The Effect of Public Pension Wealth on Saving and Expenditure: Evidence from Poland's 1999 Pension Reform[1]

Work in Progress

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Abstract

In order to study whether public pension systems displace private saving, we use the quasi-experimental variation in pension wealth created by Poland's 1999 pension reform. The reform decreased pension generosity overall, but it had a differential effect on individuals depending on their year of birth. Using the 1997–2003 Polish Household Budget Surveys, we begin by estimating "difference-in-differences" regressions, where we compare household saving and expenditure across time and between cohorts affected and unaffected by the reform. Next, we use the post-reform change in pension wealth to estimate the extent of saving crowd-out and consumption crowd-in. Using two-stage least squares, we identify the effect of pension wealth on private saving by using the cohort-by-time variation in pension wealth that is explained by the reform. We find that one additional Polish zloty, or PLN, of pension wealth crowds out about 0.24 PLN in household saving, while one additional PLN of pension wealth crowds in about 0.21 PLN in household consumption. We also find heterogeneity in responses. For the middle-aged cohorts, we find a large crowd-out of saving (about 0.54), while the crowd-out for younger cohorts equals about 0.30. We find evidence of close to complete crowd-out among highly educated households.

Keywords: Public pension, Crowd-out effect, Saving, Difference-in-differences, Natural experiment

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1 Introduction

In 1999, a drastic reform of the public pension system was launched in Poland. Prior to the reform, Poland offered generous public pension benefits and abundant possibilities for early retirement. Deemed to be fiscally unsustainable, that system, in 1999, was reformed. The reform greatly reduced the generosity of pension benefits and provided incentives for postponing the age of retirement and increasing voluntary saving.

Our paper studies Poland's 1999 pension reform to answer whether public pension systems have a displacing effect on private saving. Our aim is to estimate the public pension crowd-out—in other words, to estimate by how much a marginal increase in public pension wealth depresses private saving. This issue is of current interest, as many defined-benefit pension systems are funded by current contributions, and because of higher life expectancy and lower fertility these systems find it difficult to meet the promises made to older generations. Understanding the relationship between pension wealth and private saving helps us to understand how much individuals would save for their retirement in the absence of a mandated pension system.

To estimate the crowd-out, we use the fact that the 1999 pension reform had a differential impact on individuals depending on their year of birth. Some individuals were allowed to stay fully in the pre-reform system with high replacement rates, while others were directly affected by the reform. The reform created a large variation across cohorts in expected pension wealth, thus fostering a setting similar to that of a natural experiment.

We begin by estimating a set of difference-in-differences regressions where we calculate the change in household saving and expenditure before and after the reform for the cohorts affected and unaffected by the reform. This procedure allows us to control for unobserved time-invariant differences between various cohorts and for secular time trends in the outcome variables. In order to estimate the public pension crowd-out, we complement the simple difference-in-differences estimation with a more structural approach. For each household, we model the expected pension wealth under the pre-reform and post-reform legislation and relate this variable to household saving. Because pension wealth is likely to be endogenous with respect to saving, we use the instrumental variables technique. Specifically, we use the variation across cohorts and time created by the reform to construct year-of-birth-cohort-by-time dummies and use them as instrumental variables for pension wealth. By doing so, we can separate the variation in pension wealth that is due to unobserved heterogeneity, such as differences in taste for saving, and identify public pension crowd-out by using the variation in pension wealth created by the reform.

The quasi-experimental variation is valuable because there is ambiguity as to whether public pension systems crowd out private saving. On one hand, if public pension wealth is a perfect substitute for private wealth, then the canonical life-cycle model predicts there to be a one-for-one relationship between a marginal increase in pension wealth and a decrease in private saving. Another conceivable effect, discussed by Feldstein (1974), is that the pension system might increase saving, as it makes people retire earlier, hence extending the period when individuals consume out of accumulated assets. If so, then a marginal increase in public pension wealth will *crowd in* saving. Furthermore, public pension wealth is usually an illiquid asset, which may complicate any sharp theoretical predictions about the relationship between private saving and mandatory public pension saving. It is also worth noting that at the time of the reform Poland had a relatively undeveloped capital market. Also, the saving decisions of some individuals may be unaffected by changes in pension wealth because they are not interested in how the pension system works or are very present-biased in their discounting of the future.[4] Finally, as pointed out by Gale (1997), individuals save for other reasons than

[4] Bottazzi, Jappelli, and Padula (2006) use data on how well informed individuals are about pensions and find the largest crowd-out effects among the well-informed groups.

retirement and may view their voluntary saving as a different form of saving than that mandated by the pension system.

In addition to theoretical uncertainty, the empirical literature on public pension crowd-out has been inconclusive. Feldstein (1974) finds that household savings and U.S. Social Security wealth are close substitutes and concludes that Social Security depresses personal saving by up to 50 percent, hence reducing the stock of capital and national income. Among other studies that have found large crowd-out effects are Feldstein and Pellechio (1979), Bernheim (1987), and Alessie, Kapteyn, and Klijn (1997). Other research has found modest crowd-out effects: King and Dicks-Mireaux (1982), Hubbard (1986), and Hurd, Michaud, and Rohwedder (2012) find relatively low crowd-outs, ranging from 0.20 to 0.33 in absolute value. Furthermore, Pozo and Woodbury (1986) find support for a Social Security *crowd-in* and also find that Social Security wealth induces people to retire early. [5,6]

The dispute over the magnitude and direction of the crowd-out are in part due to different empirical strategies. A key difficulty in estimating the relationship between pension wealth and household saving lies in how to account for unobserved traits which influence saving decisions as well as the determinants of pension wealth; see Gale (1998) for a discussion of other biases in the estimates of crowd-out. More recently, the literature on crowd-out effects has searched for exogenous shifts in pension wealth as a source of identification. Attanasio and Rohwedder (2003), Attanasio and Brugiavini (2003), Bottazzi, Jappelli, and Padula (2006), Aguila (2011), Feng, He, and Sato (2011), Banerjee (2011), and Yang (2013) use differential impacts across groups and time created by pension reforms as a source of variation in pension wealth and apply variants of the difference-in-differences estimator to estimate the crowd-out effect. Whereas Attanasio and Rohwedder (2003), Attanasio and Brugiavini

[5] Katona (1965) also finds evidence of private pension crowd-in.

[6] In addition to the dispute over the displacing effects public pensions, there exists a closely related literature concerned with the displacing effects of private pensions (e.g., Cagan [1965], Katona [1965], Munnell [1976], Engelhardt and Kumar [2011]) and tax-deferred pension accounts (e.g. Venti and Wise [1990], Gale and Scholz [1994], Chetty et al. [2014]). Bernheim (2002) and Gale (2005) provide a recent literature review.

(2003), Bottazzi, Jappelli, and Padula (2006), and Aguila (2011) find crowd-out effects of 0.50 or more in absolute value, Feng, He, and Sato (2011), Banerjee (2011), and Yang (2013) report modest crowd-out, ranging between 0.10 and 0.27 in absolute value. In sum, the literature relying on quasi-experimental variation, too, remains in dispute about the magnitude of public pension crowd-out.

In our main results, where we assume a subjective discount factor of 2 percent, we find that one additional PLN of pension wealth crowds out about 0.24 PLN in household saving, while one additional PLN of pension wealth crowds in about 0.21 PLN in household consumption. We also find heterogeneity in responses. The crowd-out of saving for the older and middle-aged cohort affected by the reform is close to full. Our findings also show that for highly educated households, public pension wealth and private saving are very close substitutes.

We also present several sensitivity checks, where we vary our assumptions regarding the households' subjective annual discount factor. We show that the degree of public pension crowd-out is inversely related to how heavily households discount the future. If the annual discount rate equals 10 percent, then crowd-out is almost zero and the 2SLS estimates approach OLS estimates. If we assume that the annual discount rate is 1 percent, the crowd-out is estimated to be between 0.30 and 0 .40.

The rest of the paper is organized as follows. Section 2 provides background information about Poland's public pension system in years before and after the reform. Section 3 describes the data and variables from the Polish Household Budget Surveys and the empirical strategy used to analyze the data. Section 4 describes the results, Section 5 discusses the findings. The final section draws conclusions.

2 A brief overview of Poland's 1999 Pension Reform[7]

In the early 1990s Poland had, relative to its living standards, a generous public pension system financed on a pay-as-you-go basis. However, the combination of ample use of early retirement options and a falling fertility rate raised questions about the system's fiscal sustainability. In order to help finance the pension system, the contribution rate was successively raised after the early 1990s. Soon it became apparent that, rather than changing the contribution rate or the indexation of the benefits, Poland needed to reform its public pension system. The initial steps toward a major reform of the system were formulated by the left-wing coalition in 1994, and, in the years following, negotiations were held regarding the choice of funding and transition rules.

The plan to reform the pension system moved forward after the electoral victory of the center-right-wing coalition in the fall of 1997. Although it was anticipated that a pension reform would take place in some form, the details of who would be affected and to what extent was still a matter of debate in 1998. The vote was passed in October 1998, and the new pension system was launched on January 1, 1999.[8] As Chłoń-Domińczak (2002) points out, one of the factors driving the haste in reforming the pension system was a strong public backing of pension reform, which perceived the old pension system as a carry-over from communist days.

Arguably, the most salient components of the reform were the following:

- To relate the generosity of the pension benefit formula to the lifetime earnings profiles, thus providing a clear incentive to postpone retirement. Projections that assumed no change in the timing of retirement forecast alarming drops in the replacement rates (defined as the ratio of first pension benefit to last salary) from about 65–76 percent to

[7] This section is based on Chłoń-Domińczak (2002), who provides a detailed description of Poland's pension system and the events leading up to the reform.
[8] See Hausner (2002) and Chłoń-Domińczak (2002) for a description of the political negotiations preceding the reform.

about 40–60 percent for men. For women, this drop would be as high as from 70 percent in the pre-reform system to a 30–50 percent post-reform replacement rate. This dramatic reduction for women stems from the fact that the post-reform pension formula rewards longer careers, while women tend to have spotty labor force participation.

- To "nudge" the public to take an interest in their pensions by altering the formula for the pay-as-you-go part to resemble the structure of a funded defined contribution pension—a so-called notionally defined contribution (NDC) pension.[9] NDC pensions are accounts of pension rights, based on an individual's entire earnings profile, with a rate of return based on the economy-wide wage growth. The NDC pension is funded by current contributions, but the formula is set up to mimic a fully funded plan (hence the term "notional"). The reform also introduced a small, fully funded defined contribution pension plan.
- To make the system more actuarially fair—i.e., structuring the benefit formula so that in expectation the present value of contributions to the system would equal the present value of future benefits.
- To increase the effective retirement age to the statutory retirement age, which even before the reform was 60 years for women and 65 years for men. However, because of a variety of early retirement options, the effective retirement age before the reform was 59 years for men and 55 years for women.[10]

[9] Such plans are also called notional or nonfinancial defined contribution plans. A similar system has also been adopted in Sweden; see Holzmann, Palmer, and Robalino (2012).
[10] Reaching an agreement regarding the early retirement privileges proved to be one of the major obstacles of the pension reform. The negotiations illustrated that retaining the option to retire early is a "focal point" of the pension debate in Poland. In the end, a compromise was reached where the transition cohorts working in certain occupations could still retire early, and also women retained the possibility to retire early; see Table 2 for details.

- Limiting the scope of early retirement privileges for various occupations, broadly defined as “demanding.” For example, miners could retire after contributing to the system for 25 years, regardless of age (Perraudin and Pujol 1994).

In Table 1 we highlight more of the differences between the pre-reform and reformed pension systems. Note that pension reforms tend to be implemented gradually, and for the 1999 reform it will take until the 2030s before the cohorts fully covered by the reformed system will transition to retirement. However, since life-cycle theory suggests that households are forward-looking and form their saving decisions by taking into consideration expectations of their lifetime income, a large change in future pension benefits may induce households to alter their saving behavior even if retirement is years away. In the second column of Table 1, we describe the features of the post-reform system once it reaches a “steady state.”

2.1 The impact of the reform across cohorts

The gradual implementation of the reform created a variation in how it affected individuals depending on year of birth; see Table 2. This lends itself to studying the impact of the reform on four different cohorts: one unaffected cohort and three cohorts affected by the reform with varying intensity.

- First, all those born before 1949 (i.e., those who were older than 50 years at the time of the reform) remained in the pre-reform system. We refer to this cohort as the *comparison cohort*.
- Second, the first five year-of-birth cohorts of women, born from 1949 to 1953 would receive a mix of pre-reform benefits and post-reform benefits; see Table 2. This exception was motivated by the fact that the new pension formula punishes short careers, and many women of this generation had careers of short duration. Since this cohort was only partially affected by the reform, we expect it to have a

milder impact on this cohort. We refer to the 1949–1953 cohort as the *older cohort*.

- Third, those born after January 1, 1949, but before January 1, 1969 (i.e., between 30 and 50 years of age at the time of the reform), also retained early retirement privileges, but had their pension formula calculated according to the post-reform formula. Hence, even if these individuals choose to exercise the option to retire early, their pension benefit will be calculated according to the post-reform formula. Since the post-reform formula rewards longer careers, one might suspect that the saving rate of these groups would increase in order to finance their longer retirement period. We refer to the cohort born 1954–1968 as the *middle-aged cohort.*
- Fourth, those born after 1969 (i.e., younger than 30 at the time of the reform) are fully in the post-reform pension system, with no early retirement privileges and no exemptions to the post-reform pension formula. We refer to this cohort as the *younger cohort*.

2.2 Was the public aware of the pension reform?

Existing literature on financial literacy (e.g. Gustman and Steinmeier [2005]) has shown that people may not fully understand how the pension system works. In order to expect a pension reform to have an effect on saving, the public should at least know about the main provisions of the reform.

To put the 1999 pension reform in perspective, it is worthwhile to point out that it was one of four other major reforms conducted in the same time period (the other reforms included a reform of the educational system, a new local government and administration division, and a reform of the medical care system). Chłoń-Domińczak (2002) points out that one of factors motivating the pension reform was a strong backing from the public opinion, which suggests that the public was to some degree aware of the reform.

To develop a sense of how the “main street” might have perceived the pension reform, we searched the archives of Poland’s tone-setting national daily newspaper, *Gazeta Wyborcza*, for terms “pension reform,”“pension system,” “reform of pension system,” and “pension” for the years 1997–1999. Based on this collection of articles, we noted that one salient feature of the coverage was the emphasis on how the reform would impact cohorts born on and after January 1, 1949. The media coverage included “information boxes” that showed practical examples of what the pension formula would be for a certain types of workers in the pre-reform and post-reform systems. This coverage leads us to think that the readers of *Gazeta Wyborcza* were aware that the pension reform would have a differential impact depending on year of birth and we hope that this information diffused through society.

The reporting about the pension reform continued in 1998 and 1999, suggesting that there was an on-going demand for information about the pension reform. Since information may diffuse slowly, it is reasonable to assume that some people might not have immediately understood the incentives of the post-reform pension system. As we describe below, for that reason we follow cohorts over five year after the reform.

3 Data and Methods

3.1 Data

Our data come from Polish Household Budget Surveys (BBGD), collected by the Polish Central Statistical Office; see Barlik and Siwiak (2011). The BBGD is a monthly survey of household expenditures that also collects a rich set of demographic data. Each month about 3,100 households are interviewed, which adds up to about 37,500 households annually (about

1/1000 of Poland's population). The BBGD collects information on monthly household expenditure, available income, labor income, and demographic information.

We use data for the years 1997–2003; this allows us to observe four years after the reform year of 1999. We include these years to allow for any lag during which households adjust their behavior after reform. We use two years before the reform, 1997 and 1998, to test for anticipation effects and group by time trends. If there are pre-reform differences in outcomes between groups affected by the reform and groups unaffected by the reform, then we must question whether the responses we observe after the reform are really due to the reform.

Although in the later years there is a small longitudinal sample in the BBGD, it is too small for the purpose of our study, and so we use pooled cross-sections of the BBGD. Following the literature, we construct household saving as the residual between household available income and total household expenditure. The saving rate is defined as household saving divided by household available income.

Our regression sample consists of households whose head was born between 1937 and 1980, and for each year we restrict the sample to include 18- to 65-year-old heads of household. Appendix A details additional sample cuts.

In order to relate saving to pension wealth, we need to construct the pension wealth based on the demographic information in the BBGD and institutional knowledge. We define household expected pension wealth as the present value of the sum of future pension benefits of both spouses, adjusted by survival probabilities obtained from the Polish life tables; see Brugiavini, Maser, and Sundén (2005) for a discussion of how to estimate pension wealth.

In order to compute pension wealth, first we need to forecast lifetime earnings profiles for both spouses. We estimate Mincer labor income profiles for heads of households and spouses separately. To forecast pension wealth, one needs detailed knowledge of the pension legislation before and after the reform. For the computation of pension wealth, we try to make assumptions about the labor supply decisions that are presumably typical. Appendix A details the assumptions we make at this stage of the analysis. The model could be made more realistic, but the objective of our paper is not to model pension wealth level as an end in itself, but rather as the relationship between pension wealth and private saving *on the margin*. Later in the paper, we check the sensitivity of our assumption by conducting several robustness checks.

In order to account for cross-sectional differences in planning horizons of the households and different points in the life cycle of when the reform occurred, we correct the expected pension wealth by a discrete-time version of "Gale's *Q*" (Gale 1998) adjustment factor derived in Attanasio and Brugiavini (2003) and Attanasio and Rohwedder (2003). Following this literature (Attanasio and Brugiavini [2003], Attanasio and Rohwedder [2003], and Bottazzi, Jappelli, and Padula [2006]), we assume that the subjective discount rate equals 2 percent and that the coefficient of relative risk aversion equals one. We discuss this factor in Appendix B and conduct sensitivity checks of these assumptions later in the paper.

3.2 Descriptive Statistics

Table 3 presents the descriptive statistics for the estimation sample. For income, expenditure, pension wealth, and saving variables, we report the sample mean, standard deviation, and median. For the other variables, we report means and standard deviation (although not for proportions).

The average saving rate in the BBGD is quite low, about 2 percent (because of a large number of negative values), but the median is about 9 percent.[11] Turning to the computed pension benefit, we see that, on average, the ratio of household gross pension benefits to current gross household labor income is about 0.50.

[11] The household net saving rate in Poland between 1997 and 2003 was about 10.5 (OECD 2010 Factbook).

Since a lower pension benefit implies a lower pension wealth, but it is more difficult to interpret changes in pension wealth, in order to develop intuition for the source of variation in pension wealth, in Table 4 we compute the median pension benefit replacement rate under the pre- and post-reform legislation for the cohort unaffected by the reform and the three cohorts affected by the reform. We calculate the replacement rate using data in the BBGD and define it as the ratio of the first pension benefit of the head of household to the last preretirement salary of the head of household.

Prior to the reform, all of the cohorts considered in our analysis could expect a median replacement rate of about 60–64 percent. After the reform, the median replacement rate for the comparison cohort (born between 1937 and 1948 and unaffected directly by the reform) remained at about 60 percent. After the reform, the median replacement rate falls for the older, the middle-aged, and the younger cohort by about 20 percentage points. Although the percentage point decrease is similar across the affected cohorts, we expect cohorts late in their life cycle to react more strongly than the younger cohort. Such differences in treatment intensity across cohorts allow us to study whether changes in saving behavior differ in the direction predicted by the life-cycle model.

3.3 Estimating the effect of the reform

In order to investigate whether the 1999 reform did have an impact on saving behavior, we begin by comparing the mean outcomes for the cohorts affected by the reform and the mean outcomes of cohorts unaffected by the reform (those born before 1949), before and after the reform. To do so, we estimate a set of multiyear difference-in-differences regressions, such as the following:

$$y_{it} = \alpha_t + \alpha_g + \alpha_{tg} + x_{it}\beta + \varepsilon_{it}, \qquad (1)$$

where y_{it} is an outcome (saving rate, saving, or log of expenditure), α_t stands for time effects (year 1998 is the omitted category), α_g denotes the cohort fixed effects (the unaffected cohort born 1937–1948 is the omitted category), and α_{tg} is the interaction between time dummies and cohort dummies. In order to allow for heterogeneity in responses, we compare the outcomes of the older cohort (those born from 1949 to 1953), the middle-aged cohort (those born from 1954 to 1968), and the younger cohort (those born after 1969) separately.

We focus on the estimated effects on the interaction terms between the time dummies and cohort dummies, α_{tg}. These interacted terms are relative to the cohort born between the years 1937 and 1948 (and unaffected directly by the reform), while holding any pre-reform cohort differences constant.

In order to increase the precision of our results, we also include a vector of controls, denoted x, which includes month-of-year dummies, a quadratic polynomial in age, gender, number of children, marital status, education, a dummy for whether the head of household's spouse is younger, occupation dummies, a dummy for working in the private sector, and a dummy for whether the household owns the house it lives in.[12] Since the analysis is conducted on the household level, all of the variables reflect the characteristics of the head of household.

In order to attribute a change in outcomes to the reform, our identifying assumption is that conditional on observables x, time effects α_t, and cohort fixed effects α_g, the time-by-cohort effects α_{tg} affect the outcomes because of the reform. Because we have two years of data preceding the reform, an indirect test of this assumption is to check for pre-intervention time-by-cohort effects. If the saving behavior of the cohorts affected by the reform differed already in the years before the reform, it calls into question whether our empirical strategy does indeed identify reform effects. As it turns out, we do not find evidence of preprogram time-by-cohort differences, suggesting that the difference-in-differences estimates can be interpreted as program effects.

[12] We do not include lifetime earnings on the right-hand side of Equation (1), as lifetime income is likely to be correlated with pension wealth and saving behavior. Instead, we use education and occupation dummies that serve as proxies for lifetime income.

3.4 Estimating the effect of pension wealth

The reduced-form difference-in-differences regressions have the advantage of being transparent, but they are not informative of the economic magnitude of the change in outcome. In the next part of the analysis we move beyond the simple difference-in-differences approach and impose more structure on our analysis. In order to identify the main parameter of interest of this study—the degree of substitutability between private saving and public pension wealth, we need to relate the change in saving behavior to the change in expected pension wealth. To do so, we estimate the following model:

$$sr_{it} = \theta\left(\frac{PW_{it}}{y_{it}}\right) + \alpha_t + \alpha_g + x_{it}\beta + \epsilon_{it}. \qquad (2)$$

sr is household *i*'s saving rate, and $\frac{PW_{it}}{y_{it}}$ equals a household i's expected pension wealth, divided by current household labor income. In our analysis, we correct $\frac{PW_{it}}{y_{it}}$ by a discrete-time version of "Gale's *Q*" (Gale 1998) adjustment factor; see Appendix B for details. The parameter of interest, the substitutability between private household saving and pension wealth, is given by the estimate θ.[13]

Since people who tend to save more may have higher pension wealth because of different lifetime income trajectories or because of an unobserved "taste for saving," simply regressing the saving rate on pension wealth may introduce a positive bias in the estimate of θ. At the same time, it is likely that pension wealth is measured with error and this measurement error will bias the OLS estimate of θ toward zero. Together, measurement error and unobserved heterogeneity are likely to bias the OLS estimate of θ in opposite directions;

[13] In addition to looking at saving rates, we also estimate models using the log of expenditure and saving (defined as available income minus total expenditure). When we use saving as the outcome variable, we do not normalize the expected pension wealth by household income. Instead, we estimate $saving_{it} = \theta PW_{it} + \alpha_t + \alpha_g + x_{it}\beta + \epsilon_{it}$, so that both pension wealth and saving are expressed in stocks as opposed to flows.

see Alessie, Angelini, and van Santen (2013) for a discussion of measurement error and omitted variable bias problems in the context of pension crowd-out studies.

We correct this error-in-variables problem and identify the effect of pension wealth on saving rate, by using instrumental variables techniques. We instrument pension wealth with the time-by-cohort interactions α_{tg}, which are now excluded from the structural Equation (2). (See Meyer [1995], p. 159, for a discussion on combining instrumental variables and difference-in-differences studies.) By doing so, our identifying assumption is that, after controlling for observables, time, and cohort fixed effects, the time-by-cohort interactions have no independent effect on household saving rate other than through pension wealth. In order to use an instrumental variable to correct for measurement error in pension wealth, the time-by-cohort interactions cannot be correlated with the measurement error in pension wealth. Since we do not expect that pension wealth mismeasurement will vary systematically by cohort and by year, we think that this is a reasonable assumption. Finally, in addition to being valid, our instrumental variable needs also to be relevant. This is turns out to be easily fulfilled, as pension wealth strongly varies over time-by-cohort interactions.

3.5 Threats to validity

External validity indicates the degree to which the conclusions from a study can be generalized to other populations and settings. Because the 1999 pension reform was a large reform on a nation-wide scale and due to its segmented implementation bears resemblance to a natural experiment, we believe that external validity of our study to be high.

At the same time, because our identifying variation stems from comparing households from various cohorts over time, this may present a potential threat to internal validity, i.e., the degree to which the 1999 pension reform is exogenous and the degree to which cohorts are comparable. For example, internal validity may be compromised if the reform was anticipated before 1997, thus leading households to adjust their behavior in advance. Another challenge is if the cohorts studied differ in unobserved ways before and after the reform, which would lead to a situation where there is correlation between the cohort-by-time dummies α_{tg} and the regression error term.

We think that the internal validity is reasonably high, as the particulars of who would and who would not be affected by the 1999 pension reform were not decided upon before the fall of 1998. In consequence, this left little time for the affected cohorts to adjust their spending before the reform.

Since in our study we are comparing the saving and spending behavior of older and younger households before and after the reform, it is important to net out the life-cycle effects on saving and expenditure. To do this, in all of our specifications, we condition the regressions on age polynomials and other demographics. However, if unobserved heterogeneity across the cohorts before and after the reform remains, this may weaken our ability to identify the effect of the reform.

4 Results

4.1 Difference-in-differences results

In Figure 1, we begin with a time series plot of average saving rate for the different cohorts across time. The saving rate is calculated as average household expenditure minus household income, divided by household income. Figure 1 shows the secular downward trend in Polish household net saving rates across the 1990s. The graph shows that relative to 1998, in 1999 the saving rate tends to go up more for the cohorts affected by the reform than for the cohort unaffected by the reform. Next, in order to make this point come across more clearly, we go beyond the simple time series plots and present the difference in saving rates of the affected cohorts relative to the unaffected cohort and relative to the pre-reform year 1998.

Figures 2–4 present the point estimates from multiyear difference-in-differences regressions of saving rate and saving. Presenting the results visually allows us to detect signs of existing pre-reform cohort-by-time trends. In order to be able to interpret the point estimates as effects of the reform on saving behavior, we should not see any significant differences in the household saving rate between the cohorts affected by the reform and the cohort unaffected by the reform in the years preceding the reform. This is a falsification-type test for the difference-in-differences model; see Angrist and Pischke (2009), pp. 237–241.

Figures 2–4 show point estimates from regression model (1) using saving rate, saving, and log expenditure as dependent variables. All figures are plotted, along with 95 percent confidence intervals, across the pre- and post-reform years. The omitted time period in these plots is the immediate pre-reform year, 1998, and the omitted cohort is the comparison cohort, those born between 1937 and 1948. The figures do not control for demographics—as we show in Appendix C, the results are very similar when demographic controls are included.

In order to see whether it takes time for households to adjust their saving behavior, we present the results for five years after the reform (1999–2003). Although the results have the expected sign—that is, saving (expenditure) tends to increase (decrease) over time for the affected cohorts in the post-reform years—the estimates are sometimes imprecise. We are unable to detect statistically significant pre-reform differences in saving behavior between the affected and unaffected cohorts. The outcome variables for the affected cohorts and the comparison cohort tend to move in parallel fashion, suggesting that the post-reform differences in outcome variables can be interpreted as an effect of the reform.

Since the BBGD in 1997 collects expenditure categories on a more aggregate level than in the later years 1998–2003, only a few subcategories are comparable across all of the years. One of the subcategories we observe consistently across 1997–2003 is food expenditure. Figure 5 presents the point estimates from multiyear difference-in-differences regressions of log of food and non-alcoholic beverage expenditure. Since food and non-alcoholic beverage consumption are typically considered necessities, we would not expect households to cut back much on food expenses due to the reform. Indeed, the results in Figure 5 suggest that, for middle-aged and younger cohorts, compared to the Figure 4, the reaction regarding food expenditure was smaller and mostly not statistically different from zero. For older cohorts, we observe a less than 10 percentage point decrease. We can only speculate why this is so, but perhaps this indicates that older households may reduce food expenditure by increasing food production and preparation at home; see Hurst (2008).

The magnitude of the estimated effects on saving rate in the post-reform years in Figure 2 is between 0 and 5 percentage points; this magnitude is, however, not very informative of the economic size of the effect. In order to ascertain the size of the response, we now turn to results from the model in Equation (2).

4.2 The effect of pension wealth on saving and expenditure

Table 5 shows the estimated crowd-out effect of public pension wealth on household saving rate, log expenditure, and saving in levels.

Columns (1) to (6) present the results using simple OLS—columns (1) to (3) use the unadjusted pension wealth, while columns (4) to (6) use the "Gale's *Q*-adjusted" pension wealth (see Appendix B for a discussion of the adjustment factor). Columns (7) to (12) instrument pension wealth with the time-by-cohort interaction, using 2SLS. This interaction consists of a post-reform dummy taking on a value of one for all of the post-reform years (and zero otherwise) and three dummies taking on a value of one if the household belongs to one of the three cohorts directly affected by the reform (and zero if it belongs to a cohort unaffected by the reform). Hence, the number of variables used to instrument pension wealth equals three: post-reform × oldest cohort, post-reform × middle-aged cohort, and post-reform × youngest cohort, making our model overidentified.

We do not report coefficients on other controls. These other variables include controls for month-of-year dummies, a quadratic polynomial in age, gender, number of children, marital status, education, a dummy for whether the head of household's spouse is younger, occupation dummies, a dummy for working in the private sector, a dummy for whether the household owns the house it lives in, a "post-reform" dummy, and three "affected cohort" dummies.

In the OLS specifications using the unadjusted pension wealth, the estimated crowd-out is small, and in column (2) it is of the unexpected sign: a marginal increase in pension wealth tends to decrease household spending. Columns (4) to (6) estimate the effect of pension wealth on outcomes using the *Q*-adjusted pension wealth. Since the *Q*-factor rescales pension wealth variable, the sign of the estimated θ-coefficient does not change. The *Q*-factor magnifies the estimated coefficient in absolute terms.

In contrast, the 2SLS estimates in columns (7) to (12) are both of the expected sign and are larger in absolute terms. These crowd-out estimates suggest that a marginal increase in pension wealth by 1 PLN reduces the household's private saving by about 0.24 PLN or, looking at columns (8) and (11), crowds-in between 0.21-0.24 PLN of household spending. Note that the absolute value of the crowd-out and the absolute value of the crowd-in are statistically not different from one another.

When we use saving in levels as the dependent variable, the estimate of crowd-out is greater in absolute value (about 0.57) than when using saving rate as the dependent variable. This is in part because our definition of saving (monthly available income minus monthly expenditure) is negatively skewed, which might make simple average effects less informative. In the last column of Table 5, we instead estimate an instrumental-variable (IV) quantile regression (QR) using saving as the dependent variable. We find that, at the median, the IV-QR estimate of crowd-out, θ, is about 0.36 in absolute value, which is much closer to the mean estimates of crowd-out in columns (7) and (8) in Table 5. Also, when using expenditure in levels, the crowd-in estimates are greater in absolute value than when we use the logarithm of expenditure as our dependent variable, where the latter is approximately normally distributed. [14] For this reason, we our preferred estimates are those using saving rate and the logarithm of expenditure.

The row labeled IV *F*-statistic shows the statistic from the *F*-test of relevance of the instrumental variable. We see no indication of a weak instrument problem. Below the *F*-statistic, we report *p*-values from a *J*-test for overidentification. For saving and saving rate, the *J*-test *p*-value is well above any conventional significance level; however, for log expenditure the test gives a low *p*-value. This may suggest heterogeneity in treatment effects across cohorts. We study this issue in the next subsection.

Since the model is overidentified, following Angrist and Pischke's (2009, pp. 205–216) suggestion, we also estimate a limited information maximum likelihood (LIML) model. The coefficients change slightly, which leads us to conclude that the degree of overidentification is not problematic.

4.3 Measurement error and unobserved taste for saving

The change in sign between the OLS and 2SLS estimates in Table 5 is consistent with measurement error in pension wealth combined with unobserved heterogeneity in the propensity to save. Recall that measurement error in pension wealth will bias the θ-coefficient toward zero, but it will not change the direction of the correlation between pension wealth and saving. On the other hand, if the unobserved propensity to save and pension wealth are positively correlated, then unobserved heterogeneity in the propensity to save may introduce an upward bias in the θ-estimate. Since measurement error and unobserved heterogeneity are

[14] These results are available upon request.

likely to bias the θ-coefficient in opposite directions, we can only infer the extent of the combined bias by observing how the OLS estimates differ from the 2SLS estimates.

The change in the results in Table 5 suggests a substantial unobserved variable bias in OLS estimates. This is not unexpected, as going from OLS to 2SLS, Attanasio and Rohwedder (2003) (see Tables 4 and 5 in their paper) report a similar change in the magnitude of their estimated crowd-out effect. Similarly, using OLS Engelhardt and Kumar (2009) find a positive θ-coefficient that equals 0.23, while when using 2SLS, the θ-coefficient changes to -0.53.

4.4 Analysis by subsamples

Economic theory suggests that those who are at a late point in their life cycle will react the strongest to the decrease in pension wealth, as they have a relatively short time horizon in which to adjust their behavior. Also previous research studying the effects of pension wealth on household saving often finds heterogeneous responses. In order to understand which cohorts are driving these results, Table 6 presents the 2SLS results separately for the three cohorts affected by the reform.

For each dependent variable (saving rate, log of expenditure, and saving), we "net out" the effect of demographics (including age and its square) by regressing each deponent variable on the vector of observables, x, and saving the residual.[15] Then, for each cohort affected by the reform, we estimate a separate 2SLS model using the comparison cohort and the affected cohort. In each column, we regress the residualized outcome variable on a "post-reform" dummy and an "affected cohort" dummy. The model is just-identified using the dummy "post-reform" interacted with a dummy for "affect cohort" as the excluded instrumental variable.

[15] The x vector consists of month-of-year dummies, a quadratic polynomial in age, gender, number of children, marital status, education, a dummy for whether the head of household's spouse is younger, occupation dummies, a dummy for working in the private sector, and a dummy for whether the household owns the house it lives in.

Table 6 suggests that the crowd-out is the biggest in absolute terms for the older and middle-aged cohorts. For the middle-aged cohort, the crowd-out estimate of saving rate and the crowd-in estimate for spending show that each additional PLN in pension wealth displaces about 0.45 PLN in private saving and crowds in 0.54 PLN in consumption. When using saving or saving rate as the dependent variable, the crowd-out estimate for the older cohort is less imprecise, but overall suggests large crowd-out. For the younger cohort, we observe that the crowd-out is smaller in absolute value: the crowd-out effect is about 0.29 when using saving rate as the dependent variable and the crowd-in is about 0.18. This is consistent with the interpretation that this cohort has a longer horizon over which to increase saving and reduce spending relative to the older cohort and therefore does not react as strongly.

Below each coefficient, the row labeled IV *F*-statistic shows the statistic from the *F*-test of relevance of the instrumental variable. Again, for each of the just-identified models, we do not see a weak instrument problem.

Previous research on financial literacy has found that households may not understand how pension systems work. We speculate that people with a college degree might be better informed about pension systems in general and aware how a pension reform might affect them. If so, we expect the crowd-out effect for highly educated households to be larger in absolute value; Bottazzi, Jappelli, and Padula (2006) find the largest crowd-out effects among the individuals informed about pension systems. Also, better educated households might also be “active” savers; see Chetty et al. (2014). We do not have direct measures of how financially literate a household is, so instead we estimate crowd-out separately for households where the head reports having tertiary (that is, college) education.

Theory also suggests that households that have accumulated enough buffer stock might not be as sensitive to pension wealth changes as those without assets. For the year we are considering, the BBGD does not include information about financial assets, but it does include data on whether the household owns the house or owns the condominium that lives.

Table 7 presents the 2SLS estimates of crowd-out for different types of households: the top panel shows the estimate of θ for households where the head has tertiary education and where the head of households has less than tertiary education. The lower panel shows the estimates for households that do and do not own their place of residence.

For households where the head has tertiary education, Table 7 shows a complete crowd-out when using saving and saving rate and a large crowd-in when using log expenditure as an outcome variable. These 2SLS estimates are larger in absolute value than the 2SLS estimates from Table 5. Turning to households where the head of household does not have a tertiary education, we see that the crowd-out equals about 0.14 using saving rate as an outcome and about 0.40 when using saving in levels as an outcome. This set of findings is similar to Bottazzi, Jappelli, and Padula's (2006) study of households informed about pension systems that finds a substantial crowd-out of about 0.8.

For household that do not own their place of residence, we find a larger point estimate of expenditure crowd-out than for households that do, but overall this set of estimates is less precise.

4.5 Sensitivity checks

In this section we present some sensitivity checks of our main results. First, we study the sensitivity of the θ-coefficient to different assumptions regarding the subjective discount factor, β. If households do not put much weight on the future, (i.e., β is low) then we expect the crowd-out estimates will be small and similar to the unadjusted OLS estimates in Table 5. In contrast, the crowd-out estimate ought to be larger in absolute value if households are more patient (i.e., if β is high).

In Figure 6, we plot the 2SLS estimates of θ as a function of β (for log expenditure we change the sign on θ to reflect the crowd-out, as opposed to crowd-in). We let β vary from 0.90 to 0.999 (note that Attanasio and Brugiavini [2003], Attanasio and Rohwedder [2003], and Bottazzi, Jappelli, and Padula [2006] set β = 0.98, while Gale [1998] sets it to β = 0.96).

Figure 6 shows that for as low a subjective discount factor as 0.90, the crowd-out estimates are very close to zero and are similar to the unadjusted OLS estimates from Table 5. Beyond β = 0.97 and as β is approaches one, the θ-coefficient becomes about -0.30 to -0.40. The estimated relation between θ and β is, hence, not linear. Generally, the crowd-out estimated using saving in levels as a dependent variable is larger in absolute value than when using the other dependent variables. This difference is the greatest when β is equal to 0.98.

Tables 8–10 present other robustness checks. In Table 8, we re-estimate the model from Table 5, but this time without the year 1997. We do so because the design of the BBGD in 1997 with respect to expenditure categories was different than in the years 1998–2003. By dropping the year 1997 and using 1998 as the only pre-reform year, we want to ensure that our interpretation of our main results is robust. By dropping year 1997 the size of our comparison group shrinks and this reduces the precision of our crowd-out estimates. The point estimates remain, however, very similar to the main results in Table 5, where θ is estimated to be around 0.20 using saving rate and log of expenditure as outcome variables.

In Table 9, we restrict our analysis sample to include only 18–60 year old males and 18–55 year old females. The results are similar to the main results in Table 5. In Table 10, we conduct the following three robustness checks. First, we pool together the older and middle-aged cohort to a big "transition" cohort and re-estimate equation (2); the reported 2SLS estimates are very similar to the 2SLS estimates in Table 5. Second, when calculating the pension wealth, we change the assumption regarding retirement age for men and for women: we assume that men retire at 55 years of age (instead of 60 as in Table 5) and women retire at

50 (instead of 55). Again, the results are very similar. Third, we change the assumption regarding women's employment path and re-calculate pension wealth under the assumption that women work and contribute to the pension system for 10 years (as opposed to 20 as in Table 5). Again, the results are similar to Table 5, although, for log expenditure, they are less precise.

5 Discussion

Our difference-in-differences results show that the reform had a causal effect on household saving and expenditure. The structural analysis estimates the overall public pension crowd-out to be between 0.21 and 0.24. For the older and middle-aged cohorts, we find a large and statistically significant crowd-out ranging between 0.45 and complete crowd-out. Interestingly, using a regression-discontinuity design to compare the 1948 cohort to the 1949 cohort to understand the effect of the 1999 pension reform on labor supply, Lindner and Morawski (2012) find no effect of the reform on labor supply. Their findings, together with ours, suggest that when faced with a reduction in future pension benefits, older households adjust their saving rather than labor supply.

The younger households display a statistically significant crowd-out of about one-third. We speculate that the smaller response among the younger households could be due to liquidity constraints, incomplete information, or uncertainty about how enduring the 1999 reform would be. Finally, we find that highly educated households—households we expect to be informed about the reform or households who are financially more able to adjust—exhibit a close to complete crowd-out.

Overall, our results indicate a sizeable, although, not complete public pension crowd-out. This suggests that in the absence of a public pension system, some households might not save for retirement and for these households, a public pension system may enhance welfare.

How do our estimates relate to the existing literature on public pension crowd-out? On one hand, when compared to studies that conclude that find a large crowd-out, our estimate of 0.21 to 0.24 is at the lower end of the range of existing estimates.[16] On the other hand, we argue that the estimates of crowd-out depend to a degree on the assumptions that researchers make about the subjective discount rate of households. For example, our main estimates of crowd-out assume that the annual subjective discount rate equals 2 percent. Assuming instead a lower discount rate, i.e., assuming that the household is more patient, yields a crowd-out closer to 0.40. Clearly, this difference is not trivial and carries implications for policy. In order for researchers to make recommendations about the impact of public pensions on saving, we need to know more about the subjective discount rate and its determinants.

6 Conclusions

This paper studies the large change in expected pension wealth induced by Poland's 1999 pension reform to estimate the effect public pensions have on household saving behavior. The implementation of the reform created quasi-experimental variation in pension wealth suitable for evaluating whether public pensions depress household saving. We find that public pensions do crowd out private saving; and this effect is the strongest for among highly educated and older households. For these groups, we find that public pension and private saving are close to perfect substitutes. However, for other groups, the crowd-out is less than one-for-one. For the young, building up a buffer stock is likely to be a question of age. For the other groups, such as those who are not college educated, the results, however, show that in a counterfactual world without a mandated public pension system, these households would have a lower standard of living than presently.

[16] Feldstein (1974): estimates the crowd-out to be between 0.30 and 0.50; Gale (1998) estimates it to be 0.50; Attanasio and Brugiavini (2003) report a range of effects between 0.30 and 0.70; Attanasio and Rohwedder (2003) report the crowd-out to be 0.65-0.75; Bottazzi, Jappelli, and Padula (2006) estimate it to be 0.7; and Aguila (2011) reports it to be 0.50.

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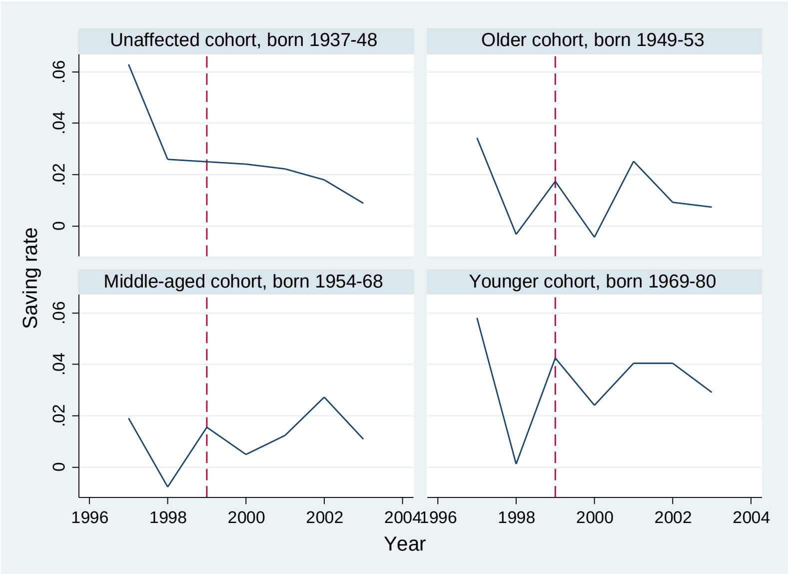
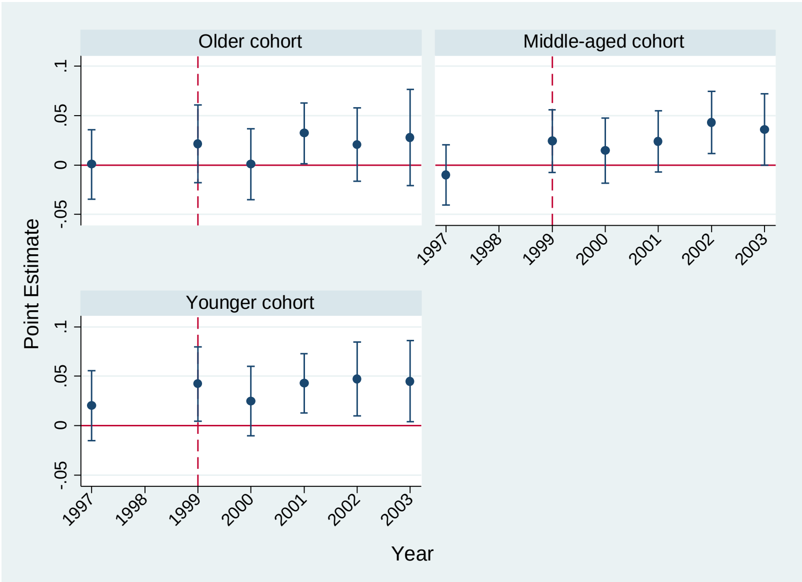
Results

Figure 1: Aggregate saving rate in the BBGD



NOTE: Author's calculations using the BBGD 1997–2003. Saving rate is defined as average expenditure minus average labor income divided by labor income. The dashed vertical line indicates the first year of the reform.

Figure 2: Estimated effect of the 1999 pension reform on saving rate, by cohort



NOTE: The figure above shows point estimates from a multiyear difference-in-differences regression of saving rate on three cohort dummies (older cohort, born 1949–1953; middle-aged cohort, born 1954–1968; and younger cohort, born 1969–1980), six year dummies, and cohort-by-year interaction terms. For each cohort, each panel presents the cohort-by-year interaction point estimate over time. The omitted categories are Year 1998 (the year before the reform) and the cohort born 1937–1948 (the cohort unaffected by the reform). The regression uses robust standard errors clustered by year of birth, and the figure presents 95 percent confidence intervals. The dashed vertical line indicates the first year of the reform.

Figure 3: Estimated effect of the 1999 pension reform on saving (in levels), by cohort

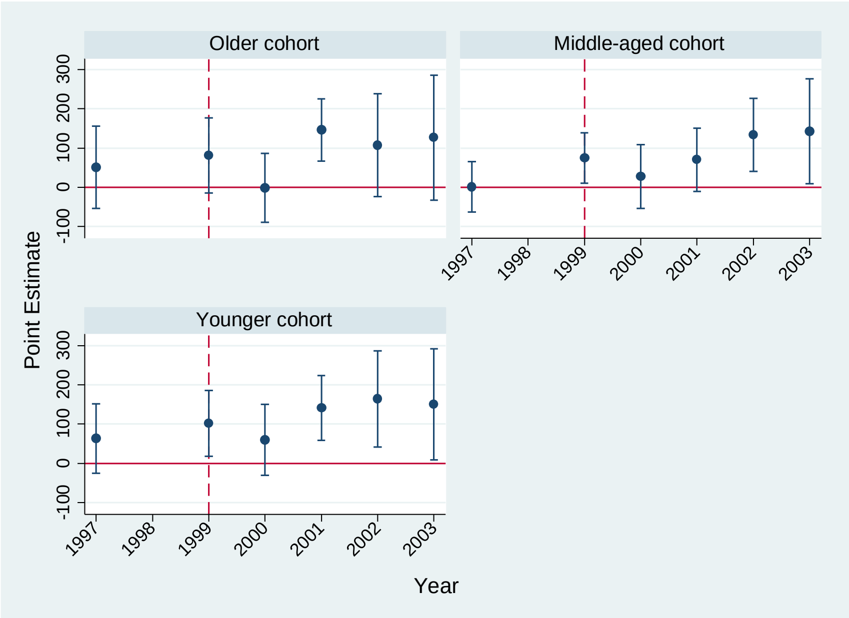
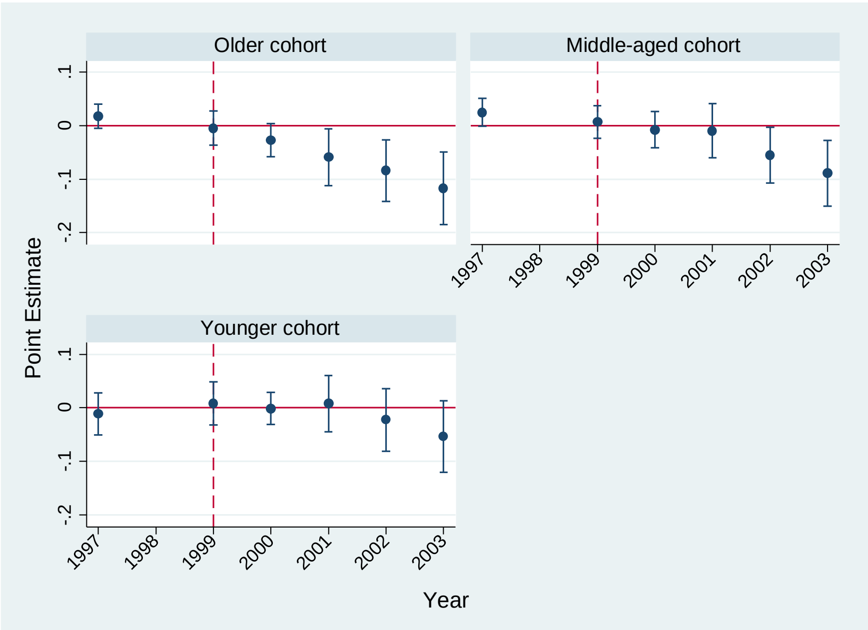
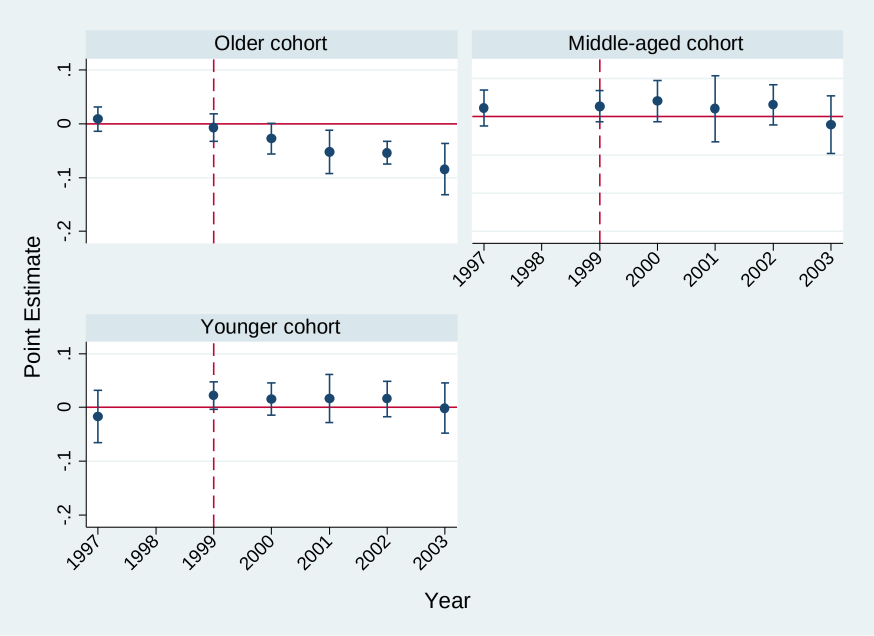


Figure 4: Estimated effect of the 1999 pension reform on log expenditure, by cohort



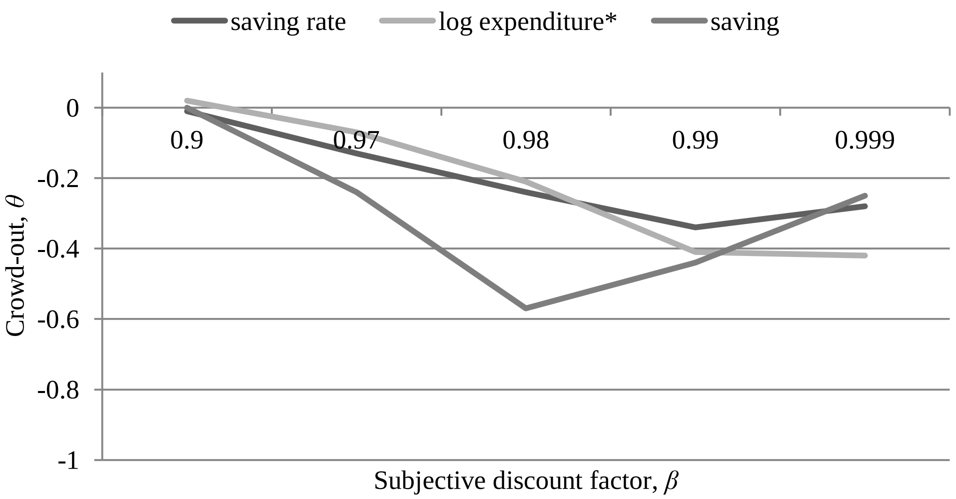
NOTE: Figures 3 and 4 show point estimates from a multiyear difference-in-differences regression of saving (top) and log expenditure (bottom) on three cohort dummies (older cohort, born 1949–1953; middle-aged cohort, born 1954–1968; and younger cohort, born 1969–1980), six year dummies, and cohort-by-year interaction terms. For each cohort, each panel presents the cohort-by-year interaction point estimate over time. The omitted categories are Year 1998 (the year before the reform) and the cohort born 1937–1948 (the cohort unaffected by the reform). The regression uses robust standard errors clustered by year of birth, and the figure presents 95 percent confidence intervals. The dashed vertical line indicates the first year of the reform.

Figure 5: Estimated effect of the 1999 pension reform on log food and non-alcoholic beverage expenditure, by cohort



NOTE: Figure 5 shows point estimates from a multiyear difference-in-differences regression of log food and non-alcoholic beverage expenditure on three cohort dummies (older cohort, born 1949–1953; middle-aged cohort, born 1954–1968; and younger cohort, born 1969–1980), six year dummies, and cohort-by-year interaction terms. For each cohort, each panel presents the cohort-by-year interaction point estimate over time. The omitted categories are Year 1998 (the year before the reform) and the cohort born 1937–1948 (the cohort unaffected by the reform). The regression uses robust standard errors clustered by year of birth, and the figure presents 95 percent confidence intervals. The dashed vertical line indicates the first year of the reform.

Figure 6: Robustness check: 2SLS crowd-out estimates of pension wealth as a function of the subjective discount factor



NOTE: Figure 6 shows point estimates from estimating pension crowd-out, θ, using various assumptions regarding the subjective discount factor, β.

*To ease comparability, the estimations using log expenditure as an outcome have the reverse sign in Figure 6 than in the regression output.

Table 1: Some main features of Poland's 1999 pension reform

	Pre-reform System, before 1999	Post-reform System, since 1999 (steady state)
Financing and contributions		
Financing	Pay-as-you-go, defined benefit.	Pay-as-you-go, notionally defined contribution (NDC) plan (1st tier) and a funded defined contribution (FDC) plan (2nd tier). NDC contribution is 12.22% of salary, FDC is 7.3%.[a]
Benefit calculation		
Benefit formula	Flat rate and an earnings related component.	Actuarially-adjusted and annuity-based on total contributions.
Pension base	Average of 10 best years out of 20 years prior to retirement.	Life-time earnings.
Min. years of contributions	20 for women, 25 for men.	20 for women, 25 for men.
Min. (and max.) pension benefit	35% of average national wage. (Max. earnings related benefit: 250% of average national wage).	20% of average national wage. (Max. contribution: 250% of average national wage.)
Retirement age		
Normal retirement age	Because of early retirement options, the effective retirement ages: 59 for men, 55 for women.	65 for men, 60 for women.
Early retirement provision	Available for most occupations.	Certain groups, women, and workers in the public sector, still have early retirement privileges.
Transition rules	Cohorts born before 1949 are fully in the pre-reform system, including the right to retire early as in the pre-reform system.	Cohorts born after 1969 are fully in the new system. Cohorts born between 1949 and 1968 could choose to only contribute to the NDC part.[c] Separate rules for the first five cohorts of women affected by the reform (born 1949-1953).
Replacement rate at 65 yrs. (men) and 60 yrs. (women)[d]	65-76% for men, 70% for women.	40-60% for men, 30-50% for women.

NOTE: Adapted from Chłoń, Góra, and Rutkowski (1999) and Chłoń-Domińczak (2002).
[a] Unisex life tables used in the NDC plan.
[b] Maximum benefit is set implicitly by the maximum contribution rate; see Chłoń-Domińczak and Strzelecki (2013).
[c] Majority chose the participate in NDC plan; see Chłoń-Domińczak (2002).
[d] Replacement rate defined as the ratio of first benefit to last salary. Calculations from Chłoń, Góra, and Rutkowski (1999), pp 36-37 and Chłoń-Domińczak (2002), p.128. Simulation assumes the statutory retirement age under both regimes: 60 for women, 65 for men.

Table 2: Between-cohort variation in the post-reform pension system

Cohorts	Born ≤ 31/12/1948	Born 1/1/1949 - 31/12/1968 (transitory cohorts)	Born ≥ 1/1/1969
Benefit formula	Pre-reform formula.	Post-reform formula with some exceptions.	Post-reform formula.
Exceptions to the benefit formula?	No	Separate rules for the first five cohorts (born 1949-1953) of women.[a] The 1949 cohort receives part of the benefit according to the old pension system formula (80%) and the rest according to the new formula (20%). The 1950 cohort receives a 70/30% mix. The 1951 cohort receives a 55/45% mix. The 1952 cohort receives a 35/65% mix. The 1953 cohort receives a 20/80% mix.	No
Early retirement provisions?	Yes	Yes, conditional on age and contribution requirement being fufilled before 31/12/2007.	No early retirement provisions. In the post-reform system men retire at age 65 and women at age 60.

NOTE: [a] From Chłoń, Góra, and Rutkowski (1999). p. 21.

Table 3: Descriptive statistics

Variable	Mean	Std. dev.	Median
Dependent variables			
Log available household income	7.74	0.47	7.74
Log household expenditure	7.65	0.51	7.64
Saving rate	0.02	0.51	0.09
Household expenditure (in 2005 PLN)	2,417	1,568	2,078
Available household income (in 2005 PLN)	2,577	1,264	2,308
Saving (in 2005 PLN)	160	1,292	189
Characteristics of head of household			
Age of head of household	40.4	9.11	
Head of household is a woman	0.32		
Marital status			
Unmarried	0.09		
Married	0.81		
Widowed	0.03		
Divorced or Separated	0.07		
Educational attainment			
Tertiary education	0.16		
Post-secondary non-tertiary education	0.03		
Upper secondary education	0.06		
Lower secondary vocational education	0.29		
Gymnasium	0.02		
Primary vocational education	0.37		
Primary education	0.08		
Pre-primary education	0.02		
Occupation*			
Legislators, senior officials and managers	0.09		
Professionals	0.11		
Technicians and associate professionals	0.12		
Clerks	0.09		
Service workers and shop sales workers	0.09		
Craft and related trades workers	0.27		
Plant and machine operators and assemblers	0.13		
Elementary occupations	0.09		
Armed forces	0.01		
Works in the private sector	0.52		
Belongs to the cohort unaffected directly by the reform	0.12		
Belongs to the older cohort affected by the reform	0.18		
Belongs to the middle-aged cohort affected by the reform	0.51		
Belongs to the younger cohort affected by the reform	0.20		
Characteristics of the household			
Labor income (in 2005 PLN)	3,063	1,799	2,527
Pension benefit (in 2005 PLN)	1,540	633	1,526
Pension wealth (in 2005 PLN)	13,100	6,853	11,384
Number of persons	3.53	1.34	
Number of children below the age of 15	0.88	1.00	
Age difference between spouses			

Spouse older than head of household	0.47
Spouse younger than head of household	0.33
No spouse	0.20
Household owns the place of residence	0.59
Year of observation	
Year is 1997	0.14
Year is 1998	0.14
Year is 1999	0.14
Year is 2000	0.16
Year is 2001	0.14
Year is 2002	0.14
Year is 2003	0.14
Number of observations	107,708

NOTE: "Saving" is defined as available household income minus total household expenditure. "Saving rate" is defned as saving divided by available household income.
[a]Occupation is presented here at the 1-digit level.

Table 4: Median replacement rate before and after the pension reform, by cohort

Cohort	Average age	(1) Pre-Reform	(2) Post-Reform	(3) Change (2)-(1)
Unaffected cohort (born 1937-1948)	55	0.60	0.60	0.00
Affected cohorts				
Older cohort (born 1949-1953)	49	0.61	0.39	-0.23
Middle-aged cohort (born 1954-1968)	40	0.62	0.40	-0.22
Younger cohort (born 1969-1980)	28	0.64	0.44	-0.20

SOURCE: Author's calculations using BBGD 1998 and 1999.
NOTE: Replacement rate is defined as the ratio of first gross pension benefit to last gross salary of the head of the household. Average age is the average age in the post-reform period.

Table 5: OLS, 2SLS, LIML, QR, and IV-QR crowd-out estimates of the effect of pension wealth on household saving rate, log of expenditure, and saving (in levels)

A.	OLS			QR
Variables	Saving rate (1)	Log Expenditure (2)	Saving (3)	Saving
Pension wealth	0	0	0.02***	0.01
	0.00	0.00	0.00	(0.00)

B.	OLS			QR
Variables	Saving rate (4)	Log Expenditure (5)	Saving (6)	Saving
Adjusted pension wealth	-0.01	-0.02	0.65***	0.38***
	(0.01)	(0.01)	(0.04)	(0.01)

C.	2SLS			IV-QR
Variables	Saving rate (7)	Log Expenditure (8)	Saving (9)	Saving
Adjusted pension wealth	-0.24**	0.21**	-0.57***	-0.36***
	(0.10)	(0.08)	(0.20)	(0.10)
IV *F*-statistic	44.52	44.52	997.1	
J-test *p*-value	0.303	0.000455	0.525	
Number of IV	3	3	3	

D.	LIML		
Variables	Saving rate (10)	Log Expenditure (11)	Saving (12)
Adjusted pension wealth	-0.24**	0.24***	-0.57***
	(0.10)	(0.09)	(0.20)
IV *F*-statistic	44.52	44.52	997.1
Anderson-Rubin test *p*-value	0.303	0.000481	0.525
Number of IV	3	3	3
Observations, *N*	107,708	107,708	107,708

NOTE: Robust standard errors are in parentheses (*** $p<0.01$, ** $p<0.05$, * $p<0.1$). In rows B, C, and D, pension wealth is adjusted by the *Q*-factor described in Appendix B. Regressions that use either saving rate or log expenditure as the dependent variable use pension wealth normalized by income. Omitted categories: born 1937–1948 and years 1997 and 1998. Other controls include month-of-year dummies, a quadratic polynomial in age, gender, number of children, marital status, education, a dummy for whether the head of household's spouse is younger, occupation dummies, a dummy for working in the private sector, a dummy for whether the household owns its place of residence, a "post-reform" dummy, and three cohort dummies (older cohort, born 1949–1953, middle-aged cohort, born 1954–1968; and younger cohort, born 1969–1980). The instrumental variables consist of interaction terms between the "post-reform" dummy and the three cohort dummies.

Table 6: Heterogeneity analysis: 2SLS crowd-out estimates of pension wealth, by cohort

A. Older cohort

Variables	Saving rate	Log Expenditure	Saving
Adjusted pension wealth	-1.09*	1.81***	-0.97**
	(0.61)	(0.60)	(0.49)
IV *F*-statistic(1, *N*-k)	64.81	64.81	419.3
Average age of the affected cohort		49.46	
Observations, *N*		31,989	

B. Middle-aged cohort

Variables	Saving rate	Log Expenditure	Saving
Adjusted pension wealth	-0.45***	0.54***	-0.78***
	(0.15)	(0.13)	(0.24)
IV *F*-statistic(1, *N*-k)	80.36	80.36	1001
Average age of the affected cohort		40.48	
Observations, *N*		67,482	

C. Younger cohort

Variables	Saving rate	Log Expenditure	Saving
Adjusted pension wealth	-0.29***	0.18**	-0.82***
	(0.08)	(0.07)	(0.21)
IV *F*-statistic(1, *N*-k)	111.5	111.5	746.2
Average age of the affected cohort		27.80	
Observations, *N*		34,359	

NOTE: Robust standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Pension wealth is adjusted by the Q-factor described in Appendix B. Each regression uses a dependent variable that has been residualized with respect to month-of-year dummies, a quadratic polynomial in age, gender, number of children, marital status, education, a dummy for whether the head of household's spouse is younger, occupation dummies, a dummy for working in the private sector, and a dummy for whether the household owns its place of residence. The regression controls a "post-reform" dummy (with born 1937–1948 being the omitted category) and an "affected cohort" dummy (with years 1997 and 1998 being omitted categories). The instrumental variable is defined as an interaction term between the "post-reform" dummy and the "affected cohort" dummy.

Table 7: Heterogeneity analysis: 2SLS crowd-out estimates of pension wealth for selected subsamples of households

	Head of household has tertiary education			**Head of household has less than tertiary education**		
Variables	Saving rate (1)	Log Expenditure (2)	Saving (3)	Saving rate (1)	Log Expenditure (2)	Saving (3)
Adjusted pension wealth	-1.70***	0.84**	-1.69**	-0.14	0.14*	-0.40**
	(0.52)	(0.42)	(0.70)	(0.10)	(0.08)	(0.19)
IV *F*-statistic	459.4	459.4	118.2	35.81	35.81	1057
J-test *p*-value	0.528	0.0760	0.838	0.743	0.00604	0.738
Observations, *N*	17,103	17,103	17,103	90,605	90,605	90,605
	Household owns the place of residence			**Household does not own the place of residence**		
Variables	Saving rate (7)	Log Expenditure (8)	Saving (9)	Saving rate (7)	Log Expenditure (8)	Saving (9)
Adjusted pension wealth	-0.16	0.35**	-0.56*	-0.25**	0.04	-0.46*
	(0.17)	(0.15)	(0.30)	(0.10)	(0.09)	(0.25)
IV *F*-statistic	9.906	9.906	533.1	535.5	535.5	473.7
J-test *p*-value	0.862	0.178	0.906	0.009	0.265	0.001
Observations, *N*	63,220	63,220	63,220	44,488	44,488	44,488
Number of IV	3	3	3	3	3	3

NOTE: Robust standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Pension wealth is adjusted by the Q-factor described in Appendix B. Big city is defined as a city with 500,000 or more inhabitants. Regressions that use either saving rate or log expenditure as the dependent variable use pension wealth normalized by income. Omitted categories: born 1937–1948 and year 1998. Same controls as in Table 5.The instrumental variables consist of interaction terms between the “post-reform” dummy and the three cohort dummies.

Table 8: Robustness check: crowd-out estimates of the pension wealth using only years 1998–2003

	A. OLS		
Variables	Saving rate (1)	Log Expenditure (2)	Saving (3)
Pension wealth	-0.01***	-0.02***	0.02***
	(0.00)	(0.00)	(0.00)
	B. OLS		
Variables	Saving rate (4)	Log Expenditure (5)	Saving (6)
Adjusted pension wealth	-0.20***	-0.58***	0.68***
	(0.03)	(0.09)	(0.04)
	C. 2SLS		
Variables	Saving rate (7)	Log Expenditure (8)	Saving (9)
Adjusted pension wealth	-0.22**	0.14	-0.59**
	(0.10)	(0.09)	(0.24)
IV *F*-statistic	1198	1198	650.8
J-test *p*-value	0.537	0.136	0.259
Number of IV	3	3	3
	D. LIML		
Variables	Saving rate (10)	Log Expenditure (11)	Saving (12)
Adjusted pension wealth	-0.22**	0.14	-0.59**
	(0.10)	(0.09)	(0.24)
IV *F*-statistic	1198	1198	650.8
Anderson-Rubin test *p*-value	0.537	0.136	0.259
Number of IV	3	3	3
Observations, *N*	92,203	92,203	92,203

NOTE: Robust standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1). In columns (4)–(12) pension wealth is adjusted by the Q-factor described in Appendix B. Regressions that use either saving rate or log expenditure as the dependent variable use pension wealth normalized by income. Omitted categories: born 1937–1948 and year 1998. Same controls as in Table 5. The instrumental variables consist of interaction terms between the “post-reform” dummy and the three cohort dummies.

Table 9: Robustness check: crowd-out estimates of pension wealth for a sample of 18–60 male heads of household and 18–55 female heads of households

	A. OLS		
Variables	Saving rate (1)	Log Expenditure (2)	Saving (3)
Pension wealth	-0.00	-0.00	0.02***
	(0.00)	(0.00)	(0.00)
	B. OLS		
Variables	Saving rate (4)	Log Expenditure (5)	Saving (6)
Adjusted pension wealth	-0.01	-0.02	0.65***
	(0.01)	(0.01)	(0.04)
	C. 2SLS		
Variables	Saving rate (7)	Log Expenditure (8)	Saving (9)
Adjusted pension wealth	-0.23**	0.21**	-0.54***
	(0.10)	(0.09)	(0.20)
IV *F*-statistic	41.99	41.99	955.5
J-test *p*-value	0.193	0.00466	0.325
Number of IV	3	3	3
	D. LIML		
Variables	Saving rate (10)	Log Expenditure (11)	Saving (12)
Adjusted pension wealth	-0.23**	0.23***	-0.54***
	(0.10)	(0.09)	(0.20)
IV *F*-statistic	41.99	41.99	955.5
Anderson-Rubin test *p*-value	0.194	0.00480	0.325
Number of IV	3	3	3
Observations, *N*	106,364	106,364	106,364

NOTE: Robust standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1). In columns (4)–(12) pension wealth is adjusted by the Q-factor described in Appendix B. Regressions that use either saving rate or log expenditure as the dependent variable use pension wealth normalized by income. Omitted categories: born 1937–1948 and years 1997 and 1998. Same controls as in Table 5. The instrumental variables consist of interaction terms between the "post-reform" dummy and the three cohort dummies.

Table 10: Robustness check: 2SLS crowd-out estimates of pension wealth using alternative specifications

Specification 1: older cohort and middle-aged cohort pooled together

Variables	Saving rate (1)	Log Expenditure (2)	Saving (3)
Adjusted pension wealth	-0.26**	0.16*	-0.67***
	(0.11)	(0.09)	(0.22)
IV *F*-statistic	53.54	53.54	1283
J-test *p*-value	0.144	3.70E-05	0.491
Number of IV	2	2	2

Specification 2: pension wealth calculation assumes that men retire at 55 years of age and women at 50 years

Variables	Saving rate (4)	Log Expenditure (5)	Saving (6)
Adjusted pension wealth	-0.20***	0.19***	-0.39***
	(0.08)	(0.07)	(0.13)
IV *F*-statistic	56.87	56.87	2486
J-test *p*-value	0.454	0.00142	0.823
Number of IV	3	3	3

Specification 3: pension wealth calculation assumes that women contribute to the pension system for 10 years

Variables	Saving rate (7)	Log Expenditure (8)	Saving (9)
Adjusted pension wealth	-0.21*	0.12	-0.43*
	(0.11)	(0.09)	(0.26)
IV *F*-statistic	36.46	36.46	596.5
J-test *p*-value	0.0987	3.34E-05	0.0369
Number of IV	3	3	3
Observations, *N*	107,708	107,708	107,708

NOTE: Robust standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Pension wealth is adjusted by the Q-factor described in Appendix B. Regressions that use either saving rate or log expenditure as the dependent variable use pension wealth normalized by income. Omitted categories: born 1937–1948 and years 1997 and 1998. Same controls as in Table 5.The instrumental variables consist of interaction terms between the "post-reform" dummy and the three cohort dummies.

Appendix A: Sample and variable definitions

In order to calculate future pension benefits, we make several assumptions. In this appendix, we discuss the main assumptions and the steps used in our calculations.

A.1 Sample selection

1. In order to reduce the influence of outliers, for each year of the BBGD, we trim the available household income below the 1st and above the 99th percentile.
2. In years 1998, 1999, 2000, 2003, and 2004, the BBGD contains information on year of birth. In other years we compute it as the difference between the year of the survey and the current age of the respondents.
3. We keep households whose head was born between 1937 and 1980; hence, the age of the head of household ranges between 19 and 62 years at the time of the reform. Each year, the sample is restricted to include 18- to 65-year-old heads of household.
4. We only include households for whom we observe the head of household’s occupation at the time of the survey. The information on occupations is necessary for the computation of lifetime labor income (see below).
5. We drop all the households where the head or the spouse works in farming, in the agricultural industry, or if the main household income comes from agriculture. We do this because farmers were outside of the regular pension system and because income and consumption information is not informative of the saving behavior of these households.
6. We drop households whose main source of income comes from pensions.
7. The final sample consists of 107,708 observations, with about 14,600-17,000 observations in each wave.

A.2 Lifetime labor income profiles

- Labor income in the BBGD is measured net of taxes and Social Security contributions. We use the SIMPL tax-benefit micro simulation model for Poland (see Bargain et al. 2007) to gross up the net labor incomes to include taxes and Social Security contributions. We define total labor income for each person as the sum of labor income from temporary and permanent employment in the private and public sector, and we express all values in 2005-constant prices.
- We forecast labor income (earnings) separately for heads of households and spouses using the 1997–2003 waves of the BBGD. For heads of household, we calculate the labor income profiles by estimating ordinary-least-square regressions of the income of the head of household on age, age squared, gender, marital status, interaction between gender and marital status, education level, occupation dummies, industry dummies, indicators for decade of birth, and year dummies. The latter is controlled for in order to allow cohort-specific intercepts to reflect differences in cohort productivity. We use the predicted earnings profile to forecast labor income for each head of household, given his (her) characteristics, from the age the head of household was at the time, 23 (25), until 60 (55).
- We model the earnings process separately for female and male spouses. For female spouses (77 percent of spouses are women) we forecast the earnings profiles using a Heckman selection correction. This is to include the large number of zero labor incomes of this group. The labor income of the spouse is regressed on age, age squared, education-level dummies, occupation dummies, industry dummies, decade-of-birth dummies, and year dummies. The “selection equation” for labor force participation (defined as labor earnings greater than zero) uses age, age squared, the number of children in the household who are 14 or younger, an interaction term

between age and the number of children, the level of education, and decade-of-birth dummies. For male spouses we estimate labor income profiles by ordinary-least-square regressions of the income of the male spouse on age, age squared, education level, occupation dummies, industry dummies, indicators for decade of birth, and year dummies. We use the predicted earnings profiles to forecast labor income for each spouse, given his (her) characteristics, from the age the spouse was at the time, 23 (25), until 60 (55).

- When computing the lifetime earning profiles, we assume that, except for age and its square, all the current characteristics are fixed and the profile changes with age and its square.

A.3 Pension benefit and pension wealth calucation

We calculate the future public pension benefits based on the entitlement that individuals will have acquired by the time they transition into old-age retirement *according to the legislation at the time of the observation*. Hence, the changes induced by the pension reform will reflect on the expected pension benefits in the years 1999–2003. In 1997 and 1998, the expected pension benefits are calculated according to the pre-reform legislation.

Pre-reform pension benefits

In the pre-reform system (see Chłoń-Domińczak [2002]), the old-age pension formula consisted of a common economy-wide component and an individual earnings-based component:

The common economy-wide component of the pension benefit consisted of 24 percent of the economy-wide average earnings. The individual earnings-based component was based on the individual's 10 best consecutive years of work out of the 20 years prior to retirement. This individual-based average was then multiplied by the number of years of work contributions and by 1.3 percent. In the pre-reform system, nonwork contributory years also counted (for example, years spent in college, in military service, and on maternity leave), and the individual-based average was multiplied by a factor of 0.7 percent. In the pre-reform system, there were also a minimum pension and a maximum. The individual earnings-based component was capped at a maximum of 2.5 times the economy-wide average earnings. The minimum pension benefit was set at 35 percent of the economy-wide average earnings.

Specifically, we compute the pre-reform pension benefit as

$$benefit = max\{0.35BA, 0.24BA + min\{CAE, 2.5BA\} \times (0.013C_W + 0.007C_{NW})\}.$$

- *BA* stands for the *basic amount*—that is, the average economy-wide earnings published by the Polish Statistical Office, GUS.
- *CAE* stands for *countable average earnings*—that is, the average of the 10 best years of work contributions out of the last 20 years.
- C_W stands for years of work contributions, which were at least 20 years for women and 25 for men.
- C_{NW} stands for years of nonwork contributions (for example, military service or maternity leave), which were limited to a maximum of one-third of the total number of years of contributions.

Assumptions for computing pre-reform benefits: We compute the 10 best years of each individual based on the forecast lifetime earnings profiles described previously. In our calculations, we assume that men and women contribute fully to the system, according to the pre-reform legislation: 25 years of work contributions for men and 20 for women. We also assume that men have three years of nonwork contributions (at the time, there was a two-year compulsory military service) and that women have five years of nonwork contributions. We assume that women retire at 55 and men at 60. Since the pre-reform minimum pension benefit was benchmarked to the economy-wide average earnings published by GUS, we assume that this economy-wide average grows by 2 percent annually in real terms.

<u>Post-reform pension benefits and initial capital</u>

The cohorts we study who have participated for at least one year in the pre-reform system were entitled to an "initial capital" sum that converted the contributions they had made so far into a starting capital sum, beginning as of 1999 for the reformed NDC plan; Chłoń-Domińczak (2002, p. 126) provides a detailed explanation of how the initial capital sum was computed.

The formula for the initial capital requires computing a correction factor, CF:

$$CF = min\left\{1, \sqrt{\frac{age\ in\ 1998 - 18}{retirement\ age - 18} \times \frac{years\ of\ contributions\ in\ 1998}{required\ years\ of\ contributions}}\right\},$$

where the formula set the *retirement age* to 60 for women and 65 for men and the required *years of contributions* to 20 years for women and 25 for men. The initial capital is computed as $0.24 \times BA \times CF \times G_{62}$, where G_{62} is the unisex life expectancy for a 62-year-old in 1998 and *BA* is the basic amount, defined above.

Assumptions for computing initial capital: In our calculations, we compute years of contributions as of the end of 1998 as the age of an individual in 1998 minus 23 years (minus 25 for women, to account for spotty labor force participation). We compute G_{62} as a simple average of 62-year-old men and women's life expectancy in 1998.

For the years after the 1999 reform until the year of retirement, we calculate contributions as 19.52 percent of an individual's earnings. The post-reform pension benefit equals

$$benefit = \frac{initial\ capital + 0.1952 \sum_{t=1999}^{year\ of\ retirement} earnings_t}{unisex\ life\ expectancy\ at\ retirement}.$$

In all our computations, we express all values in 2005-constant prices and assume that the real pension benefits will grow by 4 percent annually.[1]

Assumptions for computing post-reform benefits: We assume that men contribute continuously until they retire at 60 years of age and that women contribute continuously until they retire at 55 years of age. The pension benefit is computed as the sum of initial capital and the contributions of an individual's earnings divided by the remaining unisex life expectancy at the statutory age of retirement.

<u>Pension wealth</u>

The general formula for computing pension wealth is the following:

$$PW(i) = \sum_{\tau=ret.age}^{max.age} \frac{pr_{\tau|age(i)} \times benefit(i) \times (1+g)^{\tau-ret.age}}{(1+r)^{\tau-age(i)}}.$$

- *PW(i):* pension wealth of an individual *i*.
- *ret.age*: retirement age, set at 65 for men and 60 for women.
- *max. age*: maximum attainable age, set at 100 years (the end of the life table).
- $pr_{\tau|age(i)}$: the probability that someone aged *age*(*i*) will be alive at age τ = *ret.age*, …, *max.age*.[2]
- *benefit(i)*: pension benefit of an individual *i*, computed as described above.
- *g*: real growth rate of pension benefits, set at 0.04.
- *r*: real interest rate, set at 0.02.

To compute pension wealth, we make the following assumptions:

[1] By doing so, we implicitly assume that the return on the fully funded pension (7.3 percent contribution to FDC) in the post-reform system has the same return as the notionally defined contribution pension (12.22 percent contribution to the NDC pension), which is financed on a pay-as-you-go basis by current contributions. This is because we 1) do not have the data on households' FDC portfolio choices and 2) ex-post, the returns on the FDC plans have performed below initial projections. This is in part because the funds have invested a large share of the portfolios in government bonds, in effect making the FDC pension plan a liability in a similar way as the pay-as-you-go NDC plan.

[2] We use separate male and female 1999 life tables from the Polish Statistical Office, GUS.

- When calculating pension wealth, we adjust the future stream of pension benefits by using separate male and female survival probabilities from the 1999 Polish life tables. The maximum age is also taken from the life tables and is set to 100 years for everyone.
- We compute the pension benefits separately for the head of the household and the spouse and then take their sum.
- The actuarially adjusted stream of future pension benefits of the head of the household and the spouse is discounted back to the current age of the head of the household.
- Finally, we divide the expected pension wealth by the estimated current labor income of the household.

Appendix B: Adjustment Factor

Gale (1998, 2005) points out that a simple comparison of saving rates at one point in time with a stream of benefits occurring in the future will bias the crowd-out estimates toward zero. Gale (1998) proposes the so-called "Gale's *Q*" adjustment factor (see Bottazzi, Jappelli, and Padula [2006], Engelhardt and Kumar [2011], and Alessie, Angelini, and van Santen [2013]) that is a function of the subjective discount rate, the point in the life cycle when an individual is observed, and the point in the life cycle when a change in the expected pension benefits takes place.

For each individual, we multiply pension wealth by a discrete-time version of the adjustment factor (see Attanasio and Brugiavini [2003], Attanasio and Rohwedder [2003], Feng, He, and Sato [2011], and Alessie, Angelini, and van Santen [2013]). In order to develop intuition for this adjustment factor, we first present a simple version of the finite-horizon optimization problem from Attanasio and Rohwedder (2003). Suppose each individual has an initial asset equal to a_1 that, for simplicity's sake, does not grow or depreciate. In each period, the individual has to decide how much to consume and how much to save for the future. Assuming log utility, the problem can be expressed as:

$$\max_{\{c_t,\, a_{t+1}\}_{t=1}^{T}} \sum_{t=1}^{T} \beta^{t-1} \log c_t \;\; s.t.\, a_{t+1} = a_t - c_t \; for \; t = 1, \dots, T,$$

where $a_1 > 0$ is given, a_{T+1} is greater than or equal to zero, c denotes consumption, and β is the subjective discount factor.

Suppose that, as in Attanasio and Rohwedder (2003), there are four time periods, $T = 4$. The optimal consumption policy is:

$$c_1 = \frac{a_1}{1+\beta+\beta^2+\beta^3},$$

$$c_2 = \frac{\beta a_1}{1+\beta+\beta^2+\beta^3} = \frac{a_2}{1+\beta+\beta^2},$$

$$c_3 = \frac{\beta^2 a_1}{1+\beta+\beta^2+\beta^3} = \frac{\beta a_2}{1+\beta+\beta^2} = \frac{a_3}{1+\beta},$$

$$c_4 = \frac{\beta^3 a_1}{1+\beta+\beta^2+\beta^3} = \frac{\beta^2 a_2}{1+\beta+\beta^2} = \frac{\beta a_3}{1+\beta} = a_4 .$$

For $t > 1$ we can derive more than one expression. The first expression presented is the optimal consumption for period $t = 1, 2, 3, 4$, as seen from period 1. The second and third expressions show the optimal consumption for period $t = 2, 3, 4$, as seen from periods 2 and 3, and so on.

If there has been no unexpected change in the periods following $t = 1$, then, for each c_t, the second and third expressions are equal to the first one.[3] However, if an unexpected change does occur in the periods following $t = 1$, then the consumer has to reoptimize her consumption given the level of assets she has carried over from the previous period. For example, if the unexpected change has occurred at the end of period 1, then in period 2, the

[3] This can be verified by plugging in the solution for c_t from the first expression into the dynamic budget constraint $a_{t+1} = a_t - c_t$.

level of assets available to the consumer a_2 is given and cannot be changed retrospectively. Hence, the level of c_2 given by the second expression, $\frac{a_2}{1+\beta+\beta^2}$, will not equal the level of c_2 from the first expression, $\frac{\beta a_1}{1+\beta+\beta^2+\beta^3}$.

The take-away from this simple model is the illustration that the consumption and saving response following an unexpected change in wealth depends not only on the magnitude of the change in expected wealth, but also on subjective time preferences (β), the remaining planning horizon (T minus the time at which the shock occurs), and the age of the consumer (t). The pattern for the Q adjustment factor if there is no shock can be generalized to T periods using the formula for the sum of a finite geometric series:

$$Q(t;tr=0)=\frac{\beta^{t-1}}{1+\beta+\beta^2+\cdots+\beta^{T-1}}=\frac{1-\beta}{1-\beta^T}\beta^{t-1},$$

where t denotes the point in a consumer's life cycle. The pattern for this adjustment factor following a shock to wealth in any period $tr > t$ can be expressed as:

$$Q(t,tr)=\frac{\beta^{t-tr-1}}{1+\beta+\beta^2+\cdots+\beta^{T-tr-1}}=\frac{1-\beta}{1-\beta^{T-tr}}\beta^{t-tr-1}\ for\ tr>t,$$

where tr denotes the point in a consumer's life cycle when the unexpected change occurs.

In practical terms, we must adjust our measure of pension wealth to reflect that we observe individuals at various ages (hence with different remaining life expectancies) who also experience the reform at various points in their life cycle. For each observation i, we multiply pension wealth by the adjustment factor:

$$\frac{(1-\beta)\beta^{age(i)-start\ work}}{1-\beta^{life\ exp(i)-age(i)}},$$

where *age*(*i*) is the current age of the head of household, *start work* is the age at which the head of household starts working, *life exp(i)* is calculated as the sum of current age and remaining gender-specific life expectancy, and β is set to equal 0.98.[4]

Following the reform, the affected households need to reoptimize their consumption and saving behavior. The adjustment factor must take into account the remaining life expectancy after the reform, their current age, and when during their life cycle the reform occurred:

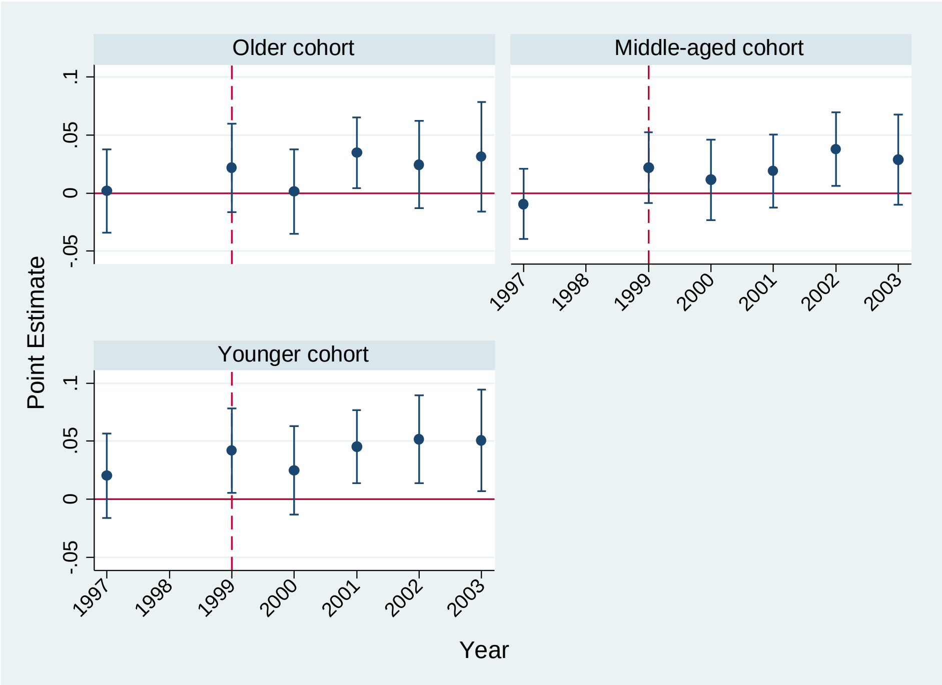
$$\frac{(1-\beta)\beta^{age(i)-age\ at\ reform(i)}}{1-\beta^{life\ exp(i)-age\ at\ reform(i)}},$$

where $age\ at\ reform(i)$ is the head of household's age at the end of 1998 + 1, as we assume that the reform occurs at the end of 1998 and that 1999 is the first year of the reform. We apply the factor $\frac{(1-\beta)\beta^{age(i)-age\ at\ reform(i)}}{1-\beta^{life\ exp(i)-age\ at\ reform(i)}}$ to all the households affected by the reform and $\frac{(1-\beta)\beta^{age(i)-start\ work}}{1-\beta^{life\ exp(i)-age(i)}}$ to the households unaffected by the reform and households in the pre-reform period.

[4] As in Feng, He, and Sato (2011), we treat the postretirement period as several time periods.

Appendix C: Other Results

Figure A.1: Estimated effect of the 1999 pension reform on saving rate, by cohort



NOTE: The figure above shows point estimates from a multiyear difference-in-differences regression of the saving rate on three cohort dummies (older cohort, born 1949–1953; middle-aged cohort, born 1954–1968; and younger cohort, born 1969–1980), six year dummies, cohort-by-year interaction terms, and controls from Table 4. For each cohort, each panel presents the cohort-by-year interaction point estimate over time. The omitted categories are Year 1998 (the year before the reform) and the cohort born 1937–1948 (the cohort unaffected by the reform). The regression uses robust standard errors clustered by year of birth, and the figure presents 95 percent confidence intervals. The dashed vertical line indicates the first year of the reform.

Figure A.2: Estimated effect of the 1999 pension reform on saving (in levels), by cohort

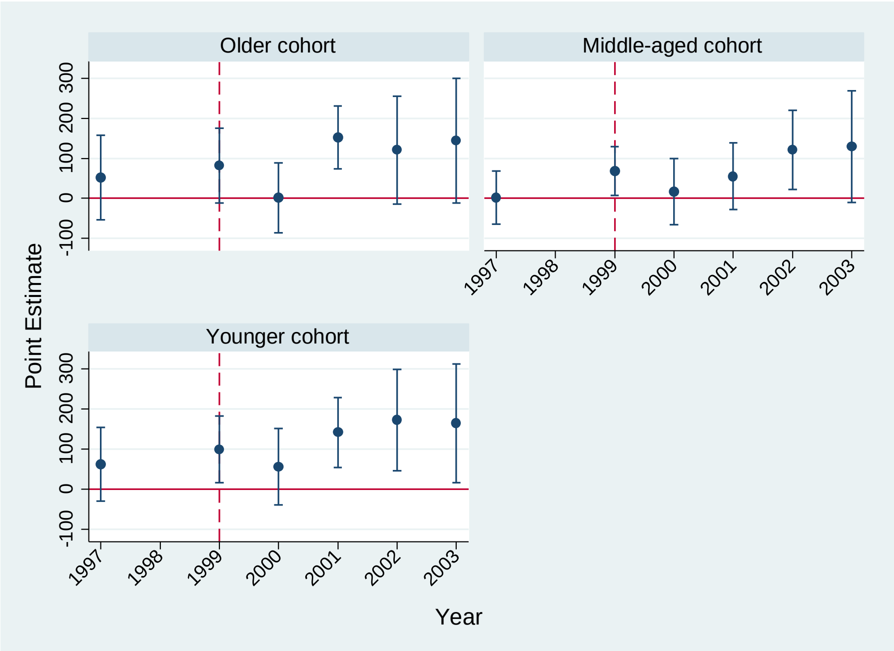
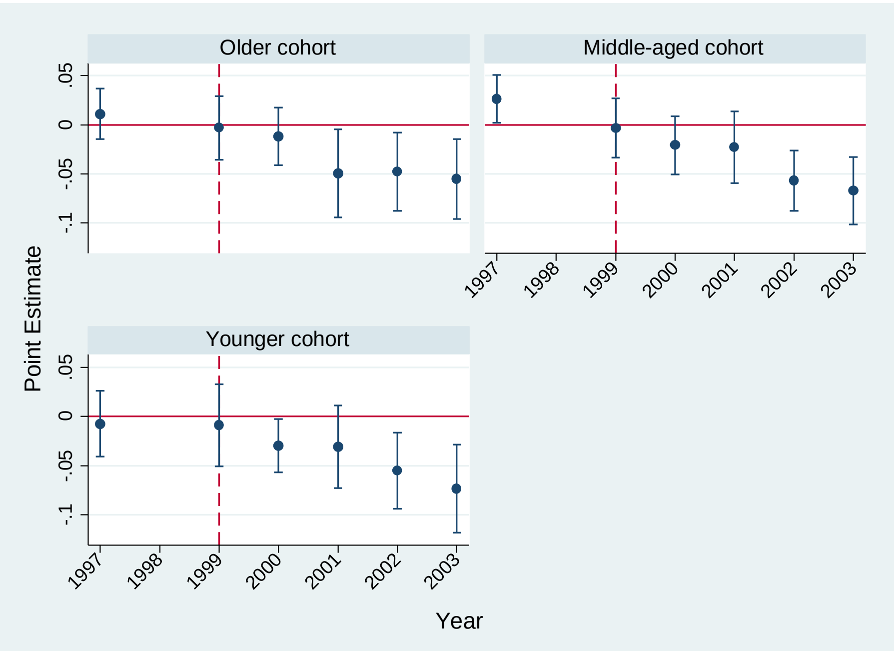


Figure A.3: Estimated effect of the 1999 pension reform on log expenditure, by cohort



NOTE: The figure above shows point estimates from a multiyear difference-in-differences regression of saving (top) and log expenditure (bottom) on three cohort dummies (older cohort, born 1949–1953; middle-aged cohort, born 1954–1968; and younger cohort, born 1969–1980), six year dummies, cohort-by-year interaction terms, and controls from Table 4. For each cohort, each panel presents the cohort-by-year interaction point estimate over time. The omitted categories are Year 1998 (the year before the reform) and the cohort born 1937–1948 (the cohort unaffected by the reform). The regression uses robust standard errors clustered by year of birth, and the figure presents 95 percent confidence intervals. The dashed vertical line indicates the first year of the reform.