfügbare Geld wird in EW angegeben. Achtung: Sie können keine Ersparnisse aus dem Probedurchgang in den Hauptteil des Experiments übertragen. Verfügbares Geld = Erspartes + aktuelles Einkommen

Aktuelles Einkommen

In jeder Runde erhalten Sie ein aktuelles Einkommen. Die Höhe des aktuellen Einkommens ist zufällig bestimmt und variiert in jeder Runde. Im Durchschnitt beträgt das aktuelle Einkommen ungefähr 100 EW. Wie hoch das aktuelle Einkommen in jeder Runde genau ist, wird Ihnen zu Beginn jeder Runde angezeigt.

Vermögen

Ab der ersten Runde des Probedurchgangs sowie ab der ersten Runde des Haupteils steht Ihnen ein Vermögen zur Verfügung. Ihr Vermögen ist Teil ihres verfügbaren Geldes. Sie können es also genau wie das restliche verfügbare Geld ausgeben oder sparen. Da Sie einen armen Haushalt repräsentieren, beträgt ihr Vermögen 0 EW.

Informationen in jeder Runde

In jeder Runde werden Erspartes, aktuelles Einkommen und das daraus resultierende verfügbare Geld in einer Tabelle angezeigt. Auch die Werte der vergangenen Runden sind in der Tabelle für Sie sichtbar.

Ihre Entscheidung

Ihre Entscheidung, wie viel Ihres verfügbaren Geldes Sie in der Runde ausgeben wollen (und damit indirekt auch wie viel Sie sparen wollen), tragen Sie im Feld "Ihre Entscheidung" ein. Der EW-Betrag, den Sie in jeder Runde ausgeben, wird in Eurocent umgerechnet. Die Summe der EW-Beträge des Hauptteils wird am Ende des Experiments an Sie ausgezahlt.

Umrechnungstabelle

Wie viel Eurocent Sie für einen bestimmten EW-Betrag bekommen, können Sie auf dem Bildschirm mit dem Umrechnungskalkulator probeweise berechnen. Dabei gilt, je mehr EW Sie ausgeben, desto mehr Eurocent erhalten Sie. Die Umrechnung der Ausgaben in Eurocent wird auch von der Rundenzahl beeinflusst. Je höher die Rundenzahl, desto weniger Eurocent bekommen Sie für einen bestimmten Ausgabenbetrag. Achtung! Die Werte in der Umrechnungstabelle sind zur Übersichtlichkeit auf ganze Zahlen gerundet. Im Spiel werden die Werte jedoch nicht gerundet. Erst ganz am Ende wird der Betrag in Eurocent, den Sie erspielt haben, auf das nächsthöhere Vielfache von 10 Eurocent gerundet. Der Umrechnungskalkulator zeigt die Werte mit vier Nachkommastellen genau an.

Umrechnungstabelle Runde 1 bis 10

	Runde:									
	1	2	3	4	5	6	7	8	9	10
Ausgaben in EW:	Verdienst in Eurocent:									
0	0	0	0	0	0	0	0	0	0	0
20	21	20	19	18	17	16	15	15	14	13
40	42	40	38	36	34	32	31	29	28	26
60	63	60	57	54	51	49	46	44	42	40
80	84	79	75	72	68	65	61	58	55	53
100	105	99	94	90	85	81	77	73	69	66
120	125	119	113	108	102	97	92	88	83	79
140	146	139	132	125	119	113	108	102	97	92
160	167	159	151	143	136	129	123	117	111	105
180	188	179	170	161	153	146	138	131	125	119
200	209	199	189	179	170	162	154	146	139	132
220	230	218	207	197	187	178	169	161	153	145
240	251	238	226	215	204	194	184	175	166	158
260	272	258	245	233	221	210	200	190	180	171
280	293	278	264	251	238	226	215	204	194	184
300	314	298	283	269	255	243	230	219	208	198
320	334	318	302	287	272	259	246	234	222	211
340	355	338	321	305	289	275	261	248	236	224
360	376	357	340	323	306	291	277	263	250	237
380	397	377	358	340	323	307	292	277	263	250
400	418	397	377	358	340	323	307	292	277	263
420	439	417	396	376	357	340	323	307	291	277
440	460	437	415	394	375	356	338	321	305	290
460	481	457	434	412	392	372	353	336	319	303
480	502	477	453	430	409	388	369	350	333	316
500	522	496	472	448	426	404	384	365	347	329
1000	1045	993	943	896	851	809	768	730	693	659
1500	1568	1489	1415	1344	1277	1213	1152	1095	1040	988
2000	2090	1986	1886	1792	1702	1617	1536	1460	1387	1317
3000	3135	2978	2829	2688	2553	2426	2305	2189	2080	1976
...	...	...	...	...	...	...	...	...	...	...

Entscheiden Sie sich also beispielsweise in Runde 7 für einen Ausgabenbetrag von 100 EW, erhalten Sie dafür 77 Eurocent.

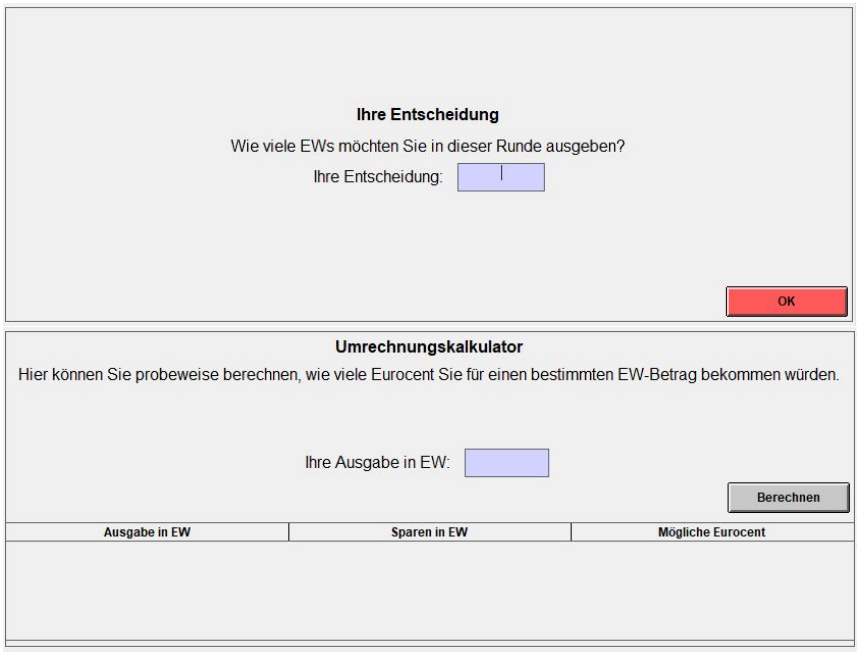
Umrechnungstabelle Runde 11 bis 20

	Runde:									
	11	12	13	14	15	16	17	18	19	20
Ausgaben in EW:	Verdienst in Eurocent:									
0	0	0	0	0	0	0	0	0	0	0
20	13	12	11	11	10	10	9	9	8	8
40	25	24	23	21	20	19	18	17	17	16
60	38	36	34	32	31	29	28	26	25	24
80	50	48	45	43	41	39	37	35	33	32
100	63	59	56	54	51	48	46	44	42	39
120	75	71	68	64	61	58	55	52	50	47
140	88	83	79	75	71	68	64	61	58	55
160	100	95	90	86	82	77	74	70	66	63
180	113	107	102	97	92	87	83	79	75	71
200	125	119	113	107	102	97	92	87	83	79
220	138	131	124	118	112	107	101	96	91	87
240	150	143	136	129	122	116	110	105	100	95
260	163	155	147	139	133	126	120	114	108	103
280	175	166	158	150	143	136	129	122	116	110
300	188	178	169	161	153	145	138	131	125	118
320	200	190	181	172	163	155	147	140	133	126
340	213	202	192	182	173	165	156	149	141	134
360	225	214	203	193	183	174	166	157	149	142
380	238	226	215	204	194	184	175	166	158	150
400	250	238	226	215	204	194	184	175	166	158
420	263	250	237	225	214	203	193	184	174	166
440	275	262	248	236	224	213	202	192	183	174
460	288	273	260	247	234	223	212	201	191	181
480	300	285	271	257	245	232	221	210	199	189
500	313	297	282	268	255	242	230	218	208	197
1000	626	594	565	536	510	484	460	437	415	394
1500	939	892	847	805	764	726	690	655	623	592
2000	1251	1189	1129	1073	1019	968	920	874	830	789
3000	1877	1783	1694	1609	1529	1452	1380	1311	1245	1183
...	...	...	...	...	...	...	...	...	...	...

Entscheiden Sie sich also beispielsweise in Runde 18 für einen Ausgabenbetrag von 320 EW, erhalten Sie dafür 140 Eurocent.

Ablauf der Runden

Wie schon beschrieben, müssen Sie in jeder der 20 Runden eine Entscheidung treffen, wie Sie Ihr verfügbares Geld zwischen ausgeben und sparen aufteilen. Sobald Sie sich einmal für eine Aufteilung entschieden haben und die Runde verlassen haben, können Sie diese Entscheidung nicht mehr ändern. Sie können also nicht mehr in vergangene Runden zurückkehren. Das folgende Bild zeigt, wie Ihr Entscheidungsbildschirm im Experiment aussieht.



Ihre Auszahlung

Die Runden des Probedurchgangs sind nicht auszahlungsrelevant. Erst die Entscheidungen im Hauptteil des Experiments bestimmen die Auszahlung. Nach Ablauf der 20 Runden des Hauptteils werden alle Eurocent, die Sie durch Ihre Ausgaben in den einzelnen Runden des Hauptteils gesammelt haben, addiert. Diese Summe bekommen Sie am Ende des Experiments bar ausgezahlt.

Verständnisfragen

Bevor der Probedurchgang beginnt, beantworten Sie eine Reihe von Verständnisfragen am Bildschirm. Das Experiment beginnt erst, wenn Sie alle Fragen korrekt beantwortet haben. Diese Fragen haben keinen Einfluss auf Ihre Auszahlung. Sollten Sie Fragen zu den Instruktionen haben, melden Sie sich bitte per Handzeichen. Ein Mitarbeiter kommt dann zu Ihnen und beantwortet Ihre Fragen.

[a]

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Chapter 4

Are the Poor more Impatient than the Rich? Experimental Evidence on the Effect of (Lab) Wealth on Intertemporal Preferences

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Jan Siebert[1]

Are the Poor More Impatient Than the Rich?

Experimental Evidence on the Effect of (Lab) Wealth on Intertemporal Preferences

Abstract

Poor people have, on average, a higher marginal propensity to consume. One (out of many) possible explanations for this is that poverty affects impatience. This would have important implications for monetary and fiscal policy. While some macroeconomists simply assume lower individual discount factors for poorer households, little is known about this phenomenon from a behavioural point of view. This paper presents a laboratory experiment to test whether the poor show more impatient behaviour. In the experiment, half of the participants gets a high participation fee, while the other half gets a low participation fee. All participants perform an intertemporal multiple price list task. The participation fee has a significant effect. Surprisingly, participants with a lower participation fee are less impatient.

JEL-Code: C9, D9, E2

Keywords: Intertemporal preferences, patience, saving, consumption, experiments

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"A poor man is more impatient than a rich man of the same personal characteristics."

(Fisher, 1930, p.83)

1 Introduction

A good understanding of intertemporal preferences is indispensable for monetary and fiscal policy. Numerous studies show that poor people spend a greater proportion of an income shock than rich people (see e.g. Jappelli and Pistaferri, 2010; Lawrance, 1991). Theories to explain that frequent observation are diverse. Different explanations reach from liquidity constraints in combination with high impatience (see e.g. Deaton, 1991), or selection effects, i.e., impatient people become poor (see e.g. Golsteyn et al., 2014), or low possibilities for self-punishment (Bernheim et al., 2015), to over-proportional consumption of temptation goods like alcohol and fat (Banerjee and Mullainathan, 2010). One possible further explanation is that poverty affects intertemporal preferences (see e.g. Fisher, 1930).

The last explanation would have important implications for monetary and fiscal policy. In reality, it is difficult to distinguish between causal effects and selection effects. Therefore, a laboratory experiment under controlled conditions is a good way to test whether the poor show more impatient behaviour. A simple laboratory experiment is also a good starting point before testing this question in more complex experiments in the field.

A few earlier experiments, which actually had a different intention, paid different amounts to participants and then measured their intertemporal preferences. These data can be used to examine whether the poor (participants who earned less in the previous experiment) show more impatient behaviour than the rich (participants who earned more). Analysing data of different experiments reported in Andersen et al. (2006, 2008, 2014)[1] and comparing the intertemporal individual discount rate of the poor to that of the rich, reveals no significant difference between the two (See Appendix A). However, the participants in these experiments have to perform a variety of tasks whose effects may outweigh the effect of the payments.

This paper presents a laboratory experiment to test solely whether the poor show more impatient behaviour. Participants receive different amounts of participation fees and perform an intertemporal allocation task. Different participation fees are an external random microscopic change in participants real life wealth. The complex social, physical and psychological conditions of being poor are not reproduced in the laboratory. This study examines only the effect of a microscopic change in wealth. Earlier experiments use small payments to test their effects on risk aversion under different conditions and establish the term

[1] Andersen et al. (2006) and Andersen et al. (2008) describe two parts of the same experiment. The first part (described in Andersen et al. (2006)) was conducted in laboratories, the second part (described in Andersen et al. (2008)) was a field experiment.

'lab wealth' (see e.g. Cox and Sadiraj, 2009; Cox et al., 2013; Harrison et al., 2017; Andersen et al., 2018). To measure intertemporal preferences, this study uses a simple version of the multiple price list task designed amongst others by Andersen et al. (2008). Participants must decide between an earlier (three months after the experiment) lower payment or a higher later payment (six months after the experiment). This design gives a good comparison value for their impatience.

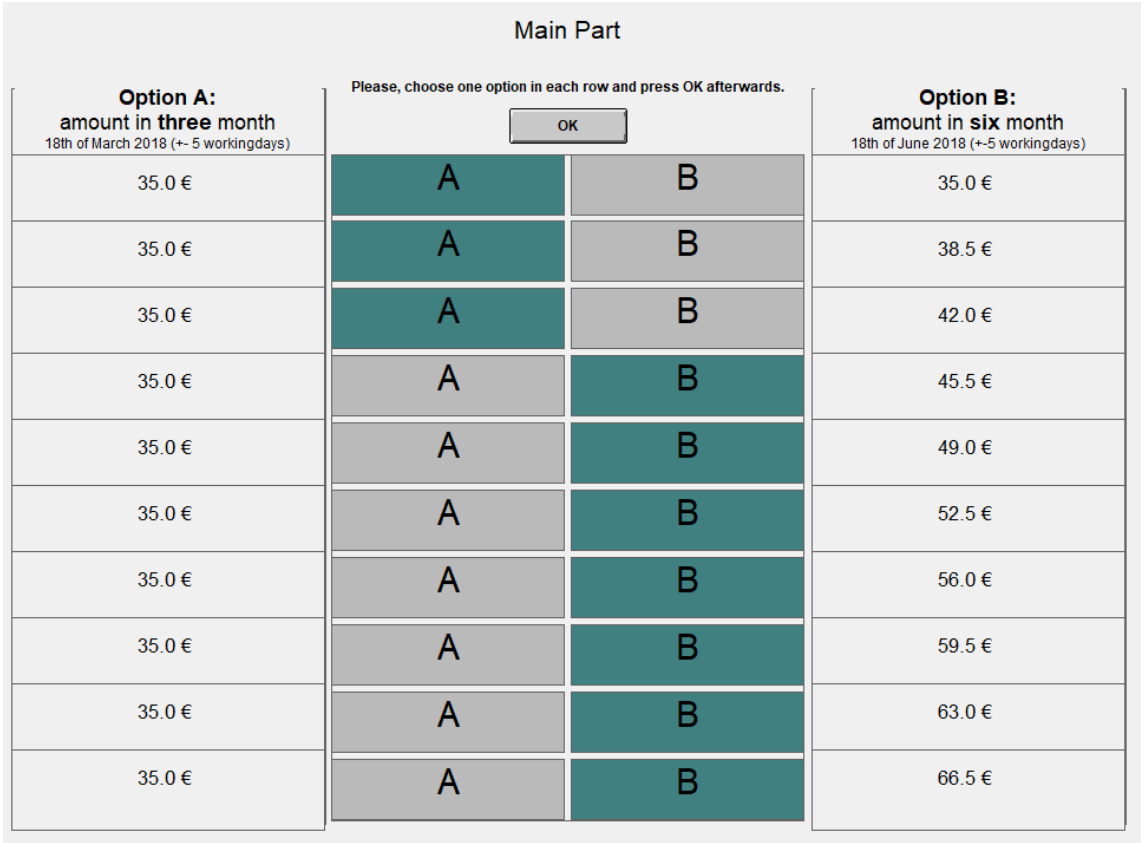
The results of this experiment contradict the above-mentioned theory. Participants with lower lab wealth show less impatient behaviour. The difference between both groups is significant. The rest of this paper is as follows. Section 2 explains the experimental design. Section 3 presents the results and section 4 discusses the results in context of previous research.

2 Experimental Design

The experiment was conducted in the Essen Laboratory for Experimental Economics (elfe), using zTree (Fischbacher, 2007) and ORSEE (Greiner, 2015). The participants were 59 students of the University of Duisburg-Essen from different subjects (average age 23.9, 20 men and 39 women). The experiment lasted less than 60 minutes. The average earning was 16 Euros. One half of the participants of each session received high lab wealth, while the other half received low lab wealth.

The course of events was as follows: After entering the laboratory, the participants were randomly assigned to separate computer workstations. Then, they read instructions, which were placed in the workstations (see Appendix B for a translated version of the instructions). Participants' questions were answered in private. The instructions begin with information about the participation fee. The participation fee was either 5 Euro (lower lab wealth), or 20 Euro (higher lab wealth). This was determined by the random draw of the workstation at the beginning of the experiment. Each participant was just informed about their own participation fee. In order to make sure that participants were aware of the amount of their participation fees, they were asked explicitly for it, as the first and the last of eight comprehension questions. The other six questions were about the intertemporal multiple price list task. A participant could start the experiment only after they answered all questions correctly (see Appendix C for all questions). The evaluation of the intertemporal preferences followed. A simple version of the multiple price list task was used. Participants had to choose between two payment options in ten different decision rows. Option A was a payment of 35 Euros in three months after the experiment in each decision row. Option B was a payment of 35 Euros in six months after the experiment in the first row and increased by 3.5 Euros per decision row. The relatively long front-end delay of three months has the following reason. It is to avoid measuring time inconsistency instead of impatience. See Figure 1 for a screenshot of the multiple price list task. Only one switching point between Option A and Option B was allowed.

Figure 1: Screen Shot of the Intertemporal Multiple Price List Task



Main Part

Please, choose one option in each row and press OK afterwards.

OK

Option A: amount in three month (18th of March 2018 (+- 5 workingdays))			Option B: amount in six month (18th of June 2018 (+-5 workingdays))
35.0 €	A	B	35.0 €
35.0 €	A	B	38.5 €
35.0 €	A	B	42.0 €
35.0 €	A	B	45.5 €
35.0 €	A	B	49.0 €
35.0 €	A	B	52.5 €
35.0 €	A	B	56.0 €
35.0 €	A	B	59.5 €
35.0 €	A	B	63.0 €
35.0 €	A	B	66.5 €

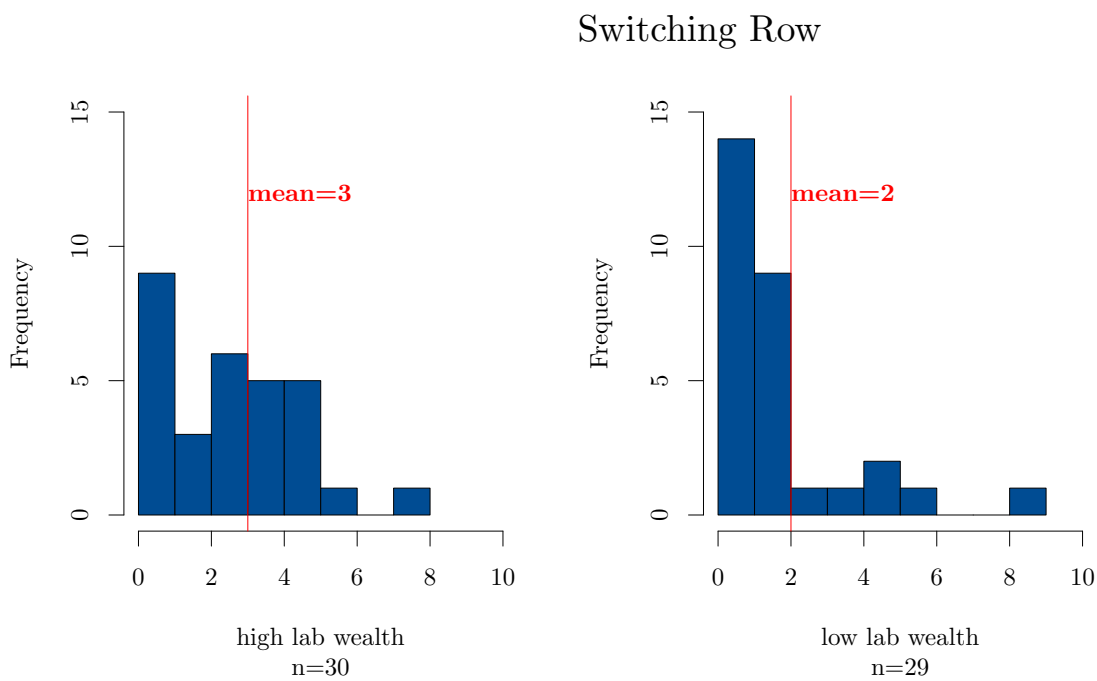
The experiment took place in December, so future payments were either in March or in June (not in high consumption Christmas season). As a test before the actual task, the participants could play one-time payment irrelevant through the task. A first random draw decided *which* decision row would be realized (each row had a 10 percent chance) and a second random draw decided *if* the payment was realized (each participant had an independent chance of $1/16 = 6.25$ percent that any future payment was realized). Participants that were chosen by the random draw to receive a future payment (in fact four participants) were asked for information about their banking accounts to transfer at the specific dates. The administration of the University of Duisburg-Essen transferred the money to the chosen participants three months or six months after the experiment. The participants were informed that the payment could be within five working days before or after the specified date.

After the multiple price list task, the participants had to answer a questionnaire. They were asked for demographics, self-assessment of personality traits, and financial situation. Table 2 in Appendix D shows participants demographics. Column four of Table 2 reports the standard deviation. We do not find significant differences in any of the demographics between both groups.

3 Results

Participants switch on average after 2.51 rows to Option B. Mean switching row of participants with 5 Euros participation fee is 2. Participants with a participation fee of 20 Euros have on average a switching row of 3. See Figure 2 for a comparison between both groups.

FIGURE 2: The Row in which Participants Switch from Option A to Option B



A two-sided Mann-Whitney U test reveals significant differences between both groups (p-value 0.025). The null hypothesis that there are no differences between both groups can be rejected. However, testing against the alternative that participants with lower lab wealth switch *later* from Option A to Option B – the poor are more impatient, – the p-value is 0.988. To conclude, low lab wealth makes participants significantly *less* impatient.

The result of the experiment suggests that lab wealth affects behaviour of participants. This again suggests that the underlying (marginal) latent preferences of participants are affected as well. However, it is not obvious which latent variables are affected and how they are affected. In the following, I will try to illuminate the space of possible changes in latent preferences, following Andersen et al. (2008).

Assuming that individuals discount future payments exponentially, an individual is indifferent between an earlier income option M_t and a later income option $M_{t+\tau}$, if

$$(\omega + M_t) + \frac{1}{(1+\delta)^\tau}\omega = \omega + \frac{1}{(1+\delta)^\tau}(\omega + M_{t+\tau}). \tag{1}$$

Where ω is a measure of background consumption[2], δ is the discount rate, t is the delivery date of the sooner payment option, $t + \tau$ is the delivery date of the later payment option. Without any further assumptions, we must conclude that lab wealth has an effect on the latent variable δ. However, it is a common assumption that utility of individuals does not increase linearly with income. The most common way to model this is a Constant Relative Risk Aversion (CRRA) utility function

$$U(M) = \frac{(\omega + M)^{(1-r)}}{(1-r)} \tag{2}$$

for $r \neq 1$. The CRRA coefficient r indicates risk averse preferences if $r > 0$ and risk loving behaviour if $r < 0$. Based on these two simple assumptions (risk aversion and exponential discounting), it is not possible to clearly state which of the latent variables (r and δ) was changed by the payment in the laboratory. I used the maximum-likelihood estimation procedure like Andersen et al. (2008) to estimate which latent variable is most likely. First, I assume that r is unaffected. Then I assume that δ is unaffected.

Assuming an unaffected r of 0.79[3], I find $\delta = 0.65$ for the whole sample, $\delta = 0.44$ for the participants with show up fee of 5 Euro, $\delta = 0.82$ for the participants with a show up fee of 20 Euro. Assuming, on the other hand, an unaffected δ of 0.65, the whole sample has an $r = 0.81$, the 5 Euro participants have an $r = 0.23$, the 20 Euro participants have an $r = 0.89$. It is not possible to say whether impatience (δ) has changed or whether risk aversion (r) has changed. Of course, it is also possible that both (r and δ) have changed in the same direction or one in one and the other in the other direction. Andersen et al. (2008) show how both aspects interact with each other. It also cannot be ruled out that individuals make their decisions based on a different, perhaps more complex, utility function. However, based on the experiment we can say that poor people (participants with less lab wealth) *behave* less impatiently.

4 Discussion

This paper presents an experiment to test whether the poor are more impatient than the rich. The simple experiment focuses solely on answering that single question, to avoid many tasks within the experimental session. Possible influence of other decisions or payments is thus excluded. This paper finds clear evidence that the poor are not more impatient, but *less* impatient.

However, the difference is not significant in earlier experiments that deal with other research questions but can nevertheless be used to approach the above question. This obvious discrepancy suggests that the

[2]In lack of data for German students, I followed Andersen et al. (2008) and set $\omega = 118$ DKK $= 15.79$ Euro. That is probably a bit too high for German students. Therefore, I checked results with an ω half of the size. It reduces the whole sample δ to 0.53 and the whole sample r to 0.61. However, the comparative statistics hold.

[3]Andersen et al. (2006) found this value in a sample of students which is roughly comparable to ours.

circumstances play a major role.

Ifcher and Zarghamee (2011) as well as Drichoutis and Nayga (2013) test whether "induced mood" affects intertemporal and risk preferences. Ifcher and Zarghamee (2011) affect the mood of participants by showing them short video clips. They find that positive mood makes participants more patient. Similarly, Drichoutis and Nayga (2013) test the effect of mood on financial decisions. They trigger different moods by giving participants different math tasks. Difficult math tasks together with the information that the participant performed below average should trigger negative mood. Easy math tasks together with the information that participant performed above average, should trigger positive mood. They find that both – positive emotions and negative emotions – decrease impatience. Both experiments suggest that it is not irrelevant whether payments in experiments are lucky profits or whether participants are paid for appearing and know nothing about whether someone else receives more or less.

In summary, explanations for the divergent evidence could be: 1. Lower wealth could make impatience less affordable and thus decreases impatience (income effect); 2. Lower wealth could trigger a negative mood which decreases the current value of future consumption which increases impatience (mood effect). The circumstances determine which effect dominates. That poor households have a higher propensity to consume can therefore only be explained if the mood effect clearly exceeds the income effect.

This study cannot find support for Fisher's above-mentioned assertion – a poor man is more impatient than a rich man. However, this study is limited to examining only a microscopic change in wealth and is not able to reproduce the complex social, physical and psychological conditions of poverty. To conclude, the income effect must be considered *together* with many other aspects, such as emotional conditions, in order to make a robust statement about impatience of the poor.

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A Comparison of Experiments

Table 1: Comparison of the Experiments

n	n lw	n no lw	idr all	idr lw	idr no lw	cor lw idr	cor test	MWU p
My Experiment (Lab Wealth by Show Up Fee)								
59	30	29	8.43	9.88	6.94	0.2	0.12	0.03**
Andersen et al. (2006) (lab part of the experiment; lab wealth by RA task)								
90	7	83	36.23	42.08	35.73	0.03	0.76	0.6
Andersen et al. (2006) (lab part of the experiment; lab wealth by endowment)								
90	90	0	36.23	36.23	–	-0.07	0.51	–
Andersen et al. (2008) (field part of the experiment; lab wealth by RA task)								
253	22	231	14.05	12.5	14.2	-0.05	0.44	0.32
Andersen et al. (2014) (field part of the experiment; lab wealth by RA task)								
413	20	393	36.09	35.12	36.14	-0.06	0.23	0.78
Andersen et al. (2014) (field part of the experiment; lab wealth by show up fee)								
413	208	205	36.09	36.69	35.49	0.03	0.6	0.58
Andersen et al. (2014) (field part of the experiment; lab wealth by show up fee + RA task)								
413	219	194	36.09	36.6	35.52	-0.05	0.28	0.63
Andersen et al. (2014) (field part of the experiment; lab wealth by RA task, only RA first)								
229	20	209	37.55	35.12	37.78	-0.1	0.14	0.53
Andersen et al. (2014) (lab part of the experiment; lab wealth by RA task)								
88	8	80	36.46	38.44	36.26	-0.06	0.6	0.83

$p < 0.1$*; $p < 0.05$**; $p < 0.01$***

The first column (n) shows the number of (independent) participants. In the second column (n lw) is the number of participants who got (high) lab wealth. The number of participants without (high) lab wealth is in column 3 (n no lw). The average individual discount rate assuming risk neutral individuals (idr) of all participants is in column 4 (idr all). The average idr of participants with (high) lab wealth is in column 5 (idr lw) and the average idr of participants without (high) lab wealth is in column 6 (idr no lw). The correlation coefficient between lab wealth and the idr is in column 7 (cor lw idr). The p-value of a Pearson correlation test between lab wealth and the idr is in column 8 (cor test). The p-value of a Mann Whitney U test comparing the idr between the group with (high) lab wealth and the group without (high) lab wealth is in column 9 (MWU p).

B Instructions

Welcome to the experiment!

You take part in a study of decision-making behaviour within the framework of experimental economic research. During the investigation, you will be asked to make decisions. You can earn money doing it. How much money that will be depends on various factors. Detailed instructions are given below.

Basic Payment

Irrespective of the further course of the experiment, you will receive a basic payment of **5 EUROS [20 EUROS]**. This amount will be paid to you in cash at the end of the experiment. Therefore, please remain seated at your seat after the experiment until your seat number is called.

Comprehension Questions

Before starting the actual experiment, you will be asked 8 comprehension questions and you will go through a trial run. The comprehension questions and the trial run have no effect on your payment. They only serve the purpose of understanding and have no effect on the further course of the experiment. Should you have any questions before the start of the main part of the experiment, please contact a member of the laboratory staff by hand signal. He will then come to your place and help you. No more questions can be answered during the experiment. Any communication with the other participants is not permitted during the experiment. Violation of this rule will result in immediate exclusion from the experiment.

Main Part

In the main part of the experiment you will be asked to make several decisions. You will be presented with a list of 10 decision options. For this list you make 10 decisions by choosing between payment Option A and payment Option B. These payment options represent potential transfers to you in the future. If you choose **payment Option A** and the corresponding decision option is randomly selected and randomly realized, the corresponding amount will be transferred to you in **three months**, i.e. on 18 March 2018. If you choose **payment Option B** and the corresponding decision option is randomly selected and randomly realized, the corresponding amount will be transferred to you in **six months**, i.e. on 18 June 2018. However, the payment may be delayed by up to five working days. Whether and which decision possibility is realized, depends on two random generators. The first random generator selects one of the ten decision options. After the main part of the experiment, when you have made all ten decisions, one of your decisions from the main part of the experiment is randomly drawn by the computer and displayed on your screen. The probability of being selected is the same for all ten decision options.

It is therefore 1/10 or 10%. Whether the randomly selected decision option is actually made with the corresponding payment option selected by you, depends on the second random number generator. The probability that this payment will actually be made is 1/16, i.e. 6.25%. Whether the payment is actually made or not, is shown on the screen. If you are notified that your selected payment option is actually being made, you will automatically be asked for your IBAN and address. The amount will then be transferred to your account at the specified time. The transfer is carried out by the Finance Department of the University of Duisburg-Essen. Your address is required for the transfer to be made by the University of Duisburg-Essen. We will not use your personal information for purposes other than processing the transaction.

The decision situation is described in more detail below. A list is displayed on your screen. In this list, you can choose between two payment options in each row. The difference between the two disbursements lies in the amount and the time of disbursement. The example illustrates a corresponding decision situation:

Main Part

Please, choose one option in each row and press OK afterwards.

OK

Option A: amount in three month 18th of March 2018 (+- 5 workingdays)			Option B: amount in six month 18th of June 2018 (+-5 workingdays)
35.0 €	A	B	35.0 €
35.0 €	A	B	38.5 €
35.0 €	A	B	42.0 €
35.0 €	A	B	45.5 €
35.0 €	A	B	49.0 €
35.0 €	A	B	52.5 €
35.0 €	A	B	56.0 €
35.0 €	A	B	59.5 €
35.0 €	A	B	63.0 €
35.0 €	A	B	66.5 €

The payment time described indicates when you will receive the potential payment. You therefore choose between a potential payment of the amount of Option A at an earlier date and a potential payment of the amount of Option B at a later date. The amount of the later payment increases as you continue to

go down in the list of decision options.

For each of the ten choices, click A or B to make a choice between the two. You have the following options:

- You can choose A in all rows
- You can choose B in all rows
- You can choose A in one or more rows and then change to B.

If you change from A to B in a row, the program also automatically selects B for the following rows. But you can still change the decision. After you have chosen one of the two options in each of the ten rows, please confirm your choice with "OK".

Payment Modalities

Once you have made your decision, the computer randomly selects one of the ten options. Depending on your decision, Option A or Option B of the selected decision option is the potential payment option for you. Whether or not this payment is actually made depends on the second random draw. After the random draws happened, a questionnaire opens. Only after all participants have answered the questionnaire completely, the payment of the basic payment of **5 EUROS [20 EUROS]** in cash begins. After completing the questionnaire, please remain seated until your cabin number is called.

C Comprehension Questions

- Question 1: How high is the basic payment, which you receive in cash regardless of the main part of the experiment, in Euro?
- Question 2: In how many months is the earlier payment date of the main part?
- Question 3: In how many months is the later payment date of the main part?
- Question 4: What is the probability (in percent) that one of the ten decision options will be selected?
- Question 5: What is the probability (in percent) that a selected payment option will be executed?
- Question 6: How often is it possible to switch between Option A and Option B?
- Question 7: How many trials are there before the main part?
- Question 8: How high is the basic payment, which you receive in cash regardless of the main part of the experiment, in Euro?

D Characteristics of Participants

Table 2: Characteristics

	high lab wealth	low lab wealth	all	sd
n	30	29	59	
female	22	17	39	0.48
age	22.7	25.1	23.9	4.4
semester	5.1	6.4	5.7	4.2
school	2.4	2.5	2.4	0.7
(Home) Wealth	516.7	615.4	562.5	294.8
Savings	66.8	36	52.8	74.6

Chapter 5

Coordination Problems Triggered by Sunspots in the Laboratory

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Coordination Problems Triggered by Sunspots in the Laboratory

Abstract

A sunspot variable is any random variable that is not related to fundamental factors of the economy but a potential coordination device. The coordination power of sunspots has been analysed in theory and in experiments. However, some have discussed whether sunspots, e.g., public announcements such as financial market ratings, can create coordination problems. That discussion reached a new peak during the European sovereign debt crisis. We ask: can a sunspot variable, in form of a random forecast, trigger coordination problems? To answer that, we use a repeated three-player stag hunt game with fixed groups. In our experiment, a sunspot variable points randomly at the risk-dominant or the payoff-dominant choice. We find out-of-equilibrium behaviour caused by the sunspot variable in the short run. In the long run, the sunspot variable can lead to coordination on payoff-dominated equilibria. Only if the sunspot variable points more often to the payoff-dominated alternative, some groups use the sunspot variable consistently as a coordination device.

JEL-Code: C92, C72, D81, E40, J52

Keywords: Sunspot; coordination; equilibrium selection; correlated equilibria; focal point

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1 Introduction

The term "sunspot" can be traced back to Jevons (1878) because he mistakenly believed that solar activity drives the business cycle. In modern macroeconomic parlance, a sunspot variable is any random variable that is not related to fundamental factors (Farmer, 1999).[1] That variable can be a coordination device in a situation where agents face multiple equilibria. A public announcement can be a sunspot variable (e.g., economic forecasts, financial market ratings, or communicated inflation rate targets of central banks). Such an announcement can be a self-fulfilling prophecy, even if its informative value about fundamentals is zero. The coordination power of sunspot variables has been analysed in theory and in experiments. Recently, Fehr et al. (2019) have shown the coordination power of sunspots in the laboratory. They conclude: "Salient public messages can indeed change beliefs and behaviour in the desired direction, even if they are not backed by a commitment to actions affecting fundamentals. However, in a world of public and private messages, the power of public messages may be lower and adding public signals to existing private signals may even reduce welfare."

The latter aspect, namely the welfare reduction or coordination *problems* caused by a sunspot variable, is the focus of the paper at hand. Cole and Kehoe (2000) present a model where self-fulfilling debt crises are possible. Krugman (1996) explains in their model, "a crisis can occur depending on the realization of a random event that is extrinsic to the fundamentals of the model, a sunspot variable. An unfavorable realization of this sunspot variable can lead to a panic[...]." Some discuss whether financial market ratings are such destabilizing variables (see, e.g., Kaminsky (2002), Andritzky et al. (2007), Gärtner and Griesbach (2012), or Alsakka and ap Gwilym (2013)). To some degree, destabilizing effects of financial market ratings are spillovers due to costly information and, therefore, have nothing to do with sunspots (see, e.g., Calvo and Mendoza (2000)). However, Böninghausen and Zabel (2015) show that the informative value of the ratings is not the only force behind the spillovers: "Strikingly, this [the spillover effect] cannot be explained by fundamental linkages and similarities between countries." A financial market rating in the form of a forecast for country x that has no informative value about country y can be seen as a sunspot variable for country y. In reality, it is difficult to measure the real informative value of a forecast. However, in the laboratory, it is easy to generate a random, information-free forecast.

We design a laboratory experiment to answer the question: **Can a sunspot variable, in form of a random public forecast, trigger coordination problems?**

We use a three-player stag hunt game. It has a payoff-dominant equilibrium (Equilibrium A), in which all players use the cooperative choice, and a divergent risk-dominant equilibrium (Equilibrium B), in which all players choose the safe option. Coordination problems in the game are either unequal actions

[1] The theory of sunspots (see, e.g., Cass and Shell (1983)) is closely related to the theory of correlated equilibria (Aumann, 1987) and to the focal point theory (Schelling, 1980). The term "sunspot" is related to dynamic stochastic general equilibrium models, while the theory of correlated equilibria is part of game theory.

(no equilibrium) or coordination on the payoff-dominated equilibrium (inefficient equilibrium). We use fixed groups to also show whether sunspot variables *delay* convergence. A blog entry of Krugman (2011) illustrates the parallels between a stag hunt game and a sovereign debt crisis. He explains: "Equilibrium A is where investors don't believe you will default, so interest rates are low, so you don't. Equilibrium B is where investors believe you will, so rates are high, so you do." However, to our knowledge, there is no experimental paper that focuses on coordination problems caused by a sunspot variable in a stag hunt game. Using this game, we can see whether a sunspot variable triggers out-of-equilibrium-behaviour, but also whether it triggers coordination on payoff-dominated equilibria.

The sunspot variable in our experiment is semantically a forecast (either "the majority will choose strategy A" or "the majority will choose strategy B"), but it is random (determined by rolling a die) and non-binding, which is common knowledge. We use the term "sunspot" for the announcement because we interpret our results from a macro perspective (like, e.g., Fehr et al. (2019) or Beugnot et al. (2012)). However, our experiment is not a general equilibrium experiment (like, e.g., Arifovic et al. (2013)). It belongs to the class of experiments with recommended strategies (like, e.g., Cason and Sharma (2007)) or correlated equilibria (like, e.g., Duffy et al. (2017)).

We vary the random sunspot-generating process to change the risk and the payoff related to the coordination on the sunspot variable. There is no sunspot variable in the control treatment. In the so-called neutral treatment, the sunspot variable points with an equal probability to the payoff-dominant strategy and to the risk-dominant strategy. In the so-called negative treatment, the sunspot variable points with a higher probability to the risk-dominant strategy than to the payoff-dominant strategy.

We find that the sunspot variable dissuades people from choosing the payoff-dominant strategy. Furthermore, the sunspot variable delays the convergence to an equilibrium. The groups with sunspot variables converge more often to a payoff-dominated equilibrium than the control groups. We observe convergence to the sunspot equilibrium only in the negative treatment.

The rest of this paper is organized as follows. Section 2 describes our experimental design and provides details about the variation of the treatment parameters. We present the results of our experiment in section 3. Section 4 discusses earlier research and relates our experiment to it.

2 The experimental design

The experiment is computer-based and took place at the "Essen Laboratory for Experimental Economics" (elfe) at the University of Duisburg-Essen.[2] We recruited the participants via ORSEE (Greiner, 2015).

[2]You can find the data, the raw data, the zTree code, the original instructions, and translated instructions of the experiment in Siebert and Yang (2019).

We coded the experiment with zTree (Fischbacher, 2007). A total of 6 sessions with 87 participants were conducted. The participants were mainly undergraduate students from the University of Duisburg-Essen with an average age of 24.15 years. The sessions lasted at most 60 minutes. Average payoff for the participants was 12.66 Euros with a minimum payoff of 3.00 Euros and a maximum payoff of 15.00 Euros.

The participants of the experiment form groups of three. The groups play a repeated stag hunt game over 40 periods. The groups are randomly matched and stay together over all 40 periods. We use a between-subject design, where each participant only participates in one of the treatments. The detailed course of events in the experiment is as follows. On entering the laboratory, the participants are randomly allocated to different workstations. They receive instructions (see Appendix A for the translated instructions) and have the opportunity to ask questions which are answered privately by the experimenter. Once all participants indicate that they understood the instructions, they have to answer a set of four or six control questions[3] which are mainly concerned with the general set-up of the experiment and the payoff rules. After all participants answered the questions correctly, the experiment starts. Each period consists of two stages. In the first stage, participants in the same group receive an identical announcement. The announcement is either "**the majority will choose strategy A**" (A-sunspot; the payoff-dominant strategy) or "**the majority will choose strategy B**" (B-sunspot; the risk-dominant strategy). These announcements are random since they are determined by rolling a die. The experimenter throws the die into an open box in front of all participants in all sessions. The participants are not able to see the die result directly. Instead, the participants see the number on the die via video transmission in the first session. The participants see the taped videos of the first session in the following sessions to keep the results fixed. The participants have to write the number that they see in a dialogue box on their computer screen. Participants receive one of the two announcements according to the different treatments.

In the second stage, the participants play a standard stag hunt game with three players. A participant has to choose between alternative A and alternative B. The participants' payment is based on their decisions and the decisions of the other players in the group. All groups in this experiment receive the same payoff table. Table 1 shows the payoff. In this table, the rows show the participants' decision for A or B and the columns show the decisions of the other players in their group. Each cell shows what a participant will receive depending on their decision and the decisions of the others in the group. If the participant chooses A and if any of the other members of their group choose B, the participant receives 0 Euros, whereas if the participant chooses A and if both of the other players also choose A, the participant receives 12 Euros.

At the end of each period, the participant is informed about their current decision, the current decisions

[3] All participants have to answer four questions; the participants in sunspot treatments have to answer two more questions about the sunspot variables.

Table 1: The three-player stag hunt game

		Other players' decisions in your group		
Your decision		If **BOTH** of the other participants choose A	If **ONE** of the other participants chooses A and the other chooses B	If **BOTH** of the other participants choose B
	A	12	0	0
	B	7	7	7

of their group members, and their payment from that period. Moreover, in each period, the participants can see information from the earlier periods (their decisions, the announcements, the decisions of the others, and their payments). These pieces of information should enable learning from one period to the next and, thereby, convergence to an equilibrium. For the final payoff, one period is randomly chosen from the 40 periods. The participants receive their earnings in that period plus a show-up fee of 3 Euros, as mentioned at the beginning of the experiment.

We use two kinds of random sunspot-generating processes: A neutral random sunspot-generating process, which points with equal probability to the payoff-dominant strategy or to the risk-dominant strategy (neutral treatment), and a negative random sunspot-generating process, which points with higher probability to the risk-dominant strategy than to the payoff-dominant strategy (negative treatment). As a benchmark, we are also interested in the game without a sunspot variable. Therefore, we run a control treatment without sunspots.

The announcements are chosen by rolling a die. In the neutral treatment, the participants see both announcements with a probability of 1/2. The participants in that treatment receive the A-sunspot if the die shows 1, 2, or 3 and the B-sunspot with 4, 5, and 6. In the negative treatment, the participants see the B-sunspot with a probability of 5/6 (if the die shows 2, 3, 4, 5, or 6). Accordingly, the A-sunspot appears with a probability of 1/6 (if the die shows 1).[4] The rules are common knowledge to the participants. Table 2 gives an overview of the different treatments.

Table 2: Treatment overview

Treatment	Number of participants	Number of groups
Control	21	7
Neutral	30	10
Negative	36	12
Total	87	29

[4]We also ran a session with a positive random sunspot-generating process (A-sunspot with probability 5/6; B-sunspot with probability 1/6). While we obtained interesting findings in the neutral treatment and the negative treatment, we did not see any effect in the positive treatment. Therefore, we decided not to run further sessions of the treatment with the positive random sunspot-generating process. We excluded the observations in that treatment from the analyses.

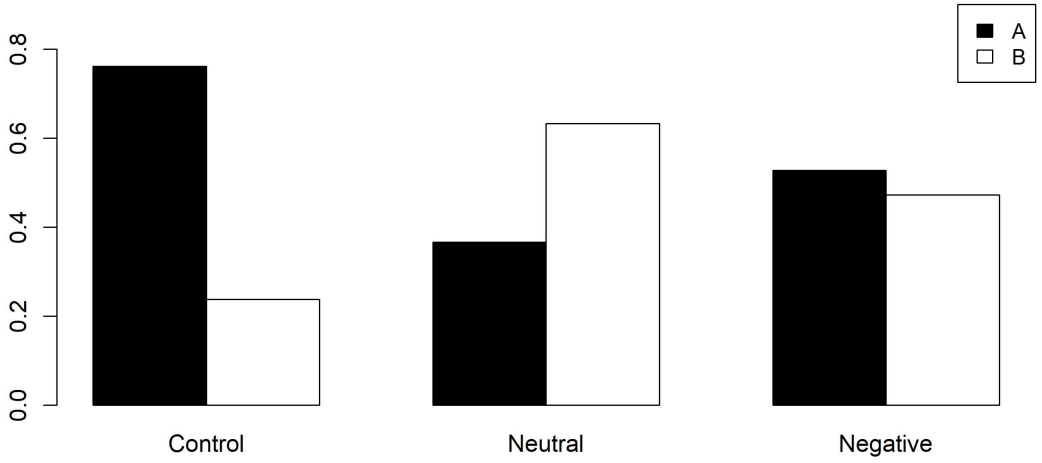

FIGURE 1: Shares of participants choosing A in the first period

3 Results

We organize section 3 as follows. In section 3.1, we discuss the decisions and the equilibria of the first period. In section 3.2, we consider the decisions, equilibria, and earnings of the entire game. We analyse the convergence types in section 3.3.

3.1 The first period

In this subsection, we look at the decisions in the first period. Of the 21 participants in the control treatment, 16 (76%) choose alternative A. Of the 30 participants in the neutral treatment, 11 (37%) choose alternative A. Of the 36 participants in the negative treatment, 19 (53%) choose alternative A. Figure 1 shows the shares of participants choosing A in the first period. Both sunspot treatments differ from the control treatment (Fisher test: control vs. neutral p=0.01; control vs. negative p=0.098). Note that the participants of both treatments see a B-sunspot in the first period.

We also compare the equilibria reached in the first period. In the control treatment, 3 of 7 (43%) groups are in equilibrium. In the neutral treatment, 4 of 10 (40%) groups are in equilibrium. In the negative treatment, all 12 groups fail to reach an equilibrium. The Fisher tests show significant differences between the negative and the control treatment (p=0.04), as well as between the negative and the neutral treatment (p=0.03). However, the neutral treatment and the control treatment do not differ significantly (p=1).

To sum up the observations in the first period, the sunspot variable keeps people from the payoff-dominant outcome. Additionally, the negative sunspot-generating process leads to an increase of out-of-equilibrium behaviour.

3.2 All Periods

Now we consider the entire game. The control groups reach the payoff-dominant equilibrium in 76% of the periods. The neutral groups reach the payoff-dominant equilibrium in 64% of the periods. The groups in the negative treatment choose the payoff-dominant equilibrium in 54% of the cases. However, a two-sided Mann-Whitney U test shows no significant differences (neutral vs. control p=0.26; negative vs. control p=0.15; negative vs. neutral p=0.95). The results for the average payoffs over all periods are similar. The average payoffs over all periods are higher in the control group (10.45 Euros on average) than in the neutral treatment (9.82 Euros) or in the negative treatment (9.08 Euros). However, the differences are not significant (neutral vs. control p=0.22; negative vs. control p=0.12; negative vs. neutral p=0.76).

The picture changes when we focus on the periods in which the sunspot variable points on the risk-dominant choice (B-sunspot). In these periods, the groups in the neutral treatment reach the payoff-dominant equilibrium in 51% of the cases. The groups in the negative treatment reach the payoff-dominant equilibrium in 45% of the periods. We compare the share of periods in which the payoff-dominant equilibrium is reached per group only in periods with a B-sunspot. We look at all periods in the control treatment. Using a two-sided Mann-Whitney U test, we find slight differences (neutral vs. control p=0.08; negative vs. control p=0.06; negative vs. neutral p=0.84). This has an effect on the earnings. The average payoffs per period are 9.16 Euros in the neutral treatment and 8.58 Euros in the negative treatment in the periods with a B-sunspot. We compare the average payoff per group only over periods with a B-sunspot. Again, we look at all periods in the control treatment. A two-sided Mann-Whitney U test shows significant differences in the payoffs (neutral vs. control p=0.13; negative vs. control p=0.046; negative vs. neutral p=0.47). To conclude, (B-) sunspots dissuade people from making the payoff-dominant choice, as we have already seen in the first period. This has a negative influence on the payoffs. However, the differences in the payoffs are stronger in periods with a B-sunspot than over the entire game.

3.3 Convergence

We define convergence as follows: all participants choose the same alternative in each of the last ten periods. If these decisions are always A (always B), we label it "A-convergence" ("B-convergence"). If these decisions always follow the sunspot variable, we label it "sunspot-convergence." If there is at least

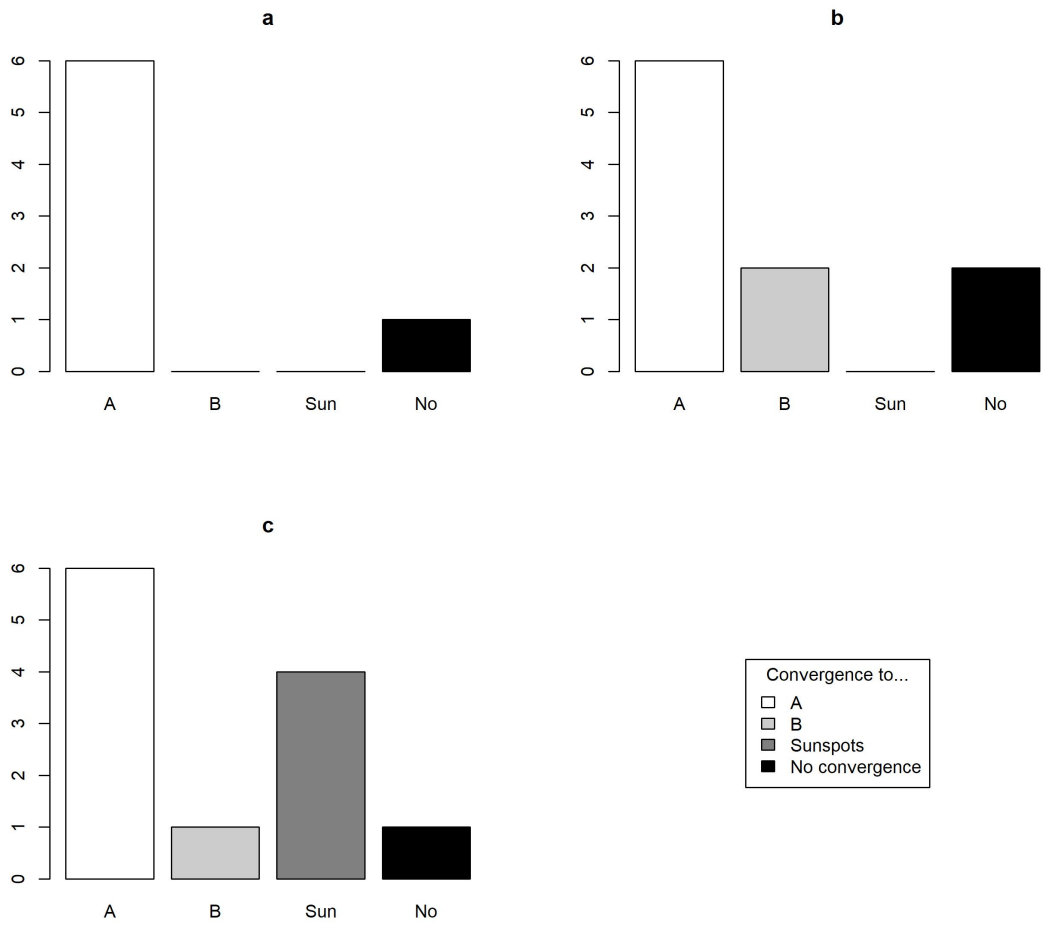

FIGURE 2: The composition of different convergence types among treatments (a: Control treatment; b: Neutral treatment; c: Negative treatment)

one deviation of one participant in the last ten periods, we label it "no convergence."

In the control treatment, six groups reach A-convergence (86%), while one group reaches no convergence (14%). In the neutral treatment, six groups (60%) reach A-convergence, while two groups (20%) reach B-convergence, and two groups (20%) reach no convergence. In the negative treatment, six groups (50%) reach A-convergence, one group (8%) reaches B-convergence, four groups (33%) reach sunspot-convergence, and one group (8%) reaches no convergence. Figure 2 shows the composition of different convergence types.

A Fisher test shows that the neutral treatment has a slight effect on A- or B-convergence (p=0.054) compared to the control treatment. The same is true for the negative treatment. A Fisher test shows that B-convergence is reached slightly more often than A-convergence in the negative treatment than in the control treatment (p=0.054). A Fisher test also shows that the negative treatment makes the sunspot

equilibrium slightly more likely than A-convergence (p=0.08) compared to the neutral treatment.

The differences in the convergence speed are also interesting. The convergence speed is the number of periods needed to achieve accord (the last period with a deviation in a group plus one). Convergence is achieved slightly faster in the control treatment than in the neutral treatment (two-sided Mann-Whitney U test p=0.09) and in the negative treatment (p=0.07). There is no significant difference in the convergence speed between the neutral treatment and the negative treatment (p=0.94).

To conclude, nearly all groups converge in the long run. Although there is coordination in nearly all groups, groups with sunspot variables coordinate more often on the payoff-dominated equilibrium. Only some groups in the negative treatment use the sunspot variable as a coordination device. They coordinate on the payoff-dominated sunspot equilibrium. Additionally, the groups with sunspot variables need more periods to converge. Once more, we can say sunspot variables lead to out-of-equilibrium behaviour in the short run and to coordination on the payoff-dominated equilibria in the long run.

4 Discussion

Many experiments show the coordination power of sunspot variables. Marimon et al. (1993) were the first to run a laboratory experiment to investigate sunspot equilibria. They used an overlapping generation design with a stationary equilibrium and a cyclic equilibrium. They showed blinking squares in red and yellow on the computer screen. Marimon et al. (1993) found that, without training, participants ignored the sunspot variable. In training periods, the experimenter artificially correlated the occurrence of real shocks with the colours. After the training periods, they removed the real shocks. The price fluctuations persisted without a tendency of convergence to the cyclic equilibrium. Although they found some sunspot-influenced behaviour in the laboratory, they did not generate a sunspot equilibrium.

We group the following sunspot experiments roughly into four classes: first, experiments in which the sunspot equilibrium is payoff-equivalent to the other equilibria; second, experiments in which the sunspot variable points to the payoff-dominant equilibrium; third, the sunspot variable points to a payoff-dominated equilibrium; fourth, the sunspot variable switches between payoff-rankable equilibria. Duffy and Feltovich (2010) report an experiment that compares these classes. They search for the circumstances under which a sunspot equilibrium can be set up. They find that people can play a sunspot equilibrium even if it is not a Nash equilibrium (NE). However, it is necessary that the sunspot equilibrium is Pareto-efficient. Similarly, Bone et al. (2013) conclude that the sunspot equilibrium prevails if it is Pareto-efficient in a game with an asymmetric payoff function.

Examples of the first class – the sunspot equilibrium is payoff-equivalent to the other equilibria – are provided by Duffy and Fisher (2005) and Fehr et al. (2019). Fehr et al. (2019) conduct a two-person

coordination game where agents have to pick a number from zero to one hundred. Players are punished according to the deviation in their respective decisions. Each combination of two equal numbers is an NE. In this game, fifty is the risk-dominant NE. They use a semantically salient message in the form of an extrinsic public/private signal as the sunspot variable.

Examples of the second class – the sunspot variable points to the payoff-dominant equilibrium – are provided by Cason and Sharma (2007), Devetag et al. (2013), and Arifovic et al. (2019). Cason and Sharma (2007) show that a lack of knowledge of others' expectations can inhibit the sunspot equilibrium, even though it is a payoff-dominant equilibrium. To show this, they let participants play against robots with straightforward and known decision rules.

An example of the third class – the sunspot variable points to a payoff-dominated equilibrium – is given by Bosch-Domènech and Vriend (2013). They find that people coordinate on the only payoff-dominated equilibrium, which simultaneously makes it a focal point.

Examples of the fourth class – the sunspot variable alternatively points to payoff-rankable equilibria – are presented by Beugnot et al. (2012), Arifovic and Jiang (2014), and Shurchkov (2016). Beugnot et al. (2012) conduct a base game with Pareto-ranked equilibria where the payoff-dominant equilibrium is the same as the risk-dominant equilibrium. They use an unbinding random public announcement as sunspot variable. They find that 27% of the participants fail to coordinate on the Pareto-superior equilibrium due to the sunspot variable.

The paper at hand belongs to the fourth class. Like Arifovic and Jiang (2014) and Shurchkov (2016), our base game has a payoff-dominant and a divergent risk-dominant equilibrium. However, Arifovic and Jiang (2014) and Shurchkov (2016) do not compare a situation with a sunspot variable to a situation without a sunspot variable. Therefore, and in contrast to our work, they do not investigate coordination problems caused by a sunspot variable. Arifovic and Jiang (2014) and Shurchkov (2016) both change the risk of the payoff-dominant alternative. They find that the sunspot variable is only relevant if the sunspot equilibrium lies in the middle of the risk-dominant and the payoff-dominant alternatives. This result corresponds with our results. Given the fact that our negative sunspot-generating process is more effective than the neutral sunspot-generating process, we can add: the sunspot variable is more relevant if convergence to it is not much riskier than convergence to the risk-dominant equilibrium, but much less risky than convergence to the payoff-dominant equilibrium.

Like Beugnot et al. (2012), we focus on coordination problems by comparing a situation with a sunspot variable to a situation without a sunspot variable. In contrast to Beugnot et al. (2012), however, the base game of our experiment has fixed groups a payoff-dominant, and a divergent-risk dominant equilibrium. We can, thus, observe not only whether a sunspot variable leads to out-of-equilibrium behaviour, but also whether it leads to coordination on payoff-dominated equilibria. Beugnot et al. (2012) find frequent

out-of-equilibrium behaviour caused by sunspot variables. Our results partly support their findings. In fact, we find out-of-equilibrium behaviour in the short run. In the long run, we find evidence for coordination on payoff-dominated equilibria caused by a sunspot variable. The divergent payoff-dominant and risk-dominant equilibria together with the fixed groups seem to help coordination, but not necessarily on the payoff-dominant equilibrium.

To sum up, a sunspot variable, in the form of a random public forecast, can, indeed, trigger coordination problems. It leads to out-of-equilibrium behaviour in the short run and coordination on payoff-dominated equilibria in the long run.

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A Instructions for control treatment [neutral treatment] [negative treatment]

Welcome to the experiment!

Preliminary remark

You take part in a study on decision-making behaviour within the framework of experimental economic research. During the research, you and the other participants will be asked to make decisions. None of the participants will be informed about your decisions during or after the experiment. Please read the following instructions. After you have read the instructions, we will come to you to answer open questions. When all questions are answered, the experiment will start. If you have any questions during the experiment, you can contact us at any time by hand signal. During the experiment, you are not allowed to talk to the other participants in the experiment. At the end of today's experiment, you will receive your payment for the experiment plus 3 Euros for showing up in cash.

Decision situations

The experiment lasts for 40 rounds. At the beginning of the first round, you and two other participants randomly form a group of 3. Please note that **the members of the group are the same in each of the 40 rounds**. The members of a group are randomly assigned. All groups in the experiment consist of 3 members.

In each round, you and your group members can choose between two alternatives; "**A**" or "**B**".

[Announcement

You and your group members will receive an announcement at the beginning of each round. The announcement will be either "**the majority will choose strategy A**" or "**the majority will choose strategy B**". The announcement is ***the same*** for all participants in the experiment and therefore for all members in your group.

The **announcements are random** and are determined by rolling a die. The experimenter will roll it in front of all participants. You have the possibility to watch the roll of the die live by video transmission on one of your screens and, thus, see the cast number. The announcement is "**the majority will choose strategy A**" if the number cast is 1, 2, or 3 and "**the majority will choose strategy B**" if the number cast is 4, 5, or 6.]neutral treatment

[Announcement

You and your group members will receive an announcement at the beginning of each round. The announcement will be either "**the majority will choose strategy A**" or "**the majority will choose strategy B**". The announcement is ***the same*** for all participants in the experiment and therefore for all members in your group.

The **announcements are random** and are determined by rolling a die. The experimenter will roll it in front of all participants. You have the possibility to watch the roll of the die live by video transmission on one of your screens and, thus, see the cast number. The announcement is "**the majority will choose strategy A**" if the number cast is 1 and "**the majority will choose strategy B**" if the number cast is 2, 3, 4, 5, or 6.]negative treatment

Your payout is based on **your decisions and the decisions of your group members in each round**. All groups in the experiment receive the same payout table below.

		Other players' decisions in your group		
Your decision		If **BOTH** of the other participants choose A	If **ONE** of the other participants chooses A and the other chooses B	If **BOTH** of the other participants choose B
	A	12	0	0
	B	7	7	7

In this table, the rows show your possible decisions, "**A**" or "**B**", while the columns show the decisions of your group members. Each cell represents a decision combination and shows the merits in Euro, should this decision combination materialize. It is important that your payout is, thus, dependent on your decision **and** the decisions of your group members. For example, if you choose "**A**" and both of your group members choose "**B**", you receive **0 Euros**; if you choose "**A**" and both of your group members choose "**A**", you receive **12 Euros**.

Information

After each round, you will be informed of your decision, the decisions of your group members, and your resulting payout in that round. In addition, you will see information about the past rounds (your decisions, the decisions of your group members, and your payouts). In the table displayed, each row represents one of the past rounds. Your past decisions are displayed in the second column and the past decisions of your group members are displayed in the third column. The last column shows your payouts from past

rounds. The screen you will see later, looks like this:

ROUND	Your decision	Decisions of your group members	Your payout in round
...	...	...	...

Your payout

At the end of the experiment, one of the 40 rounds is randomly selected. This randomly selected round is relevant for your payout. In addition to the payout from the randomly selected round, you will receive a show-up fee of 3 Euros.

Chapter 6

Conclusion

This dissertation presents four macroeconomic experiments. Three experiments deal with intertemporal consumption decisions and one experiment deals with macroeconomic coordination.

We find evidence that people do not use information optimally in the experiment "The More You Know? – Consumption Behavior and the Communication of Economic Information" presented in Chapter 2 . More information about the development of their income reduces the "Rule of Thumb"-consumption, but it also causes people to save more than needed. With more information, participants tend to be further away from the optimum than with less information.

The experiment "Preferences Over Wealth: Experimental Evidence" shows that people have a tendency to save more than needed. Most participants perform the task well and achieve the optimal result. However, although every saving is sub-optimal and the task is simple, about half of the participants save. We interpret this as evidence for preference over wealth. It seems to be justified to assume preferences over wealth in macroeconomic models.

The experiment "Are the Poor more Impatient than the Rich? Experimental Evidence on the Effect of (Lab) Wealth on Intertemporal Preferences" shows: the rich are more impatient than the poor. Contrary to the previous

assumption, I find evidence that poor people are more patient. The poor participants choose the later payment option on average.

The experiment "Coordination Problems Triggered by Sunspots in the Laboratory" shows that publicly announced forecasts can harm coordination. Although all participants know that the predictions are random, they significantly affect behaviour. In the short run, the forecasts hinder coordination. In the long run, the forecasts ensure coordination to sub-optimal equilibria.

To give a rough summary of all four experiments, the traditional assumptions of most macroeconomic models do not survive laboratory testing; people do not use the information to improve their intertemporal decisions; people have unexpected preferences over wealth; people's intertemporal preferences are affected by small payments; people are triggered by irrelevant forecasts. It seems that the findings of this dissertation have at least scratched important cornerstones of macroeconomics. However, does that mean that these assumptions are not good approximations? This is, of course, not the case. The experiments can show that phenomena exist, but they do not reveal their extent in the real world. However, the findings are a good starting point for future experimental research, they confirm the non-traditional assumptions of some models, and challenge the non-traditional assumptions of other models.

In this dissertation, I gain some insights into macroeconomics using laboratory experiments. Thus, I can not support the quote from Gregory Mankiw of the beginning. On the contrary, I like to reply to Mankiw with the words of Kagel and Roth (2016, Preface page xiii): "Analogously, evolutionary biologists can't conduct experiments directly on the fossil record or on species extinction, but our understanding of evolution is enhanced by experiments on fruit flies and on DNA."

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