The Effect of Public Pension Wealth on Saving and Expenditure

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By Marta Lachowska and Michał Myck*

This paper examines the degree of substitution between public pension wealth and private saving by studying Poland's 1999 pension reform. The analysis identifies the effect of pension wealth on private saving using cohort-by-time variation in pension wealth induced by the reform. The estimates, which are based on the 1997–2003 Polish Household Budget Surveys, show that 1 Polish zloty (PLN) less of pension wealth increases household saving by 0.3 PLN. Among highly educated households, pension wealth and private saving appear to be close substitutes. (JEL D14, E21, H55, I38)

In 1999, Poland reformed its public pension system so as to ensure its solvency, altering the benefit formula and introducing a defined-contribution system. This paper examines the 1999 reform to estimate the response of private saving to changes in public pension wealth—that is, to identify the extent to which private saving substitutes for mandatory public pension wealth—using the fact that the reform had a differential impact on individuals depending on their year of birth. Individuals who were older than 50 years at the time of the reform were not directly affected by the reform and were allowed to stay in the pre-reform system with high benefit-to-salary replacement rates. Individuals who were 50 years old or younger at the time of the reform were to receive pension benefits computed according to a less generous post-reform pension formula. The reform therefore created large variation among people of similar ages in expected public pension wealth, providing a setting similar to a natural experiment.

Longer life expectancy and falling fertility have led to reform of many countries' public pension systems, and understanding how such reforms are likely to affect private saving is important because resources accumulated as private savings affect investment in capital, economic growth, and living standards. Accordingly, the degree of substitution between public pension wealth and private saving is a key aspect of debates over public pension reform.

We begin by estimating a set of difference-in-difference regressions, where we calculate the change in household saving rates and expenditures before and after the reform for the affected and unaffected cohorts. Next, in order to estimate the degree of substitution between private saving and public pension wealth, we calculate expected pension wealth under the pre-reform and post-reform legislation for every household and relate this variable to the observed household rate of saving. Because pension wealth is likely to be endogenous with respect to saving, we instrument pension wealth using an interaction indicator for whether a household head belongs to a cohort affected by the reform and whether the household is observed after the reform. Instrumenting in this way allows us to purge variation in pension wealth due to unobserved differences among households in tastes for saving, and hence to identify an exogenous source of variation in pension wealth.

The quasi-experimental variation in pension wealth is useful because the substitutability between private saving and public pension wealth is theoretically ambiguous. The canonical life-cycle model predicts perfect substitution between private saving and pension wealth; however, Feldstein (1974) suggests that, if pension systems induce people to retire earlier and extend the period during which they consume out of accumulated assets, a public pension system could in fact increase private saving. It seems safe to conclude that the illiquid nature of public pension wealth complicates any sharp theoretical predictions about its relationship with private saving.

The empirical literature on substitution between public pension wealth and private saving has also been inconclusive. Feldstein (1974) finds that an additional $1.00 of Social Security wealth depresses private saving by up to $0.50—a degree of substitution between private saving and Social Security wealth of 0.5. Feldstein and Pellechio (1979); Bernheim (1987); and Alessie, Kapteyn, and Klijn (1997) also find a high degree of substitution, typically 0.5 or more. Other research finds less substitution (King and Dicks-Mireaux 1982; Hubbard 1986; Hurd, Michaud, and Rohwedder 2012), while Pozo and Woodbury (1986) find evidence that Social Security increases private saving.[1]

Early differences over the estimated degree of substitution between private saving and public pensions were due largely to different empirical strategies, but recent papers have found varying degrees of substitution despite similar approaches to identification. A key difficulty lies in how to account for unobserved traits that influence both saving decisions and public pension wealth (see Gale (1998) for a discussion of other econometric biases in this literature). Much of the recent literature

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[1] In addition to the debate over substitution between public pensions and private saving, a related literature estimates whether private household saving is reduced by private pensions (e.g., Cagan 1965, Katona 1965, Munnell 1976, Engelhardt and Kumar 2011, and Yang 2014) and by tax-deferred pension accounts (e.g., Venti and Wise 1990, Gale and Scholz 1994, and Chetty et al. 2014). Bernheim (2002) and Gale (2005) review this literature.

has searched for exogenous shifts in public pension wealth as a source of identification. Attanasio and Brugiavini (2003); Attanasio and Rohwedder (2003); Bottazzi, Jappelli, and Padula (2006); Aguila (2011); Banerjee (2010); and Feng, He, and Sato (2011) use differential impacts across groups and time created by pension reform as a source of variation in pension wealth and apply variants of the difference-in-difference approach. However, whereas the first four papers find a degree of substitution ranging between 0.50 and 0.75, Feng, He, and Sato (2011) report a modest relationship of less than 0.20. (Table 7, later in the paper, summarizes the findings of these studies.) Finally, an influential paper by Chetty et al. (2014) uses detailed administrative data to study the effects of introducing government-mandated automatic pension contributions in Denmark and finds evidence of no substitution between private saving and public pensions.

Thus, despite relying on convincing identification strategies, the empirical literature remains divided about the degree of substitution between public pensions and private saving. It is therefore important to complement the existing literature with analysis from other settings and different institutional arrangements.

The main results reported here show that 1 Polish zloty (PLN) less of public pension wealth increases household saving by about 0.30 PLN, on average—that is, the degree of substitution between public pension wealth and private saving is estimated to equal about 0.3. The degree of substitution is less for less-educated households than for those with college education (for whom public pension wealth and private saving appear to be close substitutes). We present several sensitivity checks, in which we vary assumptions about households' subjective discount rate and projections of future earnings and pension wealth, and use somewhat different samples. The results are robust to these checks.

The rest of the paper is organized as follows. Section I provides background on Poland's public pension system in the years before and after the reform. Section II describes the data and variables from the Polish Household Budget Surveys and discusses the empirical strategy used in the analysis. Section III describes the results, and Section IV discusses the findings and concludes. We relegate detailed variable definitions and the discussion of criteria used to construct the analysis sample to online Appendices A and B.

I. Poland's 1999 Pension Reform

A. *Overview*[2]

In the early 1990s, Poland had a relatively generous public pension system financed on a pay-as-you-go basis. However, the combination of use of early retirement options, increased life expectancy, and low fertility raised questions about the system's fiscal long-term solvency. In order to help finance the system, the contribution rate was successively raised in the early 1990s, but it soon became apparent that these increases provided only a temporary solution and that Poland's public pension

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[2]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.

system needed a major reform. The initial steps toward reform were formulated in 1994, and in the following years, negotiations were held regarding the choice of a funding system and transition rules.

Following the initial phase, the plan to reform the pension system accelerated in the fall of 1997. Although it was expected that a pension reform would take place in some form, the details of who would be affected and to what extent were still a matter of uncertainty in 1998. The final details were approved in October 1998, and the new pension system took effect on January 1, 1999. As Chłoń-Domińczak (2002) points out, an important factor driving the haste in reforming the pension system was a supportive public, which perceived the old pension arrangements as a carry-over from communist days.

Table 1 highlights the main differences between the pre-reform pension system (in column 1) and the post-reform pension system (in column 2). Like many pension reforms, the Polish reform was implemented gradually so as to give individuals time to adjust. Column 2 in Table 1 describes the features of the post-reform system once it reaches a "steady state."

B. *Impact of the Reform across Cohorts*

The gradual implementation of the reform affected individuals differently depending on their year of birth, which allows us to study the impact of the reform by comparing a cohort unaffected directly by the reform with a "treated" cohort affected by the reform.

We define the *comparison group* as consisting of households whose head was born between 1939 and 1948 and thus was older than 50 years at the time of the reform (and hence unaffected by the reform). The *treated group* consists of households whose head was born between 1949 and 1958 and thus was 50 years old or younger at the time of the reform. Hence, the comparison and treatment groups consist of households whose head was born within ten years before or ten years after January 1, 1949, the date separating the groups. Later in the paper, we conduct a robustness check in which we limit the estimation sample to only include those born between 1944 and 1953—i.e., within five years before or five years after 1949.

Figure 1 shows how the treated group was affected by the reform. It plots the average household gross replacement rate by birth year of the household head. The replacement rate is defined as the ratio of the first gross monthly pension benefit to the last preretirement gross monthly earnings. For each birth year, the line with the black circles shows the replacement rate according to the post-reform legislation. Hence, those born prior to 1949 were not directly affected by the reform and could expect a gross replacement rate of about 60 percent both before and after the reform. However, those born in 1949 or later will receive a less generous pension and hence have a lower replacement rate.[3]

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[3] All calculations in Figure 1 hold the retirement age the same for both scenarios; see online Appendix A for details. The percentage point drop reported in the figure corresponds closely to the net replacement rate drop reported in chart 8 of Chłoń-Domińczak (2002, 128).

_{TABLE 1—KEY FEATURES OF POLAND'S PUBLIC PENSION SYSTEM BEFORE AND AFTER THE 1999 REFORM}

	Pre-reform system (1)	Post-reform system (steady state) (2)
Financing and contributions		
Financing	Pay-as-you-go, defined benefit	Pay-as-you-go, notionally defined contribution (NDC) plan (first tier) and a funded defined contribution (FDC) plan (second tier). NDC contribution is 12.22% of salary; FDC is 7.3%[a]
Benefit calculation		
Benefit formula	Flat rate plus 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	Lifetime earnings
Minimum years of contributions	20 for women, 25 for men	20 for women, 25 for men
Minimum (and maximum) pension benefit[b]	35% of average national wage (maximum earnings-related benefit: 250% of average national wage)	20% of average national wage (maximum 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 fully covered by the pre-reform system, including the right to retire early as in the pre-reform system	Cohorts born after 1969 fully covered by the new system. Cohorts born 1949–1968 could choose to make only NDC contributions.[c] Separate rules for the first five cohorts of women affected by the reform (born 1949–1953)
Replacement rate at 65 years (men) and 60 years (women)[d]	65–76% for men, 70% for women	40–60% for men, 30–50% for women

[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 to participate in the NDC plan; see Chłoń-Domińczak (2002).
[d]Replacement rate defined as the ratio of first annual benefit to last annual salary. Calculations from Chłoń, Góra, and Rutkowski (1999, 36–7) and Chłoń-Domińczak (2002, 128). Simulation assumes the statutory retirement age under both regimes: 60 for women, 65 for men.

For example, those born in 1957 are affected by the reform and can expect to receive a gross replacement rate equal to 40 percent. The line with the hollow circles denotes the counterfactual average replacement rate had the pre-reform system continued unchanged. In this counterfactual world (without the pension reform), those born in 1957 would expect to have a gross replacement rate equal to about 60 percent. Hence, those born in 1957 experienced a drop of about 20 percentage points in their expected replacement rate. By any standard, this is a large reduction.
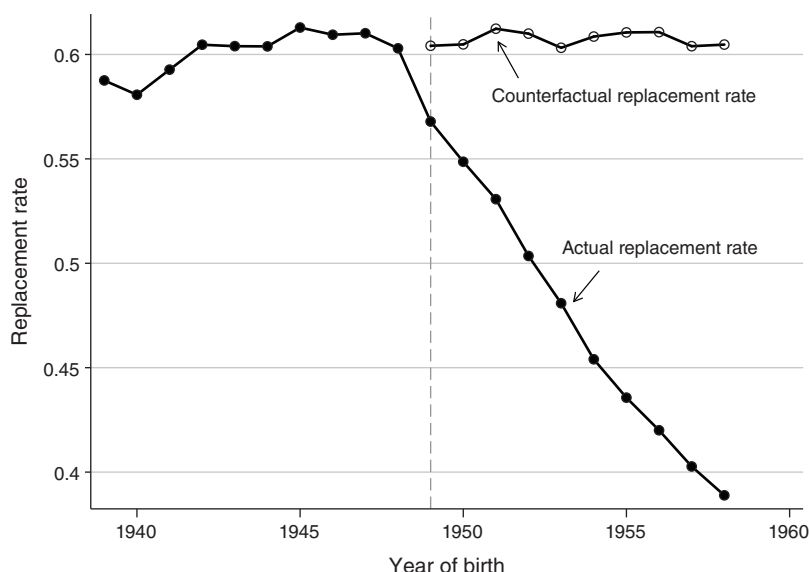
FIGURE 1. SIMULATED MEAN HOUSEHOLD GROSS PENSION REPLACEMENT RATE, BY YEAR OF BIRTH, 1939–1958

Notes: The replacement rate is defined as the ratio of the first monthly gross pension benefit to the last preretirement gross monthly earnings; see online Appendix A for details on how pension benefits are calculated. The line with the black circles denotes the actual mean replacement rate by the household head's birth year. The line with the hollow circles denotes the counterfactual pre-reform mean replacement rate (i.e., the replacement rate that would have applied in the absence of the reform) for the cohorts affected by the reform by the household head's birth year. The dashed vertical line indicates 1949, the first birth-year cohort affected by the reform.

II. Data and Methods

A. *Data*

The data we use come from the Polish Household Budget Surveys (Badanie Budżetów Gospodarstw Domowych, or BBGD), collected by the Polish Central Statistical Office (see Barlik and Siwiak 2011). The BBGD is a monthly survey of household income and expenditure; it also includes detailed demographic and labor market information (e.g., earnings, occupation, and industry). Each month, about 3,100 households are interviewed, or about 37,500 households annually (about 0.3 percent of Poland's population). Demographics, labor market information, and most sources of income are collected at the individual level, while expenditure and housing information are reported at the household level. We use data for the years 1997–2003, which allows us to observe five years after the implementation of the 1999 reform. The main analysis sample consists of households whose head was born between 1939 and 1958. The data include a small longitudinal component. Overall, 70 percent of our estimation sample is observed only once, and we therefore treat the data as repeated cross sections. We cluster the standard errors at the

household level to account for the correlation of the residuals for the households that appear more than once in the sample.

Following the literature (e.g., Attanasio and Brugiavini 2003, Attanasio and Rohwedder 2003, and Aguila 2011), we construct the household saving rate as a household's available income minus total household expenditure divided by household available income.

The pension wealth variable is constructed in three steps (described in detail in online Appendix A). First, we estimate lifetime earnings profiles for each household head (and for the spouse if present). Second, pension wealth is computed using pension regulations in force in the year the household is observed. (Online Appendix A details the assumptions made in computing pension wealth.) Third, we define "expected pension wealth" as the household's present value of the sum of benefits, adjusted by survival probabilities obtained from the Polish life tables (see Brugiavini, Maser, and Sundén (2005) for a discussion of approaches to estimating pension wealth).

There are clearly other approaches to estimating the level of pension wealth, and in the results section, we conduct several robustness checks. However, because our analysis focuses on the relationship between pension wealth and private saving *at the margin*, the method of modeling the level of pension wealth should be less important than correctly measuring the *changes* in pension wealth (Attanasio and Brugiavini 2003).

Table 2 presents descriptive statistics for the estimation sample. For expenditure, the saving rate, earnings, and pension wealth (divided by earnings), we report sample means, standard deviations, and median values. For the other variables, we report means and (for continuous variables) standard deviations. The median saving rate is about 9 percent, and the average saving rate is about 2 percent. The average age of the household head is about 48 years ("treated" household heads are on average 46 years old, and "comparison" household heads are on average 54 years old).

B. *Consequences of the Reform: Identifying Effects Using Difference-in-Differences*

We begin our analysis of the effects of the 1999 reform by comparing the mean outcomes of the comparison and treated groups. To do so, we estimate multiyear difference-in-difference (DD) regressions of the following form:

$$(1) \quad SR_{it} = \sum_j \alpha_j Year_j + \varphi Treated_{it} + \sum_j \delta_j (Year_j \times Treated_{it}) + \mathbf{x}_{it}\gamma + \varepsilon_{it},$$

where *SR* is the saving rate of household *i* in year *t*, *Year* denotes year dummies ($j = 1997, 1999, \ldots, 2003$ and so year 1998 is the omitted category), *Treated* is a dummy that equals one if the household head belongs to the cohort directly affected by the reform (those born between 1949 and 1958) and zero otherwise (those born between 1939 and 1948, are the omitted category), *Year* $\times$ *Treated* denotes interactions between the year dummies and the treated-group dummy, and ε is the

Table 2—Sample Descriptive Statistics

Variable	Mean	Standard deviation	Median
Dependent variables			
log expenditure	7.63	0.50	7.62
Saving rate	0.02	0.47	0.09
Characteristics of household head			
Age	47.75	4.65	
Female (percent)	39		
Marital status (percent)			
Unmarried	4.70		
Married	79.74		
Widowed	5.95		
Divorced or separated	9.61		
Educational attainment (percent)			
Tertiary education	10.65		
Postsecondary non-tertiary education	3.09		
Upper secondary education	30.39		
Lower secondary vocational education	7.39		
Gymnasium	0.03		
Primary vocational education	36.37		
Primary education	12.04		
Pre-primary education	0.04		
Occupation[a] (percent)			
Legislators, senior officials, and managers	6.29		
Professionals	6.89		
Technicians and associate professionals	15.33		
Clerks	11.90		
Service workers and shop sales workers	6.44		
Craft and related trades workers	27.16		
Plant and machine operators and assemblers	14.79		
Elementary occupations	11.20		
Works in the private sector (percent)	45		
Comparison group (percent)	24		
Treated group (percent)	76		
Observed before reform (percent)	33		
Observed after reform (percent)	67		
Characteristics of the household			
(Gross) Current earnings (2005 PLN)	2,260	872	2,019
(Gross) Expected pension wealth/current earnings	11.78	4.32	11.12
Number of persons in the household	3.38	1.36	
Number of children below the age of 15	0.48	0.79	
Household owns the place of residence (percent)	58		
Sample size, Observations	37,404		

Note: The saving rate is defined as available household income minus total household expenditure, divided by available household income.

  [a] Occupation is presented here at the one-digit level.

regression-error term. Finally, because about 30 percent of households appear in the estimation sample more than once, we cluster the standard errors by household.[4]

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[4] We have also estimated models where we cluster standard errors by year of birth. Our results remain statistically significant, but our preferred approach is to cluster on the household level as clustering by year of birth effectively leaves us with only 20 clusters.

The estimated δs are the reduced-form, regression-adjusted differences in saving rates of the treated group, relative to the comparison group and holding pre-reform differences between the treated and comparison groups constant.

To increase the precision of the estimates, we include a vector of controls, denoted by $\mathbf{x}$, that includes an intercept, month-of-year dummies, a quadratic polynomial in age, gender, number of persons in the household (household size), number of children, marital status, education dummies, occupation dummies, a dummy for working in the private sector, and a dummy for whether the household owns the house it lives in (i.e., place of residence). We do not include estimated lifetime earnings on the right-hand side of equation (1), as lifetime earnings may have been affected by the reform. Instead, we use education and occupation indicators, which were largely determined before the reform. The analysis is conducted at the household level. All control variables reflect the characteristics of the household head, except for household size, number of children, and a dummy for whether the household owns the house it lives in, as those variables are household characteristics.

In addition to using the saving rate as the outcome variable, we also estimate equation (1) using the log of household expenditure as the outcome. We view the log expenditure regression as a robustness check. Specifically, finding that the δ-estimates from the log expenditure model are a mirror image of the δ-estimates from the saving rate model would imply that the effect of pension reform on the measured saving rate (available income minus expenditure, divided by available income) results from pension reform's effect on expenditure rather than on available income.

The data cover the years 1997–2003. Using two years of data prior to the reform, 1997 and 1998, allows us to test for preexisting group-by-time trends. The presence of pre-reform differences in outcomes between the comparison and treatment groups would call into question whether the differences observed after the reform can be interpreted as its consequences. Using five years of data after the reform, 1999–2003, allows us to examine whether the response to the reform was delayed.

C. Consequences of the Reform: Estimating the Degree of Substitution between Public Pensions and Private Saving

While the DD estimator presented in equation (1) has the advantage of being transparent, it is not directly informative of the degree of substitution between public pension wealth and private saving. In particular, we need to estimate how changes in expected pension wealth affect the saving rate. This subsection discusses the instrumental variable (IV) estimator we use to identify the degree of substitution. We then describe an additional adjustment to the pension wealth variable, Gale's Q adjustment (Gale 1998), which corrects the bias occurring due to observing households with varying planning horizons.

IV Estimator.—The model of interest can be written as follows:

$$(2) \qquad SR_{it} = \theta PW_{it} + \sum_j \alpha_{1,j} Year_j + \alpha_2 Treated_{it} + \mathbf{x}_{it}\gamma_1 + e_{it},$$

where *SR* is the saving rate of household i in year t, *PW* is expected household pension wealth divided by current gross household earnings, *Year* denotes year dummies ($j = 1997, 1999, \ldots, 2003$, with 1998 as the omitted category), *Treated* is a dummy equal to one if the household head belongs to the cohort directly affected by the reform (zero otherwise), $\mathbf{x}$ is a vector of controls described in Section IIB, and e is an error term.

The coefficient of main interest is the substitution parameter θ, which gives the change in the saving rate in response to a change in public pension wealth as a proportion of current gross household earnings. We define the degree of substitution as the absolute value of θ: if a decrease in *PW* increases household saving, we would expect θ to lie between -1 (complete substitution) and 0 (no substitution).

OLS estimates of equation (2) will be inconsistent for θ if *PW* and e are correlated. For example, some individuals may have an unobserved "taste for saving" that leads them both to save more and to have higher pension wealth. If so, then the OLS estimator of θ will be positively biased, although precision will not necessarily be affected. Also, pension wealth may be measured with error. If so, under the classical error-in-variables assumption, the OLS estimator of θ will be attenuated and imprecise (although θ should have the correct sign).[5]

To correct these potential sources of bias, we make use of two institutional features of the 1999 pension reform previously described. First, the 1999 pension reform shifted the expected level of *PW* for some households but not for others. Second, this shift depended only on predetermined factors, namely individuals' year of birth. It follows that a valid instrumental variable for *PW* will be the interaction term between *Post-reform*—a dummy equal to one if the household head is observed in 1999 or later (zero otherwise) and *Treated*—the indicator for whether the household head belongs to a cohort directly affected by the reform, as already described. (Meyer (1995, 159) discusses combining IV and DD methods.)

This leads to the following first-stage equation for the determination of pension wealth:

$$(3) \quad PW_{it} = \kappa_0 (Post\text{-}reform \times Treated)_{it} + \sum_j \kappa_{1,j} Year_j + \kappa_2 Treated_{it} + \mathbf{x}_{it}\gamma_2 + \xi_{it},$$

where the interaction term, *Post-reform* $\times$ *Treated*, is the IV for *PW*. Because it varies only due to the reform, this IV is unlikely to be correlated with the error term in equation (2). The exclusion restriction is that the reform affected the saving rate only through its effect on *PW*. Given these assumptions, the estimate of θ is the estimated effect of pension wealth on the saving rate, identified through the differential impact of the reform on the treated and comparison groups. Furthermore, this IV is relevant as it is highly correlated with *PW* (the first-stage regression F-test statistic exceeds 100). (In estimating the IV model, as with the DD estimator, we cluster the standard errors by household.)

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[5] Alessie, Angelini, and van Santen (2013) discuss problems with measurement error in pension wealth.

As with the DD estimator (equation (1)), we also use the log of household expenditure as an outcome in equation (2). In the IV case, the change in log expenditure is estimated as a response to a change in pension wealth (proportional to current gross household earnings). By analogy to the DD estimator, we expect the θ-estimates from the log expenditure model and saving rate model to be mirror images.

Accounting for Differences in the Planning Horizon.—Gale (1998, 2005) shows that estimates of substitution from a cross-sectional regression of saving in year t on pension wealth in year t—i.e., the present value of a stream of benefits occurring in the future—will be biased toward zero. Specifically, in the case of complete substitution ($\theta = -1$), the cross-sectional estimate of θ will equal $-Q$, where $0 < Q < 1$. This attenuation occurs because the θ-estimate will reflect a one-time increase in saving (i.e., in year t) following a decrease in pension wealth rather than an increase in saving over the full planning horizon. As a remedy, Gale (1998) proposes an adjustment factor, known in the literature as Gale's Q, which is a function of the subjective discount rate, the point in the life cycle at which an individual is observed, and the point in the life cycle when the individual (re)optimizes her saving—e.g., after a change in expected pension wealth.

To see how this factor can be derived, consider the following simple discrete-time model adapted from Attanasio and Rohwedder (2003) and generalized in Feng, He, and Sato (2011). Suppose an individual lives T periods. From period $t = s$ until $t = TR - 1$, she works and receives exogenously determined income y, and from period $t = TR$ until $t = T$, she is retired and receives pension benefits, p. In each period, she has to decide how much to consume and how much to save for the future. The problem can be expressed as

$$(4) \qquad \max_{c_t} \sum_{t=1}^{T} \beta^{t-s} u(c_t)$$

subject to

$$\sum_{t=s}^{T} \frac{c_t}{R^{t-s}} \le \sum_{t=s}^{TR-1} \frac{y_t}{R^{t-s}} + \sum_{t=TR}^{T} \frac{p_t}{R^{t-s}},$$

where c denotes consumption, $R = (1 + r)$ with r representing the real interest rate, and β is the subjective discount factor.

Suppose that, as in Attanasio and Rohwedder (2003), $u(c) = \log(c)$. Without loss of generality and to simplify the notation, assume that $R = 1$. Consumption for any period t, as seen from period s, can then be expressed as

$$(5) \qquad c_t = \beta^{t-s} c_s, \quad \text{where } c_s = \left(\frac{1 - \beta}{1 - \beta^{T-s+1}} \right) \left[\sum_{t=s}^{TR-1} y_t + \sum_{t=TR}^{T} p_t \right],$$

which implies that the saving rate in any period t, as seen from period s, can be expressed as

$$(6) \qquad SR_t \equiv \frac{y_t - c_t}{y_t} = 1 - \beta^{t-s} \left(\frac{1 - \beta}{1 - \beta^{T-s+1}} \right) \left[\frac{\sum_{t=s}^{TR-1} y_t + \sum_{t=TR}^{T} p_t}{y_t} \right].$$

Gale (1998) shows that if one estimates equation (6) by regressing the saving rate on pension wealth, in the scenario where the true degree of substitution is complete, the coefficient on pension wealth will not equal -1, but rather $-Q$, where

$$(7) \qquad Q = \beta^{t-s}\left(\frac{1-\beta}{1-\beta^{T-s+1}}\right).$$

Gale shows that, in principle, one can recover the unbiased estimate of substitution by dividing the substitution estimate by Q or by multiplying each household's pension wealth by Q.[6] The additional information needed includes an assumed value for β, as well as specifying s—the point in time when the household made its consumption plan—and $(T - s)$—the remaining planning horizon for each household whose head is t years old in the data.

Equation (6) describes the optimal saving rate for each period t as seen from period s. However, if an unexpected shock to pension wealth occurs at some later period—e.g., at the end of period $\tau - 1$—then, from period τ onward, the household would behave according to a reoptimized consumption plan, given the level of assets carried over from the previous period. Therefore, for households experiencing a shock to pension wealth, the appropriate adjustment factor for any period $t \geq \tau$ is

$$(8) \qquad Q^* = \beta^{t-\tau}\left(\frac{1-\beta}{1-\beta^{T-\tau+1}}\right),$$

which takes into account the shorter remaining planning horizon.

In practice, for households affected by the reform, we apply the Q^* adjustment, setting τ equal to the age of the head of household when the 1999 pension reform occurred and setting t equal to the current age of the household head. The value $T - \tau$ is set to equal the head's remaining life expectancy after the reform. For households unaffected by the reform, we apply the Q adjustment factor, setting t equal to the current age of the household head and setting s equal to the age when the head last reoptimized her optimal consumption plan. We assume this to be the time of the collapse of the People's Republic of Poland in 1989, an event that changed the economic environment in Poland (although it did not affect pensions directly). The value $T - s$ is set to be equal to the remaining life expectancy of the household head. For both Q and Q^*, we follow Attanasio and Brugiavini (2003) and Attanasio and Rohwedder (2003) and assume that β equals 0.98. We examine this assumption in more detail in our sensitivity analysis in Section IIID.

D. *Validity of the Estimates*

Internal validity of our estimates depends on a number of factors. First, the substitution estimate would be attenuated if the pension reform were anticipated before 1999, leading households to adjust their behavior in advance. Second, because our identifying variation stems from comparing households from various cohorts over time, internal validity depends on the degree of comparability of the treated and

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[6] Gale (1998) also shows that even if the true degree of substitution is less than complete, a regression of the saving rate on pension wealth will understate the true degree of substitution by a factor of Q.

comparison groups. Third, if the groups studied differed in unobserved ways before and after the reform (e.g., if unobserved factors affected the difference in trends between cohorts), the *Post-reform* $\times$ *Treated* dummy and the regression-error term would be correlated. Fourth, internal validity would be compromised if other factors confounded the effect of the reform.

A number of factors arguably strengthen the internal validity of the analysis. First, the particulars of who would and would not be affected by the 1999 pension reform were not decided upon before October 1998. In consequence, the treated group had little time to adjust their behavior before the reform. In Section IIID, we conduct a robustness check where we drop households observed between October and December 1998, to exclude those who may have reacted to the legislated changes before they came into force on January 1, 1999. These estimates are similar to the main estimates.

Second, the comparison and treated cohorts are observed in our data at slightly different stages of their lives, which might result in unobserved heterogeneity across the cohorts before and after the reform that could be due to different age patterns of saving. However, the cohorts are, arguably, sufficiently close in age for their patterns of saving to be very similar absent the reform. We also condition the estimates on age polynomials and other demographics. In Section IIID, we conduct robustness checks in which we narrow the age span between cohorts still further. Our estimates turn out to be robust to these different assumptions.

Third, in order to correct for measurement error in pension wealth using our IV approach, the *Post-reform* $\times$ *Treated* dummy cannot be correlated with measurement error in pension wealth. Because measurement error in pension wealth is likely to be of greater concern the more different in age the treated and comparison groups are over time, we focus our analysis on a relatively narrow age span.

Fourth, Poland was undergoing other reforms at the time of the pension reform. Hence, one may wonder whether our estimates are confounded by the effects of these other reforms. To our knowledge, though, no reform or other change during the period 1999–2003 (the post-reform observation period) affected people who were born in 1949 or later in a different way from people born before 1949.

Finally, we believe that because the 1999 pension reform was a large, nationwide reform and because its implementation resembles a natural experiment, estimates based on the reform should provide generalizable insights for retirement policy in other contexts.

III. Results

A. *Difference-in-Difference Estimates*

Figure 2 plots the values of the average saving rate for the comparison group (dashed line) and the treated group (solid line) between 1997 and 2003. Between 1997 and 1998, the saving rates of both the treated and comparison groups declined in parallel, supporting the common trends assumption needed to identify the effect of the reform. However, starting in 1999, the saving rate of the treated group recovered from its 1998 low, whereas the saving rate of the comparison group continued to fall.
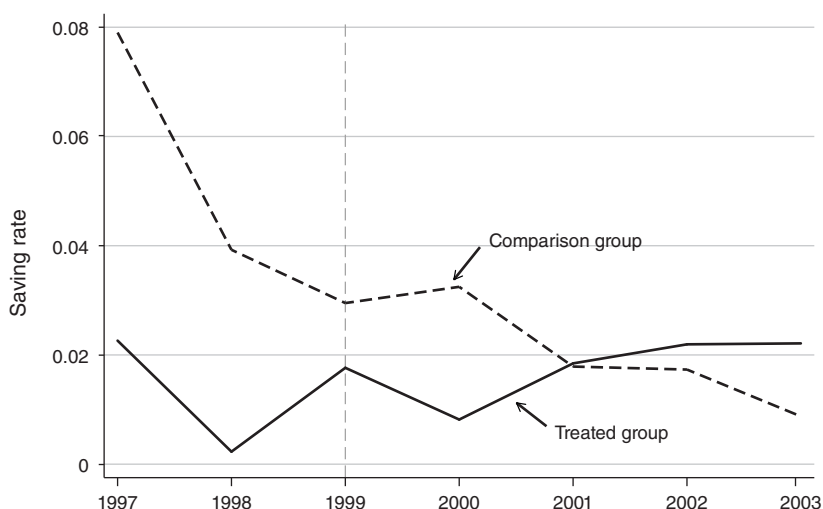
FIGURE 2. MEAN SAVING RATE IN THE BBGD, BY YEAR AND GROUP

Notes: The saving rate is defined as available household income minus total household expenditure, divided by available household income. The dashed line indicates the comparison group, born 1939–1948, and the solid line indicates the treated group, born 1949–1958. The dashed vertical line indicates the first year of the reform.

The falling saving rate of the comparison group and the comparatively steady saving rate of the treated group suggest that the saving rate of the treated group after the reform increased relative to the comparison group and relative to before the reform.

In order to interpret this relative increase as a causal effect of the reform, we need to be able to interpret the time profile of the saving rate of the comparison group as a valid counterfactual. Available evidence suggests that during 1997–2003 the overall aggregate voluntary household saving rate in Poland (calculated in relation to gross domestic product) declined in a pattern similar to that experienced by the comparison group in Figure 2. Specifically, the aggregate voluntary household saving rate fell from about 10 percent in 1997 to about 5 percent in 2003 (2014 World Bank Report on Poland, figure 2.11, page 15). That the aggregate voluntary saving rate and the saving rate of the comparison group both fell during the period in question tends to support the identifying assumption—in the absence of the pension reform, the saving rates of the treated and comparison groups would have fallen in parallel.

Figure 3 complements Figure 2 by showing regression-adjusted differences between the treated and comparison groups for the saving rate (top panel) and for log expenditure (bottom panel). (These are estimates of δs from equation (1), so the outcomes of the treated group are shown relative to the comparison group and relative to the pre-reform year 1998, which allows us to examine potential pre-reform group-by-time trends.) The point estimates are presented for 1997–2003 with 95 percent confidence intervals (the whiskers).

Panel A. Saving rate
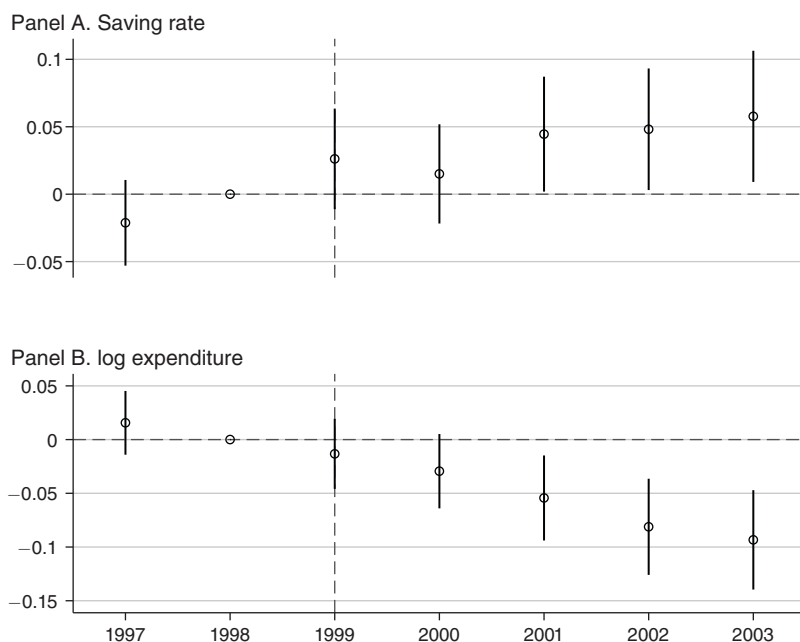
Panel B. log expenditure

FIGURE 3. ESTIMATED EFFECT OF THE 1999 PENSION REFORM ON THE SAVING RATE AND LOG EXPENDITURE, BY YEAR

Notes: The figure shows point estimates from equation (1), where the outcome variable is regressed on six year dummies, a "treated" dummy (if born 1949–1958), treated-by-year interaction terms, and the same controls as listed in the notes to Table 3. Each panel shows the treated-by-year interaction point estimates (δs) over time. The omitted categories are year 1998 (the year before the reform) and the comparison group (if born 1939–1948). The figure presents 95 percent confidence intervals (whiskers) based on standard errors that are robust and clustered by household. The dashed vertical line indicates the first year of the reform.

The absence of statistically significant differences between the treated and comparison groups in the pre-reform year 1997 lends further support to the common trends assumption required to interpret the point estimates for 1999–2003 as effects of the reform (Angrist and Pischke 2009, 237–41). The relative changes after 1999 show that the saving rate tended to increase over time (and log expenditure tended to decrease) for the treated group in the post-reform years, although the estimates are somewhat imprecise in 2000.[7]

In summary, Figure 3 suggests that the estimated effects on the saving rate in the post-reform years are positive and lie between 0 and 5 percentage points. This finding suggests that the reduction in pension benefit generosity due to the reform led to an increase in the rate of saving and a decrease in expenditures. Although the DD estimates suggest a causal link between the pension reform and saving behavior of households, they are not directly informative about the degree of substitution. To estimate the latter, we turn to the IV estimates of the model in equations (2) and (3).

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[7] In Figure B.1 in online Appendix B, we test for preexisting group-by-time trends by using the 1995 and 1996 waves of the BBGD. Unfortunately, these two waves do not have information on occupation, a key variable in the definition of our sample and calculation of pension wealth, so we are unable to use these waves in our main analysis. Nevertheless, Figure B.1 tends to confirm the lack of significant differences in pre-reform saving and expenditure patterns between the treated and comparison groups.

TABLE 3—OLS AND IV ESTIMATES OF THE EFFECT OF UNADJUSTED AND Q-ADJUSTED
PENSION WEALTH ON THE HOUSEHOLD SAVING RATE OR LOG EXPENDITURE

	Saving rate	log expenditure
Panel A		OLS
Unadjusted pension wealth	−0.002	−0.007
	(0.001)	(0.002)
Panel B		IV
Unadjusted pension wealth	−0.014	0.016
	(0.004)	(0.004)
Panel C		IV
Adjusted pension wealth	−0.293	0.339
	(0.084)	(0.079)
Sample size, Observations	37,404	37,404

Notes: The column on the left shows estimates of substitution between pension wealth and private saving (θ from equation (2) with the saving rate as the dependent variable). The column on the right shows estimated effects of pension wealth on log expenditure (θ from equation (2) with log expenditure as the dependent variable). Panel A displays OLS estimates. Panel B displays IV estimates, with pension wealth instrumented by an interaction term between the "post-reform" dummy and the "treated" dummy. Panel C shows IV estimates, with pension wealth instrumented as in panel B and adjusted by the Q-factor. (The first-stage regression F-statistic test for weak instruments equals 8,384.) Controls include month-of-year dummies, a quadratic polynomial in age, gender, number of persons in the household, number of children, marital status, education dummies, occupation dummies, a dummy for working in the private sector, and a dummy for whether the household owns its place of residence. Other variables include a full set of year dummies (with 1998 as the omitted category) and a "treated" dummy (if born 1949–1958; born 1939–1948 is the omitted category). Robust standard errors clustered by household are in parentheses.

B. *Estimated Effects of Pension Wealth on the Saving Rate and Expenditure*

The left column of Table 3 shows the estimates of substitution (θ) from equation (2) using the saving rate as the dependent variable. The right column of Table 3 shows the estimated effect of pension wealth on log expenditure from equation (2). Panel A shows the estimates obtained using OLS without instrumenting *PW* by *Post-reform × Treated* and where *PW* is not adjusted by the Q-factor. Panel B shows the estimates obtained using IV where *PW* is instrumented by *Post-reform × Treated* but where *PW* is not adjusted by the Q-factor. Panel C shows the estimates obtained using IV where *PW* is instrumented by *Post-reform × Treated* and where *PW* is adjusted by the Q-factor. We do not report coefficients on other right-hand-side variables.

The OLS estimates of substitution (θ) presented in panel A of Table 3 are very close to zero, possibly because of measurement error in the dependent variable. Furthermore, when using log expenditure as an outcome, the OLS point estimate has an unexpected negative sign, implying that a decrease in pension wealth increases household expenditures. The two IV estimates in panel B are also small, but both have the expected sign: negative for the saving rate and positive for log expenditure. In the case of log expenditure as an outcome, the difference in sign between the OLS estimate in panel A and the IV estimate in panel B is consistent with the OLS

estimator being biased because of unobserved heterogeneity, a result also found by Attanasio and Rohwedder (2003) and Engelhardt and Kumar (2011).

The estimates of main interest are in panel C, using IV with Q-adjusted pension wealth. These estimates have the expected signs and are larger in absolute terms than the estimates in panel B. This is because in panel C each household's pension wealth is multiplied by the Q-factor, and while multiplying pension wealth by Q does not change the sign of the estimate of substitution, it does rescale the estimate. Hence, the estimates in panel C suggest that a 1 PLN decrease in pension wealth increases private saving by about 0.29 PLN and decreases spending by about 0.34 PLN.[8]

The change in magnitude between the IV estimates in panels B and C is comparable to the change reported by Feng, He, and Sato (2009), where the substitution estimate obtained using unadjusted pension wealth equaled -0.014, while the substitution estimate obtained using Q-adjusted pension wealth equaled -0.257.

The estimates presented in Table 3, panel C, as in most recent studies of public pension substitution, differ from those of Chetty et al. (2014), who find that in Denmark the relationship between private saving and public pensions is zero. The reason could be as simple as differences in cultural norms with regard to saving between Denmark and countries such as Poland, Italy, or the United Kingdom.[9] Another possible reason is that while our analysis—as well as that of Attanasio and Brugiavini (2003), Attanasio and Rohwedder (2003), and Bottazzi, Jappelli, and Padula (2006)—identifies substitution in the context of reforms that reduced pension wealth, Chetty et al. (2014) examine a setting that increased pension wealth. Similarly, Feng, He, and Sato (2011) study the effects of introducing a pension system and find a low degree of substitution. Hence, although standard expected utility theory predicts that the saving response should be symmetrical with respect to increases and decreases in pension wealth, the response of private saving may in fact depend on the direction of change in pension wealth.[10]

Differences between Chetty et al. (2014) and other studies could also result from differences in the degree of awareness of the respective reforms. As with the Italian reform, the debate about Poland's 1999 pension reform was highly visible in the media, which could be reflected in a relatively large observed response. Finally, whereas Chetty et al. use a regression-discontinuity design to identify the effect of mandated saving for individuals close to the discontinuity, we use variation resulting from a broad-based reform to identify the degree of substitution across an entire population.

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[8]When using ten separate dummies for each of the year-of-birth cohorts affected by the reform interacted with *Post-reform* and controlling for year dummies and year-of-birth cohort dummies, we obtain somewhat smaller effects: $\theta = -0.175$ (standard error $= 0.11$) for the saving rate and $\theta = 0.131$ (standard error $= 0.10$) for log expenditure.

[9]For example, using comparable cross-country data from the Survey of Health, Ageing and Retirement in Europe (SHARE), Alessie, Angelini, and van Santen (2013) study public pensions and saving in different regions of Europe. However, contrary to both Chetty et al. (2014) and evidence from the Italian pension reforms (Attanasio and Brugiavini 2003; Bottazzi, Jappelli, and Padula 2006), they find that substitution is largest in Northern European countries and smallest in Southern (and Eastern) European countries.

[10]This asymmetry could be understood in the context of prospect theory, which highlights the importance of reference points and holds that individuals react more strongly to losses than to corresponding gains.

Table 4—Heterogeneity Analysis: IV Estimates of the Effect of Q-Adjusted Pension Wealth on the Household Saving Rate or log Expenditure for Selected Subsamples of Households

	Head of household has at least tertiary education		Head of household has less than tertiary education	
	Saving rate	log expenditure	Saving rate	log expenditure
Adjusted pension wealth	−1.076	1.200	−0.252	0.278
	(0.504)	(0.439)	(0.084)	(0.081)
Sample size, Observations	3,983	3,983	33,421	33,421
	Household owns the place of residence		Household does not own the place of residence	
Adjusted pension wealth	−0.276	0.301	−0.320	0.380
	(0.121)	(0.107)	(0.110)	(0.117)
Sample size, Observations	21,880	21,880	15,524	15,524

Notes: The columns on the left show the estimates of substitution between pension wealth and private saving (θ from equation (2) with the saving rate as the dependent variable). The columns on the right show estimated effects of pension wealth on log expenditure (θ from equation (2) with log expenditure as the dependent variable). All estimates are obtained by IV, with pension wealth adjusted by the Q-factor and instrumented by an interaction term between the "post-reform" dummy and the "treated" dummy. Controls are the same as in Table 3. Robust standard errors clustered by household are in parentheses.

C. *Analysis of Subsamples*

Previous research on financial literacy has found that households may not fully understand the details of how public pension systems work (Lusardi and Mitchell 2014). One might speculate that better-educated individuals are better informed about pension systems in general, are more likely to be "active" savers (Chetty et al. 2014), or are financially more able to adjust their savings. If so, we would expect a larger degree of substitution for better-educated households. For example, using three Italian reforms (in 1992, 1995, and 1997), Bottazzi, Jappelli, and Padula (2006) find the degree of substitution to be about 0.8 among individuals who are well informed about the pension system. Gale (1998) also finds substitution close to 0.7 for highly educated households in the United States (compared with 0.5 in the full sample).

The accumulated value of assets other than pension wealth might also influence the sensitivity to changes in pension wealth. In theory, we would expect households that have accumulated a buffer stock to be less sensitive to pension wealth changes than those without assets. Since the BBGD does not collect information on financial assets, we split the sample by house-ownership status, treating house ownership as an indicator for housing wealth.

Table 4 presents IV estimates for different subsamples: in the top panel, we split households by the head's level of education, while in the bottom panel we split households by house-ownership status. For households where the head has at least tertiary (that is, university) education, the point estimates suggest complete substitution. For households with less-educated heads, the estimated substitution is less than one-third.

We find little difference between the substitution estimates of households that own and do not own a house. This finding is puzzling because it suggests that Polish

households ignore their housing wealth when making decisions about saving. A possible explanation is that Polish households treat their housing assets as a key element of their future bequest. For example, there is very little evidence of household downsizing in Poland as individuals age and become widowed. In such a scenario, housing represents a very illiquid asset, limiting the extent to which the household would be willing to substitute between discretionary saving, pension wealth, and housing wealth.

Another reason for the lack of difference by house ownership, might be a limited ability to borrow against housing equity (e.g., in the form of home equity loans). Angelini, Brugiavini, and Weber (2011) find a clear negative correlation between measures of mortgage market development, such as loan-to-value ratios, and the share of elderly homeowners who report difficulties making ends meet (an indicator of financial distress). Using data from the 2006–2007 wave of the SHARE survey, they show that in Poland about 70 percent of elderly homeowners reported financial distress, the highest value in their sample of 13 European countries. At the same time, the authors report that between 2003 and 2006, the typical loan-to-value ratio in Poland was about 50 percent, the second lowest value in their sample. Hence, although elderly homeowners in Poland at this time were likely to own their homes outright, they were also likely to report a high degree of financial distress, and this financial distress appears to be correlated with a low level of development of the market for home equity. Although we do not have direct evidence, we speculate that low levels of development of the mortgage market can be viewed as a proxy for the absence of financial instruments allowing homeowners to borrow against housing equity.

D. *Robustness Analysis*

In this section, we conduct four robustness checks by changing the definitions of the analysis sample, one robustness check where we alter the computation of pension wealth, and a robustness check where we change the assumptions regarding the *Q*-factor.

Redefining the Analysis Sample.—The main estimation sample consists of 8,854 households in the comparison group and 28,550 households in the treated group (see Table 2), so we begin our sensitivity analysis by examining the role of this imbalance by randomly selecting 8,854 households from the treated group. The IV estimates are given in panel A of Table 5, and the degree of substitution estimated is similar in magnitude and precision to the main results in panel C of Table 3.

In panel B of Table 5, we restrict the analysis sample to cohorts whose birth year is closer to 1949, in order to limit potential unobserved heterogeneity between the comparison and treated groups. We select household heads born between 1944 and 1948 for the comparison group and household heads born between 1949 and 1953 for the treated group. The estimates in this sample are close to the main estimates in Table 3—about -0.39 for the saving rate and 0.23 for log expenditure.

The BBGD expenditure categories were redefined starting in 1998, so in panel C of Table 5, we re-estimate the model after excluding the data from the 1997 survey. By using this smaller sample, the point estimates are somewhat smaller in absolute

TABLE 5—ROBUSTNESS CHECKS: IV ESTIMATES OF THE EFFECT OF Q-ADJUSTED PENSION WEALTH ON THE
HOUSEHOLD SAVING RATE OR LOG EXPENDITURE OBTAINED USING ALTERNATIVE ANALYSIS SAMPLES OR
ALTERNATIVE CONSTRUCTION OF PENSION WEALTH

	Saving rate	log expenditure
Panel A. Sample restricted so that the "treated" group has the same size as the comparison group		
Adjusted pension wealth	−0.243	0.271
	(0.106)	(0.096)
Sample size, Observations	17,708	17,708
Panel B. Sample restricted to cohorts born 1944–1953		
Adjusted pension wealth	−0.390	0.227
	(0.164)	(0.150)
Sample size, Observations	20,147	20,147
Panel C. Sample restricted to using only years 1998–2003		
Adjusted pension wealth	−0.215	0.286
	(0.098)	(0.095)
Sample size, Observations	31,149	31,149
Panel D. Sample where male household heads are younger than 61 years and female household heads are younger than 56 years		
Adjusted pension wealth	−0.283	0.305
	(0.086)	(0.081)
Sample size, Observations	36,723	36,723
Panel E. Pension wealth calculation assumes that men retire at 55 years of age and women at 50 years of age		
Adjusted pension wealth	−0.432	0.500
	(0.123)	(0.116)
Sample size, Observations	37,404	37,404

Notes: The column on the left shows estimates of substitution between pension wealth and private saving (θ from equation (2) with the saving rate as the dependent variable). The column on the right shows estimated effects of pension wealth on log expenditure (θ from equation (2) with log expenditure as the dependent variable). All estimates are obtained by IV, with pension wealth adjusted by the Q-factor and instrumented by an interaction term between the "post-reform" dummy and the "treated" dummy. Controls are the same as in Table 3. Robust standard errors clustered by household are in parentheses.

terms than in the main specification—about –0.22 for the saving rate and about 0.29 for log expenditure.[11]

In the main estimates, we include households headed by men up to age 65 and women up to age 60. However, these age limits are close to typical retirement ages and could include households that are already transitioning to retirement. To exclude such households, panel D restricts the analysis sample to households headed by men up to age 60 and women up to age 55. The main results are robust to this restriction as well.

Different Assumptions about Pension Wealth Calculation.—In panel E of Table 5, we recalculate pension wealth by assuming retirement occurs at 55 for men and 50

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[11] We also re-estimated the model excluding observations for October–December 1998 as well as the year 1997. Recall that the reform bill was passed in October 1998, so a sizable share of pre-reform observations is observed in the last months of 1998. If these households had reacted to the reform before January 1999, we would expect to see a slightly lower degree of substitution. For this sample, we obtain $\theta = -0.24$ (standard error = 0.11) for the saving rate and $\theta = 0.29$ (standard error = 0.11) for log expenditure.

TABLE 6—ROBUSTNESS CHECK: IV ESTIMATES OF THE EFFECT OF Q-ADJUSTED PENSION
WEALTH ON THE HOUSEHOLD SAVING RATE (θ) AS A FUNCTION OF THE SUBJECTIVE
DISCOUNT FACTOR, β

Value of β (1)	Mean Q implied by β (2)	Estimate of θ implied by β (3)
0.90	0.076 [0.023, 0.121]	−0.076
0.96	0.048 [0.028, 0.067]	−0.203
0.97	0.044 [0.028, 0.060]	−0.243
0.98	*0.040* *[0.027, 0.053]*	*−0.293*
0.99	0.036 [0.026, 0.047]	−0.354
0.999	0.033 [0.025, 0.042]	−0.421

Notes: Column 1 lists different values of the subjective discount factor, β, and column 2 shows the corresponding mean Q-factor (with minima and maxima in square brackets). Column 3 shows the resulting IV estimate of the effect of Q-adjusted pension wealth on the household saving rate (θ from equation (2)). The baseline model (in italics) sets β equal to 0.98 and is the same as in Table 3, panel C.

for women, instead of 65 for men and 60 for women as with the main estimates. The resulting point estimates are larger in absolute value—about −0.43 for the saving rate and 0.50 for log expenditure. This higher degree of substitution makes sense, as the calculation shortens the contribution period of the cohorts affected by the reform.[12]

Sensitivity to Different Values of the Subjective Discount Factor.—Section IIIB showed that the estimated substitution effects depend on whether the value of pension wealth is adjusted by the Q-factor. This factor in turn depends on the choice of the subjective discount factor, β. Typically, β has been set between 0.96 and 0.98, but there is no clear consensus as to what value it should take.[13] In this subsection, we check the sensitivity of the substitution estimates to different assumptions about β.

Table 6 maps selected values of β to the values of the Q-factor and to the estimates of substitution using the saving rate as the outcome. For each value of β listed in column 1, column 2 shows the corresponding mean value of Q in our sample, its minimum value, and its maximum value. Column 3 shows the resulting IV estimate of substitution from the regression that uses the same specification as our main estimates (Table 3, panel C) as a function of β (and, hence, a function of Q).

Table 6 shows that for the selected range of β, the Q-factor decreases as the subjective discount factor β increases so that, holding other factors equal, large values of Q (low values of β) imply smaller estimates of substitution than do small values of Q (high values of β). For example, for β equal to 0.90, the substitution estimate

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[12] We also re-estimated the model with female spouse's lifetime earnings estimated using OLS rather than a selection model. The estimates are similar, although larger than the baseline estimates in Table 3, panel C.

[13] Aguila (2011) assumes it to be 0.90, Gale (1998) assumes it to equal 0.96, Alessie, Angelini, and van Santen (2013) set it to 0.97, and Attanasio and Brugiavini (2003), Attanasio and Rohwedder (2003), Bottazzi, Jappelli, and Padula (2006), and Feng, He, and Sato (2011) assume it to be 0.98; see also Table 7.

is about -0.08, while for β equal to 0.999, the estimate is about -0.42. In the range of the most popular choices of β used in the literature—between 0.96 and 0.98—we obtain substitution estimates in a relatively narrow range—between about -0.20 and -0.29. Still, the sensitivity of estimates to the choice of the discount factor is an important result to note.

IV. Discussion and Conclusion

We have studied the large change in expected pension wealth induced by Poland's 1999 pension reform to estimate the effect of public pensions on private household saving. Implementation of the reform created quasi-experimental variation in pension wealth suitable for investigating whether households increase private saving in response to reduced generosity of a public pension plan.

The difference-in-difference estimates displayed in Figure 3 show the reform increased household saving and decreased expenditure. The main IV estimates in panel C of Table 3 suggest that, overall, reducing public pensions increases private saving by about 0.30 PLN for each 1.00 PLN decrease of pension wealth. This is a sizable degree of substitution, although it is far from complete. Combined with the conclusions of Lindner and Morawski (2012) that the reform had little effect on labor supply, the estimates suggest that, when faced with a reduction in future pension benefits, older households in Poland choose to adjust their saving rather than their labor supply.

We find that for highly educated households—those we expect to be informed about the reform or who are financially better able to adjust—substitution between private saving and pension wealth is close to complete. We speculate that the more modest response among lower-educated households could be due to liquidity constraints, incomplete information, or uncertainty about how enduring the 1999 reform would be. The relatively passive behavior of less-educated households echoes findings in the literature on financial literacy, which suggests that by remaining passive, these households risk being inadequately prepared for retirement (Lusardi and Mitchell 2014). This, in turn, may suggest a need for improved financial literacy, especially among groups at risk of insufficient retirement resources.

The main estimate of the degree of substitution of about 0.3 is at the lower end of the range of existing estimates. Table 7 summarizes recent studies using methods similar to ours, lists the data and sample definitions applied, and, whenever possible, documents the variation in the degree of substitution by age. Among the studies that split the analysis by age, substitution tends to be higher for people aged roughly 40 to 55 years, an age interval similar to the one examined in this paper. (See, for example, Attanasio and Rohwedder (2003), where the degree of substitution equals about 0.65 for ages 43–53, and Feng, He, and Sato (2011), where it equals about 0.38 for ages 46–59.) In comparison, the estimated degree of substitution from the Polish reform is relatively small and closest to the estimate for 46–55-year-olds reported by Attanasio and Brugiavini (2003), about 0.24. Hence, despite using similar methods and imposing similar age restrictions for their samples, these papers report varying degrees of substitution. A systematic study of the reasons for these differences across countries and pension reforms would be highly useful.

Table 7—Estimates of the Degree of Substitution between Public Pension Wealth and Private Saving, Selected Studies of Pension Reforms Using Methods Similar to This Paper

Study	Source of variation in pension wealth	Sample and data	Degree of substitution between public pension wealth and private saving
Attanasio and Brugiavini (2003)	Pension reform in Italy	Employed or retired household heads in the Italian Survey of Household Income and Wealth (SHIW) in 1989, 1991, 1993, and 1995, aged 20–65. Subjective discount factor set to 0.98	0.10 for 20–35-year-olds; 0.75 for 36–45-year-olds; 0.24 for 46–55-year-olds; <0.10 for 56–65-year-olds. Overall substitution between 0.04 and 0.33 (table 5, column 3 and table 4)
Attanasio and Rohwedder (2003)	Series of pension reforms in the United Kingdom	Exclude households headed by someone self-employed or retired from the UK Family Expenditure Survey in 1974–87. Subjective discount factor set to 0.98	0 for 20–31-year-olds; 0.55 for 32–42-year-olds; 0.65 for 43–53-year-olds; 0.75 for 54–64-year-olds. No estimate for overall substitution reported (table 5, first column)
Bottazzi, Jappelli, and Padula (2006)	Series of pension reforms in Italy	Employed or self-employed household heads in the Italian SHIW in 1989–2002, aged 20–50. Subjective discount factor set to 0.98	0.65 overall substitution (table 9, fourth column)
Aguila (2011)	Pension reform in Mexico	Households headed by a person covered by Social Security in the Mexican National Income and Expenditure Survey in years 1992, 1994, 1996, and 1998. Subjective discount factor set to 0.90	0.55–0.60 overall substitution (p.18)
Feng, He, and Sato (2011)	Pension reform in China	Employed, urban households in the 1995 and 1999 China Household Income Project, aged 25–59. Subjective discount factor set to 0.98	0 for 25–39-year-olds; 0.38 for 40–59-year-olds. Overall substitution about 0.20 (table 9)

Notes: The degree of substitution refers to the absolute value of the proportional increase (or decrease) in private saving in response to a decrease (or increase) in public pension wealth. Reported estimates are from the papers cited.

Finally, the robustness checks in Table 6 show that the estimates of the degree of substitution depend to some extent on assumptions about the subjective discount rate. For example, our main estimate of the degree of substitution of about 0.3 assumes a discount rate of 2 percent. Assuming instead a higher discount rate (4 percent) yields a degree of substitution closer to 0.2. This difference could be large enough to carry 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 values of subjective discount rates and their distribution in the population. On the whole, we prefer to err on the side of caution and interpret our main estimate of the degree of substitution as an upper bound.

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