Procrastination and Life-Cycle Savings
Some Micro-Evidence for Hyperbolical Discounting

by

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Abstract

This thesis revisits the basis of recent research on the empirical regularities of time-preferences and life-cycle saving behavior. For this, marginal propensities to consume out of anticipated income changes are estimated on a large panel dataset, the Panel Study of Income Dynamics, containing detailed data on American households. The reported estimates, using the latest available methods in instrumental-variables estimation, are compared with the single reported number of 23 % from which estimates of savings-related short-term and long-term discount rates have been calculated recently. Some weaknesses of the common specification in the literature are exhibited, and more reassuring estimates are offered in the range of 30 %-70 %.

The main contribution of this thesis is that it reexamines some proposed innovations of behavioral economics on micro-level field data, with longer time-series and wider domain than done before. It shows that the key moment varies considerably between time-periods and groups, and thus methods neglecting the richness of micro data are seriously compromised. However, it finds, in line with earlier literature on the comovement of income and consumption, that the permanent income hypothesis is not a full description of household savings behavior. Hyperbolical discounting remains a plausible explanation for the deviations, yet convincing corroborating evidence from field micro data is still disturbingly scarce.

JEL classification: D91 (Intertemporal Consumer Choice; Life Cycle Models and Saving)

Contents

List of Tables

Chapter 1

Introduction

Gone are the days when economic analysis was more excruciating for the analyst than for the subjects. As experiments are in fashion, it can easily happen nowadays that the latter are kept in the dark and in the waiting while the devious (not dismal) scientist sets the scene. Just like McClure, Ericson, Laibson, Loewenstein and Cohen (2007) did, as they denied drinks from (voluntary) subjects kept waiting (without explanation) yet casually fed them chips nevertheless. Yet the whole point of such an ordeal was still economic research: to measure individual time-preferences about relief from thirst, in the form of squirts of orange juice. Surprising or not, 60 % of people chose an immediate squirt instead of some more five minutes later, yet only 30 % chose similarly when both options were pushed back in time by the same twenty minutes (after four hours of waiting). An average discount rate, in the usual sense, can be estimated to be roughly 50 % on a 5-minute horizon but minuscule on the 20-minute horizon.

In other studies, the volunteers are not subject to physiological distress, maybe only some condescending sympathy: Read, Loewenstein and Kalyanaraman (1999) offered movies to their subjects, who consistently pencilled in high-brow movies for later occasions but jumped for supposedly lower-brow ones when they were choosing for the same night.[1] Shiv and Fedorikhin (1999) on the other hand found that people

[1] The popular press reassures the external validity of the study, as the postal movie rental firm Netflix (setting no due dates nor late fees) is said to see significantly longer turnovers for artistic

doing a more mentally challenging task (remembering a 7-digit while others a 2-digit number) choose less responsibly in a long-term perspective (going for a cake instead of fruit) — very much in line with common intuition.

For an economist who does not shy away from some introspection, the *theoretical* results of Akerlof (1991) or O'Donoghue and Rabin (1999, e.g.) on the economics of procrastination[2] are equally appealing: a reasonable set of parameters can result in the ever-going delays of the preplanned exercise regime or the (truly) last cigarette.[3] What is more, fascinating empirical results from field research have also been accumulating recently. For instance, DellaVigna and Malmendier (2006) find evidence that either people systematically deviate even from a precommitted plan (an error in planning or in self-control), or they overpay to precommit themselves and get at least *closer* to a preplanned path: In particular, they find people paying monthly membership fees of $70 for gyms, where they turn up only 4.3 times a month, while they could pay $10 per visit with another pass.

This thesis fits into the latter strand of research using field data, and by doing so it reverts to the more painful way of economic analysis: It analyzes available long-term micro-data on income and consumption to comment on the recent empirical justification and estimation of available theories of life-cycle saving, mainly by Laibson, Repetto and Tobacman (2007, at the latest). Thus instead of (or rather: in addition to) experiments and limited field data on some particular behavior (on that, see Section 2.1 on page 11), I revisit a rigorous analysis of actual decisions on bread-and-butter issues making use of a representative sample of the American middle-aged population. Specifically, this thesis re-examines the deviations from the permanent-income hypothesis on the publicly available data from the Panel Study of Income Dynamics.[4] In this, it is motivated by the state-of-the-art research of

films — which are also often pushed down in the queues the customers are waiting for. (Philips, 2006)

[2]To paraphrase a title of Hamermesh and Slemrod (2005), *The Economics of Workaholism: We Should Not Have Worked on This Paper*, this thesis could have had the subtitle: *Why I Should Have Worked More on This Thesis.*

[3]The *economic* literature on time-inconsistent behavior is also usually traced back to the first empirical results of Thaler (1981), who has done laboratory experiments.

[4]Data last retrieved from `http://simba.isr.umich.edu/` on May 25$^{\text{th}}$, 2008. Accompanying

Laibson et al. (2007), though it does not revert to their method of estimation on simulated data with calibrated moments. This study is more in line with the method of Hall (1978), and all the following literature (see Browning and Lusardi, 1996, for a survey). A similar exercise, however, was the basis of the work of Angeletos, Laibson, Repetto, Tobacman and Weinberg (2001) and now Laibson et al. (2007). Since their simulated world is hard to verify by any means, the strength of their results depends on the strength of their assumptions, and this thesis carefully examines one of them.

The study of Angeletos et al. (2001) and later Laibson et al. (2007) estimated parameters (time-preference key among them) by making moments of their simulated data match empirical moments found in field data. For self-control problems, a crucial such moment is the marginal propensity to consume out of anticipated income changes. The conventional permanent-income hypothesis predicts that consumers should smooth their consumption as much as not to change consumption when income changes predictably, *ceteris paribus*. Instead, many studies have found that consumption reacts to such changes. The own estimation of Laibson et al. (2007) sets this marginal propensity to be 23 %, and this drives their further estimation. I argue that picking that point estimates neglects much variation of that moment, in time and across various dimensions of society.

The value of field data is worth re-emphasizing. However well the circumstances are controlled in the laboratory, the limited number and pool of subjects often questions the results' external validity. It is even more so in the case of experiments with intertemporal choice. The problems are eightfold, as collected by Chabris, Laibson and Schuldt (2008, and the references in their Table 1).

1. Many worry that subjects do not find the experimenter's claims for future rewards as credible (or certain) as those regarding the present.

2. In addition, future rewards obviously incur further transaction costs, which can be spared by going for the immediate payoff.

economy-wide data on prices and interest rates were fetched from `http://research.stlouisfed.org/fred2/`, last on May 27$^{\text{th}}$, 2008.

3. The usual concern that subjects react differently to hypothetical rewards have been directly addressed repeatedly, yet the issue is still not reassuringly settled for everyone.

4. A further (and interesting) concern is that some subjects could (or at least *should*) view a later payoff as an possible investment, contrasting gains with the market interest rate, and not revealing his or her time-preference.

5. Also, the timing of consumption is rarely controlled strictly, and larger payoffs allow longer consumption streams (however discounted those may be) — and they duly seem to be discounted less steeply.

6. If the subject has a standard utility function over some composite consumption, her revealed preference over payoff does not help much to discern her time-preference unless her external sources of (expected) income (or consumption) are taken into account.

7. As many experiments have proven, framing effects have an unfortunate impact on choices.

8. Finally, and somewhat relatedly, the issue that an outcome is often seen as 'right' or suggested by the experimenter is another common complaint.

All this implies that the limited field evidence, reviewed in 2.1 on page 11 is extremely valuable. However, this thesis reexamines the use of such field data in the foremost exercises in investigating time-inconsistent (quasi-hyperbolic) discounting in the case of life-cycle savings, and has some misgivings. Of course, many problems similar to those above (esp. 4, 5 and 6, though perhaps less so using the comprehensive PSID[5]) arise with field data, where, they are just as hard to control for. This is a reason why proper microeconometric studies have been scarce in this regard.

[5]That said, it is questionable how much more embarrassed an interviewee is to report drinking, smoking or even perhaps dissaving to an interviewer than a subject to pick a tantalizing immediate reward, even a low-brow movie, in front of an eager experimenter.

1.1. Time-inconsistent present-bias: quasi-hyperbolic discounting

Either for descriptive (positive) or prescriptive (normative) purposes, it is of utmost importance that people tend to prefer current rewards to delayed ones of similar magnitude. Tentative candidates to justify this behavior can be classified as mortality effects, impatience effects and salience effects (Chabris et al., 2008). The first reason is the easiest to defend (with rational axioms or even ethical norms), yet for the young and the middle-aged mortality rates are a hundred times less than what could justify the usual intertemporal choices. However, even as one accepts the use of some discounting introduced by Ramsey (1928), the widely used exponential form has been challenged by a wide body of psychological evidence starting with Herrnstein (1961).[6] The term 'hyperbolic discounting' for an alternative is traced back to Ainslie (1992), and got its name because of the original formulation with hyperbolas (Chung and Herrnstein, 1961), like discounting future utility τ period ahead with the factor $1/\tau$, $1/(1+\alpha\tau)$ or $1/(1+\alpha\tau)^{-\gamma/\alpha}$ instead of an exponential factor of δ^{τ}.[7]

The challenges to time-consistent preferences and exponential discounting in particular, as well as some proposed solutions, came to the forefront of attention with the summary of Loewenstein and Prelec (1992), and the research strand was picked up quickly, most notably by David Laibson. With his many students and co-authors, they have derived many applications (for instance Harris and Laibson, 2001; Laibson, 1997, 1998; Laibson, Repetto and Tobacman, 1998) adopting the simpler form of quasi-hyperbolic discounting originally formulated by Phelps and Pollak (1968). A review of the more recent literature is available from Frederick, Loewenstein and O'Donoghue (2002).

The quasi-hyperbolic form is a common, and arguably the simplest, extension

[6] Note also that the unease with the theory of exponential discounting had some rather early beginnings with the works of Ramsey (1928) and Strotz (1956).

[7] Ainslie is still one of the main figures examining hyperbolical discounting in the psychological literature, and has dedicated a website to the project at `Picoeconomics.com`.

to exponential discounting, when all future utilities are further discounted by the same additional factor in comparison with the present, whenever that present comes. Apart from this, all utilities experienced at various points in time simply add up, but are worth less the further their respective realization is ahead, with a constant factor multiplying the utility for every additional time-period ahead.[8] So the overall utility U experienced at the present t add up from future instances of utility u_{t+s} ($s \geq 0$) as

$$U_t = u_t + \beta\delta u_{t+1} + \beta\delta^2 u_{t+2} + \beta\delta^3 u_{t+3} + \ldots + \beta\delta^s u_{t+s} + \ldots. \tag{1.1}$$

Here the parameter β allows for self-control problems. The $\beta = 1$ case implies the standard model with time-consistent exponential discounting, while in the case with $\beta < 1$ the present is preferred in a time-inconsistent way. The literature also discusses issues with overconfidence in future self-control (planning ahead with a postulated $\widehat{\beta} > \beta$ for future decisions, or even full naivety with $\widehat{\beta} = 1$) (O'Donoghue and Rabin, 2001, for instance), and issues of conflict between a planner and a doer system (Fudenberg and Levine, 2006, inter alia).

To see what empirical studies are after with this motivation, it is useful to sketch two simple cases illustrating how time-inconsistency follows from a quasi-hyperbolical setup of (1.1), with overconsumption of some goods and underconsumption of others. Imagine a situation with only three time-periods, $t \in \{0, 1, 2\}$. Investment goods, like exercise or thesis-work for a graduate student, has a feature that they require an early effort, utility $b_1 < 0$ at $t = 1$, and later some rewards of $b_2 > 0$ utils at $t = 2$. Then beforehand (at $t = 0$), one plans to invest in his fitness or his thesis if he expects this to yield positive utility overall (against an opportunity of staying put, accruing 0 utility), which is the case when

$$U_0 = u_0 + \beta\delta u_1 + \beta\delta^2 u_2 = \beta\delta b_1 + \beta\delta^2 b_2 > 0, \tag{1.2}$$

[8] Laibson (1997) coined the term 'quasi-hyperbolic discounting', as it only captures a particular feature of the hyperbolic formulation, namely the faster rate of decline in the discount factor in the short run, in the sense of the derivative over the levels $(-f'(\tau)/f(\tau))$.

or

$$b_1 + \delta b_2 > 0. \tag{1.3}$$

However, when $t = 1$ arrives, the same comparison against the comfortable option of idleness implies the activity only if

$$U_1 = b_1 + \beta\delta b_2 > 0. \tag{1.4}$$

When $b_1 \in [-\delta b_2; -\beta\delta b_2]$, the student refrains from the exercise or the thesis work, although he had the best of intentions to engage in them as soon as their time comes.[9] The opposite case, when $b_1 > 0$ and $b_2 < 0$ corresponds to tempting opportunities to binge however dire are the later consequences, like considering the Friday night party and the Saturday morning hangover, or more seriously, years of plenty at the age of 50 and years of famine during retirement.

To see the over– or underconsumption in quantities (yielding the utilities through $u(\cdot)$), one should simply note that the agent plans to follow a path where marginal utilities of consumption increase (thus quantities decrease) so that $u'(c_1) = \delta u'(c_2)$,[10] yet when the first period comes, he will binge on the good (until it is a good) and leave less for the future: the new optimum is where $u'(\widetilde{c_1}) = \beta\delta u'(\widetilde{c_2})$, while the sum of the two quantities is predetermined by the original plan. Thus a consumer who faces intertemporal decisions with quasi-hyperbolical discounting will binge on consumption goods and delay investments or savings.

For the consumption-saving behavior examined in this thesis, the main implications of such time-inconsistent behavior with self-constraint problems can be summed up into four points (Angeletos et al., 2001; Laibson et al., 2007, e.g.): First, for commitment reasons, households will hold their wealth in illiquid assets. Still, they will not fully resist the temptation and will borrow some on credit cards.

[9] Another typical example of the dynamic inconsistency manifesting itself in overturned decisions is the one of the often preset but later disregarded alarm clock, however strongly one commits himself not to 'snooze' it — e.g. by planting it in the opposite corner of the bedroom. This author felt this anecdotal result vindicated during his research break. As a commitment device on the market, even Clocky® the runaway alarm clock is available for $50.

[10] Of course, only in the simplest case when quantities can be reallocated one-to-one.

However, because of the illiquidity of their main assets, the households' opportunity to smooth their consumption will be dented. The resulting comovement of income and consumption, even when the changes are predictable, is similar to the one in otherwise motivated models like those of buffer-stock savings of Carroll (2001b, 2004) due to (not necessarily self-inflicted) liquidity constraints and precautionary saving — though, of course, all theories have the same motivation to begin with, namely to explain this ubiquitous phenomenon. Finally, this comovement will be most conspicuous around retirement, when income falls steeply. This thesis tests a form of the first of these two statements.[11]

As a side remark, I note that though the *cause* of some preferences are not necessarily a topic for economics, for the empirical regularities of present-bias, procrastination or time-inconsistency, a joint effort with neuroscience has produced some results in this regard. Temptation models and dual-brain neuroeconomic models have been both proposed, and proved reasonably successful, to explain these phenomena. In the latest strand of such research, neuroimaging helped McClure, Laibson, Loewenstein and Cohen (2004) to locate brain activity as the subjects were choosing between different Amazon gift certificates valid some different time in the future. Their basic result is that the 'analytic brain' (the fronto-parietal cortex) is involved in any decision concerning the future and is patient, yet the 'emotional brain' (the mesolimbic dopamine system) is more active when immediate options are under consideration, and enhanced 'analytic' activity is needed to suppress its activity when a later option is chosen against an immediate one. To control for the role of delayed consumption instead of the timed delivery, the authors ran the aforementioned experiment with the thirsty subjects (McClure et al., 2007). Not only did they observe that the choice between "bundles of squirts at a given time" depended on the delay to the earlier one as well as on the distance in time between the two and did they locate the systems associated with patience or impatience;

[11] The last result, however telltale the huge drop in consumption seems to be, is very much complicated with substitution effects (between leisure and material consumption) and some costs of work accounted for in consumption.

the authors could estimate their corresponding discount rates — at least for orange juice for thirsty people. The impatient regions discounted with cca. 4 % per minute, while the patient ones with 1 %, on average.

Ideally, this line of research could be extended by estimating a model of saving as general as possible on field data, and test empirically which extensions — including impatience and time-inconsistency — are relevant. Doing so is a strong point of the study of Laibson et al. (2007), though they do not do that on actual data. I choose to revisit the basic moment that is a starting point of their modeling, and instead of commenting on the underlying reasons like liquidity constraints (in the end delving down to psychology exactly as Laibson's research tries to do), I aim to show that the moment that their results are built upon varies among periods and within groups, to an alarming extent. It is still interesting what kind of self-control problems could reproduce the observed general savings patterns, yet one must stop and comment on what is there to know about that general pattern.

I find that considerably higher estimates of that moment are just as plausible as the baseline used by the authors.

1.2. Outline

The rest of this thesis is organized as follows. 2 discusses briefly reviews the literature on empirical studies on time-inconsistency and the comovement of consumption and income in general. In Chapter 3, I specify the estimated relationships, before turning into the particular data I retrieved and cleaned, in Chapter 4. Some descriptive tables and notes on the database are relegated to Appendix A. I present the results in Chapter 5. Chapter 6 provides some discussion of the results and possible directions for future research. Chapter 7 concludes.

Chapter 2

Literature review

2.1. Field studies of time-inconsistent behavior

The existing literature on time-inconsistency and life-cycle savings has been focusing much more on simulations calibrated using various data sources than proper field data. The two often-cited works (Angeletos et al., 2001; Laibson et al., 2007) remain in a representative consumer framework, yet introduce illiquid and liquid assets with interest on liquid debt (amounting to actual average credit card interest adjusted by default rates), liquid assets yielding average municipal bond yields and illiquid assets yielding only a consumption flow like durable goods, while risk and income profiles (including transfers, especially for retirement) are calibrated from the PSID, and effective household sizes are determined by an exogenous age-dependent survival hazard calibrated from health statistics. The papers' main contribution is that they explain high credit card borrowing and parallel illiquid wealth accumulation, where the former is a consequence of self-control problems while the latter is in high demand as commitment devices.

It is clear however, that the claim that these authors have finally tested the theory of hyperbolic discounting on *field data* is somewhat stretched. This thesis revisits the part of their work that is the basis of their claim. The field data that drives these most-cited (and presumably only) results on savings and hyperbolical

discounting enters only to estimate a moment, the marginal propensity to consume out of expected (predictable) income.[1] This is the moment I return to analyze in this thesis. These regressions are not unlike others in the literature on excess sensitivity on consumption, which I review briefly in Section 2.3 on page 16.

However, field evidence for results of behavioral economics of any kind has been few and far between until recently. On them, DellaVigna (2007) offers a good and recent survey, with his Section 2.1 on self-control problems in particular. As he himself repeats some common complaints (claims of Levitt and List, 2007; List, 2007, among many others) against experimental results (and theories motivated by them), many argue that people have the important opportunity to learn in real markets, while they react to different incentives in experiments anyway. Though the relevance of psychological results, like all on human cognitive limitations, self-constraint problems or alike, is ultimately an empirical question, which both proponents and critics of behavioral economics agree upon, one should readily note that sometimes the criticism is the harder stance to defend. The scope for learning is limited in the case of such decisions of utmost importance as choosing oneself an education, a career or the time of retirement. Moreover, as DellaVigna and Malmendier (2004) have derived, firms have an incentive to exploit the *actual* behavior of their costumers, which can result in an equilibrium where the consequences of some 'irrational' behavior are amplified, and sustained not eliminated. Actually, their aforementioned result on health clubs in Boston reinforce this claim (DellaVigna and Malmendier, 2006).

Among empirical studies of field data, the one of Ausubel (1999) is a famous (and envied) one. His unique dataset is on credit card offers, take-up and usage history with randomly assigned offers from a company. Three different market experiments were designed along similar lines (Ausubel, 1999, Table 1), varying the interest rates in an introductory period or thereafter, or the length of such a period. In one, against a control group facing a 'teaser rate' of 6.9 % for the first six months and 16 % thereafter, some were offered 4.9 % for the first period and some 14 % for

[1] Laibson (1998) chose different figures (preretirement wealth holdings) which to replicate by choosing time-preference parameters. That longer study is also better documented.

the second. Considering how much the customers borrowed in the first six months and the following fifteen months while followed, neglecting selection effects and substitution effects of the different interest rates, the group facing the lower teaser rate should have responded less enthusiastically than the other treatment group. Since customers borrowed around $2,000 in the first six months, the (annually) two percentage point benefit is less of a boon to them than to those save on the latter period when they could enjoy it for fifteen months (though only on $2,000, on average). However, 386 took customers up the offer with the lower teaser rate out of the 100,000 and only 184 (out of another 100,000) the one where they could borrow for less later. This phenomenon is consistent with the view that people were naive about their borrowing after the teaser period.[2] In a follow-up study, Shui and Ausubel (2004) do estimate time-preference parameters of the direct mail recipients' allowing exponential or hyperbolical discounting. They get short-run discount factors around 76 %–80 %.

Another study of the few that strives to directly infer discount functions from field behavior is the one of Ashraf, Karlan and Yin (2006b), who report on their designed experiment in the Philippines. They offered a commitment product to a 710-strong random subset of banking clients and compared their savings behavior to 466 households in a control group receiving nothing more than verbal encouragement to save. The chance of increasing savings proved to be statistically significantly higher in the group that could commit themselves, though the impressive 81 % increase in savings dissipated to a statistically insignificant 33 % over the course of three years, as Ashraf, Karlan and Yin (2006a) admit, though they do not conclude on the reasons.[3] A similar demand for commitment devices in retirement savings instruments in the United States is reported by Thaler and Benartzi (2004). They give evidence that employees are sympathetic to prescriptive savings plans, if they

[2] Though only the cited results are relevant for this thesis, the unique data source also yielded other important conclusions. The main focus of the paper is adverse selection, which is clearly present among respondents to the offer, and worse in the group that could accept a better offer.

[3] Interestingly enough, the popularity of the product might have had other causes as well, as the participating families reported higher decision-making power for women after the first year, and even spent more on "female-oriented durable goods", like washing machines.

no longer have defined-benefit retirement savings accounts available. Instead of the low participation and a puny 3.5 % average saving rate beforehand, 78 % of those offered a committed-savings program joined, 80 % of whom actually remained in for at least four raises and raised their saving rates to 13.6 %, on average, in 40 months.

Taking a different angle, Fang and Silverman (2007) analyze the choice of never-married women with children between the home, work, or getting on welfare. Their structural estimates on micro data (but still with a calibrated likelihood function) from transitions in the National Longitudinal Survey of the Youth imply that time-inconsistency is indeed very much present in these women's staying away from work. Paserman (Forthcoming) on the other hand inferred on time-preference from unemployment spells and accepted wages in the same survey.

Finally, field studies of self-control problems affecting household finance also discuss the default effects on savings. It is well-documented that opt-out regimes produce substantially higher take-up of financial products, schemes and programs, than opt-in regimes, even if the liberty of the subject to choose either option is intact in both cases, and the individual has exactly the same information whatever the default. As O'Donoghue and Rabin (1999) has shown, self-control problems, e.g. those described by a simple (β, δ) utility like the one introduced in (1.1), can result in infinite procrastination of decisions even with small transaction costs and substantial later gains, especially when a decision-maker is naive about his future self-control. Madrian and Shea (2001) report a 86 % participation rate in a 401(k) plan after a change to a default of participation, while in a control group with the opposite default the same rate was only 49 %. Choi, Laibson and Madrian (2005) found similarly explained suboptimal saving among employees of seven companies, and also found out with a field experiment that indeed those more prone to delay were the ones not exploiting their saving opportunities, and that informing a randomly selected subset about their foregone gains resulted in only a negligible response. However, it is important to note that the Active Decision plan with mandatory choice in the experiment of Choi et al. (2005) resulted in almost as high enrolment rates,

80 % in a 401(k) plan after a year, as the automatic enrolment (but potential exit) in the study of Madrian and Shea (2001), which supports the idea that transaction costs, however small, are the key deterrents of the superior options, and most people are time-inconsistent with their retirement plans, and naive about this fact. On default-effects, however, the most notable study is probably the one of Cronqvist and Thaler (2004), which reports that after the privatization of Swedish social security in 2000, 43.3 % of new entrants chose the default plan among retirement funds though 456 were available, and this number rose up to 91.6 % once the government campaign encouraging individual choice has ended.[4]

A less related, but insightful study of procrastination was conducted on students by Ariely and Wertenbroch (2002), when their students were allowed to set deadlines for themselves at the beginning of a course. 68 % has chosen this inflexibility. However, a related experiment showed that self-chosen inflexibility (pre-commitment) still yielded inferior results to exogenous inflexibility: students who had equidistant deadlines for some exercise outperformed the ones who could set deadlines for themselves. Still the poorest result was of the group who could not commit at all.

For further reference, I report the discount rates found by all these field studies. The latest study of Laibson et al. (2007) conclude on a benchmark estimate of a 40 % short-term discount rate ($\beta = 0.6$) in addition to an exponential 4.3 % in the long-term ($\delta = 0.96$), both annualized. In the earlier paper, the same parameters are estimated to be 45 % and 4 %, respectively (Angeletos et al., 2001). This sizable short-term bias might be surprising, and renders my efforts to reinvestigate the strength of the link to field data all the more important. These estimates all follow from an estimated moment around 0.23, estimated on a restricted domain of high school graduates between 1978 and 1992. For a different problem with different data (on job search), Paserman (Forthcoming) found very large short-term discounting for low-wage workers ($\beta = 0.4$) but much smaller for high-earners ($\beta = 0.89$), with the

[4]Such results also have substantial welfare implications. One of the founding fathers and a leading light of the subfield of behavioral economic and finance, Richard Thaler has even reached the point that he has just published a popular book aimed at laymen, politicians or any 'choice-architect', with an accompanying blog at `Nudges.org`. (Thaler and Sunstein, 2008)

same long-term discounting for both groups ($\delta = 0.99$). For the admittedly peculiar group in the focus of Fang and Silverman (2007), even more serious short-term discounting was found, but also higher long-term impatience, with $\beta = 0.34$ and $\delta = 0.87$.

2.2. Characteristics associated with self-control problems

As Chabris et al. (2008, Table 2) collects, time-inconsistent behavior has been associated with numerous behavioral traits or abilities. Smokers have higher discount rates, in general (and not just for the mortality reason, of course). Studies on heavy drinkers, on active versus abstinent alcoholics, and on detoxified alcohol-dependents have found higher discount rates all as intuition would have suggested. For cocaine, crack-cocaine, heroin and amphetamines, *inter alia*, discount rates were found to be higher among users, just like for pathological gamblers (in the laboratory as well as in realistic settings). Those with higher discount rates are even found to gamble more often. On another dimension, age makes people significantly more patient, even at later stages of life. Finally, various measure of cognitive accomplishments (GPA, test scores or college attendance) seem to be positively correlated with impatience (or self-control). All this research is mainly conducted by psychologists, see Reynolds (2006) for a review. However, the studies do not identify clearly short-term and long-term time-preference. Still, they suggest that the analysis of the economic behavior by various group is a worthwhile exercise, and might question the validity of representative-consumer modeling. I report my own results on saving behavior of various groups along some thoughts whether the savings differences are reasonable, see Chapter 5 on page 31.

2.3. The corresponding literature on consumption

Exactly the sort of empirical analysis that this thesis conducts, namely studies of field data (though often in aggregate), has motivated other developments in consumption

theory, like the theories of precautionary saving, liquidity constraints, habit formation and models of buffer-stock saving (see for recent developments Carroll, 2001a,b, 2004; Carroll and Kimball, 2001, to name only one leading author). The literature on the comovement of income and consumption go back to two main branches of such empirical literature: one on the *excess sensitivity* of consumption, meaning that whether, how and why consumption follows predictable income movements, and another on the *excess smoothness* of consumption, which concerns why consumption should not respond *more* to the *un*expected changes in income. The two phenomena do not rule out each other.

Hall (1978) originally examined whether past values of income, consumption or stock prices can predict changes in consumption, and found that only stock prices could. To build the exact links between the predictive power of lagged values on future income and between this future income and consumption, the instrumental variables approach is the standard since the work of Campbell and Mankiw (1989), though they wrote and estimated their model on aggregate data. They found that past changes of income are poor predictors of later changes, in the aggregate, which questions my choice of instruments somewhat. However, today there are tests available whether the instruments are too weak to do any good, and I report such results for my micro data in Section 5.1 on page 31.

Shea (1995) chose instead union contracts to predict wage growth for a subsample of the PSID, and found that householdswith these consumed 89 % (with a large standard error) of thus-predictable income changes. Interestingly, he got the same result for households with and without liquid assets, which suggests that liquidity constraints are not the main driving forces behind excess sensitivity. I myself can make a similar comparison of such groups, which is reported in Section 5.1.4 on page 36. The other evidence Shea (1995) found against liquidity constraints is his comparison of families with positive and negative predicted income growth — and the counterintuitive predictable comovement of consumption and income (stronger for those facing declines).

Some more recent literature analyzes particular (and thus presumably expected) changes to income, like policy changes or predictable effects of a policy long in place. The emerging pattern from such studies (briefly suveyed by Romer, 2006, p.261) is that households react to large changes in their income according to the permanent-income hypothesis (thus they smooth their consumption) yet they consume way too much in line with their income changes until those our small (like below 10 % of family income). As in this study most changes fall into the latter category, one can expect to find evidence for excess sensitivity once more.

Laibson et al. (2007) themselves cite Shea (1995) as well as the aggregate study of Carroll and Summers (1991) to say that their estimated marginal propensity to consume of 23 % is in line with the literature. They also refer to the survey of Browning and Lusardi (1996) and say that the key moment is usually found to be between 0 and 0.5. Considering how much their estimated discount rates depend on this number, such a wide range of results can hardly be seen as a reassuring professional consensus.

On the issues that Browning and Lusardi (1996, Sections 5.1) raise about similar Euler regressions, only the length of the panel used in this thesis is reassuring. The literature on Euler equations like the one estimated here is well-aware of the importance of measurement errors, the serial correlation rendering even the standard instruments of lags dubious, the importance of individual heterogeneity, all which I do not solve here any more than Laibson et al. (2007) have done. The path to allow individual effects seemed the most tantalizing, yet even simple additive fixed or random effects are not trivial in instrumental-variables regressions, and I did not consider them well-specified for such dynamic equations like the Euler equations — should they mean that a family observes a positive (or a negative?) consumption shock each year, throughout decades?

Only the survey of Browning and Lusardi (1996, Table 5.1) has collected 25 studies of Euler equation consumption studies using micro data. For my work here, the reported “excess sensitivity” estimates are relevant (Browning and Lusardi, 1996,

p.1831). The studies that focus on food consumption are thus directly comparable with my study, and they have not found the marginal propensity to be larger than 0.2, apart from the result of Shea (1995) on the group expecting the decline in income, where the estimated impact of an anticipated change in the real wage is 2.24 on consumption growth, which is huge (though with a large standard error as well). All other groups exhibit lower estimated effects than the baseline result of Laibson et al. (2007). My estimates duly follow in Section 5.1 on page 31.

Chapter 3

Specification and methodology

3.1. The comovement regression

The relationship in focus[1] is about the marginal propensity to consume out of predictable changes in income. Hereby I instrument income growth with lagged income, and control for family composition effects, hours worked, and demographics in the family (age and cohort effects), as well a business cycle effect which Laibson et al. (2007) also allow. As a robustness check, I estimate restricted versions, where only family composition and the age of the youngest children is allowed to play a role, as more appealing to intuition. The domain of estimation is the family unit.

The estimated equation is

$$\Delta \log C_{it} = \alpha E_{t-1} \Delta \log Y_{it} + X_{it}\beta + \epsilon_{it}, \tag{3.1}$$

with i standing for households and t denoting years.

The literature usually calls the estimated relationships Euler equations, as they easily follow from the eponymous relationship of marginal utilities if the discounting is exponential and a convenient (say, CRRA) utility function is specified: the equation

[1] The relevant Stata do-files, with references to the standard names of variables in the databases retrieved, are all publicly available at `http://www.personal.ceu.hu/students/06/Laszlo_Sandor/Thesis.html`. The whole exercise is replicable from the recoding and reshaping of variables to the exporting of graphics and tables to LaTeX.

contains the ratio of consecutive consumptions, and once logarithmized, it can be approximated by the change.Harris and Laibson (2001) have derived Euler relations for "hyperbolic consumers" with liquidity constraints and uncertain income:

$$U'\left(C\left(x_t\right)\right) \geq E_t\, R\left[C'\left(x_{t+1}\right)\beta\delta + \left(1 - C'\left(x_{t+1}\right)\right)\delta\right]\, U'\left(C\left(x_{t+1}\right)\right), \tag{3.2}$$

where x_t stands for liquid wealth at period t. However, the resulting endogenous effective discount factor, as they call the term in brackets, varies with the marginal propensity to consume. This does not allow an analytical solution, this is why Laibson et al. (2007), solve their model calibrated numerically, and cannot infer on parameters directly. I also simply repeat their relationship used for calibration.

3.2. Subsamples and homogeneity

It is important to estimate the same relationship on various domains, especially by breaking down the population along such variables as educational attainment, alcohol use or some behavioral indices, which are easily associated with self-constraint problems and dynamic inconsistency. While Chabris et al. (2008) readily cites the evidence that several studies have delved into the correlation of present-bias (or simple impatience) and smoking, drinking, illicit drug use, gambling, age, cognitive ability, Laibson et al. (2007) report results only on all families whose head has a high school diploma but no college degree. Instead, I have repeatedly redone the analysis on the widest pool of subjects available, with various breakpoints allowed. This way, I report more general results than Laibson et al. (2007), as well as more particular ones: the homogeneity of the sample is in the forefront, yet until one maintains the hypothesis of homogeneity in one dimension (across time, e.g.) one can see how results differ in another one.

3.3. The method of estimation

The literature I link to takes the setup of (3.1) as given, with the income changes instrumented by the second and third lag of the logarithmized income in a conventional two-stage least squares framework. I touch upon three issues that extends this work.

First, I considered conducted panel unit root tests on the longest series available for both consumption and income. Though by all expectations, the series should be integrated (at least consumption moving around some permanent income, with the income possible following a deterministic trend), this would have been the prudent way of conduct. However the tests available for me did not allow an unbalanced panel of PSID with missing observations.

Second, Laibson et al. (2007) uses two-step least squares by default. I considered using more efficient continuously updated generalized method of moments estimator, yet the small number of instruments promised small benefits from this approach, and it introduced computational difficulties (in the form of concave moments function to minimize). I did not pursue this extension further. However, I allowed for the most robust standard error estimation possible — with cluster errors suspected within groups of repeated observations of the same person. Yet this ruled out autocorrelation with the currently available estimators. Panel versions of the estimates were also calculated but are not reported.

Third, the recent literature on weak instruments generated renewed interest whether instruments introduce a net benefit in an estimation otherwise suspected to be susceptible to some endogeneity bias. I tested for the standard lags being weak instruments, and also estimated the equation by a method more robust to weak instruments and non-IID disturbances. I comment on the specification tests in Section 5.2 on page 40.

Chapter 4

Data

The data come from two sources. First, in Section 4.1, I briefly describe the sources themselves, and then in Section 4.2 on page 28 I summarize how I interpreted the model on the available data of the PSID.

4.1. Data Sources

The Panel Study of Income Dynamics (PSID) is a longitudinal survey of American households since 1968. The study, which is run and maintained by the Institute for Social Research (ISR) at the University of Michigan, aims to track all families and individuals from the original sample. The sample started with 4,800 families, and by following the children of those families, the PSID had more than 7,000 families after 2000. The PSID core sample is a combination of a cross-sectional national sample drawn by the Survey Research Center at the ISR and the sample from the Survey of Economic Opportunity. The most important changes in the survey took place in 1997, when it was decided to continue as two-yearly, the core sample was reduced and families representing those that immigrated after 1968 (and their adult children) were introduced to the sample. I do not use this new information since their shorter tenure in the sample compromises the advantages of using a long panel study.[1]

[1] A more detailed description of the most important features of the PSID is available online at `http://psidonline.isr.umich.edu/Guide/Overview.html`.

The panel can (and was) be linked by individuals, and age and education data was indeed retrieved at the individual level. Most economic decisions and sociological data was asked on the current head and sometimes the wife (or a similar partner) in each family each year. The PSID has detailed information on incomes throughout all years, while investment decisions and balances by asset classes are asked only in 1984, 1989, 1994 and in the newly-two-yearly survey since 1999. Various potentially relevant data were asked only in some years, but I tried them to check for breakpoints for the behavior in focus. For instance, some standard test scores or behavioral attitude questions, like on risk-aversion, self-control and forward-looking behavior, were asked only around the start of the panel. A detailed list of the variables investigated to help explain the savings patterns of families are tabulated in Appendix A on page 51. Rather naturally, when a variable lacked a natural unit and scale, I used their standardized z-scores instead.

Raw data was available from the survey's website. All linkage and data-cleaning was performed by the author. This primarily meant the recoding of missing values and some further recoding where consistently was lost throughout the years. On education, the highest grade completed by any family member in any given year was assigned to the family for that year. Annual working hours were constructed from average working weeks and working hours a week, if it was otherwise unavailable. Data on wealth was available as net value for various asset classes, which I collected into illiquid (real estate) and liquid categories (all the rest, including debts). Due to the limited availability of wealth data, I used them only to distinguish people facing liquidity constraints and compare the difference in behavior along this line.

Data on family size, the number of family members younger than 18, the age of the youngest child were all readily available in the survey. Educational attainment was asked in grades, and I attributed the highest number in the family to the family. Working hours were constructed as I reported, from data available on main jobs for one year prior to the interview.

In my reported results, I use logarithmized real consumption and income per

capita, using the definition of Laibson et al. (2007) of effective family size: all adults count as 1, while all younger than 18 count with 0.4 weight. This measure is dubious, and as a robustness check, I estimate all the regressions on families with no change in composition. To be able to compare the results with the other regressions, I use the same variable definitions, yet the scaling does not effect the coefficients in focus in these cases.

It is important to note that I did not wholly neglect family members apart from the head. Not only did I account for their possibly higher education, which I think is more appropriate to classify families, but I also linked all observations of the same person in the sample, which let me use the past of newly separated family heads as well, which is often available due to the design of the PSID, where all individuals once asked about are intended to be followed.

Rather many series, including the important ones of working hours and food consumption, exhibit an unfortunate break in 1994, as since then they were reported by the respondents, potentially on different time frames. This introduces undue noise into the data, as I present it in Section A.3 on page 53 with time-series of average food consumption in Figure A.1 on page 54. Still, I chose to do the analysis on this domain as well. It can be compared with some earlier estimates, like the baseline one of Laibson et al. (2007) for the years between 1978 and 1992. Also, the PSID turning two-yearly in 1998 led me to include the later years only in an estimation using the odd years of the full sample. This restriction was necessary since the specified relationship explicitly uses the dynamic nature of the data, and the unbalanced panel would not have allowed the analysis in the changes in consumption with respect to the change in income. This serves as a useful step to test the stability of the estimated moment over time and extend this common regression to the latest data available.

Price indices and interest rate series were downloaded from the FRED® (Federal Reserve Economic Data) service of Federal Reserve Bank of St. Louis. The standard, unadjusted Consumer Price Index series was used to deflate the nominal amounts

1982- to 1984 prices. A futile attempt to impute rents into consumption has been done using the average three-month treasury-bill yield on the reported dwelling value. As I argue in the next section, I had to neglect this measure in the end. Eventually, no attempt was made to adjust the price indices and interest rates for more relevant ones that a family probably faced. However, this is not because of lack of interest, but due to the complexity involved. Not even the corresponding regional CPI figures were available to attach to the data on the families, lest liquidity constraints or relevant consumption bundles.

These latter series were linked to the PSID data by calendar years, taking into account that the financial questions of the PSID concern the previous years. This discrepancy between the financial matters and other questions are usually overlooked in the literature, and I have not assumed the risk of further discrepancy, in family composition e.g., introduced by a simple shift of these "lagged" series.

4.2. Variable definitions

First I considered defining consumption using a common definition, as of Laibson et al. (2007, inter alia): food, rent plus utilities in the PSID. This was mainly due to data availability. However, after trying to replicate the setup of those authors as closely as possible, I turned to an even more restrictive definition, with only food expenditure on food at home or in restaurants, since rents are rarely reset each year — especially if it had to be imputed from the dwelling value for owner-occupiers, itself interpolated for the gap years, using the average yields on three-month treasury bills for each year.[2] In such a constructed measure, I hardly expected any meaningful change as an effect of income shocks, predictable or unpredictable. As further justification, I cite that Browning and Lusardi (1996) acknowledge the only consumption data available in the PSID to be exactly the two same kinds of food consumption measures I have

[2]Laibson et al. (2007) impute such rents using a long-term average rate of 5 % for all owner-occupiers. I also note that I did not find relevant data for utilities in the PSID even for the restricted period used by Laibson et al. (2007), apart from the cases when they are included in the rents — which makes imputation for owner-occupiers even more dubious.

fetched.[3] The method how Laibson et al. (2007) project total consumption from the PSID measure, using an estimated relationship between total consumption and food (plus rents and utilities) consumption at the CEX, would not have applied to my project sticking to the original micro data for the whole exercise.

If this basket represents a fixed proportion of income, like with Cobb-Douglas preferences (at least in composite goods), the marginal propensity to consume is the same for these as for overall consumption. It is reasonable to expect, however, that these goods have an income elasticity of consumption less than 1, which only implies that any evidence for a positive marginal propensity to consume these out of predictable income changes is evidence for the same for overall consumption.

[3]Though Browning and Lusardi (1996) also quote alarmingly high numbers how much of the between-year variation of food consumption (our chosen change in consumption measure) is actually noise: 76 % or even 95 % according to Runkle (1991) and Shapiro (1982), respectively.

Chapter 5

Results

5.1. Estimation results

I report the regressions following closely the specification of Laibson et al. (2007), with trend variables, demographic effects and business-cycle effects suggested. In addition, I disclose the ones with a more careful set of control variables, or even restricted to families not observing compositional change in the last year. Table headers note which time period and/or estimator distinguishes a model from its counterparts, but among the sets of three columns, the different control variables also determine different samples: in one sense because no effort was put into synchronizing the observations lost due to missing values in one or another control variable, but also because the models excluding even a control for the change in composition are the ones restricted to the sample of stable families. When breaking down samples, only Laibson's specification and this most restricted one were estimated.

5.1.1. Benchmark estimates

At first sight, my results approximate relatively well the original estimates of Laibson et al. (2007). Using annual data, their original sample period (1978-2002), and their simple 2SLS estimator (Table 5.1 on page 34, column 1), my estimate for the marginal propensity is 0.28. However, several caveats are due. First and

foremost, Hansen's overidentification test rejects the very strongly hypothesis of the instruments being valid in all specifications. It would not be surprising if the income from one-and-a-half or two years ago affected consumption growth, and not only possibly through the current income growth. This provides further justification to consider the two-yearly panel — unless the instruments become too weak in such long horizons. Also, I note that the test's rejection is not what obvious to attribute to, as Davidson and MacKinnon (2004, p.368), opine in feasible GMM estimation it is not unusual that the sample distribution of the test differs considerably from its asymptotic distribution. Having said that, the test rejects so strongly in such a large sample (over 100,000 observations for most specifications), that one can find small comfort in the general difficulties of small-sample GMM testing. However, as Laibson et al. (2007) do not report test results, doubts about the validity of the instruments casts doubt on their result as well.

Still on the benchmark, annual results (Table 5.1 on page 34), it is also to note that the estimated "excess sensitivity" is the smaller the more carefully one selects the control variables — they are the smallest in the cases where only families without compositional changes are in the sample (Columns 3, 6, 9, 12). Since the theories about excess sensitivity (hyperbolic discounting among them) are not about intrafamily matters, it is suspicious that a restricted set of 'business-as-usual' consumer reactions show much less sensitivity than the 23 % behind the β, δ estimates. Finally, once the equation is estimated on the longer panel, the marginal propensity turns significantly negative, at least in the more restrictive specifications.

Turning to the two-yearly panel (Table 5.2 on page 35), using both the considerably more recent data, 'purer' (though perhaps weaker) instruments and, alas, noisier variables, the estimates radically change. Even with the more restrictive specifications, and even the more robust estimator yields estimates economically and statistically significantly higher than the benchmark of Laibson et al. (2007), or for that matter many in the literature cited by Browning and Lusardi (1996). Even with the recent (noisy) data neglected, the point estimates of the excess sensitivity is still

above 50 %. As the overidentification test rejects only for some specifications of the full, long sample, while the other two robust tests strongly reject underidentification, these higher estimates are stronger results than the more common yearly estimation using immediate lags.

Panel regressions are usually an important step towards taking individual heterogeneity into account. However, I have already argued that I am not convinced that any constant individual effect in consumption change is meaningful, possible throughout almost four decades. I do not report random effects or fixed effects estimates. However, if nothing more, it is once more clear that predictable-income effects seem much larger using only two-yearly observations.

5.1.2. Groups with different educational attainment

Broken down by education, the regressions seem valid, though the two-yearly panel again yields the more reassuring test results. In addition, it also yields higher estimates overall (see Tables 5.3 on page 36 and 5.4 on page 37). However, contrary to the common notion about that self-control and cognitive abilities associated with higher education are also correlated with more time-consistent preferences, it is not present in the data if one identifies quasi-hyperbolic discounting from the excess smoothing. Households with a member who have finished some college significantly spend more of their anticipated income. I find no evidence for the claim of Laibson et al. (2007, footnote 7) that there are "qualitatively similar results across educational categories."

5.1.3. Groups with different health conditions

Again perhaps somewhat contrary to common intuition, but very much in line with the mortality reasoning behind discounting, people who report good health follow more closely they income with their consumption. (See Table 5.5 on page 38.)

Table 5.1.: Estimation on annual data

	1978-1992			Two-step GMM (1978-1992)			Robust estimation (1978-1992)			All available		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Δ logReal income	0.280***	0.177***	0.123**	0.346***	0.270***	0.156**	0.280***	0.177***	0.123**	0.218***	-0.102***	-0.272***
	(0.0176)	(0.0257)	(0.0430)	(0.0235)	(0.0337)	(0.0599)	(0.0176)	(0.0257)	(0.0430)	(0.0163)	(0.0259)	(0.0559)
Age (Head's)	0.00181**			0.00185***			0.00181**			-0.000906**		
	(0.000630)			(0.000420)			(0.000630)			(0.000309)		
Age^2	-0.0000229***			-0.0000246***			-0.0000229***			0.0000121***		
	(0.00000665)			(0.00000436)			(0.00000665)			(0.00000323)		
Hours worked (Head)	-0.00000570*			-0.0000110***			-0.00000570*			0.00000973***		
	(0.00000247)			(0.00000246)			(0.00000247)			(0.00000172)		
Head female (dummy)	-0.0123**			-0.0124***			-0.0123**			-0.0282***		
	(0.00402)			(0.00300)			(0.00402)			(0.00217)		
Δ Effective family size	-0.125***			-0.115***			-0.125***			-0.133***		
	(0.00377)			(0.00538)			(0.00377)			(0.00353)		
Education (highest in family)	-0.292**			-0.260**			-0.292**			-0.425***		
	(0.0917)			(0.0841)			(0.0917)			(0.0374)		
Education × calendar year	0.000146**			0.000130**			0.000146**			0.000215***		
	(0.0000462)			(0.0000424)			(0.0000462)			(0.0000188)		
Δ logGDP	0.00248**			0.00222*			0.00248**			-0.00114*		
	(0.000783)			(0.000874)			(0.000783)			(0.000567)		
Age of youngest (<17)		-0.000885**	-0.00227***		-0.000790***	-0.00219***		-0.000885**	-0.00227***		-0.00172***	-0.00403***
		(0.000328)	(0.000368)		(0.000210)	(0.000287)		(0.000328)	(0.000368)		(0.000167)	(0.000233)
D.Family_Size		-0.0988***			-0.0885***			-0.0988***			-0.124***	
		(0.00372)			(0.00526)			(0.00372)			(0.00377)	
Constant	-0.0298	-0.00936***	0.00155	-0.0181	-0.0115***	-0.00158	-0.0298	-0.00936***	0.00155	-0.0362***	-0.0231***	-0.00759***
	(0.0177)	(0.00248)	(0.00272)	(0.0143)	(0.00153)	(0.00193)	(0.0177)	(0.00248)	(0.00272)	(0.00903)	(0.00128)	(0.00168)
Observations	149532	125831	92436	149532	125831	92436	149532	125831	92436	240127	196406	141594
p-value of Hansen's J	7.88×10^{-22}	2.31×10^{-12}	4.59×10^{-08}	7.19×10^{-09}	1.05×10^{-08}	0.00000137				0.0000682	0.00401	0.000261
p-value of underidentification	0	7.67×10^{-211}	1.11×10^{-72}	8.59×10^{-69}	9.16×10^{-24}	0.000000602				1.16×10^{-118}	8.47×10^{-34}	4.73×10^{-10}
F statistic for weak identification	1139.6	487.5	166.3	174.5	59.05	14.19				329.0	87.12	21.68

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted. Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001

The dependent variable is in all cases the change in logarithmized real food consumption. All data come from the publicly available Panel Survey of Income Dynamics.

Income and expenditure on food have been deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.

The null hypotheses of the tests are that the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006), and their robust F statistic for Stock and Yogo (2004) whether the instruments are weak).

Table 5.2.: Estimation on two-yearly data

	1978-1992			Two-step GMM (1978-1992)			Robust estimation (1978-1992)			All available		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Δ logReal income	0.665***	1.251***	0.526***	0.677***	1.260***	0.592**	0.665***	1.251***	0.526***	0.342***	0.265	0.453**
	(0.0440)	(0.217)	(0.114)	(0.0462)	(0.282)	(0.187)	(0.0440)	(0.217)	(0.114)	(0.0303)	(0.141)	(0.144)
Age (Head's)	0.00354**			0.00357***			0.00354**			-0.00422***		
	(0.00127)			(0.000917)			(0.00127)			(0.000701)		
Age^2	-0.0000490***			-0.0000496***			-0.0000490***			0.0000488***		
	(0.0000135)			(0.00000948)			(0.0000135)			(0.00000728)		
Hours worked (Head)	-0.0000336***			-0.0000349***			-0.0000336***			0.0000166***		
	(0.00000684)			(0.00000683)			(0.00000684)			(0.00000447)		
Head female (dummy)	-0.00352			-0.00341			-0.00352			-0.0315***		
	(0.00800)			(0.00659)			(0.00800)			(0.00477)		
Δ Effective family size	-0.0806***			-0.0791***			-0.0806***			-0.126***		
	(0.00651)			(0.00777)			(0.00651)			(0.00551)		
Education (highest in family)	-0.614**			-0.590**			-0.614**			0.690***		
	(0.233)			(0.204)			(0.233)			(0.0496)		
Education × calendar year	0.000307**			0.000295**			0.000307**			-0.000345***		
	(0.000117)			(0.000103)			(0.000117)			(0.0000250)		
Δ logGDP	0.0118***			0.0116***			0.0118***			0.0124***		
	(0.00144)			(0.00170)			(0.00144)			(0.00109)		
Age of youngest (<17)		-0.000483	-0.000271		-0.000489	-0.000244		-0.000483	-0.000271		0.000595	0.000100
		(0.000967)	(0.000773)		(0.000816)	(0.000660)		(0.000967)	(0.000773)		(0.000458)	(0.000492)
D.Family_Size		0.000633			0.00135			0.000633			-0.0854***	
		(0.0185)			(0.0244)			(0.0185)			(0.0132)	
Constant	-0.0768*	-0.0309***	-0.0283***	-0.0727*	-0.0308***	-0.0315***	-0.0768*	-0.0309***	-0.0283***	-0.214***	-0.105***	-0.103***
	(0.0377)	(0.00722)	(0.00632)	(0.0320)	(0.00536)	(0.00562)	(0.0377)	(0.00722)	(0.00632)	(0.0241)	(0.00315)	(0.00388)
Observations	63932	54409	32593	63932	54409	32593	63932	54409	32593	99031	85388	52550
p-value of Hansen's J	0.520	0.962	0.0242	0.763	0.982	0.0308				0.00632	0.000984	0.170
p-value of underidentification	1.60×10^{-111}	1.07×10^{-09}	5.87×10^{-15}	1.55×10^{-80}	0.0000102	0.00737				1.02×10^{-99}	0.000000232	0.000308
F statistic for weak identification	257.1	20.67	32.83	188.0	11.62	5.014				241.6	15.69	8.404

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted. Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001

The dependent variable is in all cases the change in logarithmized real food consumption. All data come from the publicly available Panel Survey of Income Dynamics.

Income and expenditure on food have been deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.

The null hypotheses of the tests are that the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006), and their robust F statistic for Stock and Yogo (2004) whether the instruments are weak).

Table 5.3.: Estimation by educational attainment on annual data

	Below high school		High school graduate		Above high school	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ logReal income	0.0972***	-0.469***	0.239***	-0.123*	0.274***	0.143*
	(0.0263)	(0.132)	(0.0267)	(0.0583)	(0.0307)	(0.0692)
Age (Head's)	-0.00155**		0.00170**		-0.000343	
	(0.000594)		(0.000568)		(0.000569)	
Age^2	0.0000245***		-0.0000160**		-0.000000315	
	(0.00000589)		(0.00000618)		(0.00000615)	
Hours worked (Head)	0.0000254***		0.00000944**		-0.00000447	
	(0.00000296)		(0.00000315)		(0.00000308)	
Head female (dummy)	-0.0384***		-0.0162***		-0.0243***	
	(0.00411)		(0.00402)		(0.00393)	
Δ Effective family size	-0.152***		-0.134***		-0.117***	
	(0.00562)		(0.00584)		(0.00651)	
Education (highest in family)	-1.109***				-0.331***	
	(0.124)				(0.0493)	
Education × calendar year	0.000561***		0.000283***		0.000169***	
	(0.0000626)		(0.0000347)		(0.0000248)	
Δ logGDP	-0.00736***		0.00143		0.00112	
	(0.00118)		(0.000963)		(0.000846)	
Age of youngest (<17)		-0.00378***		-0.00374***		-0.00282***
		(0.000584)		(0.000370)		(0.000341)
Constant	-0.00933	-0.0449***	-6.835***	-0.00866**	-0.0558**	0.000968
	(0.0174)	(0.00556)	(0.833)	(0.00306)	(0.0186)	(0.00290)
Observations	76155	40786	84487	51718	79485	48987
p-value of Hansen's J	0.134	0.643	0.00641	0.640	0.00171	0.0306
p-value of underidentification	1.24×10^{-105}	0.000000167	2.65×10^{-57}	0.000000312	6.50×10^{-28}	0.000685
F statistic for weak identification	272.3	15.99	183.0	16.21	64.84	6.826

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted.
Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001
The dependent variable is in all cases the change in logarithmized real food consumption.
All data come from the publicly available Panel Survey of Income Dynamics.
Income and expenditure on food have been deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.
The null hypotheses of the tests are that the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006), and their robust F statistic for Stock and Yogo (2004) whether the instruments are weak).

5.1.4. Groups with and without access to liquid and illiquid wealth

I find no evidence that people differ in their consumption's excess sensitivity along the lines of whether they have liquid savings or not, which contradicts the interpretation that whatever their cause, liquidity constraints are the direct determinants of excess sensitivity. The evidence is inconclusive on which group reacts more to anticipated income changes, the one with very low liquid assets at most, or the one with some. By some estimates (in the most restricted specification, see Tables 5.6 on page 39 and 5.7 on page 40) those with some liquid wealth react more. This result is similar to the one of Shea (1995).

Meanwhile on illiquid (housing) wealth, the evidence refutes the notion that those precommit themselves to illiquid savings who have self-control problems. Instead of

Table 5.4.: Estimation by educational attainment on two-yearly data

	Below high school		High school graduate		Above high school	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ logReal income	0.365***	-0.157	0.354***	0.162	0.315***	1.003***
	(0.0759)	(0.369)	(0.0569)	(0.0848)	(0.0389)	(0.261)
Age (Head's)	-0.00843***		-0.000141		-0.00102	
	(0.00184)		(0.00122)		(0.00113)	
Age^2	0.000107***		0.00000260		0.00000685	
	(0.0000175)		(0.0000129)		(0.0000118)	
Hours worked (Head)	0.0000380**		0.0000154		0.00000259	
	(0.0000135)		(0.00000868)		(0.00000557)	
Head female (dummy)	-0.0556***		-0.0320***		-0.00785	
	(0.0122)		(0.00818)		(0.00770)	
Δ Effective family size	-0.168***		-0.127***		-0.104***	
	(0.0125)		(0.00915)		(0.00764)	
Education (highest in family)	-1.759***				0.748***	
	(0.278)				(0.0572)	
Education × calendar year	0.000892***		-0.000282***		-0.000374***	
	(0.000140)		(0.0000557)		(0.0000288)	
Δ logGDP	0.00729*		0.0141***		0.0193***	
	(0.00284)		(0.00187)		(0.00142)	
Age of youngest (<17)		-0.000892		-0.000243		0.000979
		(0.00155)		(0.000741)		(0.000908)
Constant	-0.180*	-0.155***	6.493***	-0.0878***	-0.299***	-0.126***
	(0.0728)	(0.0258)	(1.344)	(0.00605)	(0.0372)	(0.0107)
Observations	18142	8643	39152	20756	41737	23009
p-value of Hansen's J	0.0300	0.0632	0.163	0.0193	0.0992	0.843
p-value of underidentification	4.47×10^{-29}	0.0270	6.38×10^{-33}	0.000233	3.09×10^{-57}	0.000120
F statistic for weak identification	71.93	3.623	78.05	9.288	135.9	9.093

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted.
Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001
The dependent variable is in all cases the change in logarithmized real food consumption.
All data come from the publicly available Panel Survey of Income Dynamics.
Income and expenditure on food have been deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.
The null hypotheses of the tests are that the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006), and their robust F statistic for Stock and Yogo (2004) whether the instruments are weak).

the self-imposed liquidity constraint increasing such a presumed effect, the simple comparison of groups with and without illiquid wealth shows that those *without* such assets follow more closely their income with their consumption. (Tables 5.8 and 5.9)

5.1.5. Groups according to alcohol use

Here again, somewhat counterintuitively and contrary to the experimental findings. only those who do not drink show statistically significant excess smoothing (Table 5.10), though much smaller samples due to data availability weaken this conclusion.

Table 5.5.: Estimation by health scores of the head on two-yearly data

	Health: excellent		Health: very good		Health: good		Health: fair		Health: poor	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Δ logReal income	0.400***	1.938	0.457***	1.252**	0.598***	0.828*	0.729***	3.121	0.367*	0.503*
	(0.103)	(1.331)	(0.0864)	(0.392)	(0.166)	(0.370)	(0.167)	(1.969)	(0.143)	(0.246)
Age (Head's)	0.000105		0.00439		-0.00163		-0.00825		-0.00379	
	(0.00331)		(0.00277)		(0.00323)		(0.00611)		(0.00900)	
Age^2	-0.00000948		-0.0000544		-0.000000956		0.0000619		0.0000566	
	(0.0000351)		(0.0000281)		(0.0000322)		(0.0000541)		(0.0000724)	
Hours worked (Head)	-0.0000517*		-0.0000219		-0.0000540*		-0.0000267		0.0000436	
	(0.0000217)		(0.0000132)		(0.0000262)		(0.0000215)		(0.0000283)	
Head female (dummy)	-0.0137		-0.00146		-0.0232		0.0331		-0.0765*	
	(0.0220)		(0.0171)		(0.0187)		(0.0274)		(0.0373)	
Δ Effective family size	-0.113***		-0.0948***		-0.0490		-0.0890**		-0.116**	
	(0.0164)		(0.0183)		(0.0270)		(0.0326)		(0.0445)	
Education (highest in family)	1.791***		1.572***		1.416***		2.052***		1.918***	
	(0.160)		(0.135)		(0.175)		(0.297)		(0.443)	
Education × calendar year	-0.000902***		-0.000787***		-0.000710***		-0.00104***		-0.000973***	
	(0.0000805)		(0.0000678)		(0.0000880)		(0.000149)		(0.000222)	
Δ logGDP	0.0218***		0.0357***		0.0330***		0.0286***		-0.0130	
	(0.00383)		(0.00321)		(0.00424)		(0.00661)		(0.00793)	
Age of youngest (<17)		-0.00685		-0.0000647		-0.00514		-0.00228		0.00436
		(0.00666)		(0.00214)		(0.00324)		(0.00896)		(0.00519)
Constant	-0.0539	-0.154**	-0.457***	-0.154***	-0.176	-0.0805***	0.153	-0.0771	0.227	-0.104**
	(0.103)	(0.0547)	(0.0726)	(0.0195)	(0.111)	(0.0200)	(0.197)	(0.0602)	(0.303)	(0.0370)
Observations	9986	6220	13393	8790	12471	8184	5616	3756	2360	1636
p-value of Hansen's J	0.122	0.316	0.174	0.883	0.248	0.656	0.232	0.981	0.00683	0.0392
p-value of underidentification	1.43×10^{-12}	0.475	8.17×10^{-17}	0.00354	0.000000493	0.0160	1.17×10^{-09}	0.256	0.000145	0.0136
F statistic for weak identification	26.71	0.722	38.89	6.455	15.51	4.456	21.49	1.411	8.650	5.143

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted.

Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001

The dependent variable is in all cases the change in logarithmized real food consumption.

All data come from the publicly available Panel Survey of Income Dynamics.

Income and expenditure on food have been deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.

The null hypotheses of the tests are that the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006), and their robust F statistic for Stock and Yogo (2004) whether the instruments are weak).

Table 5.6.: Estimation by by the availability of liquid wealth on annual data

	No liquid wealth		Some liquid wealth	
	(1)	(2)	(3)	(4)
Δ logReal income	0.221***	-0.258***	0.259*	0.229
	(0.0166)	(0.0550)	(0.108)	(0.153)
Age (Head's)	-0.000925**		0.00105	
	(0.000317)		(0.00286)	
Age^2	0.0000126***		-0.0000294	
	(0.00000332)		(0.0000291)	
Hours worked (Head)	0.0000103***		-0.0000389***	
	(0.00000176)		(0.0000118)	
Head female (dummy)	-0.0275***		-0.0124	
	(0.00222)		(0.0192)	
Δ Effective family size	-0.134***		-0.0681*	
	(0.00356)		(0.0268)	
Education (highest in family)	-0.412***		-0.00101	
	(0.0374)		(0.00275)	
Education × calendar year	0.000209***			
	(0.0000188)			
Δ logGDP	-0.00122*			
	(0.000567)			
Age of youngest (<17)		-0.00387***		-0.00342**
		(0.000240)		(0.00111)
Constant	-0.0371***	-0.0112***	0.116	0.0328**
	(0.00924)	(0.00178)	(0.0749)	(0.0127)
Observations	234143	136840	5984	4754
p-value of Hansen's J	0.0000185	0.000596	0.165	0.285
p-value of underidentification	9.38×10^{-110}	1.64×10^{-10}	0.000335	0.0173
F statistic for weak identification	300.7	22.94	6.427	3.626

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted.
Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001
The dependent variable is in all cases the change in logarithmized real food consumption.
All data come from the publicly available Panel Survey of Income Dynamics.
Income and expenditure on food deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.
Null hypotheses of the tests: the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006) and F statistic for Stock and Yogo (2004) whether the instruments are weak).

5.1.6. Groups with attitude indices

Along various attitude indices that have been asked (most recently in the seventies), which I summarize in Appendix A.2 on page 52, the groups below, around or above the mean rarely differed in their consumption behavior. In Table 5.11 on page 44 I report the Euler regression along the lines of a question whether the head of the household prefers saving to consumption. These answers were somewhat correlated with the excess sensitivity estimates from the two-yearly panel, yet even these results are weak.

Table 5.7.: Estimation by by the availability of liquid wealth on two-yearly data

	No liquid wealth		Some liquid wealth	
	(1)	(2)	(3)	(4)
Δ logReal income	0.288***	0.0865	0.198*	0.278
	(0.0295)	(0.0890)	(0.0877)	(0.164)
Age (Head's)	-0.00434***		0.00199	
	(0.000700)		(0.00350)	
Age^2	0.0000552***		-0.0000278	
	(0.00000730)		(0.0000323)	
Hours worked (Head)	0.0000325***		-0.00000988	
	(0.00000452)		(0.00000942)	
Head female (dummy)	-0.0448***		0.0356	
	(0.00483)		(0.0186)	
Δ Effective family size	-0.133***		-0.192***	
	(0.00528)		(0.0332)	
Education (highest in family)	-0.339***		1.641	
	(0.0800)		(6.829)	
Education × calendar year	0.000175***		-0.000817	
	(0.0000403)		(0.00341)	
Δ logGDP	0.0104***		0.242***	
	(0.00115)		(0.0298)	
Age of youngest (<17)		-0.00202***		-0.00124
		(0.000457)		(0.00153)
Constant	-0.212***	-0.0474***	-1.632***	-0.358***
	(0.0245)	(0.00365)	(0.187)	(0.00901)
Observations	90900	46452	8131	6098
p-value of Hansen's J	0.0284	1.87e-17	0.629	0.000000140
p-value of underidentification	5.03×10^{-91}	0.00125	7.65×10^{-21}	0.00000304
F statistic for weak identification	218.2	6.961	57.59	14.86

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted.
Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001
The dependent variable is in all cases the change in logarithmized real food consumption.
All data come from the publicly available Panel Survey of Income Dynamics.
Income and expenditure on food deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.
Null hypotheses of the tests: the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006) and F statistic for Stock and Yogo (2004) whether the instruments are weak).

5.2. Specification tests

For all the reported regressions, I have reported robust test results on overidentification (Hansen's J test), weak instruments (the Wald F statistic of Kleibergen and Paap (2006) with critical values tabulated by Stock and Yogo (2004)) or underidentification (the LM statistic of Kleibergen and Paap (2006)). Empirical significance levels for the most robust versions of the tests available have been reported in the tables of results.[1] The two tests on underidentification have rejected the null of

[1] As the state of the art, I used the recommended and robust options of the `IVREG2` package of Baum, Schaffer and Stillman (2002), documented in Baum, Schaffer and Stillman (2007), and

Table 5.8.: Estimation by the availability of illiquid wealth on annual data

	Illiquid wealth		Some illiquid wealth	
	(1)	(2)	(3)	(4)
Δ logReal income	0.296***	-0.177	0.113***	-0.271***
	(0.0243)	(0.0926)	(0.0217)	(0.0583)
Age (Head's)	-0.000535		-0.00333***	
	(0.000567)		(0.000392)	
Age^2	0.0000102		0.0000302***	
	(0.00000602)		(0.00000400)	
Hours worked (Head)	0.00000880*		0.00000574***	
	(0.00000356)		(0.00000165)	
Head female (dummy)	-0.0241***		-0.0257***	
	(0.00358)		(0.00305)	
Δ Effective family size	-0.119***		-0.166***	
	(0.00456)		(0.00549)	
Education (highest in family)	-0.539***		-0.378***	
	(0.0752)		(0.0384)	
Education × calendar year	0.000273***		0.000191***	
	(0.0000379)		(0.0000193)	
Δ logGDP	-0.00330**		0.000760	
	(0.00108)		(0.000585)	
Age of youngest (<17)		-0.00582***		-0.00324***
		(0.000549)		(0.000224)
Constant	-0.0438*	-0.00621	0.0354***	-0.00858***
	(0.0171)	(0.00324)	(0.0102)	(0.00184)
Observations	102296	54390	137831	87204
p-value of Hansen's J	0.00388	0.148	0.0321	0.00872
p-value of underidentification	1.41×10^{-90}	0.000416	8.56×10^{-29}	0.000000464
F statistic for weak identification	223.9	7.872	78.66	15.16

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted.
Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001
The dependent variable is in all cases the change in logarithmized real food consumption.
All data come from the publicly available Panel Survey of Income Dynamics.
Income and expenditure on food deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.
Null hypotheses of the tests: the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006) and F statistic for Stock and Yogo (2004) whether the instruments are weak).

weak instruments in most cases. This also implies that the estimates robust to weak instrument, as suggested by Mikusheva and Poi (2006)[2], are not superior to the other estimates.

On the other hand, the null hypothesis of serially uncorrelated errors could not have even been tested. Computational constraints forced me to estimate the equations with no respect for possible serial correlation. This is likely to be a serious issue in a

also the implementation by Kleibergen and Schaffer (2007). The exact settings are available from the author, or publicly on the referred website.

[2]This method was implemented in the `CONDIVREG` Stata command of Mikusheva and Poi.

Table 5.9.: Estimation by the availability of illiquid wealth on two-yearly data

	No illiquid wealth		Some illiquid wealth	
	(1)	(2)	(3)	(4)
Δ logReal income	0.523***	0.801	0.135***	0.351**
	(0.0498)	(0.494)	(0.0376)	(0.111)
Age (Head's)	-0.00619***		-0.00714***	
	(0.00148)		(0.000869)	
Age^2	0.0000681***		0.0000677***	
	(0.0000154)		(0.00000870)	
Hours worked (Head)	0.00000694		0.0000105**	
	(0.0000105)		(0.00000406)	
Head female (dummy)	-0.0273***		-0.0222***	
	(0.00824)		(0.00634)	
Δ Effective family size	-0.104***		-0.172***	
	(0.00781)		(0.00801)	
Education (highest in family)	0.253*		0.739***	
	(0.119)		(0.0511)	
Education × calendar year	-0.000125*		-0.000367***	
	(0.0000598)		(0.0000257)	
Δ logGDP	0.00573**		0.0193***	
	(0.00210)		(0.00112)	
Age of youngest (<17)		-0.000776		0.00111*
		(0.00190)		(0.000506)
Constant	-0.0863	-0.0953***	-0.203***	-0.111***
	(0.0516)	(0.0114)	(0.0247)	(0.00414)
Observations	40018	18443	59013	34107
p-value of Hansen's J	0.184	0.454	0.000128	0.430
p-value of underidentification	9.32×10^{-50}	0.240	9.46×10^{-43}	0.0000194
F statistic for weak identification	108.0	1.498	102.5	12.06

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted.
Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001
The dependent variable is in all cases the change in logarithmized real food consumption.
All data come from the publicly available Panel Survey of Income Dynamics.
Income and expenditure on food deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.
Null hypotheses of the tests: the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006) and F statistic for Stock and Yogo (2004) whether the instruments are weak).

dynamic setup like this one, yet both the available options in ready-made software and time constraints led me to leave a more satisfying specification to further studies.

As I have already noted above, the overidentification tests have rejected rather many specifications. Though its meaning is not clear, one should be somewhat sceptical about those results. It happens to affect more often the yearly panel used in most studies with such lagged values as instruments.

Table 5.10.: Estimation by the drinking behavior of the head on two-yearly data

	Less than one drink per day		More than one drink per day	
	(1)	(2)	(3)	(4)
Δ logReal income	0.384***	1.075***	0.158	0.629
	(0.105)	(0.250)	(0.133)	(0.366)
Age (Head's)	0.00826*		0.00206	
	(0.00400)		(0.00521)	
Age^2	-0.0000869*		-0.00000949	
	(0.0000368)		(0.0000482)	
Hours worked (Head)	-0.0000249*		0.00001000	
	(0.0000122)		(0.0000138)	
Head female (dummy)	0.0261		0.00502	
	(0.0196)		(0.0310)	
Δ Effective family size	-0.134***		-0.189***	
	(0.0364)		(0.0462)	
Education (highest in family)	-5.720		-3.533	
	(7.632)		(12.12)	
Education × calendar year	0.00285		0.00177	
	(0.00381)		(0.00605)	
Δ logGDP	0.214***		0.204***	
	(0.0323)		(0.0512)	
Age of youngest (<17)		-0.00251		-0.00406
		(0.00245)		(0.00279)
Constant	-1.589***	-0.520***	-1.538***	-0.153***
	(0.187)	(0.0153)	(0.328)	(0.0154)
Observations	6795	4958	4379	3251
p-value of Hansen's J	0.148	0.0000407	0.136	0.00932
p-value of underidentification	6.95×10^{-23}	6.69×10^{-09}	2.51×10^{-09}	0.0843
F statistic for weak identification	58.66	20.20	24.99	2.958

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted.
Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001
The dependent variable is in all cases the change in logarithmized real food consumption.
All data come from the publicly available Panel Survey of Income Dynamics.
Income and expenditure on food deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.
Null hypotheses of the tests: the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006) and F statistic for Stock and Yogo (2004) whether the instruments are weak).

Table 5.11.: Estimation by score on "Head prefers saving" on two-yearly data

	Below 1/2 S.D. of mean		Within 1/2 S.D. around mean		Above 1/2 S.D. of mean	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ logReal income	0.395***	0.867	0.323***	0.534	0.323***	0.127
	(0.0774)	(0.819)	(0.0416)	(0.276)	(0.0527)	(0.0750)
Age (Head's)	-0.000492		-0.00517***		-0.00590***	
	(0.00153)		(0.00117)		(0.00148)	
Age^2	0.0000148		0.0000562***		0.0000662***	
	(0.0000160)		(0.0000121)		(0.0000153)	
Hours worked (Head)	0.0000190		0.0000152*		0.0000186*	
	(0.0000111)		(0.00000666)		(0.00000818)	
Head female (dummy)	-0.0293**		-0.0411***		-0.0244*	
	(0.0108)		(0.00806)		(0.00980)	
Δ Effective family size	-0.114***		-0.137***		-0.121***	
	(0.0141)		(0.00802)		(0.00987)	
Education (highest in family)	0.888***		0.668***		0.517***	
	(0.121)		(0.0832)		(0.0931)	
Education × calendar year	-0.000445***		-0.000333***		-0.000256***	
	(0.0000610)		(0.0000419)		(0.0000469)	
Δ logGDP	0.00901***		0.00930***		0.0166***	
	(0.00269)		(0.00176)		(0.00210)	
Age of youngest (<17)		0.000182		0.00162		0.00112
		(0.00163)		(0.000931)		(0.000879)
Constant	-0.255***	-0.0997***	-0.165***	-0.109***	-0.236***	-0.105***
	(0.0521)	(0.0115)	(0.0395)	(0.00758)	(0.0481)	(0.00688)
Observations	22555	11956	33516	17743	23788	12642
p-value of Hansen's J	0.0324	0.868	0.0996	0.0225	0.674	0.446
p-value of underidentification	4.07×10^{-17}	0.404	1.60×10^{-47}	0.00436	5.87×10^{-49}	0.0000367
F statistic for weak identification	38.83	0.890	138.9	5.801	108.1	17.42

Source: author's own calculations. Cluster-robust (within households) two-step GMM estimators unless otherwise noted.
Standard errors in parentheses. * p<0.05, ** p<0.01, *** p<0.001
The dependent variable is in all cases the change in logarithmized real food consumption.
All data come from the publicly available Panel Survey of Income Dynamics.
Income and expenditure on food have been deflated to the 1982-1984 price level by the CPIAUCNS series from the Federal Reserve.
The null hypotheses of the tests are that the instruments are valid (Hansen's J statistic) and the equation is under- or weakly identified (robust rank test of Kleibergen and Paap (2006), and their robust F statistic for Stock and Yogo (2004) whether the instruments are weak).

Chapter 6

Discussion

Overall, my results both confirm the approach of Laibson et al. (2007) and weaken their results. By this I mean that the key moment is indeed significantly different from zero under a wide range of specifications and on various domains. There is indeed a comovement of predictable income and consumption. Some form of time-inconsistency with self-control problems like in the case of quasi-hyperbolical discounting provide a plausible explanation. That said, the point estimate itself varies considerably among specifications, and it is key in the process to estimate the time-preference parameters, which could thus also differ accordingly. This points out a weak point in the sophisticated method of the authors, who while introducing impressively many extensions (e.g. in bequest motives, survival rates between and within couples, different intrahousehold returns to scale, liquidity constraints, just to name a few) neglect the statistical properties of the inputs of their model. Laibson et al. (2007, Section 6.6) comment on that they could have chosen different moments to estimate their model (like illiquidity of investment by Angeletos et al. (2001)), and "analyzing different moments is a potential test of [their] model and a priority for future research", yet they do not use the second moment of the marginal propensity to consume, which we have seen is sizable.[1]

[1] Indeed its size is some defense for the calibration, as different regression provided overlapping confidence intervals. Yet since this overlap is due to the large standard errors (even with so many datapoints), this should be small comfort for such a macroeconomic calibration-simulation exercise.

Also, the range of results can be disturbing in another sense. It is interesting to see how different smokers and non-smokers turn out to be in their savings behavior. The differences in self-control could be an appealing explanation for their addiction itself. However, the same line of thought adapted to case of health, net wealth or educational attainment leads to a problem of this research agenda. The differences in the estimated moments would directly imply *worse* self-constraint problems for the better educated, at least while the point estimates are used only (neglecting the fact that the, say, 95 % confidence intervals might overlap). Taking into account that these groups are formed only along wide cross-sections of society, and they themselves are large samples of people otherwise considered to be similar, the differences implied by these results and the method of Laibson et al. (2007) are unrealistic (though admittedly not recalculated here).

On the presented results, one further remark is justified. Admittedly, I do not *test* in all cases, whether the estimated moments are statistically significantly different from the one used by Laibson et al. (2007) and each other. However, it also immediately follows that the method chosen in that work loses the possible heterogeneity between groups or time periods. The other side of the same coin is that during the estimation of the final β and δ the uncertainty inherent in the estimation of the key moment of the marginal propensity to consume, or for that matter all the other moments, parameters and statistics taken from elsewhere to calibrate and estimate upon.

Furthermore, the fact that some plausible estimates even on the whole population showed much higher excess sensitivity than Laibson et al. (2007) calculate with, questions their method used to estimate the time-preference parameters, since such higher moments would imply implausibly higher discount rates, as one discerns from their comment on whether the unobserved part of consumption reacts more to income and that that means that their discount rate estimates are downward biased. The difference shown by the two-yearly data, even for simple food consumption, is much higher than that prospect to be an option.

For further research, it is noteworthy that the excess sensitivity estimates differed considerably for those who had been living in an unperturbed family. However, the evidence is far from definitive that the consensus values of a 0-0.5 marginal propensity to consume out of predictable income changes would be spurious. In some cases, the estimates using these observations are actually higher. Still, the general pattern seems to suggest that the effect on such households tends to be smaller.

6.1. Extensions

For further research, it would be definitely worth to specify a proper empirical study using only the micro data itself. Once that done, and assuming that that exercise would reassuringly confirm the hypothesis of quasi-hyperbolical discounting, it would be also important to check whether some further hypotheses, maintained throughout this thesis, hold. An important way forward would be to reconsider whether people, however futile their efforts to control themselves, are perfectly aware of the availability of their options to save and borrow, and allocate their money between those vehicles and the final use, viz. consumption, freely. Contrary to this is the evidence for mental accounting, one implication of which is that people allocate separate budgets for different purposes, and react to changes with less than perfect substitution between the budgets (see Thaler, 1999, for a brief survey). This phenomenon is usually confirmed empirically, and if it mattered even in the case of life-cycle savings, it could imply that the source of income determines whether it is spent or saved. Even the restrictive definition of consumption used in this paper (i.e. food consumption) is likely to be affected. However, even the PSID did not provide enough consistent data to pursue this extension, and this strand of research was abandoned.

One must also note that even this thesis barely scratches the surface on the issue of homogeneity and idiosyncrasies in the population: I discuss the main parameter of Laibson et al. (2007) in terms of differences along some dividing line of the

population (education, mainly), yet I still do not identify individual parameters of time-preference and self-control. Though individual heterogeneity, or even possible 'heterogeneity' throughout the life cycle of each of us, is an evidently important issue[2], I also estimated a more conservative, homogenous model for various groups. The estimation of individual parameters is a huge promise of panel studies and the findings can be of obvious importance.[3] With some assumptions about the expectations about the earnings profile, mortality or the bequest motives, the newly fashionable longitudinal financial surveys (primarily focusing on retirement[4]) would allow some sort of such investigation.

[2]None less but Kenneth Arrow has noted that "One of the things that microeconomics teaches you is that individuals are not alike" (admittedly after saying "Well, I've got to tell you: I've never understood macro."). (Colander, Holt and Rosser, 2004, p.301)

[3]Agarwal, Driscoll, Gabaix and Laibson (2008) investigate the behavior of various age groups on the credit market. An interesting finding of theirs is that middle-aged borrowers (around 53) pay lower fees and interest payments than their younger and older counterparts, *ceteris paribus*. Also, Laibson et al. (2007) comment on some simulation of theirs allowing some calibrated heterogeneity.

[4]The Health and Retirement Study on Americans above the age of fifty, run by the same research center as the PSID, or the English Longitudinal Study of Ageing are the prime candidates to identify time preferences around retirement fully from micro data.

Chapter 7

Conclusion

The issue of time-inconsistent preferences is of obvious importance. As I already noted some examples in Section 2.1 on page 11, some of the literature, like Thaler and Benartzi (2004) on retirement savings and Paserman (Forthcoming) on the job market already address welfare issues and the implied policy recommendations. Thaler and Sunstein (2008, 2003) even provide some philosophical justification and have dedicated a book to practical applications. For the academic economic profession, Beshears, Choi, Laibson and Madrian (2008) have written a brief but insightful paper when and why we cannot rely on revealed preferences for normative purposes, meaning nothing less than "the economic actor's true interests". A prime case is when some behavior is the result of a time-inconsistent decision, as is the case with hyperbolic discounting explored in this thesis. What is more, structural estimation is a leading recommendation of the authors that benevolent policy-makers (or at least academics) can do to get closer to the normative preferences.

This thesis has not gone that far to contribute hard results on such preferences, though it repeatedly noted the importance of such empirical research, and also its possibility with more and more panel surveys on savings available. That said, this thesis has raised warnings about the foundations hyperbolical discounting have recently been discerned from. The results are important reminders how important field data actually are, and what richness and statistical rigor is lost when they are

left behind. The estimated time-preference parameters from the path-breaking field studies of life-cycle savings are put into perspective seeing the different point estimates reproduced in this paper. That the same data with some plausible specification (and sometimes more attractive statistical properties) have produced remarkably higher estimated 'excess sensitivity' suggests that self-constraint problems can be even more prominent, and at least, heterogeneity cannot be totally neglected during its analysis. This thesis contributes to the empirical literature on consumption and savings, and mostly questions the robustness of the recent estimates of time-inconsistent preferences for household saving behavior.

Appendix A

Data

A.1. Descriptive statistics

Table A.1.: Summary statistics

Variable	Mean	Standard Deviation	Minimum	Maximum	# observations
Female	0.344	0.475	0	1	736814
Age (Head)	42.653	14.857	15	101	454099
Age (Wife)	39.44	12.851	14	93	293179
# Drinks (Head)	0.786	1.274	0	25	27981
Self-reported horizon index, 1-8	3.995	2.295	0	8	614924
Assets: dwelling value	34695.45	72421.798	0	9999996	453025
Health (Head)	2.514	1.155	1	5	199699
# children below 18	1.659	1.84	0	13	454205
Age of youngest (<18)	5.086	5.229	0	19	389663
Income	10259.799	111372.675	-36750	50000000	413164
Current region	2.632	0.947	0	6	405064
Head being female	0.276	0.447	0	1	454187
Actual rents	998.71	2993.282	0	1199952	430212
Family size	3.753	2.229	1	19	454205
Annual hours worked (Head)	1495.981	1013.93	0	5824	447343
Annual hours worked (Wife)	562.583	862.518	0	10864	451350
Annual vehicle insurance per net value	0.647	11.022	0	400.302	235960
Mortgage_interest_rate_on_t	4.322	4.08	0	75	33189
z: Carries out plans (Head)	0.014	0.995	-1.969	0.752	544816
z: Carries out plans (family 1976)	-0.014	0.995	-0.752	1.969	544816
z: Plans ahead (Head)	-0.002	0.996	-1.33	1.533	614414
z: Plans ahead (family 1976)	-0.005	0.998	-1.153	0.938	546346
z: Prefers saving (Head)	-0.019	1.001	-1.596	1.633	614074
z: Prefers saving (Head, latest)	0.003	0.997	-0.787	1.512	572390
z: Thinks about future (Head)	-0.015	1.001	-1.249	1.776	614414
z: Thinks about future (Head, latest)	-0.029	1.002	-1.131	0.930	573410
# smoked per day (average)	2.592	5.104	0	50	264316
Effective family size	2.758	1.31	0.8	13.6	454205
z: Test score (sentence completion)	0.028	0.986	-3.538	1.641	573546

Continued on next page...

... table A.1 continued

Variable	Mean	Standard Deviation	Minimum	Maximum	# observations
Highest education in family	6.385	6.541	0	18	714262
Liquid assets	7327.504	219460.941	-224770	100564000	736814
Illiquid assets	22949.082	93894.040	-71319	25450000	736814
All assets	30276.585	258975.911	-224770	100914000	736814
Net wealth	164445.544	845778.272	-9956997	100914000	65740
Food consumption	1995.707	118076.735	0	52002548	454205
Imputed assets dwelling value	34526.426	71598.047	0	9999996	479742
Change in Log GDP	7.667	2.649	3.168	12.989	736818
CPIAUCNS (annual mean)	99.653	47.22	33.358	188.883	736818
TB3MS (annual mean)	6.326	2.641	1.011	14.025	736818
Rents	3012.619	4450.126	0	1199952	454204
Imputed-interpolated rents	2077.63	3890.822	0	299749.875	479742
Imputed rents	2969.623	4419.635	0	1199952	480279
Consumption (Food)	3350.203	118128.292	0	52003748	454205
Consumption (imputed)	3350.203	118128.292	0	52003748	454205
Real consumption	33.495	797.002	0	350843.313	454205
Real consumption (imputed)	33.495	797.002	0	350843.313	454205
Real food consumption	19.365	796.62	0	350835.219	454205
Real income	110.287	650.148	-225.449	290360.031	413164
Log income	8.641	1.158	-2.001	17.728	412965
Log consumption	7.544	1.001	-4.201	17.767	440754
Log consumption (imputed)	7.544	1.001	-4.201	17.767	440754
Log food consumption	7.056	0.878	-1.03	17.767	407457
Log real income	4.343	0.963	-6.945	12.579	412965
Log real consumption	3.198	0.761	-9.174	12.768	440754
Log real consumption (imputed)	3.198	0.761	-9.174	12.768	440754
Log real food consumption	2.701	0.679	-5.599	12.768	407457

A.2. Behavioral indices

Among the rich data on the individuals in the PSID (at least in some point in their past 40 years) I found the following attitude indices and test scores possibly relevant to their saving behavior, time-consistent or not. If data allowed, I broke down the sample into groups with scores within one half standard error around the mean, or below, or above. Though the data is rather old, it is available for a relatively large subsample (more than 10,000 observations with scores on most questions), and I exercised caution while inferring from those questions to later life. At least, I averaged out multiply recorded data, if any available, yet also considered to use the latest (in 1976, closest to most observations). The more interesting results have been shown in Section 5.1.6 on page 39.

Whether the head carries out plans (Attitude items, head) in 1968, 1969, 1970, 1971, 1972 and 1975 was asked with following question was set up: "When you make plans ahead, do you usually get to carry out things the way you expected, or do things usually come up to make you change your plans?" The answer could have been chosen on a scale of 1-6. (Value

1: Usually gets to carry out things the way expected, value 6: Does not plan)

About their habit to plan ahead (Attitude items, head) heads were asked in the same years. The question was: "Are you the kind of person that plans his life ahead all the time, or do you live more from day to day?" The answer could have been given on a scale of 1-5. (Value 1: Plans ahead, value 5: Lives more from day to day).

About their preferences over saving to spending (Attitude items, head) heads were asked in 1968, 1969, 1970, 1971 and 1972. The question was the following: "Would you rather spend your money and enjoy life today or save more for the future?" The answer could have been given on a 1-5 scale. (Value 1: Would rather spend money and enjoy life today, value 5: Save more for the future).

Heads were asked how they think about future (Attitude items, head) in 1968, 1969, 1970, 1971, 1972. The question was: "Do you think a lot about things that might happen in the future, or do you usually just take things as they come?" The answer could have been given on a 1-5 scale. (Value 1: Think a lot about things that might happen, value 5: Usually just take things as they come).

A.3. Income and consumption over time

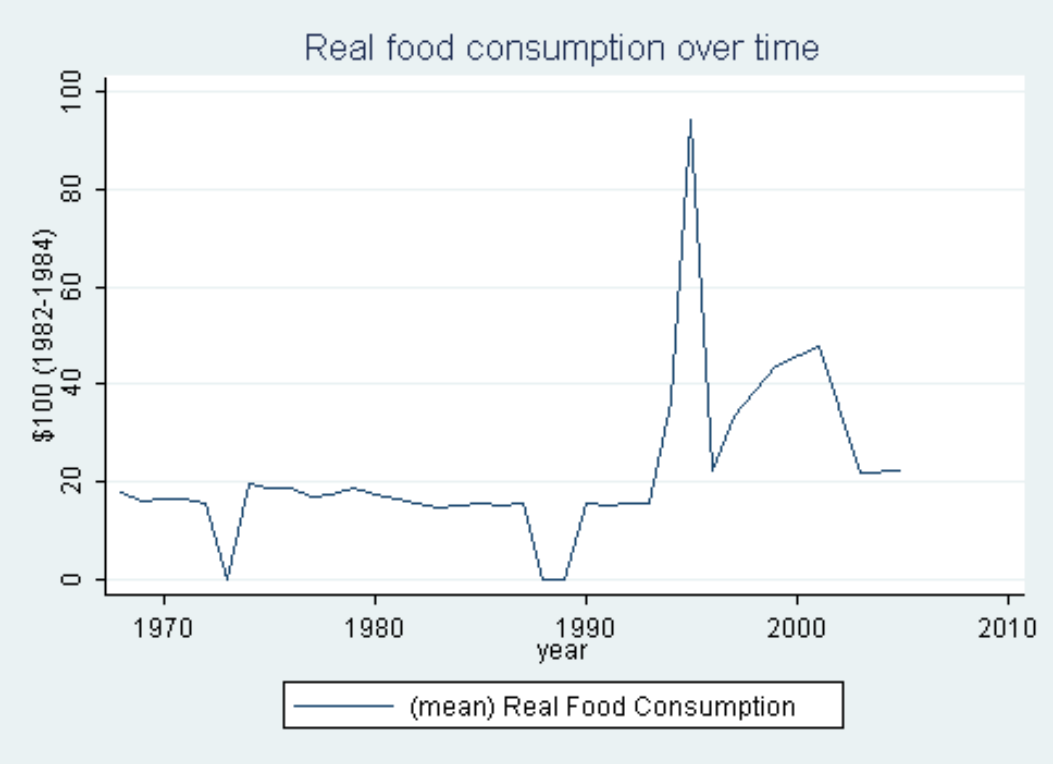

Figure A.1.: Average real amount spent on food, by years

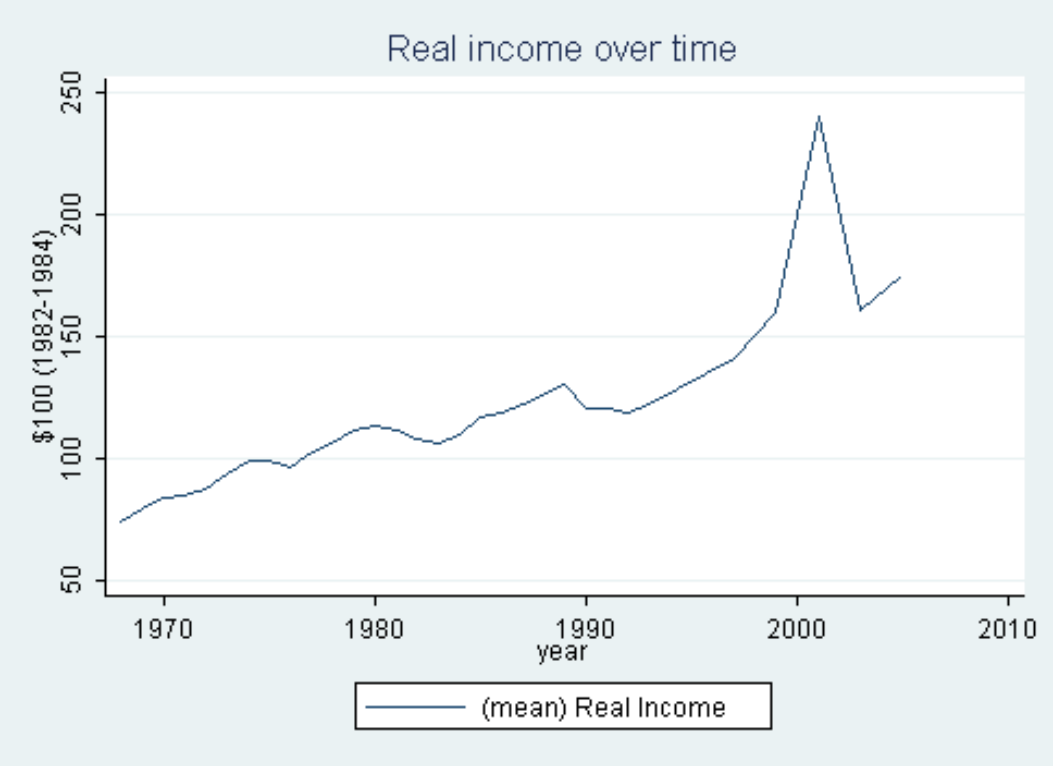

Figure A.2.: Average incomes, by years

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