The Effects of Schooling on Wealth Accumulation Approaching Retirement

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Education and wealth are positively correlated for individuals approaching retirement, but the direction of the causal relationship is ambiguous in theory and has not been identified in practice. We combine administrative data on individual total wealth with a reform expanding access to lower secondary school in Denmark in the 1950s, finding that schooling increases pension annuity claims but reduces the non-pension wealth of men in their 50's. These effects grow stronger as normal retirement age approaches. Labour market mechanisms are key, with schooling increasing job mobility, reducing housing equity, increasing leverage, and improving occupational pension benefits.

As social security systems struggle under demographic pressure, sustaining household consumption after retirement increasingly depends on personal savings. However, even with respect to their lifetime income, households reach retirement age with extremely different wealth levels (Hendricks, 2007). Schooling explains part of this inequality: in 2013, the median wealth relative to income of a US household whose head held a professional or graduate degree was about 4 times higher than that of a household whose head did not complete high school (Emmons and Noeth, 2015).[1]

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[1]Figure A.1 in the Appendix shows that the average lifetime accumulation of wealth is similar in Denmark (using administrative register data) and the United States (using the 2013 wave of the Survey of Consumer Finances). In both countries people reach the age of 60 with on average 4 times their income in net worth.

Establishing a causal relation between schooling and wealth accumulation is difficult because of common confounding factors such as endowments and inheritance. On one hand, schooling might reduce time and risk preferences (Becker and Mulligan, 1997), causing wealth accumulation; on the other hand, schooling might increase job security, fringe benefits and assortative matching in the marriage market (Oreopoulos and Salvanes, 2011), disincentivizing personal wealth accumulation. Both the size and the direction of the causal effect of schooling on wealth accumulation are open empirical questions.

We are the first to provide a comprehensive picture of the causal effect of schooling on wealth accumulation and wealth composition for individuals approaching retirement, highlighting the role of labour market channels in driving these effects. To identify the causal effect of schooling on wealth, we combine administrative data from third-party reports of individual wealth and pension claims with reform-driven variation in distance to public schools for the population of Danish men born between 1939 and 1951. In the spirit of Card (1995), we use historical data on school locations to compute a measure of distance to the closest public school offering 8th grade (ages 14-15) between 1953 and 1965 from the parish of birth of each individual in our sample. Because distance to school is unlikely to be random in a cross-section, we augment this measure with school openings and a 1958 reform that established 8th grade teaching in rural municipalities. These institutional changes provide exogenous variation in distance to schools over time for the same parish of birth, conditional on municipality fixed effects and cohort trends by implementation year.

By comparing individuals born in the same municipality but facing different distances to public schools, we show that schooling causally reduces wealth held outside of pension funds by the age of 60, and that the positive correlation between schooling and non-pension wealth is therefore due to third, unobserved factors. This negative effect of schooling on wealth held outside of a pension fund is partially offset by an increase in pension annuity claims.

We characterize the distributional impact of the effects of schooling on both pension and non-pension wealth, and highlight their implications for portfolio composition: schooling affects housing equity at both at the extensive and intensive margins and decreases the probability of being in the top 20% of the wealth distribution, suggesting that more educated individuals are more likely to rent their home and to leverage more on their assets. Our results are robust to a battery of checks that use different specifications (e.g. including municipality of birth trends and municipality of residence fixed effects) and

different samples (e.g. excluding data after 2007), and we show that our identification strategy produces results for other outcomes consistent with the literature on the causal effects of schooling.

Throughout this paper we normalise wealth holdings by a measure of permanent income, calculated as the moving weighted average of disposable income in the previous 5 years. [2] We show in the Appendix that our results do not depend on this normalisation, and in line with the results of Devereux and Hart (2010), in our sample we find no effect of schooling on permanent income after age 50. Nevertheless, this normalisation provides both a standardised and an internationally comparable measure of wealth accumulation, and a direct link to lifecycle consumption models (Carroll, 1997, 2009; Kaplan and Violante, 2014).

Our findings have a positive, rather than a normative, interpretation. In contrast to studies on the effect of financial literacy on retirement preparedness (Lusardi and Mitchell, 2007a,b) and wealth accumulation (Lusardi and Mitchell, 2007a; Van Rooij et al., 2011; Behrman et al., 2012; Hastings et al., 2013), our results do not necessarily imply that individuals make myopic or irrational choices when managing their wealth. Schooling is an imperfect proxy for financial literacy while also affecting cognitive skills (Falch and Sandgren Massih, 2011; Banks and Mazzonna, 2012; Brinch and Galloway, 2012; Carlsson et al., 2015), preferences (Burks et al., 2009; Perez-Arce, 2011) and labour market outcomes (Card, 2001). Rational adjustments to changed economic environments can therefore explain the overall effect of schooling on wealth accumulation.

We test for family and labour market mechanisms that connect schooling and wealth, finding that schooling increases labour market mobility, thereby reducing the attractiveness of real estate investments, and reduces self-employment, a strong predictor of wealth (Cagetti and De Nardi, 2006).[3] Moreover, the increase in pension annuity claims is entirely due to occupational pension schemes, suggesting that schooling increases the likelihood of obtaining better jobs in terms of fringe benefits. We do not find evidence of strategic wealth allocation within households despite strong evidence of assortative matching, and we find no effect of schooling on completed fertility.

Our paper is closely related to the literature estimating causal effects of education on

[2] Wealth strongly correlates with schooling even after conditioning on permanent income. Emmons and Noeth (2015) show that the ratio of average wealth to average income for US families with a postgraduate or professional degree was 5.58; the same ratio for families without a high school degree was 1.43.

[3] Cagetti and De Nardi (2006) show, using the Survey of Consumer Finances, that in the US the 16.7% of self-employed in the data hold 52.9% of the total wealth, and that 39% of people in the top 20% of the wealth distribution are self-employed.

isolated wealth components. Cole et al. (2014) use compulsory school attendance laws in the United States as an instrument for education, and find that schooling increases the probability of direct stock ownership and decreases the probability of debt delinquency and foreclosure. Black et al. (2015) use a Swedish schooling reform to show that, while having no effect on the probability of investing in mutual funds, schooling increases direct stock market participation and the share of liquid wealth invested in risky assets. [4]

With respect to these studies, our paper makes three important contributions. First, we estimate and characterize in a single framework the comprehensive effect of schooling on both pension and non-pension wealth holdings. Other papers focus on financial investments, but these assets constitute on average only between 10 and 18 percent of non-pension net worth for Danish men between 50 and 60 years of age, with much larger holdings in housing equity and pension wealth.[5] Second, using the same framework, we decompose the overall effect into that on specific wealth components, thereby investigating portfolio composition and asset choice. Third, we document the existence of a range of labor market channels that, in light of the detailed decomposition of our results, connect schooling to wealth accumulation over the lifecycle.

Our results challenge the view that schooling can uniformly improve every individual economic outcome in late life. Specifically, we show that general education does not boost wealth accumulation as individuals approach retirement age. Increasing personal savings and wealth accumulation would likely require more targeted investment in specific competencies and financial literacy (Lusardi and Mitchell, 2007a). Nonetheless, our results are consistent with the finding that more educated individuals are able to adapt and exploit institutions (e.g. occupational pension schemes) to their advantage when choosing their saving strategies (Chetty et al., 2014).

The remainder of the paper is organized as follows. Section 1 presents our data and illustrates the correlation between schooling and wealth in Denmark, which is similar to that observed in other countries. Section 2 describes the Danish school reforms that provide exogenous variation and our identification strategy. We show that our strategy

[4] In our data we cannot consistently separate direct investments in stocks, mutual funds and bonds, and while we estimate a negative effect on participation in any of these markets, we do not find any effect of schooling on total financial investments. Moreover, due to the timing of the Danish school reform, our sample is on average older than that studied by Cole et al. (2014) and Black et al. (2015), and is representative of men approaching retirement. When estimating separately by age, we show that the point estimate of the effect of schooling on financial investments is positive at the age of 50 and turns negative as age increases.

[5] Using data from the 2001 Survey of Consumer Finances Campbell (2006) shows similar figures for the US: Only the wealthiest 20% of the US population holds more than 20% of their non-pension net worth in financial assets.

produces estimates consistent with the literature when applied to other outcomes. Section 3 presents our results on non-pension wealth, pension wealth, and the labor market mechanisms connecting schooling to wealth accumulation. Section 4 concludes.

1. Data

Our analysis combines historical geographic information with administrative register data on 376,827 men born in Denmark between 1939 and 1951. These birth cohorts are chosen because they span the school reform and enable observation until normal retirement age. This section describes the data that we use and the relevant sources of variation.

The Danish administrative data combines information from four groups of registers and constitutes the bulk of our dataset. Demographic and education registers provide information on the highest level of schooling obtained,[6] municipality and parish of birth, civil status and spouse identifier for each individual in our sample. Tax records from 1980 to 2011 provide information on income over the lifecycle. Reports from financial institutions (primarily banks and investment funds) and the real estate register give us a complete overview from 1996 to 2011 of the December 31 market value of financial assets and publicly assessed value of real estate holdings at the individual level. Finally, the pension entitlement register gives us a snapshot of both pension wealth and the pension annuity claims of all individuals in our sample when they turn 60.

With the exception of wealth held abroad, the administrative register data is third-party reported and describes the net wealth of both our core sample and their spouses. We use this information to split the total non-pension wealth of each individual in our sample into five categories. We distinguish among the amount of wealth held in liquid assets (saving and check accounts), the market value of financial investments (stocks, bonds and mutual funds), real estate value (public evaluations from the administrative housing register), collateralised debts (mostly mortgages) and non-collateralised debts. We compute housing equity as the difference between real estate value and collateralised debt.

In our main analysis we normalise wealth components by permanent income, which we

[6]The Danish Ministry of Education defines the minimum duration of study required for obtaining each qualification by the fastest route. Statistics Denmark defines highest level of schooling for each person as the qualification with the longest minimum required study duration. Our education attainment measure is the minimum required duration of study associated with each person's highest schooling level.

Table 1
Wealth holdings of Danish men born 1939-1951 (years 1996-2011)

	Nominal values			Over permanent income		
	$< 8^{th}$ grade	Secondary school	University	$< 8^{th}$ grade	Secondary school	University
Perm. income	207.2 (89.12) [192.5]	238.4 (99.6) [220.1]	312.4 (125.9) [284.6]			
Net Worth	476.2 (1222) [142.5]	575.8 (1351) [286.0]	840.3 (1594) [517.6]	2.103 (5.152) [0.723]	2.294 (5.132) [1.240]	2.678 (5.300) [1.744]
Liquid assets	90.71 (260.6) [21.67]	103.1 (285.0) [28.57]	144.6 (375.8) [45.84]	0.421 (1.110) [0.114]	0.425 (1.105) [0.129]	0.473 (1.327) [0.159]
Housing equity	477.0 (1013) [149.5]	563.2 (1104) [286.7]	718.5 (1132) [455.1]	2.108 (4.119) [0.716]	2.270 (3.987) [1.208]	2.332 (3.827) [1.494]
- value	829.2 (1509) [502.0]	992.8 (1559) [741.4]	1274 (1465) [1028]	3.468 (5.225) [2.482]	3.850 (5.035) [3.179]	4.032 (4.624) [3.388]
- owner	0.625	0.722	0.827			
- equity to value†	52.09	51.33	49.97			
Fin. investments	50.27 (336.7) [0.000]	71.32 (456.5) [0.000]	151.3 (750.4) [0.000]	0.202 (1.116) [0.000]	0.259 (1.477) [0.000]	0.440 (2.068) [0.000]
- participation	0.282	0.357	0.458			
Unc. Debts	141.8 (405.0) [44.25]	161.8 (463.5) [55.48]	174.0 (581.2) [50.17]	-0.628 (1.732) [-0.239]	-0.659 (1.839) [-0.257]	-0.567 (2.062) [-0.176]
# observations	795621	2046434	807045	795621	2046434	807045
# individuals	90171	206515	80141	376827	376827	376827

Notes: Standard deviations in (parentheses), medians in [brackets]. Nominal values are in thousands Danish Kroner (DKK), adjusted for inflation to 2010 prices. In December 2010, 1 USD = 5.57 DKK. The table shows descriptive statistics for the subsamples attending only compulsory schooling (first and fourth columns), middle and high schools (second and fourth columns) and university (third and sixth columns).
† Computed on a sample of real estate owners only.

compute as a weighted moving average of disposable income in the preceding five tax years. Table 1 presents means, standard deviations and medians of individual wealth holdings by educational levels in both nominal values and values normalised by permanent income.

The average person in the sample holds non-pension wealth equivalent to less than four years of permanent income, and has a high debt burden even after age 50. Housing equity represents most of the assets in our data: Over 50% of the sample invests more than 90% of their personally held assets in real estate. [7] As schooling increases, net worth grows both at the mean and at the median, and the portfolio composition changes. More educated individuals invest more in the financial markets, hold relatively fewer liquid assets and leverage more on their real estate.

While we observe non-pension wealth annually, we only observe pension wealth and pension annuity claims once, in the year an individual turns 60.[8] Table 2 shows that at the median 40% of assets are in pension funds. This wealth originates both from employer pension contributions and from voluntary individual private contribution schemes.[9] In contrast to the mixed origin of pension wealth, claims to pension annuities are almost exclusively due to employer contributions. More education is associated with greater pension annuity claims; and the association holds when normalising annuities by permanent income, suggesting that people with more schooling secure jobs with better fringe benefits, in which employers offer more competitive packages in terms of pension annuity contributions.[10]

To identify the causal effect of education on wealth accumulation approaching retirement, we combine historical and geographical information on school openings with individual information on the parish of birth. Access to 8th grade schooling in Denmark in the mid-20th century did not solely depend on whether a secondary school existed within a municipality. In smaller municipalities, children could easily attend 8th grade in a neighbouring town. In areas where parish and municipal areas were larger, the distance to a school offering 8th grade could have been long even within a single municipality.

We use our historical location data to calculate the minimum linear distance between the closest public schools offering 8th grade and the residence of an individual, proxied

[7] These statistics are consistent with the description of wealth and debt of Danish households by Andersen et al. (2014).

[8] Pension wealth at turning 60 has been reported by financial institutions to the tax authorities since 1999 in connection with means testing of early retirement benefits against private pension wealth (Ministry of Taxation, law 543 of 30 June 1999).

[9] While contributions to pension funds are tax deductible, they entail a significant loss in liquidity, as withdrawing money from a pension account before retirement incurs a penalty of 60% of the amount withdrawn. See Chetty et al. (2014) for a description of different retirement saving vehicles in Denmark.

[10] Using event studies of individuals who switch firms, Chetty et al. (2014) show that people adjust their savings in response to changes in pension contributions by the employer. Total savings increase by 0.8 DKK for every DKK contributed by the employer.

Table 2
Wealth holdings of Danish males born 1939-1951 at the age of 60

	Nominal values			Over permanent income		
	$< 8^{\text{th}}$ grade	Secondary school	University	$< 8^{\text{th}}$ grade	Secondary school	University
Perm. income	215.9	241.7	314.1			
	(89.61)	(98.80)	(126.8)			
	[200.0]	[223.1]	[286.5]			
Non-pension assets	1316	1494	2019	5.559	5.890	6.424
	(2206)	(2219)	(2245)	(7.495)	(7.247)	(6.629)
	[839.8]	[1097]	[1556]	[4.086]	[4.732]	[5.219]
Pension wealth	394.4	634.9	1022	1.643	2.371	3.060
	(643.8)	(966.1)	(1528)	(2.166)	(3.010)	(5.047)
	[195.3]	[328.6]	[584.0]	[0.972]	[1.474]	[1.977]
- by employer	162.1	336.6	552.2	0.709	1.246	1.656
	(372.1)	(724.2)	(1104)	(1.276)	(2.205)	(3.018)
	[79.11]	[108.6]	[158.4]	[0.428]	[0.556]	[0.609]
Annuities (per year)	17.70	29.97	102.5	0.082	0.123	0.343
	(40.93)	(61.08)	(183.7)	(0.187)	(0.240)	(0.634)
	[5.233]	[5.936]	[76.89]	[0.027]	[0.029]	[0.258]
- by employer	17.36	28.80	97.74	0.081	0.118	0.330
	(40.73)	(60.15)	(183.4)	(0.186)	(0.236)	(0.635)
	[4.987]	[5.273]	[64.31]	[0.025]	[0.026]	[0.221]
# observations	59161	161403	67489	59161	161403	67489

Notes: Standard deviations in (parentheses), medians in [brackets]. Nominal values are in thousands Danish Kroner (DKK), adjusted for inflation to 2010 prices. In December 2010, 1 USD = 5.57 DKK. The table shows descriptive statistics for the subsamples attending only compulsory schooling (first and fourth columns), middle and high schools (second and fourth columns) and university (third and sixth columns)

by the location of the church of the parish of birth.[11] In 1953 the average distance to these public school for boys finishing 7th grade was approximately 5 kilometers. However, the heterogeneity in distance across parishes remained substantial, and there was far from universal access to secondary education. One out of five children finishing 7th grade lived more than 10 kilometers from the closest public school, and the proportion of pupils attending 8th grade in these distant areas was 15 percentage points below the national

[11]Residential location is first registered in 1968. Computing distance to 8th grade school from parish of birth will induce error for children living elsewhere at age 15 when they can attend 8th grade. However, families might change address for the sake of schooling oportunities, thereby making distance to school endogenous. By using parish of birth we reduce endogeneity bias at the cost of increased measurement error and reduced precision of our estimates.

average (67.5%). The government took action to alleviate this disparity, supporting school constructions to provide universal access to secondary education in the countryside. These interventions provide us with policy-induced changes in minimum distance to public schools within a parish over time as an instrument for schooling.

2. Identification

During the mid-20th century Denmark progressively reformed its education system to expand access to secondary schooling.[12] While 7th grade attendance was mandatory and a school offering 7th grade teaching existed in every municipality, schools that offered 8th grade teaching were fewer and scattered across the country. To remedy these inequalities, in 1937 the government required all 82 existing market towns, or *købstæder*,[13] to build a school that offered at least 8th grade teaching.[14] However, primarily because of World War II, the reform implementation was delayed and rolled out over the years until 1958, when the government mandated that 8th and 9th grade teaching be available in all municipalities.[15]

These interventions had a dramatic impact on the average distance pupils had to travel in order to attend 8th grade. Figure 1 shows how openings of secondary schools throughout the country and the 1958 reform drastically increases access to 8th grade education for most parishes. While in 1953 more than five hundred parishes were more than 10 kilometers away from the closest public school, by 1959 this number decreased to eleven.

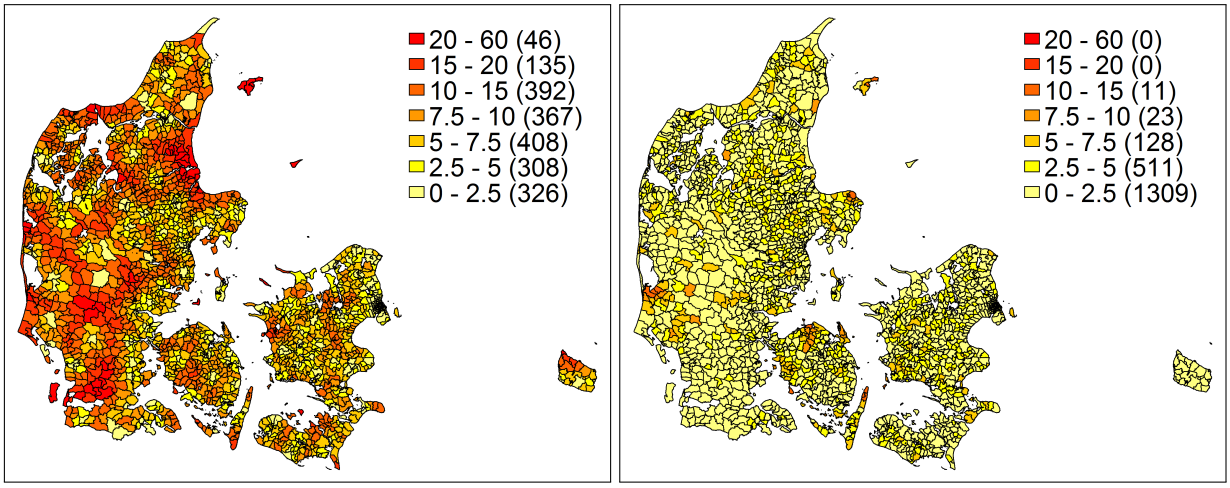
Because cross-sectional distance to the closest public school across municipalities is unlikely to be random, we exploit these policy-driven changes in distance to identify the causal effect of schooling on wealth accumulation. One threat to this identification strategy is that municipalities might strategically decide to build a public school at a specific time. To control for such confounding correlations and isolate the policy-driven

[12] For a review (in Danish) see Gjerløff et al. (2014).

[13] These were towns with enough economical or historical importance to be granted the status of "market town", which entailed a higher degree of administrative autonomy than that of other towns.

[14] Legal Gazette of the Kingdom of Denmark, Series A, Number 160, page 866-884, 18 May 1937.

[15] The reform modified the administrative school organisation, so that 8th grade schooling was offered in the same physical structure as 7th grade. See Legal Gazette of the Kingdom of Denmark, Series A, Number 220, pp. 625-640, 18 June 1958. This 1958 reform has been used for providing instruments for schooling effects on health outcomes (Arendt, 2005, 2008). Attendance at 8th grade was first made compulsory in 1972/3, with attendance in 9th grade made compulsory the following school year. See Danish Parliament Gazette 1974-75, Collection of Supplements Number 549, 26 June 1975.



(a) *1953* (b) *1959*

Fig. 1: *Distance to closest public school offering* 8th *grade (parish level)*
Notes: Maps produced from data by the Digital Atlas of Danish Historic Administrative Geography (digdag.dk). Parishes are colored by linear distance to closest public school (number of parishes in each distance interval in parentheses).

changes in access to schooling, we not only control in all our regressions for municipality fixed effects, but also allow for different cohort linear trends between municipalities receiving access to 8th grade teaching in different years.[16] Because rural municipalities – those most affected by the reforms – typically cover large areas, we further split these cohort trends within a municipality between the main and secondary parishes in terms of population.

We thus allow for 22 unique cohort linear trends and about one thousand municipality fixed effects. We further condition our analysis on year and cohort fixed effects, estimating the equations:

$$Y_{i,t} = S_i\beta + \Psi_i\gamma + T_t + \nu_{i,t} \quad (1)$$

$$S_i = f(D_i) + \Psi_i\gamma + \varepsilon_i, \quad (2)$$

[16]Wolfers (2006) illustrates the importance of the treatment of state-specific trends in the context of US state-specific divorce law change. Meghir et al. (2012) use a roll-out of compulsory school attendance increases in Sweden to look at health outcomes, including linear time trends interacted with each year-of-reform implementation. In our main specifications we allow for implementation-year group trends as in Meghir et al. (2012), and run robustness checks including municipal trends in table A.6 in the appendix.

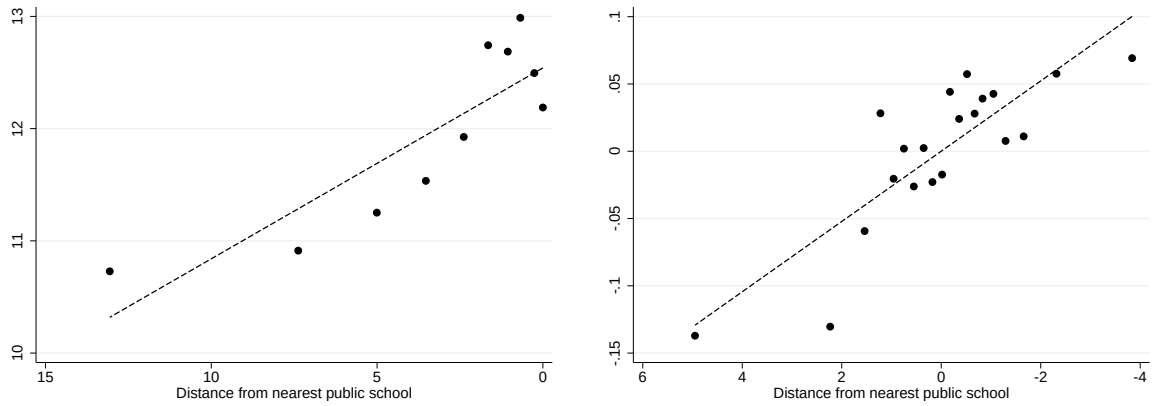
where Ψ_i represents the set of municipal fixed effect and cohort linear trends for individual i and T_t the set of year fixed effects. If $S_i \mid \Psi_i \not\perp \nu_{i,t}$, OLS estimation of this model returns a biased estimate of β. However, as we argue that policy-induced changes in the distance to closest public school $D_i \mid \Psi_i$ is independent of $\nu_{i,t}$, instrumental variable estimation of (1) consistently estimates the Local Average Treatment Effect (LATE) on the complier population – those who attend 8th grade because of a school opening, but who otherwise would not have attended.

Figure 2 visualizes our first stage non-parametrically. The left panel of the figure plots unconditional average length of schooling by ventile of distance to closest public school. The difference in average years of schooling between the closest and farthest 5% of individuals in our sample is approximately 2 years of schooling. However, much of this correlation is due to cross-sectional differences between municipalities. Isolating the effect of the policy-induced variation in distance to closest public school, the right panel of Figure 2 shows averages conditional on Ψ_i. While, consistent with our expectations, the correlation between distance to school and years of schooling decreases sharply after conditioning, the relation is still clearly negative and, as Table A.3 in the Appendix shows, significant. As a placebo, we also regress schooling on distance to the closest private school offering 8th grade and find no significant effect of changes in linear or quadratic distance to private schools on school attendance. [17]

To further validate our identification strategy, we replicate in our sample a number of previous estimates of financial and non-financial returns to schooling, reproducing estimates consistent with the established literature when applied to other outcomes. More specifically, we estimate financial returns to schooling of 3-4% between ages 40 and 50, but we also replicate (in table A.2 in the appendix) as many of the non-financial returns to schooling reported by Oreopoulos and Salvanes (2011) as our data allow. Consistent with their findings, we show evidence of assortative matching in the marriage market, and we show that schooling increases occupational prestige.

We run our regressions using as instruments either a quadratic function of distance to school (F-statistic of 20) or a dummy indicating the presence of a public school within a 2.5 kilometers radius (F-statistic of 53). The discrete instrument helps characterise our complier population – those who attend 8th grade because of the accessibility of a new secondary school building, but who would otherwise not have attended. Having a public school within a 2.5 kilometer radius increases the probability of attending 8th grade by

[17] These results appear in Table A.4 in the Appendix.



(a) *Length of schooling, unconditional* (b) *Length of schooling, conditional*

Fig. 2: *First-stage regressions in graphs*

Notes: The figure shows the conditional and unconditional correlations between distance from the nearest public school to the parish of birth of an individual in our sample at the time of 8th grade attendance. The left pane plots the unconditional average years of schooling for each 5% of distance to schooling. The right pane plots the conditional average years of schooling on the residuals of a regression of distance to schooling on municipality and cohort fixed effects, and trends by year of implementation$\times$rural/urban parish.

3.86% (the proportion of compliers among treated is 3.84%).

These complier probabilities are common in instrumental variables (IV) studies of returns to schooling (Angrist and Krueger, 1991; Acemoglu and Angrist, 2000).[18] By exploiting changes in distance to schools, we estimate a LATE that has important policy relevance. While studies using compulsory school reforms typically estimate LATEs on the compliers who are the least willing to attend school (Oreopoulos, 2006), our compliers lie much closer to the median, would have liked to stay in school, but were constrained in their choices by high costs. The always-takers in our sample are 75% of the population, and the never-takers are 21%.

[18] A notable exception was the school-leaving age reform in England (Oreopoulos, 2006; Devereux and Hart, 2010; Clark and Royer, 2013).

3. Results

Having shown in the raw data similar schooling wealth accumulation gradients in Denmark and the US, and having established that the 1958 Danish school reform provides variation explaining causal effect in a range of outcomes consistent with findings from other studies, in this section we estimate the causal effect of schooling on wealth accumulation. We proceed in three steps. First, we characterise the effect of schooling on non-pension wealth accumulation between ages 50 and 60. We describe the effect of schooling on average wealth levels, portfolio composition and distribution. Second, we describe how the effect of schooling unfolds as retirement age approaches and the effect of schooling on pension assets, observed at age 60. Third, we provide evidence on labour market and household mechanisms that contribute to explaining our results.

3.1. The effect of schooling on non-pension wealth

Table 3 presents our estimates of the effect of schooling on the wealth holdings of men aged from 50 to 60 between 1996 and 2011. The first three columns of the table show the LATE of schooling on mean non-pension net worth normalised by individual permanent income and the effect of schooling on each wealth component. The last three columns show the effect of schooling on the distribution of wealth within a cohort at a specific age. For each outcome we present the results of an OLS regression and two IV regressions, where we use quadratic distance to the closest school offering 8th grade and a dummy indicating the presence of such a school within a 2.5 kilometers radius, respectively. We condition all regressions on the set of fixed effects and cohort trends presented in Section 3 to isolate the policy-induced, exogenous variation in distance to school, and we allow for arbitrary correlation in the error terms within parish of birth.

While schooling is positively correlated with wealth and all its components, we find that the causal effect of schooling on wealth over permanent income is negative.[19] By decomposing total net worth, we show that the majority of this negative effect is due to housing equity, and that schooling significantly decreases the probability of owning real estate .

Liquid assets also decrease by between 6.5% and 9% of permanent income per year of schooling. While in nominal terms the reduction in liquid assets is much smaller than the

[19]Table A.1 in the Appendix replicates the results in Table 3 for nominal values not normalised by permanent income (in thousands of DKK), and shows that the normalisation does not drive our findings.

Table 3
Effect of schooling on wealth holdings

	Effect on the average			Effect on rank (0-100)		
	OLS	IV (quad.)	IV (disc.)	OLS	IV (quad.)	IV (disc.)
Net Worth	0.087 **	-0.598 **	-0.461 **	1.182 **	-4.089 **	-3.626 **
	(0.005)	(0.200)	(0.160)	(0.037)	(1.384)	(1.150)
Liquid assets	0.008 **	-0.090 **	-0.065 **	0.602 **	-3.094 **	-2.761 **
	(0.001)	(0.029)	(0.023)	(0.024)	(0.922)	(0.753)
Housing equity	0.049 **	-0.492 **	-0.418 **	1.093 **	-3.810 **	-3.279 **
	(0.004)	(0.153)	(0.134)	(0.035)	(1.205)	(1.045)
- value	0.088 **	-0.683 **	-0.604 **	1.253 **	-3.809 **	-3.287 **
	(0.006)	(0.219)	(0.192)	(0.037)	(1.292)	(1.152)
- owner	0.027 **	-0.048 *	-0.037 *			
	(0.001)	(0.019)	(0.017)			
Fin. investments	0.023 **	-0.019	0.020	1.088 **	-1.747 +	-1.079
	(0.001)	(0.041)	(0.032)	(0.019)	(1.051)	(0.799)
- participation	0.021 **	-0.038 +	-0.028 +			
	(0.000)	(0.020)	(0.015)			
Unc. Debts	0.008 **	0.003	0.002	0.221 **	-1.142	-1.149 +
	(0.001)	(0.036)	(0.032)	(0.014)	(0.708)	(0.590)
First stage F-stat		20.508	53.619		20.508	53.619

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. All regression include cohort trends by implementation year and rural versus urban areas and municipality of birth, year and cohort fixed effects. IV regressions in columns two and five use quadratic polynomial of distance to the closest public school offering 8th grade as an instrument for schooling; IV regressions in columns four and six, a dummy indicating the presence of such a school within a 2.5 kilometers radius. Ranks range from 0 (bottom) to 100 (top) and are calculated within each cohort-year combination.

reduction in housing equity, schooling affects the relative distributions of the two outcomes similarly. One additional year of schooling at the 8th grade margin moves individuals 3 percentage points down in the distribution of housing equity and the distribution of liquid asset values within a cohort-year group.

In contrast, schooling does not significantly affect the amount of wealth invested in financial markets. While the proportion of people investing in financial markets decreases, average financial investment does not vary significantly, suggesting that for our compliers the direction of the intensive margin response is opposite to that of the extensive margin response.

The contrast between IV and OLS estimates implies that the omitted variable bias

Table 4
Effect of schooling on portoflio composition (proportion of total assets)

	Effect on the average			Effect on rank (0-100)		
	OLS	IV (quad.)	IV (disc.)	OLS	IV (quad.)	IV (disc.)
Liquid assets	-0.024 **	0.035 *	0.027 $^{+}$	-1.333 **	1.302	0.892
	(0.001)	(0.016)	(0.015)	(0.033)	(0.928)	(0.882)
Fin. inv.	0.003 **	0.001	0.004	0.998 **	-1.379	-0.913
	(0.000)	(0.004)	(0.003)	(0.017)	(0.933)	(0.716)
Housing value	0.021 **	-0.036 *	-0.031 *	1.071 **	-1.763 *	-1.515 $^{+}$
	(0.001)	(0.015)	(0.014)	(0.031)	(0.828)	(0.837)
Equity to value†	-0.000	-0.026 **	-0.023 **	-0.012	-2.408 **	-2.094 **
	(0.000)	(0.008)	(0.007)	(0.020)	(0.695)	(0.618)
First stage F-stat		20.941	54.292		20.941	54.292
First stage F-stat†		29.024	70.870		29.024	70.870

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$. All regression include cohort trends by implementation year and rural versus urban areas and municipality of birth, year and cohort fixed effects. IV regressions in columns two and five use quadratic polynomial of distance to the closest public school offering 8th grade as an instrument for schooling; IV regressions in columns four and six, a dummy indicating the presence of such a school within a 2.5 kilometers radius. Ranks range from 0 (bottom) to 100 (top) and are calculated within each cohort-year combination.
†Computed on a sample of real estate owners only.

is of the expected sign. Our identification strategy reveals that unobserved factors such as intergenerational transmission of wealth – e.g. through endowments or inheritances (Bowles and Gintis, 2002; Boserup et al., 2014) – and individual preferences are responsible for the positive association between schooling and wealth held before retirement age. Our results are robust to a battery of sensitivity checks. We replicate the same qualitative results using different specifications (i.e. allowing for municipality-specific cohort trends) and different samples (e.g. excluding renters or land-owners). These robustness checks appear in Tables A.6 and A.7 in the Appendix.

Tables 4 and 5 further characterise the effect of schooling on wealth holdings in terms of portfolio composition and wealth distribution. Table 4 focuses on how savers invest their assets, and shows that the combination of the effects in Table 3 induces more liquid wealth portfolios and higher leverage for those owning real estate. For real estate owners, the equity-to-value ratio decreases by approximately 2 percentage points per year of schooling. Because we estimate the impact of schooling on equity-to-value ratios for a conditional sample, this result does not have a causal interpretation. Nevertheless, these

Table 5

Effect of schooling on the probability of belonging to each quintile in the distribution of a specific wealth component

Quintile	Lower 20%	20-40%	40-60%	60-80%	Upper 20%
	Panel A: Wealth outcomes				
Net Worth	0.023 *	0.029 **	0.017 *	-0.014	-0.054 **
	(0.011)	(0.010)	(0.007)	(0.009)	(0.018)
Liquid assets	0.010	0.023 **	0.021 **	-0.001	-0.053 **
	(0.007)	(0.008)	(0.007)	(0.006)	(0.014)
Housing equity	0.015 *	0.022 *	0.035 **	-0.009	-0.062 **
	(0.007)	(0.010)	(0.009)	(0.010)	(0.017)
- if owner	0.028 **	0.007	-0.005	-0.018 *	-0.001
	(0.009)	(0.006)	(0.007)	(0.008)	(0.014)
Fin. investments			0.036 *	-0.033 **	-0.003
			(0.018)	(0.009)	(0.013)
- if participant	-0.001	-0.025 **	-0.007	0.004	0.037 *
	(0.010)	(0.007)	(0.005)	(0.005)	(0.015)
Unc. Debts	-0.005	0.023 **	0.012 +	-0.014 *	-0.016 *
	(0.008)	(0.009)	(0.006)	(0.007)	(0.007)
	Panel B: Pension outcomes at 60				
Non-pension assets	0.015	0.049 *	0.002	0.000	-0.054 **
	(0.012)	(0.019)	(0.009)	(0.009)	(0.018)
Pension wealth	0.006	0.013	-0.005	-0.011	0.002
	(0.008)	(0.015)	(0.010)	(0.011)	(0.013)
Annuities (per year)		0.013	-0.039 **	-0.022 *	0.047 **
		(0.013)	(0.010)	(0.010)	(0.010)

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. All regression include cohort trends by implementation year and rural versus urban areas and municipality of birth, year and cohort fixed effects. IV regressions in columns two and five use quadratic polynomial of distance to the closest public school offering 8th grade as an instrument for schooling; IV regressions in columns four and six, a dummy indicating the presence of such a school within a 2.5 kilometers radius. Each column reports the estimated effect on schooling on dummy variables indicating whether an observations belongs to a given quintile in the wealth distribution of a specific wealth component within a single cohort and year. Coefficients sum to zero horizontally.

results show that schooling not only decreases individual wealth accumulation over the working life but also changes its qualitative composition.

Table 5 estimates the effect of schooling on the probability of belonging to each of the quintiles in the distribution of each wealth component, characterising in detail

the distributional effects behind the mean estimates in Table 3. The negative effect of schooling is not homogeneous across the wealth distribution but concentrated in the highest quintile. One year of schooling decreases the probability of belonging to the top 20% of the wealth distribution by about 5 percentage points, and increases the probability of belonging to the bottom half of the wealth distribution. Schooling has a similar impact on the distributions of liquid assets and housing equity.

3.2. Approaching normal retirement age

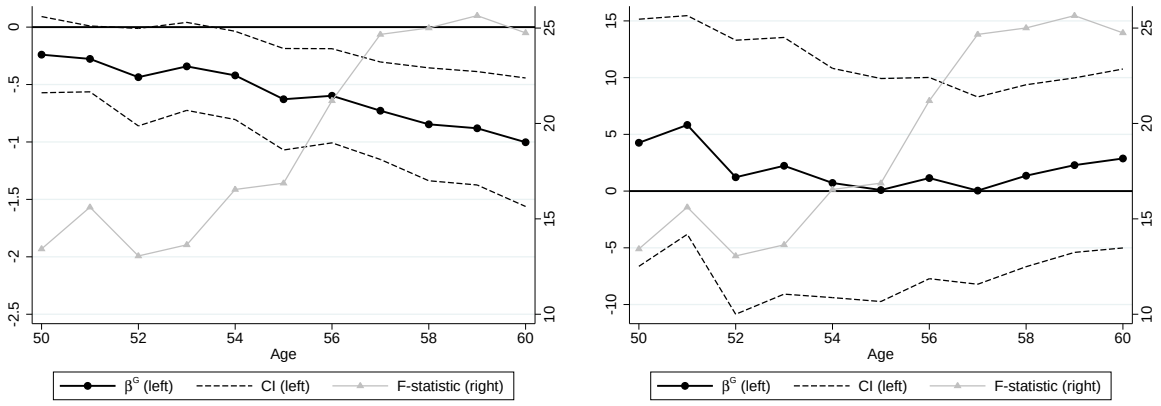
We find suggestive evidence that the negative effect of schooling on wealth holdings after age 50 is due to faster depletion of assets when approaching retirement rather than to a slower accumulation of assets while young. Figure 3 shows how our results change as individuals age. The negative effect of schooling on wealth is stronger at older ages, driven primarily by a depletion of housing equity when approaching retirement. We also cannot reject the hypothesis that schooling has a positive effect on financial investments at age 50, a result consistent with the Black et al. (2015) finding that schooling increases stock market participation on a younger sample.

The top right panel of Figure 3 shows that permanent income does not drive our results, as schooling has no effect on permanent income paths. Moreover, we find no evidence of schooling causing differential trends in house ownership and uncollateralised debts.[20] Nonetheless, the evolution of the causal effect of schooling on total non-pension wealth as retirement age approaches hints at the important role played by pension wealth in the development of saving strategies.

Table 6 reports the effect of schooling on pension rights accumulated at age 60. As a comparison, we also report the effect of schooling on total non-pension assets accumulated by that age. Although non-pension assets, consistent with the results in Table 3, decrease with schooling, pension wealth does not change significantly. However, the value of pension annuities to be paid after retirement increases by about 3.7% per year of schooling. This positive effect on annuities is almost entirely due to employer-based contributions determined by employment contracts, suggesting that schooling has a positive effect on job quality in terms of fringe benefits.

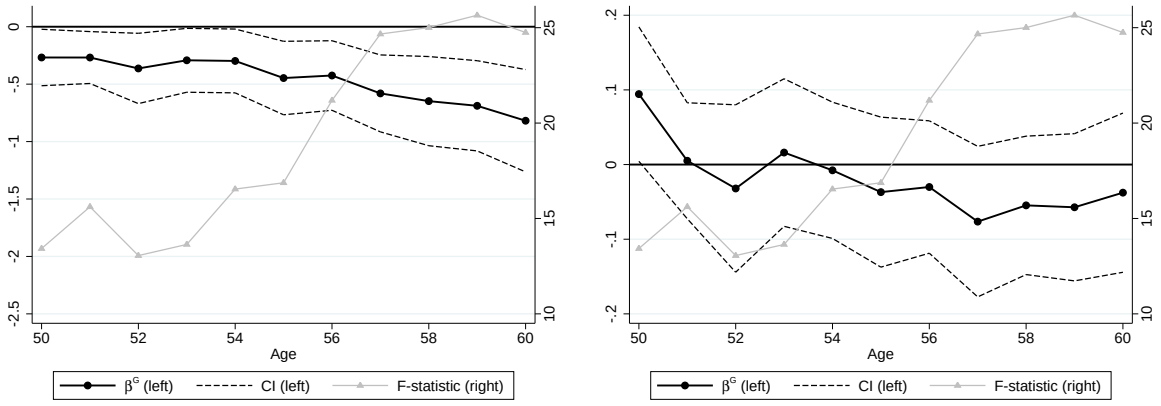
The positive effect of schooling on employment-based pension annuities reduces the incentive to prepare for retirement in other savings vehicles. (Chetty et al., 2014) finds that these occupational pensions crowd out other savings for the most financially sophisticated.

[20] Table A.5 in the Appendix reports results by age category for each wealth component.



(a) *Non-pension net worth*

(b) *Permanent income*



(c) *Housing equity*

(d) *Financial investments*

Fig. 3: *Effects of schooling as normal retirement age approaches*

Notes: The figure estimates the effect (solid black line) and confidence intervals (dashed black line) of schooling separately by age, using a quadratic polynomial in distance to public schools as an instrument for schooling. The gray line plots the first-stage F-statistics for each regression. The scale of the graphs is in years of permanent income except for that in the top right panel, which is in thousands DKK. Table A.5 reports the effect of schooling by age bins.

Table 6
Effect of schooling on pension holdings (60 years of age)

	Prop. of permanent income			Rank in distribution (0-100)		
	OLS	IV (quadratic)	IV (discrete)	OLS	IV (quadratic)	IV (discrete)
Non-pension assets	0.128 **	-1.511 **	-1.398 **	1.148 **	-3.983 **	-3.489 **
	(0.012)	(0.468)	(0.403)	(0.042)	(1.447)	(1.179)
Pension wealth	0.139 **	-0.076	-0.021	1.505 **	-0.277	-0.134
	(0.003)	(0.114)	(0.106)	(0.019)	(0.939)	(0.820)
- by employer	0.081 **	0.057	0.053	0.927 **	1.230	0.682
	(0.002)	(0.066)	(0.061)	(0.024)	(0.752)	(0.658)
Annuities (per year)	0.022 **	0.037 **	0.033 **	1.483 **	1.813 *	1.623 *
	(0.000)	(0.008)	(0.007)	(0.037)	(0.765)	(0.721)
- by employer	0.020 **	0.034 **	0.030 **	1.263 **	1.357 +	1.112
	(0.000)	(0.008)	(0.007)	(0.035)	(0.778)	(0.714)
First stage F-stat		21.731	58.082		21.731	58.082

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. All regression include cohort trends by implementation year and rural versus urban areas and municipality of birth, year and cohort fixed effects. IV regressions in columns two and five use quadratic polynomial of distance to the closest public school offering 8th grade as instrument for schooling; IV regressions in columns four and six, a dummy indicating the presence of such a school within a 2.5 kilometers radius. Ranks range from 0 (bottom) to 100 (top) and are calculated within each cohort.

However, this mechanism alone cannot explain all of the negative effect of schooling on wealth.[21] Several other labour market mechanisms, such as job security and individual mobility, can explain why schooling causes less wealth to be accumulated outside pension funds as retirement approaches. In the remainder of this section we explore the role of labour market outcomes and household composition.

3.3. Labour market and household mechanisms

In the top panel of Table 7 we focus on labour market mechanism, computed for ages 40 through 49 for all individuals in our sample. We test whether schooling causally impacts average gross and permanent income, the probability of receiving unemployment benefits, the likelihood of changing the municipality of work in any given year (labour mobility), and the probability of self-employment. We find that while schooling has a positive impact on average income during one's 40's, it does not affect unemployment risk.

[21] Even assuming a zero discount rate and perpetual annuities, the effect of schooling on annuity claims would take more than 38 years to compensate for the negative effect on wealth held outside pension funds.

Table 7

Mechanisms: Effect of schooling on labour market outcomes and household composition

	Age 40-49			Age 50-60		
	OLS	IV (quad.)	IV (disc.)	OLS	IV (quad.)	IV (disc.)
	Panel A: labour market outcomes					
Personal income	16.85 **	14.35 **	16.93 **	16.93 **	7.718	11.29 *
	(0.261)	(4.560)	(3.978)	(0.279)	(5.588)	(4.745)
Permanent income				10218 **	1556	4457
				(171.9)	(4413)	(3473)
Unemployment	-0.013 **	0.001	-0.003	-0.006 **	-0.000	-0.003
	(0.000)	(0.008)	(0.007)	(0.000)	(0.006)	(0.006)
Mobility	-0.003 **	0.015 **	0.015 **	-0.001 **	0.009 *	0.004
	(0.000)	(0.006)	(0.005)	(0.000)	(0.004)	(0.003)
Self-employed	-0.003 **	-0.057 **	-0.044 **	0.000	-0.036 **	-0.028 **
	(0.000)	(0.013)	(0.010)	(0.000)	(0.011)	(0.009)
	Panel B: Household composition					
Single	-0.019 **	0.027 *	0.013	-0.021 **	0.027 *	0.014
	(0.000)	(0.011)	(0.010)	(0.000)	(0.013)	(0.012)
Divorced	-0.007 **	0.031 **	0.025 **	-0.008 **	0.028 **	0.027 **
	(0.000)	(0.009)	(0.009)	(0.000)	(0.011)	(0.010)
# children				0.016 **	-0.038	-0.037
				(0.001)	(0.035)	(0.032)
	Panel C: Spouse outcomes					
Years of schooling				0.361 **	0.533 **	0.544 **
				(0.003)	(0.087)	(0.087)
Personal income				7.166 **	7.413 *	7.518 *
				(0.120)	(3.269)	(3.148)
Net worth				0.038 **	-0.037	0.003
				(0.001)	(0.034)	(0.031)
House owner				0.016 **	0.017	0.034 **
				(0.000)	(0.013)	(0.013)

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.01$, + $p < 0.1$. All regression include cohort trends by implementation year and rural versus urban areas and municipality of birth, year and cohort fixed effects. IV regressions in columns two and five use quadratic polynomial of distance to the closest public school offering 8th grade as instrument for schooling; IV regressions in columns four and six, a dummy indicating the presence of such a school within a 2.5 kilometers radius. The results in the top panel are estimated on a cross-section of all males in the sample. The results in the middle panel are computed on the full panel dataset. The results in the bottom sample are computed conditional on the individuals being married or in a registered partnership.

However, schooling increases job mobility and significantly decreases the probability of being self-employed in our sample. Because higher geographical mobility makes housing equity less attractive, this mechanism contributes to explaining our findings on wealth held outside a pension fund, effects primarily driven by reductions in housing equity and home ownership rates. Moreover, as the self-employed are over-represented in the top 20% of the wealth distribution (Cagetti and De Nardi, 2006), that schooling influences the likelihood of self-employment is consistent with the results in Table 5, which shows that the effects of schooling on non-pension net-worth are driven by the richest quintile of the population.

Conditional on being married, we find no evidence of wealth re-allocation within couples, consistent with the Danish tax system's being neutral to asset allocation within couples. As in McCrary and Royer (2011), we find no significant effect of schooling on completed fertility, dampening the role of bequest motives as a driver for our results.

The results in this section show that labour market rather than household mechanisms are the strongest channels linking schooling with wealth accumulation. These labour market mechanisms operate through increased mobility, reduced self-employment and better employer-based pension provisions.

4. Conclusions

This paper is the first to exploit policy-induced variation in the supply of schooling to identify the causal effect of schooling on wealth accumulation approaching retirement. We show that while wealth normalized by permanent income correlates positively with schooling, this association is likely due to unobserved confounders. The causal effect of schooling is to decrease the total amount of wealth accumulated between ages 50 and 60, normalised by individual permanent income.

We show that the majority of the effect of schooling on reducing wealth over permanent income originates from reduced housing equity, greater leverage and more liquid portfolios. Causal mechanisms operate through the labour market: schooling increases the value of pension annuities through employer contributions and increases job mobility, making lumpy investments in housing equity less attractive. Schooling also lowers the likelihood of self-employment.

The effects of schooling on saving trajectories has broader consequences than on the under/over-saving margin via the ability to plan due to increased literacy. Schooling affects a variety of individual economic choices over a working life, and these choices need

not imply higher levels of wealth approaching retirement.

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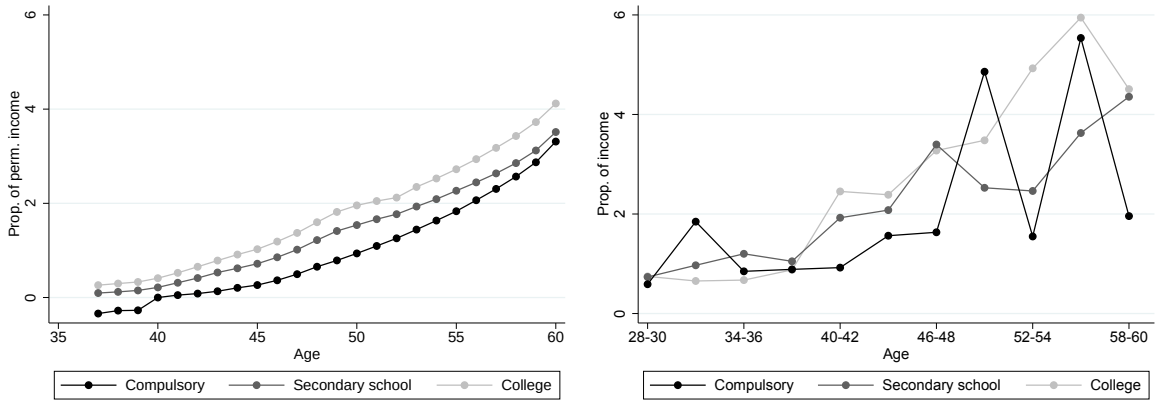
Appendix



(a) *Denmark* (b) *United States*

Fig. A.1: *Lifecycle wealth accumulation byeducation type*

Table A.1

Main results, not normalized by permanent income (1000s of Danish Kroner)

	OLS	IV (quad.)	IV (disc.)
	Wealth outcomes (50-60)		
Net Worth	40.71 **	-125.125 *	-89.414 *
	(1.563)	(56.46)	(44.09)
Liquid assets	5.674 **	-18.873 *	-11.763 *
	(0.211)	(7.778)	(5.906)
Housing equity	28.42 **	-105.993 *	-85.574 *
	(1.155)	(44.39)	(37.82)
- housing value	0.000 **	-0.001 **	-0.001 **
	(0.000)	(0.000)	(0.000)
- house owner	0.027 **	-0.048 *	-0.037 *
	(0.001)	(0.019)	(0.017)
Fin. investments	9.49 **	-1.813	11.67
	(0.367)	(13.69)	(10.13)
- participation	0.021 **	-0.038 +	-0.028 +
	(0.000)	(0.020)	(0.015)
Unc. Debts	2.875 **	-1.553	3.748
	(0.198)	(10.21)	(8.866)
First stage F-stat		20.508	53.619
	Pension outcomes (60)		
Non-pension assets	69.72 **	-295.008 *	-233.152 *
	(3.226)	(117.1)	(93.24)
Pension wealth	59.49 **	-8.713	6.494
	(0.808)	(41.50)	(36.10)
- by employer	33.34 **	24.32	17.13
	(0.493)	(23.66)	(21.49)
Annuities (per year)	7.359 **	11.00 **	10.03 **
	(0.128)	(2.125)	(2.048)
- by employer	6.903 **	9.72 **	8.649 **
	(0.117)	(2.074)	(1.975)
First stage F-stat		21.463	58.018

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. All regressions include cohort trends by implementation year and rural versus urban areas and municipality of birth, year and cohort fixed effects. IV regressions in columns two and five use quadratic polynomial of distance to the closest public school offering 8th grade as instrument for schooling; IV regressions in columns four and six, a dummy indicating the presence of such a school within a 2.5 kilometer radius.

Table A.2
Replication of Oreopoulos and Salvanes (2011)

	Twins (NO)	Compulsory schooling (US)	Our identification (DK) IV (quad.)	IV (disc.)
Log income[a]	0.048 **	0.131 **	0.034 **	0.038 **
	(0.003)	(0.006)	(0.012)	(0.011)
Occupational prestige		0.063 **	0.334 **	0.341 **
		(0.003)	(0.042)	(0.035)
Unemployed	-0.005 **	-0.005 *	0.001	-0.003
	(0.000)	(0.002)	(0.008)	(0.007)
Divorced	-0.003 *	-0.010 **	0.031 **	0.025 **
	(0.001)	(0.002)	(0.009)	(0.009)
Spouse schooling[b]	0.229 **		0.533 **	0.544 **
	(0.017)		(0.087)	(0.087)

Notes: Columns 1 and 2 in the table report the results in Tables 2 and 3 (without income controls) in Oreopoulos and Salvanes (2011). They obtain the results in the first column by comparing twins with different levels of schooling in Norway, and those in the second column by exploiting compulsory schooling law changes in US states. The third and fourth columns in the table replicates their results on our sample between 40 and 50 years of age with our identification strategy. ** $p < 0.01$, * $p < 0.01$, + $p < 0.1$.
[a]Income is weekly in the second column.
[b]Spouse schooling is measured between the ages of 50 and 60 in the last two columns.

Table A.3
First stage: Effect of distance from public school on years of schooling

	Wealth sample (full)			Pension sample (age 60)		
	Linear	Quadratic	Discrete	Linear	Quadratic	Discrete
Distance (km)	-0.023 **	-0.068 **		-0.024 **	-0.072 **	
	(0.004)	(0.012)		(0.004)	(0.012)	
Distance squared		0.309 **			0.322 **	
		(0.065)			(0.067)	
Distance $\leq$ 2.5Km			0.299 **			0.311 **
			(0.041)			(0.041)
# Observations	3649100	3634071	3634071	288053	288053	288053
# clusters	2002	1964	1964	1998	1998	1998
F-statistic	32.406	20.611	53.666	32.416	21.463	58.018

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. All regressions include cohort trends by implementation year and rural versus urban areas and municipality of birth, year and cohort fixed effects.

Table A.4

First stage, placebo: Effect of distance from private school on years of schooling

	Wealth sample (full)			Pension sample (age 60)		
	Linear	Quadratic	Discrete	Linear	Quadratic	Discrete
Distance (km)	-0.004	0.009		-0.006	-0.001	
	(0.004)	(0.015)		(0.004)	(0.016)	
Distance squared		-0.055			-0.024	
		(0.057)			(0.061)	
Distance $\leq$ 2.5Km			0.213 **			0.265 **
			(0.071)			(0.071)
# Observations	3649100	3649100	3649100	288053	288053	288053
# clusters	2002	2002	2002	1998	1998	1998
F-statistic	1.123	1.441	9.108	2.642	1.746	13.800

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$. All regressions include cohort trends by implementation year and rural versus urban areas and municipality of birth, year and cohort fixed effects.

Table A.5
Effect of schooling on wealth by age

	Age at which the effect is estimated					
	50-51	52-53	54-55	56-57	58-59	60
Net Worth	-0.261 $^{+}$	-0.390 $^{+}$	-0.531 *	-0.665 **	-0.862 **	-1.002 **
	(0.151)	(0.201)	(0.207)	(0.209)	(0.247)	(0.285)
Liquid assets	-0.025	-0.050	-0.071 *	-0.075 *	-0.133 **	-0.171 **
	(0.027)	(0.037)	(0.033)	(0.031)	(0.036)	(0.045)
Housing equity	-0.268 *	-0.331 *	-0.377 *	-0.507 **	-0.667 **	-0.818 **
	(0.117)	(0.146)	(0.150)	(0.160)	(0.194)	(0.227)
- value	-0.410 *	-0.559 *	-0.600 **	-0.665 **	-0.851 **	-1.084 **
	(0.182)	(0.231)	(0.223)	(0.218)	(0.257)	(0.299)
- owner	-0.055 *	-0.053 *	-0.042 *	-0.038 *	-0.042 *	-0.048 **
	(0.024)	(0.026)	(0.020)	(0.017)	(0.017)	(0.017)
Fin. investments	0.045	-0.003	-0.023	-0.054	-0.056	-0.038
	(0.037)	(0.051)	(0.047)	(0.046)	(0.047)	(0.054)
- participation	-0.026	-0.052 $^{+}$	-0.049 *	-0.040 *	-0.040 *	-0.044 *
	(0.026)	(0.029)	(0.023)	(0.019)	(0.018)	(0.019)
Unc. Debts	-0.012	-0.005	-0.060	-0.029	-0.006	0.025
	(0.049)	(0.054)	(0.051)	(0.042)	(0.042)	(0.048)
Perm. Income	5.141	1.783	0.361	0.573	1.803	2.874
	(5.116)	(5.862)	(5.027)	(4.310)	(3.964)	(4.024)
First stage F-stat	14.860	13.629	16.847	23.193	25.393	24.748

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$. All regressions include cohort trends by implementation year and rural versus urban areas and municipality of birth, year and cohort fixed effects. All regressions use quadratic polynomial of distance to the closest public school offering 8th grade as instrument for schooling.

Table A.6
Robustness checks - alternative specifications

	Baseline	Before 2008	Only 2000	Municipality of residence FE	Municipality of birth cohort trends
	Panel A: Wealth outcomes				
Net Worth	-0.598 **	-0.561 **	-0.646 **	-0.633 **	-0.571 **
	(0.200)	(0.186)	(0.198)	(0.200)	(0.128)
Liquid assets	-0.090 **	-0.088 **	-0.090 **	-0.093 **	-0.063 **
	(0.029)	(0.028)	(0.029)	(0.030)	(0.019)
Housing equity	-0.492 **	-0.455 **	-0.485 **	-0.510 **	-0.472 **
	(0.153)	(0.139)	(0.141)	(0.152)	(0.099)
- housing value	-0.683 **	-0.652 **	-0.605 **	-0.690 **	-0.670 **
	(0.219)	(0.203)	(0.188)	(0.212)	(0.138)
- house owner	-0.048 *	-0.048 *	-0.048 *	-0.045 *	-0.036 **
	(0.019)	(0.019)	(0.019)	(0.019)	(0.012)
Fin. investments	-0.019	-0.020	-0.046	-0.022	-0.006
	(0.041)	(0.041)	(0.047)	(0.041)	(0.026)
- participation	-0.038 +	-0.041 *	-0.026	-0.040 *	-0.030 *
	(0.020)	(0.020)	(0.019)	(0.020)	(0.013)
Unc. Debts	0.003	0.001	-0.025	-0.008	-0.031
	(0.036)	(0.036)	(0.044)	(0.037)	(0.027)
First stage F-stat	20.508	20.624	21.577	21.415	41.060
Observations	3649100	3380224	333977	3649100	3649324
	Panel B: Pension outcomes				
Pension wealth	-0.076	-0.184	-0.381	-0.051	-0.005
	(0.114)	(0.130)	(0.278)	(0.107)	(0.074)
Annuities	0.037 **	0.032 **	0.013	0.039 **	0.036 **
	(0.008)	(0.009)	(0.021)	(0.008)	(0.006)
First stage F-stat	21.731	18.346	5.278	25.033	47.786
Observations	284160	212218	20245	288053	288114

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. All regressions use quadratic polynomial of distance to the closest public school offering 8th grade as instrument for schooling.

Table A.7
Robustness checks - alternative samples

	Baseline	Participate in fin. mkts.	Home howners	Never public servant	Never *over-gangsy-delse*	Never self-employed	Never owned land	Before actual retirement
	Panel A: Wealth outcomes							
Net Worth	-0.598 **	-0.395 +	-0.469 **	-0.641 **	-0.686 **	-0.337 *	-0.341 *	-0.866 **
	(0.200)	(0.209)	(0.158)	(0.216)	(0.205)	(0.159)	(0.171)	(0.276)
Liquid assets	-0.090 **	-0.053	-0.084 **	-0.104 **	-0.082 *	-0.046 +	-0.062 *	-0.175 **
	(0.029)	(0.032)	(0.024)	(0.032)	(0.032)	(0.024)	(0.030)	(0.059)
Housing equity	-0.492 **	-0.417 *	-0.401 **	-0.513 **	-0.497 **	-0.248 *	-0.244 *	-0.641 **
	(0.153)	(0.169)	(0.125)	(0.165)	(0.147)	(0.112)	(0.115)	(0.206)
- housing value	-0.683 **	-0.584 *	-0.504 **	-0.714 **	-0.684 **	-0.207	-0.185	-0.647 **
	(0.219)	(0.227)	(0.163)	(0.233)	(0.205)	(0.140)	(0.143)	(0.236)
- house owner	-0.048 *	-0.018 +		-0.054 **	-0.038 +	-0.039 +	-0.033	
	(0.019)	(0.010)		(0.020)	(0.020)	(0.021)	(0.021)	
Fin. investments	-0.019	0.027	-0.011	-0.018	-0.074	0.002	0.014	-0.062
	(0.041)	(0.062)	(0.038)	(0.043)	(0.051)	(0.038)	(0.041)	(0.062)
- participation	-0.038 +		-0.024	-0.042 *	-0.061 **	-0.028	-0.018	
	(0.020)		(0.015)	(0.021)	(0.022)	(0.021)	(0.021)	
Unc. Debts	0.003	0.048	0.027	-0.005	-0.033	-0.044	-0.049	0.012
	(0.036)	(0.036)	(0.023)	(0.039)	(0.046)	(0.034)	(0.042)	(0.070)
First stage F-stat	20.508	28.198	28.808	19.619	17.349	15.764	14.028	18.438
Observations	3649100	1324794	2642107	3437604	1697773	3048611	3365788	110810
	Panel B: Pension outcomes							
Pension wealth	-0.076	0.115	0.043	-0.043	-0.103	-0.102	-0.139	
	(0.114)	(0.137)	(0.106)	(0.116)	(0.124)	(0.129)	(0.140)	
Annuities	0.037 **	0.050 **	0.049 **	0.032 **	0.039 **	0.036 **	0.031 **	
	(0.008)	(0.010)	(0.009)	(0.007)	(0.010)	(0.010)	(0.010)	
First stage F-stat	21.731	21.743	24.885	21.445	16.029	17.245	14.843	
Observations	284160	122266	218823	270198	152636	240828	265018	

Notes: Standard errors clustered at the parish of birth level in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$. All regressions use quadratic polynomial of distance to the closest public school offering 8th grade as instrument for schooling.