Essays on household saving, religion and pay frequency

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Proefschrift

ter verkrijging van de graad van doctor aan Tilburg University, op gezag van de rector magnificus, prof. dr. Ph. Eijlander, in het openbaar te verdedigen ten overstaan van een door het college voor promoties aangewezen commissie in de aula van de Universiteit op maandag 16 september 2013 om 10.15 uur door

Nathanaël Vellekoop

geboren op 31 juli 1981 te Den Haag.

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	prof. dr. A.H.O. van Soest

For Becky

Acknowledgements

Now is the time to get a little personal. I must confess that it has been a dream for me to do a PhD. Not because of any status (there is none), but I like the academic life of scholarship, pushing limits, and the interaction with a diverse group of incredibly smart people. The Tilburg experience had it all. It is with a heart full of gratitude that I write this section. Over the past six years my life took many surprising and unexpected turns. Reflecting on these years, my general feeling is that on many occasions I have been at the right time, at the right place, meeting the right people. And, I cannot take any credit for this at all. Depending on one's point of view one can call it either luck or providence – and since I believe in Divine grace I attribute it to the latter.

My research interests are in applied microeconomics and behavioral economics. Tilburg is probably the best place to study these topics for a PhD. The timing was perfect. I started in September of 2007. Netspar was founded in 2005 and every year new young and enthusiastic people started to work in Tilburg. Just a year before my arrival, Peter Kooreman had come from Groningen to Tilburg. I am very fortunate that he agreed to be my advisor. His plain honesty and encouragement was exactly what I needed – I cannot think of a better advisor. He is critical, creative, and slightly impatient. He holds high standards of academic scholarship to everybody, regardless of name and fame – including himself. He taught me to be analytical, to push one step further, and to think independently. I learned a lot from doing two projects with him – and I cannot believe some of the mistakes I made. I am firm believer in the master-apprentice model, or teaching by example. The following verse from the Gospels very well describes my feelings:

"A disciple is not above his teacher, ...,
it is enough for the disciple that he be as his teacher."
Matthew 10,24–25.

In the Spring of 2010 two gentlemen came to The Netherlands, Bill Evans and David Card. They are both wonderful examples of highly productive researchers, who do not think it above themselves to discuss research with PhD students, and they actually reply to emails within 24 hours (Peter does too). Many thanks to Bill for encouragement and help with the startup of my job market project. The fact that he believed in the project meant a lot to me. Many, many thanks to David for inviting me to California. The Spring of 2011 in Berkeley proved to be a wonderful, life-changing semester. Academically challenging, intellectually stimulating, and the best came at the end when I met Becky.

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I am very grateful to Eric Wassink and Andries Kuipers at Statistics Netherlands for offering me a job as a statistical researcher, and for being very helpful when there was a total eclipse in the Fall of 2012: work at Statistics Netherlands, a teaching job, a jobmarket, a thesis, and a baby.

Being a Phd-student truly is a unique job. You are a student, but you get paid (well). You can study what you like, and grow as a person. And you meet all sorts of wonderful people while doing your work, and many become friends. To highlight just a few. I thank Patrick for being an amazing office mate. We share enough flaws to make life pleasurable, and complement each other well enough to grow. We still need to do a project together though. Chris Müris is an incredible source of support, and a dear friend. I cannot think of a better best man, and I hope that someday we will be on the same continent again. Jan Stoop's time management skills are an example to me, and the sheer joy with which he does everything makes him a pleasure to be around. Meltem Daysal came in fresh from Maryland as an assistant professor, and became a friend. She truly cares about students, and I am grateful she pushed my limits (not my buttons). Every department needs somebody like her to help students develop a professional attitude.

Thanks to my parents and my in-laws for wonderful love and care. Finally, to my lovely wife: thank you for your encouragement, faith and prayers. I seriously doubt whether without you I would have finished this early, or would have finished at all. Thank you for always keeping good confidence. I am looking forward to starting new dreams, together with you.

Soli Deo Gloria

July 31, 2013

Contents

CHAPTER 1

Introduction

1.1 Four different studies

This thesis contains four different studies in applied microeconomics. In order to show the diversity I list the research questions in order of appearance:

1. How are risk and religion correlated?
2. Does a deposit scheme help gym members to visit the gym more often?
3. Do households smooth consumption expenditures around the date of rent and mortgage payments?
4. How do employees save out of different salary components?

The four studies are quite different – they differ in topic, method, and data used. The first combines experimental data with survey data, both collected through an internet panel. The second is a field experiment with a gym. The third study uses US budget survey data, in which households wrote down their daily expenditures. The last one uses administrative data from two Dutch firms. The first study is in the field of cultural economics, and the last three are in behavioral economics.

Behavioral economics is a combination of economics and psychology. It attempts to refine and enrich our understanding of the microfoundations of economics, by introducing findings from psychology. To categorize the three essays in behavioral economics I will use the classification of an overview article by DellaVigna (2009) – for an earlier overview, see Rabin (1998). DellaVigna classifies studies in behavioral economics according to the order of events in the decision making process, from preferences, beliefs to deciding. Compared to the benchmark of neoclassical economics individuals can have nonstandard preferences,

hold incorrect beliefs, or have systematic biases in their decision making. Nonstandard preferences are e.g. hyperbolic discounting (time preferences), reference-dependence (risk preferences), and other-regarding (social) preferences. Studies (2) and (3) in this thesis are applications of hyperbolic discounting, and fit as such into the category nonstandard preferences.

DellaVigna's second category is incorrect beliefs. An example of incorrect beliefs is overconfidence. Though some would classify religion as incorrect beliefs, this is not how economists usually treat religion. Economists model religious behavior as an investment good with some payoff in the afterlife (e.g. Azzi and Ehrenberg, 1975). Religion is typically not thought of as being part of the behavioral economics family. However religion can be a source of social norms, or could act as a reference point in prospect theory. In those cases religion can be classified as nonstandard preferences. The study on religion in this thesis is not about norms, but presents correlations with risk attitudes. As such it fits in the body of research on the association of cultural differences and economic outcomes (see for example Guiso et al., 2006).

The third group of deviations in DellaVigna's overview article is systematic biases. Examples are framing, persuasion and social pressure, limited attention, and menu effects amongst others. The subject of the last study is framing effects, and this study therefore belongs into the category of systematic biases. Framing is one of the more prevalent findings in psychology. It is the effect that the context or phrasing of a problem determines the outcome. In survey methodology it is well-known that respondents give different answers to the same question if the answer categories are framed differently. For example, patients are more willing to give consent for treatment if the treatment is presented as 99% safe, compared to the situation where they are being told that there are complications in 1% of the cases (Gurm and Littaker, 2000, cited in Keren, 2012). Within the class of framing effects, there are many different effects identified, coming with different names. For example, there is labeling, which is the finding that the name attached to an income component is correlated with the spending (Kooreman, 2000; Epley, et al. 2006; Card and Ransom, 2011). There is narrow bracketing, which is the finding that people evaluate risks in smaller subcategories, but not over all categories jointly (e.g. Gneezy and Potters, 1997; van der Heijden et al., 2012). Different effects and names point to the same phenomenon: systematic biases related to presentation, absorption and recall of information. One challenge for behavioral economics is to unify all sorts of findings and effects in one general framework. One attempt is done by Gabaix, (2012*a* and 2012*b*).

I would like to elaborate a little bit more on two themes: mental accounting, and commitment. Both are commonly explained within one model – hyperbolic discounting – though this is not the only possible explanation. Another reason is that despite many recent studies, there still is not one convincing explanation for either mental accounting or commitment. These two themes give a small overview of the recent literature, and a flavor of the topics of the thesis.

1.2 Mental accounting

Behavioral economics is a departure from the standard model of utility maximization. It is useful to take a step back, and be more explicit where the departure is from. Most relevant for this thesis is the lifecycle consumption theory, or the permanent income hypothesis (Friedman, 1957). The problem at hand is how households make decisions how to allocate consumption over their lifetime – and the mirror image is how people allocate savings. The theory is relevant in order to design pension schemes, unemployment systems, and fiscal policy amongst others. For policy makers the theory is relevant because of the saving/consumption responses that can be expected of certain policies. For example, during economic recessions governments can try to stimulate consumption by lowering taxes or giving tax rebates. If households would save a tax rebate, then the effectiveness of such a policy measure can be questioned.

The basic assumptions of the lifecycle consumption theory are that households are forward-looking, possibly impatient (but not time-inconsistent) and are optimizing utility from consumption. Asset markets should function to some extent, so that households are able to save and borrow money. The predictions (of some forms) of the permanent income hypothesis are: (1) consumption is smoothed over the lifetime of the household; (2) permanent increases in income raise the level of consumption by almost the same amount; and (3) the savings ratio out of temporary increases in income is close to 1. Note that there are different versions of the lifecycle model, and adding e.g. labor income uncertainty, longevity risk, or asset portfolio risk can change the specific predictions (Carroll, 2001). The theory does *not* predict hand-to-mouth behavior, which is the behavior where consumption tracks income. Two puzzling findings in the literature are that consumption *does* track income, and that the consumption share out of temporary income increases is large.

Mental accounting is one theory that potentially can explain both findings. Mental accounting is the practice that people tend to classify money into (mental) spending categories. The best way to illustrate what mental accounting is, is by the following survey question, with the answers given by respondents in brackets (taken from Tversky and Kahneman, 1981, p. 457).

> Problem 8: Imagine that you have decided to see a play where admission is \$10 per ticket. As you enter the theater you discover that you have lost a \$10 bill. Would you still pay \$10 for a ticket for the play?
> Yes [88 percent] No [12 percent] $N = 1,831$.
>
> Problem 9: Imagine that you have decided to see a play and paid the admission price of \$10 per ticket. As you enter the theater you discover that you have lost the ticket. The seat was not marked and the ticket cannot be recovered. Would you pay \$10 for another ticket?
> Yes [46 percent] No [54 percent] $N = 2,001$.

From the perspective of a wallet, both problems are the same: \$10 is lost. From an accounting perspective this is different if a person has two mental accounts: one for theater tickets and one for all other expenses. In the first case the \$10 lost is an expense for the all-other-expenses-account. The \$10 reserved for the theater is still present in the theater-budget. In the second case the theater-account is depleted. From the answers respondents give it is clear that people tend to break up a large maximization problem into smaller ones, depending on how the mental accounts are set up.

Thaler (1990) is one of the first to hypothesize that people have three mental accounts: one for current income, one for asset income, and one for future income. He argues that consumption tracks current income and is less sensitive to future income changes. This implies that the marginal propensity to consume differs by income source. Or differently: money is not fungible, which is a basic economic intuition. Four examples of recent empirical evidence for mental accounting are Kooreman (2000), Card and Ransom (2011), Beatty et al. (2011), and Abeler and Marklein (2008).

Kooreman (2000) finds that Dutch households spend child benefits differently than other sources of income. The marginal propensity to consume an extra Guilder of child benefits is about ten times as large as an extra Guilder of other income. Card and Ransom (2011) study pension savings of university professors. Contributions to pension savings

consist of three parts. A mandatory employer contribution, a mandatory employee contribution, and a voluntary employee contribution. Card and Ransom study the voluntary contributions of employees. According to economic theory, employees should take into consideration the entire amount of mandatory contributions, not who is paying what. They find that the mix of who pays the mandatory contributions matters. An extra dollar of mandatory contribution paid by the employee, reduces voluntary savings more compared to the situation in which this marginal dollar would have been paid by the employer. From the perspective of the employer it does not matter whether she would pay all the mandatory pension contributions, or pays it all to the employee and withholds it as mandatory employee contributions. To the employer the total labor cost of the employee is the same in both cases.

UK elderly households spend more on heating their houses after receiving a heating subsidy (Beatty et al., 2011). Restaurant visitors spend relatively more on drinks after receiving a surprise voucher for drinks, compared to a voucher of the same amount for the entire bill (Abeler and Marklein, 2008). In both studies it is expected that consumers increase the expenditures on heating and drinks due to the income effect – more income usually means that consumption of all goods increases. However, this does not explain why almost all of the subsidy is spent on the designated goods – if people think in two accounts, than extra money for heating can free up money in the other account. There is no perfect fungibility of money between the mental accounts.

In principle policy makers could use these findings to increase the effects of programs intended to stimulate the economy. One policy debate is on the size of the multiplier of government spending. The argument is that forward looking tax-payers realize that any tax-decrease now has to be paid back in the future with higher taxes, so they will save most of the tax rebate. However several studies show that tax stimulus programs and tax rebates increase consumption (e.g. Johnson et al. (2006); Agarwal et al. (2007); Parker et al., *forthcoming*). Sahm et al. (2012) find that the matter of payment also matters. A check in the mail stimulates consumption more than a decrease in withholding taxes from the paycheck. The conclusion is the same – consumption measured by consumption expenditures responds to increases in income, where the lifecycle theory of consumption predicts it would not.

There is one problem for both policy makers and economists. It is not entirely clear how mental accounting works. It could be that people engage in 'vertical' mental accounting: categorization at one point in time. This implies that mental accounting is a heuristic,

a rule-of-thumb to reduce information needed to optimize utility. Labeling, source-effects and power of suggestion are examples of this. For some households this could mean that the fact that some money comes with a label somehow implies that it is supposed to be spent in a certain way. The open questions are how information is processed – or not. One study shows that customers in the US tend to forget to include sales taxes when doing groceries, which they find out at the counter, where it is added (Chetty et al., 2009).

Another possibility is that mental accounting is best explained by models of intertemporal decision making: 'horizontal' mental accounting. The most prevalent model in this case is the model of hyperbolic discounting (Strotz, 1956; Laibson, 1997). In this model there are two parameters that govern intertemporal behavior. There is a long-term discount rate, which is supposed to be stable over longer periods of time. There is a short-term rate of impatience, which depends on time itself. Depending on whether the agent is aware of this short-term factor, there could be three types of behavior. The first type is time-consistent, where the short term discounting factor does not play a role. If the short-term discount factor kicks in, then agents could be aware (to some extent) of their short-term impatience, in which case they would like to search for commitment mechanisms to bind their future selves. Some agents are aware of this short term factor, in which case they engage in time-inconsistent behavior. This behavior is best described by: "tomorrow I will go to the gym/loose weight/quit smoking/start studying, tomorrow really" – and tomorrow there is another tomorrow. The implication for the lifecycle theory of consumption is that changes in income are closely tracked by changes in consumption, and that windfall income is mostly spent (Thaler, 1990).

One problem is that these two types of mental accounting can have different policy implications. In the case of horizontal mental accounting it is a matter of time preferences. In this case it is not clear whether there is a need for policy interventions. If agents prefer consumption now over later, and time-inconsistency is part of their preferences, what exactly is the welfare improvement? There is an argument for policy intervention in the case where agents are time-inconsistent, are aware of it, and where they search for ways to bind their future selves. In the case of vertical mental accounting the question is how agents process and use information. Empirically the two types can generate the same evidence. On top of that the borders between vertical and horizontal mental accounting are fuzzy. An example of a case in between the two is limited attention (Karlan et al., 2012), which is an information problem over time.

Another open issue is how agents define the categories of the mental accounts. Do agents generate mental accounts as they are presented? The issue is whether mental structures are superimposed, or mental accounts are malleable, see Cheema and Soman (2006). In another paper Soman and Cheema (2011) conduct a field experiment where mental accounting and partitioning are closely related. They randomized low-income households who wanted to save for their children's eduction into a $2 \times 2 \times 2$ treatment design. The treatments were a high/low savings target, partitioning/no partitioning of salary, and picture/no picture of the children on the savings envelope. The experimenters chose which households got a high or a low savings goal. In the partitioning treatment the wages were paid out in two envelopes, one with the amount of the savings in it, and the remainder in a second envelope. In the photo treatment a photo of the children was put on the savings envelope to remind the parents of the savings' goal. Partitioning – paying out wages into two envelopes – increases savings, as long as the savings goal is not "too high". Households with the low savings goal leave the savings envelopes untouched, but households with the high savings goal break open savings envelopes more often. Once a savings envelope is opened, households spend most of it. A study where categories are endogenously determined by a present-biased agent is by Hsiaw (2011). She builds a model of goal-setting, in which present-biased agents endogenously slice up a larger goal into smaller goals as a self-control mechanism. This fits in the interpretation of horizontal mental accounting.

1.3 Commitment

The second theme is commitment, and this is a recent object of study for economists. To commit oneself is to bind oneself. The act of commitment has a relational aspect, since one commits to somebody – e.g. a spouse, an employer/employee, voters, or even to oneself. Commitment is probably the closest modern economic thinking comes to moral philosophy. To see this, just read "fidelity" for commitment to a spouse, or "integrity" for commitment to oneself. Commitment can be done by means of contract, promises, asking others to hold one accountable or liable, and in many other ways. In essence commitment increases the net gain of being in a committed state versus breaking out of one. There are various ways to increase this gain: increasing the costs of the non-committed state, increasing the transaction costs of changing states, or the promise of benefits of being in a committed state.

For theories of rational choice commitment is puzzling, since binding oneself effectively shrinks the choice set. If A is the optimal choice in set A, B, C, it will also be the optimal choice in A, B. One needs departures of the rational choice paradigm in order to explain commitment. In the previous section commitment appeared in models of hyperbolic discounting. Economists model time-inconsistency as an intra-personal game between the self now and future selves. Agents have a demand for commitment if they have a self-control problem, and if they are aware of it – sophisticates in the language of self-control models. For example drug addicts can and do find clever ways to prevent their future selves from indulging when they crave for drugs, for example giving their housekey to a friend (Elster, 2000). Examples in the financial domain are: owning a house as a savings account (Laibson, 1997), paying with cash instead of a debit or credit card, or even freezing your credit card in a block of ice to prevent oneself from impulses to buy. Time inconsistency, temptation, and arousal are typical situations where agents express a desire for commitment. As Elster (1979) puts it: fully rational agents do not need commitment devices, but agents are not totally passive either. He proposes theories of imperfect rationality, where agents are weak and they know it – but in an indirect way they can achieve the same outcome as fully rational agents (Elster, 1979, p. 1).

A different strand of literature in economics studies the consequences of commitment. Theories of self-control explain the demand for commitment devices. The literature on consumption commitments studies the consequences when households have fixed part of their budget. Housing costs, heating, and utilities are expenditure categories that have large budget shares (Chetty and Szeidl, 2007), are pre-committed (e.g. by contract), and have a flat fee structure. It is interesting to see how many contracts have a predominant flat fee structure, with total costs varying little with usage (DellaVigna and Malmendier, 2004). The existence of consumption commitments can explain why people are risk-averse to moderate wealth shocks, and at the same time buy insurance as well as lottery tickets (Chetty and Szeidl, 2007). Consumption commitments can also explain why optimal labor contracts smooth wages, with a small risk of unemployment. Wage-earners with consumption commitments prefer a smooth wage with a small risk of lay-off to a contract with large variations in income.

Commitment devices and consumption commitments both have commitment in common, but they are conceptually different. The first is the outcome of models with self-control problems, the second is the starting point to reconcile empirical puzzles of risk aversion.

1.4 Overview of the thesis

Study 1: Risk aversion and religion
Joint with Charles Noussair, Stefan Trautmann, and Gijs van de Kuilen.
A version of this chapter is accepted for publication in the Journal of Risk and Uncertainty.

We use a dataset for a demographically representative sample of the Dutch population that contains a revealed preference risk attitude measure, as well as detailed information about participants' religious background, to study three issues. First, we find strong confirmatory evidence that more religious people, as measured by church membership or attendance, are more risk averse with regard to financial risks. Second, we obtain some evidence that Protestants are more risk averse than Catholics in such tasks. Third, our data suggest that the link between risk aversion and religion is driven by social aspects of church membership, rather than by religious beliefs themselves.

This study is not related to behavioral economics, but to cultural economics. It uses an experimental procedure to elicit risk attitudes and correlates this risk measure with religious behavior and beliefs of the Dutch population. This study fits into a more recent literature that studies how cultural variables influence economic behavior (Guiso et al., 2003 and 2006). Variables as religion, culture, and gender can be controlled for if they do not concern the main research question, see e.g. Dohmen et al. (2011) where religion is a background variable in explaining risk aversion. However, sometimes religious doctrines can be helpful in explaining different economic outcomes, e.g. Kumar et al. (2011) – who exploit the differences between Protestant and Catholic opinions on lotteries. Causality is an interesting issue here, since economic outcomes can also feedback and change beliefs, doctrines, and interpretation of doctrines. A relatively unexplored link is religion as a commitment device (e.g. Deaton, 2011).

Study 2: Goal setting and gym attendance
Joint with Henriëtte Prast and Peter Kooreman.

We conduct a field experiment with a large, non-student gym, with members who want to go more often to the gym. The desired attendance goal is elicited in a survey prior to treatment revelation. There are two treatments: one with psychological cost, and one with psychological and potential monetary cost – a deposit contract. Treatment-takers were asked to set a performance goal of weekly visits. We report three findings. We find a low take-up of the deposit contract, in line with other studies on commitment devices.

Treatment-takers in both treatments set a lower goal for evaluation compared to the goal stated in the survey. Attendance in both treatments increases during the treatment period, but only takers of the deposit contract realize their performance goal more often.

This study is related to commitment. The question is whether there is demand for a commitment device. Is there a residual willingness to pay for commitment among gym members who have already paid for a subscription? DellaVigna and Malmendier (2006) show that gym members buy annual memberships to commit themselves, but that they overestimate the use of it. This finding is confirmed in our study: almost all members in our subject pool have the most expensive subscription offered by the gym, and still express they could use some help in going more often. We find a low take-up, comparable to the take-up of other studies with commitment devices. An open question is why the demand for commitment is so low. Framed in terms of the hyperbolic discounting model: do agents not know their short-term discount factor, or do they know, and are the commitment devices offered not improving welfare? As a sidenote: in our field experiment the deposit treatment is framed as a separate contract from the existing gym membership. An interesting avenue for future research would be to see how demand changes if the deposit contract is integrated in a gym membership – framing might very well matter.

Study 3: Explaining intra-monthly consumption patterns: the timing of income or the timing of consumption commitments?

A number of recent studies have concluded that consumer spending patterns over the month are closely linked to the timing of income receipt (e.g. Stephens, 2003; Mastrobuoni and Weinberg, 2009; Evans and Moore, 2012). This correlation is mainly interpreted as evidence of hyperbolic discounting. A suggested welfare improvement is to partition paychecks, and pay the same monthly amount of income in more and smaller installments. More frequent paycheck distribution could potentially serve as a commitment device, to help individuals to smooth consumption. One implicit assumption is that households are not able, or unwilling to borrow over short periods of time. They need to be liquidity constrained. I show that more frequent paycheck distribution is no longer a commitment device for hyperbolic households with liquidity constraints in the presence of consumption commitments. Consumption commitments are expenditures on rent/mortgage, health insurance, and loan repayments. They have a large budget share, are fixed in the short run, and are usually to be paid once a month. I show that cycles still appear in a model with liquidity constrained households, who receive their paychecks more frequently.

I re-examine patterns of spending in the diary sample of the U.S. Consumer Expenditure Survey, incorporating information on the timing of the main consumption commitment for most households – their monthly rent or mortgage payment. Rent and mortgage payments are most often made at the first of the month, so responses to the pattern of committed expenditure are easily confounded with the timing of "first of the month" income streams. In the empirical analysis I control for both the timing of rent/mortgage payments and the timing of income, proxied by the weeks of the month. I find that consumption spending is strongly related to the timing of rent/mortgage payments, and only weakly related to the weeks of the month. Moreover, households with weekly, biweekly and monthly income streams but the same timing of rent/mortage payments have very similar consumption patterns. Focusing on Social Security recipients, I find that the sharp intra-monthly decline in spending first documented by Stephens (2003) is only present for the 20% of recipients who make monthly rent or mortgage payments. These findings suggest that any policy prescriptions for altering the timing of income payments should also take into account the impact of consumption commitments on consumer spending and welfare.

Study 4: Framing effects in an employee savings scheme
Joint with Peter Kooreman, Bertrand Melenberg and Henriëtte Prast.
A version of this chapter appeared as Netspar Discussion Paper, no. $01/2013-001$*, and IZA Discussion Paper no.* 7154*.*

Previous studies have found evidence that seemingly irrelevant details of an income component such as its label have an effect on how it is used. Using a data set with more than one million employee-month observations, we investigate the role of functional form assumptions and time aggregation in the analysis of these effects. In most cases we find evidence that marginal propensities to save differ across income components. Our analysis reveals a large degree of heterogeneity in savings behavior within the year.

This study is connected to the literature on framing, and in particular on mental accounting. We find that marginal propensities to save differ with salary components, but it is not clear whether this is due to informational processing or intertemporal behavior. It is an open issue whether the best explanation is vertical or horizontal mental accounting. As an aside: the savings account we study has features of a commitment device. It is easy to contribute, but for liquidating the employee needs consent from the employer.

CHAPTER 2

Risk aversion and religion

2.1 Introduction

Recent microeconomic research has revealed some strong relationships between religion and economic behavior.[1] Measures of religiosity and religious affiliation exhibit correlations with investment and managerial decisions, organizational behavior, and financial market outcomes (Hillary and Hui, 2009; Kumar et al., 2011). These studies provide a microeconomic foundation for macroeconomic cross-country research that finds evidence of an important role of religion in economic development and institutional structure (Barro and McCleary, 2003; 2006, Guiso et al., 2003; 2006). One potential mechanism that could generate a relationship between religion and economic behavior is a correlation between religious belief, or practice, and risk attitudes. Studying this link is potentially an important ingredient in our understanding how religion shapes economic outcomes.

A positive relationship between risk aversion and religiosity has been observed in a number of studies (Dohmen et al., 2011; Hilary and Hui, 2009; Liu, 2010; Miller and Hoffmann, 1995). A few studies also find a negative association with religiosity and excessive gambling (Diaz, 2000; Ellison and McFarland, 2011; Hoffmann, 2000). The results with respect to differences in risk aversion between Christian denominations are mixed. Barsky et al. (1997) and Benjamin et al. (2010) find that Protestants are more risk averse, and Kumar et al. (2011) find that they make safer financial investments than Catholics, while Renneboog and Spaenjers (2011) and Dohmen et al. (2011) observe the opposite. While some of these studies control for a variety of social and economic variables that differ between the countries in which they were conducted (the United States, the Netherlands,

[1] A version of this chapter is accepted for publication in the Journal of Risk and Uncertainty.

and Germany), international differences in doctrine and history, particularly within the Protestant segments of the population, might account for the mixed results.

The studies listed above have used two different approaches. The first is to correlate data on religiosity with measures of financial risk taking at the individual level. Barsky et al. (1997), Dohmen et al. (2011), Renneboog and Spaenjers (2011), and Liu (2010) rely on hypothetical risk preference decisions in large population samples. Benjamin et al. (2009) use a student sample and a risky experimental decision task with monetary stakes. The second approach is to correlate county- or regional-level religiosity, or sectarian demography, with the financial conduct of individuals, companies, mutual funds, or CEOs (Hilary and Hui, 2009; Kumar et al., 2011; Shu et al., 2010).

In this chapter, we report new evidence of a relationship between religion and risk aversion in a demographically representative sample of the Dutch population. Our work differs from previous studies in two principal aspects. First, our data provide the first evidence for a link between an incentivized risk aversion measure and church membership at the individual level. Second, apart from a person's church membership, we also have access to an extensive set of variables concerning religious background and practice. These include parents' church membership, own and parents' church attendance, own and parents' denomination, own frequency of prayer, and own specific religious beliefs in God and core Christian theological concepts. Using our measure of aversion to financial risk, we test whether there are differences in risk aversion between church members and non-members, as well as between Protestants and Catholics. We also study the role of parental religious activity, religious beliefs, prayer, and church attendance.

The Netherlands constitutes a good arena to study these questions. The country is characterized by religious diversity, with just over half of the population (51.6%) reporting an affiliation to an established religion. 27% are members of the Catholic Church while 16.6% are members of a Protestant denomination. The Southern and Southeastern regions of the country, particularly the provinces of North-Brabant and Limburg, have a strong Catholic majority, while Zeeland, South-Holland, and the Northeast of the country have a clear Protestant majority. Religious identity has historically been important, due to the regional division, the role of Protestantism in the original war for independence against Spain in the 16th and 17th centuries, and the fact that the Netherlands has at times served as a refuge for Protestants and Jews from neighboring countries. There is a Muslim minority comprising roughly 4 to 6% of the population.

Identifying the nature of the connection between risk attitudes and religion is important for understanding the mechanism underlying the effects of culture on economic outcomes (Guiso et al., 2006; McCleary and Barro, 2006). In particular, it might clarify the nature of the link between religion and financial market behavior. Kumar et al. (2011) conjecture that differences in financial decisions between Protestant and Catholic regions are due to greater aversion to gambling on the part of Protestants. On the other hand, Shu et al. (2010) find no evidence that Protestants hold less risky stocks. Instead, they find that increased volatility of returns for mutual funds from Catholic regions is driven by aggressive trading and under-diversification. Hong et al. (2004) show that churchgoers are more, rather than less, likely to participate in the stock market, contradicting the evidence showing that religious people are typically more risk averse. Uncovering the link between religiosity, religious affiliation, and risk aversion at the individual level can potentially shed light on the nature of the relationship between religion and financial decisions.

The data we have on self-reported religious beliefs and practices allow us to study whether links between risk aversion and religion are related to particular religious beliefs, or to the social aspects of activities associated with religious practice (Barro and McCleary, 2003; Gebauer et al., 2012). Furthermore, we also have data on our subjects' exposure to religious beliefs and activities during their childhood, such as parents' church affiliation, intensity of religious practice, and church attendance. This allows us to study the role of the intergenerational transmission of risk attitudes through religious upbringing, and whether risk aversion is correlated with the decision to join or to leave the church.

We find robust evidence that risk aversion is positively correlated with religiosity, as measured by church membership. Moreover, risk aversion is positively correlated with attendance rates at religious gatherings, and the effect is mainly driven by those who are very active religiously. We also find evidence for differences in risk attitudes between denominations, with Protestants being more risk averse than Catholics. This effect is moderated by the form of incentives provided in the experiment, and suggests a special role for gambling aversion in the link between religion and risk aversion. Data on religious beliefs indicate that these beliefs are not related to risk aversion, in contrast to prayer and church attendance. Thus, the link between religion and risk attitudes appears to derive principally from the social and institutional aspects of church membership, rather than from institution-free religious beliefs themselves.

2.2 Participants and methodology

2.2.1 Participants

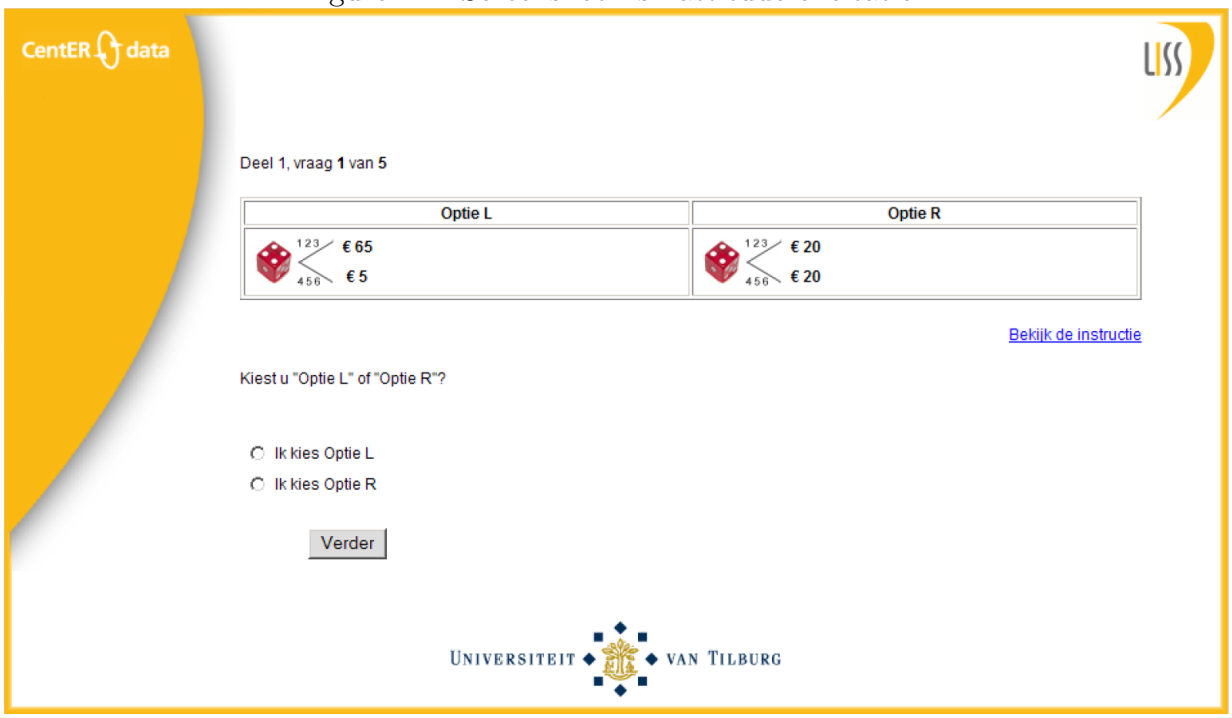
We use data from the LISS panel, managed by CentERdata, an organization affiliated with Tilburg University. The LISS panel consists of approximately 9,000 individuals, who complete a questionnaire over the internet each month. Respondents are reimbursed for the costs of completing the questionnaires four times a year. Additionally, incentivized economic experiments are conducted routinely on the LISS panel. A payment infrastructure is available to pay participants according to their decisions in experimental tasks.

In terms of observable background characteristics, the LISS panel is a representative sample of the Dutch population. A large number of background variables are available, including data from a prior survey on religious beliefs and participation, and measures of risk attitudes from a study by Noussair et al. (*forthcoming*). The experiment is offered to a random sample of 5,788 persons in December 2009. We have a measure of risk aversion for 3,451 persons. The survey on religiosity is offered to a random sample of 8,230 persons in January and February of 2009. 5,810 persons completed the survey. For 2,631 persons we have information for both risk attitudes and religiosity. We drop 327 persons because of missing observations on one or more covariates. The final sample consists of 2,304 individuals of whom 906 were in a real payoff condition in which the risk preference elicitation involved monetary incentives, 718 were in a condition with low hypothetical stakes and 680 with high hypothetical stakes. It is possible that more than one member of the same household participate in the experiment. In the empirical analysis we cluster standard errors at the household level. The final sample consists of 1,849 households.

2.2.2 Measurement of risk attitudes

Risk attitudes were measured by letting each participant choose, in five trials, between a lottery that paid €65 or €5 with equal probability and thus had an expected value of €35, and a sure payoff that differed by trial. The sure payoff varied from €20 to €40 in steps of €5. Each of the five choices was presented on a separate screen, and the order of the sequence of sure payoffs was counterbalanced among subjects. That is, for one half of participants, the first decision consisted of a choice between the lottery and a sure payment of €20, the second decision was between the lottery and €25, etc. For the other half of subjects, the first decision consisted of a choice between the lottery and a sure

Figure 2.1: Screenshot risk attitude elicitation



payment of €40, the second decision was between the lottery and €35 for sure, etc. The side of the screen (left/right) on which the lottery and the sure payoff appeared was also counterbalanced, with one half of the subjects having the lottery always displayed on the left of their screen, and the other half having it always shown on the right. Subjects did not learn of the actual outcome of any of the lotteries during the experimental session.

Each lottery was presented in terms of a die roll, with the die representing a computerized equal probability draw (see Figure 2.1 for an example of a screen shot illustrating the format). 906 subjects made these choices for potentially real stakes. For each subject in the Real stakes condition, one decision problem she faced was randomly selected to potentially count as her earnings. The prize was paid to a given individual with a probability of 1/10. This allowed for significant payoffs to some individuals (Benjamin et al., 2009).[2] The

[2]Combining large payoffs with a random selection of participants for real payment is often done in large-scale studies with the general public (e.g., von Gaudecker et al. 2011). The procedure leverages incentives, and avoids the potential problem of relatively linear utility for small payoffs when measuring risk attitude. While typically for the population as a whole no differences are observed between preferences elicited by different incentive methods (e.g. von Gaudecker et al. 2011, Noussair et al. *forthcoming*), in Section 2.4 we show that certain groups of the population may nevertheless be affected, for example due to specific religious doctrines.

Table 2.1: Overview sample construction

	Only risk[a]		Incomplete		Final sample		p-value	p-value
	$(A)^b$	fraction	$(B)^b$	fraction	$(C)^b$	fraction	$(A)-(C)$	$(B)-(C)$
Real payment	3.23	0.42	2.96	0.43	3.27	0.39	0.891	0.018
Hypo low	2.97	0.31	3.36	0.27	3.26	0.31	0.018	0.458
Hypo high	3.65	0.26	3.73	0.30	3.82	0.30	0.230	0.858
All	3.26		3.30		3.43		0.039	0.183
N		823		327		2304		

a: For these subjects we only have a risk measure, and no variables on religiosity.
b: Risk aversion is measured on scale from 0 (least risk averse) to 5 (most risk averse)

probabilities that an individual would be paid, and that any given decision would count conditional on her being paid, was known at the time she made her decisions. Another 718 subjects made the same decision, but with hypothetical payoffs. Additionally, another 680 subjects made the same choices, but with hypothetical payoffs scaled up by a factor 150. Table 2.1 shows average risk aversion for the three treatments – the real payment condition, the low hypothetical payoffs, and the inflated hypothetical payoffs. The first two columns pertain to the participants for which we have a measure of risk aversion, but for which we do not have variable on religious outcomes. Columns three and four show average risk aversion for subjects that have incomplete covariates. Columns five and six is the final sample. The last two columns show p-values of the Mann-Whitney-U test. There are some observable differences in the average risk levels between the final sample and the sample for which we only have a measure of risk aversion, columns (A) and (C). We include controls in all regressions to account for potential treatment effects, as well as controls for the counterbalancing in the presentation of the choices. The results are similar when we include the group with incomplete covariates in the final sample.

Our measure of individual risk aversion is the number of instances in which a subject chose the sure payoff. Thus, our risk aversion measure ranges from a lowest possible value of 0 to a highest possible value of 5. A risk neutral agent would make either one or two safe choices, out of the five choices, and more than two safe choices indicate risk aversion. More safe choices indicate greater risk aversion.

2.2.3 Measurement of religiosity and religious participation

The survey on religion that participants had completed earlier contains data on religious activities and beliefs of the survey participants at the date of the survey, as well as responses reporting their parents' activities when the participant was 15 years old. Table 2.2 provides summary statistics of responses to each question for each religious group.

The religiosity variables we employ are the following. We define dummy variables for frequency of church attendance. The categories are church/service attendance of more than once a week, once a week, and once a month. We also use the same categories of attendance frequency at age 15. We define denomination dummies for adherence to the Catholic and Protestant faiths. The variable "degree of belief" is measured in two ways. The first is with the response to a question in which the respondent was asked to indicate one of six degrees of belief in God. These ranged from 1: "I do not believe in God" to 6: "I believe without any doubt in God." The second measure of the strength of religious belief is a count of the number of affirmative answers on a set of seven questions asking the subjects whether they believe in specific Christian theological concepts. These are (*i*) life after death, (*ii*) existence of heaven, (*iii*) the Bible as the word of God, (*iv*) existence of hell, (*v*) the devil, (*vi*) that Adam and Eve existed, and (*vii*) that it makes sense to pray. Finally, we include dummy variables for the frequency of prayer outside of religious services.

Table 2.2 also shows the average values for two sets of independent variables that we use in our analysis. Controls A consist of the purely exogenous variables of gender, age, treatment, and counterbalancing in the presentation. Controls B consist of a set of socioeconomic background variables. These consist of marital status, number of children, income, homeownership and health status, educational and occupational status, and whether one has a Dutch passport. The table also provides averages of the responses to the religiosity questions and of the control variables, for Catholics and Protestants separately.

A number of interesting patterns are evident from the table. Overall, 42.4% of respondents are affiliated to either the Catholic or a Protestant church. This compares to 66.3% of respondents' parents at the time they were 15 years old, illustrating the decline in church membership over the last several decades in The Netherlands (Dekker et al, 1997). Almost all, more than 94%, of respondents who currently are affiliated, report that their parents were church members when they were 15 years old. On average, Protestants attend church services more often, pray more, and indicate stronger religious beliefs than Catholics. The demographics are similar between the two groups. The religiously affiliated are somewhat

Table 2.2: Summary statistics

	N	mean	Catholics	Protestants[a]	
Religion					
Church member	2304	42.4%			
Parents church member[b]	2304	66.3%	94.6%	94.1%	
Roman Catholic	2304	22.3%			
Protestant	2304	16.1%			
Attendance >1 per week	2297	3.7%	1.2%	14.2%	***
Attendance =1 per week	2297	6.9%	6.5%	27.4%	***
Attendance =1 per month	2297	7.0%	14.5%	16.9%	
Attendance >1 per week (age 15)	2297	10.8%	13.5%	23.7%	***
Attendance =1 per week (age 15)	2297	32.4%	56.6%	46.6%	***
Attendance =1 per month (age 15)	2297	6.9%	7.1%	8.1%	
Pray >1 per week	2294	25.5%	36.1%	68.7%	***
Pray =1 per week	2294	3.8%	7.0%	5.4%	
Pray =1 per month	2294	5.2%	10.5%	4.3%	***
Degree belief in God (min 1, max 6)[c]	2302	3.5	4.4	5.1	***
Belief indicators (min 0, max 7)[d]	757	2.5	3.1	5.8	***
Controls A[e]					
Female	2304	51.9%	53.9%	56.2%	
Age	2304	49.6	54.2	54.3	
Controls B					
Married	2304	63.3%	71.4%	76.3%	
Divorced	2304	8.2%	7.6%	4.3%	***
Number of children	2304	0.8	0.7	0.8	
Gross monthly income	2304	2211.0	2377.0	1903.0	
Home owner	2304	75.0%	79.6%	78.2%	
Health status (1=worst, 5=best)	2304	3.2	3.1	3.2	
High education (college or more)	2304	30.8%	27.4%	29.0%	
Civil servant	2304	10.1%	10.3%	11.0%	
Self-employed	2304	4.3%	3.5%	3.8%	
Dutch passport[f]	2304	98.1%	97.5%	100.0%	***

a: difference between Catholics and Protestants. */**/*** correspond to 10%/5%/1% significance level
b: When the respondent was aged 15.
c: Based on one question.
d: Counts the number of confirmatory answers in seven questions.
e: In regression analyses, Controls A also includes controls for counterbalancing and treatment in the risk elicitation task.
f: Multiple passports possible.

Table 2.3: Parental and own church membership

		All		Real payment		Hypo normal		Hypo high	
		N	mean[a]	N	mean[a]	N	mean[a]	N	mean[a]
Yes	Yes	917	3.54	371	3.47	283	3.39	263	3.78
Yes	No	611	3.36	250	3.04	178	3.29	183	3.86
No	Yes	61	3.56	23	3.57	19	3.00	19	4.11
No	No	715	3.35	262	3.18	238	3.12	215	3.82

Parents in church refers to parents' membership status when respondent was aged 15.
a: Risk aversion is measured on scale from 0 (least risk averse) to 5 (most risk averse).

more likely to be female and older than average. Church members are more likely to be married and less likely to be divorced than the overall population.

2.3 Results: church membership and participation

We first consider whether there is an overall correlation between risk aversion and religiosity, as measured with both current religious activity and exposure to religion during childhood. Table 2.3 gives an overview of measured risk aversion depending on current church membership status and membership of the subject's parents during her childhood. Table 2.4 shows similar data for attendance at religious services. In both tables, the risk aversion measure is the number of safe choices, out of a maximum possible of five. We give data for the whole sample, as well as separate results for participants in the real stakes and in the two hypothetical conditions.

The first pattern that is evident from the tables is that the average person is risk averse. Making more than two safe choices is incompatible with risk neutrality, and indicates risk aversion. Overall, individuals make an average of 3.43 safe choices. Table 2.3 shows that current church members are more risk averse than non-members. Table 2.4 confirms that current attendance correlates positively with risk aversion while attendance during childhood seems to have no effect. Both patterns are more pronounced for participants in the real stakes condition. Parents' membership exerts no effect beyond a correlation between current membership status and parents' membership status (Spearman's $\rho = 0.499, p < 0.01$). A respondent who renounced the church after age 15 is comparable in risk attitude to one whose parents were not church members. Thus, it does not appear that

Table 2.4: Attendance at church services

	All		Real payment		Hypo low		Hypo high	
	N	mean[a]	N	mean[a]	N	mean[a]	N	mean[a]
Current attendance								
More than once a week	86	3.83	38	3.76	19	3.58	29	4.07
Once a week	159	3.65	60	3.62	51	3.16	48	4.21
Once a month	160	3.39	71	3.42	46	3.15	43	3.60
Less often	1892	3.40	733	3.20	600	3.27	559	3.79
Attendance at age 15								
More than once a week	247	3.43	110	3.31	68	3.26	69	3.80
Once a week	744	3.44	287	3.19	246	3.39	211	3.86
Once a month	159	3.43	61	3.20	45	3.27	53	3.83
Less often	1147	3.43	443	3.32	357	3.18	347	3.81

a: Risk aversion is measured on scale from 0 (least risk averse) to 5 (most risk averse).

exposure to religion itself permanently affects risk attitudes (unless there are key variables affecting the decision to leave the church that are not controlled for). Otherwise, parents' membership would exert an influence on those who are not religious as adults (Guiso, et al., 2003; 2006). On the other hand, the pattern we observe is also consistent with relatively risk tolerant individuals being more likely to opt out of the church.

Table 2.5 gives ordered probit regression results for the whole sample (indicated in the columns labeled "All") and the subsamples of subjects who received real contingent cash payments (in the "Real" columns), or hypothetical questions (in the "Hypo low" and "Hypo high" columns). The dependent variable is the number of safe choices and each individual constitutes one observation. There are subjects in our sample who belong to the same household. Therefore we cluster the standard errors at the household level. The estimates include either a smaller set of independent variables, Controls A, or a larger set consisting of Controls A and B. Controls A consist of gender and age, which are exogenous, as well as treatment dummies and dummies for counterbalancing. Controls B are background variables, listed in Section 2.2.3, which in principle are subject to endogeneity. The table reports only the findings for the covariates of interest.

The upper panel of the table shows that church members are more risk averse than non-members. For parents' membership at the time the subject was aged 15, a directionally

Table 2.5: Risk aversion, church membership and attendance

Church membership	All respondents		Real payment		Hypothetical low		Hypothetical high	
Own membership	0.124***	0.108**	0.232***	0.202***	0.125	0.117	−0.040	−0.040
	(0.05)	(0.05)	(0.07)	(0.08)	(0.09)	(0.09)	(0.09)	(0.09)
Log likelihood	−3609	−3601	−1481	−1477	−1174	−1166	−936	−931
N	2304	2304	906	906	718	718	680	680
Church membership								
Parents' membership	0.097*	0.090*	0.087	0.076	0.156*	0.155*	0.040	0.011
	(0.05)	(0.05)	(0.08)	(0.08)	(0.09)	(0.09)	(0.09)	(0.09)
Log likelihood	−3611	−3602	−1485	−1480	−1174	−1166	−936	−931
N	2304	2304	906	906	718	718	680	680
Church attendance[a]								
More than once a week	0.278**	0.251**	0.431**	0.393*	0.177	0.154	0.191	0.210
	(0.12)	(0.12)	(0.19)	(0.20)	(0.20)	(0.20)	(0.22)	(0.22)
Once a week	0.185*	0.172*	0.295*	0.269*	−0.048	−0.056	0.319*	0.335*
	(0.10)	(0.10)	(0.16)	(0.16)	(0.17)	(0.18)	(0.19)	(0.19)
Once a month	0.021	0.010	0.220*	0.201	−0.102	−0.095	−0.120	−0.113
	(0.09)	(0.09)	(0.13)	(0.13)	(0.16)	(0.16)	(0.20)	(0.20)
More than once a week at age 15	−0.034	−0.045	−0.155	−0.176	0.072	0.085	−0.012	−0.059
	(0.09)	(0.09)	(0.14)	(0.14)	(0.15)	(0.15)	(0.17)	(0.17)
Once a week at age 15	0.037	0.034	−0.130	−0.137	0.159	0.150	0.108	0.082
	(0.06)	(0.06)	(0.09)	(0.09)	(0.10)	(0.10)	(0.11)	(0.11)
Once a month at age 15	−0.002	0.010	−0.089	−0.073	0.016	0.068	0.074	−0.006
	(0.09)	(0.09)	(0.16)	(0.16)	(0.17)	(0.18)	(0.16)	(0.17)
Controls A	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls B	No	Yes	No	Yes	No	Yes	No	Yes
Log likelihood	−3587	−3578	−1466	−1462	−1167	−1159	−933	−928
N	2290	2290	897	897	714	714	679	679

Dependent variable is risk aversion measured on a scale from 0 (least risk averse) to 5 (most risk averse). a: excluded category=less active than once a month. Ordered probit regressions, standard errors clustered at the household level in parentheses. Controls A also include treatment dummies and controls for counterbalancing. */**/*** correspond to 10%/5%/1% significance level.

identical effect is found, which becomes insignificant under real incentives. This suggests that parents' membership may exert an indirect influence by affecting current membership, which is correlated with risk aversion. The lower panel of Table 2.5 corroborates these findings. Higher frequency of attendance at religious gatherings is related to higher risk aversion, with the strongest effects for highly religiously active respondents. This effect is insignificant, however, for the attendance at age 15.[3] The regression analysis confirms that the link between church membership and revealed risk attitude is most pronounced for participants in the real stakes condition.[4]

Overall, these results clearly show a positive relationship between risk aversion and current religiosity. We thus confirm previous findings in the literature, using a unique combination of a representative population sample and experimental decision tasks with both real and hypothetical stakes. We state our first result.

Result 1: There is a positive relationship between risk aversion and active church membership.

2.4 Roman Catholics and Protestants

The previous section establishes a positive correlation between overall religiosity and risk aversion. We now consider whether there are differences in average risk attitude between Catholics and Protestants. From Table 2.2, it is clear that there are differences between the two denominations in terms of the intensity of religious activities and beliefs. On average, Protestants hold stronger religious beliefs, and the share of practitioners who are very active in terms of church attendance and frequency of prayer is greater.

One might expect, based on the results from Section 2.3, that religious activity of Protestants would be associated with stronger risk aversion on the part of Protestants relative to Catholics[5], in particular in the real stakes conditions. Table 2.6 shows that this is the case. The table shows the average risk aversion measure for Catholics, Protestants, and members of other faiths in our data. The last category includes members of

[3]More reporting errors for attendance at age 15 than for current attendance, due for example to imperfect recall of one's status at age 15, could lead to a downward bias, in the direction of less significance, in the coefficient.

[4]In Section 2.4 we note that this effect might be caused by Protestants behaving in a more risk-averse manner in the real stakes than in the hypothetical conditions, and we discuss the possible implications of this finding.

[5]Note, however, that the share of very active participants in our sample is small in both denominations.

Table 2.6: Risk aversion by denomination

	All		Real payment		Hypo low		Hypo high	
	N	mean[a]	N	mean[a]	N	mean[a]	N	mean[a]
Roman Catholic	514	3.51	211	3.39	163	3.47	140	3.75
Protestant	372	3.56	143	3.62	115	3.24	114	3.82
Other faiths	92	3.55	40	3.42	24	3.25	28	4.00

a: Risk aversion is measured on scale from 0 (least risk averse) to 5 (most risk averse).

Eastern churches, Jews, Muslims, Hindus, Buddhists, and members of other faiths, but does not include the religiously unaffiliated. Catholics and Protestants are almost equally risk averse on average for the full sample, but Protestants are more risk averse under the Real payment condition Catholics are more risk averse in the Hypothetical low payment condition.

The raw averages in Table 2.6 fail to control for other influences on risk aversion, which may fall differentially between the two groups. Table 2.7 contains tests for denomination differences, derived from ordered probit regressions that include Controls A and B discussed earlier as independent regressors. The table reports regressions that include three different samples (all participants, those who had real monetary payoffs, and those who had hypothetical payoffs), and two sets of controls, Controls A, and Controls A and B. The upper panel of Table 2.7 compares the adherents of religious groups to non-members. We find evidence that Protestants are more risk averse than non-members in the full sample and the real stakes sample. In the real stakes sample, the coefficient for Protestants significantly exceeds that for Catholics. In contrast, in the low hypothetical stakes sample, the coefficient for Protestants is smaller than that for Catholics, but this difference is not statistically significant. The lower panel of Table 2.7 restricts the sample to Protestants and Catholics only. We find that Catholics are less risk averse in the full and the real stakes samples, but more risk averse in the low hypothetical stakes sample.

These results suggest that compared to adherents of other religions, Protestants are especially risk averse in the real stakes condition. For hypothetical incentives, they hold similar risk attitudes as those who are no members of a church, and are on average even less risk averse than Catholics. Our findings have interesting implications. First, while the

literature has considered both risk aversion and gambling attitudes in their relation to religion, the result supports the view that compared to Catholics, Protestants are more averse to gambling, but not to risk per se: the real stakes condition might have been perceived as gambling because of either the real payoffs flowing from the decision, or the potential skewness introduced by the random payment mechanism.[6] Second, these findings also shed light on the mixed results obtained in the previous literature. While Kumar et al. (2011) find that Protestants are more risk averse when considering gamble-like outcome measures, Renneboog and Spaenjers (2011) and Dohmen et al. (2011) find that Catholics are more risk averse than Protestants in a self-reported hypothetical survey question. Our findings suggest that these differences are systematically related to Protestant theological doctrine regarding gambling. We state our second result.

Result 2: Protestants are more risk averse than adherents of other faiths in the real stakes gambling task.

2.5 Believing versus belonging

In Section 2.3 we found evidence supporting a positive correlation between risk aversion and religiosity, measured in terms of church membership and service attendance. An important question regarding this correlation concerns whether the relationship is driven by religious beliefs per se, or by the social effects of participation in religious institutions (see Iannaccone, 1998; Liu, 2010; McCleary and Barro, 2006). McCleary and Barro (2006) and Barro and McCleary (2003) suggest that the positive economic effects of religion are driven by religious beliefs, rather than pure communal social and cultural effects of participation and membership. They find a positive correlation between religious beliefs and economic

[6] In principle, the fact that only some individuals were selected for payment, and that in that event only one of the decisions they made was chosen for payment, meant that the choices that individuals faced were actually compound lotteries. Both the random payment of decision tasks and the random selection of individuals for payment are accepted techniques in experimental economics that do not induce systematic effects on decisions. However, if individuals make their decisions in consideration of the compound lotteries the randomization procedures induce, the choices are between positively skewed lotteries in the real stakes treatments. This positive skew is a feature that is also present in many activities that are considered as gambling, such as racetrack betting and playing the lottery, that Protestant doctrines typically discourage or forbid. Thus it is possible that differences between the behavior of members of different religious groups, or between decisions in the Real and Hypothetical treatments, could be due to an aversion to skewed lotteries, with this aversion possibly resulting from their similarity to proscribed gambling tasks.

Table 2.7: Risk aversion and denomination

All subjects	All respondents		Real payment		Hypothetical low		Hypothetical high	
Roman Catholic	0.110*	0.097*	0.161*	0.133	0.189*	0.179*	−0.059	−0.056
	(0.06)	(0.06)	(0.09)	(0.09)	(0.11)	(0.11)	(0.11)	(0.11)
Protestant	0.146**	0.124*	0.357***	0.324***	0.047	0.047	−0.038	−0.041
	(0.07)	(0.07)	(0.11)	(0.11)	(0.11)	(0.11)	(0.13)	(0.13)
Other faiths	0.118	0.107	0.184	0.158	0.075	0.044	0.052	0.051
	(0.11)	(0.12)	(0.17)	(0.18)	(0.23)	(0.24)	(0.20)	(0.20)
p-value Catholic=Protestant	0.640	0.718	0.091	0.105	0.282	0.327	0.886	0.917
Log likelihood	−3609	−3601	−1479	−1476	−1174	−1166	−936	−931
N	2304	2304	906	906	718	718	680	680
Catholics and Protestants								
Roman Catholic	−0.047	−0.039	−0.235*	−0.224*	0.139	0.168	−0.041	−0.042
	(0.08)	(0.08)	(0.12)	(0.12)	(0.13)	(0.13)	(0.14)	(0.15)
Controls A	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls B	No	Yes	No	Yes	No	Yes	No	Yes
Log likelihood	−1379	−1375	−559	−555	−447	−443	−362	−356
N	886	886	354	354	278	278	254	254

Dependent variable is risk aversion measured on a scale from 0 (least risk averse) to 5 (most risk averse). Ordered probit regressions, standard errors clustered at the household level in parentheses. Controls A also include treatment dummies and controls for counterbalancing. */**/*** correspond to 10%/5%/1% significance level.

growth, but a negative correlation between church attendance and economic growth. They interpret church attendance as a costly input and religious beliefs as a valuable output of a production process. In this section, we study the extent to which variation in risk aversion is associated with beliefs or alternatively with social aspects of religious activity.

We measure the strength of religious beliefs for an individual in two ways, as described earlier. The first is with one direct question asking the individual to report her degree of belief on a six-point scale, and is referred to as "Degree of Belief in God" in Table 2.8. The second measure is constructed from the responses to a set of questions regarding religious belief as described in Section 2.2.3, and is referred to as "Religious Belief Indicator" in Table 2.8. Belonging, the social effects of religious affiliation, is captured with church attendance (Section 2.3). While church attendance is an injunction in both Catholic and Protestant Christianity, church services are also an opportunity to experience and organize social interaction among members of the community, and expose the individual to the specific doctrines of the particular church (Kelley and de Graaf, 1997). We also use data on the frequency that individuals pray outside of church services in some specifications. Prayer has aspects of both believing and belonging, since prayer is done both privately and in groups. The frequency of prayer outside of services is presumably correlated with stronger beliefs, but also might be associated with greater interaction with other church members.

We have already shown in Section 2.3 that church attendance correlates with risk aversion for active members. We will now test whether a similar pattern exists for religious beliefs. Table 2.8 shows regression results. Measured risk aversion is the dependent variable and the strength-of-belief metrics are among the independent variables. Included in the table are regressions using the whole sample, as well as the subsamples of people who received real cash payments or hypothetical payments, with either the full set of controls (Controls A and B), or only the smaller, unambiguously exogenous set of controls (Controls A). Since there are notable differences in beliefs and frequency of prayer between Protestants and Catholics, controls for membership are added to the set of Controls B.

As Table 2.8 illustrates, we find no significant effect of the strength of religious beliefs on risk aversion. On the other hand, we find effects of praying outside of church services, with people praying more than once a week being more risk averse than the ones praying less frequently. Overall, the positive effects for church attendance and for prayer, and the absence of effects for pure belief indicators, all suggest that the link between risk aversion and religion is driven by the social aspects of belonging to, and being exposed to the doc-

trines and institutions of a religious group rather than by the religious beliefs themselves. This constitutes our third result.

Result 3: Belief in God and important Christian theological concepts is not correlated with risk aversion. Church attendance and prayer outside services are positively correlated with risk aversion. This result is robust for controlling for being a member of the Catholic, Protestant or other faiths.

One potential explanation for this pattern is that risk-averse individuals are more likely to belong to social organizations in general. However, this is not the case, and membership and participation in a religious group does not merely seem to capture the risk sharing effect of belonging to any form of organization. In Appendix 2.A we use survey questions on social integration, with the same population, to test for the relation between risk aversion and organizational membership for a large variety of organizations. We replicate the finding that members of religious organizations are more risk averse, but we do not find a general tendency of organizational membership being positively related to risk aversion. Thus, the effect of higher risk aversion for church members is likely related to the doctrines and teachings of the church, and not merely the membership in a social organization. This result is reported as our result 4.

Result 4: While the social aspects of church membership seem to play an important role in the relationship between religiosity and risk aversion, the effect is not merely due to fact that organizational membership is correlated with risk aversion more generally. Membership in non-religious organizations does not exhibit a consistent relationship with risk aversion.

2.6 Conclusion

As far as we know, our study is the first to consider the relationship between decisions in an incentivized financial risk task and religiosity with individual level data. Using a dataset containing a revealed preference risk attitude measure, as well as detailed information about participants' religious background, beliefs, and practice, we study three issues. First, we confirm the previously obtained result that religious people, as measured by

Table 2.8: Risk aversion and beliefs/prayer

Degree of belief in God	All respondents		Real payment		Hypothetical low		Hypothetical high	
Stronger belief	0.018	−0.003	0.025	−0.023	0.017	−0.003	0.005	0.022
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Log likelihood	−3610	−3599	−1483	−1474	−1175	−1166	−936	−930
N	2302	2302	905	905	718	718	679	679
Religious belief								
Stronger belief	0.026*	0.005	0.037	−0.001	0.035	0.016	−0.006	0.010
	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)
Log likelihood	−1203	−1191	−472	−463	−399	−391	−319	−314
N	757	757	285	285	246	246	226	226
Praying (outside services)								
More than once a week	0.189***	0.154**	0.183**	0.022	0.275***	0.282**	0.100	0.202
	(0.06)	(0.07)	(0.08)	(0.11)	(0.10)	(0.12)	(0.10)	(0.15)
Once a week	−0.209	−0.249*	0.010	−0.095	−0.403*	−0.409*	−0.303	−0.276
	(0.13)	(0.14)	(0.22)	(0.23)	(0.21)	(0.23)	(0.27)	(0.28)
Once a month	−0.001	−0.032	−0.182	−0.272*	0.194	0.174	0.014	0.045
	(0.10)	(0.10)	(0.15)	(0.16)	(0.19)	(0.20)	(0.17)	(0.18)
Controls A	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls B	No	Yes	No	Yes	No	Yes	No	Yes
Log likelihood	−3589	−3579	−1474	−1467	−1165	−1156	−932	−925
N	2294	2294	901	901	716	716	677	677

Dependent variable is risk aversion measured on a scale from 0 (least risk averse) to 5 (most risk averse). Ordered probit regressions, standard errors clustered at the household level in parentheses. Controls A also include treatment dummies and counterbalancing dummies. Controls B include indicators for membership of the Catholic church, the Protestant church, and other religions. */**/*** correspond to 10%/5%/1% significance level.

church membership or attendance, are more risk averse.[7] We document the new finding that risk aversion correlates strongly with current religiosity, and only weakly, if at all, with whether one had a religious upbringing. This suggests either that relatively risk tolerant individuals select out of the church, or that leaving the church makes one less risk averse. It is tempting to speculate that as religious membership has been declining in Europe over the last several decades, there may be a corresponding decline in the degree of risk aversion of the average individual. This could be the case as either a cause or as a consequence (or both) of the decline in religious affiliation. In our view, this is an interesting line of inquiry for future research to consider.

Second, we obtain some evidence that there are differences in risk aversion between major Christian denominations. Our data suggest that Protestants are more risk averse than Catholics when risk attitudes are measured with a real cash payoff gamble. This result may not be that robust however, as we observe that for hypothetical decisions, Catholics are sometimes more risk averse under some specifications. These findings do reconcile some previous results, as they suggest that Protestants might not be more risk averse than Catholics, but rather are more averse to lotteries with positive skew, a feature of many types of gambles that Protestant churches discourage (see Kumar et al., 2011).

Third, our data suggest that the link between risk aversion and religion is driven by social aspects of church membership, rather than by beliefs in religious teachings (see Gebauer et al., 2012). We find clear effects of active religious practice, i.e. church attendance and prayer outside church, correlating with greater risk aversion. These activities are likely to expose the individual to the specific doctrine and institutions of his or her church, which is not the case for those individuals who are strong believers, but practicing their faith mostly in private. We also show that religious activities and church membership behave differently in terms of their relationship to risk aversion than memberships in other social organizations that may potentially serve as risk-sharing institutions. That is, religious groups and organizations are likely to socialize their members according to certain doctrines that relate to risk aversion, or as discussed above, avoidance of gambling and skewed risks. More detailed surveys are required to establish the potential channels of transmission of these attitudes.

[7] We also have measures for higher order risk attitudes, used in Noussair et al (*forthcoming*). There are no clear patterns between religiosity and prudence or temperance.

2.A Risk aversion and organizational membership

In this appendix we test whether membership in non-religious organizations is positively correlated with risk aversion. We make use of a question asking about membership in a wide range of organizations. The results are in Table 2.9. The data replicate the finding that membership in religious organizations is positively correlated with risk aversion, especially in the real payment conditions. There is no across-the-board positive correlation between organizational membership and risk aversion.

Table 2.9: Risk aversion and organizational membership

	All respondents		Real payment		Hypo low		Hypo high	
Sport	−0.075	−0.075	−0.032	−0.030	−0.027	−0.015	−0.157*	−0.162*
	(0.05)	(0.05)	(0.08)	(0.08)	(0.09)	(0.09)	(0.09)	(0.09)
Cultural	0.184***	0.176**	0.109	0.101	0.289**	0.276**	0.205	0.171
	(0.07)	(0.07)	(0.11)	(0.11)	(0.13)	(0.13)	(0.13)	(0.13)
Union	−0.040	−0.047	−0.047	−0.069	−0.015	−0.026	−0.013	0.019
	(0.06)	(0.07)	(0.10)	(0.10)	(0.12)	(0.13)	(0.12)	(0.12)
Professional	0.001	0.025	0.018	0.069	−0.009	0.046	0.015	0.002
	(0.08)	(0.08)	(0.13)	(0.14)	(0.13)	(0.14)	(0.16)	(0.16)
Consumer	−0.098*	−0.101*	−0.222**	−0.215**	−0.177*	−0.180*	0.125	0.102
	(0.06)	(0.06)	(0.09)	(0.10)	(0.11)	(0.11)	(0.10)	(0.11)
Humanitarian	−0.142	−0.149	−0.468***	−0.463***	0.007	−0.003	0.063	0.080
	(0.10)	(0.10)	(0.17)	(0.17)	(0.16)	(0.16)	(0.21)	(0.21)
Environmental	0.061	0.054	0.230**	0.222**	−0.142	−0.137	0.046	0.025
	(0.08)	(0.08)	(0.11)	(0.11)	(0.15)	(0.16)	(0.15)	(0.15)
Religious	0.103*	0.080	0.199**	0.177**	0.129	0.124	−0.041	−0.081
	(0.06)	(0.06)	(0.09)	(0.09)	(0.12)	(0.12)	(0.11)	(0.12)
Political	−0.031	−0.028	−0.096	−0.094	−0.151	−0.124	0.017	0.030
	(0.11)	(0.11)	(0.16)	(0.16)	(0.21)	(0.20)	(0.21)	(0.21)
Scientifical	−0.028	−0.054	−0.073	−0.081	0.030	0.071	−0.082	−0.204
	(0.09)	(0.09)	(0.13)	(0.13)	(0.14)	(0.15)	(0.20)	(0.21)
Social	0.003	0.004	−0.093	−0.087	0.133	0.088	−0.058	−0.054
	(0.09)	(0.09)	(0.13)	(0.13)	(0.16)	(0.16)	(0.19)	(0.19)
Other	0.000	−0.007	−0.056	−0.052	0.066	0.038	0.012	−0.002
	(0.08)	(0.08)	(0.11)	(0.11)	(0.18)	(0.18)	(0.18)	(0.18)
Controls A	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls B	No	Yes	No	Yes	No	Yes	No	Yes
Log likelihood	−3604	−3595	−1476	−1472	−1169	−1162	−933	−927
N	2304	2304	906	906	718	718	680	680

Ordered probit regressions, standard errors clustered at the household level in parentheses. */**/*** correspond to 10%/5%/1% significance level.

CHAPTER 3

Goal setting and gym attendance

3.1 Introduction

The ability to set goals is a distinctive feature of human behavior (Locke and Latham, 1990; 2004). In order to set a goal, one needs the capacity to evaluate past behavior, the ability to look forward, combined with the quality to imagine the shape of things not present yet. Behavioral models of goal setting include elements of goal desire ('I desire to lose weight'), perceived self-efficacy ('I can do whatever is necessary to lose weight'), goal intentions, implementation desire ('I desire to enact an exercise regimen'), implementation intentions, plan completeness, plan enactment, and finally goal realization – with possible feedback into previous steps (Bagozzi and Dholakia, 1999; Dholakia et al., 2007). In economic theories of behavior typically all the steps before goal realization are treated as a black box, unobserved by the researcher. Economic theories acknowledge that an individual can have many desires, but emphasize the presence of constraints under which an individual operates. Those desires that matter in the end – of all possible desires one can have – are revealed by actual behavior and choices made.

In this chapter we provide evidence on both desired goals and goal realizations. We have measures of how often people would like to go to the gym, the performance goal they set in different treatments, and how often they actually go. We conducted a framed field experiment with a large, non-student gym in the Netherlands. Our advertisement material was aimed at recruiting gym members who wanted to go more often to the gym. We elicited desired attendance goals in a survey prior to treatment assignment. Subjects are assigned to one of two treatments: a 'goodbye' treatment, and a 'deposit' treatment. In the goodbye treatment we invited subjects to set once a goal of how many times a week

they wanted to attend the gym. We asked them to walk by the frontdesk and say goodbye every time they attend the gym. In the deposit treatment we asked subjects to set once a goal, deposit once an amount of money upfront at the gym, and walk by the frontdesk to say goodbye. Subjects could earn money back out of their personal deposit for each week they met their performance goal. Both treatments involve some form of commitment to a third party. The goodbye treatment can be viewed as a promise, and the deposit treatment adds an a monetary component to that. The deposit treatment has material consequences, and is therefore a commitment device. With these two treatments we want to investigate how subjects set a goal – and subsequently realize their goal – when the (external) incentives to meet the goal differ.

We have three main results. First, we find that the take-up rate for the deposit treatment is low, about 13% of the subjects to whom it was offered. This is in line with other studies on commitment devices (e.g. Royer et al., 2012). We also find that not everybody who is assigned to the – zero monetary cost – goodbye treatment, takes up the treatment. Second, gym attendance increases during the experiment in both treatments compared to a representative control group, and also compared to all other gym members. However, we do not find higher gym attendance after the experiment. Moreover, we do not find statistically significant differences in attendance *between* the two treatments. The third set of results is about goals. We find significant differences between the goal as stated in the survey, and the performance goal as set at take-up of treatment. Subjects in both treatments state a higher goal in the survey – prior to treatment assignment – compared to the goal they set at take-up. Both goals are 'true' goals in the sense that they are higher compared to previous gym attendance. This is evidence that proper goal elicitation needs to be incentivised in order to distinguish between the goal subjects desire and the goal they really intent. Conditional on take-up, the subjects in the deposit treatment are $29-34$%-points more likely to meet the (lower) performance goal that is sufficient to meet the treatment target, but not the (higher) goal as elicited in the survey.

A distinguishing feature of this study is the use of a non-student gym in the upper segment of the gym industry (e.g. the mean age is 43.6, the price for an annual subscription with unlimited access is around 700 euro). Our subject pool is composed of current gym members and many of them already go regularly to the gym. Many of them are also active in other sports next to the gym. However, despite being already quite active our subjects express a desire to increase gym attendance. Our subject pool is experienced with exercising, and therefore well-suited to study goal setting.

The difference in outcomes of goal realization and gym attendance is interesting for policy makers, since typically only gym attendance is observed. The number of gym visits is regarded as an important outcome variable, since it is a proxy for health improvement, and it is easy to measure. To individuals on the other hand both goal realization and gym attendance may matter. The challenge for policy makers concerned with health is how to design interventions to influence individual behavior. The question is what policy makers should aim for: should they influence goals, or target behavior. Goals are hard to measure, but interventions influencing goals are potentially cheap, and perhaps as powerful as extrinsic incentives. Goal-setting in itself can be a self-regulation device (Koch and Nafziger, 2011). There is also some evidence that ex ante visualization of the desired outcome can improve goal performance in the end (e.g. Cheema and Bagchi, 2011). Another way of thinking about visualization is aspirations. This is the way in which people visualize the future and engage in forward looking behavior. In development economics the concept of aspirations is used as an explanation for poverty traps (Dalton et al., 2013). This study presents evidence on goal setting and goal realization and gives policy makers some additional insights how to design policy interventions.

This chapter proceeds as follows. The next section relates this study to the literature on other gym studies, commitment devices, and goal setting and. The details of the field experiment are shown, followed by the results. The chapter ends with a general discussion.

3.2 Gym attendance, commitment, and goal setting

Our study fits in a larger body of work on gym attendance. DellaVigna and Malmendier (2006) study how individuals choose a gym contract, with attendance data from the administrations of three gyms. They find that from a rational point of view gym members would have been better off (ex post) if they had chosen a pay-per-visit contract, instead of an annual membership. However, they argue that this behavior makes sense from a behavioral economics viewpoint. Gym members have a time-inconsistency problem if they discount the future hyperbolically (Strotz, 1956; Laibson, 1997; O'Donoghue and Rabin, 1999; and for an overview see Frederick et al., 2002). In models of hyperbolic discounting individuals are naive, partially naive, sophisticated, or time consistent. The naive individuals are not aware of their time-inconsistency, the sophisticates are fully aware of it, and the partially naive are in between the two. Sophisticated and partially naive individuals are the ones who want a commitment device to bind their future selves to desired behavior

(Elster, 1979; see Bryan et al., 2010, for an overview on commitment devices). DellaVigna and Malmendier (2006) explain their findings by arguing that gym members commit themselves now to future gym attendance by buying an annual membership contract, instead of paying per visit. However, they also state that their findings are best explained by partially naive individuals, instead of fully sophisticated agents. Partially naive agents are aware of their time-inconsistency – they use an annual membership contract as a commitment device – but they overestimate the help of it. In other words, they still do not go as often as they want.

Based on the study of DellaVigna and Malmendier, we hypothesized that there is a demand for a commitment device by gym members who want to go more often to the gym. If DellaVigna and Malmendier's explanation of partial naiveté is correct, then there could be a demand for a commitment device that provides incentives more frequently than once a year – when the gym membership is to be renewed. Our deposit treatment provides weekly incentives to go to the gym. Three recent field experiments on commitment contracts in the gym are Goldhaber-Fiebert et al. (2010), Acland and Levy (2011), and Royer et al. (2012). None of these studies explicitly takes into account the goals of subjects. Charness and Gneezy (2009) conduct two field experiments in which they pay students to attend the gym. They find the largest treatment effect for students that never went to the gym before, and the effect also lasts after the incentives are removed.

Some examples of studies with commitment devices in other domains are: Benartzi and Thaler (2004), Ashraf et al. (2006), Beshears et al. (2011) in the context of saving; Giné et al. (2010) in quitting smoking; Volpp et al. (2008) in loosing weight; and Chow and Acland (2011) in online gaming. Take-up of the commitment contract is generally low, between $11 - 24\%$.

As stated in the introduction, there are not many studies in economics on goal-setting. Koch and Nafziger (2011) is an exception. They study the conditions under which goal setting can be a meaningful device for self-regulation. Individuals need to have some form of time inconsistency, and need to be aware of it (sophisticates), coupled with loss-aversion. The utility function consists of two subutility functions: instrumental utility – the utility of obtaining an outcome, and psychological utility – the utility gain associated with reaching a goal. The main result is that people set painful goals if the gain in instrumental utility is large, but also for small gains if the present bias is not too strong. In the case of smaller instrumental gains, people trade-off the expected loss of falling short of the goal with having no goal at all. For people with too large a present bias, goal setting does not

work as a self-regulation device. The reason is that the goal that needs to be set in order to motivate the person is too large, and therefore the expected loss of falling short is not outweighed by the benefits. We carefully interpret the fact that gym members signed up for our experiment as evidence that goal setting was not a feasible strategy for self-regulation. Jeffrey et al. (2010) show that realizing a goal can be a resource in itself. They find in two experiments that subjects choose a risky gamble over a safe one, conditional on having reached their goal. They label this a 'cushion effect' of goal attainment.

3.3 Field experiment

3.3.1 Design treatments

In the Spring of 2010 we conducted a field experiment with a large, non-student gym, with more than $4,000$ members. The order of the experiment is as follows. First we elicited attendance goals in a survey prior to treatment assignment. After completing the survey, subjects learned about their treatment assignment. Third, subjects decided whether to take up treatment or not. Conditional on take-up subjects set a performance goal of gym attendance. We have two goals for the subjects that took up treatment – the survey goal and the performance goal.

We have two treatments, which we label high-cost and low-cost. The subjects assigned to the high-cost treatment received an offer to participate in a "deposit treatment". The subjects set an attendance goal once at take-up for a period of twelve weeks. The goal is the number of visits per week, and is a whole number between one and seven. Subjects were told that this goal is registered in the administration of the gym. Apart from the membership fee, the subject paid an additional 180 euro to the gym as a deposit. This is about 45 euro more than the average price paid for a membership fee of twelve weeks. For each week the subject meets her goal, she receives 15 euro in cash out of this deposit at the end of the week. If the participant does not meet her goal, the 15 euro of that week is forfeited. Participants were told that this money was foregone, without being explicit what would happen to the money. When asked – which only happened once – gym staff members were instructed to say that the money would be used for administrative purposes. The gym explicitly did not want to have the foregone money. We considered giving it to a charity, but this would mix up the incentives. In the end the forfeited deposit money of the treatment takers (160 euro) was not at all sufficient to cover even the show-up fee.

The time period of the experiment is twelve weeks. A subject in the high-cost group could recoup the entire deposit if she meets her goal for twelve weeks; twelve times fifteen is one hundred and eighty. The gym has standard provisions in cases of illness or vacation. Members with certain types of gym contracts could freeze their contract in those instances. We followed the gym policy – if a subject would go for a vacation or called in sick, those weeks would not count for the performance goal. We added the missing weeks to the end, so that there always is a total of twelve weeks of treatment. This was all communicated in the instructions, see appendix 3.D.

These features make the high-cost treatment a commitment device. We hypothesize that this contract is only attractive to sophisticated hyperbolic discounters, and to partially naive hyperbolic discounters. Subjects with no hyperbolic discounting are not interested, and naïves are not aware of their self-control problem. It is a commitment device because a self at time 0 wants to bind future selves by committing to a goal and subsequent visiting behavior. Conditional on take-up the decision every week is to go to the gym and collect the money, or forfeit part of the deposit. Increasing the costs in the current week is supposed to outweigh the benefits of procrastinating a week. If people are partially naive, then they are aware that they have a self-control problem, but they overestimate the use of a commitment device. Gruber and Köszegi (2001) were among the first to suggest posting a bond as a self-commitment device.

We asked the subjects in the low-cost treatment to set once a weekly visit goal as well. The subjects in this group were instructed to walk by the frontdesk after each time they exercised, and say goodbye to a staff member. The hypothesis is that saying goodbye to a staff member has an incentive effect by itself. The high-cost and the low-cost treatment have the personal interaction effect in common.

We had three reasons in mind for this particular design. We chose twelve weeks, because we were interested to see if there is habit formation. Charness and Gneezy (2009) find habit formation effects after four weeks of treatment, but only for those subjects that previously never attended the gym. Given that our subjects are already regularly going, we expected that a longer time period was needed to induce new habits. The second consideration is the choice of the weekly amount in the deposit group. This needed to be large enough to provide a sufficient *weekly* incentive (Gneezy et al. 2011), but small enough that twelve times the amount would be feasible to pay upfront. The third consideration is the concern that there might be an incentive-effect of seeing a staff member for the participants in the deposit-group when they say goodbye, or when they collect the deposit money. Both

treatments are in the spirit of 'nudge' (Thaler and Sunstein, 2008). A nudge is a minor intervention that leaves the freedom of choice intact. Subjects are free to set their own goal, but there is an intervention in place that potentially could help them realize their goal. We are aware that attending the gym is not a goal in itself – most subjects express in the survey more final goals, like losing weight or improving general fitness. We choose for an attendance goal since it is easy to implement, easy to monitor, and correlated with the other goals. Since subjects knew they were participating in a research project, this is a framed field experiment in the classification of Harrison and List (2004).

3.3.2 Participants

From the gym administration we have age, gender, membership details and complete history of daily attendance for all gym members. On January 15, 2010 we advertised with posters in the gym, and on the gym's website with the following line: "Would you like to go more often to the gym? But it is not happening?". See appendix 3.B for the details of the recruitment material. The gym also sent an e-mail and a reminder to all the members for which it had an e-mail address (around a third of the members). All messages contained the same information: we are recruiting gym members, who want to exercise once a week or more, and who could use some support. We paid 25 euro as a participation fee. We did not mention anything about the content of the treatments, or that the study was an experiment – it was framed as "research" and we mentioned the involvement of university researchers. Interested members registered at the frontdesk of the gym or sent an email.

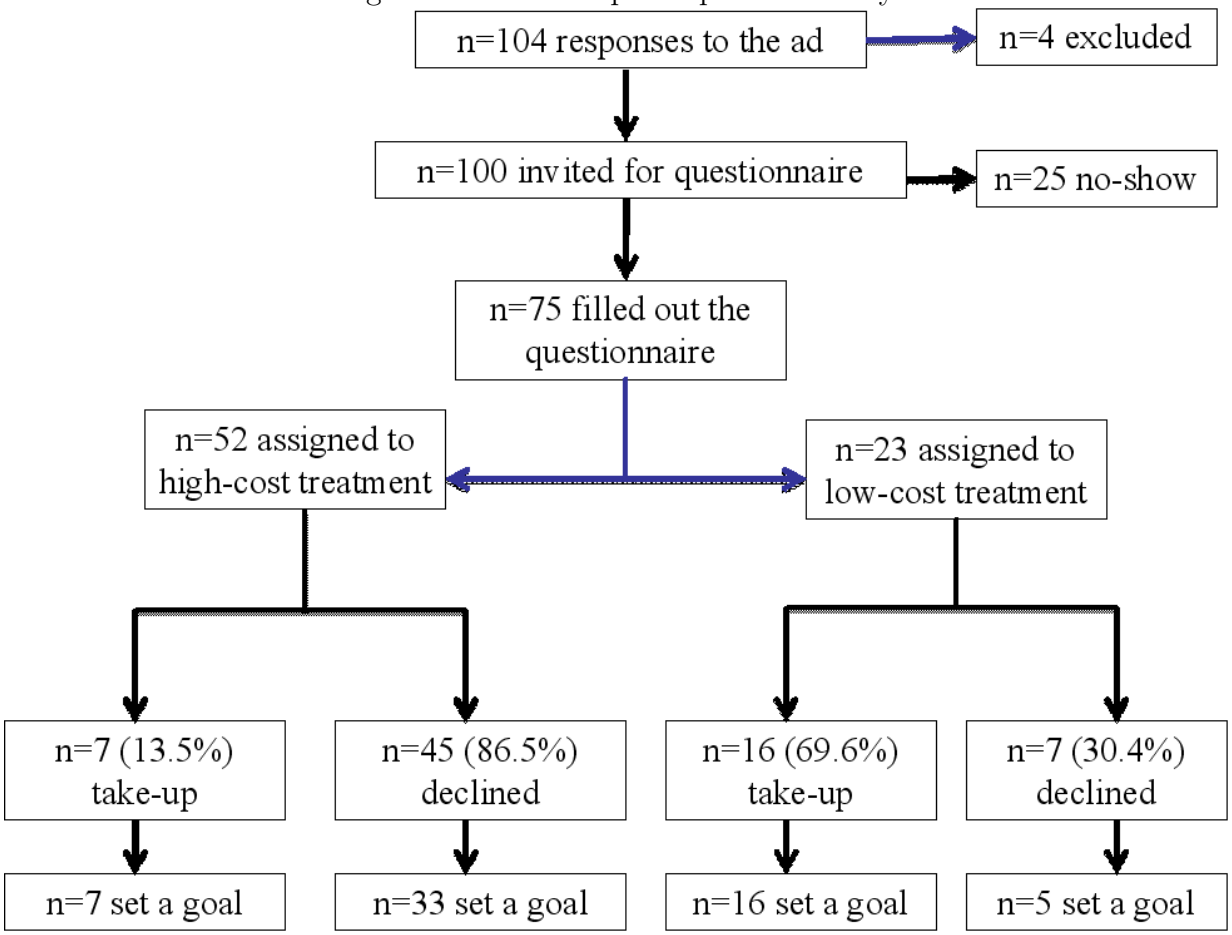
There were 104 members who registered at the frontdesk of the gym. Table 3.1 shows the characteristics of all the gym members in our gym. The last three columns show the characteristics of members that signed up for the experiment and the significance of the test of the equality of means. Compared to all other gym members, there are three characteristics of our subject pool that stand out. The members that responded to our recruitment efforts are on average older, mainly female, and are already attending the gym on a regular basis. Both the median and the mean of the average number of visits is higher in the months prior to the experiment. Though policy interventions are usually targeted at individuals who never go to the gym, apparently the group of regular gym goers expresses an interest in some help. The latter is also the group that is at risk of terminating the gym contract after the annual membership expires (DellaVigna and Malmendier, 2006). Of the 104 members who signed up for the experiment, we dropped 4 persons – one minor, two because of unavailability in the treatment period, and one without gym id. The remaining

Table 3.1: Characteristics of all gym members

	All gym members				Exp. group	All other members	
	mean	sd	min	max	mean	mean	
Subscription: only fitness	0.341	0.47	0	1	0.340	0.341	
Subscription: only indoor sports	0.148	0.35	0	1	0.184	0.147	
Subscription: fitness and indoor sports	0.330	0.47	0	1	0.350	0.329	
Other subscriptions	0.181	0.39	0	1	0.126	0.183	
Unlimited weekly access	0.841	0.37	0	1	0.874	0.840	
1 year or less visiting member	0.326	0.47	0	1	0.311	0.326	
$1-2$ years visiting member	0.256	0.44	0	1	0.320	0.255	
More than 2 years visiting member	0.418	0.49	0	1	0.369	0.419	
Female	0.541	0.50	0	1	0.738	0.537	***
Gender missing	0.006	0.07	0	1	0.000	0.006	
Age	43.6	12.3	18	86	46.4	43.5	**
Age $18-24$	0.046	0.21	0	1	0.029	0.046	
Age $25-29$	0.056	0.23	0	1	0.087	0.055	
Age $30-34$	0.071	0.26	0	1	0.068	0.071	
Age $35-39$	0.104	0.31	0	1	0.117	0.104	
Age $40-44$	0.131	0.34	0	1	0.107	0.131	
Age $45-49$	0.119	0.32	0	1	0.136	0.119	
Age $50-54$	0.098	0.30	0	1	0.214	0.095	***
Age $55-59$	0.053	0.22	0	1	0.097	0.052	**
Age $60-64$	0.035	0.18	0	1	0.078	0.034	***
Age 65 and over	0.042	0.20	0	1	0.049	0.041	
Age missing	0.246	0.43	0	1	0.019	0.251	***
Mean weekly visits Spring previous year	1.377	0.97	0	7	1.599	1.371	**
Mean weekly visits Fall previous year	1.180	0.92	0	7	1.520	1.172	***
Avg. $0-1$ weekly visits Fall prev. year	0.507	0.50	0	1	0.340	0.511	***
Avg. $1-2$ weekly visits Fall prev. year	0.278	0.45	0	1	0.311	0.277	
Avg. $2-3$ weekly visits Fall prev. year	0.115	0.32	0	1	0.184	0.114	**
Avg. >3 weekly visits Fall prev. year	0.051	0.22	0	1	0.087	0.050	*
Median weekly visits Spring previous year	1.361	1.13	0	7	1.608	1.354	**
Median weekly visits Fall previous year	1.116	1.12	0	7	1.468	1.108	***
St. dev. weekly visits Spring prev. year	0.891	0.37	0	3	0.985	0.888	**
St. dev. weekly visits Fall prev. year	0.872	0.39	0	3	1.003	0.869	***
N	4,184				103[a]	4,081	

Exp. group is all the members of the gym that signed up for the experiment. The last column shows the significance level of differences between the means. */**/*** correspond to 10%/5%/1% significance level. a: one individual could not be matched with the data.

Figure 3.1: Flow of participants in study



100 subjects were invited for a survey. We randomized these subjects in the high-cost and low-cost treatments with a ratio of 7 : 3, because we were concerned of the drop-out rate in the high-cost group. We randomized on gender and age, and randomization was done before the survey was handed out. The survey contains questions on demographics, exercising behavior, financial circumstances and two tests on self-control – see appendix 3.C for the wording of the questions. Instructors in the gym took body measures of the participants – weight, height, body fat percentage (a feature of the scale), hip size and pulse. Participants who completed the questionnaire received 15 euro as a show-up fee after handing in the questionnaire. After the experiment the remaining 10 euro was paid out at a second session of taking body measures. 75 people filled out the questionnaire. After all the participants had finished 12 weeks of treatment, we invited all 75 participants who filled out the first questionnaire to come for a second session to take body measures, and 48 out of 75 participants showed up. Figure 3.1 gives an overview of the flow of participants.

Since the gym has a large number of members we also drew a representative control group of 200 people from all non-responding members, with the same average characteristics as the group that signed up for the experiment. We do not have information on goals for the individuals in the representative control group, but we will use this group to compare the gym attendance with the subjects in the two treatments. We will also use the group of all other gym members to compare gym attendance to.

3.3.3 Construction of the attendance goals

Since goal-setting is an important aspect of this study, it is worthwhile to elaborate on how we measure goals. We have two measures of goals. There is the survey goal, which is the goal asked in the survey before treatment revelation. The wording of the question in the survey is: "*How often a month* would *you like to go to the* gym?" – emphasis also added in the survey. We take the survey goal as the desired goal (Dholakia et al., 2007). The question is asked in terms of the number of visits per month, with the equivalent number of visits per week added to some answer categories. For example, the third answer category is "$4-5$ times a month (that is 1 time a week)". The answers are recoded into a visit goal per week, since that is the time frame of the experiment. Most answers actually bunch at one, two, or three times a week. For the few respondents that chose intermediate categories, we coded their monthly goal by picking the lower answer category. The goal of a subject who chose "$6-7$ times a month" is coded as "1 time a week".

Table 3.2: Characteristics of treatment groups

	All respondents				Goal-setters: respondents with goal> 0					Control group	
	All	HC	LC		All		HC	LC		All	
	(A)	(B)	(C)	$(B-C)$	(D)	$(A-D)$	(E)	(F)	$(E-F)$	(G)	$(D-G)$
Female	0.65	0.65	0.65		0.67		0.68	0.67		0.73	
Age	47.17	47.37	46.74		46.66		47.33	45.38		45.84	
Signed up after the reminder	0.32	0.29	0.39		0.33		0.30	0.38			
Single	0.34	0.35	0.30		0.38	*	0.41	0.33			
College education or higher	0.58	0.63	0.48		0.58		0.64	0.48			
Paid work	0.74	0.78	0.65		0.77		0.82	0.67			
Disposable income less than $40,000$	0.43	0.46	0.37		0.46		0.50	0.39			
Household saved money last year	0.78	0.79	0.77		0.77		0.78	0.75			
Children present in the household	0.34	0.35	0.30		0.32		0.31	0.33			
Good or excellent health	0.87	0.85	0.91		0.85		0.83	0.90			
Smokes	0.09	0.08	0.13		0.10		0.08	0.14			
On average one or more drinks a week	0.75	0.81	0.61	*	0.77		0.85	0.62	**		
Body mass index first measurement	24.78	24.54	25.31		24.76		24.47	25.33			
Body fat percentage first measurement	29.19	28.77	30.13		29.28		28.58	30.62			
Overweight (bmi> 25)	0.47	0.44	0.52		0.46		0.43	0.52			
Planning horizon is a year or less	0.44	0.40	0.52		0.44		0.39	0.53			
Happiness (std.)[a]	0.00	-0.16	0.35	**	-0.02		-0.21	0.37	**		
Factor sloppiness (std.)[a]	0.00	0.01	-0.02		0.02		0.02	0.01			
Factor preciseness (std.)[a]	0.00	0.02	-0.05		0.03		0.03	0.04			
Factor has bad habits (std.)[a]	0.00	0.04	-0.09		-0.02		0.06	-0.18			
Factor works efficiently towards goals (std.)[a]	0.00	0.00	0.01		-0.03		-0.04	-0.03			
Subscription price for 12 weeks	136.77	133.05	145.57		135.20		127.88	149.85	**		
Exercised > 3 days a week last year[b]	0.26	0.26	0.26		0.22	*	0.23	0.19			
Has fixed days in the week to exercise	0.70	0.70	0.70		0.65	*	0.62	0.71			
Wants to exercise more[b]	0.53	0.50	0.61		0.65	***	0.64	0.67			
Does other sports next to gym	0.64	0.69	0.52		0.66		0.75	0.48	**		
Unlimited weekly access	0.85	0.83	0.91		0.82	*	0.78	0.90		0.83	
Avg. $0-1$ weekly visits Fall last year	0.39	0.40	0.35		0.43		0.45	0.38		0.39	
Avg. $1-2$ weekly visits Fall last year	0.29	0.31	0.26		0.31		0.33	0.29		0.32	
Avg. $2-3$ weekly visits Fall last year	0.16	0.17	0.13		0.10	***	0.10	0.10		0.19	*
Avg. more than 3 weekly visits Fall last year	0.11	0.06	0.22	**	0.10		0.05	0.19	*	0.08	
Mean weekly gym visits Spring last year	1.56	1.45	1.81		1.47	*	1.31	1.78	**	1.53	
Mean weekly gym visits Fall last year	1.49	1.37	1.75		1.35	**	1.23	1.59		1.41	
N	75	52	23		61		40	21		200	

*/**/*** correspond to 10%/5%/1%. HC is the high-cost group and LC is the low-cost group. The representative control group is a random draw from all other members of the gym with the same characteristics as the experimental group.

a: standardized variables based on factor analysis.

b: self-reported, question is framed as exercise in general, not only going to the gym.

The second goal measure is the performance goal, conditional upon take-up of treatment. We view this goal as the intended goal. Since 23 subjects took up one of the two treatments, we have the performance goal for 23 out of 75 subjects. There are 5 subjects that took up one of the treatments, but did not state a goal in the survey. For those subjects the survey goal is imputed with the performance goal. In the end we have at least one goal for 61 subjects. This group is the goal-setters group. The distribution of goal-setters over the two treatments is presented in the bottom row in the flowchart, Figure 3.1.

Table 3.2 shows the observable characteristics of all respondents, the subset of goal-setters, and a representative control group. Column A shows the characteristics of the subjects that completed the survey. Columns B and C in the same table show the characteristics of the subjects assigned to the high-cost treatment and to the low-cost treatment. Overall the randomization between the high-cost and the low-cost group was quite successful. Even though we only randomized on age and gender, the differences on other characteristics are small, see column $(B - C)$. Columns D, E and F in Table 3.2 shows some characteristics of the subset of respondents for which we have at least one goal, the goal-setters. There are almost no differences between the two groups.

3.3.4 Take-up of treatment

After completing the survey, 7 out of 52 subjects assigned to the high-cost group took up treatment. A take-up rate of 13.5% for the commitment contract is low, but not uncommon in studies of commitment contracts. Ashraf et al. (2006) have a take-up rate of 24%, Giné et al. (2010) find 11%, and Royer et al. (2012) have 13%. These subjects once set a weekly performance goal and deposited 180 euro upfront with the gym. In the low-cost group, 16 out of 23 participants set a weekly goal. Table 3.9 in the appendix shows the characteristics of takers and decliners of the two treatments.

Following Royer et al. (2012), we examine in more detail the factors that are correlated with take-up. Table 3.3 shows for the entire group of survey respondents – which is larger than the group of goal-setters – the correlation between observable characteristics and take-up using a linear probability model. A higher average attendance in the six months before the experiment is associated with lower probability of take-up, although never significant. Being assigned to the high-cost treatment decreases the probability of take-up with 56.9%-points in the baseline and 57.9%-points in the specification with all characteristics included. The last columns shows that women are 19.4%-points less likely to take up one of the two treatments. Being overweight, the price of the gym membership, or the goal stated in

the survey are all not significant predictors of take-up. The factor variables created from the attitude questions in the survey have the expected sign, but only the factor "chaotic" is significant. An increase of one standard deviation in the factor 'chaotic' increases the probability of take-up with 13.3%-points.

3.4 Results

We present two sets of results. The first set of results shows the effect of assignment to the two treatments and take-up of the treatments on gym attendance. The second set shows how subjects in the high-cost treatment realize the survey goal and the take-up goal.

3.4.1 Gym attendance

The columns in Table 3.4 show the effect of offering the treatments on the number of weekly visits – the intent-to-treatment. Weekly visits are collected from the gym administration. The first two columns of the table compare attendance of all subjects assigned to the high-cost group with all subjects assigned to the low-cost group. Both columns contain dummies for the the treatment period, the period after treatment, being assigned to the high-cost group, and interactions terms. The reference time period consists of the 18 weeks before the experiment. The second column adds some background characteristics collected from the gym administration: gender, age, type of subscription, length of membership and frequency of weekly visits in the six months before the experiment.

None of the coefficients in the first two columns is statistically significant at conventional levels. This means that there is no difference in attendance of being assigned to the high-cost group compared to being assigned to the low-cost group. The third and the fourth column compare the effect of being assigned to one of the two treatments with the representative control group. The control group has by construction the same observable characteristics as the experimental group *before* the experiment. The last two columns compare being assigned to one of the two treatments to all other members of the gym. The main coefficients of interest are the interaction terms 'High-cost group × Treatment period', and 'Low-cost group × Treatment period'. The coefficients differ a little bit with the group that we compare it to, as well as the inclusion of background characteristics. Being assigned to the high-cost group increases attendance during the treatment period with 0.159 to 0.212 visits per week on average, depending on the specification. Being assigned to the low-cost group increases attendance compared to the representative control group,

Table 3.3: Linear probability model on take-up of treatment

	(A)	(B)	(C)	(D)	(E)	(F)	(G)
Mean number of visits	−0.022	−0.101	−0.023	−0.011	−0.012	0.001	−0.059
Fall last year	(0.09)	(0.07)	(0.09)	(0.09)	(0.09)	(0.09)	(0.07)
St. dev. of mean number	0.107	0.218	0.108	−0.001	0.162	0.012	0.097
visits Fall last year	(0.29)	(0.22)	(0.30)	(0.31)	(0.30)	(0.30)	(0.23)
Female	−0.028	−0.037	−0.027	−0.043	−0.038	−0.120	−0.194*
	(0.13)	(0.11)	(0.13)	(0.13)	(0.13)	(0.13)	(0.11)
Age (bottom quartile)	−0.097	−0.154	−0.096	−0.104	−0.104	−0.132	−0.143
	(0.13)	(0.12)	(0.13)	(0.13)	(0.14)	(0.15)	(0.14)
Children	0.112	0.091	0.112	0.098	0.123	0.076	0.062
	(0.13)	(0.10)	(0.14)	(0.13)	(0.14)	(0.14)	(0.10)
College education or higher	−0.134	−0.025	−0.135	−0.114	−0.156	−0.111	−0.019
	(0.12)	(0.10)	(0.12)	(0.13)	(0.13)	(0.13)	(0.10)
High-cost treatment		−0.569***					−0.579***
		(0.11)					(0.12)
Overweight (BMI> 25)			0.005				−0.060
			(0.12)				(0.10)
Subscription price for 12 weeks[a]				0.181			0.177
				(0.17)			(0.16)
Goal 1x a week[b]					−0.002		0.161
					(0.32)		(0.32)
Goal 2x a week[b]					0.058		0.160
					(0.18)		(0.18)
Missing goal questionnaire[b]					−0.111		−0.070
					(0.14)		(0.10)
Factor chaotic						0.135*	0.133**
						(0.08)	(0.06)
Factor preciseness						0.026	0.003
						(0.08)	(0.08)
Factor hard time breaking						−0.124**	−0.089
bad habits						(0.06)	(0.06)
Factor able to work toward						−0.023	−0.033
long-term goals						(0.07)	(0.06)
Constant	0.320	0.678***	0.316	0.163	0.286	0.454	0.632*
	(0.26)	(0.24)	(0.29)	(0.30)	(0.28)	(0.29)	(0.33)
Adjusted R^2	0.040	0.336	0.040	0.054	0.053	0.125	0.451
N	70	70	70	70	70	69	69

Linear probability model on take-up of treatment. Robust standard errors are presented in parentheses. Numbers of respondents differ because of item non-response. */**/*** correspond to 10%/5%/1% significance level.

a: subscription price for the equivalent of twelve weeks, divided by 100.

b: visit goals as answered in the questionnaire. Not everybody who took up one of the treatments answered this question. The baseline category is wanting to go to the gym 3 or more times a week.

but only one coefficient is statistically significant. On the other hand it is not possible to reject the hypothesis that the three low-cost dummies together are equal to zero (except in the last column). This can be seen from the p-values of the F-test in the table, third line from below. To get a feeling for the magnitude of the effect, the mean number of weekly visits is 1.4 in the middle columns, and 1.18 in the last two columns. There is no discernable impact on attendance behavior after the treatment period.

We are also interested in the effect of the treatment on the treated. Since the take-up of a treatment is most likely endogenous, we need an instrumental variable. We instrument take up of a treatment with the assignment to the treatment, since assignment is randomized – this is also called the local average treatment effect (Angrist and Pischke, 2009). In Table 3.5 we show the results in a similar setup as Table 3.4. In the first two columns we measure the difference in weekly visits between subjects that took up the high-cost treatment and subjects that took up the low-cost treatment. The first column has dummies for take-up of the high-cost treatment, treatment period and post-treatment period, and interaction dummies. In the second column some covariates are added. There are no significant coefficients in the first two columns. In the two middle columns we compare the take-up of either treatment with the representative control group. In the last two columns we compare take-up of either treatment with all other members of the gym. Compared to these two groups, there is a large and significant effect of the high-cost treatment on those who took up treatment. Estimates vary from almost one extra weekly visit (0.96) to 1.5. Compared to the mean of the dependent variable, this is almost doubling weekly gym attendance. The coefficients on the take-up of the low-cost treatment during the experiment are not statistically significant. For the low-cost group the coefficients are a little bit larger compared to the intent-to-treatment estimates. On the other hand, the hypothesis that all low-cost dummies jointly are zero can be rejected at 5% and 10%, depending on the specification (but again not in the last column). To sum up, we find a strong and positive effect of the high cost treatment on gym attendance of treatment takers, compared to a representative control group as well as to all other gym members.

Other outcome variables are body mass index, body fat percentage and self-rated happiness. These measures are collected before treatment, and after the treatment period. Not all subjects showed up for the measuring session after the treatment, so we can calculate a difference only for 41 subjects. The results are presented in Table 3.11. Although not statistically significant, the low-cost group performs better on body measures than the high-cost group during the treatment.

Table 3.4: Intent-to-treatment and gym attendance

	Subjects experiment		With control group		With all gym members	
Treatment period	0.111	0.106	−0.112**	−0.100*	−0.054***	−0.035***
	(0.13)	(0.12)	(0.06)	(0.05)	(0.01)	(0.01)
After treatment period	−0.058	−0.074	−0.123*	−0.177**	−0.121***	−0.144***
	(0.25)	(0.25)	(0.07)	(0.07)	(0.01)	(0.01)
High-cost group (HCG)	−0.369	−0.076	−0.035	0.039	0.192	0.052
	(0.30)	(0.09)	(0.14)	(0.05)	(0.12)	(0.04)
HCG × Treatment period	−0.029	−0.004	0.194*	0.212**	0.136	0.159**
	(0.15)	(0.14)	(0.10)	(0.09)	(0.08)	(0.08)
HCG × After treatment period	−0.079	−0.061	−0.014	0.047	−0.016	0.023
	(0.26)	(0.26)	(0.11)	(0.11)	(0.09)	(0.09)
Low-cost group (LCG)			0.334	0.101	0.560**	0.076
			(0.28)	(0.09)	(0.27)	(0.08)
LCG × Treatment period			0.223	0.223*	0.165	0.164
			(0.14)	(0.13)	(0.13)	(0.13)
LCG × After treatment period			0.065	0.112	0.063	0.082
			(0.26)	(0.26)	(0.25)	(0.25)
Covariates	No	Yes	No	Yes	No	Yes
R^2	0.020	0.370	0.009	0.333	0.003	0.374
Mean dependent variable	1.528	1.528	1.432	1.432	1.179	1.179
p-value high-cost dummies= 0	0.479	0.840	0.062	0.053	0.006	0.092
p-value low-cost dummies= 0			0.004	0.024	0.000	0.139
Individuals	73		264		3,966	
Individuals × Weeks	3,302		11,337		165,816	

OLS regressions on the number of weekly visits. Covariates include gender, age, type of subscription, length of membership and frequency of weekly visits in the six months before the experiment. The reference time period consists of the 18 weeks before the treatment period. Robust standard errors are clustered at the subject level and are presented in parentheses. */**/*** correspond to 10%/5%/1% significance level.

Table 3.5: Average treatment effect on the treatment takers and gym attendance

	Subjects experiment		With control group		With all gym members	
Treatment period	0.111	0.101	-0.112^{**}	-0.097^{*}	-0.054^{***}	-0.034^{***}
	(0.13)	(0.12)	(0.06)	(0.05)	(0.01)	(0.01)
After treatment period	−0.058	−0.074	-0.123^{*}	-0.175^{**}	-0.121^{***}	-0.144^{***}
	(0.25)	(0.24)	(0.07)	(0.07)	(0.01)	(0.01)
Take-up high-cost treatment (THCT)	−2.644	−0.604	−0.249	0.295	1.374	0.371
	(2.27)	(0.79)	(1.01)	(0.35)	(1.04)	(0.31)
THCT × Treatment period	−0.196	−0.030	1.386^{*}	1.510^{**}	0.964^{*}	1.136^{**}
	(1.18)	(1.03)	(0.72)	(0.68)	(0.53)	(0.51)
THCT × After treatment period	−0.542	−0.454	−0.096	0.332	−0.126	0.164
	(1.90)	(1.87)	(0.81)	(0.79)	(0.64)	(0.61)
Take-up low-cost treatment (TLCT)			0.501	0.152	0.841^{*}	0.113
			(0.43)	(0.13)	(0.44)	(0.12)
TLCT × Treatment period			0.308	0.318	0.213	0.235
			(0.22)	(0.20)	(0.21)	(0.19)
TLCT × After treatment period			0.084	0.158	0.073	0.118
			(0.38)	(0.37)	(0.36)	(0.36)
Covariates	No	Yes	No	Yes	No	Yes
Mean dependent variable	1.528	1.528	1.432	1.432	1.179	1.179
p-value high-cost dummies= 0	0.623	0.875	0.094	0.078	0.033	0.075
p-value low-cost dummies= 0			0.030	0.071	0.010	0.228
Individuals	73		264		3,966	
Individuals × Weeks	3,302		11,337		165,816	

Instrumental variables regressions on the number of weekly visits. Take-up of the high-cost treatment is instrumented with assignment to the high cost treatment, and interaction terms with treatment and after treatment period. Take-up of the low-cost treatment is instrumented with assignment to the low-cost treatment, and interacted with treatment period and after treatment period. Covariates include gender, age, type of subscription, length of membership and frequency of weekly visits in the six months before the experiment. The reference time period consists of 18 weeks before the treatment period. Robust standard errors are clustered at the subject level and are presented in parentheses. */**/*** correspond to 10%/5%/1% significance level.

3.4.2 Goal setting

Next to gym attendance we are also interested in the attendance goals our subjects had in mind. We have two goal measures, the survey goal and the performance goal. Since the survey goal is not incentivised, we can think of this as a goal desire. The performance goal is the intended goal. It is also possible that gym attendance in itself is not a goal, but acts as a sub-goal for health goals, e.g. weight loss or improvement of physical condition.

In Table 3.6 we correlate the goal as stated in the survey with some background characteristics. Not all survey respondents gave an answer to the survey question, but 56 did (out of 73). The average desired goal is 2.8 visits a week, and 26 out of 56 stated they want to go 3 times a week to the gym. Looking at the table, some systematic patterns emerge. Higher attendance in the six months prior to the experiment is associated with a higher desired goal, and so is the variance of visits. Higher variability of past gym attendance translates into a higher desired goal. The presence of children in the household depresses the goal, which suggests that individuals take into account time constraints when they state how many times they would like to go to the gym. A more expensive subscription is positively associated with the attendance goal, but not in the longest specification, see the last column. Wanting to do more exercise in general – not only going to the gym – is positively correlated with the desired goal. The factor chaotic is negatively associated. The overall impression is that subjects give plausible answers to the question how often they would like to visit the gym. This suggests that something can be learned from stated preferences.

Table 3.7 presents the survey goals for each subgroup. Of the 61 goal-setters 2 could not be matched with attendance data. The first observation is that in general the desired attendance goal is high: 2.8 times a week (first row). This is two times the average number of visits in the six months prior to the experiment. Second, the difference in goals stated in the survey between the high-cost (2.68) and the low-cost group (3.05) is not statistically significant. The p-value of the paired t-test is 0.114, and the p-value of the Mann-Whitney U-test is 0.181. This is additional confirmation that the randomization over the two treatments was properly executed. Thirdly, conditional on take-up subjects set a lower goal compared to the goal in the survey. In the high-cost treatment this is 0.7 less weekly visits (p-values: paired t-test 0.047; sign-test: 0.125; signed rank test: 0.048). In the low-cost treatment this is 0.5 weekly visits less (p-values: paired t-test 0.002; sign-test: 0.008; signed rank test: 0.005). It is interesting that in both treatments there is a difference between the survey goal and the performance goal, conditional on take-up. For example, if the subjects

that took up a treatment are better motivated, then one would expect the same goal in the survey and at take-up. An explanation for this difference in goals is that the goal in the survey is the desired goal, and the goal set on take-up is the intended goal. Self-efficacy is one of the factors that can play a mediating role in the difference (Dholakia et al., 2007). A comparison of the goals of treatment takers in the high-cost and low-cost group reveals that the difference is not statistically significant. The subjects that took up the high-cost treatment set an average goal of 1.86 visits per week, versus 2.50 in the low-cost treatment (p-values: paired t-test 0.149; Mann-Whitney U-test 0.157). It should be noted that failure to detect statistically significant differences can also be due to low sample sizes. Both the survey and the attendance goal are true goals, in the sense that they are higher compared to previous attendance.

Table 3.7: Goal setting and goal realization

	Goal[a]		Mean visits before	Mean fraction goal realized			
	source	mean	treatment	before treatment	treatment	after treatment	N
All	survey	2.81	1.35	0.23	0.28	0.19	59
	performance	2.30	1.49	0.43	0.59	0.42	23
High-cost group							
All	survey	2.68	1.23	0.22	0.27	0.18	38
Decline	survey	2.71	1.25	0.21	0.23	0.18	31
Take-up	survey[b]	2.57	1.13	0.26	0.44	0.19	7
	performance	1.86	1.13	0.44	0.81	0.40	
Low-cost group							
All	survey	3.05	1.59	0.24	0.28	0.20	21
Decline	survey	3.20	1.37	0.22	0.23	0.08	5
Take-up	survey[b]	3.00	1.67	0.25	0.30	0.23	16
	performance	2.50	1.67	0.43	0.49	0.43	

Two subjects who declined the high-cost treatment could not be matched to the data with gym visits.
a: goal is defined as the number of visits per week.
b: one taker of the high-cost treatment and four in the low-cost treatment did not answer the question in the survey. For these five subjects we set the survey goal equal to the performance goal, as explained in the text.

Table 3.6: Association of survey goal and individual characteristics

	(A)	(B)	(C)	(D)	(E)	(F)
Avg. number of visits Fall last year	0.483**	0.501**	0.503**	0.585**	0.547**	0.818***
	(0.25)	(0.24)	(0.24)	(0.27)	(0.26)	(0.27)
St. dev. of mean visits Fall last year	2.199***	2.169***	1.738**	2.346***	2.635***	2.600***
	(0.74)	(0.74)	(0.79)	(0.84)	(0.78)	(0.95)
Female	0.212	0.163	0.120	0.051	0.339	−0.180
	(0.31)	(0.32)	(0.32)	(0.33)	(0.33)	(0.40)
Age (bottom quartile)	0.206	0.178	0.231	0.028	0.207	−0.061
	(0.35)	(0.35)	(0.35)	(0.37)	(0.46)	(0.52)
Children present in the household	−0.698*	−0.714*	−0.708*	−0.811**	−0.826**	−1.100***
	(0.37)	(0.37)	(0.36)	(0.38)	(0.35)	(0.40)
College education or higher	−0.217	−0.194	−0.114	−0.103	−0.296	−0.050
	(0.29)	(0.30)	(0.29)	(0.33)	(0.32)	(0.39)
Missing goal questionnaire	−0.785	−0.770	−0.785	−0.259	−0.746	−0.236
	(0.52)	(0.53)	(0.57)	(0.53)	(0.51)	(0.58)
Overweight (BMI> 25)		−0.157				−0.567
		(0.32)				(0.42)
Subscription price for 12 weeks[a]			0.837*			0.920
			(0.47)			(0.65)
Have fixed days in the week for exercising				0.412		0.951**
				(0.32)		(0.38)
Wants to exercise more[b]				0.830**		0.740*
				(0.38)		(0.44)
Does other sports next to gym				−0.383		−0.023
				(0.34)		(0.42)
Factor chaotic					−0.399*	−0.489**
					(0.21)	(0.20)
Factor preciseness					0.094	0.223
					(0.23)	(0.25)
Factor hard time breaking bad habits					−0.147	−0.142
					(0.17)	(0.19)
Factor able to work toward long-term goals					−0.227	−0.407
					(0.19)	(0.27)
Log likelihood	−55	−55	−54	−51	−51	−45
N	56	56	56	56	55	55

Ordered probit regressions on desired number of weekly gym visits as stated in the survey. Robust standard errors are presented in parentheses. Numbers of respondents differ because of item non-response. */**/*** correspond to 10%/5%/1% significance level.
a: subscription price for the equivalent of twelve weeks, divided by 100.
b: the question is framed as exercise in general, not only going to the gym.

The last three columns of Table 3.7 show the frequencies of goal realization. In the 18 weeks before the experiment subjects realized the desired goal – as stated in the survey – in 23% of the cases (top row). This is under the assumption that in the 18 weeks before the experiment subjects had the same goal as they stated in the survey. There is no difference between goal realization of subjects in the high-cost group (22%), compared to the low-cost group (24%). This means that on average one out of five subjects met their goal every week, or one subject met her goal on average once in five weeks.

During the experiment subjects in the high-cost group realize their goals more often, conditional on take-up. Taken by the survey goal, they goal realization rate is 44%, taken by the performance goal it is 81%. For the subjects in the low-cost group there is a slight increase in both goals, 30% for the (higher) survey goal, and 49% for the (lower) performance goal. After the experiment goal realization rates return to their pre-experimental level. We do not find lasting treatment effects for the treatment takers after the experiment. This is in line with other studies that find that treatment effects typically do not last after treatment – e.g. Leslie et al. (2011) in the context of weight loss. One exception is Charnes and Gneezy (2009), who do find treatment effects for students that had never been to the gym before the experiment.

Given that we have many individual characteristics and some behavioral measures, we can model self-selection on observables. We estimate take-up and goal realization simultaneously with a bivariate probit model (Tobit type-2):

$$\begin{aligned} \text{Take-up} &= 1(X'\beta + \mu > 0) \\ \text{Goal realization} &= 1(Z'\gamma + \epsilon > 0) \end{aligned} \tag{3.1}$$

Take-up is a dummy variable with value one if a subject took up one of the two treatments. Goal realization is a dummy variable with value one if the number of weekly visits equals or exceeds the goal. Since we have two goals, we estimate equations (3.1) separately for each goal. We assume that the errors μ and ϵ have a joint normal distribution with mean zero, a standard error of one, and correlation ρ between the errors. In vector Z we include experimental variables, and a specification to which we add some background characteristics. We select only the group of goal-setters. This is the group for which we have a survey goal, a performance goal, or both. We compare the period during the experiment with the period after the experiment. We leave out the period before the experiment, since we do not know whether subjects had the same goal as the survey goal in the 18 weeks before the experiment. Marginal effects are reported in square brackets

below selected coefficients. At the bottom of the table the p-value is reported of the test that all high-cost dummies are jointly zero. Standard errors are suppressed in order to display the table on one page.
The top panel of Table 3.8 shows the probit estimates of the take-up of treatment. Overall the results are quite similar to the linear probability estimates in the last column of Table 3.3. Women, being assigned to the high-cost treatment, and the factor variable 'chaotic' are negatively correlated with treatment take-up.

The first two columns of the bottom of Table 3.8 show how well subjects in the high-cost treatment did on the survey goal. This goal is usually higher than the performance goal – for which the results are shown in the last two columns. Assignment to the high-cost group is associated with a higher probability of realizing the survey goal, but only in the first regression. The marginal effects are reported below the estimates, both are not significant in the first two columns. There is no difference in realizing the survey goal between takers of the high-cost and the low-cost treatment. The last two columns show how subjects meet the goal as set at take-up of treatment. Subjects in the high-cost treatment are on average 29-34%-points more likely to meet the performance goal, depending on the inclusion of covariates. This is during the experiment, there are no lasting effects on goal realization after the experiment is over. Overall estimates should be interpreted with some caution. First of all: though the total number of observations is large, the number of individuals is small, and it is not sure that the estimators are asymptotically valid with small sample sizes. Second, the estimated correlation coefficient of the errors (ρ) is never significant. This would imply that selection effects do not play a role in goal realization – in the bottom panel of Table 3.8. That seems to be at odds with the notion that for example more motivated people are more likely to take up treatment, and also more likely to meet their goals.

3.5 General discussion

To sum up, we have three main findings. Take-up rates of both treatments are low, and lowest in the deposit treatment. Our second finding is that assignment to treatment increases attendance in both treatments during the experiment, but only compared to a representative control group, or compared to all other members of the gym. We do not find differences between the two treatments in the intent-to-treatment analysis. We find a

Table 3.8: Goal realization with correction for self-selection

	Take-up of treatment			
Female	−1.353**	−1.314**	−1.338**	−1.357**
Age (bottom quartile)	−0.614	−0.601	−0.565	−0.583
Children present in the household	0.614	0.652	0.598	0.611
College education or higher	−0.571	−0.538	−0.564	−0.519
High-cost group	−2.031***	−1.965***	−2.000***	−2.002***
Overweight (BMI> 25)	−0.439	−0.419	−0.481	−0.535
Subscription price for 12 weeks	0.861	0.808	0.840	0.808
Goal 1x a week[a]	0.330	0.373	0.279	0.229
Goal 2x a week[a]	0.895	0.835	0.900	0.895
Missing goal questionnaire[a]	7.020***	7.168***	7.716***	7.028***
Factor chaotic	0.948**	0.916***	0.934***	0.928***
Factor preciseness	0.195	0.210	0.179	0.179
Factor hard time breaking bad habits	−0.504	−0.493	−0.514	−0.516
Factor able to work toward goals	−0.462	−0.416	−0.465	−0.474
Constant	0.857	0.814	0.863	0.926
	Survey goal		Performance goal	
Treatment period	−0.022	0.077	0.171	0.173
High-cost group (HCG)	−0.166	−0.621	−0.226	−0.679
	[−0.048]	[−0.087]	[−0.081]	[−0.213]
HCG × Treatment period	0.607**	0.648	0.937***	0.928***
	[0.176]	[0.091]	[0.335]***	[0.291]**
Female		−0.538*		−0.117
Age (bottom quartile)		−1.993***		−1.146**
Children present in the household		−1.379***		−0.268
College education or higher		1.012**		0.690
Overweight (BMI> 25)		−1.102***		−0.255
Subscription price for 12 weeks		1.691*		0.152
Factor chaotic		0.476***		0.196
Factor preciseness		0.382		0.069
Factor hard time breaking bad habits		0.491		0.140
Factor able to work toward goals		0.244		−0.094
Constant	−0.869**	−2.547*	−0.282	−0.364
ρ	0.056	0.815	0.281	0.566
	(0.73)	(1.25)	(0.29)	(0.51)
Log likelihood	−694	−615	−763	−743
p-value high-cost dummies= 0	0.056	0.075	0.005	0.002

Bivariate probit estimation with 55 subjects and 1,306 subject–week observations. The dependent variable in the top panel is one if the subject took up one of the treatments. The dependent variable in the bottom panel is one if the subject meets the goal as stated in the survey (first two columns), or the performance goal as set upon take-up (last two columns). Robust standard errors are clustered at the individual level. Selected marginal effects are presented in square brackets. */**/*** correspond to 10%/5%/1% significance level.
a: visit goal as answered in the survey. Wanting to visit the gym 3 or 4 times a week is the baseline.

treatment effect for takers of the deposit treatment, we do not find such a treatment effect for takers of the goodbye treatment. The third finding is that respondents state a higher goal in the survey, compared to the performance goal set at take-up. However, both goals are real goals – they are higher than the average number of gym visits prior to the experiment. Treatment takers of the deposit treatment are more likely to meet the performance goal, compared to treatment takers of the goodbye treatment – after an attempt to account for selection. Neither treatment is positively correlated with realizing the higher goal as stated in the survey. None of the treatment effects lasts after the treatment is concluded. In the spirit of the study done by DellaVigna and Malmendier (2006), we tested whether current gym members have a demand for a commitment device. Or alternatively, whether gym members that use their annual membership as a commitment device, have a demand for an additional commitment device. Some remarks the survey indicate that some gym members literally preferred an annual membership over cheaper, but shorter membership types as a commitment device. We find a low take-up rate for the commitment device, similar to other experiments with commitment devices. The incentive per week – 15 euro – seemed about right, but paying 180 euro upfront proved to be quite a challenge for many subjects. Another design feature that maybe would have increased take-up is framing. The commitment device was now framed as a separate contract, but maybe a better framing was to incorporate it as part of a regular membership contract. The question why take-up of commitment devices is low still remains an open question.

Our study contributes to a better understanding of goal-setting. The finding that attendance goals as stated in the survey are higher than the performance goal, can be interpreted as evidence for the distinction between *desires* and *intentions*. Desires is what I would like to do, intentions is what I want to do. Intentions can be at the level of the goal, or at the level of implementation (Dholakia et al., 2007). For goal intentions self-efficacy matters, and constraints matter for implementation intentions. We cannot distinguish between the two, but we do note the difference in goal realization – the deposit treatment is associated with higher realization of the performance goal (the intended goal), but not with the survey goal (the desired goal). This finding raises the question how useful it is to ask for goals in surveys, without providing incentives for goal performance. On the other hand, the associations with the survey goal and background characteristics in Table 3.6 reveals stable and sensible patterns. Higher current performance is positively associated with higher goals, and so is higher variability of past attendance. The positive association between higher variance and higher goals points to individuals wishing for a stable, high level of

performance. The presence of children in the household is negatively associated with the survey goal, as is the self-assessed variable 'being chaotic'. This gives an indication that subjects take into account external (children) and internal (personality) constraints.

Two other possible explanations for setting a lower goal at take-up of treatment are loss-aversion and joy of winning. Loss aversion is a feature of prospect theory (Kahneman and Tversky, 1979), and is the finding that losses loom larger than gains of the same size. By understating the goal, subjects can prevent being hurt by not realizing the goal. A related idea is proposed by Koch and Nafziger (2011), where self-imposed goals can be a rational way for self-regulation. However, the subjects taking up the high-cost treatment have arguably more to loose than the subjects in the low-cost treatment. They experience the loss of falling short of the goal, and also the monetary loss associated with it. Still we find that subjects in both treatments set a lower goal on take-up. Another possible explanation is joy of winning. Conditional on take-up, subjects set a lower goal to experience the pleasure of reaching a goal. However joy of winning is more associated with inter-person comparisons, and the subjects in our treatment did not know each other.

An interesting avenue for further research is that goal setting and take-up of commitment devices seem to be related. This appears to be a joint decision making process. This raises policy questions in a broader context. How should a commitment contract look like for people who want to increase desired behavior? For example, Volpp et al. (2008) find that a deposit contract helps severely overweight people (mean BMI of 34.9) lose weight. An open challenge is the question whether commitment devices can induce lasting behavior change. In the same study of Volpp et al. (2008) and in a follow-up study by the same authors (Leslie et al., 2011), treatment effects are only temporary – after the incentives are removed, the subjects gain again weight. Another avenue of interesting research could be the issue of strategic goal-setting in intra-personal games is, especially in the context of designing commitment devices.

Table 3.9: Observable characteristics of subgroups

	All respondents					High-cost group			Low-cost group		
	All	No-show	HC	LC		Take-up	Decline		Take-up	Decline	
	(A)	(B)	(C)	(D)	$(C-D)$	(E)	(F)	$(E-F)$	(G)	(H)	$(G-H)$
Female	0.74	1.00	0.65	0.65		0.57	0.67		0.69	0.57	
Age	46.26	43.42	47.37	46.74		51.71	46.69		48.13	43.57	
Signed up after the reminder	0.33	0.36	0.29	0.39		0.29	0.29		0.38	0.43	
Single	0.34		0.35	0.30		0.14	0.39		0.38	0.14	
College education or higher	0.58		0.63	0.48		0.71	0.61		0.44	0.57	
Paid work	0.74		0.78	0.65		0.71	0.80		0.63	0.71	
Disposable income less than $40,000$	0.43		0.46	0.37		0.57	0.44		0.36	0.40	
Household saved money last year	0.78		0.79	0.77		1.00	0.76		0.69	1.00	
Children present in the household	0.34		0.35	0.30		0.29	0.36		0.44	0.00	**
Good or excellent health	0.87		0.85	0.91		0.86	0.84		0.94	0.86	
Smokes	0.09		0.08	0.13		0.14	0.07		0.19	0.00	
On average one or more drinks a week	0.75		0.81	0.61	*	1.00	0.78		0.69	0.43	
Body mass index first measurement	24.78		24.54	25.31		25.78	24.35		25.13	25.73	
Body fat percentage first measurement	29.19		28.77	30.13		29.29	28.69		31.00	28.14	
Overweight (bmi>25)	0.47		0.44	0.52		0.57	0.42		0.44	0.71	
Planning horizon is a year or less	0.44		0.40	0.52		0.00	0.47	***	0.40	0.83	*
Happiness (std.)[a]	0.00		-0.16	0.35	**	0.25	-0.22		0.45	0.13	
Factor sloppiness (std.)[a]	0.00		0.01	-0.02		0.48	-0.07		0.07	-0.25	
Factor preciseness (std.)[a]	0.00		0.02	-0.05		-0.48	0.10		0.03	-0.27	
Factor has bad habits (std.)[a]	0.00		0.04	-0.09		-0.37	0.10		-0.12	0.01	
Factor works efficiently towards goals (std.)[a]	0.00		0.00	0.01		0.11	-0.02		-0.09	0.26	
Subscription price for 12 weeks	137.40	139.26	133.05	145.57		139.17	132.10		149.57	137.01	
Exercised >3 days a week last year[b]	0.26		0.26	0.26		0.29	0.26		0.25	0.29	
Has fixed days in the week to exercise	0.70		0.70	0.70		0.43	0.74	*	0.75	0.57	
Wants to exercise more[b]	0.53		0.50	0.61		0.57	0.49		0.56	0.71	
Does other sports next to gym	0.64		0.69	0.52		1.00	0.64	*	0.50	0.57	
Unlimited weekly access	0.87	0.92	0.83	0.91		0.86	0.82		0.88	1.00	
Avg. $0-1$ weekly visits Fall last year	0.34	0.20	0.40	0.35		0.57	0.38		0.31	0.43	
Avg. $1-2$ weekly visits Fall last year	0.32	0.40	0.31	0.26		0.29	0.31		0.31	0.14	
Avg. $2-3$ weekly visits Fall last year	0.18	0.24	0.17	0.13		0.14	0.18		0.13	0.14	
Avg. more than 3 weekly visits Fall last year	0.09	0.04	0.06	0.22	**	0.00	0.07		0.19	0.29	
Mean weekly gym visits Spring last year	1.61	1.78	1.45	1.81		1.20	1.49		1.91	1.64	
Mean weekly gym visits Fall last year	1.53	1.65	1.37	1.75		1.13	1.41		1.67	1.92	
N	100	25	52	23		7	45		16	7	

*/**/*** correspond to 10%/5%/1%. HC is the high-cost group and LC is the low-cost group.
a: standardized variables based on factor analysis.
b: self-reported, question is framed as exercise in general, not only going to the gym.

3.A Construction dataset

The data come from two different sources. The attendance data are collected from the gym administration. The gym uses an electronic fingerprint reader and records the date, time and room entered – but not the exit details. We use the years $2008 - 2010$ and aggregate the number of visits by the calendar week. One problem is that the gym recycles its identifiers. Without additional information it is not easy to distinguish whether a spell with many zero visits should be attributed to different members, or belongs to one member. A cut-off of 26 weeks with zero visits seems to work well. Information on gender and type of membership is also available. The gym offers many membership types, and we categorize them along two dimensions: limited or unlimited access, and access to the fitness rooms, the indoor group sports (e.g. zumba), both indoor and fitness, and a rest category of other types (e.g. company memberships). For all members in January 2010 we also have the date of birth. Other information the gym provided is the price a member paid for a subscription (including discounts). This price is calculated into a equivalent for 12 weeks. Prices paid are only made available for the 103 members that signed up for the experiment.

The second source is the survey. Many questions are taken from the Dutch Household Survey, as administered by CentERdata, including the first ten questions on self-control (question 34). The last eleven questions on self-control are taken from Tangney et al. (2004). With factor analysis four factors are constructed from those 21 items. The response rate was 74.3% and the answers are quite complete overall. For example, many respondents took time to answer the open questions as well. Answers are imputed on three items of the self-control questions for two respondents (with the median response), and for two respondents on the question whether the household saved money last year. The survey could be linked to the administrative data by merging on birthdate and gender. The linkage rate is very high: only three out of a hundred respondents could not be linked to the visiting data. Among those three was one subject who did not show up for the questionnaire, and two who declined the high-cost treatment. A representative control group is drawn from the administrative data. We randomly sample 200 members, where we oversample members with the same observable characteristics as the experiment group. The variables for selection are gender, age $50 - 64$, whether the mean weekly visits in the Fall of the previous year was on average more than one weekly visit, and being a visiting member for a year or more. Table 3.10 shows the regression analysis.

Goal setting is identified by the answers to the question: "How often a month *would* you like to go to the *gym*?". Five treatment takers (one in the high-cost group and four in the low-cost) did not answer this survey question. For those subjects we set the survey goal equal to the take-up goal. The survey question asks for the number of visits per month in categories, where for certain categories the equivalent number of weekly visits is stated as well. Since we are interested in weekly gym attendance, we recode the answers into a weekly goal. Some respondents chose answers that involved numbers numbers in between whole numbers of weekly visits (e.g. in between 2 and 3 weekly visits). In those cases we picked the lower attendance goal to avoid an upward bias in goal realization.

Table 3.10: Comparison experimental group and representative control group

	OLS	Logit
Female	−0.071	−0.351
Age (bottom quartile)	0.034	0.166
Subscription: only fitness	−0.000	−0.003
Subscription: only indoor sports	−0.009	−0.050
Subscription: fitness and indoor sports	−0.011	−0.064
Unlimited weekly access	0.024	0.130
Avg. $1-2$ weekly visits Fall last year	−0.046	−0.235
Avg. $2-3$ weekly visits Fall last year	−0.056	−0.294
Avg. >3 weekly visits Fall last year	0.033	0.141
$1-2$ years visiting member	0.044	0.219
More than 2 years visiting member	0.036	0.182
Constant	0.297**	−0.878
F-test	0.28	
Chi-square likelihood ratio test		3.19
p-value	0.988	0.988
N	275	275

Dependent variable is one if the subject is in the experimental group, and zero otherwise. */**/*** correspond to 10%/5%/1% significance level.

3.B Content recruitment materials

Would you like to exercise more often?[1]
But is it not happening?

Maybe this is something for you. *(name gym)*, the Scientific council for government policy and Tilburg University are conducting a research project. We are looking for gym members that would like to go to the gym at least once a week and who could use a nudge. Participation is of course voluntary.

1. For your participation in the research project you receive 25 euro.
2. You will obtain more insight in your behavior
3. The research project will cost you little time next to exercising, two hours in total over the entire time period.
4. The research project starts February 1, 2010.
5. The research project takes three months.
6. You have to be, or become a paying member of *name gym*.
7. You can participate with all membership types (fitness / cardio / indoor cycling / group sports)

The researchers will of course treat your private details discretely, privately and anonymously. Would you like to participate? Please register at the front desk before January 31. You can also send an email with your name and phone number to *(name gym)*

[1]This is a translation from Dutch. This advertisement was posted on the messageboard of the gym at the entrance. Slightly different versions were posted on the website and sent to the mailing list. All versions contained the same conditions for participation.

3.C Questionnaire

Research project *(name gym)*

Introduction

Welcome at this research project! We are glad that you decided to participate and we are interested in the results of this research project.

On the next pages you will find the research project. It consists of two parts. Part 1 is a questionnaire. Part 2 is on the last page and is a form for taking body measures. Please answer in the following order:

1. Please answer **first** Part 1 - the questionnaire. Filling in takes about half an hour. Please look for a place within the gym where you can sit quietly and are able to answer the questions without being disturbed. We would like to ask you to answer the questions at the gym.
2. **Upon** completing please come to the front desk. An instructor of the gym will help you with Part 2. This is the form for taking your body measures. That will take 15 minutes at most.
3. After finishing this, please put the form in the envelop and close the envelop. Could you **drop** the envelop at the front desk? (*name gym*) will keep the envelopes in a safe place. Twice a week a researcher from Tilburg University will come and collect the envelopes.
4. The staff member at the front desk will give you a second envelop **right away** with further instructions about the remainder of the research project.
5. The researchers want to **thank** you for your participation!

We are well aware that we ask for personal information. Therefore please put the answers and the form back in the envelop and close the envelop. If you wish you can write your signature on the opening of the closed envelop. The envelops with answers will go right away to researchers at Tilburg University. Staff members from (*name gym*) will not see your answers. The researchers are experienced in dealing with private information and will act accordingly.

Part 1: Questionnaire

This section consists of a questionnaire. Please answer the questions as precise as possible. The first set of questions is about a few personal details.

1. What is your date of birth? (dd-mm-yyyy)
2. Gender
   - ☐ Male
   - ☐ Female
3. What is your marital status?
   - ☐ Married
   - ☐ Divorced
   - ☐ Living together
   - ☐ Widowed
   - ☐ Never married
4. What is your highest level of education completed with a degree?
   - ☐ (continued) special education
   - ☐ kindergarten/primary education
   - ☐ VMBO (pre-vocational education)
   - ☐ HAVO, VWO (pre-university education)
   - ☐ senior vocational training or training through apprentice system
   - ☐ vocational colleges
   - ☐ university education
   - ☐ other sort of education/training
5. What is your primary occupation?
   - ☐ employed on a contractual basis
   - ☐ works in own business
   - ☐ free profession, freelance, self-employed
   - ☐ looking for work after having lost job
   - ☐ looking for first-time work
   - ☐ student
   - ☐ works in own household

- ☐ retired (pre-retired, AOW, VUT)
- ☐ partly disabled
- ☐ unpaid work, keeping benefit payments
- ☐ works as a volunteer
- ☐ other occupation

6. How many people are living with you in the house (including yourself)? ...
7. How many children are living with you in the house? ...

The following questions are about how you experience your health.

8. In general, would you say your health is:

   - ☐ excellent
   - ☐ good
   - ☐ fair
   - ☐ not so good
   - ☐ poor

9. Compared to one year ago, would you say your health is better now or worse?

   - ☐ much better
   - ☐ somewhat better
   - ☐ about the same
   - ☐ somewhat worse
   - ☐ much worse

10. Do you suffer from a long illness, disorder, or handicap; or do you suffer from the consequences of an accident?

    - ☐ Yes (continue to question 11)
    - ☐ No (continue to question 12)

11. Please give a short description ...
12. Do you smoke cigarettes at all?

    - ☐ yes, I smoke every now and then (continue to question 13)
    - ☐ yes, I smoke every day (continue to question 13)
    - ☐ no (continue to question 14)

13. About how many cigarettes do you smoke a day?

- ☐ $1-10$ cigarettes a day
- ☐ $10-20$ cigarettes a day
- ☐ more than 20 cigarettes a day

14. About how many alcoholic drinks do you have a week?

    - ☐ 0
    - ☐ $1-3$
    - ☐ $4-6$
    - ☐ 7 or more

15. All in all, to what extent do you consider yourself a happy person? Rate on a scale $0-10$

The following questions are about the type of membership you have at (name gym).

16. What type of membership do you have at *(name gym)*?
17. What conditions of payment do you use?

    - ☐ Every 4 weeks
    - ☐ Every three months
    - ☐ Once a year

18. Do you use automatic withdrawal for payment?

    - ☐ Yes
    - ☐ No

Questions 19 through 23 are about exercising in general. Not only going to the gym, but also other sports, like tennis, football, etc.

19. On average how many times a **week** did you exercise in the past 12 months? ... times
20. Do you exercise the same time(s) each week?

    - ☐ yes
    - ☐ no

21. Do you often exercise together with somebody else?

    - ☐ always
    - ☐ often
    - ☐ sometimes
    - ☐ never

22. Would you like to exercise more often than you are doing currently?

    ☐ yes (continue to question 23)

    ☐ no (continue to question 25)

23. What is the most important reason that you do not exercise more often? ...

24. How often a month WOULD you like to go to the GYM?

    ☐ $0-1$ times a month

    ☐ $2-3$ times a month

    ☐ $4-5$ times a month (that is 1 time a week)

    ☐ $6-7$ times a month

    ☐ $8-9$ times a month (that is 2 times a week)

    ☐ $10-11$ times a month

    ☐ $12-13$ times a month (that is 3 times a week)

    ☐ more than 13 times a month

25. Do you do other sports during the year, other than the gym?

    ☐ no

    ☐ yes, like: ...

26. Next to your gym membership, are you a member of other sports clubs?

    ☐ no

    ☐ yes ...

27. What is the most important reason for you to go the gym?

    ☐ loose weight

    ☐ maintain weight

    ☐ improve condition

    ☐ maintain condition

    ☐ get a good figure

    ☐ nice atmosphere, contact with friends

    ☐ other, like ...

28. The TOTAL NET INCOME OF YOUR HOUSEHOLD consists of the income of all members of the household, after deduction of taxes and premiums for social insurance policies, over the past 12 months. Into which of the categories mentioned below did the total net income of your household go IN THE PAST 12 MONTHS?

- ☐ less than 10.000
- ☐ between 11.000 and 14.000
- ☐ between 14.000 and 22.000
- ☐ between 22.000 and 40.000
- ☐ between 40.000 and 75.000
- ☐ 75.000 or more
- ☐ I do not know

29. Do you think it makes sense to save money, considering the current general economic situation?

- ☐ yes, certainly
- ☐ yes, perhaps
- ☐ probably
- ☐ certainly not
- ☐ I don't know

30. Did your household put any money aside IN THE PAST 12 MONTHS?

- ☐ yes
- ☐ no

31. About how much money has your household put aside IN THE PAST 12 MONTHS?

- ☐ less than $1,500$
- ☐ between $1,500$ and $5,000$
- ☐ between $5,000$ and $2,500$
- ☐ between $12,500$ and $20,000$
- ☐ between $20,000$ and $37,500$
- ☐ between $37,500$ and $75,000$
- ☐ $75,000$ or more
- ☐ I don't know

32. Is your household planning to put money aside IN THE NEXT 12 MONTHS?

- ☐ yes, certainly
- ☐ yes, perhaps
- ☐ probably not
- ☐ certainly not

☐ I don't know

33. People use different time-horizons when they decide about what part of the income to spend, and what part to save. Which of the time-horizons mentioned below is in your household MOST important with regard to planning expenditures and savings?

☐ the next couple of months
☐ the next year
☐ the next couple of years
☐ the next 5 to 10 years
☐ more than 10 years from now

34. For the following statements on human behavior, please choose the statement which applies most to you. Describe yourself as you are, not as how you want to be. Describe yourself in comparison to other people you know of the same sex and of about the same age.

	not at all				very much
	1	2	3	4	5
I do chores right away	☐	☐	☐	☐	☐
I'll leave my things lying around	☐	☐	☐	☐	☐
I live my life according to schedules	☐	☐	☐	☐	☐
I neglect my obligations	☐	☐	☐	☐	☐
I have an eye for details	☐	☐	☐	☐	☐
I am accurate in my work	☐	☐	☐	☐	☐
I forget to put things back where they belong	☐	☐	☐	☐	☐
I am always well prepared	☐	☐	☐	☐	☐
I often make a mess of things	☐	☐	☐	☐	☐
I like order	☐	☐	☐	☐	☐
I have a hard time breaking bad habits	☐	☐	☐	☐	☐
I am lazy	☐	☐	☐	☐	☐
I have trouble saying no	☐	☐	☐	☐	☐
I change my mind fairly often	☐	☐	☐	☐	☐
I wish I had more self-discipline	☐	☐	☐	☐	☐
I refuse things that are bad for me	☐	☐	☐	☐	☐
I get carried away by my feelings	☐	☐	☐	☐	☐
Im not easily discouraged	☐	☐	☐	☐	☐
I have trouble concentrating	☐	☐	☐	☐	☐
Sometimes I cant stop myself from doing something, even if I know it is wrong.	☐	☐	☐	☐	☐
I am able to work effectively toward long-term goals	☐	☐	☐	☐	☐

Thank you! This is the end of Part 1. Please go the front desk for Part 2.

Part 2: Body Measures

An instructor will take the following measures together with you and write them down. In about three months you will receive a notification to collect the same measurements again. You will receive a personalized statement with both measurements.

1. Length (in centimeters) ...
2. Hip size (in centimeters) ...
3. Weight (without shoes, in kilograms) ...
4. Body fat percentage ...
5. Pulse (30 seconds times two) ...

Thank you very much - this was Part 2 of the research project. Please put the form in the envelop, close the envelop and drop it at the front desk. You will receive a second envelop with further instructions.

Table 3.11: Body mass index, body fat percentage, and happiness

	Body mass index			Bodyfat percentage			Happiness			
	before	after	change	before	after	change	before	after	change	*N*
All	24.8	24.7	-0.1	29.4	29.0	-0.4	7.5	7.7	0.2	41
High-cost										
All	24.6	24.5	-0.1	28.0	27.8	-0.2	7.2	7.6	0.4	25
Take-up	25.8	26.0	0.2	29.3	28.7	-0.6	8.0	8.1	0.1	7
Decline	24.2	24.0	-0.2	27.5	27.4	-0.1	6.8	7.3	0.5	18
Low-cost										
All	25.1	24.8	-0.2	31.6	30.9	-0.7	8.1	8.0	-0.1	16
Take-up	25.0	24.9	-0.1	31.7	30.8	-0.9	8.1	8.0	-0.1	14
Decline	25.3	24.5	-0.8	31.0	32.0	1.0	7.5	8.0	1.0	2

Happiness is rated from 0 (unhappy) to 10 (very happy). */**/*** correspond to 10%/5%/1% significance level.

3.D Treatment instructions

Instructions to the high-cost treatment group

Again a warm welcome for participating in this research project.

We would like to thank you for filling out the questionnaire. But that is not all! There is more, please take some time to continue reading.

Would you like to exercise more often? This research project is meant for people that would like to exercise more often. Perhaps you are familiar with this situation. You have the best intentions to do a certain thing, but it is not happening. Or you are exercising already regularly, but would like to go more often. Perhaps you are exercising with a certain goal in mind. Whatever the situation, you could use a nudge to achieve your goal.

Deposit scheme: a nudge

A helping hand, a nudge, sometimes we just need that. Somebody else could give you one. But in the end it is best if you do it yourself. Money could be one possible way to give you the nudge that you need. That is why the researchers and staff members of (*name gym*) give you the opportunity to join the deposit membership.

It works in the following way. You keep your current membership with (*name gym*). Nothing changes. On top of that subscription comes the deposit-part. We chose the amount for you. The research project takes 12 weeks. The amount per week is 15 euro. The total deposit is $12 \times 15 = 180$ euro. You set a weekly goal. You choose how often you would like to exercise at (*name gym*). You pay the deposit of 180 euro upfront. For every week that you reach your goal, you get 15 euro back. When you meet your goal for twelve weeks, you get your entire deposit, but more importantly: you have met your goal for twelve weeks (that is three months)!

This is the same method that three members of (*name gym*) used last year to come more often to the gym. A national television show made a special about this. You can watch this edition online. Don't worry, you will not appear on television. This is scientific research!

Can I just come to the gym?

Sure! The deposit scheme is meant to help you realize your own goals. To summarize: the deposit scheme is hundred eighty euro for twelve weeks. You pay this money upfront at (*name gym*) when you decide to participate. You also choose how many times a week you

would like to go to (*name gym*). When you participate, you can exercise in any way you would like to. Every time you are finished exercising, you walk by the frontdesk and you say goodbye. The staff members at the frontdesk keep track how many times you have been to the gym and whether you meet your goal. After three months you receive a personalized overview. For every week that you have met your goal, you receive 15 euro cash. Please note that it is important to walk by the frontdesk every time you finished exercising. The week starts on Sunday morning and ends Saturday night. It is not possible to skip a week and to go double the week after. For each week there is your goal and for each week is 15 euro available. The only exception is when you freeze your subscription in case of illness or vacation. The usual policy of (*name gym*) applies. In those cases the research project will be extended in May for the number of days you froze your membership (Note that this is not possible with all subscription types). Please let (*name gym*) know when you have vacation plans.

How do I set my weekly goal?
Exercising and workout are important. Regularly exercising and regular workout are even more important. It is better to exercise once a week for a year, than six times a week for three months and zero times the rest of the year. Regularity is key. It is better to choose a realistic goal than to chose an ambitious one. Perhaps you have a goal in mind how often you would like to come. Please fill out that goal on the form.
In case you do not have a specific goal, but you would like to come more often than you do now. You could choose to set a goal that is once a week more than you do now. For example, if you come regularly once a week, and you would like to increase this frequency, than you could set a goal of two times a week. Or once a week. Or seven times. You set your goal how often you would like to come. However, note that you only receive your deposit share of 15 euro a week when you meet your goal.
How many times a week would you like to come in the next three months? Fill in that goal on the form and drop it at the frontdesk. Can you let us know before February 14 whether you would like to join the deposit scheme? We hope that you would like to do so.

We wish you lots of pleasure with exercising.

The first week is week 7 (February 14 until February 20), the twelfth and last week is week 18 (May 2 until May 8). This is a research project of the Scientific council for gov-

ernment policy and Tilburg University. (*name gym*) participates without interest. (*name gym*), the Scientific council for government policy and Tilburg University cannot be held liable.

Instructions to the low-cost treatment group

Again a warm welcome for participating in this research project.
We would like to thank you for filling out the questionnaire. But that is not all! There is more, please take time to read on.

Exercising more often?

Would you like to exercise more often? This research project is meant for people that would like to exercise more often. Perhaps you are familiar with this situation. You have the best intentions to do a certain thing, but it is not happening. Or you are exercising already regularly, but would like to go more often. Perhaps you are exercising with a certain goal in mind. Whatever the situation, you could use a nudge to achieve your goal.

How do I set my weekly goal?

Exercising and workout are important. Regularly exercising and regular workout are even more important. It is better to exercise once a week for a year, than six times a week for three months and zero times the rest of the year. Regularity is key. It is better to choose a realistic goal than to choose an ambitious one. Perhaps you have a goal in mind how often you would like to come.
In case you do not have a specific goal, but you would like to come more often than you do now. You could choose to set a goal that is once a week more than you do now. For example, if you come regularly once a week, and you would like to increase this frequency, than you could set a goal of two times a week. How many times a week would you like to come in the next three months? Fill in that goal on the form and drop it at the frontdesk. You can go to the gym like you are used to.

Every time you are finished exercising, you walk by the frontdesk and you say goodbye. The staff members at the frontdesk keep track how many times you have been to the gym and whether you meet your goal. After three months you receive a personalized overview. The first week is week 7 (February 14 until February 20), the twelfth and last week is week 18 (May 2 until May 8). We wish you lots of pleasure with exercising.

CHAPTER 4

Explaining intra-monthly consumption patterns

4.1 Introduction

A typical month for many households probably looks like this: the household receives income, pays the bills, and has at its disposal what remains. The "bills" consist of expenditures like mortgage or rent payments, health insurance and car loan payments. These expenditures are typically to be paid every month, the budget share is large, and adjustments are not frequently made (Chetty and Szeidl, 2007). Expenditures with those properties are called consumption commitments. In this study I focus on rent and mortgage payments as the main consumption commitment of households. The research question is whether households adjust other consumption expenditures in response to the timing of payment of rent or mortgage.

It is well studied how household consumption responds to income receipt (see e.g. Browning and Collado, 2001; Stephens, 2003; and Shapiro, 2005). However, there is not much literature on the effect of consumption commitments on consumption smoothing. The focus on the income side is only half of the story. Many expenditures are fixed over short time periods, and can typically be adjusted only once a year. In the short-run income and fixed expenditures together determine the amount of "disposable liquidity" of a household. According to the lifecycle hypothesis there should not be a consumption response to either income or fixed expenditures – if there is no uncertainty over payday, size or frequency of payment of income and expenditures. In the lifecycle model both consumption commitments and income enter the utility maximization problem of the household as part of the budget constraint. The smoothing of consumption should not be affected by the timing of payment of income, nor the timing of payment of fixed expenditures.

Recent studies find that both consumption expenditures and consumption are higher in the week after Social Security checks are distributed compared to the week before (Stephens, 2003; Mastrobuoni and Weinberg, 2009). The intra-monthly cycle is stronger for households in which Social Security is the main income source and households with little assets. Hyperbolic discounting is the main explanation. For households with little or no assets Mastrobuoni and Weinberg (2009) reject the exponential discounting model in favor of the hyperbolic discounting model. In the context of food stamps Shapiro (2005) also rejects the exponential discounting model. In both studies the drop in caloric intake over the month is substantial. Food stamp recipients consume $10-15\%$ less calories the week before food stamps are disbursed. Social Security recipients with little or no assets consume around 25% less calories at the end of the month compared to the beginning.

If these households are hyperbolic discounters, then a potential welfare improvement could be to distribute Social Security checks more often, holding the total amount of Social Security constant. More frequent distribution of checks could potentially be a commitment device to smooth consumption. This policy prescription does not have to be limited to Social Security recipients only. Parsons and Van Wesep (2013) propose to distribute the paychecks of wage-earners more often, depending on the degree of self-control of employees. Employers could improve the welfare of hyperbolic discounters by distributing paychecks more often and cater to employees' desire for smooth consumption. However, I show in this study that this policy proposal is one-sided. In models with hyperbolic discounting the payment of consumption commitments starts to play a role as well.

In the empirical section I use the US Consumption Expenditures Diary Survey (CEX). The CEX is a budget survey in which households record their expenditures for up to 14 days. I exploit the variation within the month with which households pay their rent or mortgage. The empirical models I estimate control for both timing of income – as proxied by controlling for weeks of the calendar month, and timing of rent or mortgage payments. My main finding is that consumption expenditures respond to the timing of payment of mortgage or rent. Consumption expenditures are higher in the weeks after rent/mortgage payments are made, compared to the weeks before. This pattern exists for both renters and home-owners. The second finding is that weekly, biweekly, and monthly paid households with the same timing of payment of rent or mortgage, have similar patterns of consumption expenditures. Consumption expenditures are not smoother for households with higher pay frequencies. I find the same patterns for households with high education, households in the highest income quartile, and for non-smokers.

I also revisit the finding in the literature of a cycle of consumption expenditures for Social Security recipients. Analyzing the data of more recent years I replicate Stephens' finding of an intra-monthly cycle (Stephens, 2003). This result is analyzed for two subgroups: Social Security recipients with mortgage/rent payments and Social Security recipients without – those are home-owners without mortgage payments. I find that the group of Social Security recipients with current rent or mortgage payments is driving the overall group result. There is no cycle in expenditures for Social Security recipients with no rent or mortgage obligations. Taking the subgroup of Social Security recipients with current rent/mortgage obligations, the pattern appears to be even larger around the paydate of rent and mortgage. The main conclusion is that the finding of a cycle in consumption expenditures is mainly driven by the timing of rent or mortgage payments. For both the wage-earners and the Social Security recipients I find an interesting pattern in consumption expenditures. The pattern is a cycle, but the peak of the cycle is in the second week after rent/mortgage is paid.

Three candidates for interpreting the empirical findings are liquidity constraints, precautionary savings, and hyperbolic discounting. Liquidity constraints affect the level of consumption, but do not generate a cycle. Households consume their entire income, but do smooth consumption. A second explanation is that there is a short-term precautionary motive for holding liquidity. Most US households still pay their bills by check, and not by direct deposit. This means that households need to make sure that there is enough money on the bank to pay for rent/mortgage, as well as for other consumption commitments. There usually is a also some time in between the writing of a check for the rent and the cashing. Since bouncing of checks is costly for a household, this could generate a short-term precautionary motive to hold liquidity. Moreover, many consumption commitments cannot be paid with a creditcard at all, but need to be paid with cash. Telyukova (*forthcoming*) makes this distinction between 'cash-goods' and 'credit-goods' to explain the credit card debt puzzle. This is the phenomenon that around 27% of US households hold sizeable creditcard debt, and hold at the same time liquid assets.

Hyperbolic discounting is a third possible explanation. With hyperbolic discounting consumption cycles in the short run can be explained. One problem with hyperbolic discounting as an explanation is that households need to be either liquidity constrained, or have a precautionary savings motive. My findings are consistent with a combination of hyperbolic discounting with one of the two other explanations. I do not find a correlation between Social Security receipt and consumption smoothing. This suggest that the

precautionary motive is more important than liquidity constraints. Moreover, this would reconcile the findings in this chapter with the evidence of intra-monthly cycles in caloric intake (Mastrobuoni and Weinberg, 2009), financial crimes (Foley, 2011) and mortality (Evans and Moore, 2012). After rent/mortgage are paid, there is a peak in disposable liquidity of households. Households with an urge for immediate gratification respond more to disposable liquidity and to a lesser extent to direct income receipt.

In the model section I show that the theoretical argument underpinning the policy recommendation of higher paycheck distribution is one-sided. In a simple model with hyperbolic discounting I show that a higher frequency of paychecks is no longer a consumption smoothing device when consumption commitments are added to the model. The model generates conditions under which different frequencies of income payment generate the same consumption profile in the presence of a consumption commitment and hyperbolic discounting. The intuition is that a household needs to save in the time before payment of the consumption commitment. Naive hyperbolic discounters postpone saving. This creates a cycle in consumption around the paydate of consumption commitments. This result holds if the budget share of the consumption commitment is sufficiently large, and the frequency of payment of the consumption commitment is the same or lower compared to the frequency of income payment. This implies that households with the same frequency of payment of the consumption commitment and different pay frequencies have the same consumption profile.

The contribution of this chapter is that households respond to disposable liquidity, and not to the receipt of income per se. This may seem trivial, but it is the combination of income and fixed expenditures that determines short-run household liquidity. Only looking at the income side of households ignores the importance of consumption commitments and generates one-sided policy recommendations.

The next section gives an overview of monthly cycles in consumption expenditures and consumption, financial crimes, health and mortality. The data section describes the empirical strategy as well as the CEX data. I show the empirical results for wage earners and Social Security recipients. The model section shows under which conditions common wage payment frequencies generate equivalent consumption profiles. The last section concludes.

4.2 Monthly cycles and consumption commitments

This study is related to three strands in the literature. The first strand studies the response of measures of consumption to income receipt. The second literature elaborates on consumption commitments and the third is concerned with optimal cash management.

The existence of a within monthly cycle in consumption expenditures and caloric intake is well studied. Stephens (2003) uses the CEX diary data for the years $1986 - 1996$. He documents an increase in consumption expenditures in the first week after Social Security checks are distributed. This is a violation of the lifecycle hypothesis if consumption expenditures are correlated with actual consumption. Mastrobuoni and Weinberg (2009) show that caloric intake follows consumption expenditures. They use a food intake survey and find a monthly cycle for caloric intake around the payment of Social Security. This cycle is found for households with less than $\$5,000$ in savings, but not for households with higher levels of assets. For low-asset households exponential discounting is rejected in favor of hyperbolic discounting.

In both studies there is little variation in the paydate. Social Security is always paid once a month, in the first week of the month. In 1997 the distribution scheme changed. Social Security is still paid once a month, but new claimants receive checks on the second, third or fourth Wednesday of the month. Which of the three Wednesdays depends on the birthday of the claimant. This change creates a natural experiment which Mastrobuoni and Weinberg exploit in another study (Mastrobuoni and Weinberg, 2011). They use the CEX diary data for the years $1986 - 2007$. They find that after the change in 1997 consumption expenditures do not respond to the receipt of Social Security checks. However, Evans and Moore (2012) still find a cycle around the first of the calendar month for Social Security recipients for the years $1996 - 2004$. Not only for Social Security recipients, they also find a cycle around the first of the month for several other subgroups: households whose head has less than a college degree, recipients of federal assistance, and households with a family income less than $\$30,000$. In the same study they document an intra-monthly cycle in a rather different outcome variable than consumption: mortality increases in the first week of the month relative to the last week. This cycle in mortality only exists for causes of death that are related to activity such as homicides, heart attacks and traffic accidents. There is no cycle for cancer related causes of death. They argue that there is a causal chain from excess liquidity to activity, resulting in a higher probability of mortality. This is evidence that households do not respond to income per se, but to disposable liquidity. In

the empirical section I estimate models with controls for the first of the month. Two other studies document a monthly cycle in outcomes other than consumption expenditures or caloric intake. Dobkin and Puller (2007) find a 23 percent increase in drug-related hospital admissions at the beginning of the month after release of supplemental Social Security. Hospital mortality increases by 22 percent. Foley (2011) reports a cycle in financial related crimes in major US cities. There is no intra-monthly cycle for violent crimes. The cycle disappears in cities where welfare payments are not concentrated around the first of the month, but are paid out in a more staggered way throughout the month.

A monthly cycle in household expenditures could trigger a response by firms. Hastings and Washington (2010) use scanner data from the US and find that supermarkets have a procyclical pricing strategy: prices are high at the beginning of the month and low at the end. Consumers would be better off to shift shopping to the end of the month. The price effect does not corroborate the result of Stephens (2003), since he also finds an increase in probability of having consumption expenditures after income receipt.

Most studies explain their findings with hyperbolic discounting (e.g. Mastrobuoni and Weinberg, 2009). A possible welfare improvement could be to partition welfare checks. Stephens and Unayama (2011) exploit an exogenous change in the frequency of pension payments in Japan, holding total pension income constant. Public pension benefits were paid out every three months before the change, and once every two months after. They find that after the change elderly households are better able to smooth consumption expenditures. Almost all households in my data have a frequency of income payment that is higher and at most equal to the frequency of rent/mortgage payment.

All mentioned studies only focus on the payment of income and the response of the household to income receipt. To the best of my knowledge, there are no studies that look into how well households smooth consumption with respect to the payment of *other* consumption expenditures. The literature on consumption commitments acknowledges that it is costly to change the level of major consumption expenditures. Consumption commitments are defined as goods for which it is costly to change the level of consumption. Chetty and Szeidl (2007) show that consumption commitments affect risk preferences by amplifying risk attitudes towards moderate-stake risks, and that they create a motive to take large pay-off gambles. Following up in a second study, Chetty and Szeidl (2010) use consumption commitments as a foundation for a model with reference-dependent preferences and habit formation. Shore and Sinai (2005) present and test a model in which an increase in income risk in the presence of consumption commitments is associated with an increase in

consumption, contrary to what a model with precautionary savings would predict. Finally Postlewaite et al. (2008) analyze a model in which consumption commitments can induce risk-neutral households to be risk averse over small variations in income, but sometimes to seek risk over large variations. They show that optimal employment contracts will smooth wages conditional on being employed, but may incorporate a possibility of unemployment. Coulibaly and Li (2006) find that homeowners who made the last mortgage payment increase savings, but not non-durable consumption. I focus on consumption expenditures within the month, instead of the more unique life-event of paying off mortgage debt.

This study takes into account both the income side of the household and the timing of rent and mortgage payments. Disposable liquidity is the difference between income and fixed expenditures. This is liquidity that households can use for other, more variable consumption expenditures. There is a connection with a third literature as well, the literature on cash management (Baumol, 1952; Tobin, 1956). Alvarez and Lippi (2012) study a model with uncertain lumpy expenditures. They show that the optimal cash holding strategy is determined by large, infrequent expenditures. Households have higher cash holdings for the event of a large expenditure. The relation between this study and the literature on cycles in consumption is that consumption could be a function of disposable liquidity.

4.3 Data

In the empirical section I study how the timing of payment of the main consumption commitment of most households – rent and mortgage payments – affects the timing of *other* expenditures. The dataset used is the US Consumption Expenditure diary Survey (CEX diary). The CEX diary is a repeated crossection of around 4500 consumer units per year. Every household keeps a diary of consumption expenditures for up to 14 days. The survey mainly asks for high-frequency consumption categories, which implies certain categories are not asked for (notably durables and semi-durables – these expenditures are being asked in the CEX quarterly survey). Many households do record the date of rent and mortgage payment.

The start of a diary is a random *day* in the year, with oversampling of households in December. The frequency of payment of the last paycheck is asked since 1998, and I use the waves $1998 - 2010$. The survey also collects information on household composition, labor earnings and other sources of income. The date of last pay is not recorded. One

advantage of the CEX diary is that higher frequency consumption goods such as food are measured, to abstract from the problems of goods with a durables dimension. Though consumption expenditures are measured well, there are notable issues like measurement error and topcoding in income variables and incomplete reporting. Response rates are around 80%.

I select the households with 1 or 2 earners. Households that are flagged as incomplete income respondents are dropped, as are households in the top 3% of income. Wage-earners are restricted to be one the three most common types of pay frequency in the US: monthly, biweekly and weekly. For households with two earners, the frequency of pay is determined by the earner whose annual wage income is at least 70% of the total household wage income. Of these households, 31% is paid weekly, 56% is paid every other week, 4% is paid twice a month, and 9% is paid every month. Another subsample consists of Social Security recipients for whom at least 50% of the household income constitutes of Social Security.

I follow Evans and Moore (2012) by constructing two expenditure categories. The first one is the sum of food at home and food away, but no alcohol. The second category includes all sorts of non-food items: alcohol, cigarettes, apparel, gasoline, entertainment, personal products, personal services, and over-the-counter drugs. Total expenditures is the sum of these two categories, but does not include rent and mortgage payments, utilities, or insurance payments. All expenditures that are indicated as a gift for somebody outside the household are dropped, as well as the top 3% of expenditures. The expenditures are inflated by the CPI of December 2008. Following previous studies (Stephens, 2003; Mastrobuoni and Weinberg, 2011) I drop households that have all expenditures recorded on the first day of the diary week, since for those respondents the exact date of expenditures cannot be verified. When households do not record dates of expenditures in a week, the Bureau of Labor Statistics assigns all expenditures to the first day of the diary week.

Table 4.1 presents the summary statistics of the households. The mean budget share of rent/mortgage is 0.25. This is similar to the mean of expenditures on shelter that Chetty and Szeidl (2007) report using the CEX quarterly data (0.22). Other consumption commitments they report (their Table 1, p. 835) are cars (excluding gas), budget share 0.147, apparel (0.051), furniture/appliances (0.044), and health insurance (0.030). Housing expenditures have by far the largest budget share and are not frequently adjusted. Housing tenure is a separate variable in the CEX data. About 31% of the households owns the home and has mortgage obligations at the time of the questionnaire. Around 46% of the households owns the home without mortgage obligations and 23% rents the home.

Table 4.1: Summary statistics entire sample

	mean	sd	min	median	max	N
Budget share mortgage/rent (α)	0.25	0.19	0.001	0.20	1	13987
Mortgage payment (month)	1083.79	633.93	3	964	6602	7987
Rent payment (month)	723.29	435.32	2	660	3201	6000
Owned with mortgage	0.31	0.46	0	0	1	25833
Owned without mortgage	0.46	0.50	0	0	1	25833
Rented	0.23	0.42	0	0	1	25833
Male	0.48	0.50	0	0	1	25833
Age	53.47	18.13	18	52	94	25833
Less than highschool	0.16	0.36	0	0	1	25833
Highschool	0.30	0.46	0	0	1	25833
Some college	0.20	0.40	0	0	1	25833
College or more	0.34	0.47	0	0	1	25833
Urban area	0.90	0.30	0	1	1	25833
Number of household members	2.39	1.39	1	2	14	25833
White	0.86	0.34	0	1	1	25833
Black	0.09	0.29	0	0	1	25833
Other race	0.05	0.21	0	0	1	25833
Weekly pay	0.31	0.46	0	0	1	12248
Biweekly pay	0.56	0.50	0	1	1	12248
Semi-monthly pay	0.04	0.19	0	0	1	12248
Monthly pay	0.09	0.29	0	0	1	12248
Total expenditures	38.97	39.14	0	26	208	25833
Food expenditures	22.39	27.46	0	12	200	25833
Non-food expenditures	16.58	24.70	0	6	201	25833
Annual real wage income	39.71	41.66	0	30	335	25833
Annual real Social Security income	6.22	9.10	0	0	54	25833
Annual real pre-tax income	52.20	38.89	0	42	371	25833

Income variables (divided by $1,000$), mortgage and rent payments and expenditures are in 2008 US dollar.

I drop respondents in student housing, and respondents making mortgage payments but not reporting home ownership. I focus on households with rent and mortgage payments for three reasons. First, rent and mortgage payments generally have the largest budget share of all household expenditures. Second, they are to be paid every month and the consequences of missing a payment are potentially severe. Finally, the transaction costs of changing consumption are high. I take rent and mortgage payments as a proxy for total consumption commitments of the household.

About a third of the respondents in the sample report having made a mortgage or rent payment (around $14,000$ respondents). This is not necessarily evidence for underreporting. The frame for data collecting of the CEX diary data is such that households start their 14-day diary at any day of the month. It is likely that for many households the monthly rent or mortgage payment falls outside the diary window. Table 4.12 compares the households that report a rent or mortgage payment with those who do not. There are some small differences, mainly on housing tenure and race, but overall the households are quite similar in observable characteristics. On average each household contributes 12 out of 14 diary days. If a month has 30 days, then the probability that a household starts the survey that includes the date of rent/mortgage payment is 0.4. In Table 4.12 the fraction of households reporting a mortgage/rent payment is in between 0.34 and 0.39. There seems to be some underreporting, but not too much. The budget share of mortgage and rent is calculated from the CEX diary data as the observed mortgage or rent payment, times twelve, divided by annual household income after taxes.

The frequency of rent and mortgage payments is not recorded in the CEX data, but is available in the Survey of Consumer Finances. Table 4.2 shows that almost all households pay their rent and mortgage once a month: 99.3% of the renters and 98.1% of the mortgage payers. Note that some other larger expenditures also have a monthly frequency of payment – study loan repayments and car insurance.

4.4 Empirical strategy

Many households in the CEX diary survey record the date when they pay their rent or mortgage. Figure 4.1 shows the distribution of rent and mortgage payments over the month in the sample. There is a mild U-shape visible – most of the payments are concentrated at the beginning and at the end of the calendar month. However, there is quite some variation in between, with small spikes on the 8th and the 15th of the month. The profile

for mortgage payers is a little bit more flat compared to that of the renters. Given that I only observe recorded rent/mortgage payments, I cannot distinguish between early, late or timely payments. This should not be a problem, since under the lifecycle hypothesis there should not be any fluctuations in consumption expenditures around an *anticipated* paydate. Furthermore, Table 4.13 in the Appendix shows the average rent and mortgage payments made over the weeks of the calendar month. The mean payments and budget shares over the weeks of the calendar month are very similar, which does not suggest systematic late (or early) payments. I set the date of payment of rent and mortgage payment at $t = 0$, and I center the days in weeks before and weeks after paydate. Identification comes from the variation in payments of rent and mortgage within the month.

Following Stephens (2003) the empirical model is:

$$C_{it} = \beta_i + \sum_{\substack{w=-2 \\ w\neq -1}}^{2} \delta_w dWeek_w^{RM} + \sum_{\substack{v=-2 \\ v\neq -1}}^{3} \gamma_v dCalweek_v + \sum_{k=2}^{7} \kappa_k DOW_k + \sum_{l=2}^{14} \lambda_l DOS_l + \epsilon_{it}. \quad (4.1)$$

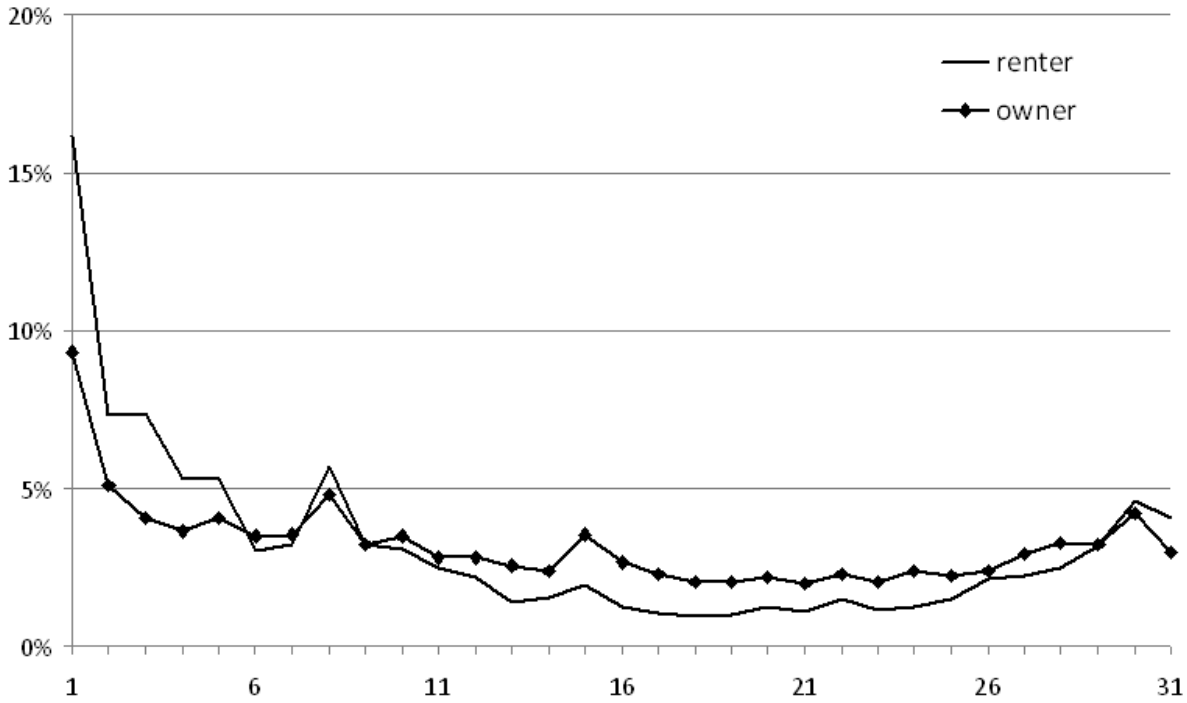
C_{it} is the expenditures of household i on day t. C_{it} can be one the three consumption categories: total food, total non-food and the sum of the two. A household specific fixed effect β_i is included in the regression. $dWeek_w^{RM}$ is a dummy for one of the four weeks around the payment of rent and mortgage. The week before payment is the omitted category. Evans and Moore (2012) report a strong cycle in consumption expenditures around the first of the calendar month. To capture fixed effects of the calendar month I add dummies for the weeks of the calendar month. Two weeks before the first of the month is $dCalweek_{-2}$, the week before the first is the omitted category, the first week of the month is $dCalweek_{+1}$ and $dCalweek_{+2}$ the second. $dCalweek_{+3}$ is not a full week,

Table 4.2: Frequency distribution of payment frequency

	Rent	Mortgage	Car lease	Education loans
Weekly	0.2%	0.1%	0.1%	0.1%
Biweekly	0.1%	0.6%	0.1%	0.1%
Semi-monthly	0.0%	0.1%	0.0%	0.2%
Monthly	99.3%	98.1%	98.0%	97.9%
Other	0.3%	1.1%	1.8%	1.6%

Fraction of households and payment frequency of selected categories. Source: Survey of Consumer Finances, waves 1998, 2001, 2004, 2007 and 2010.

Figure 4.1: Frequency distribution of rent and mortgage payments over the month



but contains 0 to 3 days, depending on the length of the month. DOW_k and DOS_l are dummies for the day of the week and the day of the survey, to pick up effects of day of the week and effects of survey fatigue. I estimate equation (4.1) by OLS and I allow for arbitrary within household serial correlation by clustering standard errors on the level of the household. There are $52,596$ households with $652,936$ observations, or around 12.4 days on average per household. This includes days with zero expenditures.

A priori the shape of the cycle (if any) can be different for wage-earners with different frequencies of pay. Therefore I estimate equation (4.1) separately for weekly, biweekly and monthly paid households. According to the lifecycle theory the coefficients δ_w should be equal to zero. In all regression tables the p-value of the F-tests on the joint significance of $dWeek_w^{RM}$ is reported.

4.5 A spike on the day of rent/mortgage payments

Figures 4.2–4.3 present a taste of the results. Each figure show the results of two separate regressions. The first regression groups the days in relation to the date of mortgage and rent payment (the line without diamonds), the second groups the days around the first of the calendar month (line with diamonds). The regressions contain dummies for each day in the 28-day window, and the day before payment of rent/mortgage is the reference day; the day before the first of the month is the baseline for the calendar days. The regressions include a household fixed effect and dummies for the day of the week and day of the survey.

The first graph includes is for all renters in the sample with a recorded rent payment. There is a clear and statistically significant spike in total consumption expenditures on the day of payment of rent. Note that rent and mortgage payments itself are not included in total expenditures. The peak is at about \$7.7, on a daily average of \$26.3. There is no spike in expenditures on the first day of the month (contrary to what Evans and Moore (2012) find). The next graph shows the result of the same regressions for house-owners with an observed mortgage payment. This gives a similar picture – a significant spike on the day that households pay their mortgage, significant both in economic and in statistical terms. Both pictures are very similar to the ones Stephens presents for Social Security recipients (Stephens, 2003). It is interesting to find similar patterns for house-owners and renters. House-owners are thought to be less prone to liquidity constraints, since they have more assets that can serve as collateral. In absolute terms the spike a little larger for renters (\$7.7 versus \$6.9), but much larger relative to average daily consumption expenditures.

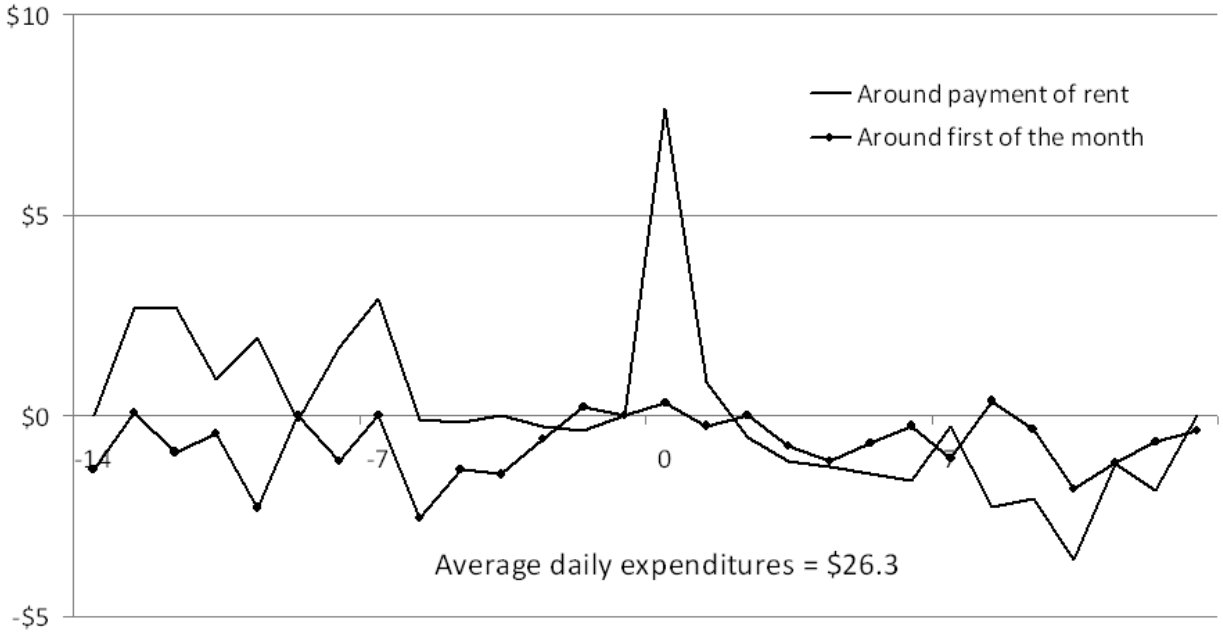


Figure 4.2: Renters: total expenditures around rent payments

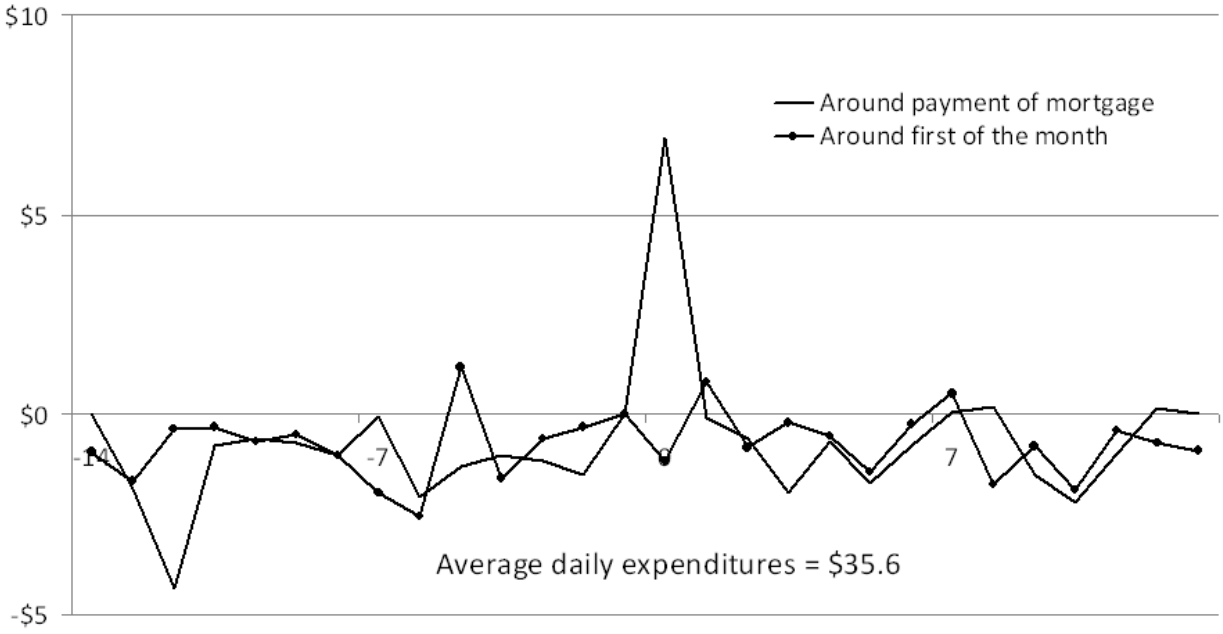


Figure 4.3: Home-owners: total expenditures around mortgage payments

4.5.1 Frequency of wage payment

The question at hand is whether a higher frequency of pay is associated with better consumption smoothing. Do households with higher frequencies of pay have different consumption profiles? There are no observations of households changing pay frequency in the sample. Even with such changes, one might be worried about endogeneity. For example, workers with lower levels of self-control could sort themselves into occupations or jobs with higher frequency of pay. Table 4.3 presents the results of multinomial logit estimations of pay frequency on observable characteristics. Weekly paid are observationally somewhat different from biweekly and monthly paid, especially in education level. Biweekly, semi-monthly and monthly paid households are rather similar in observable characteristics. However, even if there is sorting of workers with a higher desire for immediate gratification into jobs with higher pay frequencies, the frequency of rent and mortgage payment remains the same for almost everybody. Almost all US households have to pay rent or mortgage once a month. Since the group of semi-monthly paid is relatively small, I focus in the remainder of the chapter on weekly, biweekly and monthly paid households.

Table 4.4 shows the results for all households with one or two wage-earners and rent or mortgage payments. For dual wage-earners the frequency of the paycheck is determined by the frequency of the largest paycheck. The bottom panel of the table shows the results for households with only one wage-earner. Starting with the top panel, there is the same pattern over all consumption expenditures, and over all frequencies of pay. Average weekly expenditures are negative two weeks before the rent/mortgage is paid (first row). The week before rent/mortgage is paid is the reference week. Consumption expenditures are positive in the first and second after rent and mortgage are paid. There is a pronounced cycle around the payment of rent and mortgage. Coefficients are precisely estimated, and the hypothesis that the three dummies around the payment of rent/mortgage are equal to zero is usually rejected at 1% (5% for food expenditures of the monthly paid, and 11% for food expenditures of the biweekly paid). There are no patterns visible in the four dummies that capture effects of the weeks of the calendar month.

The mean of the dependent variable is the average daily expenditures of a certain category. The weekly paid households spend on average $3.6 less two weeks before they pay their rent/mortgage, compared to the week before. Weekly paid households spend on average $4.2 more in the first week after rent/mortgage are paid, and $5.6 more in the second week. These are total expenditures (excluding rent/mortgage payments). The next two columns show how the total expenditures are divided over food expenditures

Table 4.3: Characteristics of pay groups compared

	WK vs. M		BW vs. M		SM vs. M		WK vs. SM		BW vs. SM		WK vs. BW	
Budget share rent/mortgage	-0.793	*	-0.527		-0.288		-0.505		-0.239		-0.265	
Rented	0.196		0.166		0.106		0.090		0.060		0.030	
Male	0.448	***	0.067		0.231		0.217		-0.163		0.381	***
Age up to 25	0.313		0.145		-0.077		0.390		0.222		0.168	
Age $40-49$	0.206		0.085		0.050		0.155		0.035		0.120	
Age $50-64$	-0.084		-0.118		-0.161		0.078		0.043		0.035	
Age 65 and over	-0.669	*	-0.367		-1.135	*	0.467		0.768		-0.302	
Highschool	-0.520	*	-0.049		-0.345		-0.175		0.296		-0.471	***
Some college	-1.043	***	-0.214		-0.169		-0.874	***	-0.045		-0.829	***
College	-2.230	***	-0.776	***	-0.570		-1.660	***	-0.205		-1.455	***
Urban area	-0.052		0.153		0.322		-0.374		-0.168		-0.205	
No. household members	0.122	***	0.021		-0.036		0.158	***	0.057		0.101	***
White	0.699	***	0.332	*	0.174		0.525	*	0.158		0.367	**
Black	0.716	**	0.763	***	-0.039		0.755	*	0.801	**	-0.047	
HH income less than $\$5,000$	0.404		-0.547		0.430		-0.025		-0.976		0.951	***
HH income $\$5,000-\$9,999$	0.358		0.014		0.058		0.300		-0.044		0.343	
HH income $\$10,000-\$14,999$	0.286		0.003		-0.211		0.497		0.214		0.283	*
HH income $\$15,000-\$19,999$	-0.055		-0.339		-0.072		0.017		-0.267		0.284	*
HH income $\$30,000-\$39,999$	0.016		0.003		0.083		-0.067		-0.080		0.013	
HH income $\$40,000-\$49,999$	-0.266		-0.081		-0.105		-0.160		0.025		-0.185	
HH income $\$50,000-\$69,999$	-0.210		0.012		0.174		-0.383		-0.161		-0.222	*
HH income $\$70,000$ and more	-0.289		0.288		0.351		-0.639	**	-0.063		-0.576	***
Constant	1.597	***	1.837	***	-0.850		2.446	***	2.686	***	-0.240	
N	8,768											
Log Likelihood	-8347.06											

Multinomial logit estimates of frequency of wage pay on observable characteristics. Only households with observed rent and mortgage payments are included. WK is weekly, BW is biweekly, SM is semi-monthly and M is monthly. In the first three columns monthly paid households are the reference category. In the fourth and the fifth column the reference category is semi-monthly paid, and in the last column it is the biweekly paid. Baseline categories are "House-owners with mortgage" for housing tenure, "Age $26-39$" for age groups, "Less than highschool" for education, "Other race" for race and "HH income $\$20,000-\$29,999$" for household income groups. Standard errors are suppressed to facilitate readability. */**/*** correspond to 10%/5%/1% significance level.

and non-food expenditures – where in the last one most of the goods are nondurables. Since total expenditures can be zero on a day, there is a row included with fraction non-zero expenditures. Usually for total expenditures the fraction of days with zero reported expenditures is low - which justifies the use of OLS.

The bottom panel shows the results for households with only one wage-earner. This is a robustness check to see how sensitive the results are for the inclusion of households with two wage-earners with different pay frequencies. The total number of observations drops, but the results are qualitatively similar to the top panel. Dummies for the weeks of the calendar month are included, but not reported.

To sum up: there is a cycle in different expenditure categories around the payment of rent and mortgage. The pattern of the cycle is similar for different frequencies. The size of the cycle differs a little between different frequencies of pay. Overall there is no evidence for smoothing of consumption expenditures.

Tables 4.5–4.9 show different splits of the data, to check the robustness of the result. Table 4.5 splits the sample by housing tenure: renters (top panel) and homeowners (bottom panel). The regression results are similar to the results shown in the figures before. All regressions contain controls for the weeks of the calendar months, but these are suppressed for expositional purposes. Food expenditures are smoothed relatively well, except for weekly paid renters and monthly paid homeowners. There seems to be some evidence that the overall cycle in total expenditures is relatively more driven by the category other expenditures. Finally it is interesting to note that although there is a correlation between frequency of pay and housing tenure, this does not seem to affect the overall finding of a cycle in expenditures.

Table 4.6 splits the sample by education. The top panel are the households of which the head of the household has at most some college, but no college degree. The bottom panel is for those households where the household head has at least a college degree. There is a clear correlation between education and frequency of pay. Biweekly and monthly paid households are higher educated. Still, there is a cycle in consumption expenditures regardless of education, with two exceptions. Biweekly paid households both with much and less education smooth food expenditures, but not other non-durable expenditures. The other exception is monthly paid households of which the household head has less than a college degree – there are no statistically significant coefficients, though the signs in the first two rows are right. This is most likely due to the low sample size.

To test for liquidity constraints, the sample is usually split by household income (Zeldes,

1989). For each pay frequency the sample is separately split by the lowest and the highest income quartile. Income is disposable income – household income after taxes. The top panel of table 4.7 shows the households in the lowest income quartile, and the bottom panel for the highest income quartile. Sample sizes are low, but some patterns can still be distinguished. Comparing the top panel with the bottom panel, it does not seem to appear that households in the highest income quartile are better able to smooth consumption expenditures. Comparing the high wage frequency (weekly) with the lower wage frequency (monthly), it does not seem that the pattern before and after rent/mortgage payments are very different.

Two other robustness checks are presented in Tables 4.8–4.9. Table 4.8 splits the sample in years before the economic crisis (top panel), and years during the economic crisis (bottom panel). The patterns are similar to the evidence show before: negative and significant coefficients two weeks before rent and mortgage payments, positive and significant in the week after payment. However food expenditures are better smoothed by all wage frequencies in the period $2005 - 2010$, which is counterintuitive. It is a priori not clear why households would be better able to smooth food expenditures in the years of the crisis. A second observation is that in the latter years the peak of the cycle seems to be concentrated in the week the rent/mortgage payment is made.

Table 4.9 splits the sample in households that bought cigarettes and tobacco in their diary period, and households that did not. The idea is that smoking and time-inconsistency are related – although Khwaja et al. (2007) do not find direct evidence for this. There are no clear differences between smokers and nonsmokers – both groups more or less exhibit the same pattern. If there is a correlation between time inconsistency and smoking, then this table does not suggest that the cycle is driven by time inconsistent households.

Table 4.4: Wage-earners: other expenditures around payment of rent and mortgage

	Weekly paid			Biweekly paid			Monthly paid		
Single and dual wage earners	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent/mortgage paid	-3.627^{***}	-2.110^{**}	-1.517^{**}	-2.688^{***}	-0.842	-1.846^{***}	-7.018^{***}	-2.631^{*}	-4.388^{***}
	(1.16)	(0.84)	(0.75)	(0.83)	(0.56)	(0.55)	(2.13)	(1.49)	(1.32)
$0-6$ days after rent/mortgage paid	4.248^{***}	2.218^{***}	2.031^{***}	4.032^{***}	0.978^{**}	3.054^{***}	7.376^{***}	3.558^{***}	3.818^{***}
	(0.98)	(0.69)	(0.60)	(0.70)	(0.47)	(0.45)	(1.80)	(1.25)	(1.17)
$7-13$ days after rent/mortgage paid	5.572^{***}	3.074^{**}	2.498^{**}	4.684^{***}	1.177	3.507^{***}	11.041^{***}	6.431^{***}	4.610^{**}
	(1.77)	(1.29)	(1.09)	(1.27)	(0.87)	(0.81)	(3.22)	(2.34)	(2.00)
Week(-2) calendar month	1.604^{**}	1.298^{**}	0.306	-0.320	0.359	-0.679^{*}	-1.112	-0.367	-0.745
	(0.81)	(0.56)	(0.50)	(0.58)	(0.38)	(0.38)	(1.58)	(1.04)	(1.05)
Week$(+1)$ calendar month	0.675	0.091	0.584	-0.243	0.013	-0.256	-1.447	-0.557	-0.890
	(0.70)	(0.49)	(0.44)	(0.50)	(0.33)	(0.34)	(1.25)	(0.84)	(0.85)
Week$(+2)$ calendar month	0.712	0.798	-0.086	-0.338	-0.262	-0.076	-2.646^{*}	-0.845	-1.801^{*}
	(0.84)	(0.61)	(0.51)	(0.61)	(0.40)	(0.41)	(1.52)	(1.03)	(0.99)
Week$(+3)$ calendar month	2.482^{**}	1.297	1.185	-1.145	-0.045	-1.100^{**}	-1.434	0.130	-1.564
	(1.19)	(0.83)	(0.76)	(0.84)	(0.57)	(0.55)	(2.27)	(1.64)	(1.34)
R^2	0.034	0.025	0.017	0.025	0.019	0.011	0.021	0.016	0.013
Mean dependent variable	30.59	17.20	13.39	30.26	16.92	13.35	30.17	17.16	13.01
Fraction non-censored observations	0.77	0.68	0.48	0.77	0.68	0.44	0.76	0.67	0.43
p-value weeks rent/mortgage payment$=0$	0.000	0.003	0.004	0.000	0.110	0.000	0.000	0.036	0.001
Households $\times$ Days		$25,419$			$50,728$			$7,531$	

	Weekly paid			Biweekly paid			Monthly paid		
Single wage earners	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent/mortgage paid	-1.869	-1.352	-0.517	-2.475^{***}	-0.937	-1.538^{**}	-6.017^{***}	-2.258	-3.759^{**}
	(1.36)	(1.00)	(0.85)	(0.95)	(0.64)	(0.64)	(2.28)	(1.66)	(1.59)
$0-6$ days after rent/mortgage paid	4.284^{***}	2.255^{***}	2.029^{***}	4.627^{***}	1.596^{***}	3.031^{***}	6.653^{***}	3.586^{***}	3.066^{**}
	(1.19)	(0.81)	(0.71)	(0.81)	(0.54)	(0.53)	(2.02)	(1.38)	(1.35)
$7-13$ days after rent/mortgage paid	5.725^{***}	3.230^{**}	2.495^{*}	5.628^{***}	1.910^{*}	3.718^{***}	9.935^{***}	5.619^{**}	4.316^{*}
	(2.11)	(1.49)	(1.29)	(1.44)	(0.98)	(0.93)	(3.52)	(2.56)	(2.25)
R^2	0.036	0.024	0.020	0.027	0.019	0.013	0.018	0.014	0.010
Mean dependent variable	27.29	15.37	11.92	26.45	14.64	11.81	27.19	15.54	11.65
Fraction non-censored observations	0.75	0.65	0.45	0.74	0.65	0.42	0.74	0.65	0.42
p-value weeks rent/mortgage payment$=0$	0.003	0.042	0.027	0.000	0.016	0.000	0.006	0.076	0.045
Households $\times$ Days		$16,859$			$33,404$			$5,147$	

Each column is a regression with household fixed effects. All regressions contain controls for day of the survey and day of the week. The week before payment of mortgage and rent is the reference week. The reference week for the weeks of the calendar month is the week before the first of the month. Week$(+3)$ is not a full week, but contains in between 0 and 3 days depending on the total number of days of the month. Weeks of the month are suppressed in the bottom panel. Standard errors are clustered at the household level. */**/*** correspond to 10%/5%/1% significance level.

Table 4.5: Wage-earners: other expenditures around payment of rent and mortgage split by housing tenure

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: renters	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent paid	-4.086^{**}	-3.062^{***}	-1.024	-3.151^{***}	-1.227	-1.924^{**}	-8.322^{***}	-2.979	-5.343^{***}
	(1.58)	(1.11)	(0.94)	(1.19)	(0.79)	(0.79)	(2.90)	(1.96)	(1.89)
$0-6$ days after rent paid	4.929^{***}	2.726^{***}	2.204^{***}	4.628^{***}	0.905	3.722^{***}	4.843^{*}	0.786	4.057^{**}
	(1.37)	(0.92)	(0.82)	(1.00)	(0.69)	(0.64)	(2.55)	(1.80)	(1.70)
$7-13$ days after rent paid	6.069^{**}	3.435^{**}	2.634^{*}	7.401^{***}	1.531	5.870^{***}	9.090^{*}	2.737	6.353^{**}
	(2.42)	(1.64)	(1.50)	(1.76)	(1.22)	(1.14)	(4.78)	(3.47)	(2.87)
R^2	0.035	0.023	0.019	0.029	0.023	0.013	0.029	0.024	0.014
Mean dependent variable	26.69	15.06	11.63	25.06	13.87	11.19	24.66	14.10	10.56
Fraction non-censored observations	0.76	0.66	0.46	0.74	0.64	0.43	0.73	0.63	0.41
p-value weeks rent payment$=0$	0.001	0.003	0.040	0.000	0.412	0.000	0.032	0.477	0.019
Households $\times$ Days		$12,391$			$20,567$			$2,929$	

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: homeowners	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before mortgage paid	-3.383^{**}	-1.409	-1.974^{*}	-2.471^{**}	-0.679	-1.792^{**}	-6.055^{**}	-2.109	-3.946^{**}
	(1.71)	(1.25)	(1.17)	(1.13)	(0.76)	(0.74)	(2.99)	(2.10)	(1.82)
$0-6$ days after mortgage paid	3.677^{***}	1.720^{*}	1.958^{**}	3.733^{***}	1.000	2.733^{***}	8.442^{***}	5.113^{***}	3.328^{**}
	(1.39)	(1.01)	(0.88)	(0.95)	(0.63)	(0.63)	(2.46)	(1.68)	(1.61)
$7-13$ days after mortgage paid	5.001^{*}	2.650	2.351	2.947^{*}	0.881	2.066^{*}	11.904^{***}	8.618^{***}	3.286
	(2.56)	(1.95)	(1.59)	(1.76)	(1.20)	(1.12)	(4.34)	(3.15)	(2.74)
R^2	0.036	0.027	0.017	0.024	0.018	0.012	0.022	0.017	0.016
Mean dependent variable	34.29	19.23	15.06	33.82	19.00	14.81	33.68	19.10	14.57
Fraction non-censored observations	0.79	0.70	0.49	0.78	0.70	0.45	0.78	0.70	0.44
p-value weeks mortgage payment$=0$	0.032	0.341	0.087	0.000	0.215	0.000	0.007	0.024	0.029
Households $\times$ Days		$13,028$			$30,161$			$4,602$	

Each column is a regression with household fixed effects. The week before payment of mortgage and rent is the reference week. All regressions contain controls for day of the survey, day of the week, and weeks of the calendar month. Standard errors are clustered at the household level. */**/*** correspond to 10%/5%/1% significance level.

Table 4.6: Wage-earners: other expenditures around payment of rent and mortgage split by education

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: some college and less	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent/mortgage paid	-3.089^{**}	-2.906^{**}	-0.183	-4.372^{***}	-1.062	-3.311^{***}	-1.309	-0.557	-0.752
	(1.57)	(1.14)	(0.96)	(1.49)	(1.05)	(0.94)	(4.33)	(2.68)	(3.19)
$0-6$ days after rent/mortgage paid	3.038^{**}	1.679^{*}	1.358^{*}	5.091^{***}	1.534^{*}	3.557^{***}	2.146	1.379	0.767
	(1.36)	(0.96)	(0.82)	(1.31)	(0.92)	(0.83)	(3.96)	(2.54)	(2.72)
$7-13$ days after rent/mortgage paid	2.416	1.826	0.591	6.506^{***}	2.195	4.311^{***}	-0.441	-0.186	-0.254
	(2.45)	(1.82)	(1.48)	(2.38)	(1.72)	(1.48)	(7.38)	(4.51)	(5.25)
R^2	0.039	0.028	0.020	0.029	0.022	0.015	0.028	0.020	0.031
Mean dependent variable	30.21	17.03	13.18	28.33	15.71	12.62	28.53	16.15	12.38
Fraction non-censored observations	0.76	0.66	0.48	0.75	0.65	0.45	0.73	0.62	0.45
p-value weeks rent/mortgage payment= 0	0.011	0.017	0.097	0.000	0.382	0.000	0.668	0.732	0.911
Households × Days		12,795			13,787			1,519	

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: college and higher	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent/mortgage paid	-4.212^{**}	-1.333	-2.880^{**}	-2.046^{**}	-0.733	-1.313^{**}	-8.205^{***}	-3.082^{*}	-5.123^{***}
	(1.71)	(1.23)	(1.15)	(0.99)	(0.66)	(0.66)	(2.44)	(1.73)	(1.45)
$0-6$ days after rent/mortgage paid	5.317^{***}	2.615^{***}	2.702^{***}	3.651^{***}	0.774	2.877^{***}	8.528^{***}	4.048^{***}	4.480^{***}
	(1.41)	(0.98)	(0.89)	(0.82)	(0.54)	(0.54)	(2.02)	(1.43)	(1.31)
$7-13$ days after rent/mortgage paid	8.581^{***}	4.198^{**}	4.383^{***}	3.947^{***}	0.772	3.175^{***}	13.485^{***}	7.918^{***}	5.567^{***}
	(2.55)	(1.83)	(1.61)	(1.50)	(1.01)	(0.97)	(3.58)	(2.71)	(2.14)
R^2	0.031	0.023	0.016	0.023	0.018	0.011	0.019	0.015	0.010
Mean dependent variable	30.98	17.37	13.61	30.99	17.37	13.62	30.59	17.41	13.17
Fraction non-censored observations	0.78	0.69	0.47	0.77	0.69	0.44	0.77	0.69	0.42
p-value weeks rent/mortgage payment= 0	0.002	0.068	0.012	0.000	0.314	0.000	0.000	0.031	0.000
Households × Days		12,624			36,941			6,012	

Each column is a regression with household fixed effects. The week before payment of mortgage and rent is the reference week. All regressions contain controls for day of the survey, day of the week, and weeks of the calendar month. Standard errors are clustered at the household level. */**/*** correspond to 10%/5%/1% significance level.

Table 4.7: Wage-earners: other expenditures around payment of rent and mortgage split by household income

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: lowest income quartile	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent/mortgage paid	-2.739	-3.050^{**}	0.311	-4.370^{***}	-1.051	-3.319^{***}	-5.008	-2.576	-2.432
	(2.07)	(1.54)	(1.33)	(1.41)	(0.92)	(0.94)	(3.31)	(2.55)	(1.81)
$0-6$ days after rent/mortgage paid	4.198^{**}	2.175^{*}	2.024^{*}	4.759^{***}	1.379^{*}	3.380^{***}	5.326	1.598	3.728^{*}
	(1.94)	(1.30)	(1.12)	(1.20)	(0.82)	(0.76)	(3.45)	(2.36)	(2.10)
$7-13$ days after rent/mortgage paid	2.729	2.604	0.125	6.924^{***}	2.005	4.919^{***}	6.719	0.546	6.173^{*}
	(3.51)	(2.32)	(2.24)	(2.16)	(1.45)	(1.37)	(6.00)	(4.40)	(3.37)
R^2	0.043	0.027	0.027	0.031	0.021	0.017	0.029	0.022	0.021
Mean dependent variable	23.83	13.25	10.57	22.00	12.06	9.94	21.17	12.42	8.76
Fraction non-censored observations	0.71	0.60	0.44	0.71	0.59	0.41	0.68	0.58	0.39
p-value weeks rent/mortgage payment= 0	0.011	0.101	0.003	0.000	0.382	0.000	0.264	0.442	0.304
Households × Days		$6,219$			$12,344$			$1,818$	

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: highest income quartile	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent/mortgage paid	-6.751^{***}	-3.358^{*}	-3.393^{*}	-3.535^{*}	-1.321	-2.213^{*}	-10.477^{**}	-1.315	-9.162^{***}
	(2.52)	(1.81)	(1.76)	(1.91)	(1.27)	(1.24)	(4.90)	(3.60)	(2.48)
$0-6$ days after rent/mortgage paid	6.304^{***}	3.653^{**}	2.651^{*}	2.728^{*}	0.126	2.601^{**}	9.040^{**}	4.942^{*}	4.098^{*}
	(2.11)	(1.53)	(1.37)	(1.53)	(1.02)	(1.04)	(3.91)	(2.76)	(2.46)
$7-13$ days after rent/mortgage paid	10.217^{***}	7.454^{**}	2.763	2.621	-0.109	2.730	15.618^{**}	10.228^{**}	5.390
	(3.85)	(2.94)	(2.41)	(2.88)	(1.97)	(1.88)	(6.82)	(5.10)	(4.20)
R^2	0.029	0.025	0.015	0.022	0.019	0.009	0.027	0.026	0.017
Mean dependent variable	39.23	22.11	17.12	39.67	22.47	17.19	38.21	22.15	16.06
Fraction non-censored observations	0.81	0.73	0.51	0.81	0.74	0.48	0.81	0.73	0.45
p-value weeks rent/mortgage payment= 0	0.009	0.076	0.083	0.078	0.652	0.031	0.094	0.162	0.002
Households × Days		$6,456$			$13,055$			$1,967$	

Each column is a regression with household fixed effects. The week before payment of mortgage and rent is the reference week. All regressions contain controls for day of the survey, day of the week, and weeks of the calendar month. Standard errors are clustered at the household level. */**/*** correspond to 10%/5%/1% significance level.

Table 4.8: Wage-earners: other expenditures around payment of rent and mortgage split by year

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: 1998 − 2004	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
14 − 8 days before rent/mortgage paid	−3.300**	−2.292*	−1.009	−3.485***	−1.667**	−1.818**	−5.316*	−2.183	−3.133*
	(1.62)	(1.22)	(1.04)	(1.17)	(0.82)	(0.73)	(3.00)	(2.11)	(1.83)
0 − 6 days after rent/mortgage paid	5.142***	2.592***	2.549***	5.867***	2.141***	3.726***	10.625***	5.454***	5.171***
	(1.39)	(0.99)	(0.81)	(0.99)	(0.69)	(0.63)	(2.53)	(1.73)	(1.60)
7 − 13 days after rent/mortgage paid	7.609***	4.065**	3.543**	7.098***	2.381*	4.717***	15.002***	8.860***	6.142**
	(2.54)	(1.88)	(1.45)	(1.79)	(1.27)	(1.13)	(4.36)	(3.17)	(2.61)
R^2	0.036	0.027	0.017	0.022	0.019	0.009	0.025	0.020	0.014
Mean dependent variable	30.07	17.42	12.65	29.42	16.93	12.49	28.35	16.66	11.68
Fraction non-censored observations	0.78	0.68	0.48	0.77	0.68	0.45	0.76	0.67	0.42
p-value weeks rent/mortgage payment= 0	0.002	0.042	0.017	0.000	0.003	0.000	0.001	0.018	0.012
Households × Days		13,460			25,835			3,824	

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: 2005 − 2010	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
14 − 8 days before rent/mortgage paid	−3.968**	−1.917*	−2.051*	−1.851	0.052	−1.902**	−8.451***	−2.994	−5.457***
	(1.68)	(1.16)	(1.09)	(1.17)	(0.76)	(0.81)	(3.01)	(2.07)	(1.92)
0 − 6 days after rent/mortgage paid	3.285**	1.829*	1.456	2.157**	−0.203	2.360***	4.409*	2.009	2.400
	(1.38)	(0.94)	(0.90)	(0.98)	(0.63)	(0.66)	(2.56)	(1.80)	(1.74)
7 − 13 days after rent/mortgage paid	3.541	2.171	1.370	2.131	−0.048	2.179*	6.925	4.173	2.751
	(2.44)	(1.73)	(1.65)	(1.79)	(1.18)	(1.17)	(4.77)	(3.45)	(3.09)
R^2	0.033	0.022	0.019	0.027	0.020	0.014	0.023	0.016	0.017
Mean dependent variable	31.17	16.95	14.22	31.14	16.91	14.23	32.06	17.67	14.38
Fraction non-censored observations	0.77	0.67	0.47	0.76	0.67	0.44	0.77	0.67	0.44
p-value weeks rent/mortgage payment= 0	0.018	0.116	0.088	0.051	0.954	0.000	0.035	0.500	0.022
Households × Days		11,959			24,893			3,707	

Each column is a regression with household fixed effects. The week before payment of mortgage and rent is the reference week. All regressions contain controls for day of the survey, day of the week, and weeks of the calendar month. Standard errors are clustered at the household level. */**/*** correspond to 10%/5%/1% significance level.

Table 4.9: Wage-earners: other expenditures around payment of rent and mortgage split by smoker/non-smoker

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: smokers	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent/mortgage paid	-4.661^{**}	-2.464^{*}	-2.197^{*}	-3.392^{**}	-1.659	-1.734	-5.706	-6.011^{*}	0.305
	(1.95)	(1.38)	(1.27)	(1.66)	(1.05)	(1.18)	(4.08)	(3.08)	(2.78)
$0-6$ days after rent/mortgage paid	2.941^{*}	1.614	1.327	5.389^{***}	1.263	4.125^{***}	1.839	-1.207	3.046
	(1.67)	(1.12)	(1.02)	(1.32)	(0.90)	(0.89)	(4.76)	(3.18)	(3.34)
$7-13$ days after rent/mortgage paid	3.780	1.466	2.314	6.578^{***}	1.462	5.117^{***}	2.598	-0.159	2.756
	(2.96)	(2.10)	(1.86)	(2.49)	(1.72)	(1.65)	(8.48)	(5.58)	(5.71)
R^2	0.041	0.025	0.028	0.028	0.019	0.017	0.054	0.041	0.044
Mean dependent variable	32.68	16.92	15.76	32.50	16.40	16.09	32.39	16.74	15.65
Fraction non-censored observations	0.82	0.69	0.59	0.80	0.69	0.56	0.81	0.69	0.54
p-value weeks rent/mortgage payment$=0$	0.058	0.092	0.318	0.000	0.205	0.000	0.478	0.195	0.727
Households $\times$ Days		$9,308$			$13,829$			$1,377$	

	Weekly paid			Biweekly paid			Monthly paid		
All wage-earners: non-smokers	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent/mortgage paid	-3.186^{**}	-1.984^{*}	-1.202	-2.413^{**}	-0.555	-1.858^{***}	-7.203^{***}	-1.605	-5.597^{***}
	(1.44)	(1.06)	(0.92)	(0.95)	(0.66)	(0.61)	(2.50)	(1.69)	(1.51)
$0-6$ days after rent/mortgage paid	5.086^{***}	2.612^{***}	2.474^{***}	3.515^{***}	0.874	2.641^{***}	8.616^{***}	4.518^{***}	4.098^{***}
	(1.20)	(0.86)	(0.75)	(0.82)	(0.55)	(0.53)	(1.94)	(1.37)	(1.24)
$7-13$ days after rent/mortgage paid	6.798^{***}	4.110^{**}	2.689^{**}	3.952^{***}	1.081	2.871^{***}	12.872^{***}	7.745^{***}	5.128^{**}
	(2.20)	(1.63)	(1.35)	(1.47)	(1.01)	(0.93)	(3.49)	(2.62)	(2.11)
R^2	0.032	0.026	0.013	0.024	0.020	0.010	0.017	0.015	0.010
Mean dependent variable	29.38	17.36	12.02	29.43	17.11	12.31	29.68	17.25	12.42
Fraction non-censored observations	0.75	0.67	0.41	0.75	0.67	0.40	0.75	0.67	0.40
p-value weeks rent/mortgage payment$=0$	0.000	0.020	0.005	0.000	0.401	0.000	0.000	0.009	0.000
Households $\times$ Days		$16,111$			$36,899$			$6,154$	

Each column is a regression with household fixed effects. The week before payment of mortgage and rent is the reference week. All regressions contain controls for day of the survey, day of the week, and weeks of the calendar month. Standard errors are clustered at the household level. */**/*** correspond to 10%/5%/1% significance level.

4.5.2 Social Security recipients: still a cycle

After having shown evidence for wage-earners, I now revisit the results of Stephens (2003), Mastrobuoni and Weinberg (2011), and Evans and Moore (2012) for Social Security recipients. Stephens finds that Social Security recipients increase their expenditures after the payment of Social Security. Evans and Moore find qualitatively the same for later years. Mastrobuoni and Weinberg do not find a cycle after a change in the distribution scheme. I propose an alternative explanation: the cycle is driven by the payment of rent and mortgages. I start with replicating the findings of the other studies around the first of the month. Table 4.10 shows the results.

The first three columns show all Social Security recipients. There appears to be a cycle for all categories around the first of the calendar month. These results are very similar to the ones Evans and Moore (2012) find. The coefficients in the first three columns are jointly significant at $1-6\%$. I split the entire sample in two subsamples: Social Security recipients with mortgage and rent obligations (middle three columns), and those without (last three columns). Looking at the coefficient on the first week of the month ($Week(+1)$ calendar month) reveals that the cycle around the first of the month is entirely driven by households with current rent and mortgage obligations. There is no discernable cycle for Social Security recipients with no housing payments – the final three columns of the table. Some coefficients are individually significant, but there is no cycle discernable. When we look at the p-values of the test of joint significance, we see that the null hypothesis of no joint significance cannot be rejected in the last three columns. This finding is not the result of lack of power – the subgroup with housing payments contains about 20% of the observations, versus 80% for the latter group. Average consumption expenditures between the two groups are also very similar.

With this result in mind, I take the window around payment of mortgage and rent for the subgroup of Social Security recipients with rent/mortgage payments. The frequency distribution of rent and mortgage payments over the calendar month of Social Security recipients looks very similar to entire sample, with one interesting exception. For renters the second spike is at the third of the month, which suggests that Social Security recipients time the payment of rent to coincide with check arrival, since many Social Security checks are distributed at the third of the month. Also for the Social Security recipients there is quite some variation in paydates over the month.

Table 4.10: Social Security recipients with and without rent/mortgage payments: other expenditures around the first of the calendar month

	All Social Security recipients			With rent or mortgage obligations			No rent or mortgage obligations		
	Total exp.	Food	Non-food	Total exp.	Food	Non-food	Total exp.	Food	Non-food
Week(−2) calendar month	0.379	0.454**	−0.075	0.085	0.608	−0.524	0.378	0.390*	−0.012
	(0.29)	(0.20)	(0.19)	(0.74)	(0.53)	(0.46)	(0.32)	(0.22)	(0.20)
Week(+1) calendar month	1.064***	0.607***	0.457**	2.723***	1.204***	1.520***	0.623*	0.462**	0.161
	(0.30)	(0.21)	(0.19)	(0.63)	(0.45)	(0.39)	(0.34)	(0.23)	(0.22)
Week(+2) calendar month	0.706**	0.536**	0.170	2.401***	1.554***	0.847*	0.374	0.303	0.070
	(0.34)	(0.24)	(0.22)	(0.82)	(0.56)	(0.50)	(0.38)	(0.26)	(0.24)
Week(+3) calendar month	0.147	0.321	−0.174	0.793	0.780	0.013	0.031	0.216	−0.185
	(0.43)	(0.30)	(0.27)	(1.14)	(0.83)	(0.69)	(0.47)	(0.33)	(0.29)
R^2	0.030	0.019	0.016	0.036	0.022	0.020	0.029	0.019	0.015
Mean dependent variable	19.87	12.44	7.43	19.29	11.86	7.43	20.02	12.59	7.43
Fraction non-censored observations	0.60	0.52	0.30	0.60	0.52	0.32	0.60	0.52	0.30
p-value calendar week dummies= 0	0.007	0.029	0.053	0.000	0.056	0.000	0.324	0.226	0.807

Each column is a regression with household fixed effects. All regressions contain controls for day of the survey and day of the week. The week before the first of the month is the reference month. Week(+3) is not a full week, but contains in between 0 and 3 days depending on the total number of days of the month. Standard errors are clustered at the household level. The first three columns have 99,607 observations, the second three 19,765, and the last three columns have 79,842 observations. */**/*** correspond to 10%/5%/1% significance level.

Figure 4.4: Frequency distribution over the month for Social Security recipients

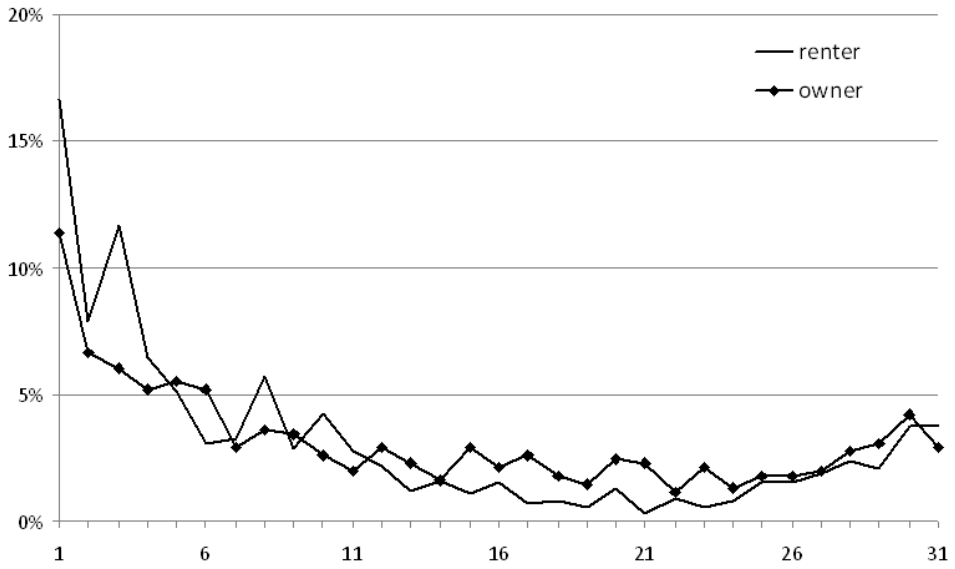


Table 4.11: Social Security recipients with rent/mortgage payments

	Total exp.	Food	Non-food	Total exp.	Food	Non-food
$14-8$ days before rent/mortgage paid (δ_{-2})	-3.155^{***}	-1.621^{**}	-1.534^{**}	-2.934^{***}	-1.728^{**}	-1.206^{*}
	(1.10)	(0.80)	(0.66)	(1.12)	(0.83)	(0.67)
$0-6$ days after rent/mortgage paid (δ_{+1})	6.350^{***}	3.503^{***}	2.847^{***}	5.866^{***}	3.276^{***}	2.590^{***}
	(0.93)	(0.67)	(0.56)	(0.95)	(0.68)	(0.56)
$7-13$ days after rent/mortgage paid (δ_{+2})	9.026^{***}	5.564^{***}	3.462^{***}	8.969^{***}	5.416^{***}	3.552^{***}
	(1.60)	(1.19)	(0.96)	(1.62)	(1.20)	(0.96)
Week(-2) calendar month (γ_{-2})				-0.153	0.450	-0.604
				(0.77)	(0.57)	(0.47)
Week$(+1)$ calendar month (γ_{+1})				1.806^{***}	0.772	1.035^{**}
				(0.67)	(0.48)	(0.42)
Week$(+2)$ calendar month (γ_{+2})				1.561^{*}	1.114^{*}	0.447
				(0.84)	(0.57)	(0.51)
Week$(+3)$ calendar month (γ_{+3})				0.277	0.487	-0.211
				(1.18)	(0.85)	(0.70)
Adjusted R^2	0.038	0.024	0.021	0.038	0.024	0.022
Mean dependent variable	19.29	11.86	7.43	19.29	11.86	7.43
Fraction non-censored observations	0.60	0.52	0.32	0.60	0.52	0.32
p-value: $\delta_{-2}=\delta_{+1}=\delta_{+2}=0$	0.000	0.000	0.000	0.000	0.000	0.000
p-value: $\gamma_{-2}=\gamma_{+1}=\gamma_{+2}=\gamma_{+3}=0$				0.064	0.378	0.033

Each column is a regression with household fixed effects. All regressions have controls for day of the survey and day of the week. The week before payment of mortgage and rent is the reference week. Standard errors are clustered at the household level. $N = 19,765$. */**/*** correspond to 10%/5%/1% significance level.

The results are in Table 4.11. Only Social Security recipients with mortgage and rent payments are included. There is a large cycle in expenditures around the payment of rent and mortgage. The bottom-to-peak is large, for example about $12 for total expenditures on a daily average of $19. The size of the cycle is a magnitude larger compared to previous studies (Stephens, 2003; Mastrobuoni and Weinberg, 2011; and Evans and Moore, 2012). Including dummies for the weeks around the first of the calendar month does not change the results much. The coefficient on the first of the calendar month of total and non-food expenditures is significant and positive, but this is not as large as the effect on the week after payment of rent and mortgage. The two cycles do more or less overlap for some Social Security recipients, but not for all. This could explain why the 2011–study by Mastrobuoni and Weinberg does not find a cycle after the paycheck schedule was changed. Previous studies ascribe the cycle in consumption expenditures to income receipt. If this were the case, there would be a cycle for Social Security recipients with no rent or mortgage payments in Table 4.10 as well. However, the cycle is driven by the Social Security recipients with mortgage/rent obligations.

4.5.3 Discussion of alternative explanations

The overall picture is that there is a cycle in expenditures for renters and house-owners with mortgage obligations. For renters the peak-to-through is about $10 on a daily average of total expenditures of $26, and for house-owners $9 on $35. The same qualitative cycle is found for weekly, biweekly and monthly paid households who pay their rent/mortgage at the same time. Finally the cycle is found for households with high and low education, and for high and low income samples. Liquidity constraints alone can be ruled out as an explanation, since liquidity constraints would affect the level, but cannot explain a cycle. Hyperbolic discounting is a potential candidate, since hyperbolic discounting is not necessarily confined to poor households. One potential objection to hyperbolic discounting is that the spike in expenditures is concentrated on the day of rent/mortgage payments. Hyperbolic discounting by itself can be used to explain a cycle in expenditures, but not why this cycle should be concentrated around the timing or mortgage/rent payments. Households need to be liquidity constrained. The most promising explanation for both a cycle and the timing is hyperbolic discounting and liquidity concerns. Shortrun household liquidity is a function of income and consumption commitments. The patterns in the data seem to suggest that households postpone other expenditures until they pay their rent or mortgage. Given that most US households still use checks, households could

be uncertain whether they have enough cash in the bank to pay the rent or mortgage. Hyperbolic discounting could be on top of that – since households have a maximum of disposable liquidity after mortgage/rent are paid.

What could be other explanations for the empirical findings? One objection is reporting behavior in the CEX diary survey. Maybe households tend to report all expenditures on the same day. Though this behavior cannot be ruled out, it is unlikely. First of all the number of diary days with positive expenditures is large. For total expenditures between $60-80\%$ of the diary days have positive expenditures. Second, households with all expenditures on the first diary day are excluded on the advice of the Bureau of Labor Statistics. Thirdly, controls for the day of the survey are included, and many respondents report the maximum of 14 diary days.

A second challenge is that consumption expenditures do not necessarily translate into consumption. There can be humps in consumption expenditures, or a shopping cycle, while consumption itself is smooth. However, the studies in the literature section show that the cycle in consumption expenditures coincides with cycles found for other outcomes, including caloric intake, financial crimes and mortality. A cycle in consumption expenditures most likely translates into consumption.

A third problem could be that the paydate of rent and mortgage is endogenous – households choose the most convenient time to pay the rent. This is no problem for the empirical findings as long as rent and mortgage payments are to be paid once a month. If a household is at some discretion to chose the paydate within the month, then there should be no cycle in other expenditures since households can pick the most convenient time. It could be a problem if households can skip a payment, but those households are not observed in the sample since they do not make a payment.

4.6 Model: pay frequency and commitments

In the empirical section I have shown that households with different pay frequencies have similar patterns in consumption expenditures around the paydate of rent or mortgage. This is evidence against the policy recommendation that household welfare can be increased by partitioning paychecks or welfare checks. This policy recommendation is given by Shapiro (2005), Dobkin and Puller (2007), Mastrobuoni and Weinberg (2009), and formally derived in a model with hyperbolic discounting by Parsons and Van Wesep (2013). I will take this policy recommendation at face value, and I will not discuss whether this would be an actual

welfare improvement or not. On the contrary, in a simple model I will show that a higher frequency of income payment still generates an intra-monthly cycle when the payment of consumption commitments is added to the model.

The model has three ingredients: borrowing constraints, hyperbolic discounting and consumption commitments. In the baseline model households are borrowing constrained, but have no consumption commitments. Here increasing pay frequency is a commitment device for households with a self-control problem. This result changes when the model incorporates consumption commitments. Different pay schedules generate different consumption paths as long as the borrowing constraint binds. When the borrowing constraint does not bind, the consumption profiles of weekly, biweekly and monthly paid households are the same in the presence of consumption commitments.

No borrowing constraints, no consumption commitments

Consider a household that wants to optimize consumption C_w in every period w. Assume that utility is time-separable and abstract from interest. The household has a constant relative risk aversion (CRRA) utility function. The household budgets for a month and the lifecycle consists of a cycle of repeated months. A month consists of four periods, say four weeks. Abstract from exponential discounting, which is reasonable over short periods of time such as a month. There are three different frequencies of salary payment: once every four weeks (monthly), once every other week (biweekly) and weekly. Monthly income is paid before week 1, biweekly paychecks are paid before weeks 1 and 3. Weekly paychecks are distributed at the end of the previous week. Total income Y^M over the month is certain and constant, only the frequency of payment can be changed (without costs). Assume that the household is liquidity constrained in the sense that she can save but not borrow, or equivalently that the household is borrowing averse. The household has a self-control problem, which is modeled by quasi-hyperbolic discounting. The maximization problem of the household at the beginning of the month is:

$$\max U = \frac{C_1^{1-\rho}}{1-\rho} + \beta \sum_{w=2}^{4} \frac{C_w^{1-\rho}}{1-\rho} \qquad \text{subject to } \sum_{w=1}^{4} C_w = Y^M. \tag{4.2}$$

The first-order conditions in week 1 are:

$$\frac{C_1}{C_2} = \frac{1}{\beta^{1/\rho}}$$

$$\frac{C_2}{C_3} = \frac{C_3}{C_4} = 1. \tag{4.3}$$

From the first-order conditions it is clear that households with $\beta < 1$ have time-inconsistent consumption plans. After week 1, the household re-optimizes in week 2, and the time subscripts in equation 4.3 shift up one period. There exists a closed form solution for this problem (see the derivation in the Appendix). The closed form solution for monthly paid households is (Mastrobuoni and Weinberg, 2009, Parsons and Van Wesep, 2013):

$$C_1 = \frac{1}{1 + 3\beta^{\frac{1}{\rho}}} Y^M, \text{for w= 1}$$

$$C_w = \frac{\prod_{s=1}^{w}(4-s)\beta^{\frac{w-1}{\rho}}}{\prod_{s=1}^{w}[1+\beta^{\frac{1}{\rho}}(4-s)]} Y^M \text{for w} \in \{2,3,4\}. \tag{4.4}$$

For $\beta < 1$ the household shifts consumption to earlier weeks at the expense of later weeks. There is a cycle in consumption. In the absence of borrowing constraints the closed form solutions for weekly and biweekly paid households are the same as for the monthly paid household. Households can borrow against future paychecks – since I assume that the costs of borrowing are zero – and mimic any consumption plan. These results are derived under the assumption that households are not aware of their inconsistent planning. Households are "naive". If households were aware of their inconsistency, the result is an upper bound in the case of $\rho > 1$ and identical with the naive case for $\rho = 1$. See Tobacman (2007) for a derivation of the Euler equation with partial naiveté. In the remainder of this chapter I assume that households are not aware of their self-control problem, and are naive.

Borrowing constraints, no consumption commitments

If households are borrowing constrained, the consumption profile changes for higher frequencies of pay. The monthly paid household has the same consumption profile as in equation (4.4). Biweekly paid households receive a paycheck at the beginning of week 1 and the beginning of week 3. For any $\beta < 1$ the household in the first two weeks would like to borrow against the second paycheck and shift consumption forward. Total wage income (Y^M) is paid in two equal installments $Y^{BW} = \frac{1}{2}Y^M$. In the first two weeks the household would like to mimic the consumption profile of the monthly paid household.

Define consumption in the first two weeks, using the result from equation (4.4):

$$C_{12} = C_1 + C_2 = \frac{1}{1+3\beta^{\frac{1}{\rho}}} Y^M + \frac{3\beta^{\frac{1}{\rho}}}{1+3\beta^{\frac{1}{\rho}}} \frac{1}{1+2\beta^{\frac{1}{\rho}}} Y^M = \lambda Y^M. \tag{4.5}$$

Take for simplicity that $\rho = 1$, then λY^M is equal to $\frac{1+5\beta}{(1+3\beta)(1+2\beta)} Y^M$. For $\beta = 1$ we have exactly $\lambda Y^M = \frac{1}{2} Y^M = Y^{BW}$. Any $\beta < 1$ implies that $\lambda Y^M > Y^{BW}$, in which case the borrowing constraint binds. With $\beta < 1$ the effective horizon of the biweekly paid household is shortened to two weeks. The closed form solution for biweekly paid households consists of two parts:

$$C_w = \frac{1}{1+\beta^{\frac{1}{\rho}}} Y^{BW} \qquad \text{for w=1 and w=3.}$$

$$C_w = \frac{\beta^{\frac{1}{\rho}}}{1+\beta^{\frac{1}{\rho}}} Y^{BW} \qquad \text{for w=2 and w=4.} \tag{4.6}$$

The same reasoning applies to weekly paid households. The borrowing constraint binds in every week for any $\beta < 1$, so the household sets $C_w = Y^W = \frac{1}{4} Y^M$ for every w. Figure 4.5 shows the evolution of consumption for different pay frequencies, with $\rho = 1$ and $\beta = 0.7$. If all wages of the monthly paid household are paid before the first week, then there is a fall in consumption over the course of the month. With $\beta = 0.7$ The household consumes 32% of total salary in week 1 and 16% in week 4. The fall in consumption is deeper when β is smaller. When the household is paid biweekly, there is a jigsaw pattern – consumption falls in between paydates. When the household is paid every week, consumption is perfectly smoothed. The household would like to consume more in earlier periods, but she cannot borrow, and so she sets consumption equal to the weekly paycheck.

With hyperbolic discounting and borrowing constraints, the household can obtain the same outcome in case of $\beta = 1$ by having the paycheck partitioned. The household will attain the same solution as in the absence of hyperbolic discounting: perfect consumption smoothing. A higher frequency of pay serves as a commitment device when the household is borrowing constrained. The welfare consequences depend on the nature of the problem. If households have a self-control problem in the sense that $\beta < 1$ and they want to attain $\beta = 1$, partitioning paychecks serves as a commitment device. Welfare increases. If however $\beta < 1$ and households are happy with binges in consumption, partitioning decreases welfare. I will assume that households have a self-control problem and prefer smooth consumption.

Consumption commitments, no borrowing constraints

The result in the previous subsection does not change much with the introduction of consumption commitments. The household has to pay a large, fixed expenditure M right after week 4. Think of M as a mortgage payment. The size of the consumption commitment is predetermined by the household and fixed in the short run. The household receives a steady flow of utility of housing services (not included in the model), but not of the mortgage payment itself. The budget share of M out of total income Y^M is α. The budget constraint of the household changes into $\sum_{w=1}^{4} C_w = Y^M - M = (1-\alpha)Y^M$. In the absence of borrowing constraints all three pay schedules generate the same consumption profile. The household chooses a consumption profile given her disposable income over the month. The consumption profile is a simple modification of the previous result equation (4.4):

$$C_1 = \frac{1}{1+3\beta^{\frac{1}{\rho}}}(1-\alpha)Y^M, \text{for w= 1}$$

$$C_w = \frac{\prod_{s=1}^{w}(4-s)\beta^{\frac{w-1}{\rho}}}{\prod_{s=1}^{w}[1+\beta^{\frac{1}{\rho}}(4-s)]}(1-\alpha)Y^M \text{for w}\in\{2,3,4\}. \tag{4.7}$$

The level of consumption is lower compared to the model without consumption commitments, but the consumption profile is the same. Weekly, biweekly and monthly paid households have the same consumption profile in the absence of borrowing constraints.

Consumption commitments and borrowing constraints

The result that with borrowing constraints different pay frequencies have different consumption profiles changes with the introduction of consumption commitments. Let us start with a comparison of the monthly and the biweekly paid household. Both households have to pay the consumption commitment after week 4. The consumption path of the monthly paid household is derived in equation (4.7). The consumption profile of the biweekly paid household depends on whether the borrowing constraint binds. The intuition is that the household is aware that it has to save for the payment of the consumption commitment at the end of the month, but that she wants to postpone this saving to later weeks. The larger the budget share of the consumption commitment, the larger the saving that the household can postpone, the more consumption the household can shift towards the present against later.

At the beginning of week 1 the household optimizes her consumption plan and allocates how much to save from the first paycheck and how much to save from the second in week 3. If the household saves from the first paycheck, she will save from the second as well. If she does not save from the first paycheck, the consumption commitment is paid entirely out of the second check. The higher the self-control problem – a lower value of β – the more the household will actually want to borrow from the second paycheck in order to consume more in the first two weeks. Depending on whether the borrowing constraint binds, there are two different consumption profiles. The borrowing constraint binds for combinations of (α, β) for which $C_1 + C_2 \geq Y^{BW}$. Again, define consumption in the first two weeks as in equation (4.5):

$$C_{12} = C_1 + C_2 \geq \frac{1}{2} Y^M$$

$$\frac{1 + 5\beta^{\frac{1}{\rho}}}{(1 + 3\beta^{\frac{1}{\rho}})(1 + 2\beta^{\frac{1}{\rho}})}(1 - \alpha)Y^M \geq \frac{1}{2} Y^M. \tag{4.8}$$

Solving for α, the borrowing constraint binds for any:

$$\alpha \leq \frac{(1 + 6\beta^{\frac{1}{\rho}})(1 - \beta^{\frac{1}{\rho}})}{2 + 10\beta^{\frac{1}{\rho}}}. \tag{4.9}$$

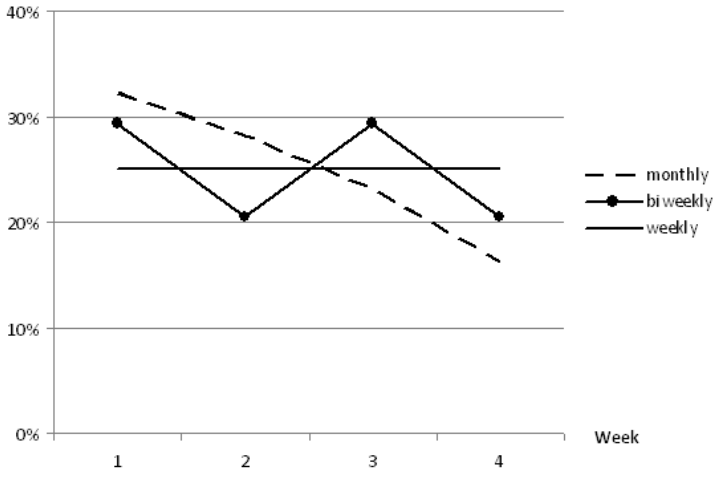
Figures (4.6) and (4.7) show for different combinations of (α, β) and for two values of ρ the pattern of the borrowing constraint. In the grey area, households want to consume in the first two weeks more than the amount of the first paycheck. For those combinations of (α, β) the borrowing constraint binds. In the white area the biweekly paid household has the same consumption profile as the monthly paid household. The white area increases for larger values of the coefficient of relative risk aversion (ρ). If the borrowing constraint binds, consumption for the biweekly paid household is given by (4.10), where consumption in week 3 and 4 is different from the solution in (4.5).

$$C_1 = \frac{1}{1 + \beta^{\frac{1}{\rho}}} Y^{BW}.$$

$$C_2 = \frac{\beta^{\frac{1}{\rho}}}{1 + \beta^{\frac{1}{\rho}}} Y^{BW}.$$

$$C_3 = \frac{1}{1 + \beta^{\frac{1}{\rho}}}(Y^{BW} - M).$$

Figure 4.5: Consumption in every week as a fraction of total income ($\beta = 0.7, \rho = 1$)



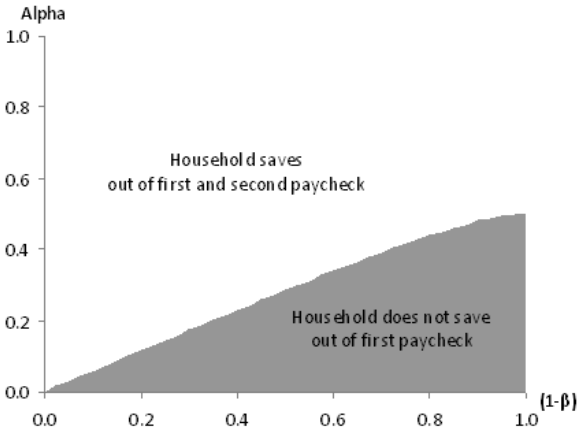


Figure 4.6: Borrowing constraints, $\rho = 1$

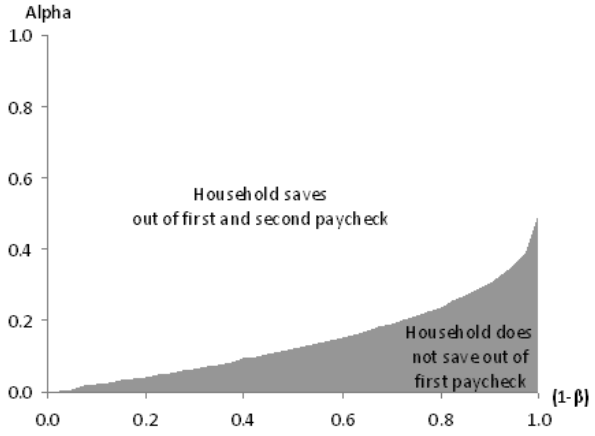


Figure 4.7: Borrowing constraints, $\rho = 3$

$$C_4 = \frac{\beta^{\frac{1}{\rho}}}{1+\beta^{\frac{1}{\rho}}}(Y^{BW} - M). \tag{4.10}$$

If the borrowing constraint does not bind, the consumption profile is the same as the monthly paid household – equation (4.7). The key result is that the biweekly pay scheme and the monthly pay scheme have an equivalent consumption profile for any combination of (α, β) for which the borrowing constraint does not bind. This result is different from the result without consumption commitments and shows the importance of incorporating consumption commitments into the analysis. The consumption profiles for two values of α and $\rho = 1$ are given in figures (4.8) and (4.9). Consumption is a fraction of disposable income to facilitate comparisons between the graphs. In figure (4.8) the borrowing constraint binds. The household consumes both paychecks in a jigsaw pattern, and finances the consumption commitment out of the last paycheck. In figure (4.9) the borrowing constraint does not bind and the household starts saving for the consumption commitment out of the first paycheck. The jigsaw pattern has disappeared and the pattern is similar to the monthly paid household. If the borrowing constraint does not bind, the biweekly and the monthly consumption profiles are exactly the same for all values of $\beta \leq 1$.

For weekly paid households the analysis is the same as with the biweekly paid household: there are combinations of (α, β) for which the borrowing constraint binds. The household gets four paychecks $Y^W = \frac{1}{4}Y^M$. The household can choose to save a fraction of each paycheck for the payment of the consumption commitment. For $\beta < 1$ the household prefers consumption now and defers saving to later weeks. The household would actually like to consume more than the first paycheck in the first week, but the borrowing constraint binds. Since there are four weeks for which borrowing constraint does or does not bind, there are four different consumption profiles possible for different combinations of (α, β). If the borrowing constraint does not bind in week 1, it will not bind in all subsequent weeks. This implies that the household saves from each paycheck. The complete derivation is given in the Appendix.

Figures (4.10) and (4.11) show the different borrowing constraints for different weeks for two values of ρ. Darker grey areas indicate that for lower values of α the borrowing constraint binds in more weeks. In area A the borrowing constraint never binds. For this combination of (α, β, ρ) weekly, biweekly and monthly pay are equivalent. In area B, the borrowing constraint binds in the first week, but not in later weeks. For combinations of (α, β) in area B, the household consumes the entire first paycheck and saves part of the other paychecks. For area C the borrowing constraint binds for the first two weeks and in

area D the borrowing constraint binds for all weeks. For area D the household entirely consumes the first three paychecks, pays the consumption commitment out of the last paycheck and consumes what is left. In order to compare the weekly and the biweekly paid household, combinations of (α, β) for which the biweekly borrowing constraint binds are all in area B. The consumption profiles of the weekly paid household are shown in figures (4.12) and (4.13) for $\rho = 1$ and two different values of α. Note that the line for $\beta = 0.7$ and $\alpha = 0.3$ in figure (4.13) is the same for the biweekly paid household in figure (4.9) and for the monthly paid household in figure (4.5). For this combination of $(\alpha = 0.3, \beta = 0.7)$ the consumption paths of all three payschemes are exactly the same.

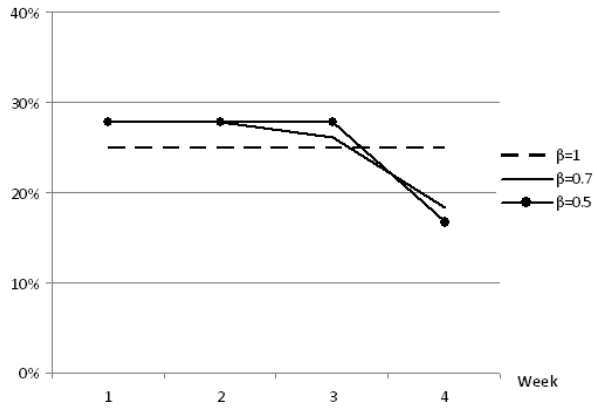


Figure 4.12: Consumption out of disposable income, budget share M (α) = 10%

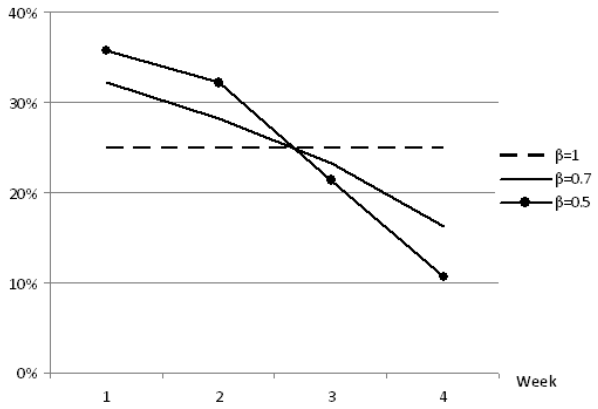


Figure 4.13: Consumption out of disposable income, budget share M (α) = 30%

Discussion of the model

The model shows that weekly, biweekly and monthly paid households have the same consumption profile in the presence of consumption commitments. Higher frequency of pay is only a commitment device for consumption smoothing if: (i) the budget share of the consumption commitment is small relative to the self-control problem of the household, and: (ii) the household is borrowing constrained. Some back of the envelope calculations show that condition (i) is easily violated. The average budget share of rent/mortgage payments is about 0.22 (Chetty and Szeidl, 2007). On average β is taken to be around 0.7. For these values of α and β, and log-utility ($\rho = 1$), biweekly paid households and monthly paid have the same consumption profiles. For weekly paid households the budget constraint only binds (borderline) in the first week, but not in all subsequent weeks. Higher values

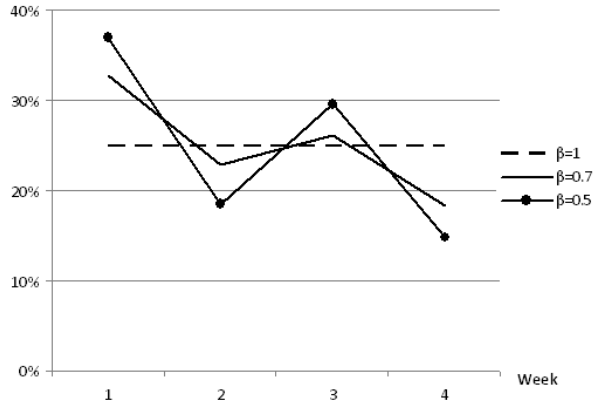


Figure 4.8: Consumption out of disposable income, budget share M (α) = 10%

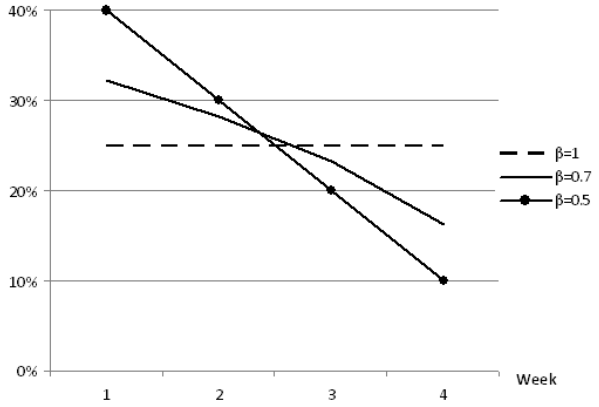


Figure 4.9: Consumption out of disposable income, budget share M (α) = 30%

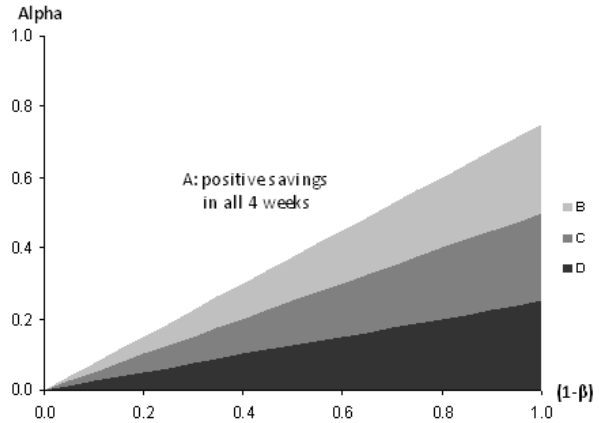


Figure 4.10: Borrowing constraints, $\rho = 1$

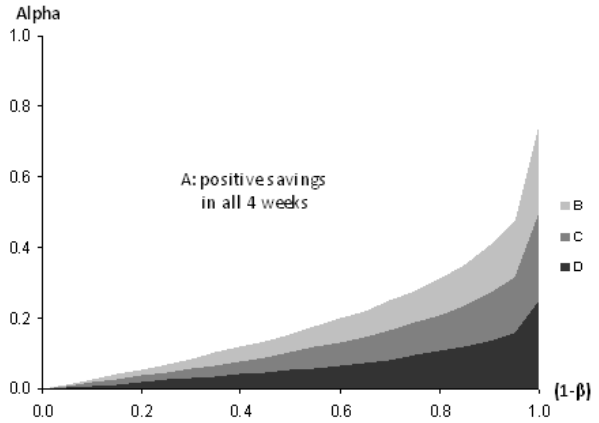


Figure 4.11: Borrowing constraints, $\rho = 3$

of the coefficient of relative risk aversion ($\rho > 1$) ensure that condition (i) is violated for larger ranges of (α, β).

The theoretical result from this model generates three additional insights. First, it shows that the timing of *payment* of large fixed expenditures can influence the *utility* flow of the household. In canonical models of consumption the payment of the consumption commitment does not affect the utility flow. The combination of borrowing constraints and lack of self-control can influence the overall utility flow of the household. The second insight is that policy recommendations of a model without consumption commitments are rather different from a model with consumption commitments. Ignoring consumption commitments gives different consumption profiles for different frequencies of pay and therefore different policy recommendations, see Parsons and Van Wesep (2013). Thirdly, this model shows how the time horizon of the household emerges as the interplay of income frequency and frequency of consumption commitments. A hyperbolic discounter on weekly pay has a horizon of a week – in the absence of consumption commitments. With a monthly consumption commitment and non-binding borrowing constraints, the weekly paid household has a horizon of a month. Models that only focus on the income side shrink the horizon of the household in β.

4.7 Conclusions

I find evidence for an intra-monthly cycle in consumption expenditures around the payment of rent/mortgage. For different subgroups I find the same qualitative cycle – consumption expenditures are higher in the week after rent/mortgage payments are made compared to the week before. The evidence and the model presented show that potential smoothing benefits of more frequent income payments are offset by consumption commitments. I also find that the documented cycle for Social Security recipients is more a response to the timing of mortgage/rent payments, and to a lesser extent a response to income receipt. The documented intra-monthly response to income receipt is easily confounded with the cycle around mortgage and rent payments. The theoretical result and empirical findings show that explanations using hyperbolic discounting should take into account the nature of the expenditures side – to what extent expenditures are fixed. Future research can take into account the large share of the household budget that is fixed in the short run on household welfare. Another avenue for future research is whether consumption commitments are a commitment device for intra-household bargaining problems (e.g. Ashraf, 2009).

4.A Derivation optimal consumption profile

Monthly paid households

First I derive the consumption profile for the monthly paid household. This will be the starting point for the analysis of biweekly and weekly paid households and for the analysis of the role of borrowing constraints. The derivation follows Mastrobuoni and Weinberg (2009), and Parsons and Van Wesep (2013). Let Y^M be the monthly paycheck, X_w cash-at-hand, and M the payment of the large fixed expenditure. The household optimizes:

$$\max U = \frac{C_1^{1-\rho}}{1-\rho} + \beta \sum_{w=2}^{4} \frac{C_w^{1-\rho}}{1-\rho} \qquad \text{subject to} \qquad \sum_{w=1}^{4} C_w = Y^M - M. \tag{4.11}$$

The first-order conditions in week 1 are:

$$\frac{C_1}{C_2} = \frac{1}{\beta^{1/\rho}}. \tag{4.12}$$

$$\frac{C_2}{C_3} = \frac{C_3}{C_4} = 1. \tag{4.13}$$

For the monthly paid household cash-at-hand in every week is defined as:

$$\begin{aligned} X_1 &= Y^M - C_1 && \text{for w=1} \\ X_2 &= X_1 - C_2 && \text{for w=2} \\ X_3 &= X_2 - C_3 && \text{for w=3} \\ X_4 &= X_3 - C_4 = M && \text{for w=4.} \end{aligned} \tag{4.14}$$

M is to be paid directly after the last week. Starting with the last two periods, the household optimizes in week 3:

$$\max_{C_3} U = \frac{C_3^{1-\rho}}{1-\rho} + \beta \frac{C_4^{1-\rho}}{1-\rho}. \tag{4.15}$$

Substituting $C_4 = X_2 - C_3 - M$ and solving for C_3 and C_4 gives:

$$\begin{aligned} C_3 &= \tfrac{1}{1+\beta^{\frac{1}{\rho}}}(X_2 - M) \\ C_4 &= \tfrac{\beta^{\frac{1}{\rho}}}{1+\beta^{\frac{1}{\rho}}}(X_2 - M). \end{aligned} \tag{4.16}$$

The maximization problem in week 2 is:

$$\max_{C_2} U = \frac{C_2^{1-\rho}}{1-\rho} + \beta \left(\frac{C_3^{1-\rho}}{1-\rho} + \frac{C_4^{1-\rho}}{1-\rho} \right). \tag{4.17}$$

Use $X_2 = X_1 - C_2$ in (4.16), rewrite C_3 and C_4, and plug both into equation (4.17).

$$\max_{C_2} U = \frac{C_2^{1-\rho}}{1-\rho} + \beta \left(\frac{(X_1 - C_2 - M)^{1-\rho}}{(1+\beta^{\frac{1}{\rho}})(1-\rho)} + \frac{\beta^{\frac{1}{\rho}}(X_1 - C_2 - M)^{1-\rho}}{(1+\beta^{\frac{1}{\rho}})(1-\rho)} \right). \tag{4.18}$$

The closed-form expressions are now:

$$\begin{aligned} C_2 &= \tfrac{1}{1+2\beta^{\frac{1}{\rho}}}(X_1 - M), \\ C_3 &= \tfrac{2\beta^{\frac{1}{\rho}}}{1+2\beta^{\frac{1}{\rho}}} \tfrac{1}{1+\beta^{\frac{1}{\rho}}}(X_1 - M), \\ C_4 &= \tfrac{2\beta^{\frac{1}{\rho}}}{1+2\beta^{\frac{1}{\rho}}} \tfrac{\beta^{\frac{1}{\rho}}}{1+\beta^{\frac{1}{\rho}}}(X_1 - M). \end{aligned} \tag{4.19}$$

Reiterating the same steps for week 1 gives the following solution:

$$\begin{aligned} C_1 &= \tfrac{1}{1+3\beta^{\frac{1}{\rho}}}(1-\alpha)Y^M, \\ C_2 &= \tfrac{3\beta^{\frac{1}{\rho}}}{1+3\beta^{\frac{1}{\rho}}} \tfrac{1}{1+2\beta^{\frac{1}{\rho}}}(1-\alpha)Y^M, \\ C_3 &= \tfrac{3\beta^{\frac{1}{\rho}}}{1+3\beta^{\frac{1}{\rho}}} \tfrac{2\beta^{\frac{1}{\rho}}}{1+2\beta^{\frac{1}{\rho}}} \tfrac{1}{1+\beta^{\frac{1}{\rho}}}(1-\alpha)Y^M, \\ C_4 &= \tfrac{3\beta^{\frac{1}{\rho}}}{1+3\beta^{\frac{1}{\rho}}} \tfrac{2\beta^{\frac{1}{\rho}}}{1+2\beta^{\frac{1}{\rho}}} \tfrac{\beta^{\frac{1}{\rho}}}{1+\beta^{\frac{1}{\rho}}}(1-\alpha)Y^M. \end{aligned} \tag{4.20}$$

Biweekly paid households

The biweekly paid household receives the same monthly income (Y^M), but this is distributed in two equal paychecks ($Y^{BW} = \frac{1}{2}Y^M$). To facilitate comparisons between the monthly paid household and the biweekly paid household I keep on using Y^M. The development of cash-at-hand for the biweekly paid household is:

$$\begin{aligned} X_1 &\leq \tfrac{1}{2}Y^M - C_1 && \text{for w=1} \\ X_2 &\leq X_1 - C_2 && \text{for w=2} \\ X_3 &\leq X_2 + \tfrac{1}{2}Y^M - C_3 && \text{for w=3} \\ X_4 &\leq X_3 - C_4 = M && \text{for w=4.} \end{aligned} \tag{4.21}$$

The equations for cash-at-hand have an inequality because the borrowing constraint can bind. There exist combinations of (α, β) for which the consumption path is the same as the monthly paid household in (4.20). But in order to obtain the same consumption path, the biweekly paid household borrows out of the second paycheck. There are two optimal consumption profiles from the set of all feasible consumption paths. The first one is similar to the one of the monthly paid household (4.20). This path is for combinations of (α, β) for which the borrowing constraint does not bind. The borrowing constraint binds if $X_2 = \frac{1}{2}Y^M - C_1 - C_2 \leq 0$. Using C_1 and C_2 from the optimal consumption profile of (4.20) and solving for α gives:

$$\alpha \leq \frac{(1 + 6\beta^{\frac{1}{\rho}})(1 - \beta^{\frac{1}{\rho}})}{2 + 10\beta^{\frac{1}{\rho}}}. \tag{4.22}$$

The second consumption profile is for combinations of (α, β) for which the borrowing constraint does bind. For these combinations of (α, β) the household would like to have that total consumption in the first two weeks is more than the first paycheck. When the borrowing constraint binds, the consumption profile consists of two parts: the first two equations in (4.23) and the last two:

$$\begin{aligned} C_1 &= \tfrac{1}{1+\beta^{\frac{1}{\rho}}} \tfrac{1}{2} Y^M, \\ C_2 &= \tfrac{\beta^{\frac{1}{\rho}}}{1+\beta^{\frac{1}{\rho}}} \tfrac{1}{2} Y^M. \\ C_3 &= \tfrac{1}{1+\beta^{\frac{1}{\rho}}} \tfrac{1}{2} Y^M (1-\alpha), \\ C_4 &= \tfrac{\beta^{\frac{1}{\rho}}}{1+\beta^{\frac{1}{\rho}}} \tfrac{1}{2} Y^M (1-\alpha). \end{aligned} \tag{4.23}$$

Over the first two weeks the household divides consumption out of the first paycheck. Over the second half the household divides consumption out of the second paycheck minus the consumption expenditure M. This gives the jigsaw pattern in figure (4.8).

Weekly paid households

The derivation of the weekly paid household follows the same logic as the biweekly paid household. The borrowing constraint binds for those combinations of (α, β) for which the household needs to borrow in order to mimic the monthly paid household. The household

receives four equal paychecks: $Y^W = \frac{1}{4}Y^M$. The development of cash-at-hand is:

$$\begin{aligned} X_1 &\leq \tfrac{1}{4}Y^M - C_1 && \text{for w=1} \\ X_2 &\leq X_1 + \tfrac{1}{4}Y^M - C_2 && \text{for w=2} \\ X_3 &\leq X_2 + \tfrac{1}{4}Y^M - C_3 && \text{for w=3} \\ X_4 &\leq X_3 + \tfrac{1}{4}Y^M - C_4 = M && \text{for w=4.} \end{aligned} \tag{4.24}$$

Since the borrowing constraint can potentially bind in each subsequent week, there are four different consumption paths for the weekly paid household. A useful way to solve this problem is to combine the first-order conditions (4.12) and (4.13) with the development of cash-at-hand (4.24). The planning problem of the household for week 1 can now be written as a system of three consumption ratios:

$$\begin{aligned} \tfrac{C_1}{C_2} &= \tfrac{\frac{1}{4}Y^M - X_1}{\frac{1}{4}Y^M + X_1 - X_2} = \tfrac{1}{\beta^{\frac{1}{\rho}}}, \\ \tfrac{C_2}{C_3} &= \tfrac{\frac{1}{4}Y^M + X_1 - X_2}{\frac{1}{4}Y^M + X_2 - X_3} = 1, \\ \tfrac{C_3}{C_4} &= \tfrac{\frac{1}{4}Y^M + X_2 - X_3}{\frac{1}{4}Y^M + X_3 - M} = 1. \end{aligned} \tag{4.25}$$

Starting with the last consumption ratio and solving backwards we can write $X_2 = \frac{2X_1 + M}{3}$. Using this in the first consumption ratio gives the solution for X_1:

$$X_1^* = \tfrac{1}{4}Y^M - C_1 = \begin{cases} X_1^A = \frac{\frac{3}{4}Y^M(\beta^{\frac{1}{\rho}} - 1) + M}{1 + 3\beta^{\frac{1}{\rho}}} = \frac{Y^M(3\beta^{\frac{1}{\rho}} - 3 + 4\alpha)}{1 + 3\beta^{\frac{1}{\rho}}} & \text{if } \alpha > \frac{3(1 - \beta^{\frac{1}{\rho}})}{4}, \\ X_1^{BCD} = 0 & \text{if } \alpha \leq \frac{3(1 - \beta^{\frac{1}{\rho}})}{4}. \end{cases} \tag{4.26}$$

Since the household is borrowing constraint, X_1^* cannot be negative. The denominator of X_1^A determines for which combination of (α, β) the borrowing constraint binds in week 1. For combinations of (α, β) that $X_1^A = X_1^* > 0$, cash-at-hand in all subsequent weeks X_2, X_3 and X_4 will be strictly positive as well. If the borrowing constraint does not bind in the first week, it will not bind in later weeks either. An important insight is that for combinations of (α, β) for which the borrowing constraint does not bind ($X_1 = \frac{1}{4}Y^M - C_1 > 0$), the weekly household has the same consumption profile as the monthly paid household.

$$\begin{aligned} C_1 = \;& \tfrac{1}{4}Y^M - X_1^A \\ = \;& \tfrac{1}{4}Y^M - \tfrac{\frac{3}{4}Y^M(\beta^{\frac{1}{\rho}} - 1) + M}{1 + 3\beta^{\frac{1}{\rho}}} = \tfrac{1 - \alpha}{1 + 3\beta^{\frac{1}{\rho}}}Y^M \end{aligned} \tag{4.27}$$

The expression for C_1 in (4.27) is the same expression for C_1 in (4.20). Expression (4.27) also shows that if the the weekly paid household is not borrowing constrained (so that X_1 can take on negative values), it would consume exactly the same amount in week 1 as the monthly paid household.Having solved for cash-at-hand in week 1, we proceed to week 2. Using solution (4.26) changes equation (4.25) into:

$$\begin{aligned} \frac{C_2}{C_3} &= \frac{\frac{1}{4}Y^M + X_1^* - X_2}{\frac{1}{4}Y^M + X_2 - X_3} = \frac{1}{\beta^{\frac{1}{\rho}}}, \\ \frac{C_3}{C_4} &= \frac{\frac{1}{4}Y^M + X_2 - X_3}{\frac{1}{4}Y^M + X_3 - M} = 1. \end{aligned} \tag{4.28}$$

Starting with the last consumption ratio and solving backwards we can write $X_3 = \frac{X_2+M}{2}$. Using this in the first consumption ratio gives the solution for X_2^*. Depending on whether the borrowing constraint in week 1 binds, the solution for X_2^* is:

$$X_2^* = \tfrac{1}{4}Y^M + X_1^* - C_2 = \begin{cases} X_2^A = \frac{Y^M(\beta^{\frac{1}{\rho}} - 1 + 2\alpha) + 4\beta X_1^A}{1 + 2\beta^{\frac{1}{\rho}}} & \text{for } \alpha > \frac{3}{4}(1 - \beta^{\frac{1}{\rho}}), \\ X_2^B = \frac{Y^M(\beta^{\frac{1}{\rho}} - 1 + 2\alpha)}{1 + 2\beta^{\frac{1}{\rho}}} & \text{for } \frac{1}{2}(1 - \beta^{\frac{1}{\rho}}) < \alpha \leq \frac{3}{4}(1 - \beta^{\frac{1}{\rho}}), \\ X_2^{CD} = 0 & \text{for } \alpha \leq \frac{1}{2}(1 - \beta^{\frac{1}{\rho}}). \end{cases} \tag{4.29}$$

The combination of (α, β) for which the borrowing constraint binds in week 2 is given by $\alpha_2 \leq \frac{1-\beta^{\frac{1}{\rho}}}{2}$. Note that for combinations of (α, β) for which the borrowing constraint binds in week 1 (4.26), it also binds in week 2. The opposite is not true. There are combinations of (α, β) for which the borrowing constraint binds in the first week, but not in the second week. The characterization of cash-at-hand in week 3 is (where the superscripts denote different paths for cash-at-hand depending on cash-at-hand in the previous weeks):

$$X_3^* = \begin{cases} X_3^A = \frac{Y^M(\beta^{\frac{1}{\rho}} - 1 + 4\alpha) + 4\beta X_2^A}{1 + \beta^{\frac{1}{\rho}}} & \text{for } \alpha > \frac{3}{4}(1 - \beta^{\frac{1}{\rho}}), \\ X_3^B = \frac{Y^M(\beta^{\frac{1}{\rho}} - 1 + 4\alpha) + 4\beta X_2^B}{1 + \beta^{\frac{1}{\rho}}} & \text{for } \frac{1}{2}(1 - \beta^{\frac{1}{\rho}}) < \alpha \leq \frac{3}{4}(1 - \beta^{\frac{1}{\rho}}), \\ X_3^C = \frac{Y^M(\beta^{\frac{1}{\rho}} - 1 + 4\alpha)}{1 + \beta^{\frac{1}{\rho}}} & \text{for } \frac{1}{4}(1 - \beta^{\frac{1}{\rho}}) < \alpha \leq \frac{1}{2}(1 - \beta^{\frac{1}{\rho}}), \\ X_3^D = 0 & \text{for } \alpha \leq \frac{1}{4}(1 - \beta^{\frac{1}{\rho}}). \end{cases} \tag{4.30}$$

The four combinations of (α, β) are depicted in figures (4.10) and (4.11) in the main text.

Table 4.12: Comparison of households with and without an observation for mortgage and rent payment

	Weekly			Biweekly			Semi-Monthly			Monthly			Social Security		
	obs.	not obs.		obs.	not obs.		obs.	not obs.		obs.	not obs.		obs.	not obs.	
Budget share rent/mortgage (α)	0.26	-		0.24	-		0.25	-		0.26	-		0.37	-	
Mortgage payment ($)	957	-		1118	-		1164	-		1134	-		722	-	
Rent payment ($)	677	-		762	-		812	-		844	-		511	-	
Owned with mortgage	0.50	0.54	***	0.58	0.60	***	0.61	0.60		0.61	0.63		0.38	0.46	***
Rented	0.50	0.46	***	0.42	0.40	***	0.40	0.40		0.39	0.37		0.62	0.54	***
Male	0.54	0.54		0.45	0.47	*	0.50	0.50		0.44	0.48	**	0.40	0.40	
Age	39.70	39.49		40.80	40.81		40.65	41.25		41.52	42.51	*	67.67	67.82	
Highschool	0.35	0.36		0.23	0.22		0.15	0.15		0.15	0.15		0.33	0.33	
Some college	0.24	0.23		0.24	0.24		0.22	0.25		0.18	0.18		0.18	0.18	
College or more	0.23	0.23		0.47	0.47		0.57	0.54		0.63	0.62		0.19	0.20	
Urban area	0.93	0.92		0.95	0.95		0.96	0.95		0.94	0.92	**	0.93	0.92	
No. household members	2.93	2.94		2.66	2.61	*	2.54	2.77	**	2.51	2.64	*	1.60	1.70	***
White	0.86	0.83	***	0.82	0.80	***	0.86	0.80	**	0.83	0.80	*	0.80	0.79	
Black	0.11	0.13	***	0.13	0.14	***	0.07	0.09		0.08	0.10		0.17	0.16	
Ann. wage income ($)	46.07	46.42		59.69	60.04		63.91	64.55		54.85	56.87		0.84	0.79	
Ann. Soc. Sec. income ($)	0.60	0.57		0.68	0.59		0.47	0.56		0.67	0.96		14.35	14.57	
Ann. pre-tax income ($)	50.46	50.71		64.46	64.93		70.46	73.66		62.07	65.58	*	21.73	21.76	
N	2631	4969		5052	9361		347	535		738	1408		1632	3059	

The first column reports the means of characteristics of households for which there is a mortgage or rent payment observed in the sample. The second column reports the means of the households characteristics without such observation. There is no observation for rent or mortgage payment because the survey period of 14 days did not overlap with the date of payment. Stars denote the level of significance of differences between the means, */**/*** correspond to 10%/5%/1% significance level.

Table 4.13: Distribution of rent and mortgage payments over the calender month

		$8-14$ days before	$1-7$ days before	$0-6$ days after	$7-13$ days after	other	total
Wage earners	Mortgage payment ($)	1,097	1,169	1,123	1,064	1,038	1,114
	Budget share rent/mortgage (α)	0.21	0.21	0.21	0.20	0.20	0.21
	$N = 7,366$	23%	29%	23%	19%	6%	100%
	Rent payment ($)	767	792	769	733	649	766
	Budget share rent/mortgage (α)	0.27	0.28	0.28	0.27	0.26	0.28
	$N = 4,989$	24%	34%	25%	13%	4%	100%
Soc. Sec. recipients	Mortgage payment ($)	759	742	674	673	790	722
	Budget share rent/mortgage (α)	0.38	0.34	0.35	0.35	0.35	0.35
	$N = 621$	21%	33%	24%	15%	7%	100%
	Rent payment ($)	519	513	503	522	466	511
	Budget share rent/mortgage (α)	0.37	0.39	0.38	0.38	0.34	0.38
	$N = 1,011$	18%	35%	29%	14%	4%	100%
All	Rent or mortgage ($)	925	952	920	921	881	929
	Budget share rent/mortgage (α)	0.25	0.26	0.26	0.24	0.23	0.25
	Fraction rented	0.44	0.46	0.45	0.35	0.34	0.43
	$N = 13,987$	23%	31%	24%	16%	5%	100%

Days are grouped in weeks around the first day of the calendar month. For each week the average real mortgage and rent payment is reported, together with the average budget share of the consumption commitment. The category "other" is not a full week, but contains different numbers of days depending on the month and the year. Rent and mortgage payments are measured in 2008 US dollar.

CHAPTER 5

Framing effects in an employee savings scheme

5.1 Introduction

A growing literature shows that seemingly unimportant details of the choice setting for which there is no role in standard economic theory can have a large impact on how individuals behave. A widely studied example is the effect of defaults on choice behavior, in particular, in enrollment in pension plans; see, for example, Beshears et al. (2008).

Another seemingly unimportant detail is the label of an income source. Using different terms[1] authors found evidence for essentially the same phenomenon: the label of an income source has an effect on how it is used. While researched to a lesser extent than defaults, labeling effects are arguably more puzzling. Default effects can often be rationalized by switching or transaction costs, labeling effects cannot.

The empirical work on labeling effects has used different methodological approaches. Early estimates of the "flypaper effect" generally relied on cross-sectional variation in grants received by local governments, taking this variation to be exogenous; see for example Hines and Thaler (1995). Other work has used (natural) experiments as a source of exogenous variation in income sources. For example, Kooreman (2000) used variation in child benefit amounts induced by policy changes to analyze the effect of the income label "child benefit" on household expenditures. He found that in two-parent families the marginal propensity to consume child clothing out of the child benefits is more than ten times as large as that out of other income sources. For adult clothing such an effect is absent. Since the difference in marginal propensities is even larger for single parent households, the effects cannot be

[1] Mental accounting effect (Thaler, 1992, 1999), flypaper effect (Hines and Thaler, 1995), label(l)ing effect (Kooreman, 2000; Beatty et al., 2011), non-fungibility, earmarking, or income framing effect (Epley et al. 2006; Card and Ransom (2011).

explained by differences in preferences for child goods between fathers and mothers.

Card and Ransom (2011) investigate the retirement savings behavior of tenured and tenure-track college professors. Faculty can make tax-deferred contributions to a retirement savings account in addition to their own mandatory contribution and their employer's contribution. Card and Ransom find that if the employer contribution increases by 1 dollar, supplemental savings fall by 0.30 dollar. If the mandatory employee contribution increases by 1 dollar, supplementary savings fall by 0.70 dollar. These effects are largely identified using cross-section variation between universities and colleges.[2]

Two recent papers have used small-scale field experiments to study (pure) labeling effects. Epley et al. (2006) asked US residents to recall how they had spent the 2001 tax rebate that provided each tax paying American household with a check between 300 dollar and 600 dollar. To remind them of the rebate, participants were made to read one of two randomized descriptions of this policy measure. Participants to whom the rebate was described as "withheld income" reported that they had spent only 25 percent of it, while those to whom the rebate was described as "bonus income" reported to have spent 87 percent. In a related experiment, Harvard undergraduates received 50 dollar and were informed that this came from a university fund financed by tuition fees. Some participants were told they were receiving a "tuition rebate", others that they were receiving an "income bonus". After a week, undergraduates who had received the "income bonus" reported having consumed twice as much out of the 50 dollar than those who had received the "tuition rebate" (22.04 dollar versus 9.55 dollar). Abeler and Marklein (2008) conducted a field experiment in a wine restaurant. People who received a voucher labeled for drinks (worth less than the average amount usually spent on drinks) spent 25 percent more on drinks than those who received a voucher for the entire bill.

The purpose of the present chapter is to further strengthen the methodology for measuring framing effects. Previous work was typically based on linear or quadratic parametric specifications, and used annual aggregates of savings, income and expenditures, choices largely guided by the aggregation level of the data and sample sizes. For the present chapter we had access to an unusually large data set with information on savings contributions and many different income components for 1.3 million employees-month observations. This allows us to analyze the sensitivity of results to using more flexible, non-parametric func-

[2]In general, income components do not only differ in terms of their label, but also in other dimensions, such as timing of payment, visibility to the recipient, and institution from which it is received. Following Card and Ransom (2011), in those cases we will use the term 'framing effects' when marginal propensities to save differ across income components. We will reserve 'labeling effects' for cases in which income components merely differ is terms of their label, all else equal.

tional forms, and to using a finer time grid (monthly versus annually).

We explain monthly inlay in a tax-deferred savings account using nonparametric equations (up to a constant) in total income, with individual fixed effects, estimated separately for each month of the year. For comparison, we also estimate linear, quadratic, and cubic equations in total income, with individual fixed effects, estimated separately for each month of the year; and the same equations imposing that slope coefficients are constant across months. In addition, we estimate models based on annual aggregates of savings and income rather than monthly data. In all of these specifications we then test whether individual income components have any additional explanatory power. In a correctly specified model and in the absence of framing effects this should not be the case.

We focus on the marginal propensity to save into a particular savings account. In general, life cycle models do not imply that marginal propensities to save (mpss) out of different income components into a particular savings account should be equal. For example, different mpss into one savings account could be offset by opposite mpss into other savings accounts. The savings account we consider can only be used to purchase future leisure, through parental leave, taking a sabbatical, or early retirement. In addition, at the time our data apply to it was the only savings account that allowed for voluntary tax-deferred savings for those specific purposes. It is unlikely that employees used other savings accounts for the same purposes, since those savings accounts lacked the favorable tax treatment. Our marginal propensities to save can therefore be interpreted as marginal propensities to purchase future leisure.

We find evidence of framing effects for most specifications, including those with the highest degree of flexibility. Our monthly results indicate a large variation in behavior within the year. These results strengthen and add detail to earlier conclusions that policy makers have effective instruments at their disposal to affect the use of income components at low cost.

The chapter proceeds as follows. Section 5.2 describes the data and provides the necessary institutional background. Section 5.3 presents the econometric model and our estimation and testing procedure. Section 5.4 presents the results and section 5.5 concludes.

5.2 Data and institutional setting

Two financial firms in the Netherlands provided salary records of all of their employees. One firm is primarily active as a bank (and will be referred to as Bank), while the other

firm is primarily active as an insurer (to be referred to as Insurer). For each employee and each month of the years 2005, 2006, and 2007 the data contains information on many different wage components which we aggregate into nine comparable salary components. We also observe deposits into two tax-favored savings schemes (and withdrawals from one). In addition, there is some information on employee characteristics.

In the years the data apply to employees in the Netherlands had three main options regarding tax-favored employee saving. One was to participate in the Life Course Savings Scheme (LCS, *Levensloopregeling* in Dutch), which was introduced in 2006. A second option was to save in the Employee Savings Scheme (ESS, *Spaarloonregeling*), another tax-favored savings scheme that had already been in place for several decades. The third option was not to participate in either scheme (participation in both schemes at the same time was not permitted). The LCS scheme allowed workers to save up to a maximum of 12 percent of gross annual income, while the maximum contribution in the EES scheme was a mere 600 euro. Therefore, our endogenous variable of main interest is how much to deposit each month in the LCS.

Panel A of Table 5.1 presents the summary statistics for all employees. The participation rate in the LCS was 6-8 percent, which is around the national average. Average monthly savings into the LCS six to eight times the size compared to savings into the ESS, conditional on participation. Panel B shows the summary statistics for a subgroup of employees with strictly positive savings in at least one pair of months (January 2006 - January 2007). This subsample receives on average a higher salary, is older and has a higher fraction of men.

5.3 The econometric model

Let s_{it} denote the deposited amount in the LCS in month t by individual i and let g_i stand for the group of people to which individual i belongs. To test for framing effects, we shall consider subgroups

$$I_{g,t,T} = \{i \mid s_{it} > 0, \quad s_{i,t-T} > 0, \quad g_i = g\}. \tag{5.1}$$

Thus, we consider subgroups of individuals that deposited a positive amount in the LCS in month t as well as T months before month t and that belong to group g. In our application, t will be each of the months in 2007, T will be 12 months, while g will stand for a group characteristic. In particular, we shall make a distinction between being employed by the Bank and the Insurer. These subgroups are convenient samples for the purpose of the

Table 5.1: Summary Statistics

	Bank			Insurer		
A. Entire sample	2005	2006	2007	2005	2006	2007
Participation						
Life Course Savings Scheme	-	0.077	0.076	-	0.062	0.074
Employee Savings Scheme	0.476	0.354	0.373	0.816	0.645	0.680
Monthly inlay of participants						
Life Course Savings Scheme	-	327	385	-	339	401
Employee Savings Scheme	52	51	48	48	47	47
Monthly wage components						
Base salary	2,897	2,978	3,093	3,223	3,271	3,449
Holiday allowance	226	231	1	234	252	263
13th month	243	240	212	254	261	274
Profit share	44	155	139	244	234	237
Overtime	10	6	6	15	12	13
Bonus	67	301	306	18	19	17
LCS contribution	0	29	0	0	0	0
Other income	41	56	39	32	105	133
Benefit scheme	0	0	718	0	0	0
Employee characteristics						
Age (years)	41.3	41.5	41.8	42.9	42.1	41.8
Male (fraction)	0.520	0.510	0.507	0.641	0.627	0.621
Person-month observations	329,953	302,474	286,641	31,832	32,131	34,154

Contributions to savings schemes and wage components are average monthly values in nominal euro. Base salary is paid every month, Benefit scheme is paid in most months for the Bank, and Other income in most months for the Insurer. The other salary components are typically paid once a year.

	Bank			Insurer		
B. Subsample	2005	2006	2007	2005	2006	2007
Inlay Life Course Savings (month)	-	449	534	-	453	490
Monthly wage components						
Base salary	-	3,537	3,791	-	4,083	4,320
Holiday allowance	-	320	0	-	268	274
13th month	-	188	193	-	421	447
Profit share	-	214	223	-	460	497
Overtime	-	5	4	-	7	7
Bonus	-	425	412	-	17	15
LCS contribution	-	43	0	-	0	0
Other income	-	46	19	-	166	253
Benefit scheme	-	0	960	-	0	0
Employee characteristics						
Age (years)	-	42.3	43.3	-	45.6	46.6
Male (fraction)	-	0.582	0.582	-	0.756	0.756
Person-month observations	-	9,569	9,569	-	1,518	1,518

Panel B is a subsample of individuals with strictly positive contributions into the Life Course Savings scheme in the same month of years 2006 and 2007 (see section 5.3). Contributions to the Life Course Savings scheme and wage components are average monthly values in nominal euro. Base salary is paid every month, Benefit scheme is paid in most months for the Bank, and Other income in most months for the Insurer. The other salary components are typically paid once a year.

present study. The subgroups are selective, but to establish whether framing effects exist, it suffices to detect them in one subgroup. We will also consider other subgroups within the Bank and the Insurer, based on gender, age, and total income.

5.3.1 Nonparametric and parametric functional forms

We first describe our most general specification. For individuals i, belonging to subgroup $I_{g,t,T}$, we postulate

$$s_{i\tau} = f_\tau \left(\sum_\ell y_{i\tau}^\ell \right) + \zeta_{i\tau}, \quad \zeta_{i\tau} = \eta_i + \epsilon_{i\tau}, \quad \tau = t, t-T, \tag{5.2}$$

where $y_{i\tau}^\ell$ is income component ℓ of employee i in month τ, f_τ is the (group g-) specific unknown link function, depending on time τ, and $\zeta_{i\tau}$ is the error term, decomposed in an individual i specific effect (η_i) and an individual and time specific idiosyncratic effect ($\epsilon_{i\tau}$). Specification (5.2) is allowed to depend on the group g, but for notational convenience, we shall suppress this dependence.

To deal with the individual effect η_i, we take time differences, resulting in

$$s_{it} - s_{i,t-T} = f_t \left(\sum_\ell y_{it}^\ell \right) - f_{t-T} \left(\sum_\ell y_{i,t-T}^\ell \right) + \left(\epsilon_{it} - \epsilon_{i,t-T} \right). \tag{5.3}$$

We shall assume

$$E \left(\epsilon_{it} - \epsilon_{i,t-T} \mid \sum_\ell y_{it}^\ell, \sum_\ell y_{i,t-T}^\ell \right) = 0. \tag{5.4}$$

Given this assumption, the unknown regression functions $f_t(\cdot)$ and $f_{t-T}(\cdot)$ of equation (5.3) can be estimated by applying, for example, Linton and Nielsen's (1995) method while imposing their regularity conditions and additional distributional assumptions. This estimation approach proceeds as follows. First, consider the auxiliary nonparametric regression for $i \in I_{g,t,T}$

$$s_{it} - s_{i,t-T} = h \left(\sum_\ell y_{it}^\ell, \sum_\ell y_{i,t-T}^\ell \right) + \epsilon_{i,t,t-T}, \tag{5.5}$$

with

$$E \left(\epsilon_{i,t,t-T} \mid \sum_\ell y_{it}^\ell, \sum_\ell y_{i,t-T}^\ell \right) = 0. \tag{5.6}$$

One can estimate h nonparametrically, for instance, using a standard Kernel estimator or a local linear regression approach. Next, consider some distribution $\mathbb{Q}$ over $\sum_\ell y^\ell_{i,t-T}$. Then taking expectation of $h\left(\sum_\ell y^\ell_{it}, \sum_\ell y^\ell_{i,t-T}\right)$ with respect to $\mathbb{Q}$, keeping $\sum_\ell y^\ell_{it}$ fixed, we find

$$\begin{aligned} E_{\mathbb{Q}}\left(h\left(\sum_\ell y^\ell_{it}, \sum_\ell y^\ell_{i,t-T}\right)\right) =& f_t(\sum_\ell y^\ell_{it}) + E_{\mathbb{Q}}(f_{t-T}(\sum_\ell y^\ell_{i,t-T})) \\ =& f_t(\sum_\ell y^\ell_{it}) + c_{\mathbb{Q}}, \end{aligned} \tag{5.7}$$

with $c_{\mathbb{Q}}$ some constant depending on $\mathbb{Q}$. Thus, we can estimate f_t nonparametrically up to a constant by calculating $E_{\mathbb{Q}}\left(h\left(\cdot, \sum_\ell y^\ell_{i,t-T}\right)\right)$, using for h its nonparametric estimator. Similarly, we can estimate $f_{t-T}(\cdot)$ nonparametrically (up to a constant) by using an auxiliary distribution $\mathbb{Q}$ over $\sum_\ell y^\ell_{i,t}$. Like Linton and Nielsen (1995) we use the empirical distribution functions of $\sum_\ell y^\ell_{i,t}$ and $\sum_\ell y^\ell_{i,t-T}$ to form the auxiliary distribution $\mathbb{Q}$.
We shall also consider the parametric specifications

$$s_{it} = \beta^J_{0t} + \sum_{j=1}^{J} \beta^J_{jt} \left(\sum_\ell y^\ell_{it}\right)^j + \eta^J_i + \epsilon^J_{it}. \tag{5.8}$$

In our application, we consider $J = 1, 2, 3$. Taking time differences to eliminate the individual effect, we have

$$s_{it} - s_{i,t-T} = \delta^J_{0t} + \sum_{j=1}^{J} \beta^J_{jt} \left(\sum_\ell y^\ell_{it}\right)^j - \sum_{j=1}^{J} \beta^J_{j,t-T} \left(\sum_\ell y^\ell_{i,t-T}\right)^j + \xi^J_{i,t,t-T}, \tag{5.9}$$

with $\delta^J_{0t} = \beta^J_{0t} - \beta^J_{0,t-T}$ and $\xi^J_{i,t,t-T} = \epsilon^J_{it} - \epsilon^J_{i,t-T}$. Imposing a distributional assumption analogous to (5.6), we can estimate this equation using standard linear regression techniques.

5.3.2 Testing for framing effects

We can test whether separate income components do play a role or not, by considering as null hypothesis equation (5.2), with

$$E\left(\eta_i + \epsilon_{it} \mid \sum_\ell y^\ell_{it}, y^1_{it}, \ldots, y^L_{it}\right) = 0. \tag{5.10}$$

We can test this hypothesis by calculating the correlation between the estimated error term $y_{it} - \widehat{f}_t(\sum_\ell y_{it}^\ell)$, and each of the independent variables $y_{it}^1, \cdots, y_{it}^L$, with $\widehat{f}_t(\cdot)$ the nonparametric estimate of $f_t(\cdot)$, by extending the test statistic like the one proposed by Fan and Li (1996) to the current case.

Testing (5.10) makes sense in case the individual effect η_i is assumed to be uncorrelated with the independent variables. In case the individual effect might be correlated with the independent variables, it seems better to consider as null hypothesis equation (5.5), with

$$E\left(\epsilon_{it} - \epsilon_{i,t-T} \mid \sum_\ell y_{it}^\ell, \sum_\ell y_{i,t-T}^\ell, y_{i,t-T}^1, \ldots, y_{i,t-T}^L, y_{it}^1, \ldots, y_{it}^L\right) = 0. \tag{5.11}$$

Again, we can test this hypothesis by calculating now the correlation between the estimated error term

$$(s_{it} - \widehat{f}_t(\sum_\ell y_{it}^\ell)) - (s_{i,t-T} - \widehat{f}_{t-T}(\sum_\ell y_{i,t-T}^\ell)),$$

and each of the independent variables $y_{it}^1, \ldots, y_{it}^L$ and/or $y_{i,t-T}^1, \ldots, y_{i,t-T}^L$, extending the test statistic like the one by Fan and Li (1996) to this case.
Since this nonparametric test procedure is likely not to be very powerful we follow an alternative approach. First, we search for parametric specifications close to the nonparametric estimates $\widehat{f}_t$ and $\widehat{f}_{t-T}$. Then we test (5.5) combined with (5.11) using these parametric estimates. Under this null hypothesis we have

$$E\left(\xi_{i,t,t-T}^J \begin{pmatrix} y_{it}^\ell \\ y_{i,t-T}^\ell \end{pmatrix}\right) = 0, \tag{5.12}$$

with $\xi_{i,t,t-T}^J \equiv \epsilon_{it}^J - \epsilon_{i,t-T}^J$, and y_{it}^ℓ and $y_{i,t-T}^\ell$ are the ℓ-th income component of individual i at time t and $t-T$, respectively. The test statistic for some couple of time periods t and $t-T$ will therefore be based on

$$\frac{1}{N}\sum_i \widehat{\xi}_{i,t,t-T}^J \begin{pmatrix} y_{it}^\ell \\ y_{i,t-T}^\ell \end{pmatrix}, \tag{5.13}$$

where $\widehat{\xi}_{i,t,t-T}^J$ denotes the estimated error term, and N is the number of observation in $I_{g,t,T}$. Under the null hypothesis, we can derive the limit distribution of (5.13) (after scaling by $\sqrt{N}$) which is a normal distribution with mean vector zero and covariance matrix, say,

$V^{J,\ell}$. Our test statistic then becomes

$$T^{J,\ell} = N \left(\frac{1}{N} \sum_i \widehat{\xi}^J_{i,t,t-T} \begin{pmatrix} y^\ell_{it} \\ y^\ell_{i,t-T} \end{pmatrix} \right)' \left(\widehat{V}^{J,\ell} \right)^{-1} \left(\frac{1}{N} \sum_i \widehat{\xi}^J_{i,t,t-T} \begin{pmatrix} y^\ell_{it} \\ y^\ell_{i,t-T} \end{pmatrix} \right), \tag{5.14}$$

with $\widehat{V}^{J,\ell}$ a consistent estimate of $V^{J,\ell}$. Under the null hypothesis this test statistic follows a χ^2-distribution with two degrees of freedom. The test can easily be extended by including higher order (cross) terms (or other transformations) of y^ℓ_{it} and $y^\ell_{i,t-T}$ in (5.12). Moreover, the test can easily be extended by combining some or all income components.
As benchmark, we will also estimate

$$s_{it} = \beta^J_{0t} + \sum_{j=1}^{J} \beta^J_{1j} \left(\sum_{\ell=1}^{L} y^\ell_{it} \right)^j + \sum_{\ell=2}^{L} \beta^J_{2\ell} y^\ell_{it} + \eta^J_i + \epsilon^J_{it}, \tag{5.15}$$

and test the hypothesis $H_0 : \beta^J_{22} = \cdots = \beta^J_{2L} = 0$ (note that y^1_{it} is excluded from the second summation), using standard panel data techniques. This test is easier to perform than (5.14), since it allows the use of standard statistical packages. But contrary to (5.8) equation (5.15) is more restrictive under the null hypothesis: We assume that the slope parameters do not vary across months. We shall estimate and test (5.15) for different groups g, using the subpanel of observations i, with $g_i = g$, for whom $s_{it} > 0$ for at least two different months. Just like the test for (5.12), we can easily extend this benchmark test by including higher order (cross) terms of y^ℓ_{it} and cross terms of y^ℓ_{it} and $\sum_\ell y^\ell_{it}$ in regression (5.15).

Finally, we will estimate specification (5.15) and its extensions using annual aggregates instead of the monthly data, and test the corresponding null hypothesis. We shall estimate and test this alternative specification for different groups g, using now the subpanel of observations i, with $g_i = g$, for whom $s_{it} > 0$ for at least one month in each of the two different years.

5.4 Empirical results

5.4.1 Estimation results

As a first step we estimate equation (5.2) for the individuals belonging to $I_{g,t,T}$, with g the group of individuals employed at the Insurer, t the months January to December

2007, and $T = 12$ months, using the nonparametric estimation procedure described in the previous section. In the original Linton and Nielsen (1995) estimator, the corresponding confidence band is based on the assumption of homoskedasticity. We follow Vollebergh et al. (2009) by extending the asymptotic limit distribution by also allowing for the possibility of heteroskedasticity. Figures 5.1 to 5.12 show the resulting nonparametric estimates, with the corresponding 95% pointwise confidence bands. Because the level is not identified in this nonparametric estimation, the level per figure is fixed at the mean of the corresponding dependent variable in the year 2006. In the same figures we also present the estimations of the parametric specifications (5.8) for $J = 1, 2, 3$, using OLS applied to (5.9). The left panels of each figure shows the outcomes for 2006, the right panels for 2007. The upper panels present the comparison between the nonparametric estimates and the parametric ones for $J = 1$, the middle panels show the corresponding outcomes for $J = 2$, while the bottom panels show the outcomes for $J = 3$. The level of the parametric curves is determined such that this curve equals (or crosses) the corresponding nonparametric curve at the median value of the income variable.

As is clear from these figures, there is a month effect, as the shape of the curves changes over the months. For instance, the shape is clearly decreasing in January, but increasing in February, while the curve has a U-shape in March. In general, one would expect an increasing relationship between the amount saved in the LCS and income. The negative relationship in January might be related to a new year's resolution to save more in the new year, if such a resolution is more prevalent among low income employees.

In a number of cases, there is also a combination of a year and a month effect, as is illustrated by, for example, the results of February and December: the shape of the nonparametric estimation of February in 2006 is increasing and concave, while in 2007 the shape is increasing and convex. For December, the shape changes from inverted U-shaped in 2006 to U-shaped in 2007. In addition, the graphs show that in most cases at least one (and sometimes more than one) of the parametric specifications ($J = 1, 2, 3$) fits inside the nonparametric confidence band (possibly after an additional change in the level, deviating from the median level, used to plot the graphs). For these months the class of parametric models seems flexible enough to fit the data sufficiently well. But there are also some clear exceptions, in particular, February, April, and October. For these months, the curvature imposed by the chosen parametric specifications seems to be too specific, preventing a good fit of the data.

5.4.2 Test results

Estimation results for equation (5.15) are presented in Tables 5.2 and 5.3, for the Bank and the Insurer, respectively. The time specific constant β_{0t}^J is decomposed into a year and month effects. For brevity, results are only reported for $J = 1$. The first column in each table presents the results for all employees; the other columns consider groups by gender, by age being below or above 45, and by total income being below or above the monthly median. The results do not vary much across groups. Tables 5.4 and 5.5 present results for the same specification, but with annual instead of monthly data. We exclude base salary as individual income component (i.e., base salary is y_{it}^ℓ in terms of (5.15)). Moreover, for the group of individuals employed at the Insurer and with income below the median, the income component "bonus" is never positive for the included individuals, and therefore excluded from the regressions.

The framing test results for equation (5.15), $J = 1$, are found in the bottom rows of tables 5.2 and 5.3 (monthly data) and 5.4 and 5.5 (annual data). For the Bank equality of the mpss is rejected in all cases. For the Insurer the equality of the mpss is not rejected, except for the group with income below median (annual data, but also almost so for monthly data). For the Bank "holidays allowance" and "LCS contribution" are the income components that most significantly affect savings contributions. For the Insurer, very few individual coefficients are significant.

The results using annual and monthly data for equation (5.15) are qualitatively similar but the magnitudes of the marginal propensities to save differ. This is not surprising given that they are different aggregations of the monthly results displayed in Figures 5.1 to 5.12.

In the cases $J = 2$ and $J = 3$ we also include all higher order terms and cross terms of the individual income components as well as the cross terms of the individual income components and total income up to order J. Table 5.6 summarizes the test results for $J = 1, 2, 3$ (with $J = 1$ following from tables 5.3 and 5.5). Equality of mpss is rejected in all cases, except for the Insurer in case $J = 1$.

Next, we present the outcomes for the nonparametric equation (5.2) based on test statistic (5.14). Table 5.7 reports the test outcomes for the Insurer, for the month couples January 2006/2007 to December 2006/2007, for $J = 1, 2, 3$, for each of the income components separately, and for all income components together. We only present the results when including the first order terms in (5.12). Including higher order (cross) terms yields similar results. We find that the null hypothesis of no framing effect is systematically rejected at the 5% significance level for the income component "other income," for the months May

Table 5.2: Bank: contributions into LCS with monthly data ($J = 1$)

	All	Female	Male	Age < 45	Age ≥ 45	Below median	Above median
Total income	0.030	0.118***	−0.015	0.036	0.039	0.030	0.034
	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)
Holiday allowance	0.166***	0.077**	0.211***	0.091***	0.223***	0.164***	0.168***
	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)	(0.04)
13th month	0.088**	−0.025	0.136**	0.034	0.097	0.080	0.031
	(0.04)	(0.06)	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)
Profit share	−0.058**	−0.156***	−0.009	−0.046	−0.119**	−0.020	−0.057
	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)	(0.04)
Overtime	−0.027	−0.141**	0.039	−0.001	−0.047	−0.056	−3.429***
	(0.04)	(0.06)	(0.06)	(0.07)	(0.08)	(0.05)	(0.45)
Bonus	−0.000	−0.129***	0.063*	−0.018	0.004	−0.001	−0.002
	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)	(0.03)
Labeled payment	0.534***	0.375***	0.621***	0.498***	0.563***	0.550***	0.609***
	(0.06)	(0.08)	(0.07)	(0.06)	(0.10)	(0.06)	(0.08)
Other income	−0.044	−0.125*	0.001	−0.103***	−0.007	−0.120	−0.049
	(0.04)	(0.07)	(0.05)	(0.04)	(0.07)	(0.07)	(0.05)
Benefit budget	0.146***	0.071	0.170**	0.151***	0.108	0.132**	−0.031
	(0.05)	(0.07)	(0.07)	(0.06)	(0.09)	(0.06)	(0.09)
February	66.974	32.266	61.176	−6.614	35.926	97.771	−216.496
	(86.82)	(112.35)	(151.06)	(92.13)	(150.65)	(119.91)	(283.19)
March	217.747**	167.156	221.403	175.873*	142.228	165.881	31.150
	(87.45)	(117.10)	(150.88)	(97.72)	(148.52)	(120.84)	(285.96)
April	75.618	148.575	4.003	8.112	37.471	107.261	−242.799
	(91.74)	(116.05)	(159.48)	(103.71)	(155.95)	(123.20)	(294.79)
May	33.914	9.413	18.955	−71.937	144.376	9.839	−300.188
	(87.95)	(112.24)	(151.72)	(104.41)	(142.78)	(123.38)	(284.23)
June	74.046	38.499	66.351	25.167	9.298	108.347	−232.581
	(85.51)	(114.15)	(148.89)	(95.03)	(145.73)	(120.71)	(281.18)
July	9.052	2.762	−20.783	−38.938	−54.229	64.563	−346.514
	(86.73)	(112.35)	(151.28)	(93.35)	(149.76)	(119.96)	(285.78)
August	84.545	38.114	86.241	20.623	44.907	110.983	−211.288
	(86.48)	(115.07)	(150.57)	(95.12)	(148.49)	(120.92)	(284.10)
September	81.885	36.750	81.905	15.068	40.836	104.358	−208.959
	(87.15)	(114.05)	(152.08)	(94.86)	(150.68)	(120.76)	(286.06)
October	82.073	40.410	79.896	15.598	39.895	99.952	−200.930
	(87.20)	(115.06)	(151.72)	(95.32)	(149.92)	(120.84)	(285.42)
November	149.083*	86.292	162.281	97.064	82.298	127.216	−76.683
	(87.88)	(115.68)	(152.31)	(96.73)	(150.83)	(120.97)	(286.06)
December	127.461	83.286	127.544	48.047	105.815	115.129	−110.885
	(87.95)	(115.76)	(152.78)	(95.67)	(151.58)	(120.85)	(286.99)
Year 2007	−4.932	−41.330	43.233	−43.637	57.761	−26.260	271.570**
	(32.13)	(31.07)	(60.78)	(26.92)	(61.97)	(29.59)	(105.61)
Constant	144.767	−152.164	391.152**	75.481	306.490	35.830	528.680
	(124.71)	(154.45)	(185.64)	(121.74)	(249.39)	(139.51)	(310.93)
R^2	0.068	0.070	0.070	0.053	0.091	0.061	0.077
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N	19138	8000	11138	11377	8234	10984	8154

The dependent variable is the amount of contributed to the LCS scheme. Employee fixed effects are included. Robust standard errors clustered at the employee-level are presented in parentheses. The p-value pertains to testing whether all income coefficients, except for 'Total income', are jointly equal to zero. */**/*** correspond to 10%/5%/1% significance level.

Table 5.3: Insurer: contributions into LCS with monthly data ($J = 1$)

	All	Female	Male	Age < 45	Age ≥ 45	Below median	Above median
Total income	0.034	−0.015	0.032	−0.061	0.186	0.036	0.001
	(0.06)	(0.02)	(0.07)	(0.04)	(0.12)	(0.02)	(0.07)
Holiday allowance	−0.027	0.014	−0.025	0.065	−0.171	−0.034	−0.005
	(0.06)	(0.02)	(0.07)	(0.04)	(0.11)	(0.03)	(0.07)
13th month	−0.026	0.015	−0.022	0.063	−0.175	−0.036	0.013
	(0.06)	(0.02)	(0.07)	(0.04)	(0.12)	(0.02)	(0.08)
Profit share	−0.031	0.015	−0.029	0.064	−0.186	−0.037	0.000
	(0.06)	(0.02)	(0.07)	(0.04)	(0.12)	(0.02)	(0.07)
Overtime	−0.126	−0.018	−0.142	0.023	−0.277**	−0.101	−0.275
	(0.08)	(0.03)	(0.10)	(0.05)	(0.14)	(0.06)	(0.23)
Bonus	−0.035	0.007	−0.032	0.059	−0.187		0.000
	(0.06)	(0.02)	(0.07)	(0.04)	(0.12)		(0.07)
Other income	0.011	0.015	0.022	0.057	−0.070	−0.016	0.045
	(0.06)	(0.02)	(0.08)	(0.04)	(0.16)	(0.02)	(0.08)
February	12.014	48.871	−0.389	−14.578	23.473	29.716*	−7.458
	(18.98)	(30.83)	(18.75)	(31.28)	(23.82)	(16.77)	(25.71)
March	0.822	6.234	−2.538	−7.282	4.840	3.129	−6.024
	(19.70)	(15.57)	(21.32)	(31.52)	(23.22)	(12.06)	(28.90)
April	−7.132	1.838	−12.756	−15.668	−11.312	2.567	−19.741
	(20.13)	(13.28)	(22.10)	(31.39)	(27.31)	(12.07)	(30.09)
May	−17.848	−3.298	−19.117	−21.483	−40.435	−11.863	50.091
	(19.14)	(7.37)	(24.11)	(26.66)	(29.64)	(14.97)	(42.89)
June	−9.589	−6.427*	−11.120	−20.744	−18.696	−14.228**	−0.189
	(25.35)	(3.58)	(29.16)	(30.12)	(38.21)	(5.65)	(43.28)
July	1.628	−5.497	2.335	−18.649	−3.221	−8.488	17.470
	(23.01)	(3.65)	(25.98)	(30.00)	(32.27)	(5.61)	(39.09)
August	−8.529	−5.692	−12.228	−27.262	1.140	−4.988	13.223
	(23.24)	(3.81)	(27.68)	(32.80)	(27.65)	(9.24)	(40.85)
September	3.166	−5.891	4.325	−18.120	−0.390	−6.502	18.926
	(24.00)	(3.70)	(27.35)	(29.92)	(33.26)	(6.33)	(41.43)
October	3.557	−6.131	4.675	−17.019	−1.202	−6.313	19.979
	(24.18)	(3.72)	(27.65)	(30.31)	(33.91)	(6.06)	(41.90)
November	−28.237	−3.894	−40.664*	−24.138	−46.970	−8.839	−53.251
	(19.78)	(5.25)	(24.46)	(26.30)	(32.73)	(6.72)	(53.56)
December	−4.038	−4.641	−3.911	−15.037	−19.074	−7.620	7.877
	(25.49)	(3.75)	(29.50)	(30.61)	(39.38)	(5.31)	(44.44)
Year 2007	24.760	0.083	34.412	−0.615	21.670	7.751	56.911
	(15.63)	(6.99)	(21.54)	(13.44)	(23.62)	(8.36)	(36.43)
Constant	307.764	270.565***	374.579	504.162***	−199.788	210.385***	583.807
	(246.43)	(45.92)	(325.65)	(175.80)	(483.54)	(58.39)	(403.08)
R^2	0.038	0.037	0.050	0.063	0.127	0.024	0.055
p-value	0.174	0.191	0.262	0.244	0.086	0.054	0.321
N	3036	740	2296	1294	1859	1582	1454

The dependent variable is the amount of contributed to the LCS scheme. Employee fixed effects are included. Robust standard errors clustered at the employee-level are presented in parentheses. The p-value pertains to testing whether all income coefficients, except for 'Total income', are jointly equal to zero. */**/*** correspond to 10%/5%/1% significance level.

Table 5.4: Bank: contributions into LCS with annual data ($J = 1$)

	All	Female	Male	Age < 45	Age ≥ 45	Below median	Above median
Total income	−0.019	0.019	−0.049	0.004	−0.045	0.001	−0.012
	(0.02)	(0.02)	(0.03)	(0.01)	(0.06)	(0.02)	(0.04)
Holiday allowance	0.833***	1.005***	0.763**	0.306	1.253***	0.441	1.001***
	(0.26)	(0.31)	(0.32)	(0.19)	(0.41)	(0.27)	(0.32)
13th month	0.095	−0.060	0.150	0.110*	0.097	0.029	0.101
	(0.10)	(0.08)	(0.14)	(0.07)	(0.22)	(0.08)	(0.14)
Profit share	0.061	−0.388*	0.241*	0.081	−0.115	0.190	−0.013
	(0.13)	(0.23)	(0.14)	(0.16)	(0.23)	(0.19)	(0.15)
Overtime	0.218***	0.243**	0.357	0.140	0.260	0.152**	2.383
	(0.07)	(0.11)	(0.25)	(0.17)	(0.19)	(0.06)	(1.53)
Bonus	0.054	−0.063	0.120*	−0.009	0.160	0.072	0.033
	(0.05)	(0.05)	(0.07)	(0.04)	(0.12)	(0.06)	(0.07)
Labeled payment	2.065	−1.258	5.186*	1.768**	3.749	1.066	2.594
	(1.73)	(1.52)	(2.88)	(0.81)	(4.84)	(0.97)	(2.83)
Other income	0.075	0.146	0.087	0.033	0.078	0.055	0.053
	(0.09)	(0.18)	(0.11)	(0.07)	(0.32)	(0.08)	(0.11)
Benefit budget	0.382***	0.334***	0.482***	0.220***	0.576**	0.225*	0.324**
	(0.10)	(0.12)	(0.14)	(0.08)	(0.26)	(0.12)	(0.16)
year 2007	360.255	−164.547	700.193	40.633	521.711	−50.282	2374.061**
	(262.00)	(265.29)	(512.26)	(224.52)	(599.45)	(257.24)	(943.09)
Constant	677.010	694.011	732.548	372.552	1489.631	550.993	−45.060
	(716.87)	(485.82)	(977.15)	(410.43)	(1692.35)	(433.25)	(1350.34)
R^2	0.074	0.135	0.079	0.098	0.083	0.072	0.086
p-value	0.000	0.000	0.000	0.019	0.010	0.037	0.001
N	3224	1334	1890	1950	1373	1612	1612

The dependent variable is the amount of contributed to the LCS scheme. Employee fixed effects are included. Robust standard errors clustered at the employee-level are presented in parentheses. The p-value pertains to testing whether all income coefficients, except for 'Total income', are jointly equal to zero. */**/*** correspond to 10%/5%/1% significance level.

Table 5.5: Insurer: contributions into LCS with annual data ($J = 1$)

	All	Female	Male	Age < 45	Age ≥ 45	Below median	Above median
Total income	0.065	0.003	0.063	0.024	0.194	0.034	−0.019
	(0.10)	(0.07)	(0.15)	(0.03)	(0.24)	(0.09)	(0.13)
Holiday allowance	1.119	0.364	1.326	0.522	0.253	2.336***	0.307
	(0.96)	(0.68)	(1.53)	(0.41)	(1.87)	(0.86)	(1.08)
13th month	0.261	0.194	0.303	0.054	−0.072	−0.156	0.398
	(0.24)	(0.32)	(0.31)	(0.14)	(1.14)	(0.35)	(0.24)
Profit share	0.117	0.268	0.205	0.073	0.620	0.027	0.236
	(0.29)	(0.43)	(0.37)	(0.10)	(0.87)	(0.41)	(0.34)
Overtime	0.348	0.569	0.254	0.101	0.398	0.460	0.575
	(0.39)	(0.47)	(0.49)	(0.08)	(0.54)	(0.41)	(0.61)
Bonus	0.356	−0.950	0.370	0.088	0.734		0.434*
	(0.24)	(0.83)	(0.30)	(0.10)	(0.63)		(0.26)
Other income	0.545	0.039	0.690	−0.039	0.670	0.300	0.631
	(0.38)	(0.14)	(0.51)	(0.06)	(0.72)	(0.31)	(0.44)
year 2007	587.922**	658.947***	335.924	201.840	430.421	414.143	1423.461***
	(251.84)	(244.65)	(370.37)	(137.13)	(667.36)	(304.53)	(483.34)
Constant	−6442.468***	−626.666	−7885.586***	−1498.061	−11782.532***	−5063.400***	644.198
	(2372.45)	(1366.65)	(2455.26)	(897.43)	(2972.40)	(1771.59)	(6025.29)
R^2	0.355	0.257	0.395	0.251	0.479	0.600	0.316
p-value	0.426	0.554	0.399	0.231	0.411	0.018	0.231
N	360	96	264	160	212	180	180

The dependent variable is the amount of contributed to the LCS scheme. Employee fixed effects are included. Robust standard errors clustered at the employee-level are presented in parentheses. The p-value pertains to testing whether all income coefficients, except for 'Total income', are jointly equal to zero. */**/*** correspond to 10%/5%/1% significance level.

Table 5.6: Test results equation (5.15)

		Monthly data			Annual data		
		All	Bank	Insurer	All	Bank	Insurer
J=1	F-stat	27.22	30.49	1.52	4.71	4.29	1.00
	p-value	0.000	0.000	0.174	0.000	0.000	0.426
J=2	F-stat	91.26	57.80	2.85	4.46	4.31	8.82
	p-value	0.000	0.000	0.000	0.000	0.000	0.000
J=3	F-stat	3.29	3.29	95.75	18.90	49.20	78.40
	p-value	0.000	0.000	0.000	0.000	0.000	0.000

up to and including December. For the months May to November rejections are found for all three parametric specifications ($J = 1, 2, 3$), while for December it only applies to $J = 2, 3$ (where $J = 2$ seems to give a fit in line with the nonparametric estimate). Moreover, the null hypothesis of no framing effect is also rejected at the 5% significance level for the income component "base salary," for the months May, July, September, October, and November ($J = 1, 2, 3$), and June and August ($J = 3$). The null hypothesis is not rejected for these two income components for the other months, and also not rejected for any of the other income components for the time period under consideration. So, assuming that the employees working at the Insurer form a homogeneous group, the conclusion seems to be that there is a framing effect, in line with the evidence summarized in Table 5.6, particularly for $J = 2, 3$.

In Table 5.8 we present the corresponding outcomes for the Bank. We only find a few rejections of the null hypothesis of no framing effect at the 5% significance level. For the month July we find a rejection of the null hypothesis for the income component "overtime" ($J = 1, 2, 3$), and for the month August we find a rejection of the null hypothesis for the income component "bonus" ($J = 1, 2, 3$). Moreover, for the months July and November we find a rejection of the null hypothesis for the income component "base salary" ($J = 3$). For the simultaneous test, we find clear rejections of the null hypothesis in case of the months August and November.

These results are not fully in line with the results summarized in Table 5.6, indicating that the parametric estimation and test results might be driven by misspecification. To investigate whether this is the case, Figures 5.13–5.14 compare the parametric and nonparametric estimation results for g = Bank for the months August and November, for which the simultaneous tests reject the null hypothesis. The figures show that in these two months the three parametric specifications have some difficulty to yield an appropriate fit (in line with the nonparametric estimates), with as only exception August for $J = 2$, where after some shifting a reasonable fit seems to be possible. But for this case the p-value of the simultaneous test is only 0.10 (although for the income component "bonus" it equals 0.03). Thus, we cannot exclude the possibility that the rejections of the null hypotheses for the Bank are partly due to misspecification.

In sum, we find evidence for framing effects in most cases, but the type of deviations from the null of equal marginal propensities to save varies across specifications. For example, when the shape of the savings-income relationship is not allowed to vary across months evidence of framing is found more often for the Bank than for the Insurer. The

reverse pattern is found when the shape of the savings-income relationship is allowed to vary across months. The former result is likely to be partly driven by misspecification. The latter result is based on a much more flexible specification and therefore more likely to reflect genuine framing effects.

5.5 Conclusion

In this study we have investigated framing effects in an employee savings scheme, using specifications that differ in functional form flexibility and in the time aggregation of the data. In most cases we find evidence that marginal propensities to save differ across income components.

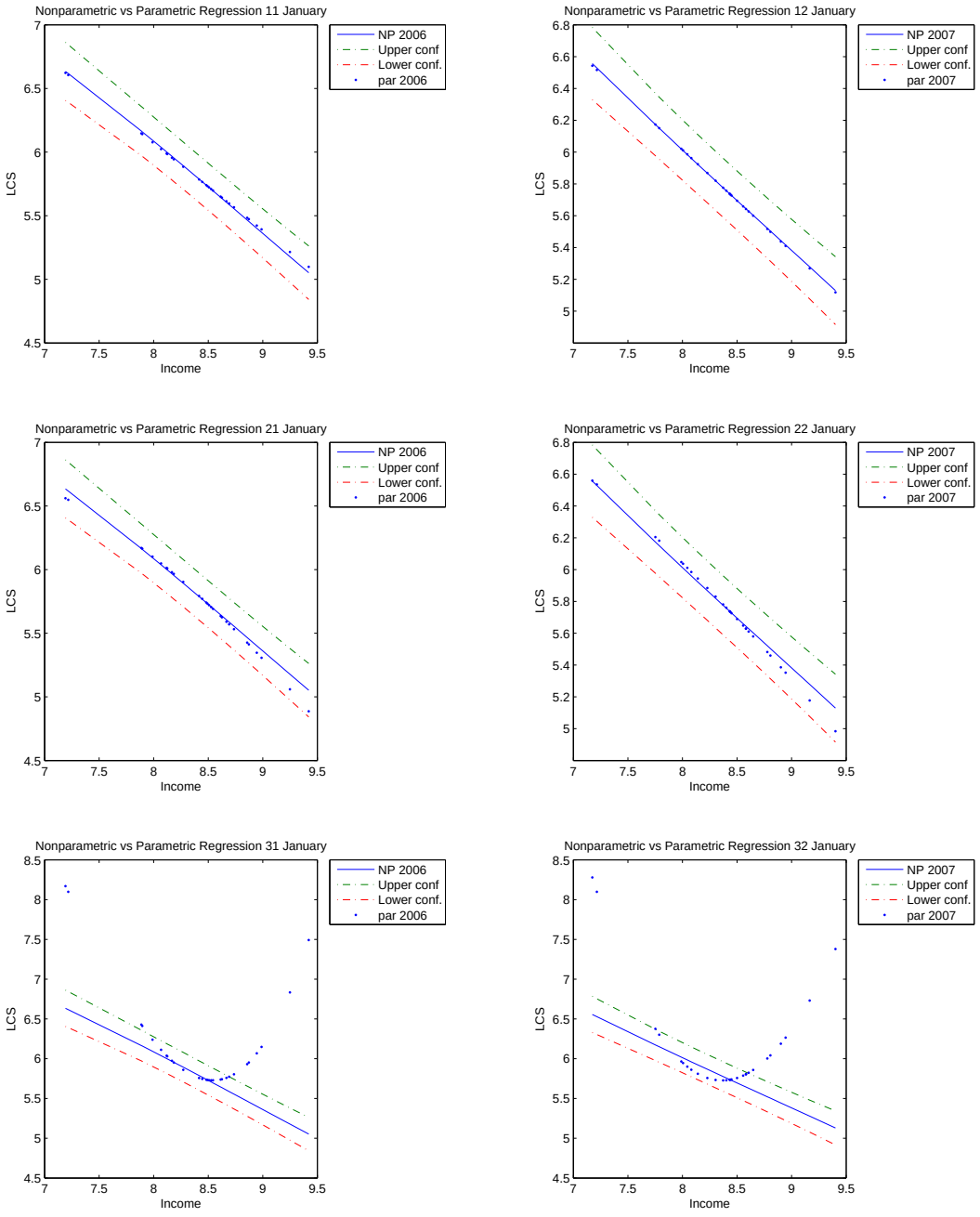
Our analysis reveals a large degree of heterogeneity in savings behavior within the year. Not only the curvature, but also the sign of the relationship between total income and savings appears to vary across months. While we find the expected positive relationship for 22 out the 24 months our data apply to, the relationship appears to be negative for January 2006 and January 2007.

The strongest evidence for a pure labeling effect is found for employees of the Insurer (Table 5.7). Even though base salary and other income are paid with identical frequency and timing (monthly) the marginal propensities to save out of these two income components differ for most of the months. This remains undetected when using the more restrictive specifications or the annual aggregation of the data.

Employees at the Bank and the Insurer have higher education levels on average than the general population and are likely to be more financially literate. The degree of framing reported here is therefore likely to be a lower bound of the framing effects that would be found in the general population.

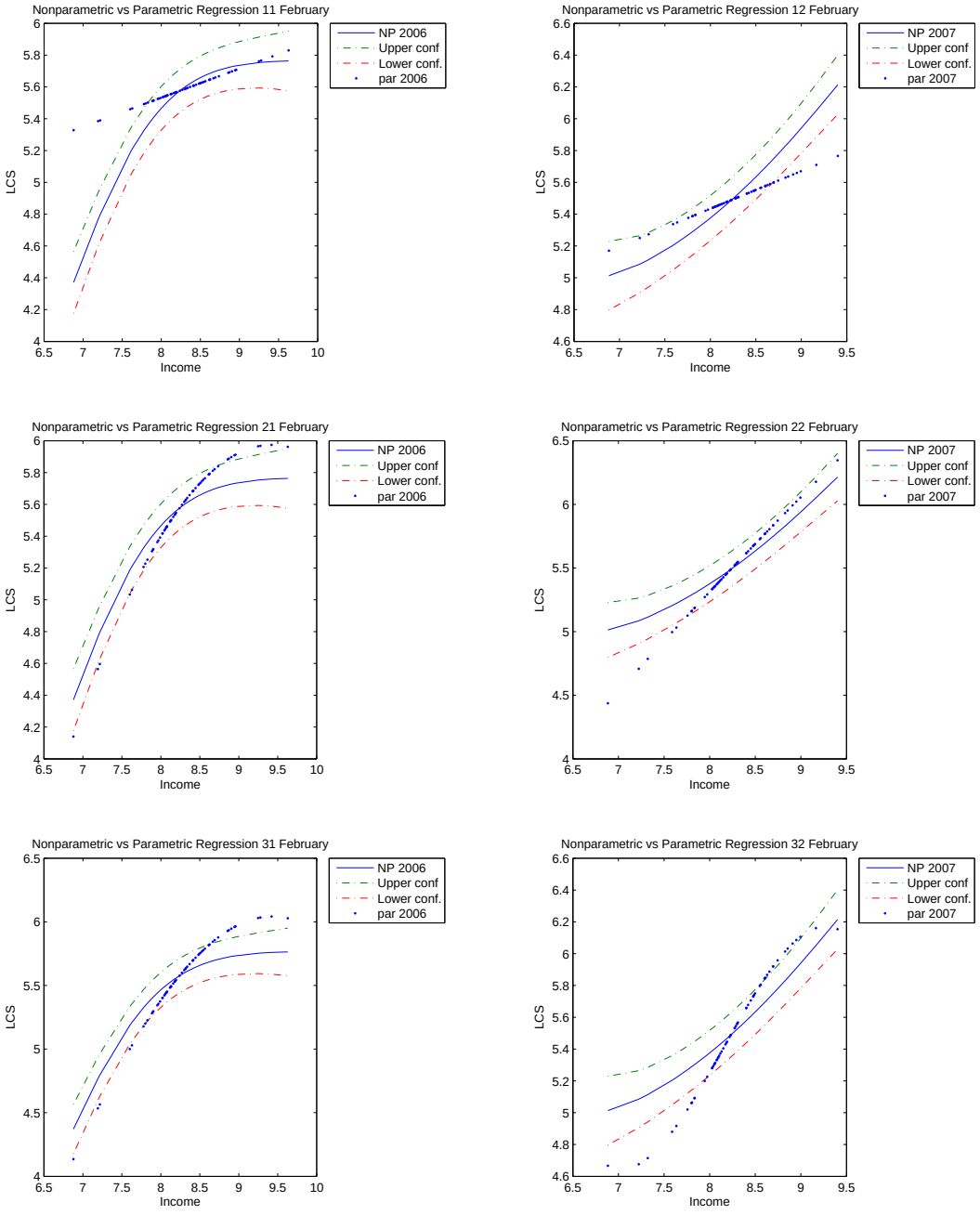
The heterogeneity within the year helps to clarify the pathways that lead to framing effects. The atypical result for January suggests that savings contributions do not only depend on the label of an income component and the frequency with which it is paid, but also on the calendar timing of these payments. Thus employers seem to have several low cost instruments for affecting savings rates: changing the labels of income components, changing their relative sizes, changing payment frequencies, and changing the calendar timing of payments.

Figure 5.1: Insurer: parametric versus nonparametric in January



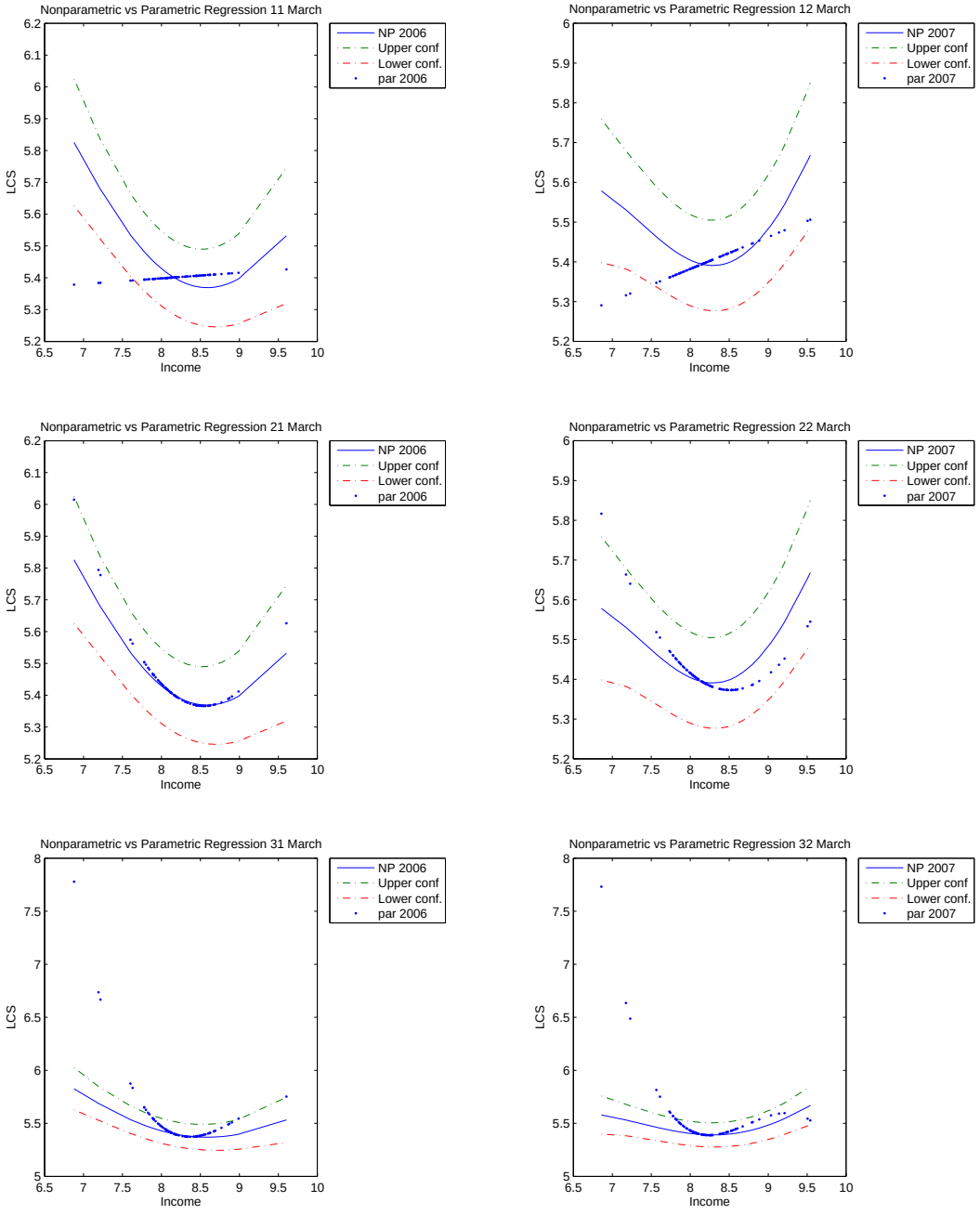
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.2: Insurer: parametric versus nonparametric in February



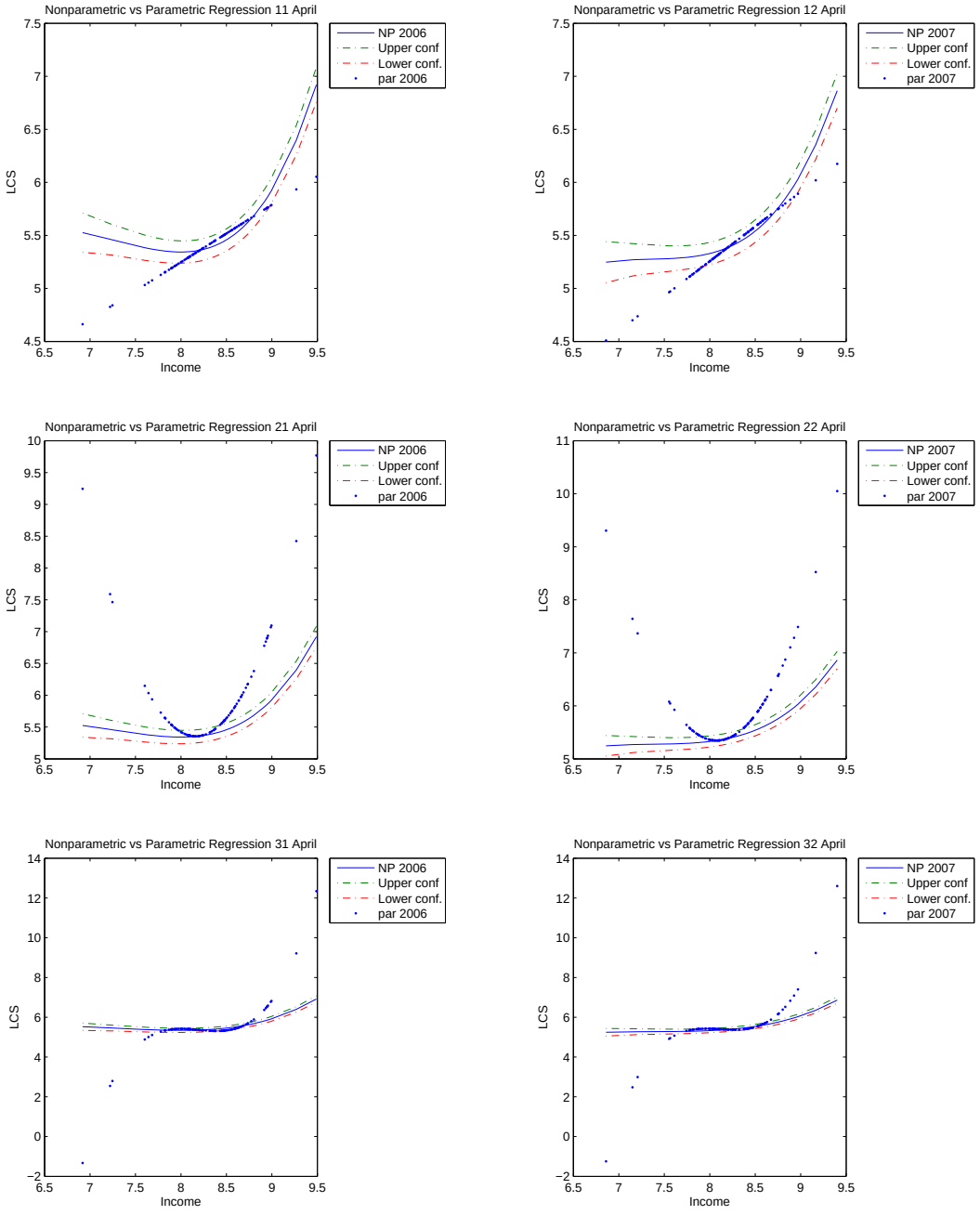
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.3: Insurer: parametric versus nonparametric in March



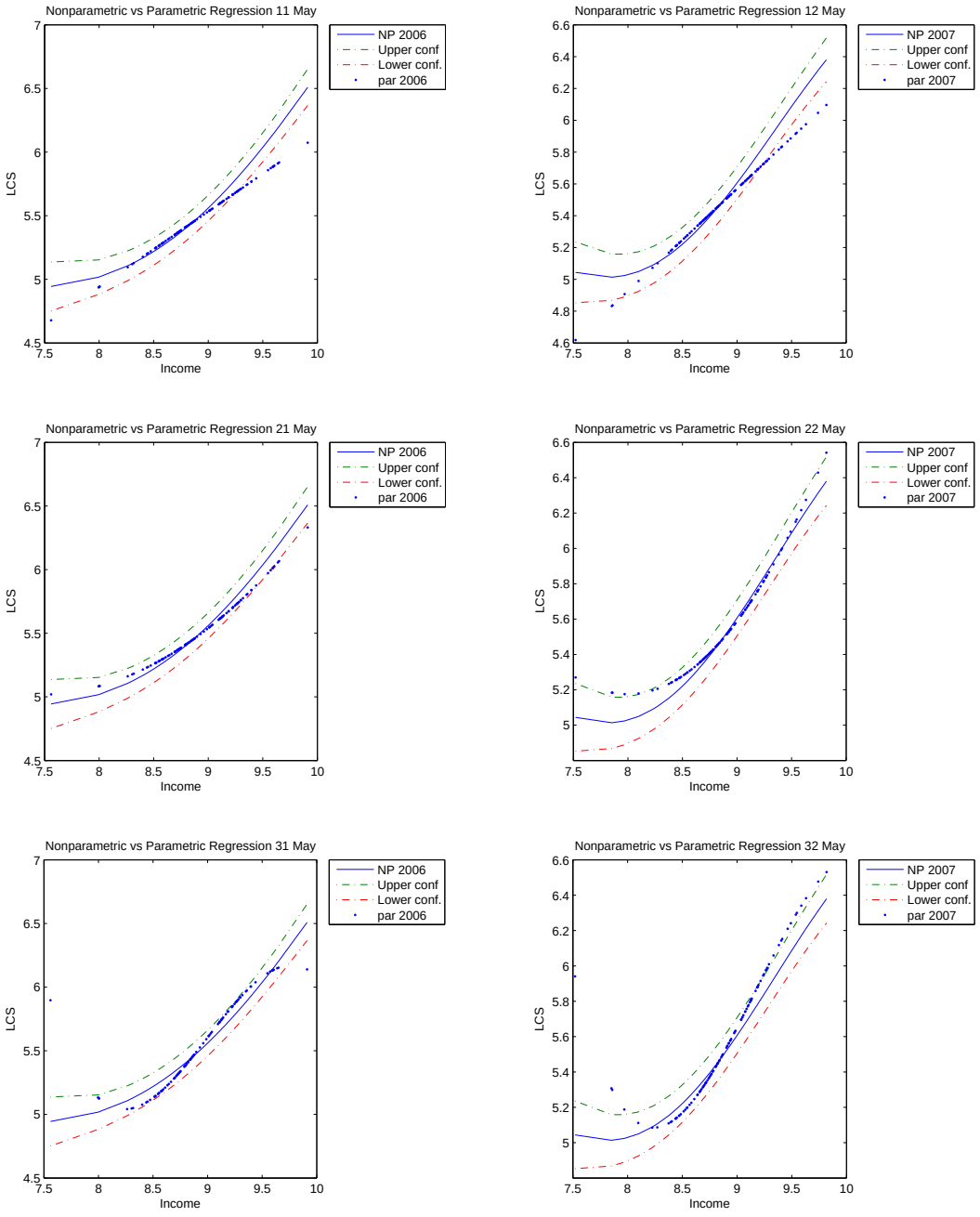
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.4: Insurer: parametric versus nonparametric in April



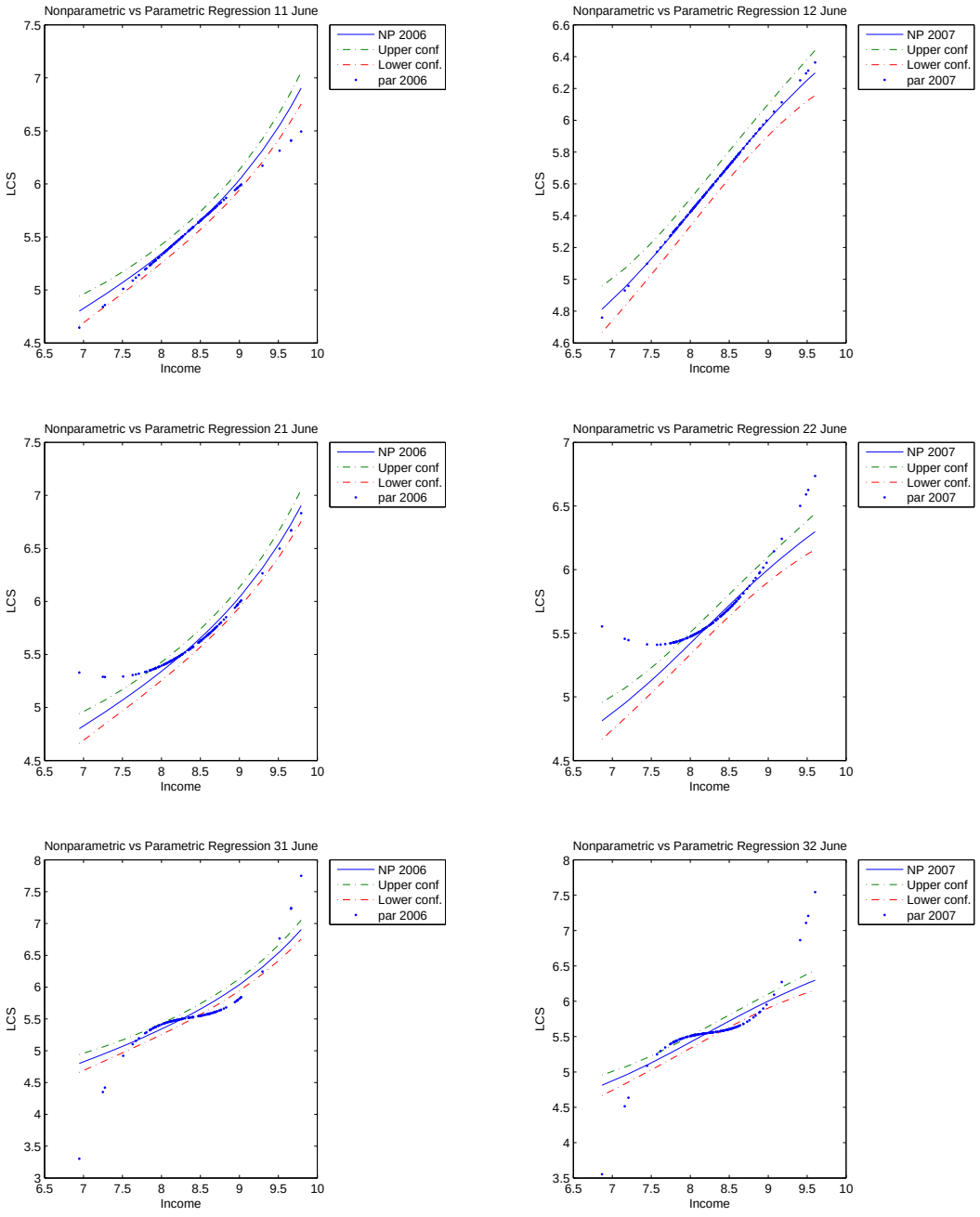
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.5: Insurer: parametric versus nonparametric in May



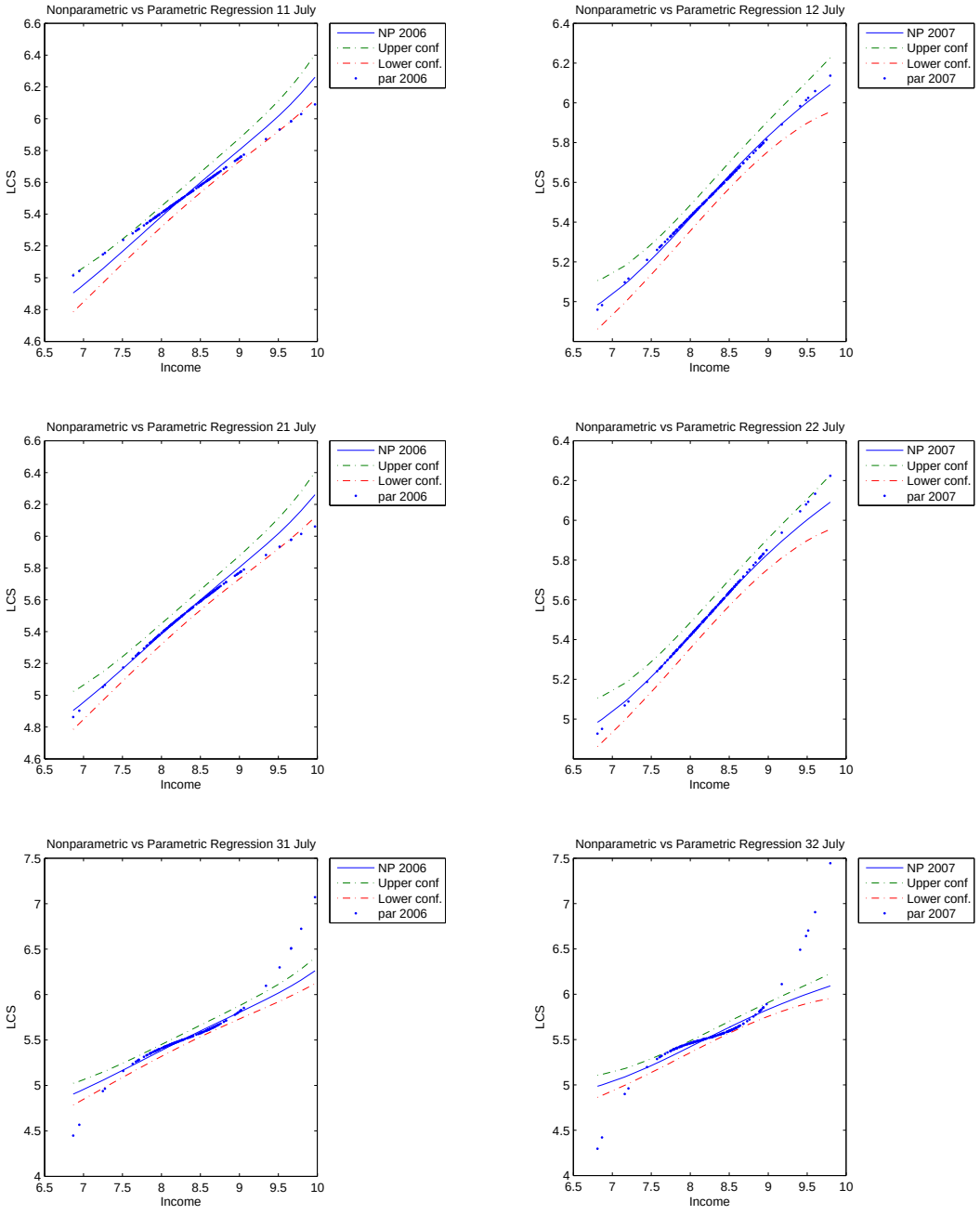
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.6: Insurer: parametric versus nonparametric in June



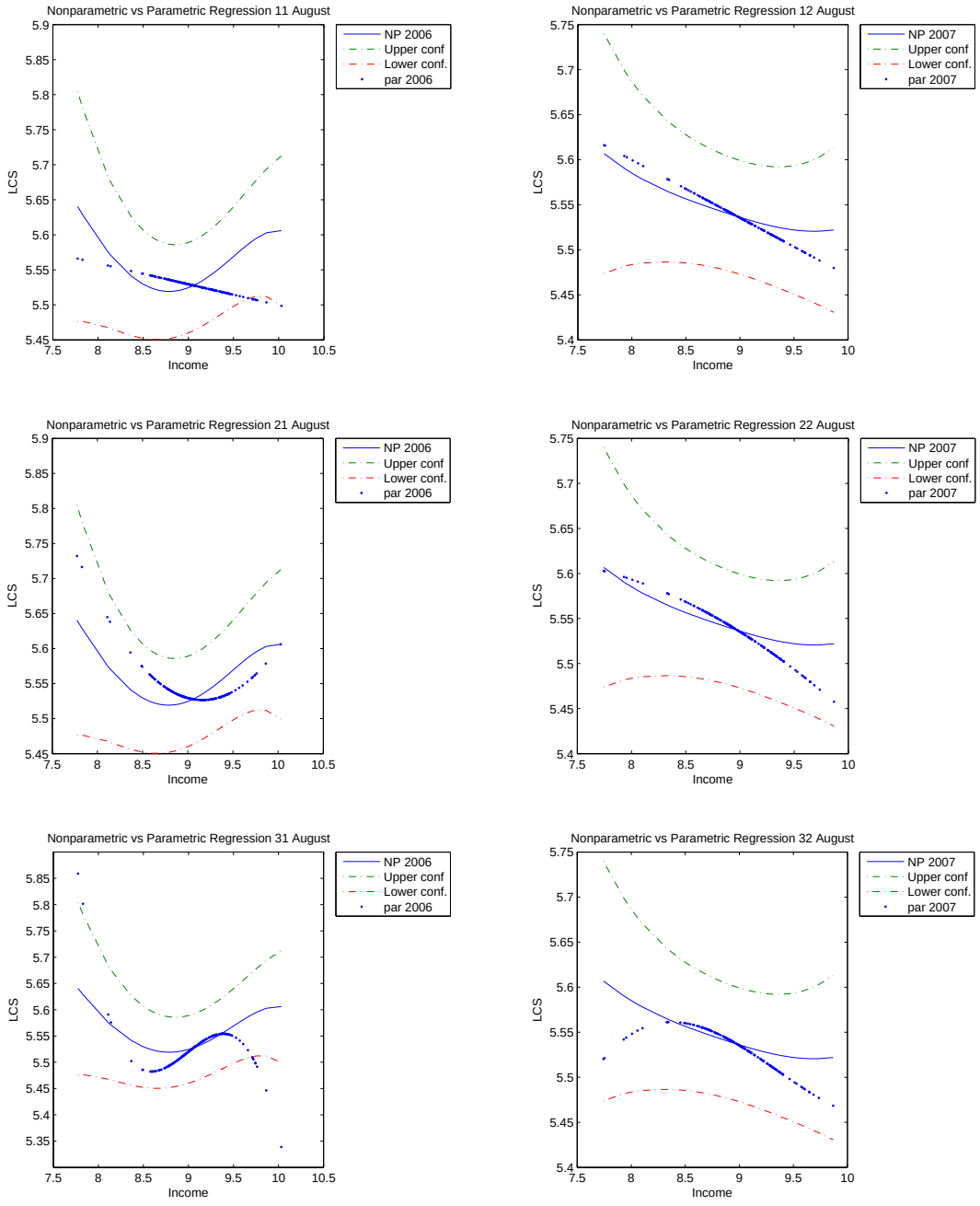
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.7: Insurer: parametric versus nonparametric in July



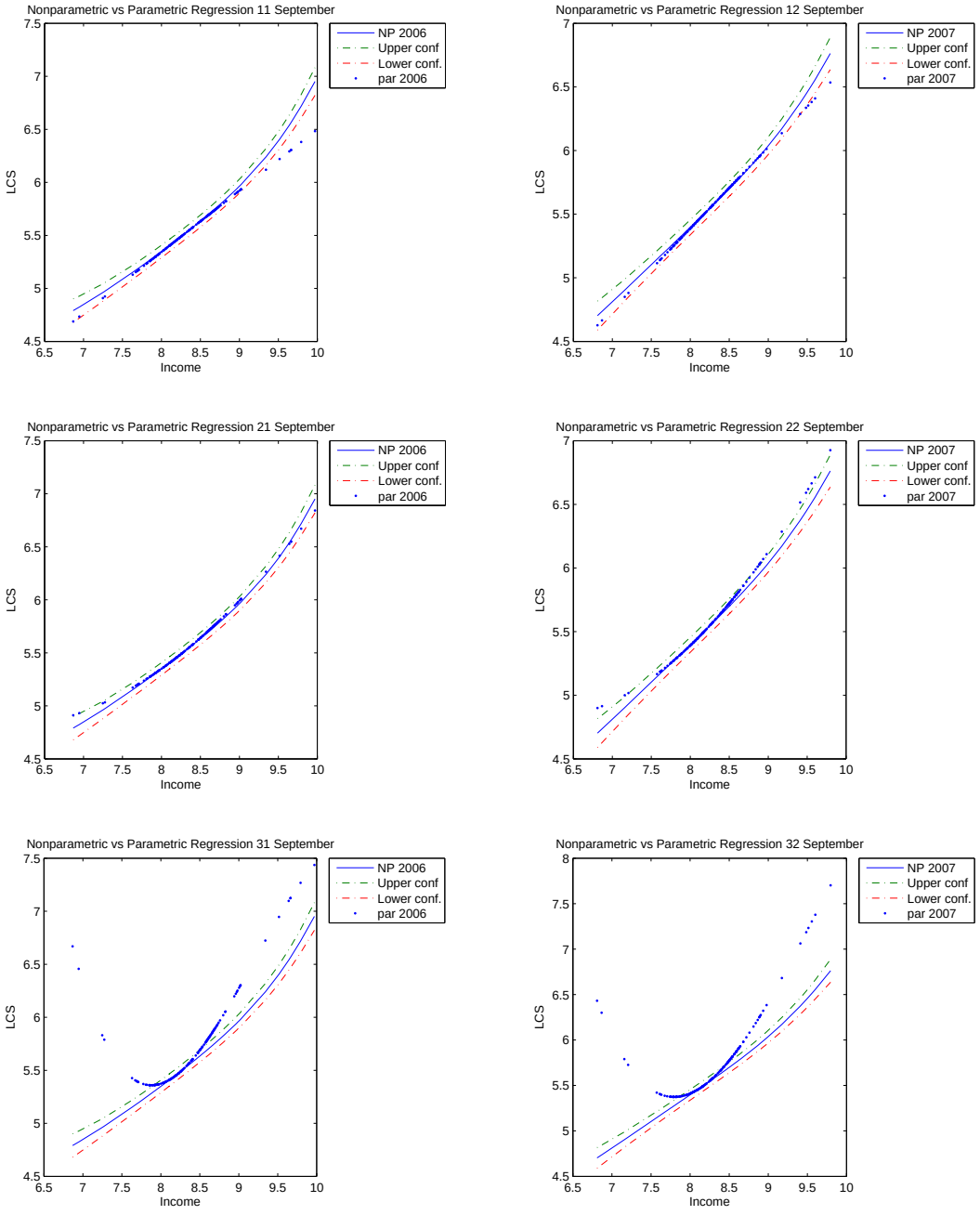
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.8: Insurer: parametric versus nonparametric in August



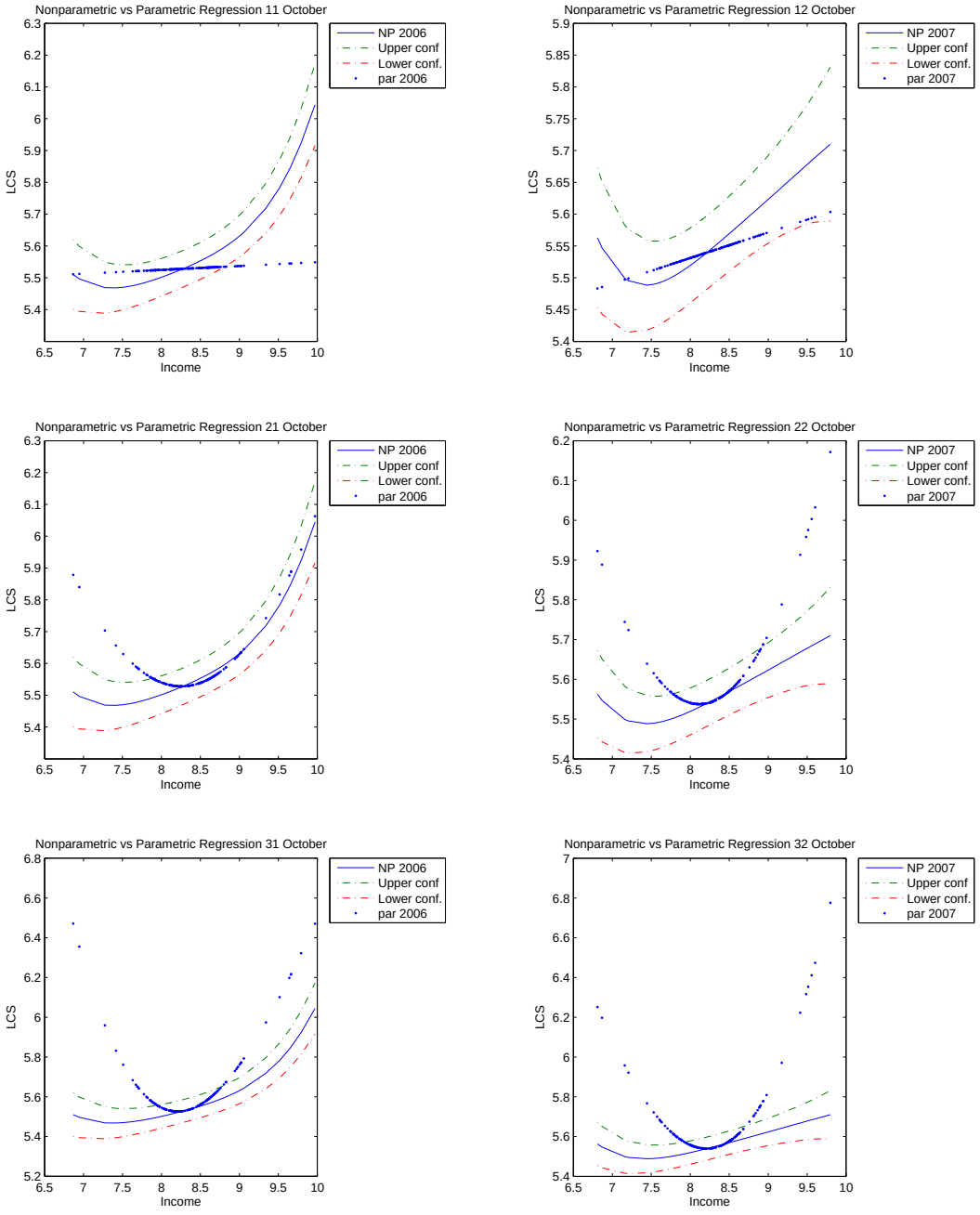
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.9: Insurer: parametric versus nonparametric in September



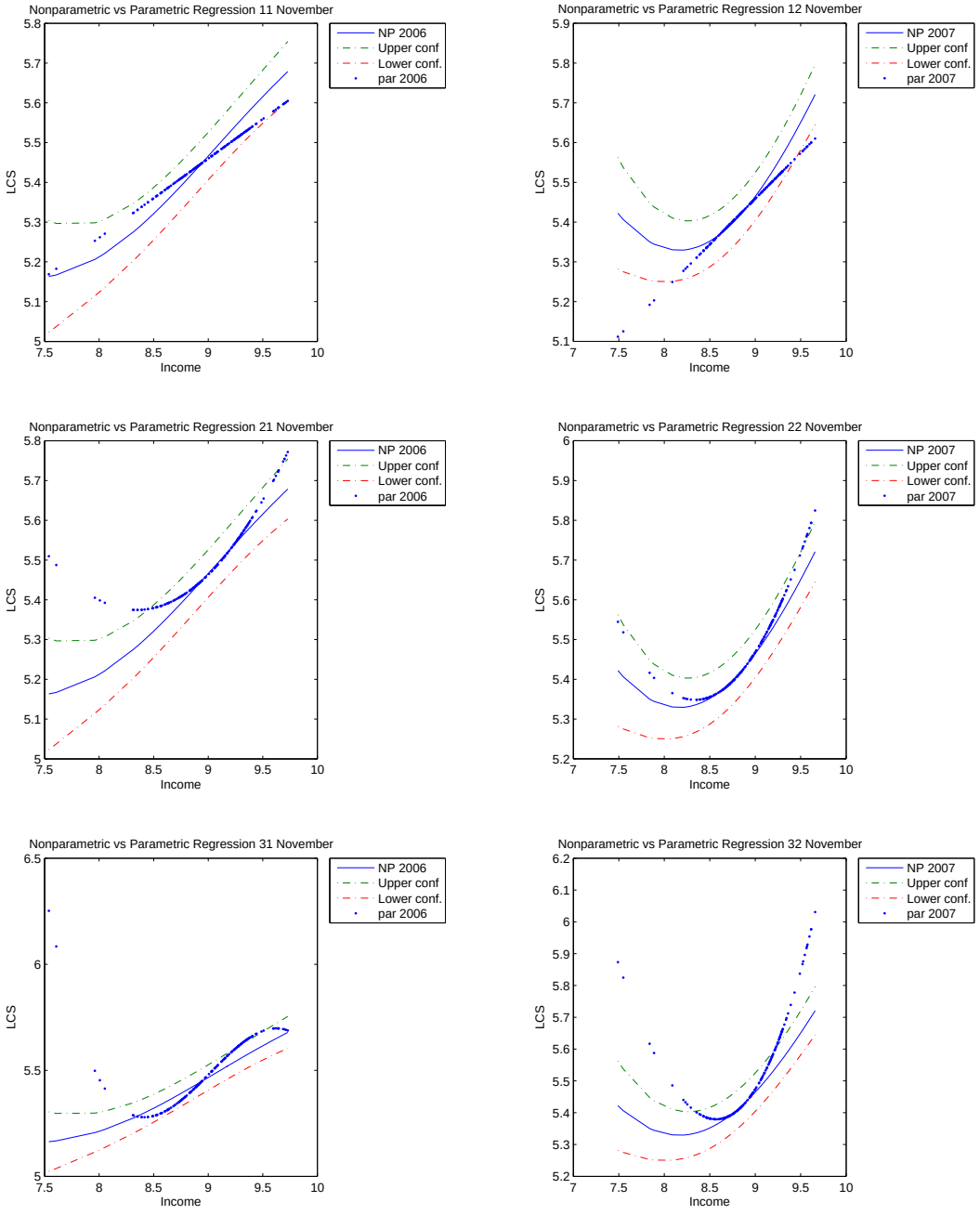
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.10: Insurer: parametric versus nonparametric in October



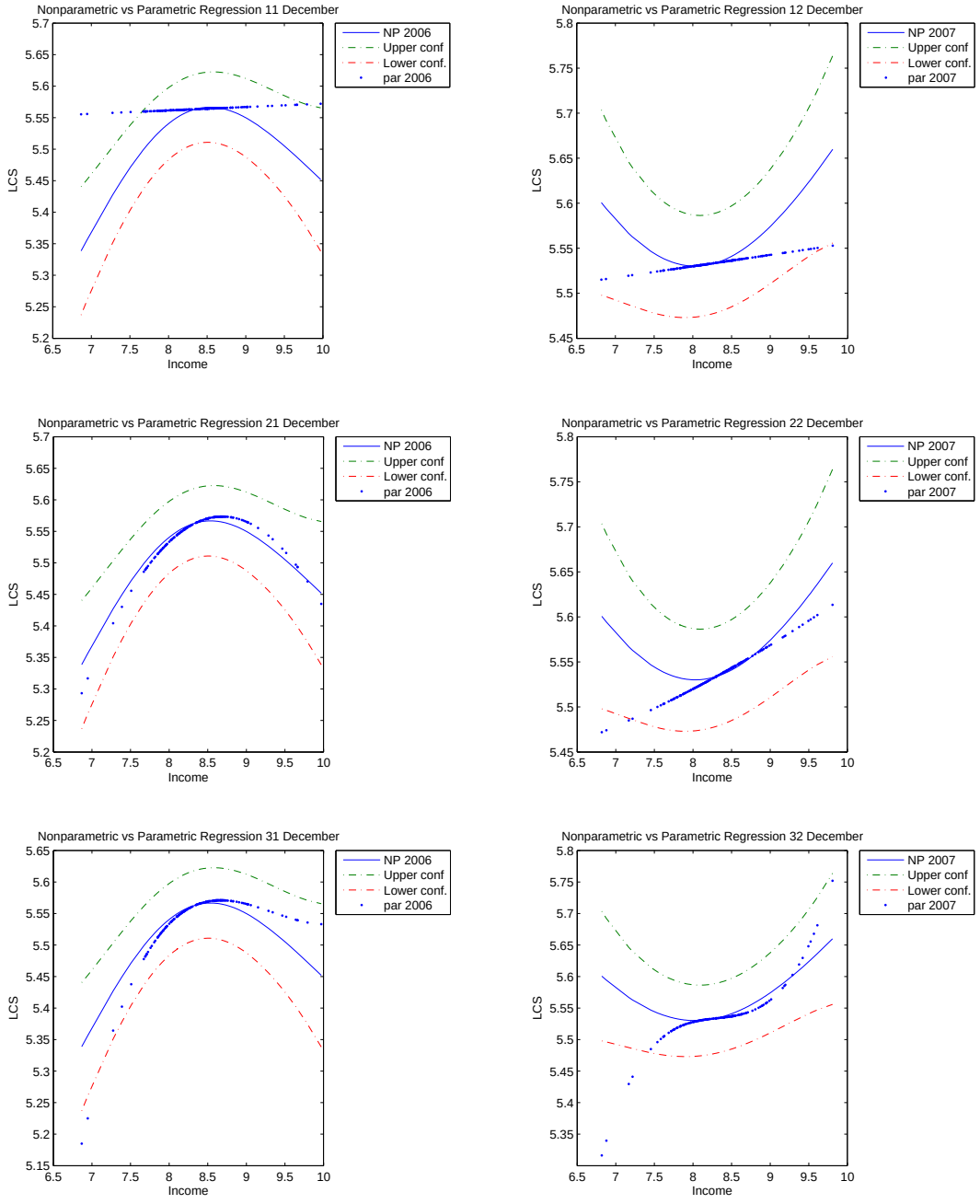
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.11: Insurer: parametric versus nonparametric in November



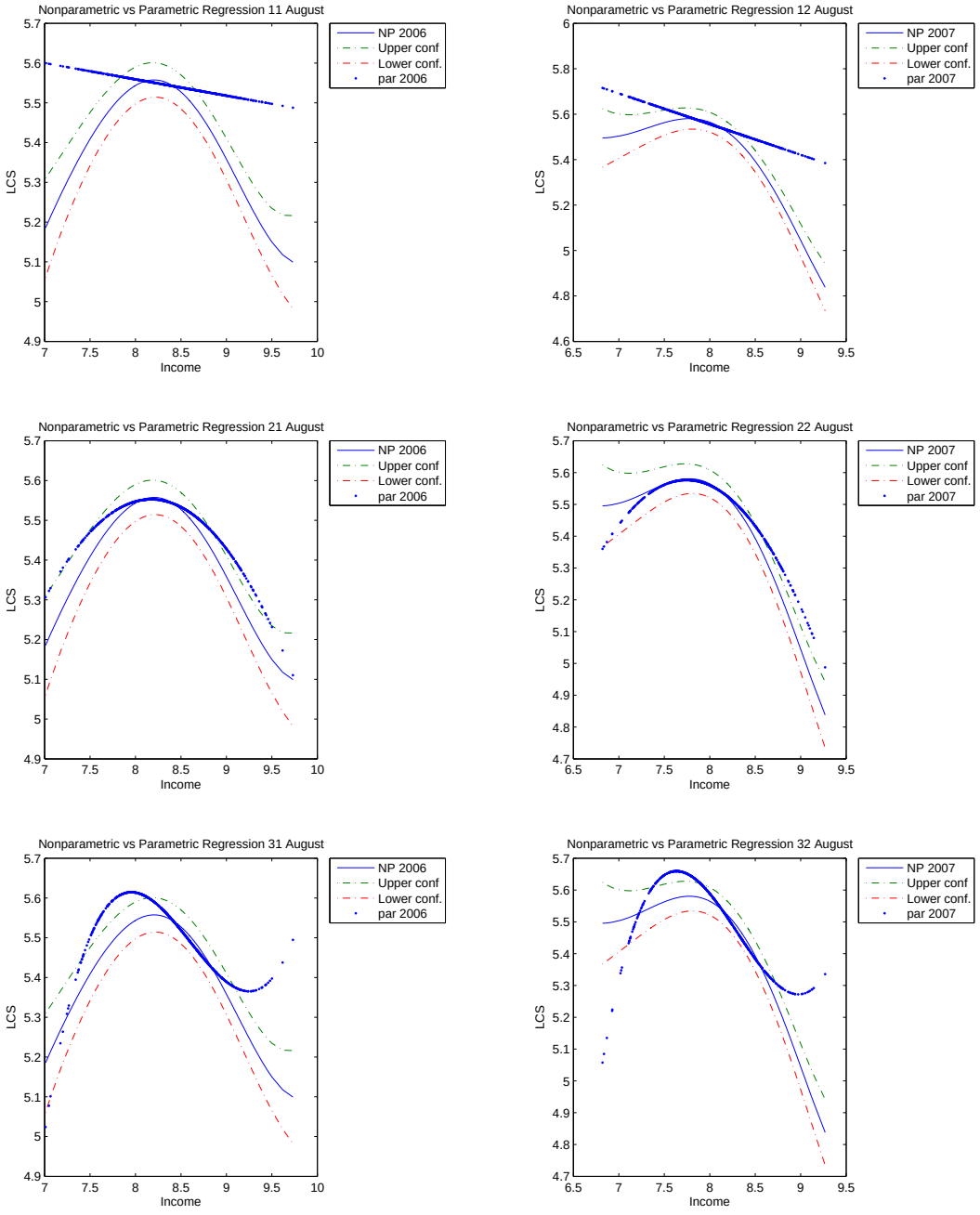
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.12: Insurer: parametric versus nonparametric in December



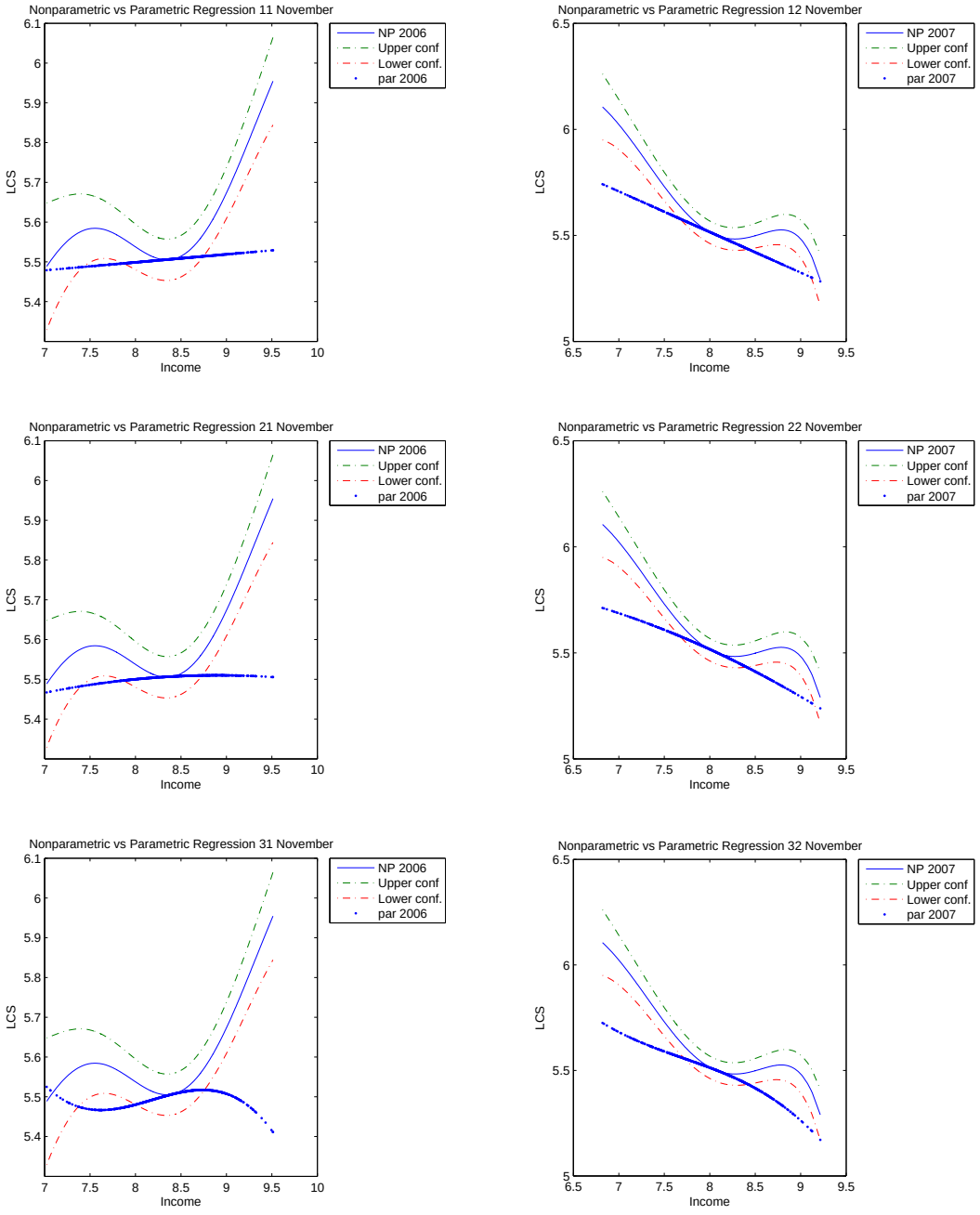
The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.13: Bank: parametric versus nonparametric in August



The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

Figure 5.14: Bank: parametric versus nonparametric in November



The left figures are for the year 2006, the right for 2007. The small line is the nonparametric estimate, with two lines for the 95%-confidence intervals. The parametric estimate is the interrupted line, with $J = 1$ (top pair), $J = 2$ (middle pair), and $J = 3$ (bottom pair). Axes differ sometimes due to scaling.

5.A Institutional details

The Life Course Savings Scheme was introduced in 2006 to promote life cycle planning and better balance work and private life. Workers are stimulated to save for those periods in their lifecycle in which their personal circumstances make it attractive to have more leisure time. The tax facilities related to the LCS include tax deferral, tax subsidies for parents who use the LCS for staying at home with young children, and tax exemptions for employers' contributions.

The LCS scheme allows workers to save up to a maximum of 12% of their salary per year, while the total balance should not exceed 210% of their annual wage. As mentioned, saving in the LCS means tax-deferral: the saved share of income is untaxed at the time of saving. LCS money can be used (and is then taxed) to finance parental leave, care leave and education, as well as early retirement. If it is used for parental leave, employees get additional tax reductions, which can amount to 632 euro a month. While the LCS was introduced to facilitate combining family life and working life over the life cycle, the ESS was introduced to stimulate savings in general. The amount of money that can be saved in the ESS is much smaller than in the LCS. After four years of savings on a blocked account, the maximum tax deduction gained by ESS participation is a mere 600 euro. Whereas LCS money must be spent on buying leisure time, money in the ESS can be spent at the discretion of the employee.

The Dutch LCS differs from the recent initiatives in Sweden and the US to privatize social security. First, saving in the LCS is encouraged, but not mandatory. Second, LCS participants do not necessarily have investment risk, as accounts typically offer a fixed interest rate. Third, taking out the LCS money is only permitted for buying leisure time. Fourth, the plan does not substitute existing social security arrangements in the Netherlands. Fifth, the main goal was to assist two-income earners to both work and have time to raise children, not for retirement planning.

Workers who up to 2006 did partake in the ESS and wanted to change had to actively op-out of the ESS and enroll in the LCS. Employees can switch every calendar year from one scheme to the other. Savings built-up in either scheme are retained when an employee switches.

Implementation at the Bank and the Insurer

Both the Bank and the Insurer offers accounts in the Life Course Savings scheme as well as the Employee Savings scheme as financial products to the general public. Therefore, both firms have are interested in encouraging LCS participation also among their employees. If an employer chooses to grant LCS contributions as a part of the wage package, law mandates that all of his employees should indiscriminately receive such a contribution, whether or not they participate in the LCS. Moreover, although labeled as LCS contribution, there are no restrictions for employees on how to spend it. Thus an employer's LCS contribution does not differ from other parts of the wage package, except for its label "LCS contribution".

Promotional activities for LCS participation differed somewhat between the Bank and the Insurer. The Bank offered a relatively high interest rate on LCS savings, and in addition awarded a bonus for employee savings for five years in a row (10% over the summed interest of these five years). Employees who preferred to invest in stocks rather than to save were exempted from paying transaction costs on buying and selling stock. In October 2005, all employees of the Bank received a letter inviting them to participate in the LCS with the details of this promotional offer, a product flyer, and a form to express interest in participating. The only action required to enroll was to put a preprinted sticker on the form and send it back to a pre-printed postpaid envelop.

The Insurer made promotional efforts among the employees stage-wise during the year 2006. The motivation to opt for phased implementation was the fact that there were already several other changes for employees in January 2006, like the major reform in the Dutch health insurance system, a change in the fiscal treatment of lease cars, and a change in the pension act. These changes required the attention of employees as well as the firms' personnel departments. In January 2006, the Insurer sent a product flyer about the Life Course Savings scheme together with the salary slip. In the months April, May, and June of 2006 the Insurer organized in all its locations information campaigns for employees. There were no advantageous interest rates for employees of the Insurer. Once an employee has an LCS account, (s)he can easily change the amount of savings by accessing the firm's intranet-site. If an employee changes the amount of his inlay before the 10th of the month, this change will be in effect the next month.

Table 5.7: Test statistics for each salary component in each month-pair for Insurer

Salary Component	J	January		February		March		April		May		June		July		August		September		October		November		December	
		$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value
Base salary	1st	0.103	(0.95)	4.850	(0.09)	1.531	(0.47)	1.524	(0.47)	**7.500**	(0.02)	**2.025**	(0.36)	**9.646**	(0.01)	4.303	(0.12)	**13.660**	(0.00)	**6.792**	(0.03)	**7.459**	(0.02)	4.007	(0.13)
	2nd	3.019	(0.22)	2.729	(0.26)	1.547	(0.46)	3.267	(0.20)	**9.050**	(0.01)	**6.021**	(0.05)	**9.923**	(0.01)	4.840	(0.09)	**9.967**	(0.01)	**8.113**	(0.02)	**12.308**	(0.00)	3.812	(0.15)
	3rd	3.471	(0.18)	3.175	(0.20)	0.672	(0.71)	5.668	(0.06)	**12.333**	(0.00)	**10.015**	(0.01)	**13.942**	(0.00)	**7.254**	(0.03)	**12.196**	(0.00)	**11.892**	(0.00)	**11.928**	(0.00)	5.679	(0.06)
Holiday allowance	1st	0	1	0	1	0	1	0	1	1.979	(0.37)	0.558	(0.76)	0	1	2.957	(0.23)	0.584	(0.75)	0.750	(0.69)	0	1	0	1
	2nd	0	1	0	1	0	1	0	1	2.931	(0.23)	0.014	(0.99)	0	1	2.780	(0.25)	0.587	(0.75)	0.017	(0.99)	0	1	0	1
	3rd	0	1	0	1	0	1	0	1	2.910	(0.23)	0.931	(0.63)	0	1	2.884	(0.24)	0.658	(0.72)	0.052	(0.97)	0	1	0	1
13th month	1st	0	1	0	1	0	1	0	1	0.046	(0.98)	0	1	0	1	0	1	0	1	0	1	0.949	(0.62)	0	1
	2nd	0	1	0	1	0	1	0	1	0.003	(1.00)	0	1	0	1	0	1	0	1	0	1	0.617	(0.73)	0	1
	3rd	0	1	0	1	0	1	0	1	0.021	(0.99)	0	1	0	1	0	1	0	1	0	1	1.383	(0.50)	0	1
Profit share	1st	0	1	0	1	0	1	0	1	0	1	0	1	0	1	3.464	(0.18)	1.078	(0.58)	0	1	0	1	0	1
	2nd	0	1	0	1	0	1	0	1	0	1	0	1	0	1	3.812	(0.15)	1.145	(0.56)	0	1	0	1	0	1
	3rd	0	1	0	1	0	1	0	1	0	1	0	1	0	1	3.251	(0.20)	1.172	(0.56)	0	1	0	1	0	1
Overtime	1st	0.415	(0.81)	0.970	(0.62)	1.923	(0.38)	2.200	(0.33)	1.221	(0.54)	1.968	(0.37)	1.138	(0.57)	1.966	(0.37)	1.542	(0.46)	2.530	(0.28)	0.753	(0.69)	0.832	(0.66)
	2nd	0.388	(0.82)	1.489	(0.48)	2.099	(0.35)	1.097	(0.58)	0.639	(0.73)	1.546	(0.46)	1.439	(0.49)	1.617	(0.45)	1.696	(0.43)	2.453	(0.29)	0.724	(0.70)	1.176	(0.56)
	3rd	1.872	(0.39)	2.515	(0.28)	2.132	(0.34)	1.010	(0.60)	0.240	(0.89)	2.350	(0.31)	1.463	(0.48)	1.640	(0.44)	2.751	(0.25)	1.879	(0.39)	1.208	(0.55)	1.202	(0.55)
Bonus	1st	0	1	2.838	(0.24)	2.392	(0.30)	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1
	2nd	0	1	1.333	(0.51)	3.470	(0.18)	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1
	3rd	0	1	1.467	(0.48)	3.246	(0.20)	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1
Other income	1st	0.084	(0.96)	0.489	(0.78)	0.608	(0.74)	4.151	(0.13)	**8.453**	(0.01)	**6.154**	(0.05)	**6.322**	(0.04)	**8.833**	(0.01)	**7.285**	(0.03)	**6.224**	(0.04)	**12.346**	(0.00)	3.187	(0.20)
	2nd	2.419	(0.30)	3.601	(0.17)	0.732	(0.69)	3.881	(0.14)	**8.472**	(0.01)	**9.388**	(0.01)	**11.261**	(0.00)	**10.114**	(0.01)	**9.073**	(0.01)	**10.168**	(0.01)	**10.276**	(0.01)	**6.864**	(0.03)
	3rd	2.455	(0.29)	3.166	(0.21)	0.724	(0.70)	3.938	(0.14)	**9.230**	(0.01)	**10.092**	(0.01)	**12.765**	(0.00)	**14.104**	(0.00)	**10.478**	(0.01)	**12.222**	(0.00)	**10.144**	(0.01)	**6.985**	(0.03)
Simultaneous test	1st	2.93	(0.71)	6.19	(0.52)	6.74	(0.57)	5.77	(0.45)	14.00	(0.12)	10.75	(0.15)	**20.18**	(0.00)	**22.82**	(0.01)	**19.39**	(0.01)	**20.30**	(0.01)	**21.38**	(0.01)	6.41	(0.38)
	2nd	3.49	(0.63)	6.72	(0.46)	7.50	(0.48)	6.00	(0.42)	14.47	(0.11)	12.33	(0.09)	**16.25**	(0.01)	**18.02**	(0.05)	13.92	(0.08)	**19.11**	(0.01)	**18.72**	(0.02)	7.20	(0.30)
	3rd	3.70	(0.59)	5.94	(0.55)	8.42	(0.39)	6.08	(0.41)	14.13	(0.12)	**15.10**	(0.03)	**19.14**	(0.00)	**19.50**	(0.03)	**17.46**	(0.03)	**18.03**	(0.01)	**17.72**	(0.02)	7.70	(0.25)

This table presents the results of test (5.14) for the Insurer, with p-values in parentheses. Each row is a separate test for every salary component for one of the three polynomials (J). Test results in bold print are statistically significant at the 5-% level. Note that the Insurer does not have salary components "LCS-contribution" and "Benefit scheme".

Table 5.8: Test statistics for each salary component in each month-pair for the Bank

Salary Component	J	January		February		March		April		May		June		July		August		September		October		November		December	
		$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value	$T^{J,k}$	p-value
Base salary	1st	1.771	(0.41)	1.614	(0.45)	1.589	(0.45)	0.372	(0.83)	4.974	(0.08)	3.099	(0.21)	5.397	(0.07)	1.064	(0.59)	1.014	(0.60)	1.657	(0.44)	6.008	(0.05)	0.557	(0.76)
	2nd	2.081	(0.35)	3.119	(0.21)	1.377	(0.50)	0.572	(0.75)	1.716	(0.42)	2.645	(0.27)	4.018	(0.13)	1.669	(0.43)	2.785	(0.25)	2.006	(0.37)	4.785	(0.09)	3.113	(0.21)
	3rd	0.462	(0.79)	3.016	(0.22)	1.591	(0.45)	0.411	(0.81)	2.290	(0.32)	2.242	(0.33)	**6.795**	(0.03)	2.125	(0.35)	3.200	(0.20)	1.236	(0.54)	**6.622**	(0.04)	1.573	(0.46)
Holiday allowance	1st	1.500	(0.47)	0.532	(0.77)	0.895	(0.64)	0.599	(0.74)	0.108	(0.95)	0.079	(0.96)	0.004	(1.00)	1.966	(0.37)	2.607	(0.27)	1.282	(0.53)	4.681	(0.10)	0.159	(0.92)
	2nd	1.754	(0.42)	0.667	(0.72)	0.925	(0.63)	0.697	(0.71)	1.382	(0.50)	0.044	(0.98)	0.002	(1.00)	1.938	(0.38)	2.511	(0.28)	1.278	(0.53)	4.410	(0.11)	0.197	(0.91)
	3rd	1.728	(0.42)	0.345	(0.84)	0.843	(0.66)	0.743	(0.69)	1.107	(0.57)	0.012	(0.99)	0	1	1.944	(0.38)	2.525	(0.28)	1.319	(0.52)	4.492	(0.11)	0.488	(0.78)
13th month	1st	2.411	(0.30)	0.532	(0.77)	0.941	(0.62)	0	1	0	1	0.095	(0.95)	0	1	0	1	0	1	0	1	0	1	0	1
	2nd	3.745	(0.15)	0.667	(0.72)	0.977	(0.61)	0	1	0	1	1.947	(0.38)	0	1	0	1	0	1	0	1	0	1	0	1
	3rd	3.674	(0.16)	0.345	(0.84)	0.967	(0.62)	0	1	0	1	1.025	(0.60)	0	1	0	1	0	1	0	1	0	1	0	1
Profit share	1st	1.013	(0.60)	1.163	(0.56)	1.002	(0.61)	1.781	(0.41)	5.134	(0.08)	0	1	0	1	0	1	0	1	0	1	1.960	(0.38)	0	1
	2nd	1.022	(0.60)	1.807	(0.41)	1.000	(0.61)	1.794	(0.41)	0.313	(0.86)	0	1	0	1	0	1	0	1	0	1	1.933	(0.38)	0	1
	3rd	1.023	(0.60)	3.076	(0.21)	1.005	(0.61)	1.797	(0.41)	0.152	(0.93)	0	1	0	1	0	1	0	1	0	1	1.931	(0.38)	0	1
Overtime	1st	2.110	(0.35)	0.839	(0.66)	0.405	(0.82)	2.941	(0.23)	0.112	(0.95)	0.607	(0.74)	**7.668**	(0.02)	3.987	(0.14)	2.661	(0.26)	1.430	(0.49)	3.868	(0.14)	1.052	(0.59)
	2nd	2.169	(0.34)	0.846	(0.66)	0.140	(0.93)	2.886	(0.24)	0.172	(0.92)	0.617	(0.73)	**7.540**	(0.02)	3.773	(0.15)	2.574	(0.28)	1.501	(0.47)	3.837	(0.15)	0.458	(0.80)
	3rd	1.845	(0.40)	0.865	(0.65)	0.086	(0.96)	2.992	(0.22)	0.152	(0.93)	0.533	(0.77)	**6.851**	(0.03)	3.527	(0.17)	2.452	(0.29)	2.029	(0.36)	3.864	(0.14)	0.058	(0.97)
Bonus	1st	4.209	(0.12)	0.985	(0.61)	2.404	(0.30)	1.116	(0.57)	1.043	(0.59)	1.019	(0.60)	1.195	(0.55)	**9.125**	(0.01)	1.135	(0.57)	0.866	(0.65)	2.714	(0.26)	4.428	(0.11)
	2nd	3.018	(0.22)	1.325	(0.52)	2.183	(0.34)	1.333	(0.51)	1.301	(0.52)	0.180	(0.91)	1.454	(0.48)	**6.764**	(0.03)	1.231	(0.54)	1.107	(0.57)	4.059	(0.13)	4.405	(0.11)
	3rd	0.719	(0.70)	2.072	(0.35)	2.326	(0.31)	1.236	(0.54)	2.115	(0.35)	0.261	(0.88)	4.365	(0.11)	**7.838**	(0.02)	1.187	(0.55)	1.508	(0.47)	**7.216**	(0.03)	3.360	(0.19)
LCS contribution	1st	1.084	(0.58)	0	1	0	1	2.841	(0.24)	0	1	0	1	1.114	(0.57)	1.406	(0.50)	3.225	(0.20)	1.781	(0.41)	4.078	(0.13)	1.899	(0.39)
	2nd	1.166	(0.56)	0	1	0	1	2.844	(0.24)	0	1	0	1	0.245	(0.88)	1.381	(0.50)	3.035	(0.22)	1.772	(0.41)	3.887	(0.14)	1.890	(0.39)
	3rd	1.182	(0.55)	0	1	0	1	2.859	(0.24)	0	1	0	1	0.037	(0.98)	1.390	(0.50)	2.869	(0.24)	1.661	(0.44)	3.936	(0.14)	1.900	(0.39)
Other income	1st	3.301	(0.19)	3.188	(0.20)	2.263	(0.32)	2.270	(0.32)	1.895	(0.39)	1.375	(0.50)	1.863	(0.39)	1.117	(0.57)	2.469	(0.29)	2.995	(0.22)	4.351	(0.11)	2.747	(0.25)
	2nd	3.353	(0.19)	3.926	(0.14)	2.194	(0.33)	2.334	(0.31)	1.910	(0.38)	1.264	(0.53)	0.981	(0.61)	0.819	(0.66)	2.362	(0.31)	3.234	(0.20)	4.283	(0.12)	1.778	(0.41)
	3rd	3.487	(0.17)	5.209	(0.07)	2.219	(0.33)	2.393	(0.30)	1.923	(0.38)	0.369	(0.83)	0.905	(0.64)	0.776	(0.68)	2.095	(0.35)	4.069	(0.13)	4.319	(0.12)	0.549	(0.76)
Benefit scheme	1st	0.000	(1.00)	0.834	(0.66)	1.017	(0.60)	0.184	(0.91)	1.118	(0.57)	0.013	(0.99)	0.742	(0.69)	0.301	(0.86)	0.050	(0.98)	0.449	(0.80)	0.061	(0.97)	0.970	(0.62)
	2nd	0.109	(0.95)	0.492	(0.78)	0.839	(0.66)	0.012	(0.99)	0.334	(0.85)	0.910	(0.63)	0.645	(0.72)	0.501	(0.78)	0.115	(0.94)	0.334	(0.85)	0.112	(0.95)	0.165	(0.92)
	3rd	0.421	(0.81)	0.553	(0.76)	0.960	(0.62)	0.010	(1.00)	0.765	(0.68)	1.247	(0.54)	0.319	(0.85)	0.782	(0.68)	0.104	(0.95)	0.303	(0.86)	0.102	(0.95)	0.373	(0.83)
Simultaneous test	1st	19.67	(0.19)	6.22	(0.86)	8.39	(0.75)	11.43	(0.49)	20.41	(0.06)	12.76	(0.31)	12.79	(0.31)	18.51	(0.07)	12.48	(0.32)	11.14	(0.43)	20.67	(0.06)	15.38	(0.17)
	2nd	24.26	(0.06)	7.69	(0.74)	7.64	(0.81)	9.94	(0.62)	13.06	(0.36)	13.95	(0.24)	16.59	(0.12)	17.38	(0.10)	15.61	(0.16)	11.58	(0.40)	**25.43**	(0.01)	15.95	(0.14)
	3rd	12.37	(0.65)	9.61	(0.57)	8.59	(0.74)	10.70	(0.56)	6.18	(0.91)	10.67	(0.47)	12.60	(0.32)	**20.19**	(0.04)	15.71	(0.15)	11.08	(0.44)	**22.89**	(0.03)	11.46	(0.41)

This table presents the results of test (5.14) for the Bank, with p-values in parentheses. Each row is a separate test for every salary component for one of the three polynomials (J). Test results in bold print are statistically significant at the 5-% level.

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