POLICY UNCERTAINTY AND PRECAUTIONARY SAVINGS

Francesco Giavazzi
Michael McMahon

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<u>ABSTRACT</u>

In 1997 Chancellor Kohl proposed a major pension reform and pushed the law through Parliament explaining that the German PAYG system had become unsustainable. One limitation of the new law -- one that is crucial for our identification strategy -- is that it left the generous pension entitlements of civil servants intact. The year after, in 1998, Kohl lost the elections and was replaced by Gerhard Shroeder. One of the first decisions of the new Chancellor was to revoke the 1997 pension reform. We use the quasi-experiment of the adoption and subsequent revocation of the pension reform to study how households reacted to the increase in uncertainty about the future path of income that such an event produced. Our estimates are obtained from a diff-in-diff estimator: this helps us overcome the identification problem that often affects measures of precautionary saving. Departing from the majority of studies on precautionary saving we also analyze households' response in terms of labor market choices: we find evidence of a labor supply response by those workers who can use the margin offered by part-time employment

Francesco Giavazzi
Department of Economics
MIT, E52-274
50 Memorial Drive
Cambridge, MA 02142-1347
and NBER
giavazzi@MIT.EDU

Michael McMahon
Centre for Economic Performance
London School of Economics and Political Science
Houghton Street
London WC2A 2AE
United Kingdom
m.f.mcmahon@lse.ac.uk

1   Introduction

Prolonged political debates about how to reform a country's system of public welfare are common in many countries. Reform reversals, that is the adoption by one government of new welfare rules and their revocation by a subsequent government, are also not infrequent. Underlying these experiences is often a 'war of attrition' among various groups in society, each trying to protect themselves and to shift the burden of the reforms on someone else. A typical case are reforms of pay-as-you-go (PAYG) pension systems in countries where population growth is decelerating. There is rarely a disagreement on the need to change the existing rules, but as one reform plan after the other is considered, decisions often keep being been postponed because political parties are unable to agree on how the burden should be shared between various groups in society and in particular between the young and the old [1]. On some occasions pension reforms adopted by one government are revoked by another—this happened in Germany in 1998, the case studied in this paper, and in Italy ten years later.

In Germany the need to reform the PAYG pension rules came to the forefront of the public debate in 1997 when Chancellor Kohl announced a major reform explaining that the existing system was unsustainable: under the existing rules public spending on pensions would increase from 12% of GDP in 2000 to 17% in 2050 (OECD, 2005). By then payroll contribution rates to the State pension system would need to reach 25%, from 18% in the mid-1990s (Borsh-Supan, 2003). The reform adopted by Kohl addressed these issues restricting the accrual of pension rights not based on contributions and gradually reducing the replacement rate from 70% to 67%. Over time the new law would have stabilized the payroll contribution rate at around 21% (Schulze and Jochem, 2007). One limitation of Kohl's reform—one that will be crucial for our identification strategy—is that it left the generous pension entitlements of civil servants intact.

In 1998, the year after his reform had been passed into legislation by parliament, Helmut Kohl lost the elections and was replaced by Gerhard Shroeder. One of the first decisions of the new government was to revoke of the 1997 pension reform act.

People do not simply sit and wait. The reversal of a previously adopted reform—

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[1] Boeri, Borsch-Supan and Tabellini (2001), using survey data, analyze the opinions of European citizens regarding pension reform trying to understand why a political consensus is so difficult to achieve. They find that conflicts of interests over welfare reform are generally aligned along three main dimensions: age, income, and the insider/outsider status in the labor market.

especially when, as in Germany, the urgency of the reform had been motivated arguing that the system in place was unsustainable—raises the uncertainty that households face regarding their future. This paper uses the quasi-experiment of the adoption and subsequent revocation of the Kohl pension reform to study how German households reacted to this event that we interpret as having produced an increase in uncertainty about the future of existing pension rules, and thus about the future path of household income. We use a difference-in-difference estimator: the two periods are the years preceding and following the revocation of the reform; the control group consists of those individuals unaffected by the Kohl pension reform, in particular civil servants. We use fixed effects to control for unobservable characteristics, such as differences across individuals in the degree of risk aversion.

Attanasio and Brugiavini (2003) use a diff-in-diff approach to show that Italian households substituted higher private pension wealth for the lower social security pension wealth following the 1992 pension reform in Italy. Our results provide direct evidence on the extent to which an increase in uncertainty about the path of future household income raises precautionary saving. (Carroll and Kimball, 2006 in their Palgrave entry define precautionary saving as "additional saving that results from the knowledge that the future is uncertain.") Fuchs-Schudeln and Schudeln (2005) find evidence consistent with a precautionary saving motive in Germany analyzing the same micro data we use in this paper but looking at a different experiment: German reunification. In a paper that is closer in spirit to ours, Murata (2003) finds evidence consistent with precautionary saving in a cross-section analysis of the answers given by a sample of Japanese households to a questionnaire in which they were asked about their perceived uncertainty regarding future pension benefits. Gourinchas and Parker (2001) provide structural estimates of a dynamic stochastic model of households expenditure over the life cycle with uninsurable labour income uncertainty. They show that the precautionary saving motive is especially important at young ages while it becomes negligible for older households who, on average, hold large amounts of liquid wealth. The results are derived using a definition of wealth that excludes social security, and the authors suggest that the age pattern of precautionary saving produced by their model could be reversed if uncertainty concerning pension wealth was introduced. Our data allows to test whether the precautionary saving induced by the uncertainty regarding the future of pensions affects individuals differently depending on their age. Finally, Attanasio and Brugiavini (2003) use a diff-in-diff approach to show that Italian households substituted higher private pension wealth for reduction in social security wealth following the 1992 pension reform.

Additional savings can be achieved either by consuming less or by working more. Departing from the majority of studies on precautionary saving we also analyze households' response in terms of their labour market choices: hours worked in the primary and (possibly) in secondary jobs by all working household members.

Our results support the view that the policy uncertainty induced by stop-and-go reforms lowered private consumption contributing to the slowdown of the German economy at the start of this millennium. (The household saving rate, as a share of disposable income, increased in Germany precisely at the time of the debate surrounding pension reform: from below 10% of disposable income in the mid 1990's to 11% at the start of the millennium; something similar also happened in Japan). We find that German PAYG workers, the large majority in the population, saved more. For instance, a household that previously was holding savings constant at 9.8% of disposable income (the average saving rate in our balanced sample in 1998) would, *ceteris paribus*, have a saving rate of about 13.7% by the year 2000. Households whose pension status was affected by the Kohl reform and by its subsequent reversal also worked more, exploiting the margin provided by part-time employment. For instance, a head of household working part-time, who previously worked 22 hours per week (the average part-time hours per week in our balanced sample in 1998), would increase her hours to 26 hours per week.

2   A Quasi-Natural Experiment

We study how households' saving and labor supply decisions respond to an increase in uncertainty about the future of pension rules exploiting data from a German quasi-natural experiment. During the 1990's Germany went through two distinct periods in the discussion about the reform of its PAYG public pension system. A long debate that increased the public's awareness about the unsustainability of the existing rules was followed by a rather comprehensive reform; a year later the reform was unexpectedly revoked. We use the timing of the announced reform and of its subsequent revocation, and the fact that not all German households were affected by the reform, to identify the effects of the increase in uncertainty induced by the policy reversal. The timing of events is as follows:

1. Up to 1997 the sustainability of the PAYG pension system was not a big public issue in Germany. It was rarely discussed and the general public was largely unaware of the inevitable need to reform the system;

2. August 1997: Chancellor Kohl in a widely reported speech announces a major reform explaining that the existing rules are no longer sustainable. The Kohl reform was put into legislation in December 1997 (*des Rentenreformgesetzes* 1999, December 16, 1997). The main provision of the new law was the indexation of pension benefits to future gains in life-expectancy: over time this provision would have reduced the replacement rate from 70% to 67%. The law was due to come into effect in 1999.

   Earlier in the same year—at the end of January 1997—a reform of the rules governing public sector employment (*Gesetz zur Reform des öffentlichen Dienstrechts*) had been adopted, which had come into force on July 1, 1997. The main purpose of the law was to create a more market-driven system for career civil servants, to introduce flexibility in work practices and performance-related pay, and to increase mobility across jobs. Among the many provisions of this law was a measure which marginally modified the rules of civil service pensions, [2] but de-facto safeguarded the generosity of the system going forward, and more importantly insulated civil servants from the effects of reforms of the PAYG system that might be introduced in the future —and indeed the subsequent Kohl reform did not apply to civil servants;

3. September 1998: Gerhard Schroeder surprisingly wins the general election. One of the first decisions of the new Chancellor is to revoke Kohl's pension reform (*Rentenkorrekturgesetz*, November 20, 1998). The revocation was not accompanied by the announcement of an alternative plan: thus the German public is left with the awareness that the PAYG system is unsustainable but with no plans for changing its rules. Despite the revocation being an election promise, the outcome of the election was unexpected. We use September 1998 as the point at which the uncertainty over the future of the social security system increased;

4. May 2001: the reform process resumes. A new pension scheme, the Riester reform, is adopted. Along with a gradual reduction of benefits, from 70 to 67 percent of average net earnings by 2030, the law also encouraged enrollment in private pension plans.

Based on this sequence of events we define the following timing for our policy variables.

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[2] The reform involved civil servants contributing to the financing of their pensions through a fixed reduction of 0.2% in the annual pay every year between 2001 and 2016.

- For households in which the head of the household is a civil servant (as we shall see our saving data refer to the entire household) we define a reform variable:

$$cs\_reform_t = \begin{cases} 1 & \text{after January 1997} \\ 0 & \text{otherwise} \end{cases}$$

- For the all other households, *i.e.* those in which the head-of-household is not a civil servant, we define two variables. The first identifies the Kohl reform, the second its revocation:

$$reform_t = \begin{cases} 1 & \text{between August 1997 and September 1998} \\ 0 & \text{otherwise} \end{cases}$$

$$revoke_t = \begin{cases} 1 & \text{between September 1998 and April 2001} \\ 0 & \text{otherwise} \end{cases}$$

2.1 Treated and controls

Various groups in German society were affected in different ways by Kohl's reform and by its subsequent revocation:

PAYG individuals: In Germany, membership in the PAYG public pension system is mandatory for almost all workers [3]. The majority of the individuals in our sample (about 64% in 1998) fall into this general category. These are the people directly affected by Kohl's reform and thus constitute our "treated" group.

Civil servants: German civil service pensions are run separately from the PAYG system. As we discussed, the rules that apply to the rather generous pensions of civil servants were exempted from the reform-revoke-reform process. Households headed by civil servants constitute our main "control" group.

Beyond these two groups there are four other categories which were only marginally–if at all–affected by the reform.

Those planning to leave Germany: These are foreign nationals who are in Germany temporarily, or who in any case expect to leave Germany before retirement. If a person who has contributed to the public pension system leaves Germany before they can claim their pension, there are rules in place to treat

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[3]To be eligible for a pension a worker's earnings must be above a certain threshold. Our sample however only includes households with positive savings: it is thus likely that all the individuals in the treated group will be part of the public pension system.

their accrued pension wealth fairly. These rules differ depending on where the person moves to: if they move to another EU country, then the years of pension contribution in Germany could count toward a public pension at home. If instead the person moves to outside the EU they can generally claim their contributions back. In either case, such a worker is likely to be less affected (if at all) by changes to the German public pension system. We identify these people using a specific question—asked only of foreign nationals—as to whether they intend to stay in Germany over the long term. In our benchmark specification we include "German leavers" in the control group.

Self-employed: These workers can choose whether or not they wish to join the public pension system. This group will thus contain some members who are affected by the reform and others who are not.

Professionals: Lawyers, accountants, vets, doctors, etc. are required to join private pension plans. This group is thus unaffected by a reform of the public pension system.

Pensioners: Most proposed reforms of the pension system protect the benefits of those already in retirement. Pensioners should thus be unaffected by such reforms.

We also experiment with (a) an extended treated group which also includes the self-employed, and professionals, and (b) excluding those who plan to leave Germany from the control group.

The diff-in-diff estimator that we shall use relies on the assumption that prior to the treatment households in the treated and in the control group are indistinguishable, *i.e.* that the treatment is random. In other words, that households headed by a civil servant are similar to all other households except for the fact that they are headed by a civil servant—and thus are shielded from the effects of the Kohl reform. We have checked this assumption looking at "propensity scores". These are reported in Figure 1.

The horizontal axis in Figure 1 shows the estimated probability of not being treated measured from a panel logit regression of "not being treated"—that is being headed by a civil servant—on various controls including a household fixed effect. The vertical axis shows the percent of households in each group. We find controls and treated groups close to both extremes of the estimated probabilities of treatment. In the top-left panel, for instance, we find that there are household heads in the general

population (PAYG individuals, a category which does not include civil servants) that have a high probability of being treated, that is whose characteristics closely match those of the treated group, namely civil servants. Symmetrically, the bottom-right panel of the figure shows that there are households headed by a civil servant who, considering their characteristics, might have been treated. This reassures us that the two groups are not too different from each other—that is that the assumption that the treatment is random is not too extreme.

[Insert Figure 1]

3 Data

Our data are from the German Socioeconomic Panel (GSOEP). This survey, first conducted in 1984, is an annual longitudinal study which now covers some 10,000 German households providing information on numerous aspects of their life, including household composition, family biographies, employment, social security, earnings and health. The number of households surveyed rises over time since subsequent waves have increased the coverage of the sample, and attrition rates are low. Balanced samples over a sufficiently long number of years are relatively small: when we restrict our analysis to households who report their savings, the size of a balanced panel covering the 6-year period 1995-2000 contains 2,854 households yielding a total of about 17,000 observations.

Two main surveys are conducted each year. The first is an individual questionnaire in which all adult household members answer questions regarding their own situation. The second is a household questionnaire in which the head of the household is asked questions regarding the entire household. We combine the information from the two questionnaires. From the first we obtain information about the head and other members of the household: age, education and employment status, which defines the future pension status, hours worked, etc. for each individual. From the second, we obtain information relating to the entire household: income, household taxes paid (including a separate measure of social security contributions), pension income received from both public and private sources, as well as demographic information such as marital status, number of children, area of residence, etc. The concept of saving we use thus refers to the entire household. The pension status, and whether

or not it is affected by the reforms, refers only to the head of the household. [4] [5]

The GSOEP survey is generally conducted early in the year, although some respondents are interviewed as late as October and November. Using an "interview month" identifier, we can tell whether at the time of the interview the respondent was aware of the Kohl reform (announced during the month of August 1997) and of its subsequent recall (expected after the September 1998 election).

We construct a balanced sample using six waves of the GSOEP survey: those from 1995 (two years before the Kohl reform) to 2000 (the year before the Reister Law). Table 1 reports the number of observations available each year [6] and those included in the balance sample: 4.532. The table also describes the characteristics of these households: the characteristics of households included in the balanced panel reflect their responses in 1998. The household proportions in terms of the key variables in the balanced sample are similar to those in the overall data. In particular, the split by labour force status (middle panel) and public pension status (bottom panel) is reasonably constant over the course of the sample.

[Insert Table 1]

3.1 Household saving

The GSOEP survey asks about household savings posing the following question: *"Do you usually have an amount of money left over at the end of the month that you can save for larger purchases, emergency expenses or to acquire wealth?"* The answer to

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[4]Our control group could include households that are headed by a civil servant–and thus should in principle be shielded from the reforms—but in which some other household member has a job that qualifies for a public pension. The saving of such a household could thus be affected by the uncertainty associated with the status of one of its members. For this reason in Table 6 (column 5) we show the results we obtain when we include in the control group only those households in which there is a single worker: the civil servant who heads them.

[5]We also make use of the variables contained in the Cross-National Equivalent File (CNEF). These data are also based on the GSOEP responses, but are constructed ex-post in order to provide variables that are comparable with the British Household Panel and Panel Study on Income and Dynamics (PSID) - see Burkhauser et al. (2001) for details. The variable we use to measure social security contributions comes from this dataset.

[6]The numbers in this table may differ slightly from those in the GSOEP survey because some observations cannot be used in our sample as they lack data on key variables, or are only observed for one year.

this question is our main household saving variable [7], which we then express as a percentage of household disposable income or, alternatively, of household consumption.

One problem with our survey data concerns those households who do not save. The GSOEP survey reports saving only for those households that declare positive saving: if a household has zero or negative saving, the question about saving is left unanswered or a zero is entered. Income is instead reported for all households. The number of households for which there is no information about saving is significant: for instance 1,678 out of 4,532 households in the balanced sample in 1998, or about 40% (see the top panel of Table 1). Among the heads of household who do not report saving 14% (in the balanced sample in 1998) are unemployed, 22% are retired and 11% are otherwise out of the labour force (these compare to 8%, 25% and 8% respectively in the entire 1998 sample). The percentage of non-savers is reasonably constant along the age distribution. We proceed in two ways, both excluding households who do not report their savings and, alternatively, attributing to such households an estimated saving rate. To do this, we proceed as follows.

We start from households who declare positive saving and compute consumption as income *minus* saving (consumption is not reported in the survey). We then use these data on consumption to estimate a consumption function. The arguments in the estimated consumption function are income, a measure of wealth, expenditure on specific items such as food, demographic variables and personal characteristics.[8] Using the estimated parameters of this consumption function we then construct an estimate of consumption for those households who did not report saving and for whom we only have information on income and on the other arguments in our consumption function. Using reported income and these estimates of consumption we finally obtain an estimate of saving. The results (not reported here but available in an Appendix posted on our websites [9]), are little changed when we use the sample which also

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[7]This variable is a measure of the flow counterpart to household wealth which would be the most desirable variable to analyse. Fuchs and Fuchs-Schudeln (2005) construct an estimate of household financial wealth interpolating a measure of asset income by assuming that each household receives an average return from the assets it holds. Such a definition however is subject to measurement error. The same asset stock will in general yield different returns, depending on the particular asset composition: if this happened, households with identical stocks of assets would be attributed different stocks. The measure of saving reported in the GSOEP survey is immune from this problem since the question is directly about additions to the stock of wealth.

[8]Browning and Leth-Petersen (2003), amongst others, discuss the issue of imputing consumption using similar household surveys.

[9]http://www.igier.uni-bocconi.it/whos.php?vedi=4063&tbn=albero&id_doc=177

includes estimated negative saving by households who do not report saving.

A second problem with our definition of saving arises from the PAYG pension system. The answers to the question about saving miss two portions of actual household saving. First, social security contributions by workers and by firms [10], which are not reported as savings although they are a form of saving (which increases with income). Thus reported savings increase over a person's working life by less than "true" saving. Second, the pension payments an individual receives are mis-reported as income, rather than being considered negative savings. Thus reported savings remain positive even after retirement when actual savings are likely to be negative [11]. A similar problem arises for private pension plans. In the GSOEP survey, individual contributions to such plans are correctly reported as saving [12], but money withdrawn from a private plan is incorrectly reported as income. The bottom line is that the savings reported in the GSOEP answers represent a fraction of actual household saving.

This problem emerges clearly from Figures 2 and 3. Figure 2 shows the age profile of the German saving rate (defined as the ratio of reported saving to disposable income) in 2002 [13]. This age profile is at odds with the life cycle hypothesis: the difference is particularly sharp when we compare it with the U.S. profile obtained from the PSID survey (also shown in Figure 2 and reported in Poterba, 1994). Rather than hump-shaped, as implied by the life-cycle hypothesis, the saving rate of German households seems to be unaffected by an individual's age [14]. Figure 2 shows the saving rate once we correct it, as discussed above, by including contributions to social security and excluding pension benefits from the measure of disposable income. (This correction and the variables used to compute it are also discussed in detail in the Appendix posted on our websites). The 'corrected' age-saving profile resembles more closely that predicted by the life-cycle hypothesis. One point that is worth

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[10] We do not observe social security contributions paid by firms. Consistent with the rules of the German social security system we assume that firms pay a contribution on behalf of their workers equal to that paid by the workers themselves.

[11] To be precise, the mis-reporting does not concern the total pension payments received, since part of these are an implicit return on pension wealth, and therefore are indeed income. We have overlooked this fact. For a discussion of this correction see Jappelli and Modigliani (2005).

[12] We do not observe contributions to private pension plans possibly made by firms and we thus overlook them.

[13] Here we use disposable income as the denominator for comparability with the U.S. data.

[14] This fact is well known from the work of Borsch-Supan et al. (1991, 2001) and Borsch-Supan (2003). Poterba (1994) makes the same observation for Japan. The age profile of the Japanese saving rate reported by Poterba is also shown in Figure 1.

highlighting are the positive saving rates of households headed by the young—while U.S. households headed by the young on average dis-save as shown in Figure 2.

[Insert Figures 2 and 3]

A word of caution is needed here. The corrected measure of saving that we have built should be considered with some prudence. To compute it we have used information on employees' contributions to private pension plans, but not–as we have already mentioned–on the contributions paid by employers to such plans (because these are not reported in the survey). The correction is thus only partial. (Note however that employers' contributions to the public system are correctly imputed). We have also assumed that contributions are regarded as a form of saving, something that is far from obvious in a PAYG system. Treating pension income as negative saving is also troublesome: for instance in a perpetual youth model, pension wealth does not decrease with age. Something similar may be happening with the contributions to, and the income from, private pension plans. We thus run our regressions using both the reported and the corrected definition of saving.

Table 2 shows sample statistics on the reported and the corrected saving rates (as a percentage of disposable income) by pension status of the head of household. Reported saving rates are generally similar across groups, and all groups display a wide within-group variation. The overall mean reported saving rate, as a percent of disposable income, is 9.4%: this is higher for civil servants and also for self-employed/professionals. Retirees also display significantly positive reported savings. Though some respondents claim to save up to almost 90% of their disposable income, the median reported saving rate for high savers (90th percentile) is 30%. As expected, correcting saving rates boosts the saving rate of those in employment, and causes positive reported saving to become negative for retirees.

[Insert Table 2]

3.2 Hours worked

The GSOEP survey reports the hours worked by the head and other members of the household each week in their main job and, possibly, in other, secondary, jobs. The questions asked are: '*How many hours do your actual working-hours consist of, including possible over time?*'. We are able to identify whether a person works, in her main job, full-time, regular part-time or occasionally, from the answer to the question '*Are you currently engaged in paid employment? Which of the following*

applies best to your status?'. A related question asks respondents to ignore their main job, and consider additional employment ('*It is possible to work in addition to regular employment, household work, education and also as pensioner. How many days a month do you engage in this additional employment? How many hours on average on these days?');* the answer to this question allows us to construct a measure of hours worked in secondary employment.

Table 3 reports descriptive statistics on the number of weekly hours worked by the head of household (top panel), the number of household members who work (second panel), the average weekly hours per head of household who works part time in their primary employment (about 10% of all those in employment, displayed in panel 3), and, in panel 4, the average number of hours worked in a 2nd job (by those who also have a main job). About 70% of households contain only a single worker (usually the head of household), and most heads of household work on average 30-40 hours per week. PAYG household heads are more likely to work part-time (around 11% of PAYG workers work part-time compared to around 4%-6% in the other categories), but part-time workers in all categories work similar numbers of hours on average (20-25 per week). Though some of the 2046 PAYG workers are employed for up to 7 hours per week in a second job, second jobs are very rare and even the 90th percentile of the distribution works an average of 0.3 hours per week in such employment. In fact only 50 of the 2046 PAYG workers in the balanced sample in 1998 engage in 1 hour or more of secondary employment per week (9 of these 50 work full-time in their primary employment; the remainder are part-time employed in their main employment). In the balanced sample of 188 civil servants, only 7 of these engage in secondary employment (in 1998) and all of these are part-time civil servants in the main job. We shall therefore study whether part-time employment offers workers greater flexibility, but we shall not use the information on secondary employment because the number of workers involved is far too small, at least in our balanced sample.

[Insert Table 3]

3.3   Descriptive statistics

Our diff-in-diff results are driven by differences across groups in the way their saving rates move over time. It is thus important to be reassured that such changes do not reflect differences in the age profile of the various groups. Although the propensity scores have already reassured us, to further check that this is not the case, Figure 4

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shows the age distribution of the heads of household in the balanced panel (by head of household's age in 1998). Age profiles are not very different across groups: the hump of the age-distribution for all groups is around the age of 40-45, although the age profile of retirees is obviously pushed much further to the right. The age distribution of those who wish to leave Germany is bimodal with peaks around the age of 30 and just before retirement. However, to make sure that our results are not driven by age differences across the various groups, in our regressions we shall include household fixed-effects. Since age is a fixed effect, to the extent that it might contribute to heterogeneity in saving behaviour, fixed effects will capture it.

Differences in the level (and in the change over time) of the saving rate of different groups could also be driven by the number of children living at home. This is shown in Figure 5. The number of children living at home is not very different across groups: in most groups at least half of the households have no children living at home; the number of children leaving at home falls to zero for retirees—as one would have expected. No household has more than 7 children living at home; only 13 households (from over 4,000) have more than 4 children living in the house. Figure 6 shows that the pattern of the number of children living in the household matches the life-cycle, peaking during the head of household's forties.

The within group distribution of household disposable income is also similar across groups (Figure 7). On average, professionals and self-employed are the richest group (mean household disposable income in 1998 is €40,500) although their distribution is the least concentrated. Civil servants are generally paid very well (€38,800) and also have the lowest within group variation. The poorest group are pensioners (€21,000). Overall these results confirm what we learned form propensity scores.

[Insert Figures 4, 5, 6 and 7]

There is a statistically significant downward trend in the household saving rate: over the six year period of our balanced sample the average change in the saving rate (controlling for household and time fixed effects) is -0.13 per cent per year. This—as we discuss in the next section—motivates our use, as dependent variable, of the change in the household saving rate.

4   Saving Results

We identify the effect on household saving of the increase in uncertainty induced by Chancellor Shroeder's decision to revoke Kohl's pension reform using a diff-in-diff

estimator. The "treated group" includes those households whose pension status was affected by the introduction and by the subsequent revocation of the reform. This group (the PAYG individuals) includes the majority (60%) of German households in our balanced sample. The "control group" consists of those households which were unaffected by the changes in the rules concerning public pensions: in our benchmark specification this group includes households which are headed by a career civil servant and those planning to leave Germany. We thus define

$$treated_{i,t} = \begin{cases} 0 & \textit{if unaffected by the Kohl reform} \\ 1 & \textit{otherwise} \end{cases}$$

Since we observe the month in which the interview is conducted, we can precisely identify those who are aware of the reform and of its subsequent revocation. For instance, during the course of 1998, we know which households were interviewed when the Kohl reform was still in place, and which were interviewed after the September elections.

The saving rates of the individual households in our sample display different trends: in most cases (as discussed in the previous section) the saving rate tends to fall over time (in the years we are considering), though at different speed for different households. To estimate the response of the household saving rate to the treatment, and to separate this effect from the trend change in the saving rate, we use, as dependent variable, the change in the saving rate and include household fixed effects [15].

Our baseline regression in difference-in-difference form is

$$\begin{aligned} \Delta sr_{it} \;=\; & \alpha_i + \beta_t + \theta x_{it} + \psi(cs\_reform_t * cs) \\ & + \delta_1 reform_t + \tau_1 \left(reform_t * treated_{i,t}\right) \\ & + \delta_2 revoke_t + \tau_2 \left(revoke_t * treated_{i,t}\right) + \varepsilon_{it} \end{aligned} \qquad (1)$$

where Δsr_{it} is the change in the saving rate measured in percentage points, α_i and β_t are household and time fixed effects respectively [16] and x_{it} is a vector of controls (for instance household disposable income). The coefficient we are most interested in is τ_2 which tells whether the behavior of treated households—that is those households whose pension status was affected by the introduction and the subsequent revocation

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[15]An additional advantage of using the change in saving rates as our dependent variable is that those households who move from zero to positive saving, or vice versa, can be analysed in the same regression without worrying about the truncation at zero of our dependent variable.

[16]Fixed effects remove the need for a separate $treated_{i,t}$ regressor.

of the reform—differs from the behavior of our controls: a positive value of τ_2 is a measure of the extent of precautionary saving. τ_1 measures a more standard effect: the shift in the saving rate resulting from the announcement of Kohl's pension reform by those households who were affected. A positive value of τ_1 indicates that households whose pension status was affected by Kohl's reform increased their savings (more precisely shifted the change in their saving rate up) offsetting the cut in pension wealth.

Our baseline results use the standard definition of saving—reported saving as a percent of disposable income—and are obtained from the balanced panel extending over six years, 1995-2000. The results are presented in Table 4 [17]. The first column of the table shows the baseline results with no controls beyond the time and household fixed-effects. The estimate of τ_2 (reported in the second row of Table 4) is both statistically (at the 95% level) and economically significant. A coefficient of 1.311 indicates that following the revocation of the reform (that is in the years 1998, 1999 and 2000) treated households have, on average, increased the *change* in their saving rate by 1.311 percentage points per year. This means, for instance, that a treated household that previously was holding savings constant at 9.8% of disposable income (the average saving rate for the balanced sample in 1997) would, *ceteris paribus*, have a saving rate of about 13.7% by the year 2000.

Column (2) adds a control for the change in household disposable income and column (3) controls for different (perhaps time-varying) characteristics of the head of household. In both cases the effect of the revocation is made stronger (the estimated effect on the yearly change in the saving rate rises from 1.311 to 2.094) and remains statistically significant. Columns (4) and (5) correct for the unavailability of the "Germany leaver" data in 1995: in column (4) we use the data on intention to leave Germany from 1996 to fill in for the missing 1995 data; in column (5) we simply drop the data from 1995 onwards. In all cases the estimate of τ_2 remains significant.

Interestingly our estimates of τ_1 (reported in the first row of Table 4) are never significant: households whose pension status was affected by Kohl's reform do not appear to have responded to the news by changing the path of their saving rate so as to offset the cut in pension wealth. This result runs against the findings in Attanasio and Brugiavini (2003). It is probably due to the fact that what we are estimating are differences between treated and controls: both civil servants (the larger group among

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[17]Some of the control variables are dropped automatically by Stata due to multicollinearity; this is especially the case when 0-1 dummies correlate perfectly with fixed effects variables. Such variables are marked with a "-" in the coefficient cells of the tables.

our controls) and PAYG individuals were hit by a change in rules during 1997: civil servants in January, PAYG individuals in August. An insignificant estimate of τ_1 is telling us that PAYG workers did not react to the reform that hit them more than civil servants did to the change in their own rules.

[Insert Table 4]

4.1   Robustness

Tables 5 and 6 present some robustness tests. Table 5 considers different definitions of the saving rate: column (1) reproduces, for comparison, column (3) of Table 4 (balanced sample with controls); column (2) uses, as dependent variable, instead of reported savings the corrected saving rate (percent of disposable income); columns (3) and (4) use consumption as the scaling factor, respectively for the reported and corrected saving rates. In all cases, the revocation dummy remains statistically significant and its size increases when we use the corrected saving rate, and again when we use consumption as the scaling variable. In terms of the magnitude of the effect on the corrected saving rate, consider that in 1997 the average household where the worker is not of retirement age had a corrected saving rate equal to 26.5% of disposable income; according to the estimates in column (2) the corrected saving rate of such a household would, ceteris paribus, have increased to reach, by the year 2000, 33.7% of disposable income .

Table 6 considers different samples and controls. Again, column (1) repeats column (3) of Table 4 (balanced sample with baseline controls). Column (2) uses the unbalanced panel and the results are similar; column (3) includes only civil servants in the control group (results are similar and statistically significant at the 12% level); column (4) uses the balanced sample but also includes self-employed and professionals in the control group. A final concern—as we mentioned—is that a household may be headed by a civil servant but include some members who belong to the PAYG system. In order to control for this possibility, in column (5) we repeat our baseline regression using only those households where the head of household is the only worker throughout the sample. In all cases our revocation dummy remains both statistically and economically significant.

[Insert Table 5 and 6]

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4.2   Is the effect on saving age-dependent?

Our results suggest that greater uncertainty about the future of pensions induces higher precautionary savings. Is this effect concentrated on particular age groups, or does it occur throughout the age distribution? Gourinchas and Parker (2001) show that the precautionary saving motive is especially important at young ages, while it becomes negligible for older households who, on average, hold large amounts of liquid wealth. Their model however excluded pension wealth. Our data allows to test whether the precautionary saving induced by the uncertainty regarding the future of pensions affects individuals differently depending on their age. Relatively older individuals have a shorter working-life horizon and thus must save relatively more to achieve a given increase in wealth.

Since we use fixed effects we cannot include an "age" control [18]. Thus, to test for an age effect, we need to explore alternative routes. We keep using a fixed effect estimator, but create an age dummy variable defined as

$$D(age_i) = \begin{cases} 0 & \textit{if head of household <50 in 1998} \\ 1 & \textit{otherwise} \end{cases}$$

We then estimate the following equation

$$
\begin{aligned}
\Delta SR_{it} =\ & \alpha_i + \beta_t + \theta x_{it} + \psi(CS\_reform * civil\_servant) \\
& ... + \delta_1 reform_t + \tau_1 \left(reform_t * treated_{i,t}\right) \\
& ... + \delta_2 revoke_t + \tau_2 \left(revoke_t * treated_{i,t}\right) \\
& ... + \tau_3 (D(age_i) * revoke_t * treated_{i,t}) + \varepsilon_{it}
\end{aligned}
\tag{2}
$$

The results when we estimate this equation using the balanced sample for each of the different definitions of saving are presented in Table 7.

The dependent variable in the regressions are, respectively, reported saving as a % of disposable income (column (1)), corrected saving as a % of disposable income (column (2)), reported saving as a % of consumption (column (3)), and corrected saving as a % of consumption (column (4)). In all cases there is no evidence that the

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[18]Even though age is time-varying, demeaning it (as fixed effects does) would transform this variable in a household-specific trend. We have tested whether we could drop fixed effects by running a Hausman test which compares the consistent (though not necessarily efficient) fixed-effects model (FE) with a random effects model (RE). The null hypothesis that the RE and the FE coefficients are identical is rejected ($Prob > \chi^2 = 0.0115$).

effect on savings of the revocation of the reform is age-specific This result is robust to the specifications in Tables 5 and 6 and also to different age thresholds in the definition of $D(age_i)$. The absence of an age effect may be the result of two forces working in opposite directions: older individuals have shorter horizons, but their pension rights are largely pre-determined. Younger workers have more time to make up for a reduction in their pension wealth, but such a reduction is more significant than for older workers. Thus an increase in uncertainty about the future of pensions will affect consumption throughout the age distribution.

[Insert Table 7]

5   Hours Results

As mentioned in the introduction, additional savings can be achieved either by consuming less or by working more. We analyze the effects on labor supply of the uncertainty created by the revocation of Kohl's pension reform considering regressions similar to those just discussed but using, on the left-hand side, labor supply variables rather than the change in the saving rate. The German labour market is relatively rigid: it is unclear the extent to which work contracts allow employees to change their working hours; overtime is also strictly regulated. Workers however can adjust their labor supply using the margin offered to those in part-time employment or by taking second jobs. As discussed in Section 3.2, around 10% of workers in our balanced sample are part-time workers, while very few work significant hours in second jobs - of the 2,046 PAYG workers in full- or part-time employment, only 49 work on average 1 hour or more per week in a 2nd job in 1998 (this number is reasonably constant across years).

Using various measures of hours worked we estimate the following equation:

$$
\begin{aligned}
hours_{it} \quad = \quad & \alpha_i + \beta_t + \theta.x_{it} + \psi(cs\_reform_t * cs) \\
& + \delta_1 reform_t + \tau_1 \left(reform_t * treated_{i,t} \right) \\
& + \delta_2 revoke_t + \tau_2 \left(revoke_t * treated_{i,t} \right) + \varepsilon_{it}
\end{aligned}
\tag{3}
$$

Our results are in Table 8. In columns (1) and (2), the object of the analysis is the number of hours worked by the head of household (as shown in Table 3, the majority of households contain only a single worker). In column (1) we consider total weekly hours worked by the head of the household in her primary employment. In column (2) we restrict the analysis to heads of household for whom primary employment is

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part-time. In columns (3) and (4) we shift the focus to the hours worked by other household members (excluding the hours worked by the head of household); column (3) limits the sample to households in which the head is a full-time worker, while column (4) focuses on the households headed by part-time workers (as in column (2)). In all cases we exclude income from the right-hand -side variables as this is endogenous to the amount of hours worked.

The estimate of τ_2, the diff-in-diff effect on hours of the revocation of the reform, varies depending on whether the head of household works full time or part time. In general (column 1) there is no evidence of a labor supply response–a result which is consistent with the rigidity of German labor contracts. However (column (2)) household heads who work only part-time—and thus presumably have more flexibility—do appear to use this flexibility: following the revocation of the pension reform their hours increase significantly (at the 10% level). The point estimate, 4.386, means that a head of household working part-time, who previously worked 22 hours per week (the average part-time hours per week in 1998) would have increased her hours to 26 hours per week—an economically significant increase.

The regressions in Column (3) and (4) examine the additional hours worked by household members other than the head of the household, distinguishing between households in which the head works full or part time. In either case there is no evidence of a labor supply effect for these workers. We obtained similar, insignificant results (not reported here) when we investigated whether the number of workers increased in households affected by the revocation of the reform.

[Insert Table 8]

6   Conclusions

The results in this paper are of interest from three different perspectives. First, we provide a direct measure of precautionary saving studying how households respond to an exogenous increase in uncertainty about the path of future income. Our estimate of precautionary savings are the result of a quasi-natural experiment and thus overcome the identification problem that often affects such measures: the findings point to a significant increase in precautionary saving following an increase in uncertainty about the future path of income. Second, we find evidence of a labor supply response by workers who can use the margin offered by part-time employment: this effect however is only marginally significant. Third, from a political economy perspective, our results

indicate that households react to the reversal of a previously adopted reform and, more generally, to the delaying of reforms that are considered eventually unavoidable. This suggests that political deadlocks not only have the effect of postponing reforms— and thus eventually making them more difficult—but may also induce a slowdown in the economy by inducing households to cut their consumption–an effect that is only partially compensated by a positive labor supply response. Interestingly we find that such an effect is not limited to households headed by older workers, but affects all households, independently of the age of the household head.

7   References

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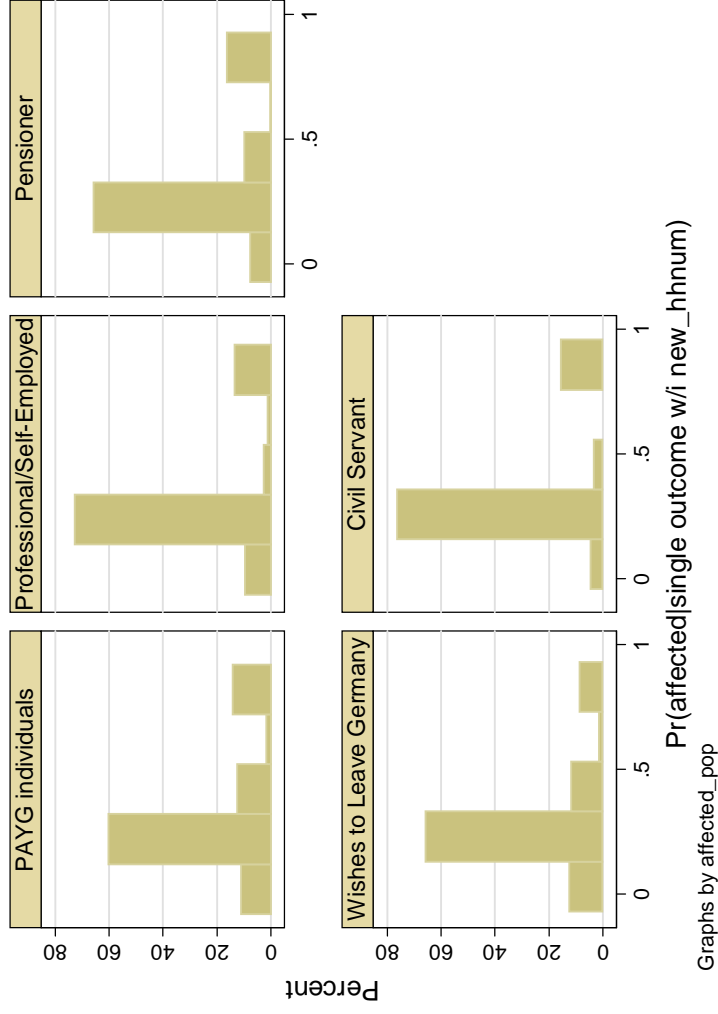
Figure 1: Propensity Scores - Probability of Not Being Treated

The horizontal axis shows the estimated probability of not being treated measured from a panel logit regression of "not being treated"—that is being headed by a civil servant—on various controls including a household fixed effect. The vertical axis shows the percent of households in each group.
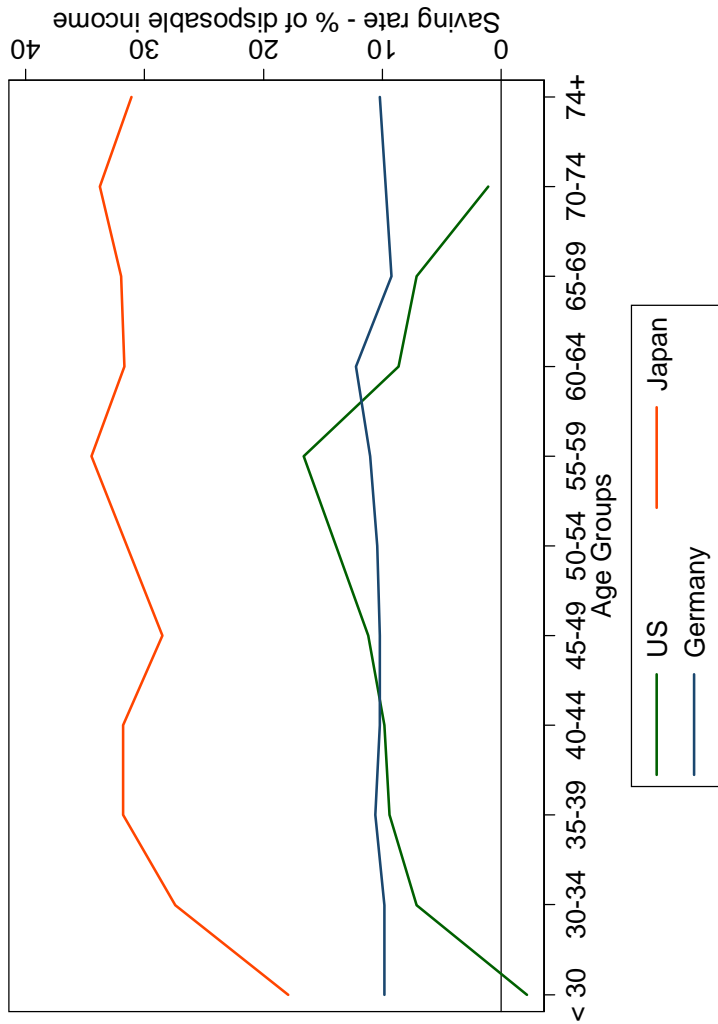
Figure 2: Reported Saving Rates in Germany, Japan and the U.S. (Source: Poterba, 1994)
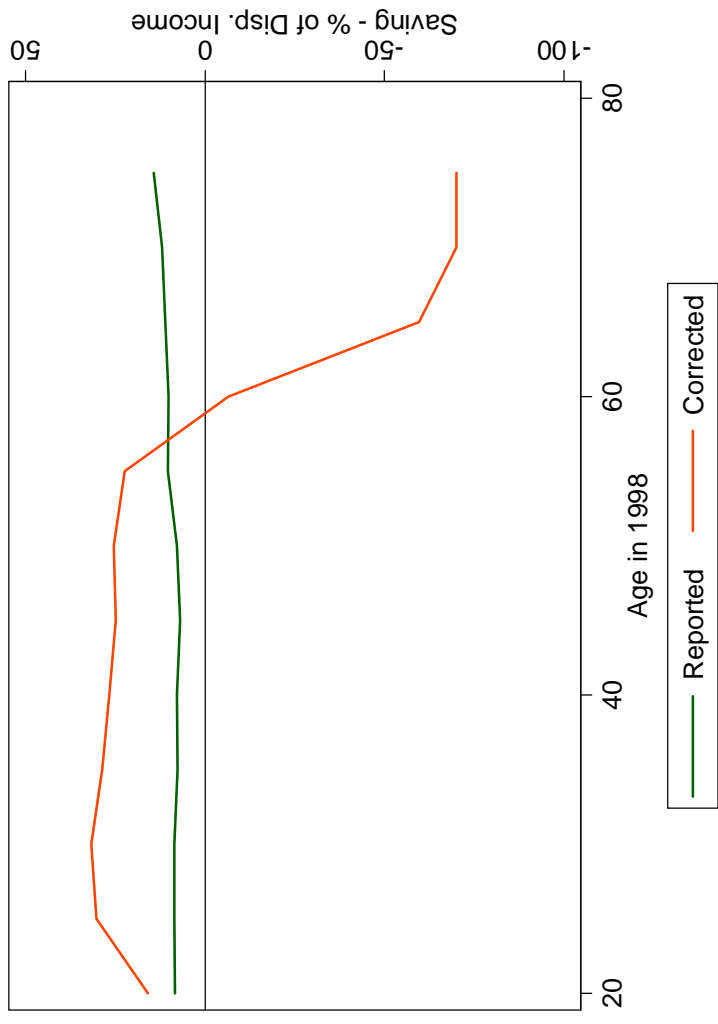
Figure 3: Reported and Corrected Saving Rates in Germany in 1998 by age of the head of the household. (Source: authors calculations using GSOEP data)
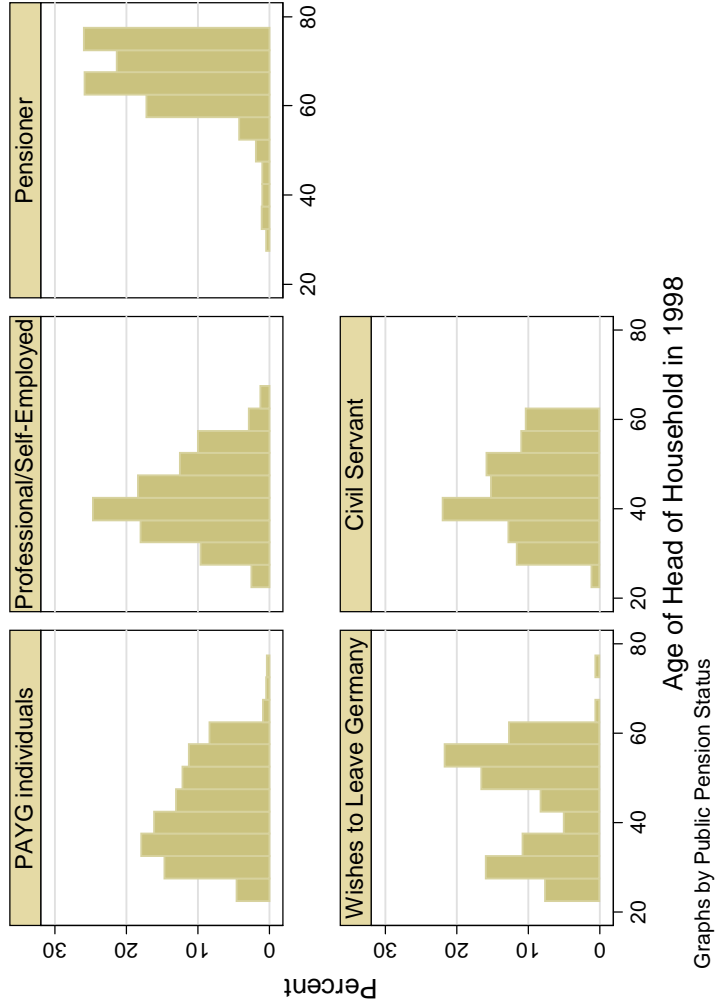
Figure 4: Age Distribution of the Head of Household in 1998 by Pension Status (Balanced Sample)
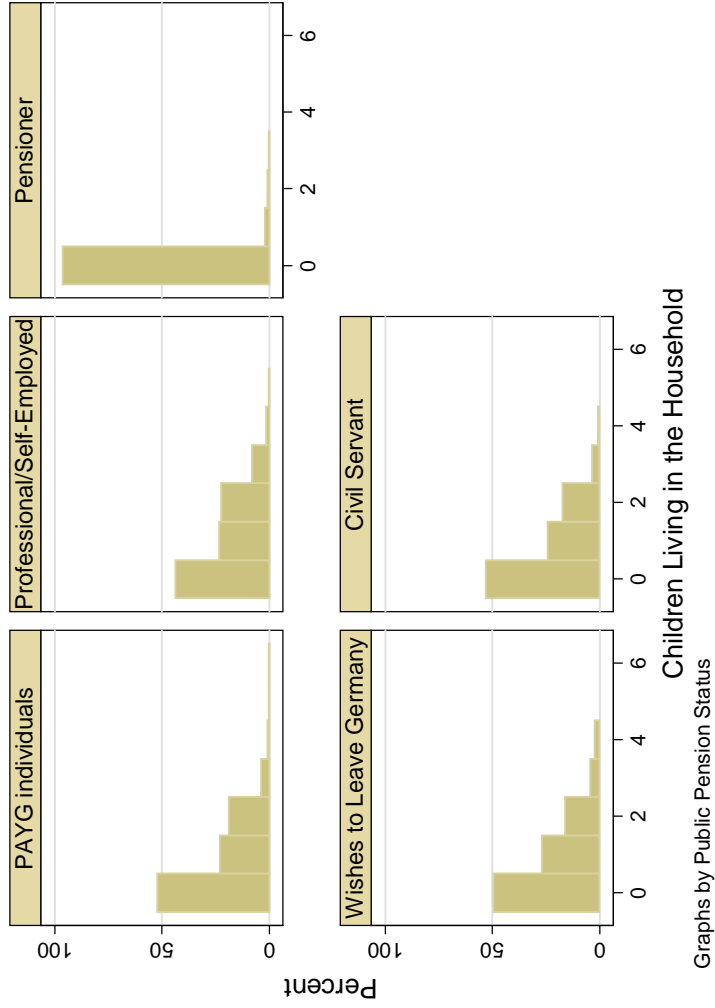
Figure 5: Number of Children Living in the Household in 1998 by Pension Status (Balanced Sample)
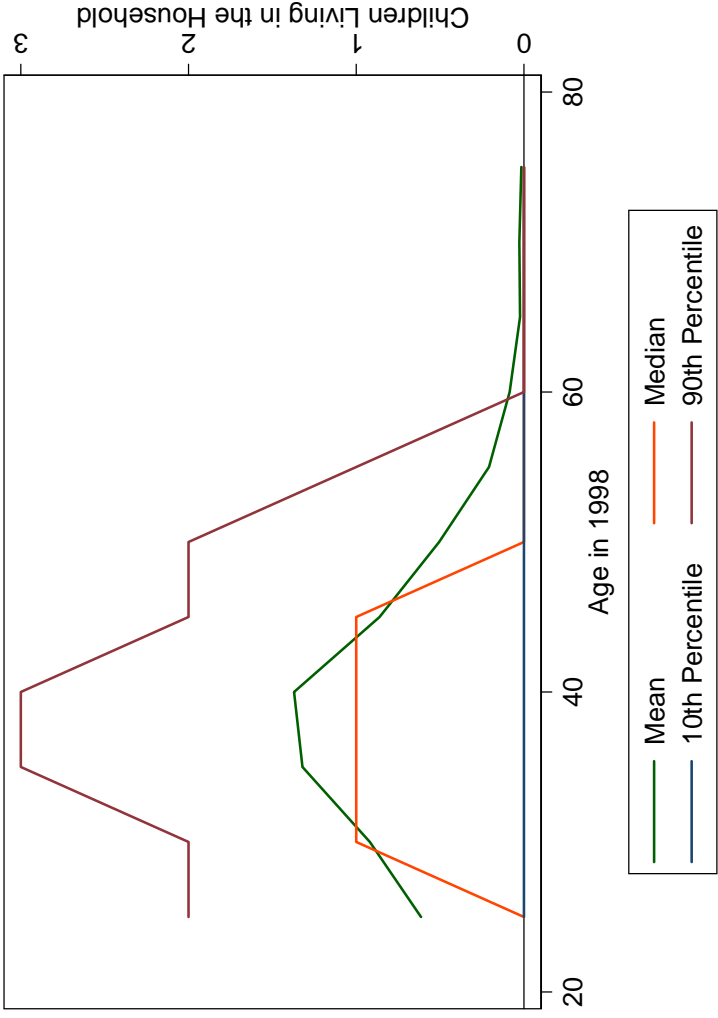
Figure 6: Distribution of the number of Children Living in the Household over the life-cycle in 1998 (Balanced Sample)
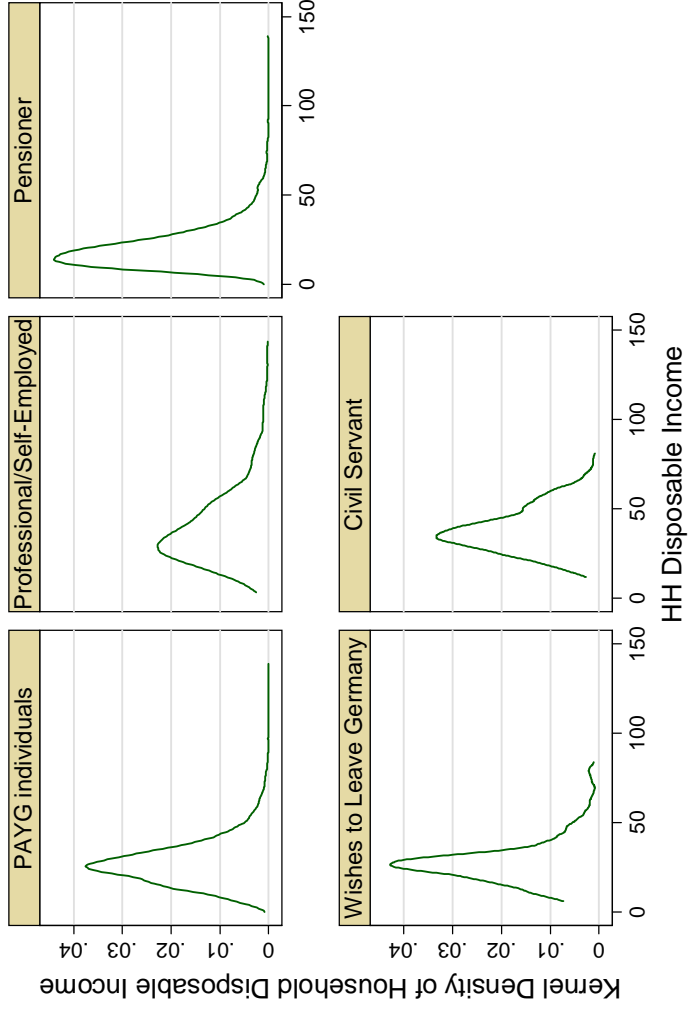
Figure 7: Distribution of Household Disposable Income in 1998 (Balanced Sample)

Table 1: Sample Size and Basic Household Characteristics: balanced and unbalanced samples

	1995	1996	1997	1998	1999	2000	1998
							balanced panel
		unbalanced samples					1995-2000
Total who report income	6,454	6,329	6,382	7,036	6,913	12,448	4,532
by household saving							
o/w positive saving	3,999	3,880	3,928	4,207	4,166	7,386	2,854
o/w saving unreported	2,455	2,449	2,454	2,829	2,747	5,062	1,678
by labour force participation							
o/w full-time	3,552	3,379	3,367	3,688	3,607	6,244	2,360
o/w part-time	24	370	366	385	470	888	245
o/w unemployed	537	557	561	578	481	799	376
o/w retired	1,355	1,324	1,549	1,856	1,826	3,526	1,259
o/w out of the labor force	986	699	539	529	529	991	292
by public pension status							
o/w PAYG individuals	4,219	4,122	3,927	4,244	4,219	7,430	2,645
o/w professionals/self-emp	271	270	288	341	341	620	244
o/w pensioner	1,355	1,324	1,549	1,856	1,826	3,526	1259
o/w prospective leaver	-	361	348	287	233	307	196
o/w civil servant	260	252	270	308	294	565	188

Table 2: Reported and Corrected Saving Rate by Pension Status in 1998: balanced sample, 1995-2000

Variable		N	mean	sd	min	max	p10	p25	p50	p75	p90
						Statistics					
Reported saving rate (% disp. income)	All	4532	9.4	11.6	0	87	0	0	6	14	25
	PAYG individuals	2645	8.4	10.6	0	87	0	0	5	13	22
	Professional/Self-emp	244	9.8	13.7	0	68	0	0	4	14	29
	Pensioner	1259	11.6	13.0	0	78	0	0	8	18	30
	Germany Leaver	196	7.4	11.1	0	50	0	0	0	12	25
	Civil Servant	188	12.1	10.4	0	59	0	5	9	16	28
Corrected saving rate (% disp. income)	All	4532	1.1	49.2	-127.6	115.9	-87.0	-26.9	21.8	35.3	46.1
	PAYG individuals	2645	27.4	24.4	-106.5	115.9	0.0	20.1	30.8	40.3	49.9
	Professional/Self-emp	244	18.3	19.4	-52.3	87.5	0.0	5.6	15.9	28.3	44.7
	Pensioner	1259	-65.2	34.9	-127.6	58.7	-99.0	-91.2	-77.4	-46.9	-10.3
	Germany Leaver	196	30.1	16.4	-32.9	84.0	9.1	22.5	30.3	38.6	50.1
	Civil Servant	188	23.1	13.5	-22.9	73.4	10.1	14.7	20.5	29.6	41.6

Table 3: Hours and Workers by Labor Force Status (Balanced Sample)

Variable		N	Statistics mean	sd	min	max	p10	p25	p50	p75	p90
Hours Worked by Head of Household	All	4532	23.4	22.3	0	100	0	0	33	42	50
	PAYG individuals	2645	31.0	20.1	0	83	0	0	39	45	50
	Professional/Self-emp	244	39.8	23.7	0	100	0	21	47	59	65
	Pensioner	1259	0.9	5.7	0	70	0	0	0	0	0
	Germany Leaver	196	29.6	20.0	0	90	0	0	38	40	45
	Civil Servant	188	39.6	11.0	0	78	30	39	40	45	50
Workers Per Household	All	4532	1.1	0.9	0	5	0	0	1	2	2
	PAYG individuals	2645	1.4	0.8	0	5	0	1	1	2	2
	Professional/Self-emp	244	1.5	0.6	0	4	1	1	2	2	2
	Pensioner	1259	0.4	0.7	0	4	0	0	0	1	1
	Germany Leaver	196	1.5	0.9	0	4	1	1	1	2	3
	Civil Servant	188	1.6	0.6	0	3	1	1	2	2	2
Part-time Hours (given Part-time worker)	All	245	22	11	0	80	8	15	21	30	35
	PAYG individuals	217	22.1	11.0	0	80	9	15	21	30	35
	Professional/Self-emp	14	17.9	11.6	0	40	0	7	19.4	25	30
	Pensioner	0	0	0	0	0	0	0	0	0	0
	Germany Leaver	6	20.9	5.3	15	31	15	19.5	20	20	31
	Civil Servant	8	25.9	6.5	19.5	37	19.5	20	25	30.25	37
Weekly 2nd-Job Hours (given employed)	All	2605	0.1	0.4	0	6.9	0	0	0	0	0.3
	PAYG individuals	2046	0.1	0.4	0	6.9	0	0	0	0	0.3
	Professional/Self-emp	223	0.2	0.6	0	4.9	0	0	0	0.1	0.5
	Pensioner	0	0	0	0	0	0	0	0	0	0.0
	Germany Leaver	148	0.1	0.3	0	1.9	0	0	0	0	0.4
	Civil Servant	188	0.1	0.5	0	4.8	0	0	0	0	0.3

Table 4: Saving Regressions - Baseline results using the *reported* saving rate

	(1) ΔSR (% income)	(2) ΔSR (% income)	(3) ΔSR (% income)	(4) ΔSR (% income)	(5) ΔSR (% income)
D(reform)*treatment	-0.686 (-0.94)	-0.392 (-0.54)	0.329 (0.42)	-0.131 (-0.14)	0.318 (0.40)
D(revoke)*treatment	1.311** (2.09)	1.428** (2.29)	2.094*** (3.00)	2.308*** (2.77)	2.100*** (2.99)
D(reform)	-1.579 (-0.61)	-2.481 (-0.97)	-3.043 (-1.18)	-1.812 (-0.68)	-2.880 (-1.11)
D(revoke)	-2.204 (-0.62)	-2.602 (-0.74)	-3.303 (-0.93)	-2.698 (-0.75)	-2.759 (-0.77)
D(CS_reform)	0.992 (1.24)	1.261 (1.58)	1.588* (1.85)	— ()	1.386 (1.45)
Δ income		-0.002*** (-13.9)	-0.002*** (-13.9)	-0.002*** (-12.9)	-0.002*** (-13.1)
D(PAYG individ)			-1.480** (-2.26)	— ()	-1.783** (-2.37)
D(prof./sel-emp)			— ()	— ()	— ()
D(retired)			— ()	7.827 (1.16)	— ()
D(Germany leaver)			— ()	1.466** (2.16)	— ()
D(civil servant)			0.210 (0.14)	— ()	1.611 (0.85)
D(unemployed)			-1.689*** (-4.53)	-1.703*** (-4.56)	-1.502*** (-3.54)
Constant	1.177 (0.33)	1.366 (0.39)	2.960 (0.83)	0.767 (0.22)	1.179 (1.61)
Controls			Civil service & leavers		
Balanced Sample			1995-2000		
Observations	18338	18338	18338	17240	15273
Number of households	3543	3543	3543	3372	3426

All regressions include household fixed-effects and time fixed-effects.

t statistics in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Control variables dropped automatically by Stata due to multicollinearity
are indicated with a "-" in the coefficient cells of the tables.

Table 5: Saving Regression - Different Definitions of Saving

	(1) ΔSR (% income)	(2) Δ corrected SR (% income)	(3) ΔSR (% cons)	(4) Δ corrected SR (% cons)
D(reform)*treatment	0.329	0.686	1.829	2.942
	(0.42)	(0.64)	(1.03)	(1.27)
D(revoke)*treatment	2.094***	2.405**	3.610**	4.923**
	(3.00)	(2.55)	(2.30)	(2.40)
D(reform)	-3.043	-5.070	-6.044	-10.537
	(-1.18)	(-1.46)	(-1.05)	(-1.40)
D(revoke)	-3.303	-4.498	-5.182	-8.256
	(-0.93)	(-0.94)	(-0.65)	(-0.79)
D(CS_reform)	1.588*	2.757**	3.043	4.952**
	(1.85)	(2.38)	(1.57)	(1.96)
Δ income	-1.480**	-1.197	-2.535	7.111*
	(-2.26)	(-1.36)	(-0.80)	(1.72)
D(PAYG individ)	—	—	—	—
	()	()	()	()
D(prof./sel-emp)	—	—	—	—
	()	()	()	()
D(retired)	—	—	0.035	10.013**
	()	()	(0.0100)	(2.20)
D(Germany leaver)	0.210	-8.788***	—	—
	(0.14)	(-4.20)	()	()
D(civil servant)	-1.689***	-7.001***	-2.445***	-8.559***
	(-4.53)	(-13.9)	(-2.91)	(-7.81)
D(unemployed)	-0.002***	-0.002***	-0.004***	-0.005***
	(-13.9)	(-10.2)	(-11.0)	(-9.67)
Constant	2.960	3.472	4.349	-3.615
	(0.83)	(0.72)	(0.52)	(-0.33)
Controls		Civil service & leavers		
Balanced Sample		1995-2000		
Observations	18338	18338	18336	18336
Number of households	3543	3543	3543	3543

All regressions include household fixed-effects and time fixed-effects.

t statistics in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Control variables dropped automatically by Stata due to multicollinearity

are indicated with a "-" in the coefficient cells of the tables.

Table 6: Saving Regression - Alternate Samples

	(1) ΔSR (% income)	(2) ΔSR (% income)	(3) ΔSR (% income)	(4) ΔSR (% income)	(5) ΔSR (% income)
D(reform)*treatment	0.329	0.018	0.832	-0.150	-0.410
	(0.42)	(0.025)	(0.61)	(-0.25)	(-0.29)
D(revoke)*treatment	2.094***	1.693***	1.956	1.169**	3.137***
	(3.00)	(2.64)	(1.54)	(2.32)	(2.59)
D(reform)	-3.043	-2.786	-3.695	-1.630	-0.658
	(-1.18)	(-1.41)	(-1.30)	(-0.68)	(-0.16)
D(revoke)	-3.303	-1.584	-5.922	-1.499	-6.052
	(-0.93)	(-0.60)	(-1.50)	(-0.44)	(-0.98)
D(CS_reform)	1.588*	1.173	1.654	0.970	1.887
	(1.85)	(1.49)	(1.34)	(1.26)	(1.18)
Δ income	-0.002***	-0.002***	-0.003***	-0.002***	-0.003***
	(-13.9)	(-16.5)	(-12.7)	(-13.8)	(-6.97)
D(PAYG individ)	-1.690	-0.942	-1.704	1.020	-3.386***
	(-1.20)	(-1.64)	(-1.22)	(0.43)	(-3.09)
D(prof./sel-emp)	-	-	-	-	-
	()	()	()	()	()
D(retired)	-	-	-	-	-
	()	()	()	()	()
D(Germany leaver)	-0.210	-	-	2.101	0.000
	(-0.14)	()	()	(0.87)	()
D(civil servant)	-	0.230	-	1.994	-0.499
	()	(0.16)	()	(0.75)	(-0.16)
D(unemployed)	-1.689***	-1.794***	-1.746***	-1.939***	-1.648***
	(-4.53)	(-5.38)	(-4.55)	(-5.23)	(-2.78)
Constant	1.391	0.664	6.003	-1.219	2.606**
	(1.04)	(1.09)	(1.51)	(-0.55)	(2.25)
Controls	Baseline	Baseline	Only Civil Servants	Baseline	Baseline 1 worker HH
Balanced Sample	1995-2000	No	1995-2000	1995-2000	1995-2000
Observations	18338	25542	17380	19743	5702
Number of households	3543	6378	3531	3783	1125

All regressions include household fixed-effects and time fixed-effects.

t statistics in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Control variables dropped automatically by Stata due to multicollinearity
are indicated with a "-" in the coefficient cells of the tables.

Table 7: Looking for an age effect of precautionary saving

	(1) ΔSR (% income)	(2) Δ corrected SR (% income)	(3) ΔSR (% cons)	(4) Δ corrected SR (% cons)
D(reform)*treatment	0.349	1.216	1.885	3.571
	(0.40)	(1.05)	(0.94)	(1.36)
D(revoke)*treatment	2.567**	2.254	4.501*	4.553
	(2.30)	(1.51)	(1.76)	(1.36)
D(reform)	-3.360	-6.341*	-6.457	-12.148
	(-1.24)	(-1.75)	(-1.04)	(-1.49)
D(revoke)	-5.181	-6.296	-7.939	-11.768
	(-1.35)	(-1.23)	(-0.90)	(-1.02)
$D(age_i)$*D(revoke)*treatment	-0.013	-0.001	-0.021	0.009
	(-0.70)	(-0.023)	(-0.48)	(0.15)
D(CS_reform)	1.769*	3.198**	3.451	5.765**
	(1.90)	(2.57)	(1.61)	(2.06)
Δ income	-0.002***	-0.002***	-0.004***	-0.004***
	(-10.1)	(-7.31)	(-7.23)	(-6.50)
D(PAYG individ)	-1.904	7.378***	-2.622	-3.479
	(-1.28)	(3.73)	(-1.58)	(-1.60)
D(prof./sel-emp)	—	—	—	—
	()	()	()	()
D(retired)	—	—	—	—
	()	()	()	()
D(Germany leaver)	-0.226	9.137***	—	—
	(-0.14)	(4.15)	()	()
D(civil servant)	—	—	0.147	-10.367**
	()	()	(0.039)	(-2.09)
D(unemployed)	-1.414***	-6.749***	-2.010**	-8.110***
	(-3.49)	(-12.5)	(-2.16)	(-6.66)
Constant	1.622	-4.904***	6.633	11.322
	(1.15)	(-2.60)	(0.75)	(0.98)
Controls		Civil service & leavers		
Balanced Sample		1995-2000		
Observations	18338	18338	18338	17240
Number of households	3543	3543	3543	3372

All regressions include household fixed-effects and time fixed-effects.

t statistics in parentheses,   *** p<0.01, ** p<0.05, * p<0.1.

Control variables dropped automatically by Stata due to multicollinearity

are indicated with a "-" in the coefficient cells of the tables.

Table 8: Labour Hours Regression

	(1) HH Head *Hours*	(2) HH Head *Hours* Part-time	(3) Non-Head *Other Hours* Full-time Head	(4) Non-Head *Other Hours* Part-time Head
D(reform)*treatment	-0.160	-0.153	0.156	-0.137
	(-0.20)	(-0.055)	(0.35)	(-0.040)
D(revoke)*treatment	-0.743	4.386*	-0.022	-2.101
	(-1.07)	(1.65)	(-0.054)	(-0.65)
D(reform)	-6.789***	-9.069	-0.407	-0.980
	(-2.70)	(-1.52)	(-0.26)	(-0.13)
D(revoke)	-3.142	-5.537	-0.997	-17.903
	(-0.90)	(-0.53)	(-0.47)	(-1.41)
D(CS_reform)	-0.156	1.049	0.056	-0.692
	(-0.18)	(0.32)	(0.12)	(-0.17)
Δ income			Omitted	
D(PAYG individ)	-10.833***	-2.120	0.412	-0.939
	(-7.85)	(-0.58)	(1.06)	(-0.21)
D(prof./sel-emp)	-	-	-	-
	()	()	()	()
D(retired)	-	-	-	-
	()	()	()	()
D(Germany leaver)	-11.295***	-	-	-
	(-7.42)	()	()	()
D(civil servant)	-	-9.710*	-0.364	11.348
	()	(-1.68)	(-0.38)	(1.60)
D(unemployed)	-30.696***	1.974	-1.301	5.931
	(-83.5)	(0.62)	(-0.66)	(1.53)
Constant	45.379***	26.211***	1.559***	12.290***
	(34.5)	(7.11)	(3.81)	(2.72)
Controls		Civil servi	ce & leavers	
Balanced Sample		1995	-2000	
Observations	18741	1113	13067	1113
Number of households	3555	457	2754	457

All regressions include household fixed-effects and time fixed-effects.

t statistics in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Control variables dropped automatically by Stata due to multicollinearity
are indicated with a "-" in the coefficient cells of the tables.