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Flexible Work Arrangements and Precautionary Behavior: Theory and Experimental Evidence

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Flexible Work Arrangements and Precautionary Behavior: Theory and Experimental Evidence

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Abstract

In the past years, work time in many industries has become increasingly flexible opening up a new channel for intertemporal substitution. To study this, we set up a two-period model with wage uncertainty. This extends the standard savings model by allowing a worker to allocate a fixed time budget between two work-shifts or to save. To test the existence of these channels, we conduct laboratory consumption/saving experiments. A novel feature of our experiments is that we tie them to a real-effort style task. In four treatments, we turn on and off the two channels for consumption smoothing: saving and time allocation. Our four main findings are: (i) subjects exercise more effort under certainty than under risk; (ii) savings are strictly positive for at least 85 percent of subjects (iii) a majority of subjects uses time allocation to smooth consumption; (iv) saving and time shifting are substitutes, though not perfect substitutes.

Keywords precautionary saving, labor supply, intertemporal substitution, experiment

JEL Classification D14 · E21 · J22 · C91 · D81

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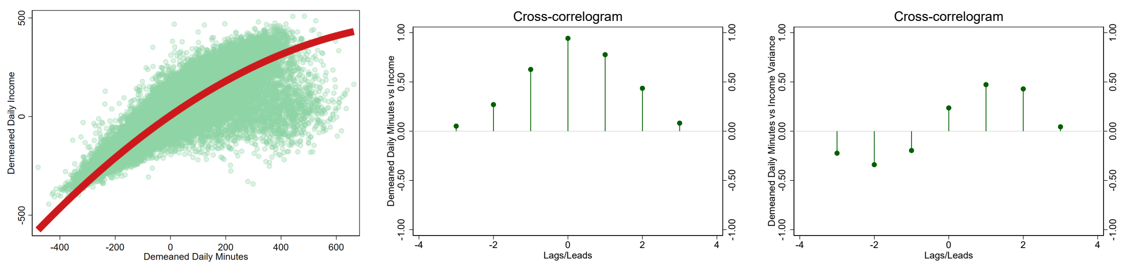
1 Introduction

The standard model based on the notion of consumption smoothing predicts leisure and thus work time to be smooth as well (Blundell and MaCurdy 1999). However, since flexible work arrangements are increasingly common, the model fails to describe observed behavior, since work time is observed not to be smooth in many instances. The so-called gig economy, for example, changed the environment for both labor supply and consumption/saving decisions of many professionals from ride-sharing drivers, and bicycle couriers to data scientists, engineers, and architects (among others). These professionals supply labor on online platforms like Uber, Delivery or Upwork without submitting to a specific work time schedule.

Figure 1 shows three stylized facts from all Chicago taxi medallions over each day of the year 2014 (these medallion holders are allowed to operate a taxi and can choose their work time and duration freely). Our first observation, in the left graph, is that income per day is strongly correlated with the number of minutes worked per day but flattens off at long hours. Second, the middle graph shows that income per day and minutes worked per day are strongly associated contemporaneously (at lag 0) but are also positively related with past and future days (cf. Farber 2005). Finally, suppose taxi drivers would smooth their work time perfectly. Then, for a given income, we would not expect any correlation between work time and the variance of income. The right graph shows that the simple sample variance measure of income is, however, correlated with minutes per day, sometimes positively, other times negatively. The objective in this paper is to study channels which may lead to such a correlation in a stripped-down model.

Figure 1: Stylized facts on Work Time, Income, and Income Risk for Chicago Taxi Medallions



(a) Income versus Minutes and Quadratic Fit

(b) Correlation of Income and Minutes across Days

(c) Correlation of the Variance of Income and Minutes across Days

Notes: We aggregated observations on all Chicago taxi medallions over 2014 by day and applied the fixed-effects or within-transformation to the data to generate these figures. The data are available at the Chicago Data Portal.

This paper reconciles the theory with the facts by showing how precautionary reasons may explain why work time is not smooth. The intuition is that instead of smoothing work time over calendar time like consumption, work time may be used as channel to smooth consumption like saving. If precautionary behavior exists, this could be one motive for regarding work time allocation as a substitute to saving. However, the current literature is ambiguous on how important precautionary behavior is.

Therefore, we first show in which sense labor supply and savings may be substitutes and discuss the mechanics of this alternative strategy for intertemporal substitution. For this purpose, we extend the standard model for consumption and labor supply decisions and introduce shifting. The intuition of shifting is that flexibility in work arrangements allows to mitigate bad wage shocks. For example, if a person has two jobs, one with a certain wage, one with an uncertain wage. Flexible work time allows to mix the earnings from the uncertain job with the uncertain job, so in case the uncertain job turns out to pay very little, the earning from the certain job can partly offset this.[1] Second, we provide evidence for the existence of precautionary saving from laboratory experiments which are tightly linked to our model. Here, we are able to control all the features of our model and to make sure that the only reason to save is precaution, which is not possible using observatory data.[2] A novel feature of our consumption/saving experiments is that we tie them to a real-effort style task. As a third contribution, we show that not only precautionary saving but also precautionary shifting exists in our experimental data, and fourth, that these two channels are indeed substitutes—though not perfect substitutes.

We set up a two-period model in which the wage is certain during the first period. In the second period, a mean-zero shock perturbs the wage (keeping the expected wage identical to that of the first period). We induce preferences that resemble a progressive tax or isoelastic utility and compare decisions in four treatments: (i) a hand-to-mouth treatment where neither a saving nor a shifting decision can be taken; (ii) a treatment where saving is allowed; (iii) a treatment where shifting is allowed; and (iv) a treatment where both saving and shifting are allowed. While precautionary leisure cuts are not possible, income has to be produced by exercising effort and subjects can adjust their work intensity in the anticipation of risk and, thus, provide extra effort. Independently of the existence of extra effort, a part of the income may be shifted from one shift to another via savings or work time allocation for the induced precautionary reasons. We derive four hypotheses from our model to show whether the hand-to-mouth model, the standard precautionary saving model, and the precautionary shifting model sustain the test with our experimental data and whether saving and shifting are indeed substitutes.

First, we examine whether subjects behave according to Jensen's inequality, which implies that they provide more effort in the first period (when the wage is certain) than in the second (when it is risky). We find that on average subjects follow this prediction and reduce effort in the experimental real-effort task by about 19 percent. Second, does precautionary saving exist in our experiments? With at least 85 percent of the subjects engaging in precautionary saving, we reject the non-existence of precautionary saving. Is precautionary effort—an increase in work intensity when facing wage risk—absent as predicted? Non-parametric tests show that medians and distributions of effort are identical. Thus, we find no evidence for precautionary effort. Third, does precautionary shifting exist? About 59 percent of all subjects engaged in precautionary shifting. Fourth, are saving and shifting perfect substitutes? Though we find that they are substitutes, we reject that saving and shifting are *perfect* substitutes on aggregate.

The paper is structured in the following way: After providing an overview of the literature in the next section, Section 3 presents our extension of the standard model of consumption and labor supply and our hypotheses. Section 4

[1] Essentially, flexible work arrangements allow to determine shift-specific average wage by choosing the length of the work shift endogenously.

[2] Our study complements research with data from outside the lab. Many attempts to quantify causal effects in real-world data are plagued by problems like absence of credible measures of wage risk, effort, wealth or shift-specific information, the presence of many different saving motives or incentives provided (e.g., by saving subventions or tax policy) and the presence of heterogeneous decision rules. In lab experiments, we can control wage risk, information settings, induce a utility function and let subjects work under wage uncertainty. Lab experiments also allow us to turn on and off savings channels and we can control for the subjects' heterogeneity by exposing subjects to various different treatments. In controlled environments, we are also able to keep variables constant that affect motivations like working for meaning and pleasure (which may influence effort, see Heyman and Ariely 2004). Given this, it is surprising that there are only very few studies that investigate heterogeneity of behavior in labor market experiments and of precautionary behavior.

describes our experimental design and procedures. Section 5 presents and discusses our findings. Finally, Section 6 summarizes and concludes.

2 Review of the Literature

To understand how shifting relates to saving (i.e., either to forgo utility by cutting consumption or incur extra disutility by cutting leisure or by providing more effort) and to put our research in the more general context of precautionary behavior, we review the literature briefly.

2.1 Precautionary Saving

Precautionary saving is usually defined as the difference between consumption under certainty and in the presence of risk (see, e.g., Kimball 1990, p. 55). Some empirical evidence from survey data shows that this kind of precautionary behavior may be economically important.[3] Gourinchas and Parker (2002) attribute 60-70 percent of wealth to precautionary saving in early life. Kazarosian (1997) and Carroll and Samwick (1998) estimate the precautionary component of wealth to be in the range of 20-50 percent. However, the evidence is not unambiguous. With subjective earnings uncertainty, Guiso et al. (1992) estimate the precautionary component of wealth at only a few percentage points. Lusardi (1998, 1997) and Engen and Gruber (2001) find small precautionary wealth as well. Hurst et al. (2010) and Fossen and Rostam-Afschar (2013) argue that estimates are sensitive to whether business owners are included in the dataset. As business owners often have more flexible work arrangements, this raises the question if the possibility to shift determines the level of precautionary wealth.

Some of the problems in survey data may be avoided in experimental settings, but the experimental literature on precautionary saving is relatively small. Fuchs-Schündeln and Schündeln (2005) show that, in accordance with a model that includes substantial precautionary effects, saving rates of most East Germans but civil servants increased sharply after the natural experiment of the German unification. By contrast, West Germans—who would have been subject to more selection into jobs based on risk preferences—exhibited little difference in saving rates between civil servants and others with riskier jobs, either before or after reunification.

Duffy (2016, section 2.1) surveys the literature on consumption/saving decisions using laboratory experiments. Here, we review some of the most important papers from this strand of literature in order to highlight how our experiment differs from them. Ballinger et al. (2003) study precautionary saving and social learning (over a life cycle of 60 periods subjects had to take saving decisions after receiving a randomly determined income). Social learning in an intergenerational structure was mimicked by allowing the subjects in the role of the younger-generation individual to sit next to an older-generation subject and to observe its behavior and to interact with him or her during some periods before taking his or her own decisions (and being later on joined by yet another younger-generation individual). Ballinger et al. find that subjects who could learn from 'elders' perform better than the ones who could not. Also, subjects' behavior is qualitatively correct but they save too little early in the life cycle. Brown et al. (2009) test two explanations for this observed undersaving. The first reason, bounded rationality, was tested by giving some subjects the

[3] See Section E in the Appendix for an overview of results on the existence and importance of precautionary behavior complementing the excellent surveys in Jappelli and Pistaferri (2017) and Lugilde et al. (2019).

opportunity to observe other (well-performing) subjects' behavior. Subjects who could learn socially performed better than the subjects who only learned on their own. The second reason, a preference for immediacy, was examined in a second experiment. Two groups of thirsty subjects received soft drink sips as period consumption. One group of subjects received their consumption immediately, the other group only with a ten-period delay. Here, the former group undersaved in comparison to the latter group and showed a lower total consumption level. Ballinger et al. (2011) correlate measured personality traits and cognitive abilities with the performance in a savings experiment and find that two cognitive abilities scales predict savings performance best. Meissner and Rostam-Afschar (2017) vary taxes in a way that should keep savings/consumption decision unchanged and show that more than 50 percent of lab subjects simplify consumption decisions by ignoring incentives for precautionary behavior.

What are the main differences between these previous experiments and ours? We test how subjects respond to different institutions, i.e., the existence of different savings channels (instead of testing which behavioral explanations determine savings/consumption decisions). Another difference is that, in all mentioned experiments, income was randomly assigned; in our experiment, subjects have to work on a real-effort style task in order to generate income. This novelty is crucial as we are explicitly interested in the precautionary effort channel that subjects may exercise facing risky environments. Whereas the papers reviewed in Duffy's survey consider complete life-cycles with up to 60 periods, our experimental design only considers two periods—the shortest time-span in which saving is meaningful. This two-period design should not be interpreted as a complete life-cycle but as an episode in an individual's life (we consider this simplicity as an advantage for theory-testing because it is easy to communicate to subjects).

2.2 Precautionary Labor Supply

With flexible hours of work a second channel emerges through which individuals may react in a forward-looking way: They may take into account their expectation about future wage risk when deciding how much to work in a given shift (see Carroll and Kimball 2008, Flodén 2006 or Low 2005). Individuals with higher risk, e.g., self-employed, would work longer *before* the realization of shocks to accumulate precautionary wealth. Precautionary labor supply is then defined as the difference between work hours supplied in the presence of risk compared to the situation without risk. As an alternative to precautionary consumption cuts, precautionary labor supply always increases savings.

On the empirical side, very little research has been devoted to precautionary labor supply. As reported by Mulligan (1998, p. 1034), "there is no empirical evidence that precautionary motives for delaying leisure are important". Pistaferri (2003) finds that the effect of wage risk on labor supply is in line with theory, but in practice negligible. Jessen et al. (2017) find that the self-employed would work 4.5 percent less if they faced the same wage uncertainty as the median civil servant. Their estimates show that inflexible work arrangements constrain workers who can adjust hours only after about four and two years, respectively, and thus act less in anticipation of risk. Because of measurement issues, various important studies treat labor supply as synonymous with effort at work and time spent working (Heckman 1993, p. 116). However, already Marshall (1920, p. 438) notes that "for even if the number of hours [of work] in the year were rigidly fixed, which it is not, the intensity of work would remain elastic". Therefore, we redefine labor supply as a function of two endogenous choices: effort and allocated time. Accordingly, we define precautionary shifting as the difference in the relative length of a work-shift under risk and without risk when leisure is held constant.

2.3 Precautionary Effort

While we focus on shifting, Eeckhoudt et al. (2012) and Wang and Li (2015) recognized that effort may also serve to accumulate precautionary savings. Empirically, precautionary effort has only been indirectly addressed by Huck et al. (2018). In their experiments, subjects work on a task and information about the two possible realizations of a piece-rate is either (i) resolved; (ii) unknown; or (iii) chosen to be learned by the subjects. About a third of the subjects are information avoiders in the last treatment who subsequently outperform the information seekers. For our study, we define precautionary effort as the difference between effort costs under risk and under certainty. Our model predicts the absence of precautionary effort. Accordingly, we only find little evidence for it in our data. While we stress that precautionary effort—like precautionary labor supply—*increases* savings, precautionary shifting is an *alternative* to saving. In other words, while labor supply (extra effort or less leisure) can be transformed into consumption and thus savings *intra*temporally, we show that savings and labor supply (i.e., the distribution of fixed work time) are substitutes *inter*temporally. In a related setting, the question whether time and money are perfect substitutes has been studied in the context of charitable giving (see Brown et al. 2019).

Our study is related to experimental research on labor markets, surveyed, e.g., in Duffy (2016, section 4.2) and Charness and Kuhn (2011). Most closely related is Dickinson (1999) who sets up a model where workers can substitute on- and off-the-job leisure and tests it in experiments. In one of the conducted treatments, the subjects are only allowed to choose their effort (while work hours are fixed), and in another, they are also allowed to leave the experiment early. In both treatments, the piece-rate for the real effort-task is varied within-subject. The results confirm the predictions of the model: subjects in the lab experiments substituted leisure on- and off-the-job, which explains the negative substitution effects. Our model provides an alternative explanation for negative wage elasticities, since a wage increase may change the optimal allocation of work-shifts. In contrast to Dickinson's experiments, we only run treatments where total work-time is fixed (as we are not interested leisure vs. work-time decisions, and, also, as we do not want to let subjects' opportunities to earn money or otherwise spend their time outside of the lab affect their decision how long in total they would like to work on our real-effort task).

3 Theory

In this section, we first introduce the general framework of our model. Subsequently, we describe the four scenarios where we, one by one, allow saving and shifting and derive the four hypotheses we test with our experiments.

3.1 A Simple Two-Period Model

Consider a simple two-period model with two work-shifts, where ex-ante total consumption C is the sum of expected consumption in work-shift 1, c_1, and expected consumption in work-shift 2, $E[c_2]$.[4] In both work-shifts $i = 1,2$, consumption is a concave function of income $c(y_i)$ which can either be interpreted as a progressive tax or as decreasing marginal utility. We abstract from discounting. The individual's problem is

[4] The expectation operator needs to be applied to both work-shifts' consumption because uncertainty about the second period's wage can also arise in the first work-shift.

$$\max_{y_1, y_2} C = E_\varepsilon[c(y_1)] + E_\varepsilon[c(y_2)]. \tag{1}$$

Work-shift specific income, in turn, depends on exogenously given, period-specific wage rates w_j with periods $j = 1, 2$ and three kinds of shift-i-specific choices: effort e_i, savings s, and choice of the relative length of the first work-shift $t \in [0, 1]$.[5] Both periods' joint absolute length is exogenously fixed and lasts T units of time. For simplicity and without loss of generality, we assume that each of the two periods takes $0.5 \times T$ units of time. At the beginning of the second period, the period-specific wage rate w_1 changes exogenously to w_2. The first-period wage rate is certain, $w_1 = w$, while the second-period wage rate w_2 is uncertain. In the second period, a mean-zero wage shock ε shifts $w_2 = w + \varepsilon$ either up or down.

It is important to emphasize that the occurrence of the wage shock is only revealed after all decisions have been made. This renders it possible to isolate the effects of uncertainty (as in Flodén 2006, Parker et al. 2005, Hartwick 2000, and Eaton and Rosen 1980), and is an element of some real-world settings. For example, to obtain a bonus payment, it might be necessary to allocate effort before the amount of the bonus is known (e.g., because it depends on the business cycle or the success of a group of comparable workers).[6]

The choice of t causes income y_i in each shift to be determined by either the wage of a single period or the wages of both periods. In particular, income in shift 1 is

$$y_1 = \begin{cases} y_1(t, w_1, e_1, s) & \text{if } t < 0.5 \\ y_1(0.5, w_1, e_1, s) & \text{if } t = 0.5 \\ y_1(t, w_1, e_1, w_2, e_2, s) & \text{if } t > 0.5 \end{cases} \tag{2}$$

and income in shift 2 is

$$y_2 = \begin{cases} y_2(t, w_1, e_1, w_2, e_2, s) & \text{if } t < 0.5 \\ y_2(0.5, w_2, e_2, s) & \text{if } t = 0.5 \\ y_2(t, w_2, e_2, s) & \text{if } t > 0.5. \end{cases} \tag{3}$$

By different choices of t, different period wage rates determine the shift-specific income in the generalized model and that $t = 0.5$ nests the standard model as a special case. While we assume shift-separable consumption, choosing t different from 0.5 implies non-separability over periods. Figures 2 and 3 illustrate this. In the standard model in Figure 2, labor supply is only chosen according to the (expected) wage of the period under consideration. However, this setup rules out the possibility of influencing the expected wage by choosing the length of a work-shift.

Figure 3 shows an example where a worker decides to end the first work-shift early. Hence, he or she earns wage rate w_1 in this first work-shift. In the second work-shift, he or she earns both wage rates: w_1 for each unit produced in the time until $0.5 \times T$ and w_2 for each unit produced until T. In this way, we generalize the standard model, which

[5] Accordingly, the second shift's relative length is then $1 - t$.

[6] E.g., UberX drivers accept rides without neither knowing the distance of the ride nor the fee they receive (which depends in part on supply and demand in the local market they serve).

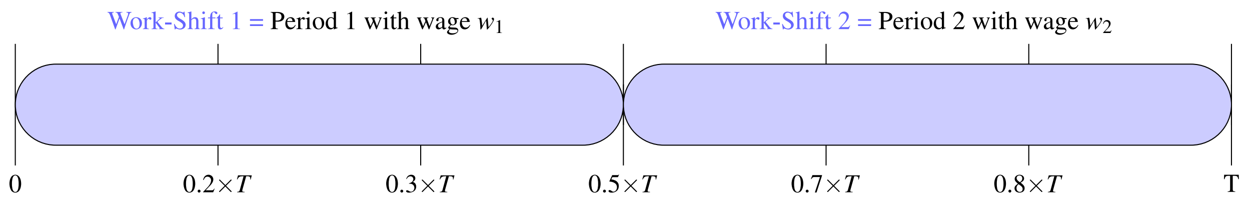


Figure 2: Labor Supply and Wage Changes in the Standard Model
Source: Authors' presentation.

does not take into account that the choice of labor supply may determine the (expected) hourly wage and that income may be valuated at the end of a shift—not at the end of a period.

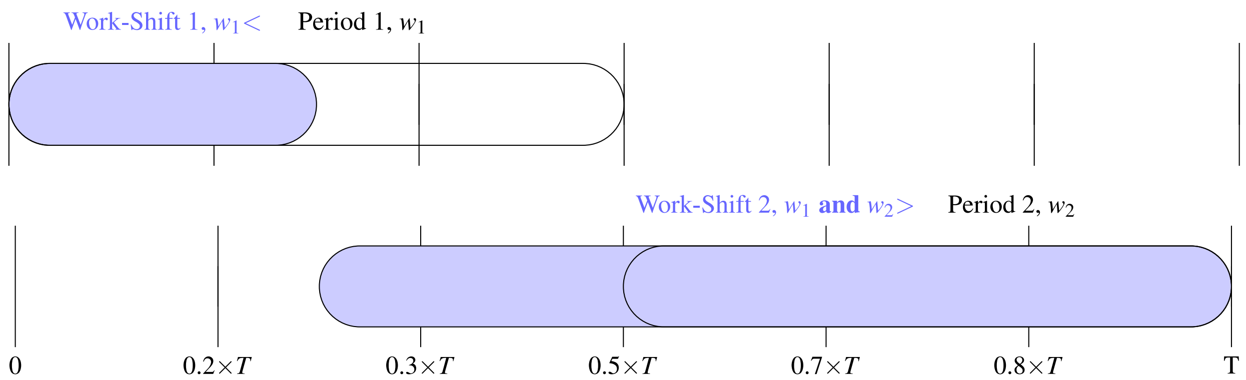


Figure 3: Labor Supply and Wage Changes in the General Model
Source: Authors' presentation.

Precautionary motive In each work-shift, after-tax consumption is related to income by a scaled and shifted isoelastic function

$$c(y_i) = \left([1/(1-\tau)(y_i^{1-\tau}-1)]-\eta\right)\zeta. \tag{4}$$

Here, τ determines the degree of risk aversion (the concavity of the consumption function) and prudence (the convexity of marginal consumption), and its reciprocal $1/\tau$ determines the intertemporal elasticity of substitution induced by the tax system. Most importantly for our analysis, this tax schedule leads to a positive third derivative implying prudence and thus risk will affect optimal choices.[7] Prudence is measured by the parameter of relative prudence (Flodén 2006; Kimball 1990). In our case, this parameter is $-y_i\frac{c'''}{c''} = 1+\tau$. Accordingly, marginal after-tax consumption is higher when before-tax income is low, and the *rate* at which marginal after-tax consumption rises when before-tax income falls is greater when before-tax income is low than when it is high.

Given the precautionary motive, there are two margins of choice that reflect precautionary behavior, both of which we will analyze. First, as in the standard model, prudent individuals have an incentive to save in anticipation of wage risk. Insurance against wage risk is the only reason for saving in our experiment since the expected wage is identical

[7] In order to maximize expected total consumption payoff, the worker must reduce income in the first shift and increase it in the second, so as to reduce the expected marginal consumption payoff of income in the second shift and increase it in the first. See Jappelli and Pistaferri (2017, p. 97) for details.

in periods 1 and 2. The possibility to end a work-shift before or after a change in wage risk creates another route to engage in precautionary behavior: prudent individuals have an incentive to sacrifice some payoff in shift 1 and end it before the wage becomes uncertain in order to ensure that some income from the certain wage rate enters the consumption function in the second shift.

Budget constraint The second major difference to standard labor supply models is that we specify a function $c(y_i)$ that valuates benefits net of costs instead of the additive separable valuation of benefits and costs of work. Instead of describing the situation of employees, where disutility of work accrues privately (and is valuated in an additive separable way), in our model the costs of work can immediately be deducted as business expenses before valuation. This resembles the situation of self-employed or freelancers more closely. The main reason for this design feature is that it disincentivizes precautionary effort, i.e., higher effort in the first work-shift under uncertainty than under certainty. Since precautionary effort works via savings, its existence does not confound our results. A positive side effect is that the self-employment specification also requires fewer non-linear functions, which makes the experimental setup simpler to explain.

In particular (we present the other cases below), the shift-specific budget constraint y_i for the case $t = 0.5$ is given by

$$y_1 = w_1 \times q(e_1) - v(e_1) - s, \tag{5}$$

$$y_2 = w_2 \times q(e_2) - v(e_2) + s. \tag{6}$$

Shift-specific effort e_i translates into a production quantity according to a production function $q(e_i)$ from which costs of effort $v(e_i)$ are deducted. Income at the end of a work-shift, before saving, is then the product of wage times production minus effort costs. From this income, savings are deducted in the first work-shift and added in the second work-shift (without bearing interest).

Cost function We specify a cost function for effort that limits the optimal level of effort:

$$v(e_i) = \sum_{k=0}^{e_i} \varphi \times (k)^2. \tag{7}$$

At the beginning of each work-shift, the cost function is reset to zero. This and the quadratic form of the cost function resembles fatigue effects with increasing effort. Note that we defined this cost function with our experiments in mind, where effort levels are discrete.

Production function We do not impose a production function in the experimental design as we suspect productivity to be highly heterogeneous. In line with Gächter et al. (2016), who introduced the experimental real effort task, our aim is to estimate the production function from the experimental data. As Gächter et al. (2016), we will estimate period-specific production functions of the form

$$q(e_j) = \beta_1 (e_j)^{0.5} + \beta_2 (e_j)^2 + \gamma, \tag{8}$$

where γ is fixed production, i.e., effortless output. As our structural predictions regarding the *level* of savings and shiftings depend on the production functions, in the Appendix, we will estimate them to obtain point predictions.

However, we will also investigate deviations from the optimal *share* of income saved or shifted as this measure does not depend on individual productivity.

3.2 Treatments

Here, we describe the four scenarios that arise due to the two precautionary channels that workers can or cannot use. From now on, we will speak of treatments in order to use the same terminology as in the empirical part of this paper. Our objective is to test if and how subjects use the different precautionary channels. We start with the simplest setup in the first treatment and allow more choices in the following treatments, as shown in Table 1.

Table 1: Treatments and Choices

	Static	Intertemporal		Choices		
Treatments	Effort	Saving	Shifting			
Treatment I (shorthand TI)	Allowed	Not Allowed	Not Allowed	e_1, e_2		
Treatment II (TII)	Allowed	Allowed	Not Allowed	e_1, e_2	s	
Treatment III (TIII)	Allowed	Not Allowed	Allowed	e_1, e_2		t
Treatment IV (TIV)	Allowed	Allowed	Allowed	e_1, e_2	s	t

Source: Authors' presentation.

Treatment I: No Intertemporal Choice Many consumers lead a 'hand-to-mouth' existence: they simply consume their net income and do not save (Kaplan and Violante 2014). This may be due to unsophisticated behavior (non-optimizing, or 'rule-of-thumb' consumers), or due to the inability to trade in asset markets because of infinitely high transactions costs. In our experiment, we restrict subjects in Treatment I to be hand-to-mouth consumers, i.e., $t = 0.5$ and $s = 0$. Thus, we generate a control treatment where intertemporal consumption smoothing is not possible. A Lagrange function $\mathscr{L}^I$ for each work-shift i describes the individual's optimization problem:

$$\mathscr{L}_i^I = E_\varepsilon[c(y_i, e_i)] + \mu^I(E_\varepsilon[w_i \times q(e_i) - v(e_i) - y_i]).$$

The expectation operator E_ε is only relevant for work-shift $i = 2$. As the two work-shifts are not connected neither via savings nor shifting, each optimization can be considered separately. With fixed work arrangements, the only choice variable in this setting is effort e_i in each work-shift.[8]

Here, the only effect of wage risk is that the optimal level of effort in the second shift is smaller than in the first. This is due to Jensen's inequality which is induced by the concave tax function. The intuition behind this is that a (higher) mean-preserving spread in the wage risk disincentivizes effort (as the benefit of a higher wage in case the good state of the world realizes cannot offset the detriment of a lower wage of the same magnitude in the other case). We, thus, formulate the following testable hypothesis:

Hypothesis 1 – Reduction of effort by risk: *Effort will be smaller under uncertainty than under certainty, i.e., effort in the second work-shift will be smaller than in the first work-shift of Treatment I.*

[8] The first order conditions for Treatments I through III can be found in Section F in the Appendix.

Treatment II: Precautionary Saving While hand-to-mouth behavior can be observed in many situations, another important behavioral tendency is to 'save for a rainy day'. Precautionary saving has received much attention in the literature, although, as mentioned in Section 2, evidence for it is mixed. The main purpose of Treatment II is to examine whether precautionary saving exists. In each work-shift, effort e_i is chosen, and, at the end of the first work-shift, the savings amount s.

The Lagrange function $\mathscr{L}^{II}$ changes only slightly compared to Treatment I's: The two work-shifts are now connected and the intertemporal budget constrains choices (see Equations (5) and (6)). Ex-ante, the sum of the net payoffs from both periods is relevant. Abstracting from the borrowing constraint, $\mathscr{L}^{II}$ can be written as

$$\mathscr{L}^{II} = c(y_1, e_1) + E_\varepsilon[c(y_2, e_2)] + \mu^{II}(E_\varepsilon[w_1 \times q(e_2) + w_2 \times q(e_2) - v(e_1) - v(e_2) - y_1 - y_2]).$$

In the absence of risk, the intertemporal condition ensures that the net payoff in both work-shifts will be smoothed. In our experiment, the expected wage in the second period is identical to the certain wage in the first period. Therefore, given productivity and effort, there is no reason for a difference in net payoffs in both work-shifts except for precautionary cuts of first-period income.

There are two things to note: First, under uncertainty a strictly positive amount of savings is optimal. In our setting, the precautionary motive is the only reason to save enumerated by Browning and Lusardi (1996, p. 1797 and pp. 1799–1801). As effort is not valuated separately from income, it will not help as a separate channel for smoothing. Second, if the third derivative of $c(y_i)$ is non-positive, (precautionary) saving will be zero: $c_{y_1} = E_\varepsilon[c_{y_2}] = c_{E_\varepsilon}[y_2]$, such that $e_1 = e_2$ and $c(y_1, e_1) = c(y_2, e_2)$. Intuitively, bad wage realizations will be foreseen and mitigated by savings.

Hypothesis 2 – Effort and precautionary saving: *i. Absence of precautionary effort: Savings do not result from an increased effort in the first work-shift. In Treatment II, effort in work-shift 1 will be the same as in work-shift 1 of Treatment I.*

ii. Existence of precautionary motive: In anticipation of the risk in the second period of Treatment II, a strictly positive fraction of income will be saved (resulting in a consumption cut in the first period).

Treatment III: Precautionary Shifting Another option for shifting income intertemporally has been missed to date: precautionary shifting. Based on the setup in Treatment I, in Treatment III we introduce the possibility to choose when work-shift 1 ends. This changes the budget constraints for each work-shift such that each has now three cases as shown in Equations (2) and (3).

The task is to choose the length of work-shifts via t and, in each work-shift, effort e_i. The Lagrange function is

$$
\begin{aligned}
\mathscr{L}^{III} \quad &= \quad c(y_1,e_1)+E_\varepsilon[c(y_2,e_2)]+\mu^{III}\Bigg\{ \qquad (9)\\
+\mathbb{1}_{\{t=0.5\}} \quad &\times \quad \bigg[2\times t[w_1\times q(e_1)-v(e_1)]-y_1\\
&+ \quad 2\times(1-t)E_\varepsilon[w_2\times q(e_2)-v(e_2)]-y_2\bigg]\\
+\left(1-\mathbb{1}_{\{t=0.5\}}\right)\mathbb{1}_{\{t<0.5\}} \quad &\times \quad \bigg[2\times t[w_1\times q(e_1)-v(e_1)]-y_1\\
&+ \quad 2\times(0.5-t)[w_1\times q(e_1)-v(e_1)]\\
&+ \quad 2\times 0.5E_\varepsilon[w_2\times q(e_2)-v(e_2)]-y_2\bigg]\\
+\left(1-\mathbb{1}_{\{t=0.5\}}\right)\left(1-\mathbb{1}_{\{t<0.5\}}\right) \quad &\times \quad \bigg[2\times 0.5[w_1\times q(e_1)-v(e_1)]\\
&+ \quad 2\times(t-0.5)E_\varepsilon[w_2\times q(e_2)-v(e_2)]-y_1\\
&+ \quad 2\times(1-t)E_\varepsilon[w_2\times q(e_2)-v(e_2)]-y_2\bigg]\Bigg\},
\end{aligned}
$$

where $\mathbb{1}_{\{\text{condition}\}}$ is an indicator that equals 1 if the condition is true (and zero otherwise).[9] Compared to Treatment II, only the budget constraint is different: instead of subtracting a specific savings amount at the end of a shift, the choice of the length of a work-shift determines how much is subtracted from income. Thus, the intertemporal optimality condition is identical in Treatments II and III. For an optimizing decision-maker, both options are substitutes for intertemporal substitution. In practice, however, behavior may vary depending on whether saving or time allocation is allowed.

In the absence of risk, there will be perfect smoothing, i.e., effort in both periods will be identical, and t will be 0.5. Since intertemporal substitution can be achieved with the same costs as in Treatment II under risk, in Treatment III under risk both effort levels will be identical to that of Treatment II and prudent decision-makers find it optimal to finish work-shift 1 early given the risk in period 2. These decision-makers will use the certain wage in work-shift 2 to build up a level of precautionary wealth before working under the risky piece-rate. In other words: The shifting channel allows to work for an average of the certain and the low wage rather than to work under the low wage alone. Assume that someone has two jobs, one with a certain wage, another one with an uncertain wage, and the flexibility to arrange when to work on which job. It would be optimal to mix some of the certain wage to the uncertain (in case the bad state o the world realizes).

Hypothesis 3 – Precautionary shifting: *In Treatment III, the first work-shift will be shorter than the second.*

Treatment IV: Precautionary Shifting and Saving Under flexible work arrangements, workers may have the option of both determining when to finish a work-shift *and* whether to store value over time. Hence, we combine the features of Treatments II and III. In Treatment IV, the budget constraints change slightly in comparison to Treatment III because savings s are not restricted to zero anymore and may be non-negative. The Lagrange function and the optimality conditions, however, are identical to that of Treatment III (hence $\mathscr{L}^{IV}=\mathscr{L}^{III}$). This is because while savings

[9] To keep the Lagrangian comparable to the two previous ones, we need to account for the two periods in our model, therefore we write *two* times the time allocation term. This is because $t=0.5$ nests Treatment I in Treatment III.

enter the work-shift budget constraints, they cancel out in the intertemporal budget constraint. Therefore, the resources on which the decisions are based remain unchanged.

In the general setup of this analysis, shiftings and savings are in fact perfect substitutes. The comparison of the data from Treatments II and III shows whether subjects achieve the same expected payoff by choosing either s or t. Treatment IV goes one step further and allows us (together with the observations from Treatments II and III) to analyze which combination of work-allocation and saving is actually chosen by the subjects (as theory does not make a statement whether one of the extreme cases of only saving or only shifting is chosen or a combination of the two). We can test the following hypothesis.

Hypothesis 4 – Precautionary saving and shifting: *i. In Treatment IV, either work-shift 1 will be shorter than work-shift 2, or there will be positive savings, or both.*

ii. Identical choices and outcomes with intertemporal substitution: subjects will reduce savings when given the opportunity to shift and will reduce time in work-shift 1 when given the opportunity to save. Expected payoffs in Treatments II, III, and IV will be identical.

4 Experimental Procedures

Our experimental design follows our model. Because we expect a high level of heterogeneity of productivity between the subjects that we want to account for, we use a within-subject design for our individual decision-making experiments. Before we describe the different stages of the experiment, we explain the real-effort task that resembles work in our experiments.

The task We use the ball-catching task introduced by Gächter et al. (2016). Figure 4 shows an example screen of this task. In the ball-catching task, subjects are presented a rectangular box. There are balls hanging at the top of the box in four columns and a tray is positioned at the bottom of the box. As soon as subjects click the start button, balls fall down the screen in either one of the four columns at constant speed (probabilities are equal for the next ball to fall down in any column). Subjects earn the piece-rate w_j within period j by catching balls with the tray (hence, the expected work-shift revenue is $Er_i = q_{i1} \times w_1 + E\left(q_{i2} \times w_2\right)$ with q_{ij} the number of caught balls in shift i and period j). In order to catch the balls, the subjects can move the tray from one column to the other by clicking two buttons under the rectangular box labeled LEFT and RIGHT.

Moving the tray is costly in monetary terms; this can be interpreted as the labor effort employed in a shift. e_i designates the number of movements in a shift.[10] To implement an increasing marginal cost of effort, we use the following unit cost function in each shift i: $\kappa(e_i+1) = 0.1 \times (e_i)^2$ with e_i+1 being the next movement and e_i the number of movements so far.[11] At the beginning of each work-shift, the unit cost function is reset, $e_i(0) = 0$. The total

[10] In contrast to many other real-effort tasks that are designed to be tedious for the subjects in order to "bring the task more in line with what people consider labor" (Charness and Kuhn 2011, pp. 243-244), the ball-catching task explicitly quantifies the cost of effort in monetary terms. Hence, we consider the ball-catching task ideal for our research questions. Even if subjects enjoy the task, the cost of effort should keep them from exercising more effort than necessary.

[11] We round the unit costs up to one integer in order not to confuse subjects with the decimals.

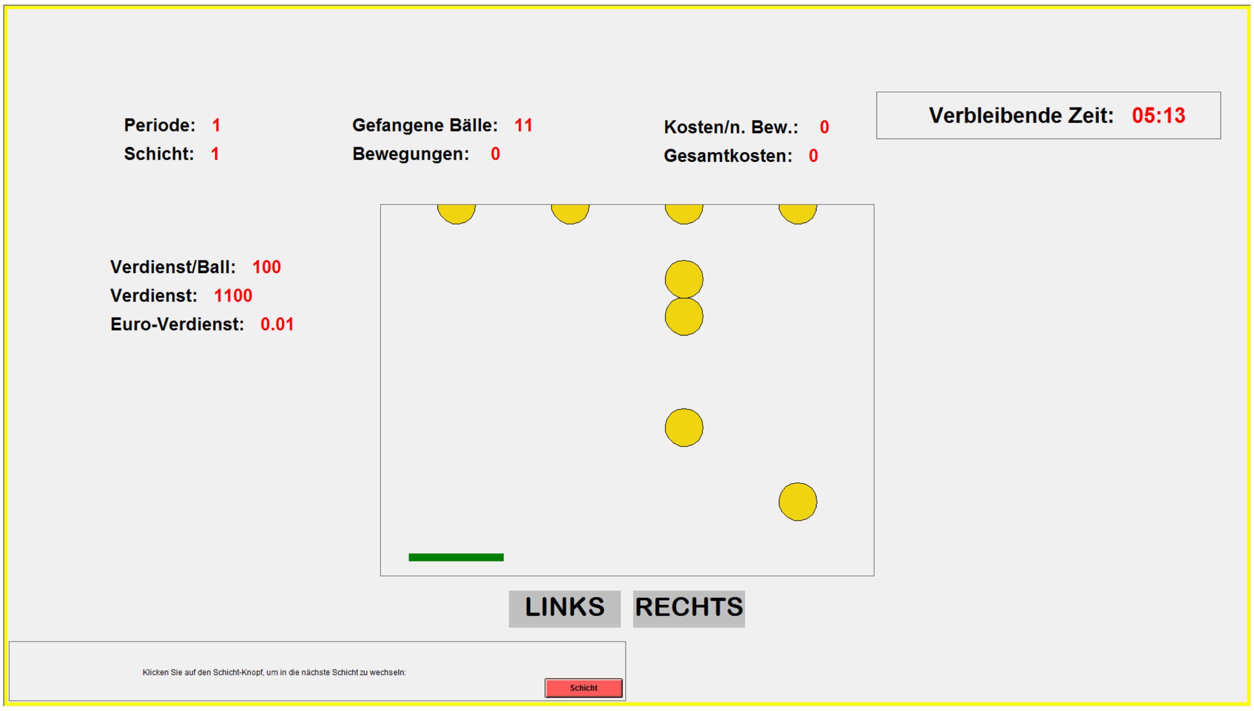


Figure 4: Example Screenshot of the Ball Catching Task (with 'Shift Change' Button at the Bottom)
Source: Authors' presentation.

cost per shift is given by the sum of unit costs, $\nu_i(e_i) = \sum_{k=0}^{e_i} \kappa(k)$. Therefore, this task generates a tradeoff between the revenue from catching balls, r_i, and the total cost of effort, $\nu_i(e_i)$. The point earnings in any one of the two shifts are then given by revenue minus cost, $y_i = r_i - \nu_i - s$, where s denotes savings. The euro earnings in each shift are calculated by $\text{Euro}_i = 4 \times [\ln(y_i) - 7]$.[12] This payoff function implies a coefficient of relative risk aversion of $\tau = 1$ and of relative prudence of $1 + \tau = 2$.[13] The variables number of caught balls, unit cost of the next movement, total cost, point earnings per ball, and total point and euro earnings in the current work-shift are continuously updated on-screen during the task. Once the task is started by pressing the start button, it cannot be paused. When the work-shift ends (either by the computer or by the subject's choice), a feedback screen with the statistics mentioned before is shown.

Let us shortly delve into the practical problem of determining the (optimal) effort level. Since balls fall at a constant speed in randomly determined columns, one might get a quick revenue increase from moving the tray into the columns containing balls, but then would not be any better off (as the cost for movements increases disproportionately which makes future movements very costly). This trade-off means that, on average (due to the randomly determined columns), there is an optimal number of moves given the (expected) piece-rate. From our own experience when programming and testing the experimental software, one is best off by exercising constant effort over time within a shift: neither letting the tray stay put in one column for a long time, nor catching many balls in a short time span are good alternatives to "effort smoothing."[14]

[12] This logarithmic utility function is a special case of the shifted isoelastic utility function in Section 3.1: $\lim_{\tau \to 1} \left([1/(1-\tau)(y_i^{1-\tau} - 1)] - \eta \right) \zeta = (\ln(y_i) - \eta)\zeta$.

[13] This is in line with estimates in Fagereng et al. (2017, p. 396) using Norwegian administrative data.

[14] In our results, we will touch these issues only very shortly and in the Appendix, in Section J, we examine the question "learning how to produce"

Part 1 of the experiment: Trial periods During this phase, we let subjects play three incentivized trial periods so they can familiarize themselves with the user interface and mechanics of the task. Only one of the three trial periods is chosen randomly for payoff and feedback about the chosen period is only shown at the very end of the experiment.[15] In a first trial period, we deviate from the costly effort-incentive structure and make movements costless. We also abstract from the concave consumption function. Subjects are given 180 seconds to catch balls, with each caught ball generating earnings of 1 euro cent. There is no tradeoff between the returns from catching balls and the monetary costs of effort in this trial period. In the following two trial periods (and for the rest of the experiment), subjects work with the concave consumption function, the convex cost function, and the point earnings outlined before. In the second trial period, subjects work on the task and earn the certain wage, $w = 100$ for 180 seconds. In the third trial period, subjects work under uncertainty and either earn the low or the high wage, $w = 20$ or $w = 180$, with equal probability, for 180 seconds.

Part 2 of the experiment: Main treatments In Part 2, we conduct the four main treatments in the order in which we described them in Section 3.[16] In the instructions and on-screen we talk about four rounds, not about treatments. Here we will stick to the term 'treatments'. Each of the four treatments consists of two periods of 180 seconds each (the first one with the certain wage, the second with the uncertain wage) and two work-shifts (which are defined as the time where subjects work without a break on the task). Only one of the four treatments is chosen randomly for payoff. Furthermore, feedback about the chosen treatment is only presented at the very end of the experiment.

Treatment I This is the simplest treatment as subjects have neither the savings nor the time allocation option at their disposal. Subjects work on the task for two work-shifts that coincide with the periods (à 180 seconds). In the first work-shift, subjects earn the certain piece-rate $w_1 = 100$. In the second work-shift, they work under uncertainty and earn either the high rate, $w_2 = 180$, or the low rate, $w_2 = 20$. The instructions stress that the probability for the low or high rate is equal and independently drawn in each of the treatments.

Treatment II This treatment differs from Treatment I only in the savings decision. If subjects earned a positive euro amount in the first work-shift, they enter a screen where they can calculate the consequences of hypothetical saving decisions with a slider. They then enter the number of points they would like to save in a separate box. (The savings amount has to be non-negative, $s \geq 0$, and the highest amount that subjects can save is limited so that the euro earnings in work-shift 1 cannot become negative). After that, they press the OK button and proceed to the second work-shift. The amount of points saved is then deducted from the point earnings of the first work-shift and added to the point earnings in the second work-shift. See Figure D.1 in Section D of the Appendix for a screenshot of the savings screen.

across our four treatments and estimate production functions that determine the number of caught balls depending on the number of moves and in Section K, we use these production functions to calculate optimal saving and shifting levels in a structural model. However, our main results in this paper are treatment comparisons of savings and shifting decisions.

[15] This is a common technique in order to avoid portfolio effects when subjects make multiple decisions. It also helps us to keep each decision salient by paying a relatively high amount per decision. See Charness et al. (2016) for a discussion of paying one or few decisions vs. paying all.

[16] We considered randomizing the order of the four treatments between subjects. But by sticking to the order in Section 3, we gradually increase the level of difficulty. Thereby, we intend to limit our subjects' confusion that could occur due to, e.g., taking both a shifting decision and then a saving decision in the first round.

Treatment III This treatment differs from Treatment I in the time allocation between the two work-shifts. Subjects can freely divide the total amount of time, $T = 360$ seconds, between the two work-shifts. This is implemented in the following way: In work-shift 1 subjects are shown a button that allows them to immediately switch to work-shift 2 at any point of time (see Figure 4 for a screen-shot of a first work-shift with the switch button at the lower left corner of the task screen). The time remaining of the initial 360 seconds is then spent in the second work-shift. As soon as subjects enter the second period, the low wage's point revenue and euro earnings are displayed on the left-hand side of the task box, and the high wage's on the right-hand side of the task box.

Treatment IV In this treatment, both the savings decision of Treatment II and the time allocation of Treatment III are available to the subjects. First, subjects have to decide when to end work-shift 1. After being shown feedback on their outcomes in work-shift 1, subjects enter the savings screen where they can enter their savings decision.

Part 3 of the experiment: Elicitation of risk aversion and prudence In order to elicit the risk aversion and prudence of the subjects, we consecutively presented them with 12 binary choices between lotteries, as suggested by Noussair et al. (2014). Figure D.2 in Section D of the Appendix shows an example screenshot. Due to the potentially very high payoff of up to 165 euros, each subject only had a 1 in 20 chance of being randomly selected to receive a monetary payment from Part 3 of the experiment (which lasted about five minutes).

Post-experimental questionnaire In the post-experimental questionnaire, we asked the subjects for their gender, age, field of study, their number of semesters at university (including undergraduate studies), and how strenuous they perceived the experiment to be (on a scale from 1 (not at all strenuous) to 6 (very strenuous)). We also asked for a subjective self-assessment of their general level of risk aversion (the wording of this question is identical that used by the German Socioeconomic Panel (SOEP), with answers ranging from 0 (not at all willing to take risks) to 10 (very willing to take risks)). Moreover, we asked whether subjects knew anybody who previously participated in this experiment and whether they tended to pay attention to the low or to the high piece-rate in the periods where the rate was uncertain.[17]

Procedures and subjects Upon arrival at the laboratory, the subjects were seated in separate booths. Then, the subjects received printed instructions which included tables with selected values and graphs of the cost and consumption functions. After reading the instructions, the subjects had to answer a set of control questions correctly in order to proceed.[18] The experiment was computerized. Only after the subjects completed the three parts of the experiment and answered the questionnaire they did receive feedback about the outcomes of the experiment and their euro earnings. Finally, the payoff took place privately in a room separate from the other subjects.

All experiments were conducted in PLEx, the Potsdam Laboratory for Economic Experiments at Universität Potsdam[19], in November and December 2017. All 192 subjects were students of Universität Potsdam and other nearby

[17] Surprisingly, we do not find that any of our measures correlates with saving behavior in the experiment.

[18] You can find a translation of the instructions, the supplied tables, and the control questions in Sections A, B, and C of the Appendix. The original instructions in German are available upon request.

[19] Economic experiments are not subject to the university's Institutional Review Board.

universities (Freie Universität Berlin, Filmuniversität Potsdam, and University of Applied Sciences Potsdam) and were 18 years or older. Subjects were invited using ORSEE (Greiner 2015). We did not apply any exclusion criteria to the registered subjects in the database. Every subject participated only once. The experiments were run on z-Tree (Fischbacher 2007), in 19 sessions of 4 to 14 subjects (depending on enrollment to the experimental sessions and attendance of subjects). The laboratory sessions took about 90 minutes. On average, subjects earned about 15 euros (with a minimum of 0 euros and a maximum of 66.20 euros).[20] Section G in the Appendix presents a summary of our sample and discusses it briefly.

5 Results

Before we turn to the tests of the four hypotheses, we illustrate the most important data. Table 2 provides summary statistics of the data from the 192 subjects, separately for each of the treatments.

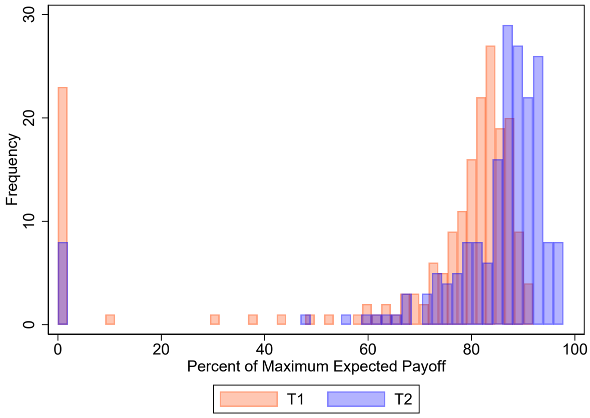
Table 2: Summary of data

Round	TI	TII	TIII	TIV
Movements in Shift 1	32.71	30.73	27.64	27.73
	[30.00]	[30.00]	[27.50]	[27.00]
	(18.44)	(17.37)	(17.15)	(16.53)
Movements in Shift 2	26.54	25.20	26.52	25.46
	[24.00]	[24.00]	[22.50]	[22.00]
	(17.53)	(14.93)	(21.60)	(19.76)
Savings		2011.64		1511.16
		[2000.00]		[1497.50]
		(1244.67)		(1115.59)
Share of Saved Income		0.30		0.25
		[0.30]		[0.24]
		(0.18)		(0.16)
Proportion With Savings		0.90		0.87
Higher than 100 Points		[1.00]		[1.00]
		(0.31)		(0.34)
Time Shift 1			165.66	170.53
			[162.00]	[181.00]
			(70.54)	(61.02)
Share of Shifted Income			0.15	0.09
			[0.15]	[0.00]
			(0.29)	(0.28)
Proportion With Work-Shift 1			0.59	0.47
Shorter than 180 Seconds			[1.00]	[0.00]
			(0.49)	(0.50)
Expected Payoff	8.77	11.20	9.07	10.93
	[11.82]	[12.69]	[12.14]	[12.72]
	(9.82)	(7.56)	(8.77)	(7.14)
Observations	192	192	192	192

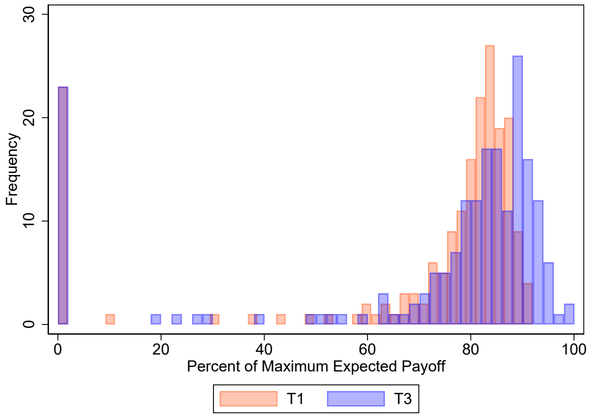
Notes: Mean, Median in square brackets, and standard deviation in parentheses.
Source: Own calculations.

Average and median movements in shift 1 are greater than in shift 2 in all treatments. Savings are larger in Treatment II than in Treatment IV, reflected also by the higher share of saved income. Average time spent in work-shift 1 is smaller than 180 seconds in both treatments, indicating precationary shifting. As the share of saved income, the share of shifted income is larger in Treatment III than in Treatment IV. However, in Treatment IV the sum of these

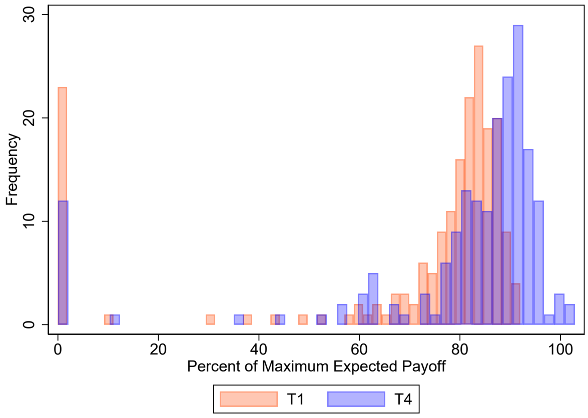
[20] One might criticize the low euro earnings in this experiment. As mentioned in the introduction, some subjects disregarded effort costs. In fact, 23 of the subjects earned zero euros. If we calculate the average without them, each subject earned about 17 euros. However, we do not exclude them from our analysis.



(a) Higher Expected Payoffs with Precautionary Saving



(b) Higher Expected Payoffs with Precautionary Shifting



(c) Higher Expected Payoffs with Saving and Shifting

Figure 5: Monetary Incentives

Notes: Actual expected payoffs relative to maximum expected payoff in percent.

Source: Authors' presentation.

two variables, 34 percent, is relevant. This means that in Treatment IV more income is transferred over time than in Treatments II and III. Finally, the proportion of subjects with savings larger than 100 points is very high, while only around 50-60 percent of subjects transfer income from shift 1 to shift 2 via time allocation.[21] Section I in the Appendix discusses differences across treatments taking subject specific effects into account.

Table 2 also shows that expected payoffs are greatest in Treatment II and smallest in Treatment I, where neither saving nor shifting is allowed. The entire distribution of expected payoffs as percentage of maximum expected payoff in each treatment is shown in Figure 5 in comparison with the first treatment without intertemporal choice. In the three treatments where subjects could smooth their consumption, expected payoffs are higher than in our baseline treatment, Treatment I.[22] This shows that the incentives provided by allowing intertemporal choice were used and shifted the payoff distributions to the right.

5.1 Tests of Hypotheses 1 to 3

In this section, we discuss our results regarding Hypotheses 1 to 3. Table 3 reports the findings for the first hypothesis. The results of t-tests suggest that there is the theoretically predicted effect of risk: in Treatment I, we observe significantly fewer movements in work-shift 2 than in work-shift 1 (p-value< 0.001). This provides strong evidence for Hypothesis 1.

Table 3: Tests of Hypotheses 1

	H1: Effort Smaller in Second Work-Shift than in First Work-Shift of TI
	Movements
Mean	TI Shift 1: 32.71
Mean	TI Shift 2: 26.54
Δ 95% Conf.	4.61 to 7.75
t-test p-val.	<0.001
χ^2 p-val.	<0.001
Rank-sum p-val.	<0.001

Notes: P-values are from t-tests with the hypothesis that the average difference of observations Δ is zero, from Pearson χ^2 tests with the hypothesis that the two independent samples were drawn from populations with the same median, and Wilcoxon rank-sum test with the hypothesis that the samples are from populations with the same distribution.

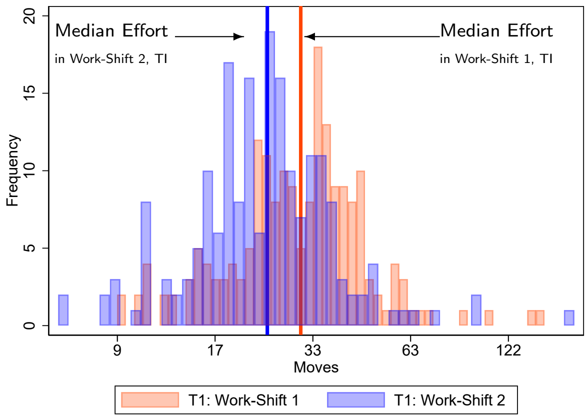
Source: Own calculations.

Figure 6a illustrates this finding. The distributions of movements are clearly different; the median values of each distribution, as indicated by the blue and red vertical lines, differ substantially (Wilcoxon rank-sum test has p-value< 0.01 and Pearson χ^2 test of the equality of the medians has p-value< 0.01).

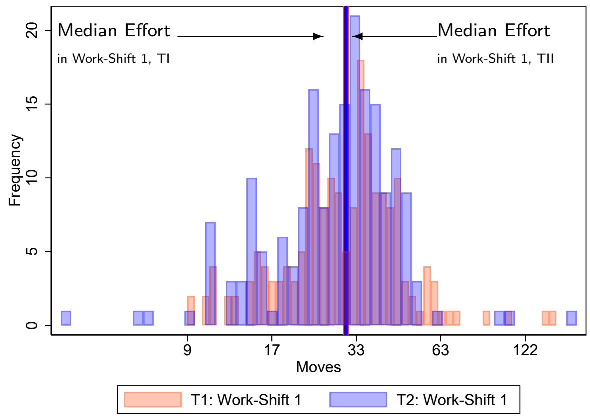
[21] We chose 100 points as the threshold in order to be far enough away from the zero-lower bound but the results change very little if we use > 0 points or > 1 point as criterion.

[22] Figure H.1 in the Appendix displays a 3D graph that shows expected earnings in all four treatments.

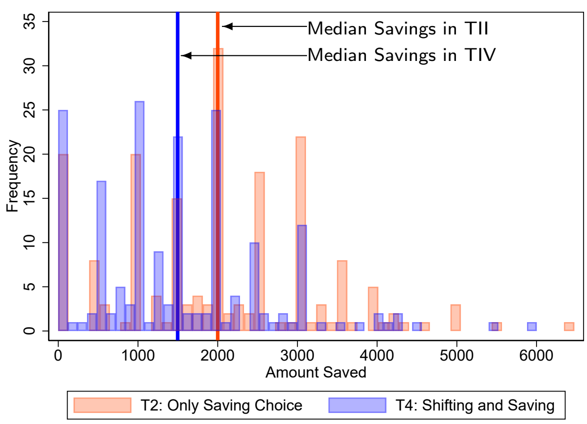
Result 1: *In Treatment I, effort in the second work-shift is smaller than in the first work-shift. Subjects behave according to Jensen's inequality and reduce effort under uncertainty by about 19 percent (according to the 95 percent confidence interval: 14-24 percent).*



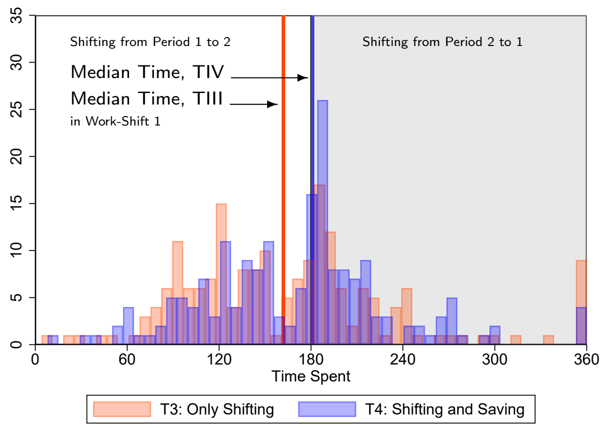
(a) H1: Effort Smaller in Second Work-Shift



(b) H2(i): Absence of Higher First Shift Effort



(c) H2(ii): Precautionary Saving



(d) H3: Precautionary Shifting

Figure 6: Tests of Hypotheses 1-3

Notes: Blue and red vertical lines represent medians, gray bars show optimal values, and gray background indicates borrowing.

Source: Authors' presentation.

Next, we discuss Hypotheses 2(i) and 2(ii). Hypothesis 2(i) states our expectation that precautionary effort does not exist. Again, we use t-tests to test our hypothesis. The results in Table 4 show that movements in both work-shifts are statistically different (p-value=0.016). Although this supports the notion that some subjects might have tried to exercise precautionary effort, the difference is not economically relevant.[23] Figure 6b illustrates this. The medians lie exactly on top of each other (Pearson χ^2 test of the equality of the medians has p-value$=0.54$) and also the distributions do not differ much (Wilcoxon rank-sum test has p-value$=0.36$). This provides evidence for Hypothesis

[23] In fact, as we show in Section I in the Appendix, subjects caught slightly fewer balls per move in all first shifts. If anything, this is evidence against precautionary effort.

2(i).

Table 4: Tests of Hypotheses 2 (i)

	H2(i): Absence of Precautionary Effort (Higher First Shift Effort in TII)
	Movements
Mean	TI Shift 1: 32.71
Mean	TII Shift 1: 30.73
Δ 95% Conf.	0.37 to 3.59
t-test p-val.	0.016
χ^2 p-val.	0.540
Rank-sum p-val.	0.358

Notes: P-values are from t-tests with the hypothesis that the average difference of observations Δ is zero, from Pearson χ^2 tests with the hypothesis that the two independent samples were drawn from populations with the same median, and Wilcoxon rank-sum test with the hypothesis that the samples are from populations with the same distribution.

Source: Own calculations.

Table 5 presents our evidence regarding the existence of precautionary saving (stated in Hypothesis 2(ii)). It shows that Hypothesis 2(ii) cannot be rejected in a test of proportions: In Treatment II 93 percent of the subjects saved more than 100 points (and 91 percent did so in Treatment IV); p-value= 0.05. According to the 95 percent interval, at least 85.26 percent of subjects in Treatment II behaved in this way (slightly less, 82.22 percent, in Treatment IV). This provides strong evidence for Hypothesis 2(ii). Both the upper and lower bound of the confidence interval are lower in Treatment IV than in Treatment II. This suggests savings were substituted by shifting. Figure 6c displays the saving amounts in Treatments II and IV. It shows that 20 subjects out of the 192 chose not to save in Treatment II (25 subjects in Treatment IV).

Table 5: Tests of Hypotheses 2 (ii)

	H2(ii): Proportion With Savings Higher than 100 Points Greater Zero			
	Mean	Std. Err.	95% Conf.	p-val.= 5%
TII	89.58	(2.20)	85.26 to 93.90	$H_0: 93.16$
TIII	—	—	—	—
TIV	86.98	(2.43)	82.22 to 91.74	$H_0: 91.02$

Notes: All proportions are given in percent. Proportions in column labeled "p-val.= 5%" are obtained from tests of proportion with p-value equal to 5%.

Source: Own calculations.

Result 2: *i. Precautionary effort is virtually absent, since median effort and thus effort costs are identical in the two work-shifts of Treatments II and IV.*

ii. In Treatment II, savings are strictly positive for at least 85 percent of subjects (and for 82 percent of subjects in Treatment IV).

Next, we test Hypothesis 3. A test of proportions reported in Table 6 strongly rejects the hypothesis that precautionary shifting does not exist. Even the hypothesis that 66 percent of the subjects in Treatment III shift work time in order to insure against risk can only be rejected at the 5-percent significance level (in Treatment IV, this applies to 54 percent of the subjects). According to the 95 percent confidence interval, at least 51.89 percent of the subjects ended their first work-shift before 180 seconds in Treatment III (in Treatment IV: 40.33 percent). This provides evidence in favor of Hypothesis 3. We can also see that a smaller fraction of subjects chose to shift in Treatment IV than in Treatment III. Once the possibility of intertemporal substitution via savings was given, many subjects substituted finishing work-shift 1 early with savings — this forestalls Hypothesis 4.

Table 6: Tests of Hypotheses 3

	H3: Proportion With Work-Shift 1 Shorter than 180 Seconds Greater Zero			
	Mean	Std. Err.	95% Conf.	p-val.= 5%
TII	—	—	—	—
TIII	58.85	(3.55)	51.89 to 65.81	$H_0: 65.57$
TIV	47.40	(3.60)	40.33 to 54.46	$H_0: 54.44$

Notes: All proportions are given in percent. Proportions in column labeled "p-val.= 5%" are obtained from tests of proportion with p-value equal to 5%.

Source: Own calculations.

Interestingly, the difference of the average proportions across treatments is smaller for saving than for shifting.[24] The reduction of saving and shifting when the subjects have both channels is in line with our theory. Figure 6d shows the time spent in work-shift 1. The median work-shift choice indicated by the solid red bar is less than 180 seconds in Treatment III, where only shifting is possible. However, the median in Treatment IV is exactly at 180 seconds. Both distributions have two local peaks, one at 180 seconds and one at 120 seconds in both treatments. This suggests that subjects choose at least two distinct strategies — the first group sticks to the learned strategy of equal time allocation to both work-shifts, the other group adjusts according to theory.

Result 3: *In Treatment III, work-shift 1 is shorter than work-shift 2 for around 59 percent of the subjects (according to the 95-percent confidence interval: 52 to 66 percent). In Treatment IV, this share is lower with around 47 percent on average and between 30 and 54 percent according to the 95-percent confidence interval.*

In the Appendix, we show the show additional results and robustness checks. Section I in the Appendix shows that subject-specific factors like individual ability do not confound the results by providing comparisons of average

[24] Because the share of savers in TII (89.58%) minus the share of savers in TIV (86.98%) is smaller than the share of shifters in TII (58.85%) minus the share of shifters in TIV (47.40%).

outcomes across treatments obtained with fixed effects regressions. Section J in the Appendix describes the estimation of the production functions used in Section K in the Appendix to calculate optimal saving and shifting levels. Even though there is substantial heterogeneity, the predicted level of savings is not significantly different from the observed level. The level of shifting is, however, overpredicted by our theory.

5.2 Test of Hypothesis 4: Is Shifting a Substitute for Saving?

In this section, we test whether and how subjects substitute savings for shiftings. Figure 7 presents visual evidence for how subjects substitute the two methods of intertemporal substitution. Figure 7a plots the share of income saved in Treatment II, where only saving is allowed, on the horizontal axis and the same figure from Treatment IV, where both saving and shifting are allowed, on the vertical axis. The linear regression line is flatter than the 45-degree line (slope 0.383 with standard error of 0.034; pairwise correlation coefficient of the data points $\rho = 0.375$; p-value=0.000). This is consistent with the substitution of saving and shifting.

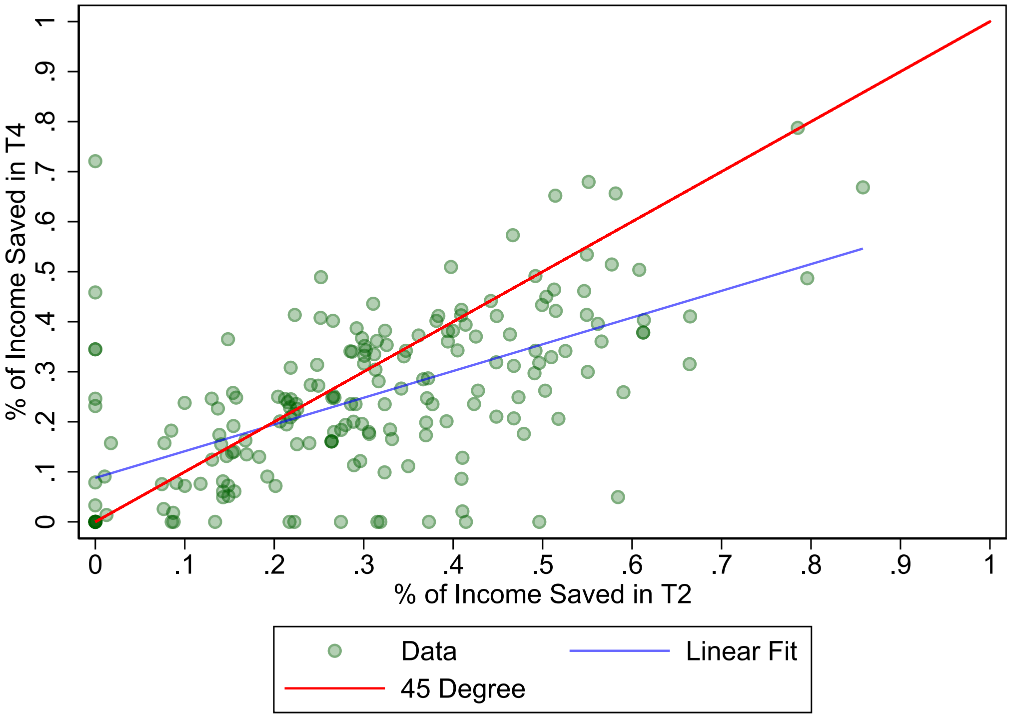
Figure 7b shows similar evidence for shifting in Treatments III and IV. Here the share of income *shifted* in Treatment III is presented on the horizontal axis and the same figure in Treatment IV on the vertical axis. Again, the regression line is flatter than the 45-degree line (slope 0.385 with standard error of 0.023; pairwise correlation coefficient of the data points is $\rho = 0.524$; p-value=0.000). This finding is also in line with the substitution of saving and shifting.

In Figure 7b, we can visually identify two groups of subjects: (i) subjects that choose to be on or close to the 45-degree line, and (ii) subjects that replicate both restrictions of Treatment I in both Treatment III and IV (resulting in the cluster around the 0 percent/0 percent point). The latter group seems to abstract from risk to simplify the intertemporal model.

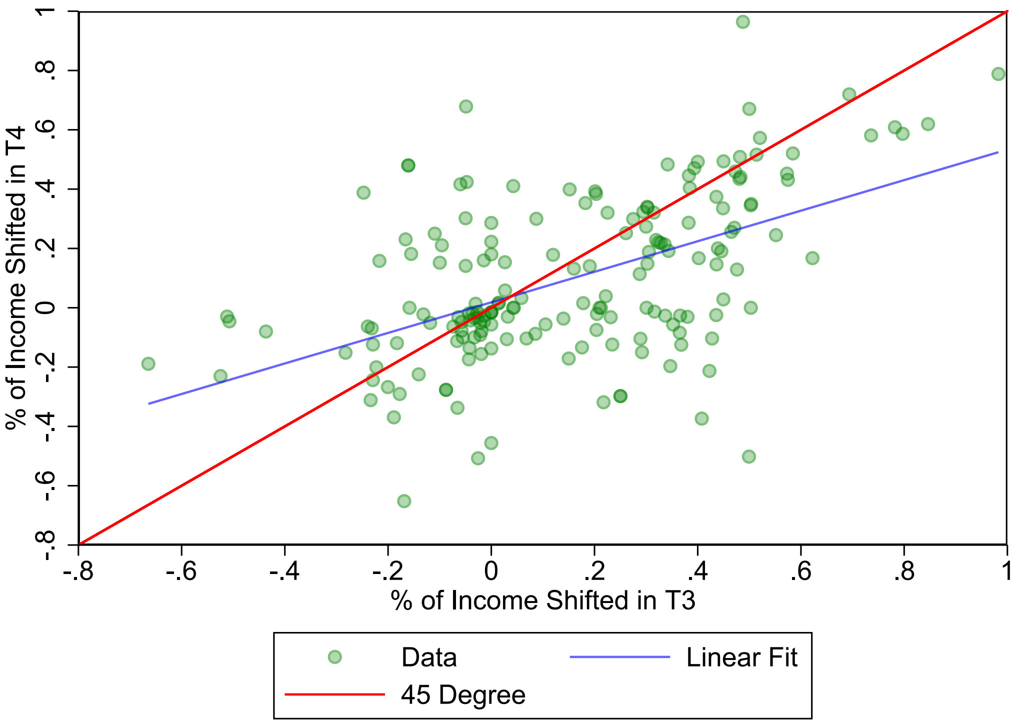
Taken together, our results from this and the previous section provide evidence that shifting and saving are indeed substitutes, though not for all subjects. If they were *perfect* substitutes, the average earnings would be identical across Treatments II, III, and IV. Do expected euro earnings differ depending on which choices are available? To answer this question, we conduct OLS regressions of expected euro earnings (euro earnings for low and high wages, weighted with equal probability), euro earnings if the low wage is realized, and euro earnings if the high wage is realized on treatment dummies (Treatment I serves as the baseline). Table 7 shows the results.

First, we consider all observations. In the first column, we observe that subjects in Treatments II, III, and IV earn significantly more than in Treatment I (before knowing which of the two possible states of the world occurs). When we compare the earning differences in Treatments II, III, and IV, we see that earnings in Treatments II and IV are not significantly different from one another and that earnings in Treatment III are significantly lower than in Treatments II and IV.

The second and third column show how euro earnings are affected ex-post. In case the ‘bad’ state of the world occurs, in column 2 the same pattern as under uncertainty emerges (only with higher magnitudes of the constant and the treatment dummies’ coefficients). This means that subjects use saving and shifting as precautionary measures against a ‘rainy day’ but not as a perfect substitute for one another.



(a) H4(i): Less Savings if Shifting is Allowed



(b) H4(i): Longer First Work-Shift if Saving Allowed

Figure 7: Test of Hypothesis 4

Source: Authors' presentation.

Table 7: OLS regression of euro earnings on treatment dummies

	Full Sample			Income Cut≥0		
Euro earnings	Expected	Low	High	Expected	Low	High
Treatment I (Constant)	8.76^{***,a,c}	2.38^{***,a,b,c}	15.14***	8.76^{***,a,b,c}	2.38^{***,a,c}	15.14***
	(0.71)	(0.84)	(0.67)	(0.71)	(0.84)	(0.67)
Treatment II-Dummy	+2.43^{***,b}	+5.01^{***,b}	-0.14	+2.43***	+5.01***	-0.14
	(0.41)	(0.58)	(0.37)	(0.41)	(0.58)	(0.37)
Treatment III-Dummy	+1.09^{**,a,c}	+2.79^{***,a,c}	-0.61	+1.48**	+3.43^{***,c}	-0.46
	(0.52)	(0.68)	(0.52)	(0.63)	(0.84)	(0.61)
Treatment IV-Dummy	+2.09^{***,b}	+4.69^{***,b}	-0.51	+2.40***	+5.06^{***,b}	-0.25
	(0.54)	(0.68)	(0.53)	(0.61)	(0.74)	(0.61)
Observations	768	768	768	665	665	665

Robust standard errors clustered at subject level.
Significantly different from zero at the 1%-level: ***, 5%-level: **.
Significantly different from Treatment II's coefficient at the 1%-level: ^{a}, from Treatment III's: ^{b}, from Treatment IV's: ^{c}.
Source: Own calculations.

In column 3 we can see how much income the subjects on average give up in case they do not need to insure, i.e., the 'good' state of the world occurs. None of the coefficients for Treatments 2, 3 and 4 is significantly different from zero: hence, the price subjects pay for their precautionary behavior is rather low though the benefits are high. Our results show that more flexibility does not necessarily lead to better outcomes. In fact, subjects attained the highest payoffs in Treatment II, where they did not need to make an impulsive shifting decision but instead had time to contemplate several possible saving choices.

Finally, we consider the last three columns where we restrict the sample to subjects who behaved in line with theory and decided not to borrow. Here, the coefficients for expected euro earnings are not significantly different across all treatments. Some coefficients for the 'good' and 'bad' state of the world are larger. These subjects are indeed better in using precautionary measures.

Result 4: *i. We reject that saving and shifting are perfect substitutes on aggregate. In our experiments, we observe considerable heterogeneity between the subjects in respect with substituting saving and shifting. Linear regressions show that subjects substituted saving and shifting when given the opportunity to do so.*

ii. As predicted, expected payoffs are significantly higher if either shifting or saving is allowed than when neither is allowed. We also observe an ordering of expected payoffs where theory does not predict one: Expected payoffs are significantly lower if only shifting is allowed compared to the case where either only saving is allowed or both saving and shifting are allowed. Thus, while saving and shifting are substitutes, they are not perfect substitutes.

6 Conclusions

In this paper, we present a two-period model with wage uncertainty. Our innovation is that we give the individual the opportunity to smooth consumption by allocating time between periods (this new channel seems to become increasingly important, as indicated by the emergence of the so-called gig economy). We use this model to compare four possible scenarios that individuals might be faced with by turning on and off our new shifting channel and the well-established saving channel. Subsequently, we derive four hypotheses that we then test in laboratory experiments. A novel feature of our consumption/saving experiments is that we tie them to a real-effort style task. This extends previous experiments by introducing an endowment effect, since subjects decide over own earnings rather than randomly drawn income. We cannot reject the first three hypotheses that test whether the subjects in our experiments behave according to theory when given either the saving or the shifting opportunity. Evidence supports our fourth hypothesis as well. We find that subjects substitute saving for shifting and shifting for saving. However, we also observe that the subjects do not substitute saving and shifting perfectly (as shown by significantly lower expected earnings in the treatment where only shifting is allowed). This is similar to the result in Brown et al. (2019) who reject the strong form model implying that gifts of time and money are perfect substitutes.

Taken together, we interpret our findings the following way: as in Ballinger et al. (2003) we find that the model predicts behavior quite well. However, in contrast to Ballinger et al., the strong heterogeneity that we document suggests that average behavior is driven by (at least) two types: one group of subjects adjusts according to theory, the other one sticks to the learned status quo.

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Appendix

Appendix

A Translation of the Instructions

INSTRUCTIONS

Welcome to this experiment!

In this experiment, you can earn a considerable amount of money. Your earnings in this experiment depend *only* on the choices *you* make during the experiment. Please read the printed instructions and those shown on-screen carefully. During the experiment, you are not allowed to use electronic devices other than your PC or to talk to other participants. Please only use the computer programs and functions designated for the experiment. Should you have any questions, please raise your hand. We will then quietly answer your question. If the question is of relevance for all participants, we will loudly repeat and answer it.

Outline

Please read the instructions carefully. Afterward, you will answer a few **quiz questions** to make sure you understand everything. Overall, the experiment will take about 1.5 hours.

The experiment is made up of **three parts**. The payoff you are able to receive in each separate part does not depend on your behavior in the other parts.

Part 1

Part 1 is made up of three test periods, which gives you the opportunity to practice the **assignment** you will work on in the second part (the assignment will be explained further down in Part 2). One of the three test periods is randomly chosen for payoff. Only at the end of the experiment, you will be informed about which period was chosen. Further information will show up on your screen.

Part 2

The second part is made up of **four different rounds**, which consist of **two shifts** each. *The time during which you are working on your assignment without an interruption is referred to as a* ***shift****.* Only one of the four rounds is relevant for your payoff. Which of the four round earnings will be paid out will be chosen at random. Only at the very end of the experiment, you will be informed about the round that was chosen for payoff.

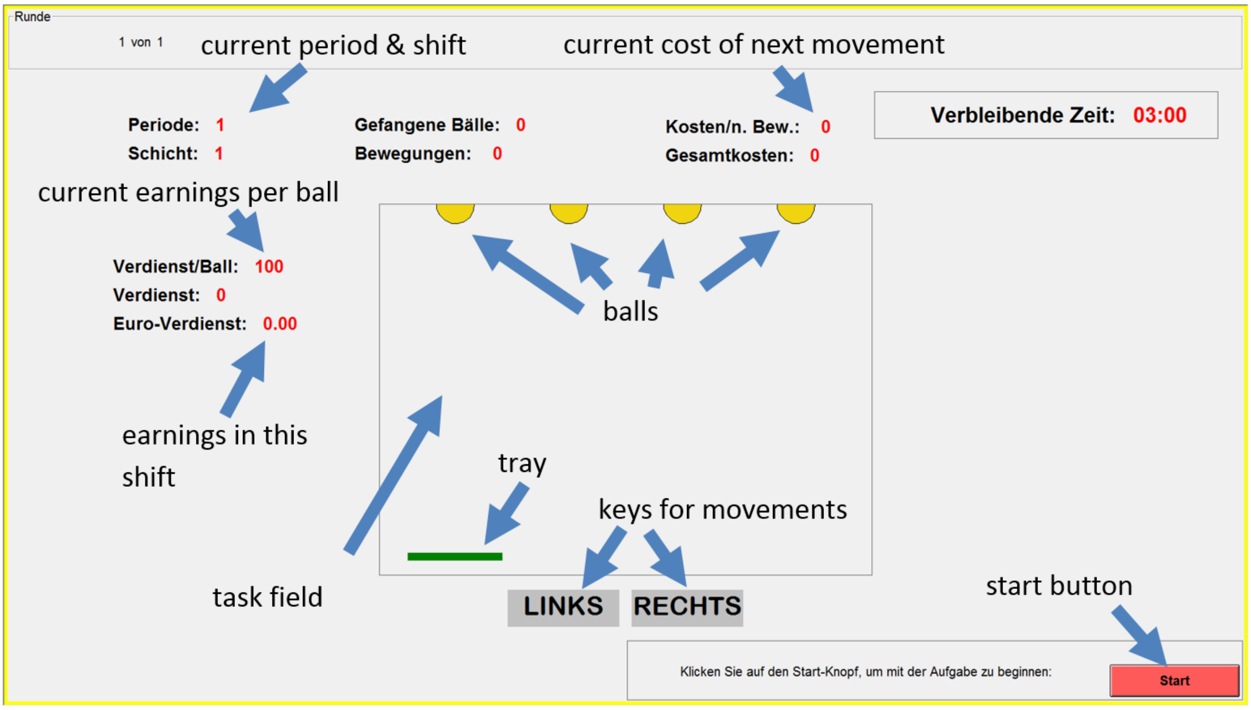
By working on the assignment you can earn **points**. Points are the currency of this experiment. The points you earn during one shift will be converted into euros.

Round 1

In the first round, you are working on the assignment consisting of two shifts. In this round, each shift consists of a period, which always lasts 180 seconds. *A* ***period*** *is the time during which a particular earning is paid.*

Your assignment

While working on the assignment you will see a **task field** in the middle of the screen, similar to the following figure. Left of the task field you can see in which **period** and which **shift** you are currently in. As soon as you click the **start button**, the countdown starts and balls start falling randomly from the upper part of the task field. The remaining time is shown in the upper right corner of the screen. The catching tray can be moved by clicking **"LEFT"** or **"RIGHT"**



at the bottom part of the task field, in order to catch the balls. To catch a ball, the **catching tray** has to be positioned right underneath the ball, at the moment the ball touches the tray. As soon as the ball touches the tray, the number of balls caught increases by one. The **number of the balls caught so far** and the **number of the current moves** are shown above the task field.

Each move of the catching tray generates costs. Each ball caught generates earnings. The **cost of the next move** is shown above the task field. Underneath you can find the **current overall costs**. The **current earnings per ball** are shown left of the task field. Underneath you can see your earnings in this shift in points and in euros.

Earnings in points are calculated as followed:

Earnings = Number of balls caught * Earnings per ball caught – Sum of the costs of the moves

Earnings per ball caught

In each period you will be informed about the **earnings per ball caught**. Your earnings per ball caught are always 100 points throughout the first period. In the **second period**, your earnings per ball caught are determined **randomly**. The earnings may either be 180 points or 20 points. Both values occur with equal probability of 50 percent. In the second period, the point and euro earnings for both 20 and 180 points can be found on the left and the right side of the task field. Only at the end of the experiment you will learn which earning will be paid in the second period.

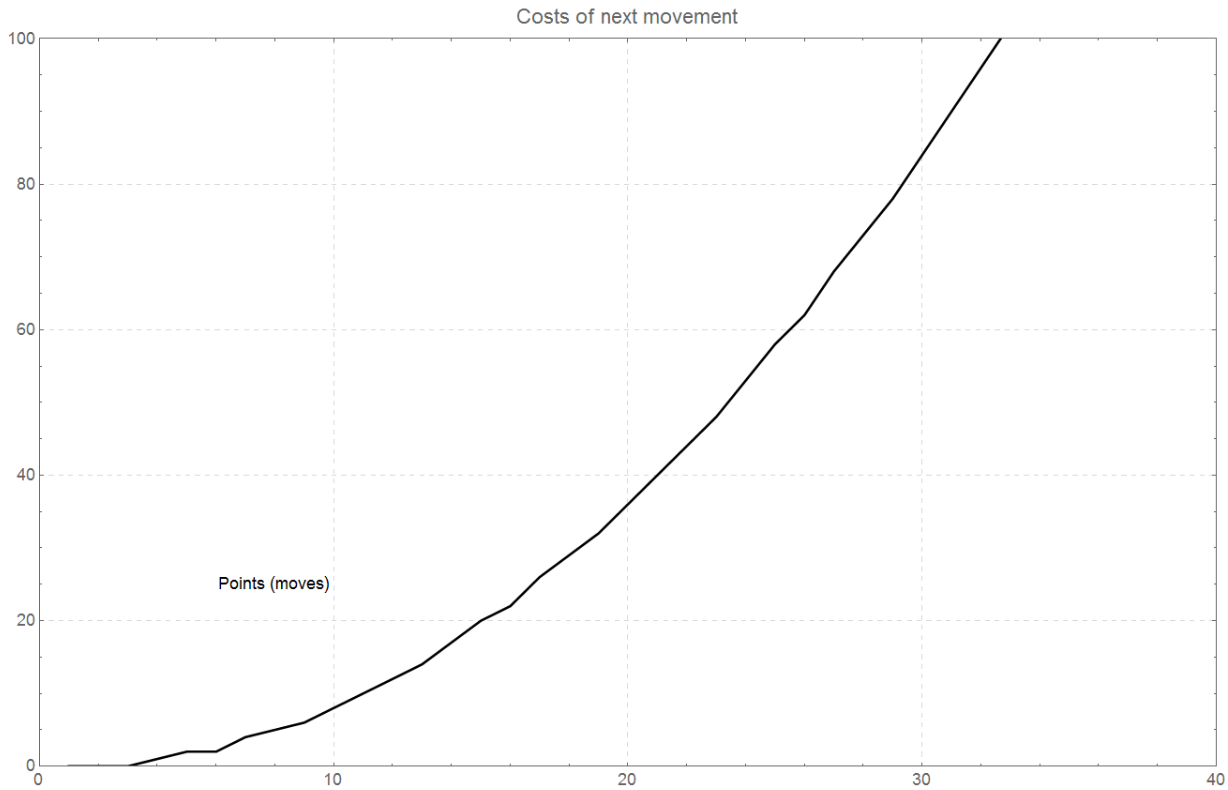
It is important to understand that your earnings per ball caught are randomly generated in the second period. Which value your earnings have in one period, **neither** depends on the value your earnings had in previous periods **nor** on the way you behaved in the previous periods. Only at the very end of the experiment you will be informed about the actual value of your earnings in the second period. That implies that for the duration of the task, you do not know which earnings are relevant for payoff, 20 or 180 points.

Costs for moves

At the start of each shift the **cost for a move** is always zero points. The cost per move increases in the number of moves:

Cost per move = 0.1 * (number of moves so far)^2

The cost per move is rounded to the closest integer. A table with chosen function values is included in the instructions.



Example: Supposing the number of your current moves is 30. The costs per move are calculated as 30*30*0.1 = 90. The next click on "LEFT" or "RIGHT" consequently costs 90 points. After the next click, the number of your current moves increases by one. The costs per move are calculated as 31*31*0.1 = 96.1. The result is rounded to the next integer, 96.

Shift result

The sum of all the points you earned in one shift is your **shift result**. The higher your shift result, meaning the sum of all points earned in one shift, the higher is the payoff in this particular shift. The shift result is converted to euros as followed:

Shift result in euros = 4*[ln(shift result in points) – 7].

The following illustration shows the shift result in euros, depending on the points earned. A table with chosen function values is included in the instructions.

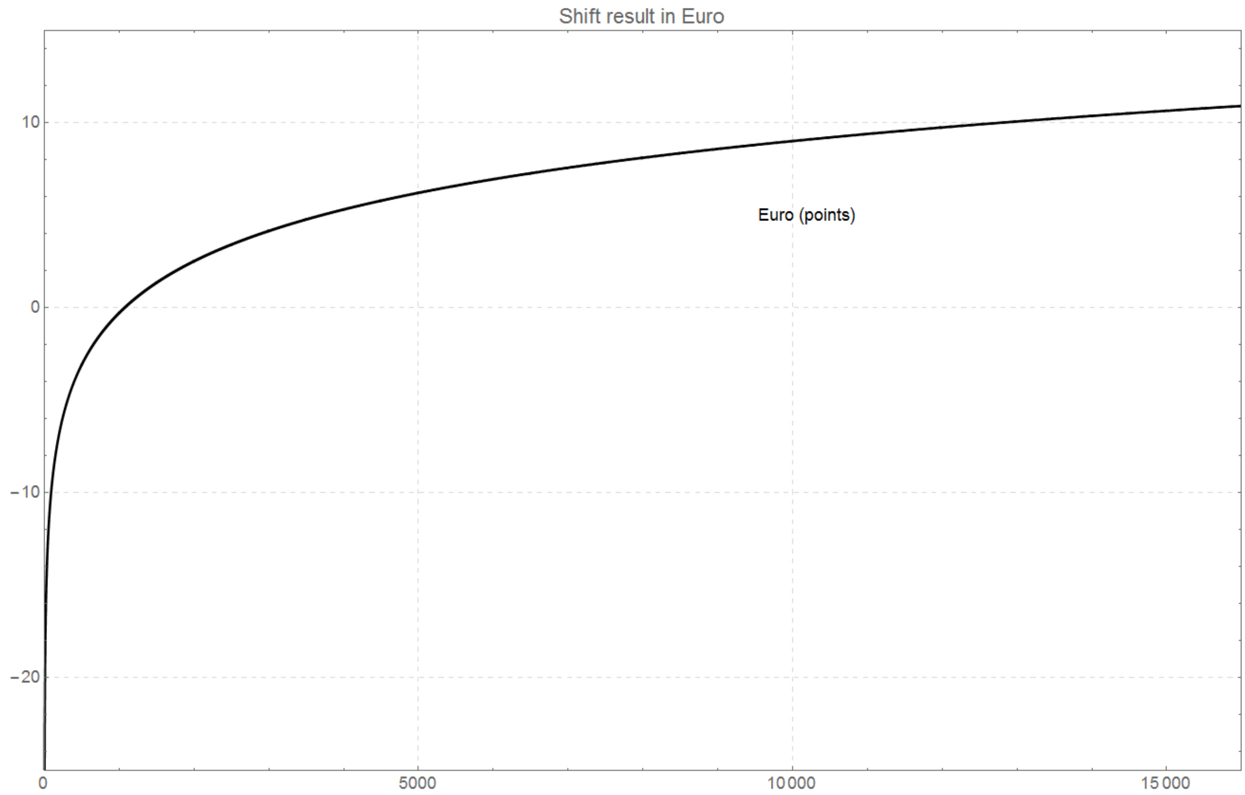
Example: Suppose the number of points you earned in the first shift of a round is 6400. Your result for this shift equals 4*[ln(6400) – 7) = 7.21 euros. In case you earn 100,000 points in the second shift, your result for this shift is 4*[ln(100,000) – 7] = 18.32 euros.

Rounds 2 to 4

The following sections inform you how rounds 2 to 4 differ from round 1.

Round 2

In round 2 you work on the task the same way you did in round 1, for two shifts (which correspond to the periods that last 180 seconds each). Now you have the opportunity to **save** points after the first shift. You can transfer points from



the first shift to the second shift. Points that you save are subtracted from the first shift result (accordingly, you earn a lower euro amount in the first shift). The saved points are added to your second shift result (thus, resulting in a higher shift result in euros).

You can save at most so many points that your euro earnings in the first shift are zero. You cannot save a negative amount of points.

Round 3

In round 3 you can decide **how much time** you want to spend in each shift. Overall, you have **360 seconds** at your disposal. The earnings per ball caught in the first period (the first 180 seconds) remain 100 points and the earnings in the second period (the following 180 seconds) remain either 20 or 180 points.

With a button under the task field, you can decide when to end the first shift. After that, the second shift begins.

Example 1: Suppose you end the first shift after 120 seconds. Your shift result for the first shift will be calculated based on the earnings and costs for these 120 seconds. (During these 120 seconds, your earnings per ball caught equal 100 points since you are in the first period.) In the following shift 2, you work on the task for 240 seconds (360 minus 120 seconds). In the first 60 seconds of the second shift, you are still in period 1, meaning you earn 100 points per ball caught. In the following 180 seconds, you are in period 2 and earn either 20 or 180 points per ball caught. Your shift result in points in shift 2 is the sum of the earnings of both periods minus the cost for moves.

Example 2: Suppose you end the first shift after 240 seconds. During the first shift, you are in period 1 during the first 180 seconds and earn 100 points per ball caught. In the following 60 seconds, you are in period 2 and earn either 20 or 180 points (of which the costs are then subtracted). Throughout the second shift (which only lasts 120 seconds) you are in period 2 and earn either 20 or 180 points per ball caught.

Round 4

In round 4 you can save points after the first shift (just as in round 2) as well as decide on the time you want to spend in each shift (just as in round 3).

Part 3

The third part is with regards to content completely unrelated to the first two parts. The instructions for the third part will be shown only on your screen.

Overall pay-out in euros

The result for a round equals the sum of both shift results.

Round result = shift 1 result in euros + shift 2 result in euros.

The overall payoff is calculated as followed:

Overall payoff = result of a random period of part 1 + result of a random round of part 2 + amount earned in part 3

The payoff of the random round is rounded to cents. This amount can drop under zero euros, meaning your payoff might be **negative**. In this case, the loss will be settled with the earnings of the other parts. You will not leave this experiment with a loss: Should the overall payoff be negative, you do not get a pay-out.

Questions

Now please answer the quiz questions about the contents of these instructions. Please raise your arm once you are done. In case you have any questions, please also raise your arm. A person in charge will come to you and answer the question.

B Tables with Selected Values of the Consumption and Cost Function (Part of the Printed Instructions)

Table B.1: Cost Function

Costs	
Number of movements so far	Cost of next movement in points
0	0
2	0
4	2
6	4
8	6
10	10
12	14
14	20
16	26
18	32
20	40
22	48
24	58
26	68
28	78
30	90
32	102
34	116
36	130
38	144
40	160
42	176
44	194
46	212
48	230
50	250
52	270
54	292
56	314
58	336
60	360

Table B.2: Consumption Function

Shift earnings	
Earned points	Value in euros
0	-25.00
1000	-0.37
2000	2.40
3000	4.03
4000	5.18
5000	6.07
6000	6.80
7000	7.41
8000	7.95
9000	8.42
10000	8.84
11000	9.22
12000	9.57
13000	9.89
14000	10.19
15000	10.46
16000	10.72

C Translation of the Quiz Questions (With Correct Answers)

QUIZ QUESTIONS

Please answer the following questions before the experiment starts. With these questions we merely intent to make sure that you understand the instructions properly.

1. True or false? Your earnings in period 1 are always 100 points.
   ☒True ☐False

2. What is the probability that your earnings per ball caught in period 2 are 180 points?
   50%

3. True or false? In rounds 3 and 4 you can influence the total duration for which you earn 100 points per ball caught.
   ☐True ☒False

4. True or false? Each time a new shift begins the costs per movement are reset to zero.
   ☒True ☐False

5. In each shift increase the costs per movement in the number of movements so far. But this increase becomes flatter in the number of movements so far.
   ☐True ☒False

6. Suppose you earned 10,000 points in the first shift and 1,000 points in the second shift. What are your euro earnings in each shift and in the round?
   10,000 points = 8.84 euros; 1,000 points = -0.37 euros; together 8.47 euros

7. Suppose that you (based on the earnings given under 6.) saved 2,000 points. What are your euro earnings in each shift and in the round?
   8,000 points = 7.95 euros; 3,000 points = 4.03 euros; together 11.98 euros

8. Suppose that you spent 100 seconds in the first shift.

   a) How many seconds will you spend in shift 2?
      260 seconds

   b) For how many seconds will you earn 100 points per ball caught in shift 2?
      80 seconds

   c) For how many seconds will you earn either 20 or 180 points per ball caught in shift 2?
      180 seconds

9. True or false? You will not learn your payoff during the entire experiment. Only at the very end you will learn this.
   ☒True ☐False

D Example Screen-Shots of the Computer Interface

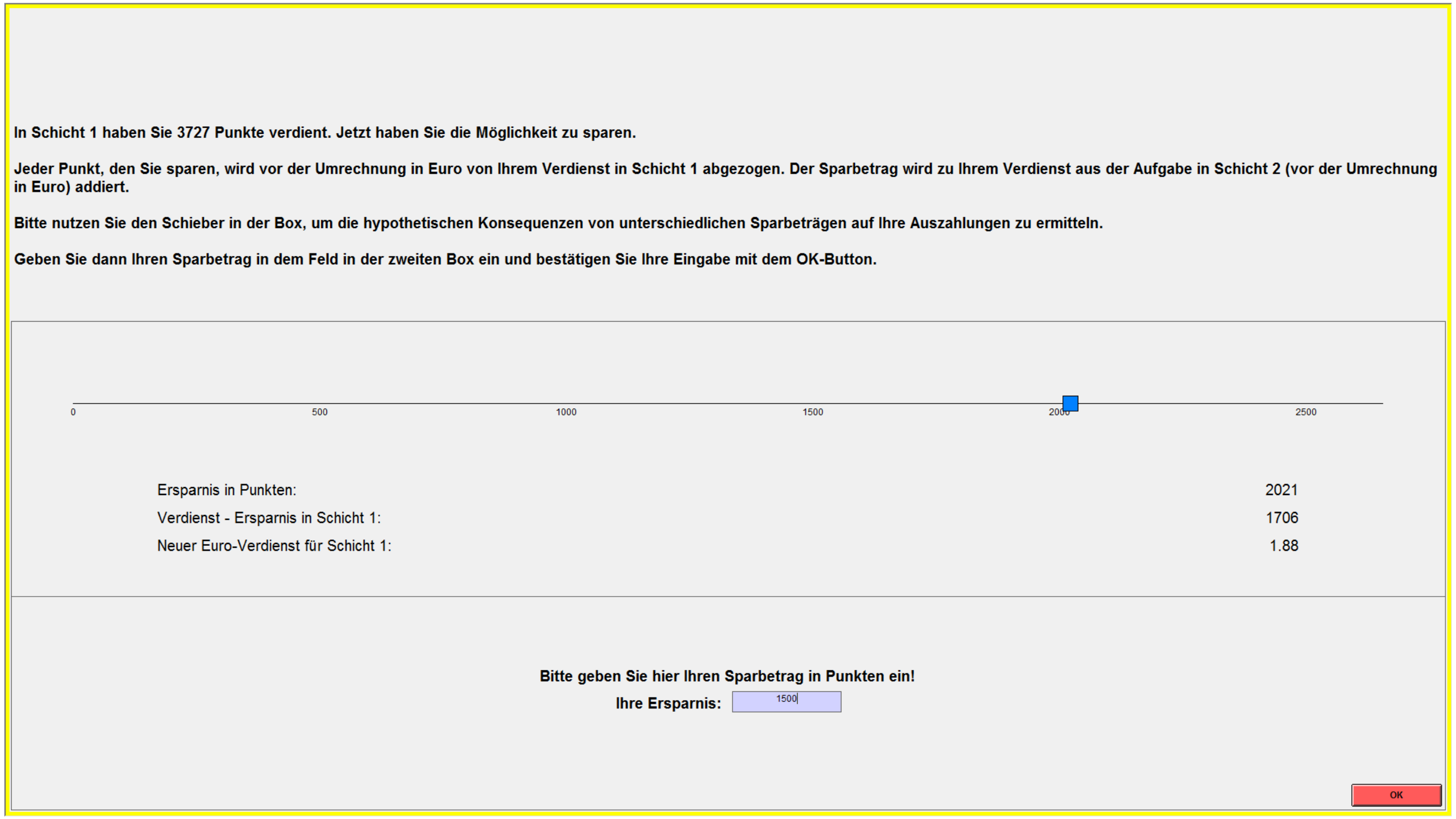


Figure D.1: Screenshot of Saving Screen.

Source: Own interface based on z-Tree.

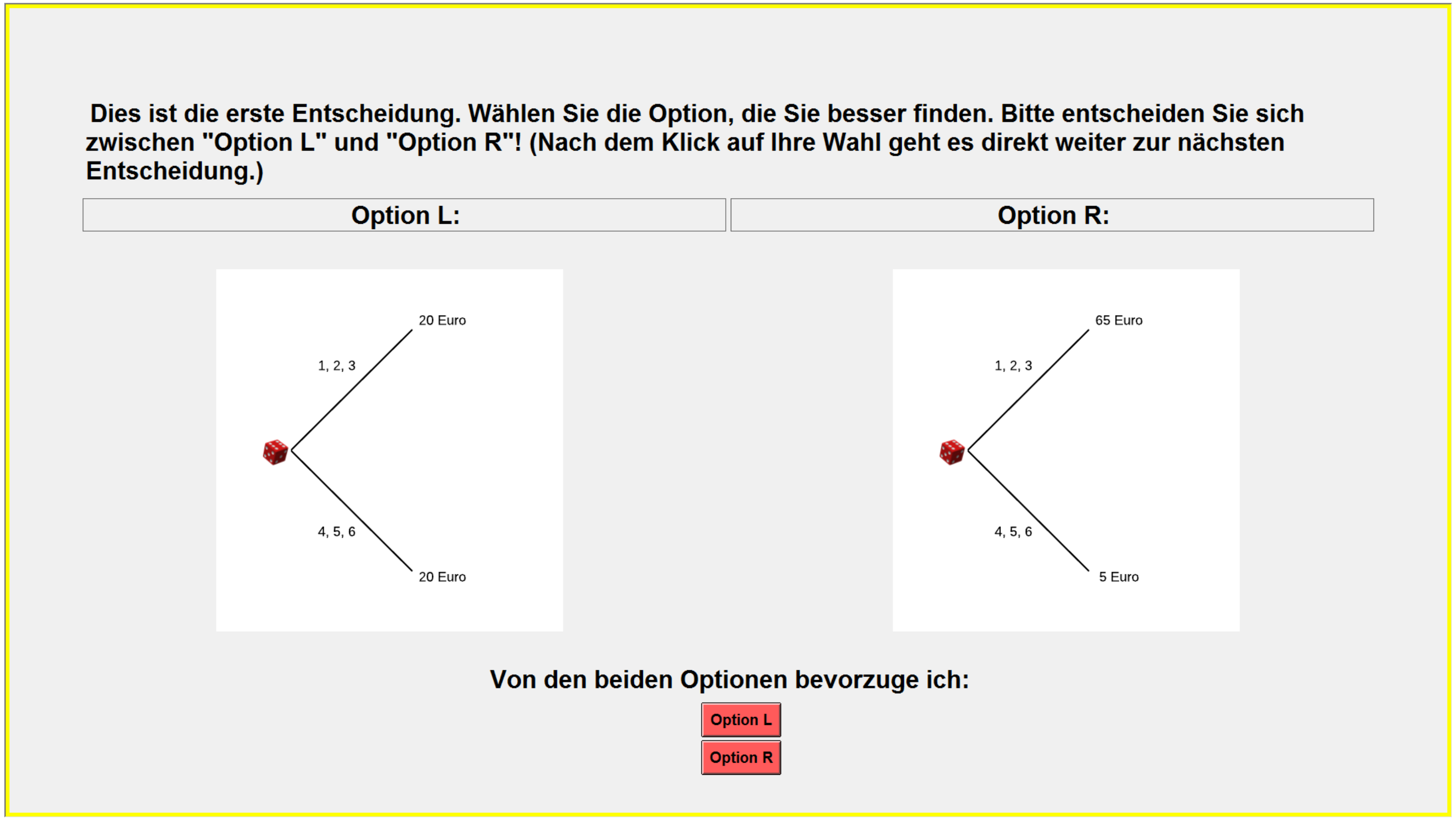


Figure D.2: Screenshot of the Experimental Interface with the Elicitation of the Risk Aversion and Prudence.

Source: Own interface based on z-Tree.

E Existence and Importance of Precautionary Saving in Extant Literature

Table E.1: Literature on Precautionary Saving

Study	Data Set	Data Period	Measures of Risk	Precautionary Saving
Lab experiment				
Meissner and Rostam-Afschar (2017)	Students at TU-Berlin	Eight life cycles à 25 periods	35% of expected value with probability 0.5	No evidence
Brown et al. (2009)	Students at National University of Singapore and California Institute of Technology	Seven life cycles à 30 periods	Log-normally distributed	Undersaving
Ballinger et al. (2003)	Students at Univeristy of Huston and Stephen F. Austin State University	One life cycle à 60 periods	Two treatments: 3 francs (5%) or 5 francs (5%); otherwise, 4 francs, 50% 8 francs and 50% 0 francs	$> 0\%$, but undersaving
Hey and Dardanoni (1988)	Students at University of York	between 5 and 15 periods	normally distributed	—
Wealth regression				
Mastrogiacomo and Alessie (2014)	DHS	1993-2008	Subjective earnings variance, second income earner	30%
Fossen and Rostam-Afschar (2013)	SOEP	2002, 2007, 1984–2007	Heteroskedasticity function	0-20%
Hurst et al. (2010)	PSID	1984, 1994, 1981-1987, 1991-1997	Permanent and transitory components of earnings regression	$< 10\%$
Bartzsch (2008)	SOEP	2002, 1980-2003	Variance of income	0-20%
Fuchs-Schündeln and Schündeln (2005)	SOEP	1992-2000	Civil servant indicator	12.9-22.1%
Carroll and Samwick (1998)	PSID	1984, 1981–1987	Variance of income	32-50%
Lusardi (1998)	HRS	1992	Self-reported	1-3.5%
Lusardi (1997)	SHIW	1989	Self-reported	2.8%
Kazarosian (1997)	NLS	1966-1981	Permanent and transitory components of earnings regression	29%
Guiso et al. (1992)	SHIW	1989	Self-reported	2%
Dardanoni (1991)	UK Family Expenditure Survey	1984	Variance of labor income	$> 60\%$

Table continued on next page.

Study	Data Set	Data Period	Measures of Risk	Precautionary Saving
Hours of work regression				
Jessen et al. (2017)	SOEP	2001-2012	Standard deviation of past detrended log wages	1.16 hours per week
Benito (2006)	BHPS	1991-2007	Difference between actual and expected financial situation	$<$ 1.4 hours per week
Parker et al. (2005)	PSID	1968-1993	Standard deviation of past wages	1.68 hours per week
Pistaferri (2003)	SHIW	1989, 1991, and 1993	Subjective information on future income	negligible
Saving regression				
Broadway and Haisken-DeNew (2018)	HILDA, CASiE	2002, 2006 and 2010	Subjective and objective uncertainty	0.35%
Ventura and Eisenhauer (2006)	SHIW	1993;1995	Average income variance	15-36%
Skinner (1988)	CEX	1972-1973	Occupation indicators	0%
Estimation of Consumption Euler Equation				
Dynan (1993)	CEX	Four quarters of 1985	Consumption variability	0%
Skinner (1988)	CEX			56%
Method of Simulated Moments				
Cagetti (2003)	SCF, PSID	1989, 1992, 1995; 1984, 1989,1994	Permanent and transitory components of earnings regression	50-100%
Gourinchas and Parker (2002)	CEX, PSID	1980-1993	Permanent and transitory components of earnings regression, prob of zero earnings	60-70%
Numerically Simulated Consumption Function				
Pijoan-Mas (2006)	PSID			18.0%
Zeldes (1989)	from other studies			1.6-10%
Skinner (1988)	CEX			56%
Calibrated Closed Form Consumption Function				
Caballero (1991)				$>$ 60%

Notes: Importance figure is sometimes calculated from several sources in the respective paper, please read the paper for details. Datasets are De Nederlandsche Bank household survey (DHS), German Socio-Economic Panel (SOEP), Italian Survey of Household Income and Wealth (SHIW), Household, Income and Labour Dynamics in Australia (HILDA), Consumer Attitudes, Sentiments and Expectations (CASiE), British Household Panel Survey (BHPS), National Longitudinal Survey (NLS), Health and Retirement Study (HRS), Consumer Expenditure Survey (CEX), Survey of Consumer Finances (SCF), Panel Study of Income Dynamics (PSID). Surveys of further related studies can be found in Jappelli and Pistaferri (2017) or Lugilde et al. (2019).

F First Order Conditions

Treatment I The first order conditions of the Lagrangian are (with partial derivatives denoted, e.g., in the form of $c_{y_i} = \frac{\partial c(y_i, e_i)}{\partial y_i}$, and the Lagrange multiplier denoted for treatment k as μ^k)

$$\begin{aligned} \frac{\partial \mathscr{L}_i^I}{\partial y_i} &= E_\varepsilon[c_{y_i}] - \mu^I = 0, \\ \frac{\partial \mathscr{L}_i^I}{\partial e_i} &= E_\varepsilon[c_{e_i}] + \mu^I (E_\varepsilon[w_i q_{e_i} - v_{e_i}]) = 0. \end{aligned}$$

Hence, after combining the two first order conditions, income and effort can be traded at a rate equal to the difference between valued marginal production and marginal costs:

$$E_\varepsilon[c_{y_i}(w_i q_{e_i} - v_{e_i})] = -E_\varepsilon[c_{e_i}].$$

Treatment II Compared to Treatment I, optimal behavior is also subject to the net income Euler equation in Equation (10):

$$\begin{aligned} c_{y_1} &= E_\varepsilon[c_{y_2}], \\ E_\varepsilon[c_{y_i}(w_i q_{e_i} - v_{e_i})] &= -E_\varepsilon[c_{e_i}]. \end{aligned}$$

Treatment III Here, the first order conditions are, again, given by:

$$\begin{aligned} c_{y_1} &= E_\varepsilon[c_{y_2}], \\ E_\varepsilon[c_{y_i}(w_i q_{e_i} - v_{e_i})] &= -E_\varepsilon[c_{e_i}]. \end{aligned} \tag{10}$$

G Sample Characteristics

Table G.1 presents summary statistics of our sample. The median person switched to the certain amount of 25 euros instead of the lottery between 65 euros or 5 euros with expected value of 35 in the questions for risk aversion. For both measures of prudence the median person preferred the most prudent option. Subjects classified as other indicated multiple switching points. This means that for all three measures, 85.4, 82.2, and 84.4 percent chose consistently to switch only once. However, they seem not all to follow expected utility theory: while almost 90 percent of subjects behaved according to a coefficient of relative prudence of greater than 2, only about 41 percent chose consistently with a coefficient of relative risk aversion of greater than 1.

Table G.1: Summary of Subjects' Observable Characteristics

	%	SD		%
Age	23.0	(3.90)	*Field*	
Female	60.9	(48.92)	Psychology	1.56
Semester	5.0	(3.84)	Other	8.85
Extremely risk averse	42.2		Economics	10.42
Very, very risk averse	10.9		Humanities	10.42
Very risk averse	15.6		Sciences	12.5
Risk averse	9.4		Other social science	17.19
Not risk averse	4.7		Law	18.75
Risk loving	2.6		Business	20.31
Other	14.6		*Subjective Effort*	
Variance			Not demanding at all	6.25
Extremely prudent	65.1		Not demanding	28.65
Very prudent	7.3		Not demanding, not effortless	35.42
Prudent	4.7		Somewhat demanding	21.35
Not prudent	4.2		Quite demanding	6.77
Other	18.8		Very demanding	1.56
Stakes			*Attention to Risk*	
Extremely prudent	68.2		Inattentive	7.29
Very prudent	7.8		Risk pessimist	59.38
Prudent	3.6		Risk realist	24.48
Not prudent	4.7		Risk optimist	8.85
Other	15.6			
RRA greater 1	46.9			
RP greater 2	89.6			
RRA greater 1 and RP greater 2	41.1			

Source: Authors' calculations.

H Expected Payoff by Treatment

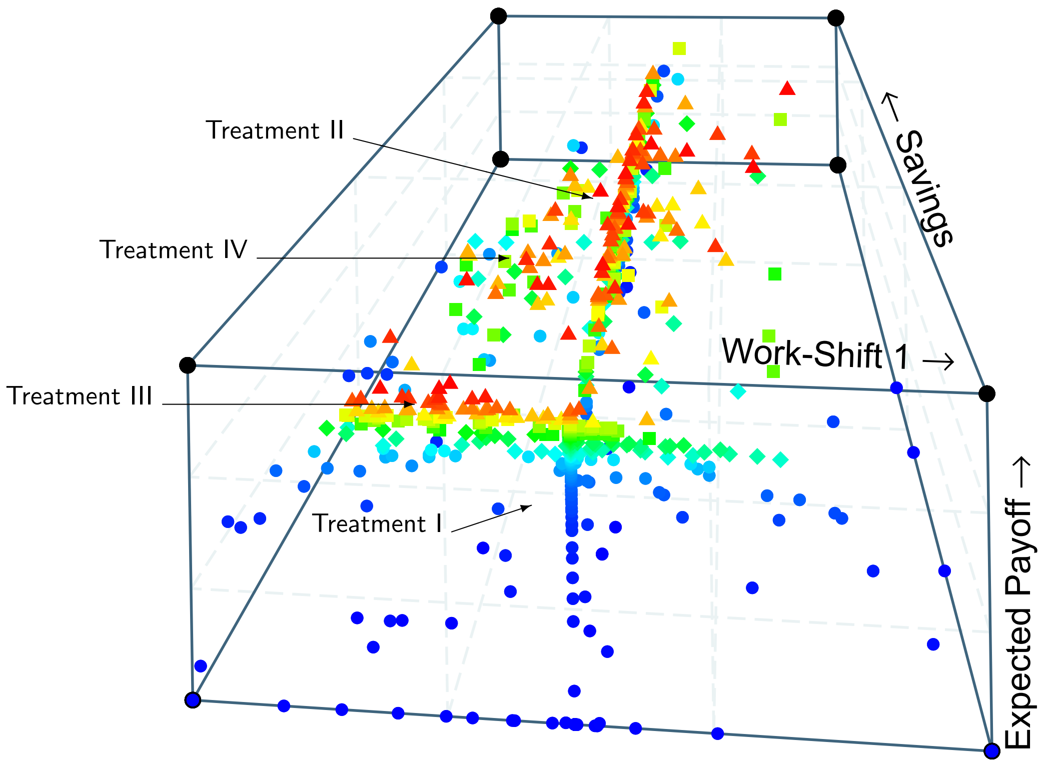


Figure H.1: Work-Shift-Savings-Payoff Space

Legend: Payoffs range from very low (blue circles) to very high (red triangles). The color changes with the distribution and each of the four markers covers a quartile of expected payoffs from 0 euros to 14.89 euros. The fraction allocated to work-shift 1 ranges from 0 to 100 percent, savings from 0 points to 6,473 points.

Source: Own presentation using GRAPH3D.

I Differences across treatment averages

The variables we consider important are savings, time spent in shift 1, (hypothetical) income cuts, and the number of balls caught per movement. The upper panel of Table I.1 shows the differences of the mentioned variables in Treatments II, III, and IV from Treatment I using subject fixed effects regressions (Treatment I's mean of the respective variable is equal to the constant in the regressions). The subject fixed effects assist in controlling for unobserved heterogeneity due to a subject's ability to catch balls or other time-invariant characteristics. The lower panel uses the same approach but compares Treatments II and III with Treatment IV. The first column considers savings. In the upper panel, we observe significant precautionary savings in Treatment II. In Treatment IV, the coefficient is smaller than in Treatment II, which suggests that some savings might have been substituted by shifting.

Table I.1: Differences Across Treatments

	Savings	Time Shift 1	Time Shift 1 ≤180	Income Cut	Income Cut≥0	Balls per Move S1	Balls per Move S2
Treatment II-I	2011.64***			2011.88***	2011.88***	0.21***	0.17
	(89.98)			(90.01)	(90.03)	(0.08)	(0.13)
Treatment III-I		-14.34***	-58.94***	934.84***	2094.02***	0.17*	0.24**
		(5.10)	(3.25)	(146.90)	(124.37)	(0.09)	(0.11)
Treatment IV-I	1511.16***	-9.47**	-54.68***	2117.63***	2593.48***	0.22***	0.31***
	(80.65)	(4.41)	(3.46)	(158.77)	(132.70)	(0.08)	(0.10)
Constant (TI)	0.00	180.00***	179.13***	0.28	142.34	2.84***	3.36***
	(49.94)	(2.77)	(1.51)	(75.71)	(119.07)	(0.05)	(0.07)
Subject Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.617	0.021	0.683	0.327	0.544	0.013	0.009
Observations	576	576	397	768	665	767	755
Treatment II-IV	500.48***			-105.75	-519.35***		
	(82.17)			(153.68)	(142.07)		
Treatment III-IV		-4.88	-7.85**	-1182.79***	-501.14***		
		(4.70)	(3.52)	(153.83)	(146.52)		
Constant (TIV)	1511.16***	170.53***	125.61***	2117.63***	2617.62***		
	(41.09)	(2.35)	(1.94)	(87.50)	(82.86)		
Subject Fixed Effects	✓	✓	✓	✓	✓		
Adjusted R^2	0.161	0.003	0.060	0.153	0.056		
Observations	384	384	205	576	473		

Estimation Equation: Differences across treatments estimated using individual-specific fixed effects.
Inference: Cluster robust (individual level) standard errors are in parentheses, significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations.

The lower panel of the table shows that this difference of about 25 percent in savings is significantly different from zero between Treatment II and Treatment IV. The results in the second column confirm this. In the upper panel, we can see that our subjects spent on average significantly less time in Treatment III (-14 seconds) and Treatment IV (-9 seconds) than in Treatment I (where time in shift 1 was fixed). The lower part of this column shows that we cannot reject that the amount of shifting is the same in Treatments III and IV, though. The point estimate for Treatment IV is smaller than for Treatment III, which again points to substitution, but is not statistically different from zero. In the third column, we restrict the sample to subjects who decided to end work-shift 1 on or before 180 seconds. Of course, the coefficients in the upper panel become larger in comparison to the second column.[25] In the lower panel, we see that the difference between Treatment III and IV is significant in the restricted sample. Our interpretation of this is that a substantial fraction of subjects understands saving and shifting as substitutes and that they choose to combine these two ways of intertemporal substitution.

How can we compare precautionary shifting (which is measured in seconds) with precautionary savings (which is measured in points)? The fourth column shows the results of a thought experiment: We know from theory that savings should be zero in the absence of wage risk and that both work-shifts should be equally long. Therefore, the observed

[25] The point estimate of -59 seconds for Treatment III suggests that subjects on average shift too much since 179-59=120 seconds is smaller than the prediction of 131 seconds that we report in the next section.

number of earned points in period 1, during the first 180 seconds, serves as the benchmark level of income y_1 under certainty.

To obtain a comparable measure of precautionary behavior, we compute the expected income at the end of shift 1 minus savings and subtract it from y_1. That means that income cuts are defined as income in period 1 − (expected income in shift 1 − savings). If shifts and periods coincide, the only difference is income cuts due to savings; if savings are zero and shift 1 is shorter than period 1, this difference measures income cuts due to shifting (we assume for this calculation that point earnings are uniformly distributed over time). Since income cuts are measured in points, we can simply compare this measure across treatments. The fourth column shows that income cuts just equal savings from the first column in this case. In the third treatment (second row), shifting is allowed, but not saving. Therefore, we calculate the difference of income in period 1 to income obtained in shift 1. This is the measure for income cuts in Treatment III. Similarly, in Treatment IV, this difference minus the amount of savings gives total income cuts.

Using the entire sample, average income cuts in Treatment III seem to be too low to achieve the optimal intertemporal substitution implied by the theoretical model calibrated with the estimated ability to catch balls, while in Treatment IV the average amount is very close to the optimal. Statistically, the former value is different from that of Treatment II but the latter is not. In the next column, we exclude all subjects from the sample that did not save, i.e., who have negative income cuts. This shows that while in Treatments II and III savings and shiftings were virtually perfectly substituted, in Treatment IV significant excess income cuts take place.

Finally, the last two columns show that productivity is on average economically not significantly different across treatments, although some statistically significant differences are detected. The constant shows that on average, the subjects caught three balls.

J Estimation of Production Functions

Figure J.1 provides an overview of the means, histograms, and kernel density distributions of the number of caught balls divided by the number of movements (balls per movement$_i$ = caught balls$_i$/ movements$_i$) in the two shifts for all four treatments, pooled for all subjects. The distributions appear to be very similar and display a fair amount of dispersion. We conduct pairwise Kolmogorov-Smirnov tests between the four treatments for the variable balls per movement in the two shifts: all pairwise comparisons cannot reject the equality of the distributions at the 5 percent percent level. We take this as evidence that we do not observe much learning in our treatments after the previous three trial periods.

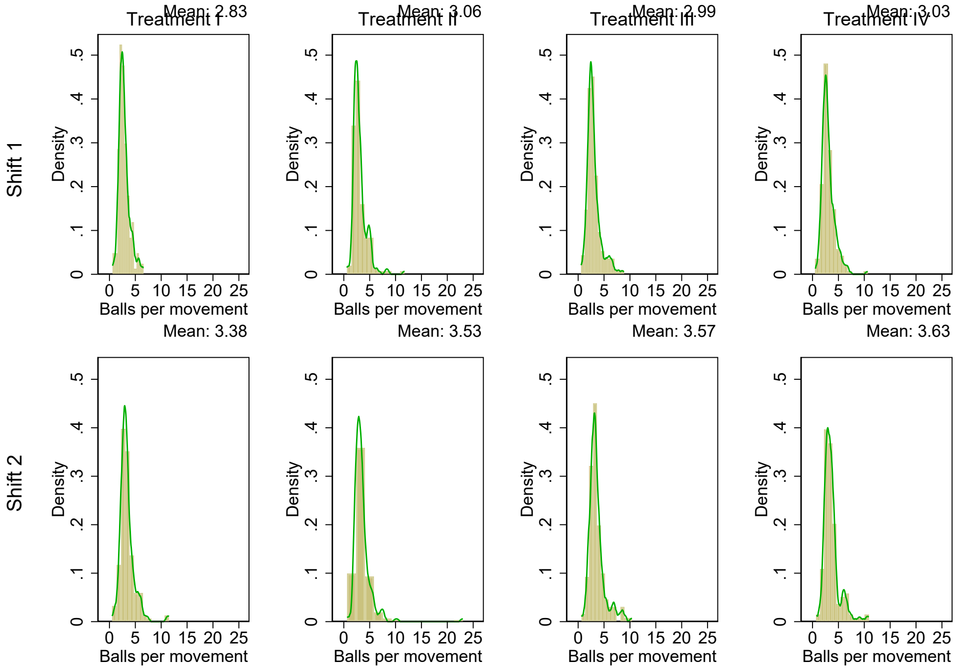


Figure J.1: Means, distributions and kernel density distributions of balls per movement in the two shifts of all four treatments

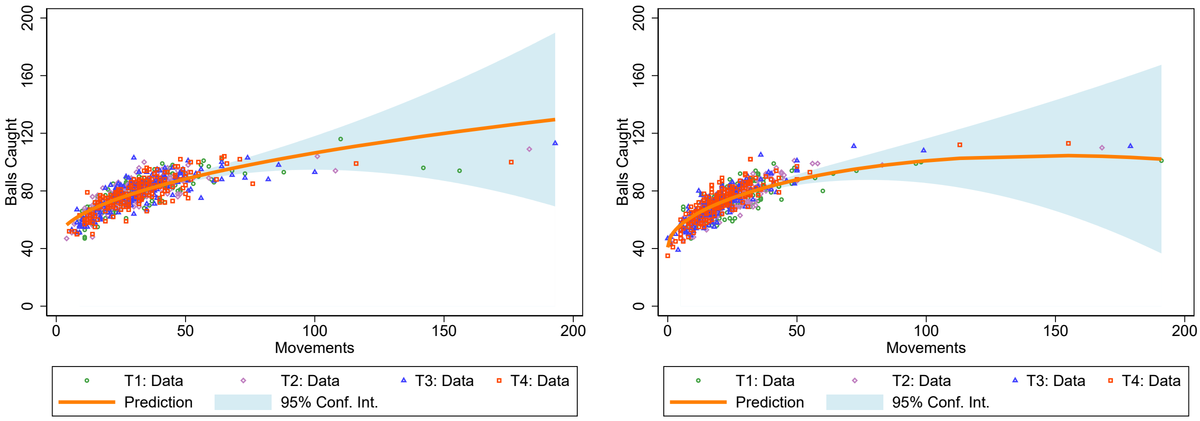
Source: Authors' presentation.

Table J.1 displays pairwise correlations between balls per movement in the different treatments and shifts. All correlation coefficients lie between 0.420 and 0.729 and are significantly different from zero at the 1 percent level. We take this as evidence for behavioral consistency as the dispersion observed in Figure J.1 is driven by the subjects' heterogeneity of ability to perform the real-effort task: subjects who perform well in the real-effort task (and catch many balls with one movement) in one treatment and shift, do so in all the other treatments and shifts as well.

Table J.1: Pairwise correlations of balls per movement in the two work-shifts in all treatments

	TI, shift 1	TI, shift 2	TII, shift 1	TII, shift 2	TIII, shift 1	TIII, shift 2	TIV, shift 1
TI, shift 1	1						
TI, shift 2	0.548^{***}	1					
TII, shift 1	0.598^{***}	0.542^{***}	1				
TII, shift 2	0.464^{***}	0.451^{***}	0.525^{***}	1			
TIII, shift 1	0.503^{***}	0.420^{***}	0.605^{***}	0.521^{***}	1		
TIII, shift 2	0.547^{***}	0.474^{***}	0.586^{***}	0.421^{***}	0.564^{***}	1	
TIV, shift 1	0.550^{***}	0.462^{***}	0.615^{***}	0.477^{***}	0.729^{***}	0.512^{***}	1
TIV, shift 2	0.553^{***}	0.570^{***}	0.597^{***}	0.429^{***}	0.533^{***}	0.626^{***}	0.620^{***}

Significantly different from zero at $p < 0.01$: ***.
Source: Own calculations.



(a) Period 1

(b) Period 2

Figure K.2: Separate estimates of the production functions and observations in all four treatments

Source: Authors' presentation.

K Fit of structural predictions

In order to calculate the point predictions for our variables of interest (savings and time spent in the two work-shifts) using a structural model, we need to estimate a production function as supplied in Equation 8 with individual-specific fixed effects.[26] We estimate this function separately for the two periods (one problem with using only one estimation is that it does not cover the values of movements and balls needed in one single period or shift; using periods comes with the advantages that the two periods are equally long and that the wage uncertainty only affects the second period).

For period 1, our OLS estimation yields the following results (overall $R^2 = 0.65$):

$$\text{balls}(\text{moves}) = 43.8091 + 6.3099 \times \sqrt{\text{moves}} - 0.0001 \times \text{moves}^2.$$

For period 2 (overall $R^2 = 0.73$):

$$\text{balls}(\text{moves}) = 40.8174 + 6.9724 \times \sqrt{\text{moves}} - 0.0010 \times \text{moves}^2.$$

Figure K.2 displays the estimated production functions and the observations in all four treatments (similar to Figure 3 in Gächter et al. 2016, p. 696). Despite the heterogeneity between the subjects' ability, the fit of these simple polynomial regressions is quite high.

Table K.2 displays aggregate predictions based on these two production functions, the realized means and their standard deviations in all treatments. First, this table shows how well the point estimates of the production functions fit the average number of balls caught per period and, second, how well our model predicts movements per period, average savings, and the average shifting choice. The first prediction exercise performs very well; the average number of balls caught is predicted quite precisely for all treatments. No t-test indicates a significant deviation of the mean from the prediction. This is not obvious, because we jointly fit the production functions for all treatments as an econometrician would when being unable to identify under which restriction choices were made.

[26] Section J in the Appendix supplies evidence that subjects do not learn over the course of the four treatments and that their performance is stable.

Table K.2: Predictions of Extended Model and Data

	TI	TII	TIII	TIV
	Prediction Mean Std. Dev.	**Prediction** Mean Std. Dev.	**Prediction** Mean Std. Dev.	**Prediction** Mean Std. Dev.
Production function predictions				
Balls Caught in Period 1	**78.77**	**77.72**	**78.80**	**78.07**
	78.47	78.32	78.98	77.97
	(10.82)	(10.53)	(11.64)	(11.43)
Balls Caught in Period 2	**74.59**	**73.95**	**70.58**	**71.22**
	73.92	73.48	70.76	72.61
	(10.42)	(11.05)	(12.28)	(12.23)
Model predictions				
Movements in Period 1	**25.46**	**25.46**	**25.46**	**25.46**
	32.71^{***}	30.73^{***}	32.99^{***}	31.50^{***}
	(18.44)	(17.37)	(19.13)	(17.82)
Movements in Period 2	**17.20**	**19.54**	**19.54**	**19.54**
	26.54^{***}	25.20^{***}	21.17	21.69^{*}
	(17.53)	(14.93)	(16.60)	(15.26)
Savings	**0.00**	**1917.16**	**0.00**	**Substitutes?**
	0.00	2011.64	0.00	1511.16
	(0.00)	(1244.67)	(0.00)	(1115.59)
Time Spent in Shift 1	**180.00**	**180.00**	**131.00**	**Substitutes?**
	180.00	180.00	165.66^{***}	170.53
	(0.00)	(0.00)	(70.54)	(61.02)
Observations	192	192	192	192

Notes: Predicted means from period-specific individual-specific fixed effects estimations (Balls Caught) and model predictions based on point estimates of these estimates. Below predictions are sample means and standard deviations. Significance levels of one-sample t-tests against predicted means are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations.

Still, the model prediction exercise is much harder, since deviations from optimal behavior of only a few subjects could lead to a rejection of the model. The model predictions are conditional on the estimates of the production functions. Of course, these estimates are measured with an error that we do not take into account in the model prediction explicitly. Instead, we report the results from using the point estimates in Table K.2.

On average, subjects systematically moved about six to eight moves more often than predicted in all treatments. Of course, this is partly due to a few subjects making up to six-times the predicted number of moves (those disregarding effort costs). This is similar for period 2 but only in Treatment I and II. In Treatments III and IV, where shifting is allowed, it cannot be rejected that the average number of movements in period 2 is equal to the prediction.

How well does the model predict precautionary savings and shiftings? The table shows that the average savings in Treatment II are not statistically different from the prediction. This shows that the model captures the savings decision extremely well. However, although shifting does occur (time spent in shift 1 is on average 166 seconds), the model predicts with 131 seconds in shift 1 even more shifting. Thus, a t-test rejects equality of average time spent in shift 1 and the model prediction. Regarding Treatment IV, there is no single prediction for savings and shiftings. Instead, we can predict optimal combinations of these two choices and compare them to actual combinations chosen by the subjects. We did this to classify substituters (as described in Section 5.2) and found that about 30 percent of all subjects follow the theoretical prediction that saving and shifting are perfect substitutes closely.