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Pension Wealth and Household Savings in Europe: Evidence from SHARELIFE*

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Abstract

We use recently collected retrospective survey data to estimate the displacement effect of pension wealth on household savings. The third wave of the Survey of Health, Ageing and Retirement in Europe, SHARELIFE, collects information on the entire job history of the respondent, a feature missing in most previous studies. We show that addressing measurement error problems is crucial to estimate the displacement effect when using survey data. We find that each euro of pension wealth is associated with a 47 (61) cent decline in non-pension wealth using robust (median) regression. In the presence of biases from measurement errors and omitted (unobserved) variables, we estimate a lower bound to the true offset between 17% and 30%, significantly different from zero. Instrumental variables regression estimates, although less precise, suggest full displacement.

Keywords: Displacement effect, Lifetime income, Retrospective survey, Measurement error

JEL: D91, H55, D31

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1 Introduction

The demographic challenge of ageing populations has led and will lead European countries to reform their pension systems. For policymakers, understanding the effect that pension reforms will have on household and national saving is crucial. In particular, the effect of changes in pension wealth on private wealth is vital information for assessing the welfare effects of these reforms. A stylized version of the life-cycle model suggests that generous social security benefits will have a negative effect on the accumulation of private savings if households save only for retirement, i.e. crowding out of private wealth by pension wealth. However, the extent to which households offset pension wealth with other forms of wealth accumulation is difficult to gauge. From a theoretical point of view, the extent of the offset depends on a variety of other factors, such as the presence of binding liquidity constraints, the distortional effects of taxation and the fact that households might save for reasons other than retirement or may lack a basic level of financial literacy. From an empirical point of view, the econometric identification of the offset is made difficult by the lack of data on lifetime earnings and by the fact that pension wealth is typically measured with error in surveys.

In this paper we estimate whether and to what extent European households offset pension wealth with private savings. An innovative aspect of our paper is that we use retrospective data from the third wave of the Survey of Health, Ageing and Retirement in Europe (SHARELIFE), which collects information on the entire job and wage histories of older workers and retirees in 13 European countries. In this way we are able to construct measures for both the present value of past and future earnings and pension wealth at the individual level, a feature missing in most studies estimating the displacement effect.

Many papers have made attempts to estimate the displacement effect but the empirical evidence is mixed. In his seminal article, Feldstein (1974) uses aggregate time-series data for the US and shows that a 1 dollar increase in Social Security Wealth (SSW) depresses private saving by about 40 dollar cents. However, Feldstein and Liebman (2002) point out that this estimate of the displacement effect might be inconsistent because of aggregation problems. For that reason many papers have used cross-section data to investigate the level of displacement between SSW and wealth (see e.g. Feldstein and Pellechio (1979), Dicks-Mireaux and King (1984), Hubbard (1986) and Jappelli (1995)). In these earlier studies non-pension wealth is typically regressed on cash earnings and pension wealth (and some other controls). Gale (1998) convincingly shows that in such regressions the estimated displacement effect is biased downwards. He proposes and applies a method to remove this bias, which boils down to multiply pension wealth by an age-specific adjustment factor, called "Gale's Q". He finds an estimated offset close to 100% for a sample of US households in which the head is employed and aged between 40 and 64. Attanasio and Rohwedder (2003) and Attanasio and Brugiavini (2003) use time series of cross-section data to estimate saving rate equations derived from life-cycle models, exploiting pension reforms in the United Kingdom and Italy respectively to identify the displacement effect. Their results indicate that the effects of pensions on wealth vary significantly across households, with nearly retired individuals showing more crowd out than young workers. Engelhardt and Kumar (2011) use data on 51-61 years old working individuals from the Health and Retirement Study (HRS) in the US and adopt an instru-

mental variables approach to account for measurement error in wealth and individual heterogeneity, such as taste for saving. They find an average displacement effect between 53 and 67 percent. However, quantile estimates show substantial heterogeneity across the wealth distribution, with crowd-in at lower quantiles, no offset at the median and significant crowd-out for affluent households. Kapteyn et al. (2005) exploit productivity differences across cohorts and the introduction of social security in the Netherlands to find a small but statistically significant displacement effect of 11.5%. Hurd et al. (2012) use cross-country variation and cross-sectional variation in education and marital status to identify the displacement effect on financial wealth from a pooled sample of retired males aged 65 to 75 from the HRS, ELSA (UK) and SHARE (ten continental European countries). To pool these samples, and because of problems with omitted variables and measurement error, all variables are aggregated by education and marital status. Their estimated displacement effect ranges between 23 and 44 percent.

We contribute to the literature by presenting new estimates of the displacement effect using micro data on both older workers and retired individuals collected by the SHARELIFE project in 13 European countries. Like Gale (1998) and Engelhardt and Kumar (2011), we perform our analysis on a cross-section of households, while exploiting cross-country variation as in Hurd et al. (2012). Thanks to the retrospective nature of the data, we are able to construct a measure of the present value of past earnings using the entire job history of each respondent and the information on the first wage earned in each job. With the exception of Engelhardt and Kumar (2011), all previous studies instead had to rely on proxy measures for past earnings, most notably current income, age, education and marital status. Moreover, actual pension benefits for those that are retired allow us to construct pension wealth; for the non-retired, we use subjective information on individuals' expected retirement age and replacement rate to compute expected pension wealth. We show that the retrospective survey data are able to generate cross-country differences in wages and pensions, as well as age-earnings profiles that are in line with expectations.

An important econometric phenomenon both in this study and the empirical literature discussed above is the impact of measurement errors on the parameter estimates. Both pension wealth and the present value of past and future earnings are typically measured with error, if not unobserved. Typically, these two measurement errors are positively correlated with each other. We show in Section 2.1 that the bias which stems from those two positively correlated measurement errors, might well lead to a spurious positive partial correlation between pension wealth and private wealth. Therefore, we introduce a restricted model for which we can sign the impact of correlated measurement errors on the estimators. Furthermore, we provide lower bounds to the true offset using a sample of retirees, for whom we know lifetime income and pension wealth from two independent series of survey questions. Although both are measured with error, the correlation between these measurement errors is likely to be small or even negligible. We cannot make this claim for the non-retired included in the full sample, for whom we infer pension benefits from multiplying the (individual-specific) expected pension income replacement rate by current income, which essentially imposes correlation between the measurement errors.

The estimated displacement effect for the full sample is equal to 47.1% using robust regression and 60.9% using median regression techniques, and in both cases significantly

different from zero and 100%. We obtain lower bounds between 17% and 30%, significantly different from zero. When we use financial wealth as the dependent variable instead of net worth, we estimate the crowd-out to be between 77.8% and 87.0%, and obtain a lower bound between 53% and 69%. Using the Instrumental Variable strategy of Chernozhukov and Hansen (2005, 2008) to avoid attenuation bias from measurement errors and unobserved heterogeneity, we obtain less precise estimates which suggest full displacement.

In the remainder of this paper, we first present a simple life-cycle model to guide our empirical analysis in Section 2. Section 3 discusses the variables used in this study and the assumptions we made in the computation of lifetime earnings and pension wealth. Section 4 presents the results and several robustness checks. Section 5 concludes.

2 Model

As most studies on this subject, we derive the equation of interest from a simple life-cycle model, which is the discrete-time counterpart of Gale (1998). Like Gale, we assume that past changes in the pension system have been fully anticipated by the agents at the beginning of their life. We ignore uncertainty and liquidity constraints, and assume perfect capital markets that produce a constant real interest rate, r . Moreover, we assume that the retirement age, R , and non capital income at age τ , y_τ , are exogenous variables. The within period utility function is assumed to be isoelastic (constant relative risk aversion [CRRA]). The consumer maximizes lifetime utility subject to the lifetime budget constraint, i.e:

$$\max_{c_\tau} \sum_{\tau=1}^L (1+\rho)^{1-\tau} \frac{c_\tau^{1-\gamma}}{1-\gamma} \quad (1a)$$

$$\text{s.t. } \sum_{\tau=1}^L (1+r)^{1-\tau} c_\tau = \sum_{\tau=1}^L (1+r)^{1-\tau} y_\tau = \sum_{\tau=1}^R (1+r)^{1-\tau} E_\tau + \sum_{\tau=R+1}^L (1+r)^{1-\tau} B_\tau \quad (1b)$$

where c_τ denotes consumption at age τ , E_τ pre-retirement earnings, B_τ pension benefits, ρ is the discount rate, L the maximum age and γ the coefficient of relative risk aversion. The first-order condition and the budget constraint characterize the consumption path:

$$c_\tau = c_1 \left(\left(\frac{1+r}{1+\rho} \right)^{1/\gamma} \right)^{\tau-1} \quad \tau = 2, \dots, L \quad (2a)$$

$$c_1 = \left(\sum_{\tau=1}^L \lambda^{\tau-1} \right)^{-1} \left(\sum_{\tau=1}^L (1+r)^{1-\tau} y_\tau \right) \quad (2b)$$

where $\lambda = \frac{((1+r)/(1+\rho))^{1/\gamma}}{1+r}$. By definition, wealth at the end of period t , A_t is equal to accumulated saving. Using (2a) and (2b), we can write this as

$$\begin{aligned} A_t &= \sum_{\tau=1}^t (1+r)^{t-\tau} (y_\tau - c_\tau) \\ &= \sum_{\tau=1}^t (1+r)^{t-\tau} y_\tau - Q(\lambda, t) \sum_{\tau=1}^L (1+r)^{t-\tau} y_\tau \end{aligned} \quad (3)$$

where

$$Q(\lambda, t) = \left(\frac{\sum_{\tau=1}^t \lambda^{\tau-1}}{\sum_{\tau=1}^L \lambda^{\tau-1}} \right) \quad (4)$$

is the so-called "Gale's Q" (see Gale (1998) and Engelhardt and Kumar (2011)). Using (1b), equation (3) can be rewritten as

$$A_t = \left(\sum_{\tau=1}^t (1+r)^{t-\tau} y_\tau - Q(\lambda, t) \sum_{\tau=1}^R (1+r)^{t-\tau} E_\tau \right) - Q(\lambda, t) \sum_{\tau=R+1}^L (1+r)^{t-\tau} B_\tau \quad (5)$$

The term $\sum_{\tau=R+1}^L (1+r)^{t-\tau} B_\tau$ denotes pension wealth at age t , i.e. the present value of pension benefits.

2.1 Empirical implementation

Expression (5) leads to the following equation to be estimated for the sample of retired and non-retired individuals:

$$A_t = \beta_0 + \beta_1 z_{1t}^* + \beta_2 z_{2t}^* + \mathbf{x}_t' \boldsymbol{\gamma} + \varepsilon_t \quad (6)$$

where

$$z_{1t}^* = \sum_{\tau=1}^t (1+r)^{t-\tau} y_\tau - Q(\lambda, t) \sum_{\tau=1}^R (1+r)^{t-\tau} E_\tau$$

$$z_{2t}^* = Q(\lambda, t) \sum_{\tau=R+1}^L (1+r)^{t-\tau} B_\tau \text{ ("Q adjusted pension wealth")}$$

\mathbf{x}_t = a vector of demographic household characteristics that might affect savings.

The main parameter of interest is β_2 , which measures the extent of displacement between discretionary household wealth and pension wealth. The canonical life-cycle model sketched above predicts full displacement ($\beta_2 = -1$) and $\beta_1 = 1$. However, the extent of displacement might be smaller because of factors which are not considered in the canonical model such as (binding) liquidity constraints, uncertainty, endogeneity of the

retirement decision and lack of financial literacy. Gale (1998) and Engelhardt and Kumar (2011) also use equation (6) as the basis of their empirical work. In the earlier literature (see e.g. Jappelli (1995) and Hubbard (1986)) the pension wealth variable is typically not interacted with the adjustment factor Q . Gale (1998) points out that this might lead to a considerable underestimation of the crowding out effect. At the same time, Gale (1998, p. 711) shows that the Q -adjustment is also valid even if the true model does not embody perfect offset.

One of the attractive features of the SHARE survey is that it contains sufficient retrospective and prospective information to proxy the variables z_{1t}^* and z_{2t}^* in a convincing way without relying on too many arbitrary assumptions. Gale (1998), who uses the 1983 wave of the Survey of Consumer Finances (SCF), instead does not observe directly the present value of past and current earnings (i.e the first term of z_{1t}). He therefore replaces the z_{1t}^* regressor in equation (6) with the following variables: current income, age of the head of household and his/her spouse and earnings interacted with age and other demographic factors. This approximation procedure, which is also used in many other studies, might provide rather imprecise proxies and consequently might lead to an inconsistent estimate of the displacement effect. In Appendix C, we show the results when applying Gale (1998)'s method to the SHARE dataset, and discuss the advantages of retrospective survey data for dealing with (correlated) measurement errors. As far as we know, Engelhardt and Kumar (2011) is the only other study to use a direct measure for the present value of past earnings, which stems from administrative records and is consequently precisely measured.

As we said before, our empirical specification is based on a very stylized version of the life cycle model. Blau (2011) formulates a richer economic model which takes into account, amongst other things, endogenous retirement choice, uncertainties and stochastic income profiles. He uses his model to generate a simulated dataset on which he fits the linear specification of Gale. He finds that this linear model over-estimates the crowd-out effect. However, Blau shows that the coefficient for pension wealth is much closer to the true displacement effect, if one adds lagged wealth to the static model of Gale. The advantage of the dynamic specification is that it controls for initial conditions such as the present value of past earnings. We believe that our model is more similar to the dynamic specification because we control for lifetime earnings in the equation.

Our first results were rather disappointing and completely refuted the basic life-cycle model: we found a negative OLS estimate for β_1 and a positive estimate for β_2 . However, we argue that these results could be driven by serious measurement error problems: instead of z_{1t}^* and z_{2t}^* , we observe the error ridden variables z_{1t} and z_{2t} :

$$z_{kt} = z_{kt}^* + \eta_{kt}, \quad k = 1, 2 \quad (7)$$

As we explain in more detail in Section 3, there are two main reasons for measurement errors in these variables. First, the wage earned (or pension benefit received) may be reported incorrectly. Second, we interpolate the wages and extrapolate pension benefits to compute the lifetime wage path and pension wealth, which is obviously a simplification of reality. Moreover, it is rather likely that in our data the measurement errors η_1 and η_2 are positively correlated with each other: $Cov(\eta_{1t}, \eta_{2t}) \geq 0$. In Appendix B, we derive the bias in the estimators $\hat{\beta}_1^{OLS}$ and $\hat{\beta}_2^{OLS}$, and show that these biases are potentially large

enough to explain the rejection of the lifecycle model. In particular, positive correlation between the measurement errors exacerbates the standard attenuation bias, pushing coefficients further away from the true values, and potentially yielding opposite signs. The OLS estimate of the displacement effect presented by Engelhardt and Kumar (2011) also suggests pensions wealth crowds in non-pension wealth. We believe that their OLS estimate of the displacement effect is severely upward biased because the measurement errors in their right hand side variables "current earnings" and "Q adjusted pension wealth" are likely to be positively correlated.¹

In order to be able to sign the bias associated with the measurement error problem, we impose the restriction $\beta_1 = 1$ in the estimation, yielding the specification

$$A_t - z_{1t} = \beta_0 + \beta_2 z_{2t} + \mathbf{x}'_t \gamma + \varepsilon_t - \eta_{1t} - \beta_2 \eta_{2t} \quad (8)$$

This restriction stems from the simple lifecycle model. With this restriction, we can show that under the validity of the lifecycle model ($\beta_2 = -1$), our estimate of the displacement effect, $\hat{\beta}_2^{OLS}$, albeit still biased, has the same sign as the true value of the displacement effect (see Appendix B, equation 20). However, if there is only partial displacement ($-1 < \beta_2 < 0$), we cannot determine the direction (upward or downward) of the bias in the OLS estimate. In the empirical section we will carry out a sensitivity analysis in which we estimate our restricted model on the subsample of retirees. As we will explain in the next section, for this subsample the measurement errors in z_{1t}^* and z_{2t}^* are likely to be uncorrelated. In that case, the estimate of the displacement coefficient will be attenuated irrespective of the true value of β_2 , hence giving a lower bound to the true offset (see equation 22). Our main result still concerns the full sample of retirees and non-retirees, as even in the presence of measurement error in pension wealth, we expect that the estimate of the displacement coefficient is negative.

Alternatively, in order to address the measurement error problem, one could opt for IV estimation as in Engelhardt and Kumar (2011). Like Attanasio and Brugiavini (2003) and Attanasio and Rohwedder (2003), they point out that Q adjusted pension wealth should be instrumented for other reasons, such as omitted variable bias resulting from unobserved heterogeneity. For instance, some 'patient' households may have a high taste for saving. We pursue this strategy in Section 4.1. In all cases, to limit the impact of outliers (e.g. due to measurement error), we use robust and median regression techniques to estimate β_2 and γ .

¹Engelhardt and Kumar (2011) do not have administrative data on the second term $(Q(\lambda, t) \sum_{\tau=1}^R (1+r)^{t-\tau} E_\tau)$ in z_{1t}^* but proxy this regressor by a survey measure of current earnings, age, expected retirement age and region of birth plus some interaction terms. They address the measurement error in the pension wealth variable by adopting IV estimation. However, they do not take into account that the measurement error in current earnings might affect their estimate of the displacement effect.

3 Data

In our empirical analysis we use data from the Survey of Health, Ageing and Retirement in Europe (SHARE). The SHARE project started with wave 1 in 2004/05, collecting information on the current socio-economic status (income, wealth, housing), health and expectations of European individuals aged 50 and over and their partners. A first longitudinal follow-up was collected with wave 2 in 2006/7, when new countries joined the project and a refresher sample was added to maintain the representativeness of the survey. In 2008/2009 the third wave of data collection, known as SHARELIFE, asked all previous respondents (waves 1 and 2) and their partners to provide information not on their current situation but on their entire life-histories. The retrospective information ranges from childhood health to relationships to housing to work careers.² SHARELIFE interviewed 15,170 females and 11,666 males in 17,901 households and was conducted in thirteen European countries: Austria, Germany, Sweden, the Netherlands, Spain, Italy, France, Denmark, Greece, Switzerland, Belgium, the Czech Republic and Poland.

Our analytical sample consists of 3,590 males born between 1931 and 1952, aged 55-75 in the interview year of wave 2. We restrict the sample to males as we would need to make many assumptions for broken careers, typical for women with children. The literature discussed in Section 1 focuses on males as well. In our sample selection, we drop those individuals who never worked or did not report any wage in SHARELIFE (2,012 cases), and respondents aged below 55 or above 75 (2,581 cases) to have a sample consisting of individuals around retirement. We keep persons that have been self-employed at any stage during their career, but drop those that worked for less than 20 years (97 cases) to exclude the disabled. We exclude males for whom only one wage point is available (1,670 cases), and retirees with missing pension benefits or workers with missing expected replacement rates (1,592 cases). We trim compounded labour income and pension wealth by 1% from above and below in each country to end up with our final sample of 3,590 observations. All monetary amounts are expressed in PPP-adjusted 2006 German Euros, irrespective of in which country and in which year these amounts were earned. To estimate equation (8), we compute the following variables.

- *Non-pension wealth, A_t* , is mostly obtained from wave 2. We resort to information from wave 1 only for those individuals who dropped out of the survey in wave 2 but were then retrieved in SHARELIFE. In our analysis we use both household net worth and net financial wealth as dependent variables. According to Gale (1998, p. 713) a narrow measure of non-pension wealth, such as financial wealth, may be unable to detect much of the displacement, as pension wealth is accumulated over a long period. On the other hand, Hurd et al. (2012, p. 10) argue that financial wealth is more liquid than real wealth and hence more prone to being displaced by pension wealth. Our measure of net financial wealth is equal to gross financial assets (bank accounts, government and corporate bonds, stocks, mutual funds, individual retirement accounts, contractual savings for housing and the face value of life insurance policies) minus financial liabilities. Net worth is the sum of net finan-

²Börsch-Supan et al. (2011) characterizes the data and presents the first descriptive statistics.

cial wealth and real wealth, where the latter is the sum of the value of the primary residence net of the mortgage, the value of other real estate, owned share of own business and owned cars. Missing values for each of the components of wealth are replaced by five simulated versions, following multiple imputation techniques (Christelis, 2011). In total, for 56% of the analytical sample one of the separate components of net worth has been imputed, although for less than 15% of the sample more than one component was imputed. All equations are estimated using multiple imputations techniques.

- *Compounded labour income*, $z_{1t} = \sum_{\tau=1}^t (1+r)^{t-\tau} E_{\tau}$, is calculated from SHARELIFE.

The job history section in SHARELIFE asks the respondents to provide start and end dates of each job the respondent has held, as well as the first monthly wage after taxes. For the self-employed, monthly income from work after taxes is asked instead. The respondent also identifies his main job during his career. For the retirees, the last monthly net wage (or, for the self-employed, net income from work) of the main job is asked. For those that are still employed at the time of the SHARELIFE interview, the current wage is asked instead. We use the data to construct a panel with one observation per year per individual, from birth to the wave 2 interview year. The wage path is obtained using linear interpolation between the first wage on each job, the last wage of the main job and the current wage for the employed. For those still working in wave 2, we use the wage in that year as an additional point on the wage path.³ As for non-pension wealth, these wages have been imputed in case of missing values (9%). During unemployment years, we assign the respondent a wage equal to 80% of their last earnings. We convert all incomes to annual PPP-adjusted German Euros of 2006 following the procedure explained in Trevisan et al. (2011). Period 1 is taken to be the start of the working career, and we compound up to the wave 2 interview year for the employed⁴, and the year before receiving retirement benefits for the retired⁵, using an annual real interest rate of 3%, as in Hurd et al. (2012) and Attanasio and Rohwedder (2003). After compounding, we have a cross-sectional dataset, with one observation per individual, as observed in the interview year of wave 2.

- *Future labour income*, $\sum_{\tau=t+1}^R (1+r)^{t-\tau} E_{\tau}$, which needs to be calculated only for

³One important difference between the first two survey waves is that wages and pensions were elicited gross (before taxes) in wave 1, and net in wave 2, which is why we only use wave 2 information to generate our main variables.

⁴We use the term employed to denote the non-retired, although it is not necessary to be actually employed in wave 2 due to e.g. unemployment. Also, this term includes the currently self-employed.

⁵For the retired, this means that the dependent variable is $A_t - z_{1R}$, and hence these two components are measured at different ages. We made this assumption to prevent correlated measurement errors, which would otherwise (using $A_t - z_{1t}$) obviously arise for the retired subsample. Moreover, we have selected respondents around retirement, which means this assumption should not much affect our results.

the employed sample, is computed under the assumption of constant real wages ($y_\tau = y_t$, $\tau = t + 1, \dots, R$). Retirement starts in the year in which the individual reaches his expected retirement age, obtained from wave 2, or the statutory retirement age (65 in each country except France (60) and Czech Republic (62) in 2007, as reported in Angelini et al. (2009)) in case of item non-response to that question. We use country-specific 2006 life tables from the Human Mortality Database (www.mortality.org) to weight all future incomes by the probability of survival.

- *Pension wealth*, $z_{2t} = \sum_{\tau=R+1}^L (1+r)^{t-\tau} B_\tau$, for the retired is calculated under the assumption of constant real pension benefits, which is more or less in line with pension systems in the countries we study. The level of benefits is taken primarily from SHARELIFE, and wave 2 pension benefits are used in case of item non-response (13% of the analytical sample). For the employed, we use the expected replacement rate⁶ from wave 2, multiplied by current wage, to obtain expected pension benefits⁷. Again, all future incomes are weighted by survival rates and we assume a maximum age of 110.
- *Pension wealth adjustment*, $Q(\lambda, t)$ is computed using expression 4, with $r = \rho = 0.03$ (or $\lambda = 1.03^{-1}$).
- *Explanatory variables*, x_t , include a set of indicator variables to capture differences across households. Specifically, we include an indicator for higher education (ISCED ≥ 4 , post-secondary and tertiary education), medium education (ISCED=3, secondary education), aged 55-60, aged 70-75, married, no children, self-reported bad health, second earner in the household, and spells without work during the career. In other specifications, we control additionally for inheritances received in the past using both an indicator and the amount; an indicator for being retired; or characteristics (education and health) of the spouse. All regressions have a full set of country fixed effects, with Germany as the base country.

Measurement errors are likely to exist in both compounded labour income (z_{1t}) and in pension wealth (z_{2t}), due to reporting errors in income or due to the inter- and extrapolation we use in compounding. We emphasize that compounded labour income and pension wealth are partly computed from the same set of questions when using the full sample of respondents. Specifically, we use current income for employees both in constructing compounded labour income and in constructing pension wealth. Therefore, for

⁶The exact question to elicit the expected replacement rate for old age pensions, occupational pensions or early retirement benefits is stated as follows: "Please think about the time in which you will start collecting this pension. Approximately, what percentage of your last earnings will your pension amount to?". We take the maximum replacement rate from these pension categories as the individual's expected replacement rate. Given our age selection (55-75), we believe that the employed respondents provide sensible answers to this question.

⁷For those that retired between waves 2 and 3, we take their pension benefit as reported in SHARELIFE.

the group of employees, measurement errors are likely to be positively correlated. On the contrary, for the retired subsample, z_{1t} and z_{2t} are computed from two different sets of questions. While both are likely measured with error, these errors are less likely to be correlated. We use this observation to conduct a sensitivity analysis in Section 4 by selecting only the retired subsample.

3.1 Sample characteristics

Table 1 shows sample statistics for the two main variables obtained from the retrospective survey, annual labour income and annual pension income, as well as for net worth and financial wealth, by country and work status. We compute average annual labour income as the sum of all annualized wages divided by years worked⁸; annual pension income is equal to the sum of pension incomes until death divided by remaining life expectancy.⁹ We emphasize that the amounts reported here are for one earner only, hence household labour income or pension income is likely to be higher. Furthermore, the amounts, although corrected for inflation and currency devaluations, could have been earned already in the 1950's, and hence are relatively low compared to current earnings. The cross-country pattern of median labour incomes is encouraging, we believe, for the reliability of retrospective data; countries like Poland and the Czech Republic have considerably lower wages and pensions compared to Western European countries, while wages and pensions in Switzerland are higher.

We also investigate the dynamic properties of earnings by estimating age-earnings profiles by country group: North represents Sweden, Denmark and the Netherlands, Mid-West includes Austria, Germany, Switzerland, France and Belgium, South includes Spain, Italy and Greece and East represents Poland and Czech Republic. In particular, we estimate a regression of the log of monthly real wage (in €1,000) on a 4th-order polynomial in age, for both low and high educated individuals. We use a fixed-effects specification to deal with unobserved heterogeneity. Figure 1 shows the implied age-earnings profiles. Earnings for low-educated individuals are lower than for high-educated persons, as expected. Moreover, we observe a more hump-shaped profile for high-educated, with wages rising faster in early ages. From what we know from earlier literature, these patterns are not surprising, and provide evidence in favor of retrospective earnings information.¹⁰

Table 2 shows sample statistics for the remaining variables used in this study. 69% of

⁸Note that this is similar to our measure of compounded labour income, using $r = 0$ instead, and dividing by years worked.

⁹Remaining life expectancy is calculated using the country-specific mortality rates, conditioning on survivorship until the real age at the wave 2 interview year.

¹⁰We do not correct for cohort effects and labor supply effects (e.g. reduced hours of work later in life). Given our sample selection (20 year-of-birth cohorts and men with at least 20 years of work experience), these are not likely to distort the age-earnings profiles much.

Figure 1: Age-earnings profiles by education level

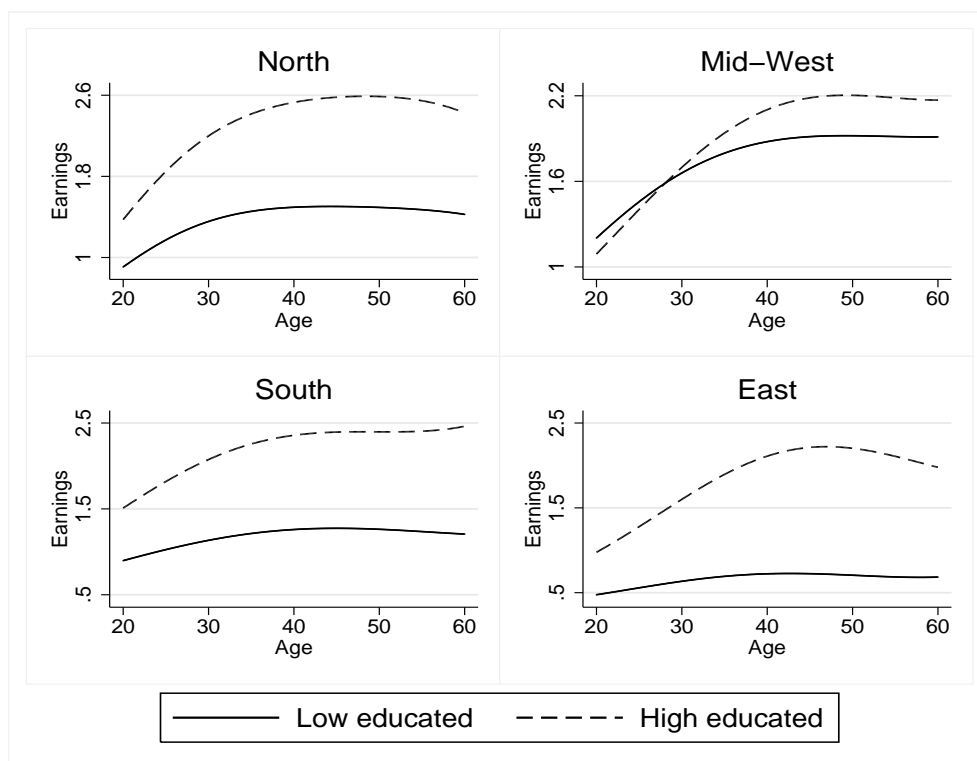


Table 1: Medians by country and retirement status

| Country | Annual labour income | | Annual pension income | | Wealth | | Observations |
|----------------|----------------------|---------|-----------------------|---------|-----------|-----------|--------------|
| | Working | Retired | Working | Retired | Net worth | Financial | |
| Austria | 20,786 | 16,236 | 21,151 | 13,209 | 180,990 | 17,698 | 123 |
| Germany | 24,226 | 17,922 | 21,999 | 11,669 | 221,174 | 36,426 | 365 |
| Sweden | 24,747 | 20,765 | 16,046 | 12,272 | 206,176 | 57,980 | 341 |
| Netherlands | 23,810 | 16,526 | 24,122 | 11,973 | 222,288 | 39,273 | 334 |
| Spain | 16,954 | 15,603 | 19,710 | 9,149 | 302,695 | 6,827 | 176 |
| Italy | 17,255 | 12,650 | 14,421 | 9,853 | 212,103 | 8,169 | 486 |
| France | 26,268 | 24,582 | 18,400 | 15,516 | 325,397 | 36,672 | 256 |
| Denmark | 23,778 | 19,153 | 14,701 | 9,524 | 216,381 | 69,687 | 328 |
| Greece | 22,914 | 16,304 | 16,695 | 12,939 | 216,650 | 2,917 | 119 |
| Switzerland | 38,930 | 33,455 | 25,051 | 20,434 | 305,083 | 99,882 | 221 |
| Belgium | 22,559 | 18,552 | 18,258 | 12,968 | 304,183 | 54,116 | 398 |
| Czech Republic | 11,375 | 9,369 | 8,226 | 5,794 | 107,005 | 8,218 | 305 |
| Poland | 8,507 | 8,056 | 7,754 | 5,349 | 58,597 | 1,946 | 138 |
| Total | 22,733 | 16,441 | 17,016 | 10,723 | 217,488 | 25,672 | 3,590 |

Table shows the median values for annualized labour and pension incomes obtained from the retrospective survey, by country and retirement status, as well as the levels of wealth obtained from wave 2. All amounts are in PPP-adjusted German Euros of 2006.

the sample is retired at the time of the wave 2 interview, while only 0.3% is unemployed. On average, the males in our analytical sample have only one year of unemployment, and have been working for 40 years. The vast majority is married, and 61% have a second earner in the household.

Table 2: Sample characteristics

| Variable | Mean | SD |
|---|------|------|
| Age | 63.7 | 5.5 |
| % Retired | 69.3 | |
| % Working | 30.4 | |
| Actual retirement age (retired) | 59.1 | 4.6 |
| Expected retirement age (working) | 63.2 | 2.5 |
| Actual replacement rate (% , retired) | 70.1 | 35.0 |
| Expected replacement rate (% , working) | 66.4 | 17.4 |
| Years worked | 40.3 | 5.5 |
| Years not worked | 1.2 | 2.6 |
| Gale's Q | 0.5 | 0.1 |
| % High educated | 29.7 | |
| % Medium educated | 33.4 | |
| % Married | 88.2 | |
| % Second earner | 61.3 | |
| % Bad health | 26.2 | |
| % Inheritance received | 36.4 | |
| Amount inherited (\times €1,000) | 14.2 | 49.4 |

Table shows the mean and standard deviation of household characteristics.
 $N=3,590$ except for retired ($N=2,487$) or working ($N=1,103$) specific variables.

4 Results

We estimate the model represented in equation (8) both using robust regression and median regression techniques, as Gale (1998) does. Since wages and pension benefits from wave 2 and the measures of non-pension wealth have been imputed five times in case of missing values, we use multiple imputation techniques to obtain the correct coefficients and standard errors (Little and Rubin, 2002).¹¹ The results are presented in Table 3. Our controls include two age dummies¹², marital status, presence of children, education, health, the country of residence and indicators for whether in the family there has been a second income earner and whether there were years of unemployment in the working career, as well as country fixed effects. These coefficients are reported in Appendix A, Table 6. For median regression, standard errors are based on 1000 bootstrap replications.

¹¹If $\hat{\beta}_m$ and \hat{V}_m denote the vector of parameter estimates and variance matrix for imputation m , respectively, the estimates equal $\hat{\beta} = \frac{1}{5} \sum_{m=1}^5 \hat{\beta}_m$ with variance matrix $\hat{V} = \frac{1}{5} \sum_{m=1}^5 \hat{V}_m + \frac{3}{10} \sum_{m=1}^5 (\hat{\beta}_m - \hat{\beta})(\hat{\beta}_m - \hat{\beta})'$, which takes into account both within- and between-imputation variance.

¹²As we estimate a cross-sectional regression, we cannot distinguish between age, cohort and time effects.

Table 3: Estimates of the displacement effect

| Variables | Robust regression | | | Median regression | | |
|----------------------------|-----------------------|----------------------|---------------------|----------------------|-------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Full sample | Retired sample | Old sample | Full sample | Retired sample | Old sample |
| Pension wealth | -0.471*** (0.0878) | -0.205** (0.0936) | -0.173* (0.0965) | -0.609*** (0.151) | -0.296 (0.180) | -0.306* (0.177) |
| Observations | 3590 | 2487 | 2415 | 3590 | 2487 | 2415 |
| p -value $\beta_2 = -1$ | 0.000 | 0.000 | 0.000 | 0.011 | 0.000 | 0.000 |
| p -value Country effects | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
 Bootstrapped standard errors for median regression, 1000 replications.

Our results for the full sample imply an estimated offset¹³ between 47.1% and 60.9% depending on the estimation method: the offset is significantly different from zero at all conventional levels and significantly different from 100%, although not at the 1% level in the case of median regression (columns (1) and (4) of Table 3). As Gale (1998), we also find that robust regression estimates of the offset are qualitatively the same as median regression estimates but quantitatively smaller. The control variables are mainly insignificant, with the exception of the indicator for gaps in the career, resulting in less wealth, and strongly significant age effects. Although insignificant of, for example, education may seem surprising, we emphasize that education correlates with compounded labour income, included in our regressions. The country-fixed effects are highly significant.

As argued in Section 2.1, the estimates for the full sample are likely to be biased, away from zero due to the fact that measurement errors in z_{1t} and z_{2t} are possibly correlated for the non-retired and towards zero due to measurement error in pension wealth. Since these biases work in opposite direction, we can only hope that these balance out on aggregate. In columns (2) and (5) we report the estimated crowd-out for the group of retirees. For this group, as argued above, the correlation between the measurement errors in compounded labour income and pension wealth should be considerably smaller or even negligible for this group, and hence, the estimate should only be affected by attenuation bias due to measurement error in pension wealth. Therefore, we can consider the estimates for the group of retirees as a lower bound for the true offset. Indeed, we find that the attenuation bias gives parameter estimates towards zero, and hence a lower estimated offset compared to the full sample results. The estimated displacement effect is significantly different from zero only with robust regression.

One issue with selecting the sample of retirees is that, although we do not explicitly model the retirement decision, it might be endogenous. Therefore, in columns (3) and (6), we do not select the sample based on retirement status, which could lead to endogenous

¹³The offset is simply the negative of the estimated coefficient for pension wealth.

sample selection and hence inconsistent parameter estimates, but using an age criterion: those aged 60 or below are dropped independent of retirement status (in our sample average retirement age is 59.1, see Table 2). In the remaining group of 2415 males, around 90% is retired, compared to 70% in our baseline results. Effectively, for this old sample, the effect of correlated measurement errors should be similar to selecting only the retirees, which is confirmed by the parameter estimates. The estimated offset is between 17.3% and 30.6%, and is significantly different from zero at the 10% level.¹⁴

Table 4: Robustness checks displacement effect

| Variables | (1) Financial wealth, full | (2) Financial wealth, retired | (3) Financial wealth, old | (4) Inheritances received | (5) Partner's characteristics | (6) Low educated | (7) High educated | (8) No occupational pensions |
|---------------------------|----------------------------------|-------------------------------------|---------------------------------|---------------------------------|-------------------------------------|------------------------|-------------------------|------------------------------------|
| Robust regression | -0.778*** (0.0738) | -0.614*** (0.0734) | -0.532*** (0.0779) | -0.527*** (0.0877) | -0.488*** (0.0876) | -0.215* (0.122) | -0.833*** (0.153) | -0.380*** (0.121) |
| p -value $\beta_2 = -1$ | 0.003 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.277 | 0.000 |
| Median regression | -0.870*** (0.114) | -0.692*** (0.121) | -0.618*** (0.118) | -0.618*** (0.163) | -0.660*** (0.162) | -0.275 (0.192) | -1.099*** (0.286) | -0.740*** (0.226) |
| p -value $\beta_2 = -1$ | 0.253 | 0.0130 | 0.001 | 0.0210 | 0.0420 | 0.000 | 0.729 | 0.253 |
| Observations | 3590 | 2487 | 2415 | 3590 | 3590 | 3590 | 3590 | 1823 |

Standard errors in parentheses; 1000 bootstrap replications for median regression; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table shows the coefficient for pension wealth from a regression similar to Table 3 with the following modifications:

(1) using financial wealth as dependent variable, full sample, (2) using financial wealth as dependent variable, retired sample, (3) using financial wealth as dependent variable, old sample, (4) controlling for received inheritances (binary and amount), (5) controlling for partner's education and health status, (6) and (7) interacting all covariates with the high-education dummy and (8) excluding countries with large occupational pensions

We check the robustness of our results in Table 4 (see Tables 7 and 8 for detailed results). In columns (1) to (3) we consider net financial wealth rather than total net worth and we include housing wealth among the control variables.¹⁵ The reason for doing so is that, according to Hurd et al. (2012), real wealth is mostly illiquid and its accumulation is likely to be driven by motives other than retirement planning. Housing in particular may be a consumption rather than an investment good, and as such affect the displacement effect. When we use financial wealth, for the full sample we cannot reject the hypothesis of full displacement using median regression. This result is in contrast with the findings of Gale (1998), according to which the offset is larger when using broader measures of wealth. For the sample of retirees, we find that for financial wealth the displacement effect is significantly different from zero, while for net worth this was true only using robust regression. Again, using the reasoning of Section 2.1, these estimates may be interpreted as lower bounds for the true offset, and hence we reject the hypothesis of no

¹⁴For both the samples of retirees and older males, the difference with the full sample estimates might be partly driven by cohort effects, although these are likely small given our age restriction in the full sample (55-75 years old).

¹⁵We have carried out our estimations including other forms of non-financial wealth as well as not controlling for housing wealth. The results are virtually unchanged (they are available upon request from the authors).

displacement. As expected, the offset for the old sample is very similar in magnitude to that estimated for the sample of retirees.

In the remaining robustness checks we focus only on the full sample because the results are qualitatively unchanged when we select the retirees or the old sample (they are available upon request from the authors).¹⁶ In columns (4) and (5) we add to our specification other explanatory variables that might be relevant in determining non-pension wealth. In particular, in column (4) we control for whether the individual has ever received inheritances or gifts worth more than €5,000 during his life and the total amount received. Indeed, for some individuals inheritances and monetary gifts might be an important component of non-pension wealth. Our results show that, although these variables are highly significant with the expected positive sign (see Tables 7 and 8), the estimated offset is still in the same range as before and significantly different from 0 and 100%. Column (5) shows that including controls for the education level and health status of the partner does not affect our main results.

As in Gale (1998) and Engelhardt and Kumar (2011), in columns (6) and (7) we interact all covariates with the high-education dummy, and report the estimated displacement effect for the high- and low educated groups¹⁷. We find that the offset is not significantly different from 100% for the highly educated, while the displacement effect is substantially lower in absolute value terms and not significantly different from zero offset for the less-educated sample. This result can be explained by the fact that individuals with higher education are more likely to be financially literate and to plan for retirement, while less educated individuals are more likely to procrastinate (see e.g. Laibson et al. (1998)).

Finally, in column (8) we exclude those countries for which occupational pensions are typically a substantial share of pension income for retirees: Germany, Sweden, Denmark, the Netherlands and Belgium. In these countries, pensions may be seen as a form of private wealth, causing wrong inference on the displacement effect. The estimated crowd-out is about 10 percentage points lower compared to our baseline result using robust regression, and 15 percentage points higher using median regression. Overall, the results do not seem to be driven by the type of pension system in a particular country. The results are also robust to leaving one country out at the time (not reported). Using robust regression, the estimated displacement effect ranges between 38.6% when The Netherlands is left out of the analysis, to 57.5% when leaving out Italy, all significantly different from zero at the 1% level.

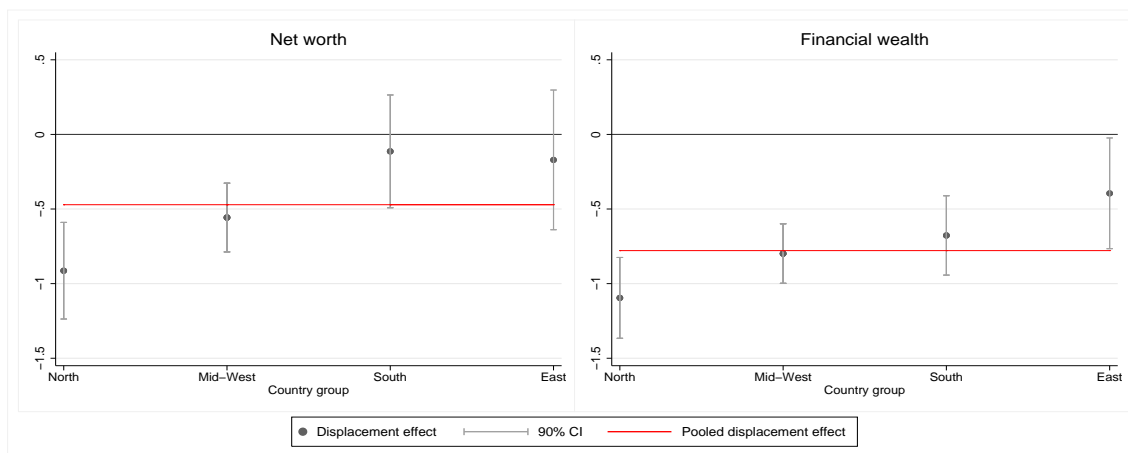
Figure 2 shows the displacement effect by country group¹⁸, where North represents

¹⁶Changing the fixed parameters r and ρ to 2% (4%) does not affect the qualitative results (not reported); the estimated offset equals 23.6% (87.7%) using robust regressions, significantly different from zero at the 1% level.

¹⁷The hypothesis of equal slope coefficients across education groups cannot be rejected for median regression ($p = 0.326$) and is marginally rejected for robust regression ($p = 0.044$).

¹⁸The sample sizes are too small to consider country-specific analysis.

Figure 2: Displacement effect across country groups



Sweden, Denmark and the Netherlands; Mid-West represents Austria, Germany, Switzerland, France and Belgium; South includes Spain, Italy and Greece and East represents Poland and Czech Republic. The estimates are obtained using robust regressions, and we plot 90% confidence intervals around the point estimates. In the Northern countries the extent of displacement of net worth is the highest (91%), and crowd-out is least in the South (11%), although the confidence intervals are wide. The pattern is similar when looking at financial wealth, although the offset is the lowest in the Eastern countries. More generous welfare systems (including social security) in the Northern countries could reduce the need to save for other reasons than retirement, such as precautionary savings. Also, more developed capital markets are likely to relax liquidity constraints. For these reasons, the displacement effect could be higher in the Northern countries compared to the Southern or Eastern European countries.

Another explanation of these findings can be found in the cross-country studies on Financial Literacy around the World¹⁹ (Lusardi and Mitchell, 2011). These “FLat World” studies investigate responses to three comparable financial literacy questions in country-specific socio-economic surveys, focusing on the concepts of interest rates, inflation and risk diversification. The results show that the Netherlands (Alessie et al., 2011) and Sweden (Almenberg and Säve-Söderbergh, 2011) score relatively well with 46.2% and 26.7% of respondents aged 25-65 answering all three questions correctly²⁰. In Germany 56.8% of the sample provides three correct answers (Bucher-Koenen and Lusardi, 2011). In Italy,

¹⁹See the special issue of the Journal of Pension Economics and Finance, Volume 10, Issue 4 (2011).

²⁰The interest rate question in Sweden was considerably more difficult compared to the other countries; the percentage of no question correct is around 10% as in the Netherlands and Germany.

28.3% answers all correctly (Fornero and Monticone, 2011), while in Russia²¹ only 3.4% answers all correctly (Klapper and Panos, 2011). Jappelli (2010) conducts a panel data study using data from the International Institute for Management Development's World Competitiveness Yearbook (IMD-WCY). Jappelli finds a positive relationship between a country's GDP per capita and its economic literacy²² using fixed-effects regressions, as well as the highest literacy scores in Denmark, Switzerland, the Netherlands and Sweden and the lowest scores in Poland, Italy and Spain. The evidence presented is certainly not exhaustive, but still seems to suggest more literate households in the Northern or Western countries, and less literacy in the Southern or Eastern countries, consistent with our results given the strong correlation between financial literacy and planning for retirement found in these same studies. Still, our cross-country results should be treated with caution, as the confidence intervals are wide, and, in fact, the group-specific estimates are never significantly different from the pooled displacement effect.

Overall, our results show that there is heterogeneity in the estimated offset across different groups of the population. However, the displacement effect is almost always significantly different from zero, indicating some degree of crowding out of private savings by pensions.

4.1 The endogeneity of pension wealth

Although we have suggested an approach to limit the effect of measurement error, our results might still be biased due to the presence of unobserved heterogeneity. For example, taste for saving is likely to influence both pension wealth and private savings. Since both the dependent and the endogenous right-hand-side variable are positively affected by the unobserved taste for saving, the estimates of the displacement effect that we have obtained so far are likely to be attenuated. Therefore, we can still conclude that there is crowding out but its magnitude might be underestimated.

We try to overcome this endogeneity problem by using an instrumental variable identification strategy, which at the same time should reduce the impact of measurement error. We construct an instrument in the same spirit of that of Engelhardt and Kumar (2011), exploiting institutional differences across countries and groups of individuals. First, we compute median²³ pension benefits by country and employment sector (employee, civil

²¹We should be cautious with comparing the Russian results. First, the Czech Republic and Poland might well score differently compared to Russia. Second, the question on inflation literacy in Russia is framed differently from the other studies but is contentwise similar, while the risk diversification question asks Russians to rate the risks of different portfolio, and the remaining countries to give a true/false answer, which may bias the results against the Russians. Also, the answer category "Refuse to answer" was missing in all Russian questions.

²²The literacy scores in the IMD-WCY are obtained by asking business leaders's and country experts's opinions on economic literacy in the population, instead of using household surveys as done in the "FLat World" studies.

²³Using average pension income by country and employment sector gives similar results, available upon request.

Table 5: IV Median regression estimates

| Variable | (1) Full sample | (2) Retired sample | (3) Old sample |
|----------------------------|--------------------|-----------------------|-------------------|
| Pension Wealth | -1.232 (0.876) | -0.622 (0.863) | -0.955 (0.813) |
| p -value $\beta_2 = -1$ | 0.804 | 0.684 | 0.960 |
| F -statistic first stage | 41.895 | 31.232 | 38.567 |
| Observations | 3590 | 2487 | 2415 |

Bootstrapped standard errors in parentheses; 1000 replications. Table shows the coefficient for pension wealth from an Instrumental Variable median regression, instrumenting pension wealth. See the text for details on the instrument.

servant and self-employed), relying on the information from the second wave of SHARE. Second, for each individual we calculate a “potential” pension wealth variable, using the relevant median benefit and the statutory retirement age that was in place at the time of retirement. Therefore, there are three sources of variation in our instrument: the country of residence, the sector of employment and the legal retirement age in place when leaving employment. For the validity of the instrument, we need to assume that, conditional on demographic characteristics, education, wealth and the country of residence, workers do not sort across employment sectors based on the taste for saving which is included in the error term. This assumption is similar to that made by Attanasio and Brugiavini (2003) and Engelhardt and Kumar (2011). Note that the instrument does not depend on any other individual characteristics which could be correlated with unobserved heterogeneity. We report results for our instrumental variables estimation in Table 5 (see Table 9 for the detailed results). There are two cautionary notes to bear in mind. First, we only present the results of IV quantile regression because the theory for IV robust regression is non-standard and we have not yet found a way to apply it to our context. Second, we employ the identification strategy of Chernozhukov and Hansen (2005), which provides consistent but imprecise estimates, a fact that has been noted by Chernozhukov and Hansen (2008) and Engelhardt and Kumar (2011) as well. Therefore, we focus only on the point estimates and not on the confidence intervals.

As expected, the estimated displacement effect is higher when correcting for the attenuation biases from endogeneity and measurement error, both if we focus on the full sample and if we consider only the retirees or the old sample. The partial F -statistic of the first stage (OLS) regression exceeds the “weak” instrument threshold of 10. The point estimates suggest full displacement, although the large standard errors yield insignificant results for pension wealth.

5 Conclusion

In this paper, we use SHARE data to come up with new estimates of the displacement effect of pensions on household wealth. The third wave of this survey, known as SHARE-LIFE, collects retrospective data on lifetime earnings, which can be linked to data on household wealth and subjective data on the expected replacement rate and retirement age collected in previous waves. Consequently, we are able to approximate in a convincing way the main variables needed to estimate the extent of crowding out between pension wealth and private wealth. In particular, we can compute both the present value of past and future income and pension wealth. According to our robust (median) regression results, each euro of pension wealth is associated with a 47 (61) cent decline in non-pension wealth. However, these results should be interpreted with caution: although we suggest an approach to limit the effects of correlated measurement errors in lifetime earnings and pension wealth, our estimates could still be biased and the direction of the bias is unclear. As Gale (1998, p. 720) stated, *“pension wealth data are of generally poor quality; all methods of calculating pension wealth in defined benefit plans are likely to create measurement error”*. For this reason, we estimate our model also on a sample of retirees and older households, for whom the information on lifetime earnings and pension wealth comes from two different sources. For this group measurement error, although present, is likely to be uncorrelated and the direction of the bias in our preferred specification is thus clear: parameter estimates are attenuated and, therefore, they can provide lower bounds to the true offset. We find that the lower bounds for the crowd-out are significantly different from zero and they range between 17% and 30%, depending on the estimation method.

We also find that the extent of the crowding out effect differs across education groups: for the low educated, we do not find any evidence of displacement whereas for the high educated pension wealth completely crowds out private wealth. Moreover, the level of displacement is limited in the Mediterranean and Eastern European countries. The IV estimates, instrumenting pension wealth to account for omitted variable bias, suggest full displacement although estimated with less precision.

Our results shed light on the impact of recent and future pension reforms in Europe. The main results suggest that European households will react to reductions in pensions by increasing private savings, although not strong enough to smooth consumption over the life-cycle. Government policy should focus especially on the less-educated and perhaps financially illiterate households, for which we have shown limited displacement.

Although we have suggested strategies to address the issues of measurement error and unobserved heterogeneity, more work needs to be done. Most notably, future waves of SHARE can be used to construct a panel data set, with which unobservable household characteristics as well as the choice of retirement date can be addressed.

A Detailed estimation results

Table 6: Full table estimation results

| Variables | Robust regression | | | Median regression | | |
|----------------------------|-----------------------|--------------------------|----------------------|-----------------------|--------------------------|----------------------|
| | (1) Full sample | (2) Retired sample | (3) Old sample | (4) Full sample | (5) Retired sample | (6) Old sample |
| Pension wealth | -0.471*** (0.0878) | -0.205** (0.0936) | -0.173* (0.0965) | -0.609*** (0.151) | -0.296 (0.180) | -0.306* (0.177) |
| Age 55-60 | -1.406*** (0.155) | -0.538** (0.207) | | -1.409*** (0.173) | -0.359 (0.226) | |
| Age 70-75 | 1.797*** (0.172) | 1.483*** (0.161) | 1.674*** (0.167) | 1.614*** (0.177) | 1.347*** (0.175) | 1.505*** (0.187) |
| Second earner | 0.128 (0.151) | 0.230 (0.159) | 0.206 (0.171) | 0.175 (0.167) | 0.189 (0.191) | 0.270 (0.188) |
| Married | 0.403* (0.233) | 0.148 (0.247) | 0.173 (0.267) | 0.332 (0.275) | -0.0066 (0.292) | -0.070 (0.304) |
| No children | 0.355 (0.254) | 0.170 (0.262) | 0.503 (0.289) | 0.469* (0.282) | 0.0579 (0.274) | 0.564 (0.293) |
| High educated | 0.0854 (0.175) | 0.229 (0.200) | 0.100 (0.202) | -0.0755 (0.197) | 0.177 (0.233) | 0.133 (0.234) |
| Medium educated | 0.255 (0.163) | 0.229 (0.186) | 0.362 (0.195) | 0.139 (0.175) | 0.233 (0.205) | 0.340 (0.211) |
| Bad health | 0.120 (0.149) | 0.0175 (0.152) | 0.179 (0.170) | 0.0712 (0.143) | 0.0674 (0.165) | 0.167 (0.163) |
| Gaps in career | -0.824*** (0.146) | -0.109 (0.180) | -0.236 (0.183) | -0.756*** (0.164) | -0.174 (0.208) | -0.275 (0.211) |
| Sweden | -0.820*** (0.287) | -0.882** (0.341) | -0.880*** (0.328) | -0.992** (0.412) | -1.104** (0.495) | -1.382*** (0.459) |
| Denmark | -0.311 (0.286) | -0.743** (0.321) | -0.716** (0.330) | -0.457 (0.369) | -0.831* (0.458) | -1.020** (0.428) |
| Netherlands | 0.528* (0.285) | 0.467 (0.322) | 0.590* (0.333) | 0.521 (0.336) | 0.365 (0.356) | 0.481 (0.346) |
| Belgium | 1.397*** (0.279) | 1.172*** (0.300) | 1.446*** (0.316) | 1.522*** (0.356) | 1.244*** (0.408) | 1.590*** (0.377) |
| France | 0.755** (0.312) | 0.295 (0.340) | 0.413 (0.389) | 0.337 (0.463) | -0.270 (0.510) | -0.267 (0.629) |
| Switzerland | -5.575*** (0.324) | -4.633*** (0.389) | -4.734*** (0.370) | -5.595*** (0.589) | -4.275*** (0.741) | -5.045*** (0.628) |
| Austria | 0.628 (0.389) | 0.362 (0.409) | 0.245 (0.442) | 0.743 (0.477) | 0.213 (0.507) | 0.197 (0.549) |
| Spain | 1.649*** (0.371) | 1.825*** (0.397) | 1.763*** (0.437) | 1.314** (0.506) | 1.544** (0.599) | 1.504** (0.585) |
| Italy | 2.477*** (0.274) | 2.237*** (0.293) | 2.563*** (0.315) | 2.325*** (0.302) | 2.171*** (0.350) | 2.431*** (0.352) |
| Greece | 2.053*** (0.400) | 1.802*** (0.465) | 2.100*** (0.490) | 1.950*** (0.742) | 1.401* (0.747) | 1.345* (0.745) |
| Poland | 3.027*** (0.377) | 2.563*** (0.408) | 2.608*** (0.472) | 2.985*** (0.330) | 2.355*** (0.340) | 2.469*** (0.363) |
| Czech Republic | 2.201*** (0.305) | 1.979*** (0.322) | 2.171*** (0.339) | 2.102*** (0.286) | 1.920*** (0.311) | 2.055*** (0.294) |
| Constant | -4.786*** (0.329) | -4.664*** (0.358) | -5.128*** (0.372) | -4.385*** (0.374) | -4.244*** (0.427) | -4.559*** (0.418) |
| Observations | 3590 | 2487 | 2415 | 3590 | 2487 | 2415 |
| p -value $\beta_2 = -1$ | 0.000 | 0.000 | 0.000 | 0.011 | 0.000 | 0.000 |
| p -value Country effects | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
 Bootstrapped standard errors for median regression, 1000 replications

Table 7: Full estimation results robustness checks, robust regression

| Variables | (1) Financial wealth, full | (2) Financial wealth, retired | (3) Financial wealth, old | (4) Inheritances received | (5) Partner's characteristics | (6) Low educated | (7) High educated | (8) No occupational pensions |
|----------------------------|----------------------------------|-------------------------------------|---------------------------------|---------------------------------|-------------------------------------|------------------------|-------------------------|------------------------------------|
| Pension wealth | -0.778*** (0.0738) | -0.614*** (0.0734) | -0.532*** (0.0779) | -0.527*** (0.0877) | -0.488*** (0.0876) | -0.215* (0.122) | -0.833*** (0.153) | -0.380*** (0.121) |
| Age 55-60 | -1.508*** (0.127) | -0.653*** (0.159) | | -1.421*** (0.154) | -1.424*** (0.155) | -1.340*** (0.178) | -1.537*** (0.293) | -1.397*** (0.204) |
| Age 70-75 | 1.898*** (0.144) | 1.579*** (0.125) | 1.784*** (0.135) | 1.820*** (0.171) | 1.812*** (0.172) | 1.733*** (0.206) | 1.862*** (0.341) | 1.390*** (0.240) |
| Second earner | -0.142 (0.123) | -0.0818 (0.123) | -0.0961 (0.137) | 0.0777 (0.149) | -0.117 (0.168) | 0.183 (0.175) | 0.151 (0.280) | 0.215 (0.203) |
| Married | 0.0023 (0.190) | -0.117 (0.191) | -0.153 (0.211) | 0.412* (0.230) | 0.255 (0.242) | 0.276 (0.271) | 0.558 (0.427) | 0.143 (0.331) |
| No children | 0.355* (0.204) | 0.239 (0.205) | 0.394* (0.233) | 0.331 (0.251) | 0.377 (0.252) | 0.346 (0.290) | 0.0912 (0.489) | 0.268 (0.345) |
| High educated | -0.677*** (0.142) | -0.397*** (0.146) | -0.557*** (0.162) | -0.0405 (0.174) | -0.0662 (0.183) | | | 0.108 (0.243) |
| Medium educated | 0.0527 (0.136) | 0.0511 (0.137) | 0.134 (0.156) | 0.201 (0.161) | 0.159 (0.165) | | | 0.152 (0.221) |
| Bad health | 0.279** (0.123) | 0.180 (0.116) | 0.292** (0.133) | 0.197 (0.148) | 0.153 (0.150) | 0.148 (0.169) | 0.0378 (0.327) | 0.287 (0.194) |
| Gaps in career | -0.810*** (0.123) | 0.0087 (0.138) | -0.234 (0.149) | -0.826*** (0.145) | -0.835*** (0.145) | -0.654*** (0.174) | -1.109*** (0.270) | -0.865*** (0.204) |
| Sweden | -0.637*** (0.237) | -0.451* (0.257) | -0.749*** (0.260) | -0.824*** (0.290) | -0.787*** (0.289) | -1.356*** (0.364) | -0.0327 (0.522) | |
| Denmark | -0.243 (0.237) | -0.646** (0.249) | -0.725*** (0.263) | -0.254 (0.284) | -0.320 (0.286) | -0.395 (0.364) | -0.206 (0.455) | |
| Netherlands | 0.520** (0.239) | 0.396 (0.259) | 0.431 (0.275) | 0.600** (0.282) | 0.604** (0.288) | 0.217 (0.363) | 0.819* (0.473) | |
| Belgium | 0.868*** (0.229) | 0.817*** (0.230) | 0.871*** (0.253) | 1.430*** (0.274) | 1.431*** (0.279) | 1.148*** (0.348) | 1.626*** (0.435) | |
| France | -0.223 (0.259) | -0.720*** (0.266) | -0.461 (0.318) | 0.830*** (0.307) | 0.818*** (0.312) | 0.829** (0.381) | 0.0730 (0.552) | |
| Switzerland | -5.555*** (0.274) | -4.195*** (0.309) | -4.753*** (0.313) | -5.695*** (0.326) | -5.517*** (0.324) | -6.236*** (0.406) | -4.760*** (0.518) | -6.393*** (0.356) |
| Austria | 0.767** (0.318) | 0.771** (0.318) | 0.560 (0.353) | 0.786** (0.383) | 0.720* (0.388) | 0.156 (0.487) | 1.537** (0.648) | -0.0631 (0.398) |
| Spain | 0.257 (0.295) | 0.340 (0.299) | 0.211 (0.327) | 1.807*** (0.366) | 1.739*** (0.369) | 1.219*** (0.415) | 3.225*** (0.955) | 0.940** (0.375) |
| Italy | 1.557*** (0.225) | 1.397*** (0.225) | 1.572*** (0.225) | 2.565*** (0.270) | 2.551*** (0.276) | 2.165*** (0.316) | 3.039*** (0.698) | 1.765*** (0.291) |
| Greece | 1.707*** (0.328) | 1.447*** (0.358) | 1.639*** (0.392) | 1.987*** (0.396) | 2.018*** (0.401) | 1.637*** (0.486) | 2.482*** (0.703) | 1.343*** (0.398) |
| Poland | 3.735*** (0.312) | 3.107*** (0.318) | 3.087*** (0.380) | 3.185*** (0.373) | 3.044*** (0.377) | 3.000*** (0.457) | 2.922*** (0.712) | 2.299*** (0.401) |
| Czech Republic | 2.459*** (0.248) | 2.161*** (0.249) | 2.267*** (0.271) | 2.296*** (0.300) | 2.240*** (0.305) | 2.021*** (0.367) | 2.249*** (0.585) | 1.470*** (0.336) |
| Constant | -5.233*** (0.274) | -5.083*** (0.279) | -5.364*** (0.297) | -4.964*** (0.328) | -4.518*** (0.372) | -4.679*** (0.396) | -4.420*** (0.514) | -3.890*** (0.436) |
| Housing wealth | -0.0035 (0.0091) | -0.0043 (0.0081) | -0.0450** (0.0179) | | | | | |
| Received inheritance | | | | 0.522*** (0.146) | | | | |
| Amount inherited | | | | 0.669*** (0.143) | | | | |
| High educated spouse | | | | | 0.458** (0.215) | | | |
| Medium educated spouse | | | | | 0.397** (0.179) | | | |
| Bad health spouse | | | | | -0.223 (0.157) | | | |
| Observations | 3590 | 2487 | 2415 | 3590 | 3590 | 3590 | 3590 | 1823 |
| p -value $\beta_2 = -1$ | 0.003 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.277 | 0.000 |
| p -value Country effects | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

See Table 4. Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
In column (8), France is used as the baseline country.

Table 8: Full estimation results robustness checks, median regression

| Variables | (1) Financial wealth, full | (2) Financial wealth, retired | (3) Financial wealth, old | (4) Inheritances received | (5) Partner's characteristics | (6) Low educated | (7) High educated | (8) No occupational pensions |
|---------------------------------------|----------------------------------|-------------------------------------|---------------------------------|---------------------------------|-------------------------------------|------------------------|-------------------------|------------------------------------|
| Pension wealth | -0.870*** (0.114) | -0.692*** (0.121) | -0.618*** (0.118) | -0.618*** (0.163) | -0.660*** (0.162) | -0.275 (0.192) | -1.099*** (0.286) | -0.740*** (0.226) |
| Age 55-60 | -1.477*** (0.154) | -0.433*** (0.163) | | -1.399*** (0.170) | -1.483*** (0.184) | -1.238*** (0.196) | -1.943*** (0.404) | -1.390*** (0.217) |
| Age 70-75 | 1.948*** (0.143) | 1.776*** (0.132) | 1.891*** (0.138) | 1.674*** (0.177) | 1.629*** (0.186) | 1.550*** (0.209) | 1.652*** (0.437) | 1.207*** (0.216) |
| Second earner | -0.143 (0.145) | -0.144 (0.145) | -0.0901 (0.162) | 0.119 (0.167) | -0.102 (0.184) | 0.165 (0.185) | 0.258 (0.406) | 0.423** (0.211) |
| Married | -0.128 (0.203) | -0.110 (0.196) | -0.194 (0.215) | 0.348 (0.253) | 0.190 (0.281) | 0.182 (0.286) | 0.524 (0.673) | -0.178 (0.378) |
| No children | 0.226 (0.222) | 0.216 (0.197) | 0.267 (0.197) | 0.290 (0.249) | 0.522* (0.290) | 0.407 (0.277) | 0.179 (0.617) | 0.180 (0.347) |
| High educated | -0.818*** (0.171) | -0.604*** (0.174) | -0.717*** (0.198) | -0.182 (0.206) | -0.267 (0.204) | | | 0.0925 (0.263) |
| Medium educated | -0.0997 (0.141) | -0.0322 (0.136) | 0.0255 (0.150) | 0.131 (0.168) | 0.0844 (0.177) | | | 0.0550 (0.214) |
| Bad health | 0.206 (0.126) | 0.167 (0.119) | 0.231 (0.140) | 0.134 (0.136) | 0.102 (0.145) | 0.140 (0.169) | -0.262 (0.391) | 0.163 (0.180) |
| Gaps in career | -0.796*** (0.143) | -0.0446 (0.136) | -0.237 (0.162) | -0.796*** (0.168) | -0.780*** (0.175) | -0.617*** (0.185) | -0.982** (0.384) | -0.742*** (0.210) |
| Sweden | -1.376*** (0.299) | -1.255*** (0.385) | -1.596*** (0.352) | -0.947** (0.396) | -1.077** (0.413) | -1.700*** (0.472) | -0.326 (0.683) | |
| Denmark | -0.476* (0.254) | -0.722** (0.341) | -0.945*** (0.326) | -0.352 (0.357) | -0.490 (0.370) | -0.429 (0.433) | -0.646 (0.626) | |
| Netherlands | 0.531** (0.230) | 0.373 (0.280) | 0.412 (0.256) | 0.693** (0.314) | 0.592* (0.320) | 0.349 (0.373) | 0.904 (0.577) | |
| Belgium | 0.815*** (0.234) | 0.788*** (0.269) | 0.843*** (0.272) | 1.537*** (0.374) | 1.543*** (0.395) | 1.576*** (0.407) | 1.174* (0.619) | |
| France | -0.991** (0.416) | -1.484*** (0.511) | -1.453*** (0.613) | 0.399 (0.408) | 0.462 (0.456) | 0.515 (0.438) | -0.240 (1.000) | |
| Switzerland | -6.319*** (0.537) | -4.428*** (0.455) | -5.137*** (0.648) | -5.800*** (0.595) | -5.586*** (0.603) | -6.190*** (0.766) | -5.061*** (1.012) | -5.889*** (0.650) |
| Austria | 0.593 (0.492) | 0.303 (0.526) | 0.0036 (0.495) | 0.944** (0.418) | 0.790 (0.480) | 0.519 (0.527) | 1.455 (0.938) | 0.367 (0.581) |
| Spain | -0.215 (0.400) | -0.0919 (0.406) | -0.127 (0.413) | 1.587*** (0.514) | 1.430** (0.581) | 1.065* (0.596) | 2.303 (1.522) | 1.093* (0.586) |
| Italy | 1.677*** (0.227) | 1.501*** (0.222) | 1.698*** (0.228) | 2.487*** (0.297) | 2.406*** (0.328) | 2.122*** (0.341) | 2.754*** (0.803) | 2.062*** (0.401) |
| Greece | 0.935* (0.506) | 0.858* (0.486) | 0.886* (0.475) | 1.503** (0.726) | 1.864** (0.772) | 1.343* (0.804) | 2.703* (1.578) | 1.354* (0.692) |
| Poland | 3.615*** (0.260) | 2.954*** (0.248) | 3.083*** (0.288) | 3.188*** (0.333) | 2.949*** (0.342) | 2.973*** (0.363) | 2.711*** (0.696) | 2.480*** (0.435) |
| Czech Republic | 2.284*** (0.217) | 2.163*** (0.213) | 2.201*** (0.217) | 2.291*** (0.293) | 2.096*** (0.296) | 2.141*** (0.335) | 1.728*** (0.619) | 1.667*** (0.520) |
| Constant | -4.669*** (0.286) | -4.797*** (0.309) | -4.945*** (0.283) | -4.691*** (0.383) | -3.992*** (0.432) | -4.507*** (0.442) | -3.681*** (0.860) | -3.528*** (0.586) |
| Housing wealth | -0.0057 (0.0240) | -0.0025 (0.0285) | -0.0425 (0.0342) | | | | | |
| Received inheritance Amount inherited | | | | 0.589*** (0.177) | | | | |
| High educated spouse | | | | | 0.619** (0.279) | | | |
| Medium educated spouse | | | | | 0.284 (0.192) | | | |
| Bad health spouse | | | | | -0.295* (0.170) | | | |
| Observations | 3590 | 2487 | 2415 | 3590 | 3590 | 3590 | 3590 | 1823 |
| p -value $\beta_2 = -1$ | 0.253 | 0.0130 | 0.001 | 0.0210 | 0.0420 | 0.000 | 0.729 | 0.253 |
| p -value Country effects | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

See Table 4. Bootstrapped standard errors in parentheses, 1000 replications; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In column (8), France is used as the baseline country.

Table 9: IV Quantile regression estimates

| Variable | (1) Full sample | (2) Retired sample | (3) Old sample |
|----------------------------|-----------------------|--------------------------|----------------------|
| Pension Wealth | -1.232 (0.876) | -0.622 (0.863) | -0.955 (0.813) |
| Age 55-60 | -0.144*** (0.022) | -0.044 (0.027) | |
| Age 70-75 | 0.179*** (0.038) | 0.136*** (0.035) | 0.172*** (0.038) |
| Second earner | 0.012 (0.019) | 0.017 (0.020) | 0.026 (0.021) |
| Married | 0.037 (0.031) | 0.006 (0.034) | 0.003 (0.035) |
| No children | 0.050* (0.027) | -0.001 (0.029) | 0.054* (0.031) |
| High educated | 0.014 (0.040) | 0.031 (0.045) | 0.029 (0.043) |
| Medium educated | 0.030 (0.026) | 0.029 (0.026) | 0.045 (0.028) |
| Bad Health | 0.005 (0.016) | 0.001 (0.017) | 0.014 (0.018) |
| Gaps in career | -0.082*** (0.024) | -0.021 (0.025) | -0.036 (0.028) |
| Civil servant | 0.038 (0.033) | 0.031 (0.030) | 0.024 (0.035) |
| Employee | -0.022 (0.028) | -0.016 (0.029) | -0.037 (0.032) |
| Sweden | -0.099** (0.040) | -0.115** (0.046) | -0.111** (0.045) |
| Netherlands | 0.049 (0.032) | 0.029 (0.038) | 0.067* (0.036) |
| Austria | 0.075 (0.051) | 0.016 (0.054) | 0.019 (0.058) |
| Switzerland | -0.521*** (0.094) | -0.425*** (0.114) | -0.435*** (0.099) |
| Spain | 0.141*** (0.051) | 0.151** (0.059) | 0.156*** (0.058) |
| Italy | 0.225*** (0.035) | 0.207*** (0.038) | 0.239*** (0.037) |
| France | 0.037 (0.050) | -0.020 (0.063) | -0.001 (0.072) |
| Denmark | -0.077 (0.056) | -0.109* (0.058) | -0.124** (0.053) |
| Belgium | 0.149*** (0.034) | 0.130*** (0.043) | 0.182*** (0.042) |
| Greece | 0.173*** (0.066) | 0.125* (0.072) | 0.135* (0.073) |
| Czech Republic | 0.161** (0.079) | 0.162** (0.073) | 0.156** (0.071) |
| Poland | 0.226*** (0.084) | 0.187** (0.076) | 0.198*** (0.076) |
| Constant | -0.366*** (0.110) | -0.381*** (0.100) | -0.382*** (0.097) |
| Observations | 3590 | 2487 | 2415 |
| p -value $\beta_2 = -1$ | 0.804 | 0.684 | 0.960 |
| p -value Country effects | 0.000 | 0.000 | 0.000 |
| F -statistic first stage | 41.895 | 31.232 | 38.567 |
| p -value first stage | 0.000 | 0.000 | 0.000 |

See Table 5. Bootstrapped standard errors in parentheses; 1000 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Dependent and endogenous RHS variable divided by 10

B The measurement error problem

In this appendix, we derive the biases in the estimator of the displacement coefficient. Our starting point is equation (6), repeated here for convenience:

$$A_t = \beta_0 + \beta_1 z_{1t}^* + \beta_2 z_{2t}^* + \mathbf{x}'_t \boldsymbol{\gamma} + \varepsilon_t \quad (9)$$

where

$$z_{1t}^* = \sum_{\tau=1}^t (1+r)^{t-\tau} y_\tau - Q(\lambda, t) \sum_{\tau=1}^R (1+r)^{t-\tau} E_\tau$$

$$z_{2t}^* = Q(\lambda, t) \sum_{\tau=R+1}^L (1+r)^{t-\tau} B_\tau \text{ ("Q adjusted pension wealth")}$$

\mathbf{x}_t = a vector of demographic household characteristics that might affect savings.

Instead of z_1^* and z_2^* , we observe the error ridden variables z_1 and z_2 , dropping the t -index for notational convenience:

$$z_k = z_k^* + \eta_k, \quad k = 1, 2 \quad (10)$$

In the SHARE survey, we expect the measurement errors η_1 and η_2 to be positively correlated with each other: $Cov(\eta_{1t}, \eta_{2t}) \geq 0$. On top of this we make the following assumptions about the measurement errors:

- $E(\eta_k z_k^*) = E(\eta_k \varepsilon) = E(\eta_k) = 0, \quad k = 1, 2$
- $E(\eta_k \mathbf{x}) = \mathbf{0}, \quad k = 1, 2$
- $E(\eta_1 z_2^*) = E(\eta_2 z_1^*) = 0$
- $Var(\eta_k) = \sigma_{\eta_k}^2, \quad k = 1, 2; \quad Cov(\eta_1, \eta_2) = \sigma_{\eta_1 \eta_2} \geq 0$ (homoskedasticity)

Substitution of equation (10) into (9) yields

$$A = \beta_0 + \beta_1 z_1 + \beta_2 z_2 + \mathbf{x}' \boldsymbol{\gamma} + \varepsilon - \beta_1 \eta_1 - \beta_2 \eta_2 \quad (11)$$

The linear projection $\hat{E}^*(A|1, z_1, z_2, \mathbf{x})$ is equal to

$$\hat{E}^*(A|1, z_1, z_2, \mathbf{x}) = \beta_0 + \beta_1 z_1 + \beta_2 z_2 + \mathbf{x}' \boldsymbol{\gamma} + \hat{E}^*(\varepsilon|1, z_1, z_2, \mathbf{x}) - \beta_1 \hat{E}^*(\eta_1|1, z_1, z_2, \mathbf{x}) - \beta_2 \hat{E}^*(\eta_2|1, z_1, z_2, \mathbf{x}) \quad (12)$$

Given our assumptions on the measurement errors, one can easily show that $\hat{E}^*(\varepsilon|1, z_1, z_2, \mathbf{x}) = 0$. So the biases, if any, are equal to $-\beta_1 \hat{E}^*(\eta_1|1, z_1, z_2, \mathbf{x}) - \beta_2 \hat{E}^*(\eta_2|1, z_1, z_2, \mathbf{x})$. Let $(\theta_{z_1}^k, \theta_{z_2}^k, \theta_{\mathbf{x}}^k)$ be the projection coefficients of (z_1, z_2, \mathbf{x}) in $\hat{E}^*(\eta_k|1, z_1, z_2, \mathbf{x})$, $k = 1, 2$. By the projection formula (see Hayashi (2000, Section 2.9)):

$$\begin{pmatrix} \theta_{z_1}^1 \\ \theta_{z_2}^1 \\ \theta_{\mathbf{x}}^1 \end{pmatrix} = \begin{pmatrix} Var(z_1) & Cov(z_1, z_2) & Cov(z_1, \mathbf{x}') \\ Cov(z_2, z_1) & Var(z_2) & Cov(z_2, \mathbf{x}') \\ Cov(\mathbf{x}, z_1) & Cov(\mathbf{x}, z_2) & Var(\mathbf{x}) \end{pmatrix}^{-1} \begin{pmatrix} Cov(z_1, \eta_1) \\ Cov(z_2, \eta_1) \\ Cov(\mathbf{x}, \eta_1) \end{pmatrix} \quad (13)$$

Given our assumptions, $Cov(z_2, \eta_1) = \sigma_{\eta_1\eta_2} \geq 0$ and $Cov(\mathbf{x}, \eta_1) = \mathbf{0}$. Obviously, $Cov(z_1, \eta_1) = Var(\eta_1) = \sigma_{\eta_1}^2$. Therefore, the projection coefficients can be rewritten as

$$\begin{pmatrix} \theta_{z_1}^1 \\ \theta_{z_2}^1 \\ \theta_{\mathbf{x}}^1 \end{pmatrix} = \sigma_{\eta_1}^2 \mathbf{a}_1 + \sigma_{\eta_1\eta_2} \mathbf{a}_2 \quad (14)$$

where \mathbf{a}_1 and \mathbf{a}_2 are respectively the first and second column of the inverse variance-covariance matrix

$$\begin{pmatrix} Var(z_1) & Cov(z_1, z_2) & Cov(z_1, \mathbf{x}') \\ Cov(z_2, z_1) & Var(z_2) & Cov(z_2, \mathbf{x}') \\ Cov(\mathbf{x}, z_1) & Cov(\mathbf{x}, z_2) & Var(\mathbf{x}) \end{pmatrix}^{-1}$$

Likewise

$$\begin{pmatrix} \theta_{z_1}^2 \\ \theta_{z_2}^2 \\ \theta_{\mathbf{x}}^2 \end{pmatrix} = \sigma_{\eta_2}^2 \mathbf{a}_2 + \sigma_{\eta_1\eta_2} \mathbf{a}_1 \quad (15)$$

Therefore the biases in the OLS estimators $\hat{\beta}_1^{OLS}$ and $\hat{\beta}_2^{OLS}$ are equal to

$$\begin{aligned} \text{plim } \hat{\beta}_1^{OLS} - \beta_1 &= -\beta_1(\sigma_{\eta_1}^2 a_{11} + \sigma_{\eta_1\eta_2} a_{21}) - \beta_2(\sigma_{\eta_2}^2 a_{21} + \sigma_{\eta_1\eta_2} a_{11}) \\ &= -(\beta_1\sigma_{\eta_1}^2 + \beta_2\sigma_{\eta_1\eta_2})a_{11} - (\beta_2\sigma_{\eta_2}^2 + \beta_1\sigma_{\eta_1\eta_2})a_{21} \end{aligned} \quad (16)$$

and

$$\begin{aligned} \text{plim } \hat{\beta}_2^{OLS} - \beta_2 &= -\beta_2(\sigma_{\eta_2}^2 a_{22} + \sigma_{\eta_1\eta_2} a_{21}) - \beta_1(\sigma_{\eta_1}^2 a_{21} + \sigma_{\eta_1\eta_2} a_{22}) \\ &= -(\beta_2\sigma_{\eta_2}^2 + \beta_1\sigma_{\eta_1\eta_2})a_{22} - (\beta_1\sigma_{\eta_1}^2 + \beta_2\sigma_{\eta_1\eta_2})a_{21} \end{aligned} \quad (17)$$

The direction of the asymptotic bias in the OLS estimator $\hat{\beta}_1^{OLS}$ depends on the signs of the elements in the vector \mathbf{a}_1 . The first element of \mathbf{a}_1 , a_{11} , is unambiguously positive (it is a diagonal element of the inverse of a variance-covariance matrix). The second element a_{21} is presumably negative because one would expect that $Cov(z_1, z_2) > 0$ and that the correlation between (z_1, z_2) and \mathbf{x} is not unusually large. In our data \hat{a}_{21} is indeed negative. Equation (16) suggests that under the validity of the simple life-cycle model ($\beta_2 = -1$ and $\beta_1 = 1$) and under the (plausible) assumptions

$$\sigma_{\eta_1\eta_2} < \sigma_{\eta_1}^2 \quad (18a)$$

$$\sigma_{\eta_1\eta_2} < \sigma_{\eta_2}^2 \quad (18b)$$

the OLS estimator $\hat{\beta}_1^{OLS}$ is downward biased. The first term on the right hand side of equation (16), $(-\beta_1\sigma_{\eta_1}^2 + \beta_2\sigma_{\eta_1\eta_2})a_{11}$ depicts the usual (downward) attenuation bias. The second term on the right hand side of equation (16) reveals that, since \hat{a}_{21} is actually smaller than zero, the measurement error in z_{2t} aggravates the downward bias in $\hat{\beta}_1^{OLS}$. The estimator could even converge in probability to a negative number! Along the same line of reasoning one can argue that $\hat{\beta}_2^{OLS}$ is upward biased and that the upward bias in

this OLS estimate is exacerbated by the measurement error in z_1 . As we said before, we find that the OLS estimate of β_2 is positive. In other words, measurement error problems could drive the estimation results indicated above.²⁴

In order to be able to sign the bias associated with the measurement error problem, we impose the restriction $\beta_1 = 1$ in the estimation. In other words, we estimate the following model instead of equation (11):

$$A - z_1 = \beta_0 + \beta_2 z_2 + \mathbf{x}'\boldsymbol{\gamma} + \varepsilon - \eta_1 - \beta_2 \eta_2 \quad (19)$$

It is easy to show that in this case the bias in the OLS estimator $\hat{\beta}_2^{OLS}$ is equal to

$$\text{plim } \hat{\beta}_2^{OLS} - \beta_2 = -(\sigma_{\eta_1 \eta_2} + \beta_2 \sigma_{\eta_2}^2) \tilde{a}_{11} \quad (20)$$

where \tilde{a}_{11} is the first diagonal element of the inverse variance-covariance matrix

$$\begin{pmatrix} \text{Var}(z_2) & \text{Cov}(z_2, \mathbf{x}') \\ \text{Cov}(\mathbf{x}, z_2) & \text{Var}(\mathbf{x}) \end{pmatrix}^{-1}$$

Obviously, $\tilde{a}_{11} > 0$. In case of 1) full displacement ($\beta_2 = -1$), 2) zero correlation between \mathbf{x} and z_2 , 3) nonnegatively correlated measurement errors ($\sigma_{\eta_1 \eta_2} \geq 0$) and 4) under assumption (18b), equation (20) implies that the OLS estimate for β_2 is upward biased and that $-1 < \text{plim } \hat{\beta}_2^{OLS} < 0$. To see this, note that if $\beta_2 = -1$ we can write the right hand side of equation (20) as

$$\frac{\sigma_{\eta_2}^2 - \sigma_{\eta_1 \eta_2}}{\text{Var}(z_2)} \tilde{a}_{11} \text{Var}(z_2) \quad (21)$$

If we additionally assume zero correlation between \mathbf{x} and z_2 ($\tilde{a}_{11} \times \text{Var}(z_2) = 1$) and $0 \leq \sigma_{\eta_1 \eta_2} < \sigma_{\eta_2}^2$, then $0 < \frac{\sigma_{\eta_2}^2 - \sigma_{\eta_1 \eta_2}}{\text{Var}(z_2)} = \frac{\sigma_{\eta_2}^2 - \sigma_{\eta_1 \eta_2}}{\text{Var}(z_2^*) + \sigma_{\eta_2}^2} < 1$ and consequently $-1 < \text{plim } \hat{\beta}_2^{OLS} < 0$.

In our data, the correlation between z_2 and \mathbf{x} is low enough, as we find that $\tilde{a}_{11} \times \text{Var}(z_2) = 1.30 \times 0.68 = 0.884$, and hence equations (20) and (21) imply that $0 < \text{plim } \hat{\beta}_2^{OLS} + 1 < 1$, or $-1 < \text{plim } \hat{\beta}_2^{OLS} < 0$. If there is only partial displacement ($-1 < \beta_2 < 0$), we cannot determine the direction (upward or downward) of the bias in the OLS estimate. In the empirical section we will carry out a sensitivity analysis in which we estimate model (19) on the subsample of retirees. As explained in section 3, for this subsample the measurement errors in z_{1t}^* and z_{2t}^* are likely to be uncorrelated ($\sigma_{\eta_1 \eta_2} = 0$). In that case, the estimate of the displacement coefficient will be attenuated irrespective of the true value of β_2 . However, we still learn something from the estimation using both retired and non-retired individuals. Even in the presence of measurement error in pension wealth we would expect that the estimate of the displacement coefficient is negative. This can be seen as follows: if $0 \leq \sigma_{\eta_1 \eta_2}$ equation (20) implies that:

$$\text{plim } \hat{\beta}_2^{OLS} = \beta_2(1 - \sigma_{\eta_2}^2 \tilde{a}_{11}) - \sigma_{\eta_1 \eta_2} \tilde{a}_{11} < \beta_2 \left(1 - \frac{\sigma_{\eta_2}^2}{\text{Var}(z_2)} \tilde{a}_{11} \text{Var}(z_2) \right) \quad (22)$$

²⁴This line of reasoning extends directly to applying Gale (1998)'s method, as we document in Appendix C.

In our data $0 < \left(1 - \frac{\sigma_{\eta_2}^2}{\text{Var}(z_2)} \tilde{a}_{11} \text{Var}(z_2)\right) < 1$ because $\tilde{a}_{11} \text{Var}(z_2) = 0.884$. Therefore equation (22) implies that $\text{plim } \hat{\beta}_2^{OLS} < 0$ if there is any displacement ($\beta_2 < 0$).

C Advantages of retrospective information

In this appendix, we discuss the advantage of retrospective earnings information compared to the traditional approach, used by, amongst others, Gale (1998). Our main interest is in estimating the parameter β_2 , the displacement effect, in the regression equation

$$A = z_1\beta_1 + z_2\beta_2 + \mathbf{x}'\boldsymbol{\gamma} + \varepsilon$$

where A denotes private wealth, z_1 permanent income, z_2 pension wealth and \mathbf{x}' a vector of controls. Using retrospective earnings data from SHARELIFE, z_1 is a one-dimensional measure of lifetime income, measured with error. Using the approach of Gale (1998), $z_1\beta_1$ is replaced by $\mathbf{g}'_1\boldsymbol{\delta}_1$, where \mathbf{g}'_1 is a vector of variables proxying lifetime income, consisting of education, age, current income, marital status and the expected age of retirement. Hence, \mathbf{g}'_1 is a multi-dimensional measure of lifetime income, again measured with error. The economic model presented in Section 2 provides a value for $\beta_1 = 1$, which we can use to estimate a restricted model, such that we can sign the direction of the bias in the estimated displacement effect. Instead, economic theory does not provide any intuition regarding the magnitude of the elements in $\boldsymbol{\delta}_1$, and hence a restricted estimator is not feasible.

The SHARE survey contains enough information to estimate Gale's model on the sample of non-retirees.²⁵ An important impediment to this approach is that SHARE asks individuals to report an expected replacement rate of pension benefits. Expected pension benefits can then simply be computed by multiplying this replacement rate by current income. In contrast, the Survey of Consumer Finances (SCF) used by Gale asks respondents to provide an expected money amount of pension benefits.²⁶ This difference in survey questions has important consequences for estimating Gale (1998)'s model: using SHARE data, pension wealth is a linear function of current income, and hence the measurement error in pension wealth correlates with the measurement error in \mathbf{g}_1 . Using the SCF, one may reasonably argue that the measurement errors in \mathbf{g}_1 and z_2 are uncorrelated. The derivation in Appendix B clearly shows that this correlation biases the coefficients further away from the true values.

Table 10 shows the results of estimating Gale (1998)'s model using SHARE data, without using any retrospective earnings information, for the sample of non-retirees. In column (I), we proxy permanent income with Age, monthly real income (in €000's), education, marital status, a dummy for a second earner in the household, and the expected

²⁵Note that the approach of Gale (1998) is not suitable for the sample of retirees, as current labour income is not observed for this sample, except for the case where individuals are followed repeatedly over time and retire in the period surveyed. Gale indeed estimates his model for the sample of non-retirees.

²⁶Given the different institutions between countries in Europe, we believe that the replacement rate is indeed the appropriate pension income measure to elicit in a multi-country survey. The money amount may be more appropriate in a single-country survey, although in, for instance, The Netherlands and Italy, the replacement rate is the construct alluded to in political and popular debate.

age of retirement. In column (II), we additionally include interactions between age and income as well as education and income.

Table 10 shows that the estimated displacement effect is positive and not significantly different from zero using Gale (1998)'s approach with SHARE data. The bias towards zero, compared to the model's prediction, is most likely driven by correlated measurement errors between pension wealth and current income, as predicted in Appendix B (see equation 17). In column (II), the marginal effect of income for a 60-year old, high educated respondent equals 0.43, significantly different from zero ($p=0.004$), which is comparable to the effect of income in column (I). We conclude that one cannot use SHARE data to estimate the displacement effect, due to the presence of measurement errors. The approach we suggest in this paper, combining economic theory with retrospective earnings information, does allow for the identification of the displacement effect.

Table 10: Gale's regression

| Variables | (1) Gale's model | (2) Gale's model with interactions |
|----------------------------|----------------------|--|
| Pension wealth | 0.150 (0.132) | 0.135 (0.133) |
| Monthly income | 0.420*** (0.0730) | -0.655 (1.366) |
| Age | 0.0215 (0.0268) | -0.00588 (0.0510) |
| High educated | 0.836*** (0.196) | 0.503 (0.368) |
| Medium educated | 0.389** (0.169) | -0.251 (0.363) |
| Age x Income | | 0.0143 (0.0240) |
| High educated x Income | | 0.224 (0.156) |
| Medium educated x Income | | 0.389* (0.183) |
| Married | 0.965*** (0.262) | 0.943*** (0.253) |
| No children | 0.0251 (0.275) | -0.00680 (0.258) |
| Bad health | -0.211 (0.179) | -0.202 (0.178) |
| Second earner | -0.101 (0.177) | -0.0874 (0.172) |
| Expected retirement age | -0.00293 (0.0298) | -0.00203 (0.0300) |
| Sweden | 0.115 (0.278) | 0.119 (0.276) |
| Denmark | 0.852*** (0.294) | 0.849** (0.290) |
| Netherlands | 0.193 (0.293) | 0.188 (0.294) |
| Belgium | 1.109*** (0.295) | 1.090*** (0.289) |
| France | 0.756** (0.345) | 0.715* (0.353) |
| Switzerland | -0.0716 (0.326) | -0.0848 (0.322) |
| Austria | 0.584 (0.455) | 0.579 (0.453) |
| Spain | 1.350*** (0.371) | 1.366*** (0.373) |
| Italy | 0.418 (0.347) | 0.355 (0.343) |
| Greece | 0.376 (0.375) | 0.385 (0.368) |
| Poland | -0.827* (0.419) | -0.823* (0.408) |
| Czech Republic | -0.258 (0.334) | -0.340 (0.336) |
| Constant | -1.043 (2.279) | 0.901 (3.081) |
| Observations | 1022 | 1022 |
| p -value g_1 | 0.000 | 0.000 |
| p -value Country effects | 0.000 | 0.002 |

Robust regression estimates. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
 g_1 : Age, Income, Expected retirement age, Education, Age \times Income, Education \times Income

Data disclosure

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