Policy Uncertainty and Household Savings

POLICY UNCERTAINTY AND HOUSEHOLD SAVINGS

Francesco Giavazzi and Michael McMahon*

Abstract—Using German microdata and a quasi-natural experiment, we provide evidence on how households respond to an increase in uncertainty. We find that household saving increases significantly following the increase in political uncertainty observed in the run-up to the 1998 German general election. We also find evidence of a labor supply response by workers who can use the margin offered by part-time employment. Our results are suggestive of the economic effects of "wars of attrition": when political disagreement leads to delays in adopting a reform or the possibility that earlier reforms may be revoked, the increased uncertainty could slow the economy.

I. Introduction

THIS paper uses German microdata and a quasi-natural experiment to provide new evidence on how households respond to an increase in uncertainty. To time our quasi-experiment, we use, as a measure of uncertainty, the number of people who respond that they are "uncertain about the general economic situation in their country over the next 12 months" when asked a specific question in the German Gfk consumer survey, the German component of the European Commission European Survey, which covers a sample of some 2,000 individuals.[1] This variable displays a sharp increase in the run-up to the German general election held in September 1998. This increase in uncertainty happens despite the fact that households (on average), at the same time, were expecting an improvement in the general economic situation and a fall in unemployment. Why then did they perceive more uncertainty? The 1998 election was one of the closest in postwar Germany (James, 2000) and ultimately marked the end of the Kohl era. We therefore view the increase in uncertainty as being driven by the election, its difficulty to call, and particularly the differential policies that might be pursued, depending on the outcome, concerning unemployment and pension rules.

We study two dimensions along which households might have responded to this increase in uncertainty: savings and labor supply decisions. The first—the effect of an increase in uncertainty on savings—could be interpreted as precautionary savings. Carroll and Kimball (2007), for instance, define *precautionary savings* as "the additional saving that results from the knowledge that the future is uncertain."[2] We measure the effect of uncertainty on labor supply looking at hours worked in the primary and (possibly) in secondary jobs by all working-age household members.

The use of a quasi-natural experiment allows us to overcome the identification problem that often affects estimates of the effects of shifts in uncertainty based on aggregate data. Microdata allow us to control for individual characteristics and thus for heterogeneity across individuals.

We estimate households' response to an increase in uncertainty using a difference-in-difference (diff-in-diff) estimator and household data from the German Socioeconomic Panel (GSOEP), an annual longitudinal study that now covers some 10,000 German households and provides information on numerous aspects of their life, including household composition, family biographies, employment, social security, earnings, and health. Using data from repeated yearly surveys, we build a panel that extends over a six-year period (1995–2000) and contains around 2,000 households, yielding about 11,600 observations. We use fixed effects to control for unobservable characteristics, such as differences across heads of households in their degree of risk aversion. We use civil servants as the control group in our diff-in-diff estimator because civil servants, with jobs for life and a separate (and protected) pension system, were unaffected by the two reasons that are the best candidates to explain the increase in uncertainty that we observe: concern about the effect of the election outcome on unemployment and on pension rules.

We find that household saving increases significantly following the increase in uncertainty about the future path of income. A household can increase its savings either by consuming less or by working more; most previous studies on precautionary savings, the literature to which our work is most closely related, focus on the former effect. We also analyze households' response in terms of their labor market choices—hours worked in the primary and (possibly) in secondary jobs by all working-age household members. We find evidence of a labor supply response by workers who can use the margin offered by part-time employment.

Gourinchas and Parker (2001) use structural estimates of a dynamic stochastic model of households' expenditures over the life cycle with uninsurable labor income uncertainty to study the importance of precautionary savings, which they define as the amount of wealth accumulated by individuals

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[1] GfK NOP is an international market research company that conducts a monthly consumer survey in Germany.

[2] Some authors limit the definition of precautionary saving to short-term income uncertainty. Thus they would not label "precautionary" the increase in savings studied in this paper, which is associated with "political uncertainty" and (as we discuss later) could be related to the debate on pension reform. Regardless of the label attached, what the paper documents is how households react to an increase in uncertainty concerning future economic conditions.

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facing an uncertain income path. They show that this precautionary savings motive is especially important at young ages, while it becomes negligible for older households. Our data allow us to test whether the precautionary savings induced by the increase in uncertainty affects individuals differently depending on their age.

Fuchs-Schündeln (2008), examining the differential saving behavior of East and West German households over the 1990s, finds that "the precautionary savings motive is essential" in order for her life cycle model to be able to match this behavior.

Carroll and Kimball (2007) conclude their excellent survey of the empirical research on precautionary savings with these words: "The qualitative and quantitative aspects of the theory of precautionary behavior are now well established. Less agreement exists about the strength of the precautionary savings motive. . . . Structural models that match broad features of consumption and saving behavior [such as Gourinchas & Parker, 2002, and Cagetti, 2003] tend to produce estimates of the degree of prudence that are less than those obtained from theoretical models in combination with risk aversion estimates from survey evidence. Direct estimates of precautionary wealth seem to be sensitive to the exact empirical procedures used, and are subject to problems of unobserved heterogeneity. . . . A problem that plagues all these efforts is identifying exogenous variations in uncertainty across households." Our experiment is immune from these problems.

We are not the first to follow this route. Fuchs-Schündeln and Schündeln (2005), using data drawn from the same German survey, employ German civil servants as a control group with low precautionary savings motives. Our paper differs from theirs in focus and timing; the focus of their paper is on the role of self-selection into safe careers by risk-averse agents, which biases typical estimates of precautionary savings down; the timing of their natural experiment is German reunification, which produced an exogenous shock to labor market risk for East Germans.[3] Lusardi (1998) splits households into groups distinguished by their self-assessed risk of job loss and uses the groups with low or zero risk to estimate the importance of precautionary savings.

We find that households facing an increase in uncertainty saved more. For instance, a household that previously was holding savings constant at 8.9% of disposable income (the average saving rate in our balanced sample in 1998) would, ceteris paribus, have a saving rate of about 15.9% by the year 2000. Households that faced an increase in uncertainty also worked more, exploiting the margin provided by part-time employment. For instance, a head of household working only part time, who previously worked ten hours per week (the 10th percentile of part-time hours per week in our balanced sample in 1998), would increase her hours to around nineteen hours per week (up to the 25th percentile).

Our results are independent of the reasons that uncertainty jumped in the run-up to the 1998 election. It is nonetheless interesting to ask what could have produced such an increase in uncertainty. The 1998 election, which was one of the closest elections in postwar German history (even professional polling institutes failed to predict the swing in voting preference in the final election run; see James, 2000), was fought on two major themes, beyond the obvious political themes of the personalities of the two candidates, Helmut Kohl and Gerhard Schröder, and the makeup of the government coalition after the election (Pulzer, 1999). The two themes were the high level of unemployment, particularly in the new eastern Länder, and the incumbent government's "reform of the century," the 1997 pension reform that Schröder was pledging to revoke.[4] The possibility that Kohl's reform might be revoked was particularly prominent in the campaign because in order to justify the adoption of new pension rules, Chancellor Kohl had explained to the German public that the existing system was unsustainable. The argument seemed convincing because under the existing rules, by 2050, payroll contribution rates would need to reach 25%, from 18% in the mid-1990s (Börsch-Supan, 2003). The reform Kohl adopted had 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 & Jochem, 2007). The possibility that Schröder might win the election and the pension system returned to an unsustainable path is thus a candidate explanation for the observed increase in uncertainty. Such an explanation appears consistent with the observation that while uncertainty about future economic conditions was increasing, German people were expecting (on average) an improvement in the general economic situation and a fall in unemployment.

Reform reversals, that is, the adoption by one government of a new set of rules and their revocation by a subsequent government, are 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 reform on someone else. Reforms of pay-as-you-go pension systems in countries where population growth is decelerating are a frequent example. There is rarely a disagreement on the fact that the rules will eventually have to be changed, but as one reform plan after the other is considered, decisions are repeatedly postponed because political parties are unable to agree on how the burden should be shared among various groups in society and, in particular, between the young and the old.[5]

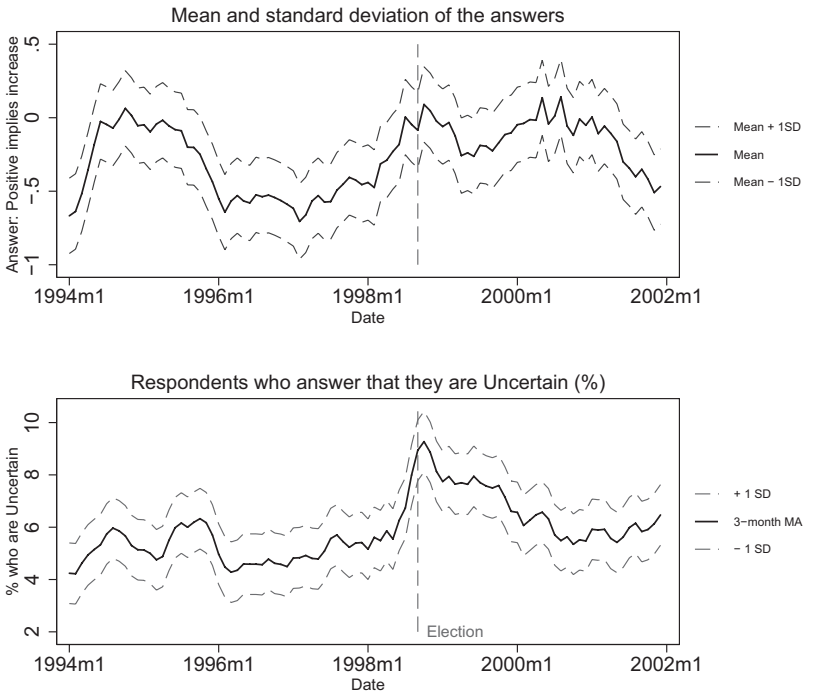
Thus, a political economy interpretation of our experiment is that such wars of attrition can have significant economic

[3] Also, as we discuss below, their papers uses a different measure of saving.

[4] This fact allows us to use civil servants as the control group in our diff-in-diff estimator: German civil servants have lifetime jobs, and Kohl's pension reform left the generous pension entitlements of civil servants intact, specifically excluding them from any change in pension rules.

[5] Boeri, Börsch-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.

FIGURE 1.—GFK CONSUMER SURVEY: EXPECTATIONS ABOUT THE ECONOMICS SITUATION OVER THE NEXT TWELVE MONTHS



effects. People do not simply sit and wait. When a reform is motivated with the argument that the system in place is unsustainable, delays in adopting new rules, or the possibility that they might be revoked once adopted, do not simply perpetuate the status quo. They raise uncertainty and induce households to save more than they would otherwise: consumption may fall and the economy might slow down for no other reason than the inability to agree on a reform.

In two papers that are relevant for this interpretation of our results, Bloom (2007, 2009) finds that uncertainty increases markedly in response to major economic and, most relevant for this paper, political shocks. He identifies increases in uncertainty in reaction to the 9/11 terrorist attacks, but also the Cuban missile crisis and the assassination of John F. Kennedy. His work shows that increased political uncertainty reduces firm investment and hiring (2009), as well as R&D (2007). He does not, however, consider the effect on households.

Our results support the view that the revocation of Kohl's reform 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-1990s to 11% at the start of the millennium; something similar also happened in Japan.)

II. The Quasi-Natural Experiment

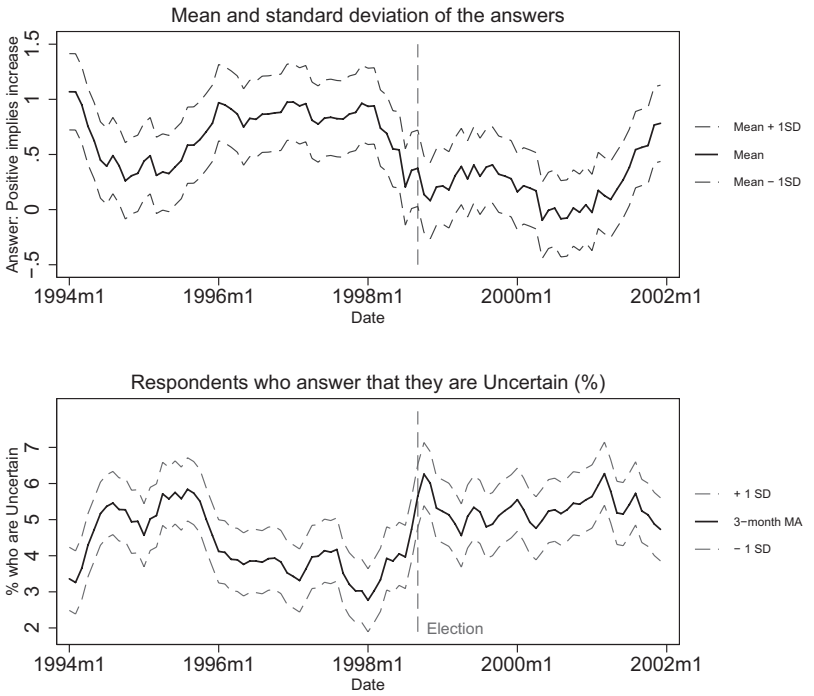
In order to measure time-varying consumer uncertainty, we use the Gfk consumer survey. Conducted monthly, the survey asks about 2,000 German households to answer a number of both backward-looking and forward-looking questions; we focus on the forward-looking component. The respondents choose from a menu of multiple-choice answers for each question. The answers are all qualitative and accord to a five-option ordinal scale: $++$ (most positive answer), $+$ (positive answer), $=$ (neutral answer), $-$ (negative answer), and $--$ (most negative answer); "don't know" is an alternative answer. Using the responses to individual questions over the period 1994 to 2002, we calculate a measure of the mean answer (to measure the average response), and also examine the "don't know" answers separately as a measure of uncertainty.[6]

Here, we focus on two specific questions taken from this survey. Figure 1 provides the mean and uncertainty measures to the question, "How do you expect the general economic situation in this country to develop over the next 12 months?" Despite the improved outlook on average (top panel), there is a marked increase in uncertainty in the months that lead up to the September 1998 election (bottom panel), and this uncertainty seems to fall back only gradually over the following three years. Figure 2 shows that answers to the question, "How do you expect the number of people unemployed in this country to change over the next 12 months?" follow a similar pattern. Although unemployment is expected to decline (consistent with falling unemployment over the period), uncertainty increases around the election.[7]

[6] We use the following mapping from qualitative answers in order to derive a quantitative measure of the average response: $++ = 2$, $+ = 1 == 0$, $- = -1$, $-- = -2$. Hence, a higher mean indicates a more positive response.

[7] The figure shows that a similar increase in uncertainty occurred earlier in the 1990s, but this increase was associated with a deteriorating labor market.

FIGURE 2.—GFK CONSUMER SURVEY: EXPECTATIONS ABOUT UNEMPLOYMENT OVER THE NEXT TWELVE MONTHS



A. *Timing*

To study how households' saving and labor supply decisions respond to this increase in uncertainty, we need to define both the period when uncertainty jumped and, in order to employ the diff-in-diff approach, a treatment and a control group. We do this by defining an uncertainty dummy, which corresponds to the period of increased uncertainty indicated in figure 1. Thus, we define:

$$\text{uncertainty}_t = \begin{cases} 1 \text{ between August 1998} \\ \quad \text{and December 1999} \\ 0 \text{ otherwise} \end{cases}.$$

Although the election, which took place on September 27, 1998, is the focal point of the uncertainty (the figures indicate that it is where the uncertainty peaks), we do not define the increase in uncertainty as occurring only then. Instead we allow for some anticipation of the close election. As shown in figures 1 and 2, uncertainty begins to increase in the months leading up to the election; we use August 1998 as the start of the uncertainty period. The end date, December 1999, is selected to coincide with the month in which general economic uncertainty returns to the level in June 1998. In the econometric analysis below, we perform robustness tests, allowing for a shorter anticipation period (a later start date for the uncertainty) and a slower return to the lower uncertainty state (a later end date).

Since we observe the month in which the interview is conducted, we can precisely identify those who answered in the uncertainty period. Our sample includes the years from 1995 (three years before the election) to 2000 (two years after the election), inclusive. We do not explore the years, beyond 2001 as in these years, a large number of other reforms were enacted, which might confuse the identification.

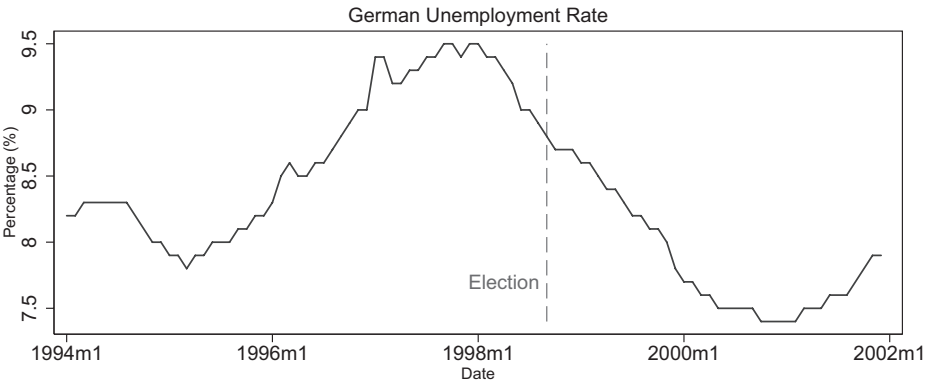
B. *Treatment and Control Group*

We identify the effect on household saving of the increase in uncertainty using a diff-in-diff estimator. The treated group includes households that are likely to have been affected by the increase in uncertainty. Our control group consists of households whose head is a civil servant.

There were two main concerns in the run-up to the 1998 election that are the best candidates to explain the increase in uncertainty: concern about unemployment and concern about possible changes in the pension system. Unemployment had been a major economic issue in Germany since reunification. Figure 3 shows that in the run-up to the 1998 election, unemployment was falling. Nonetheless, there were concerns that unemployment was still persistently high in the New Länder. Moreover, following the end of government subsidies to construction, unemployment in this sector was rising. Policies affecting unemployment were therefore a major issue of the election.

The other big election issue was pensions. Since the early 1990s Germany had gone through a long debate that increased the public's awareness of the unsustainability of the existing PAYG pension system. In August 1997 Chancellor Kohl announced a major reform explaining that the existing rules were no longer sustainable: the reform was adopted in December 1997 and was due to come into effect in 1999 (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%.

FIGURE 3.—GERMAN UNEMPLOYMENT RATE



During the 1998 election campaign, Gerhard Schröder made the revocation of this law one of his main campaign promises: when he won, one of his first decisions as new chancellor was to revoke Kohl's pension reform (Rentenkorrekturgesetz, November 20, 1998). Nothing happened on pension reform, for almost three years, until the adoption (in 2001) of the Riester reform, which, along with a gradual reduction of benefits, mostly encouraged enrollment in private pension plans. Though outside of our sample, the anticipation of this reform could have contributed to the decline in uncertainty that we observe in 2000.

The pension rights of civil servants had been insulated from the effect of the Kohl reform. This followed the adoption, at the end of January 1997 and thus before the Kohl reform, of a new set of rules for public sector employees. The main purpose of the new rules (Gesetz zur Reform des öffentlichen Dienstrechts), which had come into force on July 1, 1997, was to create a more market-driven system for career civil servants, introduce flexibility in work practices and performance-related pay, and increase mobility across jobs. Among the many provisions of this law was a measure that marginally modified the rules of civil service pensions,[8] but de facto safeguarded the generosity of the system going forward, and, more important, 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.

With unemployment and pension reform being the most likely explanations for the observed increase in uncertainty, we are presented with a natural control-versus-treatment distinction for our diff-in-diff estimator:

- *Civil servants:* Households headed by a civil servant constitute our "control" group. First, civil servants, with a job for life, face no labor income risk.[9] Second, German civil service pensions are run separately from the PAYG system, and civil servants knew that their pension rights would be insulated from the effects of the Kohl reform, whatever direction such reform might take.
- *Individuals who are not civil servants (non-CS):* The majority of the individuals in the GSOEP survey (about 64% in 1998) are members of the PAYG public pension system and constitute our "treated group."[10] people in this group face uncertainty with regard to both the future of their pension rights and unemployment.[11]

We thus define:

$$\text{treated}_{i,t} = \begin{cases} 0 \text{ if civil servant} \\ 1 \text{ otherwise (non-CS)} \end{cases}.$$

[8] 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.

[9] Civil servants can be dismissed only if they serve a certain amount of time in prison (for one year if it is a criminal charge or six months if the charge is associated with treason). More detailed institutional information is available in Fuchs-Schündeln and Schündeln (2005), who also make use of civil servants as a control variable in a study of precautionary saving resulting from German reunification.

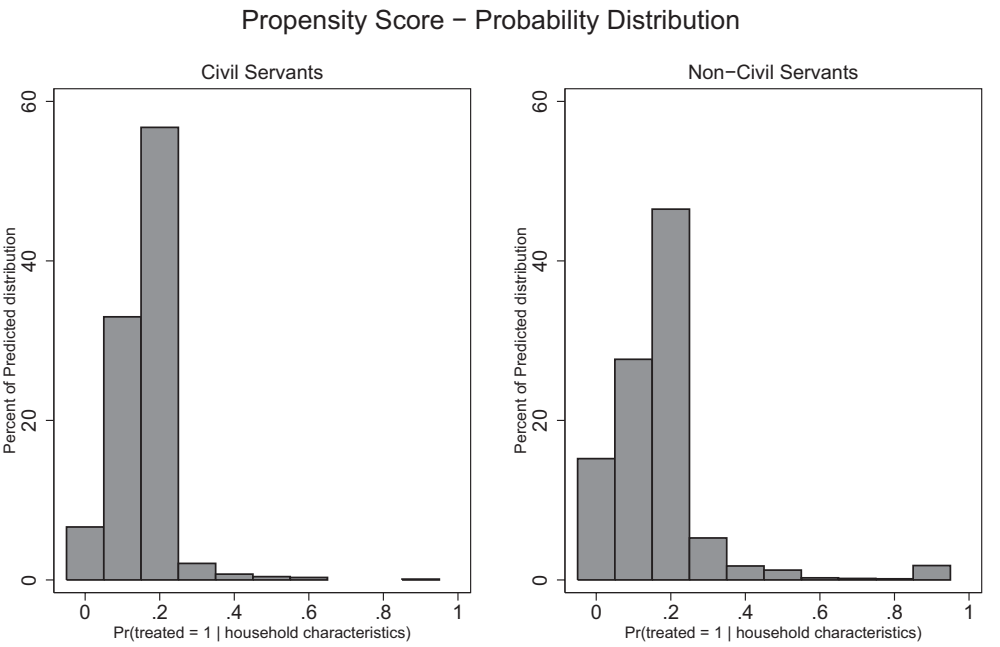
[10] Membership in the PAYG pension system is mandatory for almost all German workers. To be eligible for a pension, a worker must have earnings above a certain threshold.

[11] We drop from our sample other groups that may or may not have been affected by the reform. First, foreign nationals, a group that is overrepresented in the GSOEP survey, may have very different saving motives to German citizens. What is more, some may be in Germany only temporarily or expect to leave Germany before retirement. If a person who has contributed to the public pension system leaves Germany before claiming a pension, there are rules in place to treat accrued pension wealth fairly. (These rules can be quite complicated and differ depending on where the person moves to. If the move is 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 outside the EU, he or she can generally reclaim contributions made. In either case, such a worker is likely to be less affected, if at all, by changes to the German public pension system.) As such, we eliminate all foreigners from our sample group.

Two other groups that we exclude are the self-employed and professionals. Self-employed workers can choose whether 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. This group is also more difficult to link to the worries about unemployment. Professionals, such as lawyers, accountants, vets, and doctors, are required to join private pension plans and are thus unaffected by a reform of the public pension system. They are also less at risk (though not immune) from unemployment. In our analysis, we exclude both groups.

Finally, we eliminate pensioners from the control and treatment groups. Most proposed reforms of the pension system protect the benefits of those already in retirement, and pensioners are no longer part of the labor market. Pensioners should thus be unaffected by both types of uncertainty. We have also carried out robustness tests where we include them in our control group, and the results are similar. However, because their saving is subject to greater measurement error (discussed below) and they are no longer in the labor force, we do not include them in our baseline control group.

FIGURE 4.—PROPENSITY SCORES: PROBABILITY OF BEING TREATED



The horizontal axis shows the estimated probability of being treated measured from a panel logit regression of being treated—being affected by the uncertainty (a non-CS individual)—on various controls, including a household fixed effect. The vertical axis shows the percentage of households in each group.

The diff-in-diff estimator that we use relies on the assumption that prior to the treatment, households in the treated and in the control group are indistinguishable; that is, that the treatment is random. In other words, households headed by a civil servant are similar to all other households except for the fact that they are headed by a civil servant. We have checked this assumption looking at propensity scores.

We estimate a panel logit model of "being treated"—that is being headed by a non-CS individual—on years of education, the number of household members, the number of children, marital status, gender and age of head of household, and whether the head is unemployed. We estimate both fixed-effect and random-effect models on the whole sample. Using a Hausman test, we reject the null hypothesis that there are no differences in the regression coefficients between the two models. We therefore conclude that there is important unobserved heterogeneity to take care of. Of course, when we estimate the fixed-effects model, the identification of the propensity score estimates relies on households in which the main income earner changes status (civil servant to non–civil servant, or the reverse), and this is a relatively small group (667 observations from 127 households).

Nonetheless, we present the estimated probability of being treated in figure 4. The horizontal axis in figure 4 shows the estimated probability of being treated measured from the panel logit regression, including a household fixed effect. The vertical axis shows the percentage of households in each group. We find controls and treated groups close to both extremes of the estimated probabilities of treatment. In the left panel, for instance, we find households headed by a civil servant that have a high probability of being treated, that is, whose characteristics closely match those of the treated group—non-CS individuals. Symmetrically, the right panel of the figure shows that there are households headed by a non-CS individual who, considering their characteristics, might have been civil servants. This reassures us that the two groups are not too different from each other once we allow for unobserved, but fixed, heterogeneity across households; that is, the assumption that the treatment is random, conditional on fixed effects, is not too extreme.

Our diff-in-diff estimator also relies on there being some evidence that non-CS workers became more worried over the period 1998 to 2000 compared to civil servants. Unfortunately, we cannot decompose the responses used in figures 1 and 2 into civil servants and others.

III. Data

Our data are from the GSOEP. This survey, first conducted in 1984, is an annual longitudinal study that 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 that report their savings, the size of a balanced panel covering

the six-year period 1995 to 2000 contains almost 2,000 households, yielding about 11,600 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.[12] From the first, we obtain information about each member of the household: age, education, and employment status, which defines the future pension status, hours worked, and other measures 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, and area of residence. The concept of saving we use thus refers to the entire household. The head of household is defined in the GSOEP as "the person who knows best about the general conditions under which the household acts." In most cases, this coincides with the main earner in the household, although this not always the case. In order to establish the main public pension status of a household, and whether it is affected by the reforms, we use the information on the main earner (in terms of gross income per annum) rather than on the GSOEP-defined head of the household. When we repeated our analysis using the GSOEP head-of-household data, our results were qualitatively the same.

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 the interview happened during the period characterized by the increase in uncertainty.

We construct a balanced sample using six waves of the GSOEP survey: those from 1995 (three years before the election) to 2000 (the year before the Riester Law). Table 1 describes the characteristics of the 1,932 households in the balanced panel (table 1 considers their responses in 1998). The household proportions in terms of the key variables in the balanced sample are similar to those in the unbalanced data (not reported).

TABLE 1.—SAMPLE SIZE AND BASIC HOUSEHOLD CHARACTERISTICS: BALANCED SAMPLE, 1995–2000 (1998 DATA)

	Total	Civil Servants	Non–Civil Servants
Total who report income	1,932	167	1,765
By household saving			
Positive saving	1,281	137	1,144
Saving unreported	651	30	621
By labor force participation			
Full time	1,651	161	1,490
Part time	114	6	108
Unemployed	113	0	113
Out of the labor force	54	0	54

A. *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?" Households that answer "yes" then provide the average amount of money (in euros) left over. The amount given as the answer to this question is our main household saving variable, which we then express as a percentage of household disposable income or, alternatively, household consumption.[13]

One problem with our survey data concerns households that do not save (households that answer "no"). The GSOEP survey reports saving only for households that declare positive saving: if a household has zero or negative saving, the amount of saving is left unanswered or a 0 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, 651 of 1,932 households in the balanced sample in 1998, or about 34% (see table 1). Among the main earners of the household who do not report saving, 20% (in the balanced sample in 1998) are unemployed. The percentage of nonsavers is reasonably constant along the age distribution. We treat those with nonreported savings as zero savers; in section IV, we discuss the truncation problem this choice might induce.[14]

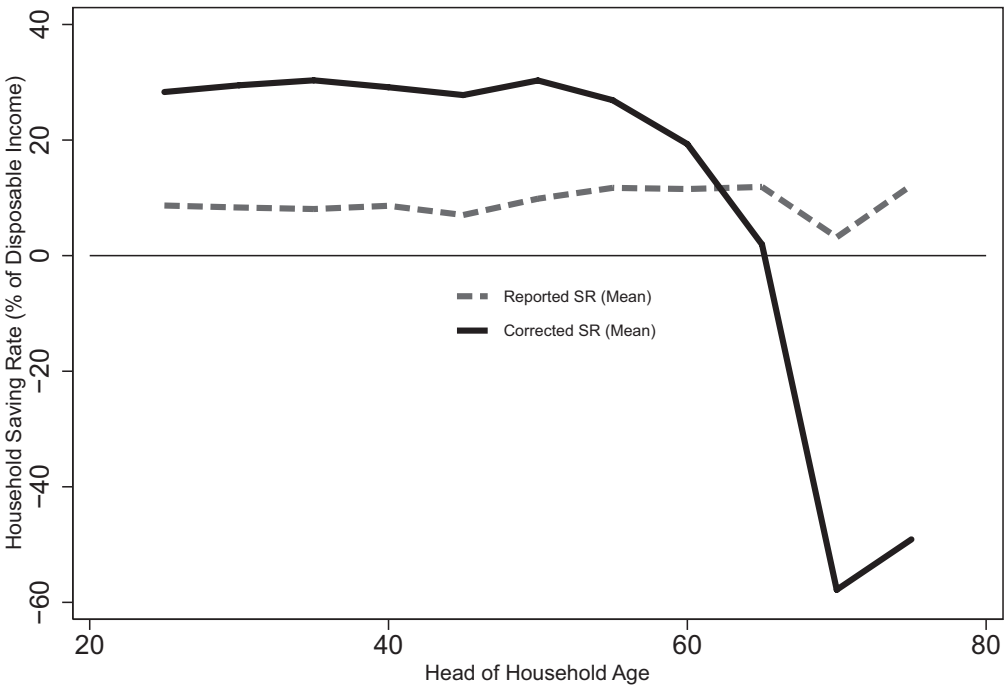
A second problem with our definition of saving arises from the PAYG pension system. The answers to the question about saving miss two portions of actual household saving. First are social security contributions by workers and by firms, which are not reported as savings, although they are a form

[12] 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 to 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 data set.

[13] Our measure of household saving differs from that used by Fuchs-Schündeln and Schündeln (2005), who use wealth levels as their dependent variable. To construct this variable, these authors assume that each household receives an average return from the assets it holds. Such a definition, however, is subject to measurement error. The same stock of total assets will in general yield different returns depending on the particular asset composition: if this happened, households with identical stocks of assets would end up being attributed different stocks of wealth. 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. Moreover, since our focus is on the reaction of household saving to a sudden increase in uncertainty, it is unlikely that household wealth (a stock) will immediately be affected by the sudden change in the saving rate (the flow). We therefore choose to examine the reported saving of households. In a more recent paper, Fuch-Schündeln (2008) measures flows into financial wealth using the same measure of saving as we do in this paper.

[14] In an appendix to Giavazzi and McMahon (2008), we follow Browning and Leth-Petersen (2003), among others, and impute saving rates for those for whom no saving is reported. The results are little changed when we use the sample that also includes estimated negative saving by households that do not report saving.

FIGURE 5.—REPORTED AND CORRECTED SAVING RATES IN GERMANY IN 1998, BY AGE OF HEAD OF HOUSEHOLD



Source: Authors' calculations using all 1998 GSOEP data. Baseline sample using GSOEP data: Mean saving rate by head of household age group in 1998.

of saving (which increases with income).[15] Thus, reported savings increase over a person's working life by less than "true" saving. Second, the pension payments an individual receives are misreported as income rather than being considered negative savings. Thus, reported savings remain positive even after retirement when actual savings are likely to be negative.[16] A similar problem arises for private pension plans. In the GSOEP survey, individual contributions to such plans are correctly reported as saving,[17] 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 is discussed in Poterba (1994), and its implications are shown in figure 5. Poterba shows that the age profile of the German saving rate (defined as the ratio of reported saving to disposable income in 1998) is at odds with the life cycle hypothesis: the difference is particularly sharp when compared with the U.S. profile obtained from the PSID survey 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.[18] Figure 5 shows the saving rate once we correct it 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 discussed in detail in the appendix posted on our Web sites.) The corrected age-saving profile resembles more closely that predicted by the life cycle hypothesis. As expected, correcting saving rates boosts the saving rate of those in employment and causes positive reported saving to become negative for retirees. But since we exclude pensioners from our main sample, we shall proceed using the reported saving rate as this is the margin of total saving, which is likely to be affected by any uncertainty and can be adjusted more directly by household behavior.[19]

Table 2 (top panel) shows sample statistics on the reported 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 percentage of disposable income, is 9%; this is slightly higher for civil servants (10.5%). Though some respondents claim to save almost 90% of their

[15] 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.

[16] To be precise, the misreporting 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).

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

[18] This fact is well known from the work of Börsch-Supan et al. (2001), Börsch-Supan and Stahl (1991), and Börsch-Supan (2003). Poterba (1994) makes the same observation for Japan.

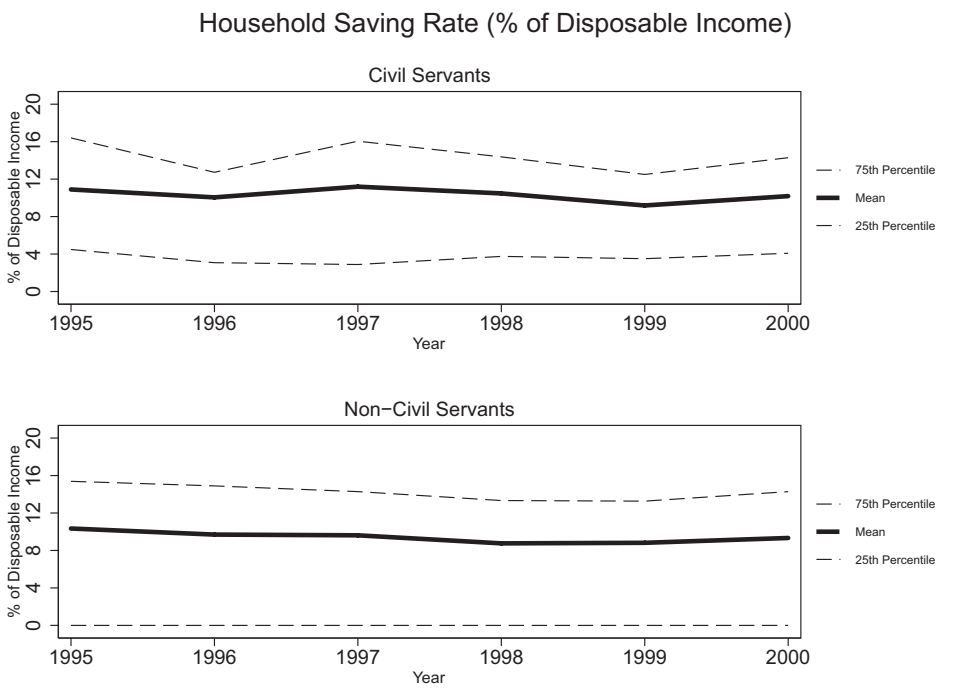
[19] Also, in Giavazzi and McMahon (2008), we show that our estimates are robust to the use of corrected saving rates, rather than reported saving rates, as the dependent variable.

TABLE 2.—SUMMARY STATISTICS BY TREATED$_{it}$ IN 1998: BALANCED SAMPLE, 1995–2000

		N	Mean	SD	Min	Max	p10	p25	p50	p75	p90
Reported Saving Rate (% of disp. income)	All	1,932	8.9	10.4	0	86.8	0	0	6.2	13.3	22.7
	Non-CS	1,765	8.8	10.4	0	86.8	0	0	5.9	13.3	22.7
	Civil servant	167	10.5	10.2	0	59.3	0	3.8	8.2	14.4	23.3
Hours	All	1,932	38.1	15.5	0	80	0	38	40	45	50
	Non-CS	1,765	37.9	15.9	0	80	0	37.5	40	45	50
	Civil servant	167	40.3	10.8	0	78	30.5	38.5	40	45	50
Workers	All	1,932	1.5	0.7	0	6	1	1	1	2	2
	Non-CS	1,765	1.4	0.7	0	6	1	1	1	2	2
	Civil servant	167	1.6	0.6	1	4	1	1	2	2	2
Part-time hours (given part-time)	All	114	23.6	9.7	0	40	10	19.5	25	30	35
	Non-CS	108	23.3	9.8	0	40	10	19.3	24.5	30	35
	Civil servant	6	29.5	5.1	20	35	20	29.5	30.3	32	35
Weekly second-job hours (given employed)	All	1,932	0.1	0.4	0	6.9	0	0	0	0	0.3
	Non-CS	1,765	0.1	0.4	0	6.9	0	0	0	0	0.4
	Civil servant	167	0.1	0.3	0	2.8	0	0	0	0	0.2

N: total observations. Mean: the sample average. p*XX* represents the *XX*th percentile of the distribution.

FIGURE 6.—DISTRIBUTION OF HOUSEHOLD SAVING RATE (SR)



Baseline sample using GSOEP data.

disposable income, the reported saving rate for high savers (90th percentile) is 23%.

Households differ not only in the level of their savings but also in their trend. Figure 6 shows the mean, median, and key percentiles of the saving rate by public pension status. Two points are important. First, the average non-CS household has reduced the level of saving over the period in question, while civil servants have (on average) increased their saving slightly. Second, there are numerous non-CS households that have been increasing their saving rate, but also many that have been lowering it. The same is true for households headed by a civil servant. Because fixed effects on levels cannot capture these trend differences, we use the first difference of the saving rate as our dependent variable. Once we include a fixed effect in such a regression, any trend differences will be eliminated, allowing us to focus on how households have changed their behavior around their trend.

B. 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 question asked is: "How many hours do your actual working hours consist of, including possible overtime?" 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?" Finally 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 2 also reports descriptive statistics on the number of weekly hours worked by the head of household (second panel), the number of household members who work (third panel), the average weekly hours of those household heads who work part time in their primary employment (about 10% of all those in employment, displayed in panel 4), and, in the final panel, the average number of hours worked in a second 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 to 40 hours per week. The main earner in a non-CS household is more likely to work part time, and civil servants are more likely to work in a household with two or more workers. Though some of the non-CS 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 45 of the 1,765 employed non-CS workers in the balanced sample in 1998 engage in 1 hour or more of secondary employment per week (42 of these 45 work full time in their primary employment; the remainder are part time employed in their main employment). In the balanced sample of 167 civil servants, only 5 of these engage in secondary employment (in 1998).

IV. Saving Results

Our baseline regression is

$$\begin{aligned}\Delta \mathrm{sr}_{it} = {} & \beta_t + \theta.x_{it} + \eta.\mathrm{treated}_{i,t} + \psi_1.(\mathrm{D(CSR)}_t \times \mathrm{cs}_{it}) \\ & + \psi_2.\mathrm{D(CSR)}_t + \delta_1.\mathrm{D(Kohl)}_t \\ & + \delta_2.(\mathrm{D(Kohl)}_t \times \mathrm{treated}_{i,t}) + \tau_1.\mathrm{uncertainty}_t \\ & + \tau_2.(\mathrm{uncertainty}_t \times \mathrm{treated}_{i,t}) + \alpha_i + \varepsilon_{it}, \qquad (1)\end{aligned}$$

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, and x_{it} is a vector of controls (for instance the change in household disposable income). The coefficient we are most interested in is τ_2, which captures the differential effect of uncertainty on the treatment group; it tells us whether the behavior of treated households—those affected by the increase in uncertainty—differs from the behavior of our controls. A positive value of τ_2 is a measure of the extent to which the household reacts to the change in uncertainty.

The saving rates of the individual households in our sample display different trends. To estimate the response of the household saving rate to the treatment and to separate this effect from the trend behavior, we use, as the dependent variable, the change in the saving rate and include household fixed effects. An additional advantage of using the change in saving rates as our dependent variable is that households that move from zero to positive saving, or vice versa, can be analyzed in the same regression without worrying about the truncation at zero of our dependent variable.

However, we may still have a truncation problem resulting from the fact that some members of our sample, whom we record as having zero saving, actually have negative saving. To the extent that these households have zero reported saving—when instead they are actually running down their wealth—we may overstate or understate the household reaction we find. If households begin to report negative saving because of the uncertainty, then we would be overstating the effect. Similarly, if civil servants who report negative saving were to react to the uncertainty period by dissaving less (despite the fact that the main sources of uncertainty do not affect them), then, by continuing to record them as (unchanged) zero savers, we would again overstate the effect of uncertainty. As there is no marked divergence in zero saving between civil service and non-CS individuals in the uncertainty period, we do not believe that these potential problems are driving the results.

A concern regarding our identification relates to whether we capture the effect of uncertainty rather than the effect on saving from changes in the mean expected outcome under the new policies. We cannot, of course, be certain that the effect we capture is only that related uncertainty but, if anything, the mean effect of the policy changes we study should lead the treated group to save less rather than more. Labor market conditions in Germany were improving (see figure 3), and Kohl's pension reform, which we discuss below, was revoked by Schröder precisely because he believed that the costs of the reform were borne too heavily by households. This means that any concern that our estimates include a mean effect, in addition to the effect of uncertainty, would bias our estimates of the effect of uncertainty down. (A similar argument applies to concerns that the civil servants were not completely immune from uncertainty: any worries by civil servants would also bias down our estimates of τ_2.)

Prior to the uncertainty associated with the electoral campaign, there were two pension-related policy changes that may influence the behavior of household saving of the treated and control groups differently. We therefore control separately for these changes. For households in which the head of the household is a civil servant, we control for the change in civil service pensions rules using a "reform" variable ($D(CSR)_t$), and its interaction with a civil service dummy:

$$\mathrm{D(CSR)}_t = \begin{cases} 1 \text{ after January 1997} \\ 0 \text{ otherwise} \end{cases}.$$

where January 1997 is the month in which a new set of rules for public sector employees was introduced. For all the other

TABLE 3.—SAVING REGRESSIONS, BASELINE RESULTS
DEPENDENT VARIABLE: REPORTED SAVING RATE

	(1) Δ SR (% income)	(2) Δ SR (% income)	(3) Δ SR (% income)	(4) Δ SR (% income)	(5) Δ SR (% income)
Uncertainty_t	−5.2**	−5.2**	−5.2**	−5.1**	−5.0**
	(−2.4)	(−2.4)	(−2.4)	(−2.3)	(−2.3)
$\text{Uncertainty}_t \times \text{Treated}_{i,t}$	3.2**	3.2**	3.1**	3.1**	3.0**
	(2.3)	(2.3)	(2.2)	(2.2)	(2.1)
$\text{Treated}_{i,t}$	−0.1	0.02	−0.2	0.09	−1.0
	(−0.1)	(0.0)	(−0.1)	(0.1)	(−0.8)
$\text{D(unemployed)}_{it}$	−2.6***	−1.7**	—	−1.5**	—
	(−4.7)	(−2.6)	(.)	(−2.1)	(.)
Δincome_{it}	−0.003***	−0.003***	−0.003***	−0.003***	−0.003***
	(−10.4)	(−10.4)	(−10.1)	(−10.6)	(−8.4)
D(Kohl)_t	−3.6	−3.6	−4.1	−3.1	−3.4
	(−1.3)	(−1.2)	(−1.4)	(−1.0)	(−1.2)
$\text{D(Kohl)}_t \times \text{Treated}_{i,t}$	2.8*	2.7*	2.6*	2.5*	2.4
	(1.9)	(1.8)	(1.7)	(1.7)	(1.6)
D(CSR)_t	−0.4	−0.4	−0.5	−0.4	−0.6
	(−0.8)	(−0.8)	(−0.9)	(−0.6)	(−1.0)
$\text{D(CSR)}_t \times \text{cs}_{it}$	2.9**	2.8**	2.9**	2.6*	2.7*
	(2.1)	(2.0)	(2.1)	(1.9)	(1.9)
Job Worries_{it}		0.3**	0.3*	0.2*	0.4**
		(2.1)	(2.0)	(1.7)	(2.5)
D(East)_{it}		−1.9	−2.8		
		(−0.9)	(−1.2)		
$\text{D(Construction)}_{it}$		0.4	0.04		
		(0.7)	(0.1)		
Constant	−0.3	−0.4	0.06	−0.10	−0.4
	(−0.2)	(−0.3)	(0.0)	(−0.1)	(−0.3)
Control: Civil servants					
Balanced sample: 1995–2000					
Observations	11,603	11,603	10,972	10,615	7,453
Number of households	1,972	1,972	1,969	1,921	1,332

All regressions include household fixed effects and time fixed effects. t statistics in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. A — in the coefficient cells of the table indicates variables dropped automatically due to multicollinearity.

households (those in which the head of household is not a civil servant), we define a variable to control for the year in which the pension reform proposed by Chancellor Kohl was announced:

$$\text{D(Kohl)}_t = \begin{cases} 1 \text{ between August 1997} \\ \quad \text{and September 1998} \\ 0 \text{ otherwise} \end{cases}.$$

This variable is also interacted with the treated_{it} dummy. The coefficient τ_1 thus captures a more standard effect: the shift in the saving rate resulting from the announcement of Kohl's pension reform by households that 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.[20]

Our baseline results use the standard definition of saving, reported saving as a percentage of disposable income, and are obtained from the balanced panel extending over the six years 1995 to 2000. The results are presented in table 3.[21] The first column of the table shows the baseline results: along with controls for unemployment and the change in income, the regression includes time and household fixed effects. The estimate of τ_2 (reported in the second row of table 3) is both statistically (at the 95% level) and economically significant. A coefficient of 3 indicates that the increase in uncertainty induced treated households to, on average, increase the change in their saving rate by 3 percentage points per year. This means, for instance, that a treated household that previously was holding savings constant at 8.9% of disposable income (the average saving rate for the balanced sample in 1998) would have a saving rate of about 15.9% by the year 2000.

Of course, we do not expect that this new higher level of saving would continue forever; it is only in the period up to 2000, during the heightened uncertainty, that we would expect the affected households to save markedly more. Since our dependent variable is the change in the saving rate, we would expect its sign to reverse. In regressions not reported here, we have included a separate dummy variable to capture the impact on the saving behavior of the treated group of the

[20] Although these policy variables are a time effect, we are able to include them, in addition to year fixed effects, as we have the houshold responses dated by the month of the year in which they were interviewed. For example, we have some households that are interviewed in 1997 for which $\text{D(Kohl)}_t = 0$ and others for which $\text{D(Kohl)}_t = 1$. The year effect captures the average of the households in 1997 whereas $\text{D(Kohl)}_t = 1$ only for some of the households.

[21] 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 dash in the coefficient cells of the tables.

TABLE 4.—SAVING REGRESSION: ROBUSTNESS

	(1) Δ SR (% income)	(2) Δ SR (% income)	(3) Δ SR (% income)	(4) Δ SR (% income)	(5) SR-level (% income)
Uncertainty_t	−5.2**	−4.5	−4.2***	−3.7**	−0.7
	(−2.4)	(−0.9)	(−2.6)	(−2.0)	(−0.4)
$\text{Uncertainty}_t \times \text{Treated}_{i,t}$	3.2**	2.1*	3.2**	3.1**	1.9*
	(2.3)	(1.9)	(2.4)	(2.2)	(1.7)
$\text{Treated}_{i,t}$	0.02	0.02	0.05	0.04	−0.3
	(0.0)	(0.0)	(0.0)	(0.0)	(−0.3)
$\text{D(unemployed)}_{it}$	−1.7**	−1.7**	−1.7**	−1.7**	−2.6***
	(−2.6)	(−2.5)	(−2.6)	(−2.6)	(−3.4)
Δincome_{it}	−0.003***	−0.003***	−0.003***	−0.003***	
	(−10.4)	(−10.4)	(−10.4)	(−10.4)	
D(Kohl)_t	−3.6	−2.2	−3.1	−3.0	−0.7
	(−1.2)	(−0.6)	(−1.1)	(−1.0)	(−0.3)
$\text{D(Kohl)}_t \times \text{Treated}_{i,t}$	2.7*	1.1	2.7*	2.7*	1.4
	(1.8)	(1.0)	(1.8)	(1.8)	(1.1)
D(CSR)_t	−0.4	−0.2	−0.4	−0.4	0.1
	(−0.8)	(−0.4)	(−0.7)	(−0.7)	(0.2)
$\text{D(CSR)}_t \times \text{cs}_{it}$	2.8**	1.0	2.8**	2.7*	2.1*
	(2.0)	(1.2)	(2.1)	(2.0)	(1.9)
Job Worries_{it}	0.3**	0.3**	0.3**	0.3**	−0.1
	(2.1)	(2.1)	(2.1)	(2.1)	(−0.8)
D(East)_{it}	−1.9	−2.0	−2.0	−2.0	0.3
	(−0.9)	(−0.9)	(−0.9)	(−0.9)	(0.1)
$\text{D(Construction)}_{it}$	0.4	0.4	0.4	0.4	1.1*
	(0.7)	(0.7)	(0.7)	(0.7)	(1.9)
Income_{it}					0.09*** (6.0)
Constant	−0.4	−0.3	0.1	−0.05	12***
	(−0.3)	(−0.3)	(0.0)	(−0.0)	(9.7)
Control: Civil servants					
Balanced sample: 1995–2000					
Observations	11,603	11,603	11,603	11,603	7,946
Number of households	1,972	1,972	1,972	1,972	1,779

All regressions include household fixed effects and time fixed effects. t statistics in parentheses, $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

end of the uncertainty. The coefficient on this dummy variable is negative, meaning that the level of saving would begin to fall toward earlier levels, but it is not statistically significant. The lack of significance is perhaps because we have only one year of postuncertainty behavior, and we cannot easily extend our sample to further years because 2001 saw the introduction of new pension (and other) reforms, which might have directly affected the saving behavior of German households.

Returning to the main results, column 2 adds controls for the labor market: the included variables are self-assessed worries about job security, an indicator of whether the household lives in one of the new Länder, as well as a dummy variable indicating whether the head of household's employment was in the construction industry. In column 3, we repeat the regression in column 2 but drop observations where the head of household is unemployed: dropping unemployed households ensures that the results are not driven by the presence of unemployed non-CS workers (since there are no unemployed civil servants in the sample).

Column 4 excludes any construction workers from the sample, and column 5 uses only nonconstruction workers living in the former West Germany. This is because Fuchs-Schündeln's (2008) results suggest that in the years covered by our sample, East German households may still be reacting to the large shock of reunification. In all cases our uncertainty effect remains both statistically and economically significant.

In these regressions, our estimates of τ_1 (reported in the first row of table 3) capture a time effect from the entire period of uncertainty, and therefore, despite being negative and statistically significant, it should not be interpreted in isolation from other year dummies. The estimates of δ_2 indicate that households affected by Kohl's reform appear to have responded to the news by changing the path of their saving rate so as to offset the cut in pension wealth.[22]

We now run a few robustness tests concerning our uncertainty variable.[23] Column 1 of table 4 reproduces, for comparison, column 2 of table 3 (balanced sample with labor market controls). Column 2 uses $\text{uncertainty}_t = 1$ between October 1998 and December 1999 (*later start*), column 2 uses $\text{uncertainty}_t = 1$ between July 1998 and June 2000 (*later end*), and, finally, column (4) uses $\text{uncertainty}_t = 1$ between October 1998 and June 2000 (*both later*). In all cases, the uncertainty effect remains statistically significant.

Our final robustness check uses the level of household saving as the dependent variable. Despite the differing trends

[22] This substitutability between private and public pension wealth is similar to the findings of Attanasio and Brugiavini (2003). They find that Italian households increased private saving in response to 1992 pension reform that reduced public pension wealth.

[23] One might expect a weaker effect (relative to other PAYG households) on households headed by PAYG workers in which there is also civil service worker. Unfortuntely our sample does not contain enough observations (32 such households in 1998) to examine the differences across households.

TABLE 5.—LOOKING FOR AN AGE EFFECT ON SAVING

	(1) Δ SR (% income)	(2) Δ SR (% income)	(3) Δ SR (% income)	(4) Δ SR (% income)
Fixed effects (FE) or random effects (RE)?	FE	RE	FE	RE
Uncertainty_t	−5.9	−5.1	−5.3**	−4.5**
	(−1.4)	(−1.6)	(−2.4)	(−2.4)
$\text{Uncertainty}_t \times \text{Treated}_{i,t}$	4.4	4.7	3.3**	3.4***
	(1.1)	(1.6)	(2.2)	(2.6)
$\text{Treated}_{i,t}$	−0.009	0.3	0.02	0.2
	(−0.0)	(0.5)	(0.0)	(0.4)
$\text{D(unemployed)}_{it}$	−1.7**	−1.2**	−1.7**	−1.2**
	(−2.5)	(−2.5)	(−2.5)	(−2.5)
ΔIncome_{it}	−0.003***	−0.002***	−0.003***	−0.002***
	(−10.4)	(−10.0)	(−10.4)	(−10.0)
D(Kohl)_t	−3.6	−3.0	−3.6	−3.0
	(−1.2)	(−1.2)	(−1.2)	(−1.2)
$\text{D(Kohl)}_t \times \text{Treated}_{i,t}$	2.7*	2.8**	2.7*	2.9**
	(1.8)	(2.1)	(1.8)	(2.1)
D(CSR)_t	−0.4	−0.5	−0.4	−0.5
	(−0.8)	(−1.1)	(−0.8)	(−1.1)
$\text{D(CSR)}_t \times \text{cs}_{it}$	2.8**	2.9**	2.8**	2.9**
	(2.0)	(2.3)	(2.0)	(2.3)
Age_{it}	0.7*	0.02*		
	(1.9)	(1.7)		
$\text{Age}_{it} \times \text{Uncertainty}_t$	0.01	0.01		
	(0.2)	(0.2)		
$\text{Age}_{it} \times \text{Uncertainty}_t \times \text{Treated}_{i,t}$	−0.03	−0.03		
	(−0.3)	(−0.5)		
D(Age)_{it}			−0.3	0.4
			(−0.4)	(1.3)
$\text{D(Age)}_{it} \times \text{Uncertainty}_t$			0.2	−0.2
			(0.1)	(−0.1)
$\text{D(Age)}_{it} \times \text{Uncertainty}_t \times \text{Treated}_{i,t}$			−0.5	−0.2
			(−0.3)	(−0.2)
Constant	−28*	1.0	−0.3	−0.8
	(−1.9)	(0.6)	(−0.2)	(−1.5)
Control: Civil servants				
Balanced sample: 1995–2000				
Observations	11,603	11,603	11,603	11,603
Number of households	1,972	1,972	1,972	1,972

All regressions include time fixed effects, and controls for Job Worries, East Germany and Construction workers. t statistics in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

between civil servant and non-CS households, column 5 of table 4 reports that the level of saving was significantly higher (at 10% level) for our treated households. This regression uses the level of household income rather than the change in household income as an independent variable.

We next ask whether the identified effect on saving is age dependent. Since our results suggest that greater uncertainty about the future of pensions induces higher savings, we are interested in whether this effect occurs throughout the age distribution. Fuchs-Schündeln (2008) shows that the impact of a change in economic regime, induced by German reunification, affects different cohorts in a differential way. Gourinchas and Parker (2001) suggest that the precautionary savings motive should be especially important at young ages, while it should become negligible for older households, which, on average, hold large amounts of liquid wealth. Their model, however, excludes pension wealth. Our data allow us to test whether the additional savings 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. We investigate whether the effect of uncertainty on saving is age dependent, estimating

$$\begin{aligned}\Delta \text{sr}_{it} = {} & \beta_t + \theta.x_{it} + \eta.\text{treated}_{i,t} + \psi_1.(\text{D(CSR)}_t \times \text{cs}_{it}) \\ & + \psi_2.(\text{D(CSR)}_t) + \delta_1.\text{D(Kohl)}_t \\ & + \delta_2.(\text{D(Kohl)}_t \times \text{treated}_{i,t}) + \tau_1.\text{uncertainty}_t \\ & + \tau_2.(\text{uncertainty}_t \times \text{treated}_{i,t}) + \tau_3.\text{Age}_{it} \\ & + \tau_4.(\text{Age}_{it} \times \text{uncertainty}_t) \\ & + \tau_5.(\text{Age}_{it} \times \text{uncertainty}_t \times \text{treated}_{i,t}) + \alpha_i + \varepsilon_{it}. \end{aligned} \tag{2}$$

The fixed-effect regression is reported in column 1 of table 5 and finds no significant effect of age on the extent to which households reacted to the uncertainty. However, including a time-varying age variable with fixed effects may be problematic; demeaning age, as fixed effects does, would transform this variable in a year-of-birth-specific trend. Therefore, we have tested whether we could drop fixed effects. A Hausman test, which compares the consistent (though not necessarily efficient) fixed-effects model (FE) with a random-effects model (RE), fails to reject the null hypothesis that the RE

and the FE coefficients are identical ($Prob > \chi^2 = 0.83$). The RE regression is reported in column 2, and our finding of no significant age effect is unchanged. We cannot reject the hypothesis that the effect of uncertainty on the treated group ($\tau_2 + \tau_5 \times \text{Age}_{it}$), for the range of ages within our sample, is 3 percentage points, the same as we find in the earlier regressions.

In columns 3 and 4 of table 5, we also run equation (2) using a dummy variable, which is 1 if the head of household is older than 50 years of age. We again find no differential impact of age on the results reported already.

V. Hours Results

Additional savings can be achieved by consuming less or working more. Bodie, Merton, and Samuelson (1992) present a theoretical model of labor, consumption, and portfolio decisions over the life cycle. They find that labor supply decisions (on hours and retirement) can be used as a form of insurance to protect against poor investment outcomes. In the context of our paper, the labor supply response can be used to provide additional savings in the face of greater pension uncertainty.

We analyze the effects on labor supply of the uncertainty by 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 labor market is relatively rigid: it is unclear the extent to which work contracts allow employees to change their working hours, and overtime is strictly regulated. Workers, however, can adjust their labor supply using the margin offered to those in part-time employment or by taking second jobs. Around 10% of workers in our balanced sample are part-time workers, but very few work significant hours in second jobs. Of the 1,598 non-CS workers in full- or part-time employment, only 45 work on average 1 hour or more per week in a second job in 1998 (this number is reasonably constant across years).

Using various measures of hours worked, we estimate the following equation using a household fixed-effects specification:

$$\begin{aligned} hours_{it} = {} & \beta_t + \theta.x_{it} + \eta.\text{treated}_{i,t} + \psi_1.(\text{D(CSR)}_t \times \text{cs}_{it}) \\ & + \psi_2.\text{D(CSR)}_t + \delta_1.\text{D(Kohl)}_t \\ & + \delta_2.(\text{D(Kohl)}_t \times \text{treated}_{i,t}) + \tau_1.\text{uncertainty}_t \\ & + \tau_2.(\text{uncertainty}_t \times \text{treated}_{i,t}) + \alpha_i + \varepsilon_{it}. \quad (3) \end{aligned}$$

Relative to the baseline saving regression, equation (1), we omit the income control because this is endogenous to the amount of hours worked. The number of hours worked is both determined by, and determines, the individual's income.[24] We include industry fixed effects to control for industrial differences in hours variables. We report the results in table 6. In columns 1 to 3, 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 part time. In columns 3 we use all households where the head of household was a part-time worker in 1996; this is designed to capture the effects of part-time workers potentially moving into full-time employment.

TABLE 6.—HOURS REGRESSIONS

	(1)	(2)	(3)	(4)	(5)
				Nonhead	
	Hours	Head Hours	Hours	Hours	Hours
	All Workers	Part Time	Part Time	All Workers	Part Time
Uncertainty$_t$	−0.2	−8.0	−11	0.6	−1.1
	(−0.1)	(−1.0)	(−1.5)	(0.6)	(−0.2)
Uncertainty$_t$ × Treated$_{i,t}$	−0.8	8.6*	8.6*	0.3	1.1
	(−0.7)	(1.7)	(1.7)	(0.5)	(0.2)
Treated$_{i,t}$	−2.0**	−2.0	−7.2*	0.2	0.04
	(−2.2)	(−0.3)	(−1.9)	(0.4)	(0.0)
D(unemployed)$_{it}$	—	—	−9.1***	—	—
	(.)	(.)	(−4.7)	(.)	(.)
D(Kohl)$_t$	−4.4*	−7.7	−6.7	1.0	0.1
	(−1.8)	(−1.0)	(−0.9)	(0.7)	(0.0)
D(Kohl)$_t$ × Treated$_{i,t}$	0.4	6.6	5.9	−0.2	0.8
	(0.4)	(1.2)	(1.1)	(−0.2)	(0.2)
D(CSR)$_t$	−0.1	0.6	0.5	−0.02	−1.4
	(−0.3)	(0.5)	(0.3)	(−0.1)	(−1.3)
D(CSR)$_t$ × cs$_{it}$	−0.9	5.7	5.9	0.7	1.3
	(−0.8)	(1.0)	(1.2)	(1.0)	(0.3)
Job Worries$_{it}$	0.4***	0.5	0.01	−0.1	0.1
	(3.0)	(1.3)	(0.0)	(−1.3)	(0.4)
D(East)$_{it}$	−0.6	0	0	0.5	0
	(−0.3)	(.)	(.)	(0.5)	(.)
D(Construction)$_{it}$	0.2	5.0	9.5	1.5	1.5
	(0.1)	(0.8)	(1.5)	(1.3)	(0.3)
Constant	46***	32***	40***	−0.8	1.2
	(20.4)	(3.5)	(4.3)	(−0.6)	(0.1)
Control: Civil servants					
Balanced sample: 1995–2000					
Observations	10,526	618	633	10,526	618
Number of households	1,950	266	106	1,950	266

All regressions include household fixed effects, industry fixed effects, and time fixed effects. t statistics in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. A — in the coefficient cells of the table indicates variables dropped automatically due to multicollinearity.

The estimate of τ_2, the diff-in-diff effect on hours of the uncertainty, 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 consistent with the rigidity of German labor contracts. However, 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 (see columns 2 and 3). The point estimate, 8.6, means that a head of household working part time, who previously worked 10 hours per week, would have increased her hours to 19 hours per week, an economically significant increase.

In columns 4 and 5 of table 6, we shift the focus to the hours worked by other household members (excluding the hours worked by the head of household). Column 4 considers all

[24] In the saving regressions, we worry less about including income because the change in saving rates has a much less clear impact on income; the causality runs much more clearly from income changes to savings.

possible households, while column 5 focuses on the households headed by part-time workers (as in column 2). There is no evidence in either case 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. Moreover, there is no evidence that the hours response is dependent on age.

VI. Conclusion

The results in this paper are of interest from three perspectives. First, we have provided a direct measure of how households respond to an exogenous increase in uncertainty about the path of future income, which could be interpreted as precautionary savings. Our estimates of the effect of uncertainty on household savings are the result of a quasi-natural experiment and thus overcome the identification problem that often affects such measures. They also control for individual characteristics and thus for heterogeneity across individuals. Second, we find evidence that faced with an increase in uncertainty, households also respond by adjusting their labor supply. They do so, in a highly regulated labor market, using the only margin that has some flexibility, part-time employment; this effect, however, is only marginally significant. Finally, while independent of the reasons that uncertainty jumped in the run-up to the 1998 election, our results are suggestive of the economic effects of wars of attrition—situations in which reforms are delayed because political parties are unable to agree on how the burden of a reform should be shared among groups in society. Delays in adopting a reform, or the possibility that a reform, after it has been adopted by one government may be revoked by another, raise uncertainty and induce households to save more: consumption may fall and the economy might slow down for no other reason than political uncertainty.

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