# *K*-Returns to Education*

Andreas Fagereng Luigi Guiso Martin B. Holm Luigi Pistaferri

First draft: 3rd September 2019 This version: 18th December 2025

**Abstract**

We study the causal effect of general education on returns to wealth (*k*-returns) using a school reform that increased the length of compulsory schooling in Norway in the 1960s. OLS estimates reveal a strong, positive, and statistically significant correlation between education and returns to individual net worth. This effect disappears in IV regressions, implying that general education has no discernible causal effect on individual performance in capital markets, whose heterogeneity largely reflects individual ability and risk tolerance. In contrast, we find that education causes higher returns in the labor market (*l*-returns). We consider a number of potential explanations for this important asymmetry.

**Keywords**: Returns to education, Heterogeneity in returns to wealth, Wealth inequality
**JEL**: G5, I26.

*We thank three anonymous referees and seminar participants at Columbia University, University of Oslo, BI Norwegian Business School, and University of Zurich for comments and suggestions. We have benefited from comments and discussions with Manudeep Bhuller, Maxwell Kellogg, Tommaso Porzio, and Antoinette Schoar. The article was written as part of a research project at Statistics Norway supported by the Finance Market Fund (Research Council of Norway, project number 294978).
**Andreas Fagereng**: BI Norwegian Business School, Email: afagereng@gmail.com.
**Luigi Guiso**: EIEF and CEPR, Email: luigi.guiso55@gmail.com.
**Martin Blomhoff Holm**: University of Oslo, Email: m.b.holm@econ.uio.no.
**Luigi Pistaferri**: Stanford University, Email: pista@stanford.edu.

# 1 Introduction

Prompted by the need to understand substantial differences in individual incomes, the second half of the $20^{th}$ century witnessed considerable research effort on understanding the determinants of returns to human capital. The seminal paper by Mincer (1958), significantly titled "Investment in Human Capital and Personal Income Distribution," provided an analytically grounded contribution to understanding the causes of income inequality, drawing attention to labor market returns to education—what we label "*l*-returns".

The second decade of the $21^{st}$ century has witnessed a reversal of attention towards returns to wealth. This shift is due to renewed interest in the determinants of wealth concentration, concerns about its rising dynamics in some Western countries, and the conclusion - after several years of research on the topic - that inequality in labor earnings (and thus in returns to human capital) is simply insufficient to explain the large concentration in wealth observed in the data (see De Nardi and Fella, 2017 for a review). In keeping with this conclusion, a new strand of literature has shifted attention from heterogeneity in returns to labor to heterogeneity in returns to financial and physical capital – what we label "*k*-returns" (Benhabib, Bisin and Zhu, 2011; Benhabib, Bisin and Luo, 2017; Benhabib and Bisin, 2018; Gabaix, Lasry, Lions and Moll, 2016; Aoki and Nirei, 2017; Lei, 2019). These papers show that models in which individuals are endowed with idiosyncratic returns to wealth that persist over time can generate a steady-state wealth distribution with a thick right tail resembling what is observed in reality. Moreover, persistent heterogeneity in returns, coupled with a positive correlation between wealth returns and wealth levels, can potentially account for rapid transitions in wealth concentration at the top (Gabaix et al., 2016), similar to those observed in the US over the last three decades (Saez and Zucman, 2016) or in France over the past two centuries (Garbinti, Goupille-Lebret and Piketty, 2021).

While theoretical developments have driven this strand of research, empirical evidence has been limited. Fagereng et al. (2020) use population data on Norwegian households to document that individuals differ systematically and persistently in returns to wealth.[1] Fagereng et al. (2020) further provide evidence that differences in risk exposure explain only part of the observed heterogeneity in wealth returns. Their findings suggest that returns to wealth also reflect systematic differences in the ability to manage wealth, which may stem from formal education, knowledge accumulated through experience, investment in information, or innate or socially acquired ability in identifying investment opportunities. Understanding the sources of this dispersion in wealth returns is key to designing efficient policies to address wealth inequality.

In this paper we study whether formal education, besides causally increasing *l*-returns (as established by a large literature in labor economics),[2] also has a causal positive effect on returns to capital (or *k*-returns). We do so by exploiting exogenous variation in years of schooling arising from a school reform in Norway, which took place in the 1960s and raised compulsory schooling by two years, from 7 to 9 years. Because the reform was implemented at different times in dif-

[1] Bach, Calvet and Sodini (2020) provide analogous evidence for Sweden.

[2] A few classical references include Becker and Chiswick (1966), Card and Krueger (1992), Ashenfelter and Krueger (1994), Rosenzweig (1995), Card (2001), Duflo (2001), Heckman, Lochner and Todd (2006), and Oreopoulos (2006). Psacharopoulos and Patrinos (2018) provide a recent review of the literature.

ferent municipalities (for arguably quasi-random reasons), it provides exogenous variation across cohorts in the amount of schooling people accumulate. This Norwegian school reform has been used by Aakvik, Salvanes and Vaage (2010) to study the effect of compulsory schooling on school attainment and *l*-returns to education. They document that the reform encouraged treated individuals to undertake more education beyond the compulsory level, which in turn caused labor earnings to rise. More recently, Bhuller, Mogstad and Salvanes (2017) have relied on the same Norwegian reform to study the causal effect of education on lifetime labor earnings, using the wage histories of individuals who were affected and unaffected by the reform. They find that reform-induced additional schooling causes higher lifetime earnings as well as generating steeper age-earnings profiles. Finally, Black, Devereux and Salvanes (2005) use the reform to study the causal effect of education on the intergenerational transmission of human capital. In all these instances, the reform instrument is statistically powerful enough to identify causal effects of education on *l*-returns or on parents' investment in their offspring's human capital. Our paper is the first to provide evidence on the causal effect of education on *k*-returns.

Following Fagereng et al. (2020), we rely on administrative records for the entire population of Norway to construct measures of annual returns to net worth and its components. Our main focus is on a comprehensive measure of returns to net wealth, net capital income divided by gross assets, otherwise known as the return on assets (*roa*). It measures how much net income the individual generates out of $1 worth of assets. However, we will also provide results for the return to individual components (e.g., deposits, housing, risky assets, etc.) on each individual's balance sheet. Because net worth reflects all sources of wealth as well as liabilities, its return captures *all* potential motives for differences in individuals' *k*-returns, and thus all potential channels through which education and individual ability may affect them. Hence, the return to net worth can be interpreted as a sufficient statistic for an individual's performance in managing her own savings. In order to trace the effect of an individual's education on the return on his/her own wealth, we focus on single individuals and study the effect of education separately for males and females.[3]

In most of our analyses, we use wealth data in 2004 as the initial condition, and the period 2004-2015 to obtain the relevant estimates of *k*-returns. Since our focus is on individuals exposed to the reform, and thus born between 1943 and 1963, the sample for the estimation of *k*-returns includes individuals aged 41-61 in 2004 and 52-72 in our last sample year (2015). In OLS regressions, we find that education (as measured by years of schooling) has a positive, large, and significant association with returns to net worth. This is true for both males and females with only small differences in the slope parameter. In the pooled male-female sample of individuals aged 41-72, an additional year of schooling is associated with 16 basis points higher returns on wealth. Hence, an individual with 16 years of schooling (a college degree) earns on average an annual return on net worth that is 64 basis points higher than that earned by an individual with a high school diploma (12 years of schooling) and 112 basis points higher than that of someone who just completed the

[3]In general, the return to wealth for married couples depends on the education level of both partners. However, the weight played by the education of each spouse depends on who is in charge of wealth management within the family. Because the allocation of the responsibility of household wealth management between spouses varies from household to household (possibly as a function of education itself as well as factors that are unobserved, at least to the econometrician) tracing the effect of education on returns without knowledge of who makes decisions is complicated. We discuss this issue in Section 6.

post-reform compulsory schooling. Assuming that the effect of education on returns to wealth is the same among people younger than 40 and constant over the working life cycle, our estimates imply that a college graduate would enter retirement with roughly 17% higher wealth than a high-school graduate, and 33% higher wealth than someone who completed just compulsory schooling.[4]

The significant association between education and the return to net worth is confirmed when looking at separate components of net worth, namely gross wealth and debt, as well as subcomponents (real wealth and financial wealth; mortgage debt and consumer debt). This implies that the correlation between education and the return to net worth results not only from differences in the allocation of wealth among its components but also from differences in individual returns within each asset or liability component of net worth.

Needless to say, the positive effect on $k$-returns we observe in OLS regressions may arise because education happens to be correlated with unobserved wealth management ability or risk tolerance. Indeed, when we run IV regressions using treatment by the reform as an instrument for education, the effect of education drops to values close to zero and loses its statistical significance. Hence, we find no discernible causal effect of education on $k$-returns. We reach the same conclusion if we use a twins design to control for unobserved ability: education predicts returns to net worth in OLS regressions on the sample of twins but the effect vanishes when we control for twin-fixed effects.

This important empirical result is specific to $k$-returns. If we estimate standard Mincerian OLS regressions of log earnings on years of education, we find a positive and highly statistically significant correlation. The effect decreases in size and remains highly significant in IV regressions that use exposure to the reform as an instrument for schooling, consistent with the presence of an omitted ability bias. We obtain the same results in the twins sample design. Thus, the gap between the OLS and IV regressions when estimating $k$-returns is not a reflection of lack of power of the instrument (a concern that is formally dismissed from first-stage statistics). Rather, our estimates suggest an important economic explanation: general skills learned in school are important in the labor market domain but not for the efficient management of own savings. What appears to matter for individual performance in capital markets are skills that are not acquired through general education, but which also serve as an important input for investment in education (hence the correlation between wealth returns and education in OLS regressions).

This interpretation is consistent with the findings of a recent paper by Barth, Papageorge and Thom (2020). Focusing on a sample of US investors, they find that genetic endowment - a measure of pre-existing ability - predicts wealth at retirement and is strongly associated with education (and clearly cannot be reverse-caused by education). They interpret their findings as suggesting that genetic endowment affects wealth at retirement also because it shapes people's capacity to deal with complex investment decisions. Our findings lend support to their interpretation

[4] If the education premium on $k$-returns is larger among younger cohorts—for example, because the effect of general education fades with age—then this back-of-the-envelope calculation should be interpreted as a lower bound of the actual effect. In Section 6.4 we discuss in detail whether general education fades with age and review evidence on the relationship between the level of general education and the investment in financial information acquisition at later stages of the life cycle.

and, importantly, pin down one key channel through which wealth management capability affects wealth accumulation: the increase in returns to wealth.

Our work is related to a recent wave of papers, partly inspired by the theory of human capital and investment in education, as well as by the seminal work of Arrow (1987). This literature argues that financial skills, whether acquired through education or stemming from innate ability, are key in explaining heterogeneity in returns to wealth and thus wealth inequality (Peress, 2003; Best and Dogra, 2019; Kacperczyk, Nosal and Stevens, 2019; Lei, 2019). For example, Christiansen, Joensen and Rangvid (2008), Cole, Paulson and Shastry (2014), and Black et al. (2018) document how various education treatments positively affect stock market participation. However, even if education causes higher stock market participation, it does not follow that it also causes a higher return on a broad measure of wealth. To establish this, one must show that increased investment in stocks translates into higher overall asset returns—possibly by shifting investments away from lower-yielding assets. Unlike studies focused only on stock market participation, our analysis leverages a comprehensive measure of wealth returns, avoiding this limitation and providing clearer evidence on the relationship between education, financial skill, and wealth outcomes.

Our paper is also related to the literature on financial literacy and financial education. Many papers document a correlation between measures of financial literacy and ("better") financial outcomes. A study close to ours is Girshina (2019) who documents a positive association between education and returns using Swedish data by comparing siblings. However, as Hastings, Madrian and Skimmyhorn (2013) argue in their thoughtful review of this literature, the causality of the effects still needs to be established. Our results suggest that unobserved heterogeneity in ability may be behind at least some of the correlations between financial outcomes and measures of financial literacy. They also shed a bit of skepticism on the use of financial education as an effective policy tool to ameliorate individual skills to effectively manage personal savings.

The rest of the paper is organized as follows. To motivate the importance of focusing on the effect of education on *k*-returns, Section 2.1 offers an illustrative example. In Section 2.2 we set up an analytical framework of the determinants of returns on wealth. We start from a frictionless environment where there is no room for education and ability to affect returns to wealth and show how the latter matters when specific frictions are allowed for. In Section 3 we lay down the empirical model and discuss the identification challenges. Section 4 describes our data sources. Section 5 illustrates the Norwegian reform and shows properties of the instrument; we also discuss here estimates of the effect of education on *l*-returns. Section 6 shows the results of the estimates of *k*-returns, starting with the OLS regressions and then moving to the IV estimates and the twins-design results. Section 7 offers an interpretation of the empirical findings. Section 8 concludes.

# 2 Analytical Framework

## 2.1 The Importance of Returns Heterogeneity on Wealth: An Example

Skill-induced heterogeneity in *k*-returns can be as influential as *l*-returns to education in generating substantial differences in wealth accumulation at retirement. This point is illustrated by the following example. Consider two individuals, $A$ and $B$, each earning a constant annual labor income of \$100,000 from age 25 until retirement at age 65. Both save 20% of their income throughout their working lives. The only difference between them is the return on their wealth: $A$ earns a persistent 3.5%, while $B$ earns 6%. This difference reflects approximately the gap in average wealth returns between a post-college graduate and an individual with only an elementary education, as indicated by our OLS estimates in Section 5 (Table B.2). Under these assumptions, at retirement, $B$ would have accumulated nearly twice as much wealth as $A$—\$3.30 million versus \$1.77 million. To put this in perspective, consider how much additional labor income $A$ would need to match $B$'s accumulated assets, holding everything else constant. We find that $A$ would need to earn almost double—approximately \$190,000 per year—to have the same wealth as $B$ at retirement. In other words, variation in $k$-returns can produce differences in asset accumulation that are even more pronounced than those caused by remarkable disparities in returns to human capital.[5] While substantial research has focused on $l$-returns to education, the role of skill-induced variation in $k$-returns has received much less attention. Our study is the first to examine the causal effect of general education on returns to wealth.

### 2.2 More general theoretical mechanisms

In classical models of portfolio allocations the only determinant of heterogeneity in returns is risk compensation for portfolio allocation choices, triggered by heterogeneity in preferences for risk. In a Merton (1975) type portfolio model the optimal share $\alpha_i$ invested in risky assets by an individual with relative risk tolerance $\tau_i$ facing a risky assets premium $r^e$ and variance of risky assets returns $\sigma^2$ is: $\alpha_i = \tau_i \frac{r^e}{\sigma^2}$. Investors are assumed to have the same information about returns and have access to all the available risky assets and thus face the same returns distribution. If the return on the safe asset is $r^f$ (the same for all individuals) the average (realized) return on individual wealth will be:

$$r_i^w = r^f + \alpha_i r^e$$

with standard deviation $\alpha_i \sigma$. In this model, the only difference in returns to wealth across individuals is due to differences in the risky asset share - a choice reflecting heterogeneity in risk tolerance. Hence, holding the share in risky assets constant, individuals should earn the same return on wealth and there would be no role for differences in education or talent. Age may affect the optimal share in risky assets because people adjust their portfolio to the life cycle of human capital, as in Merton (1975), but this too is captured by the risky asset share. [6] We call this return to wealth the frictionless return and label it $r_i^F = r^f + \alpha_i r^e$. It measures the return to wealth an

[5]For example, an $l$-return to education of 6.7% per additional year would only double labor income if there were a 15-year gap in schooling. Thus, another way to appreciate the impact of $k$-returns heterogeneity is to note that, with both individuals earning the same $k$-return of 3.5%, $B$ would need 15 more years of schooling and an annual $l$-return of 6.7% to retire with approximately double $A$'s wealth.

[6]In addition, because all people invest in the same (market) portfolio of risky securities, the Sharpe ratio on the return to wealth of each individuals, $\frac{r_i^w - r^f}{\alpha_i \sigma} = \frac{r^e}{\sigma}$ is the same for all individuals, and thus unrelated to any individual observable characteristic, and the same as the market Sharpe ratio.

individual would earn on average if the market were frictionless and individuals were all equally well informed about the available alternatives.

At each point in time the realized return is equal to

$$r_{it}^{w} = r_t^f + \alpha_i r_t^e = r_i^F + \eta_t + \alpha_i \epsilon_t$$

i.e., the sum of a time invariant component - the average frictionless return - and a time-varying random component, where $\eta_t = r_t^f - r^f$ is an aggregate random deviation of the risk free rate from its mean, and $\epsilon_t = r_t^e - r^e$ is the deviation over time of the excess return from the equity premium. Hence, a regression of observed individual returns on time dummies (to capture variation in the risk free rate), time dummies interacted with the risky share, and the risky share itself should absorb all the variation in observed returns, leaving no role for individual ability or education.

The evidence in Fagereng et al. (2020) implies that this representation fails to fit the data. They document substantial heterogeneity in returns to wealth even after controlling for portfolio composition and its changes over time. This heterogeneity may reflect differential ability and differential information about investment opportunities or may reflect systematic differences in formal education or knowledge accumulated with experience in managing own savings. Indeed, a growing literature argues that individuals do differ greatly in their ability to make investment decisions. Recent theoretical papers lend support to this idea by exploring various drivers of ability and information. Lusardi, Michaud and Mitchell (2017) show that heterogeneity in rates of returns can be driven by endogenous differences in financial knowledge accumulated over the life cycle. Building on Arrow (1987), first Peress (2003) and more recently Kacperczyk, Nosal and Stevens (2019) allow investors to differ in sophistication and thus in ability to process information, generating persistent heterogeneity in returns and in Sharpe ratios across investors. Best and Dogra (2019) and Lei (2019) rely on heterogeneity in incentives to gather information to generate heterogeneity in returns to wealth and explain wealth inequality.

To capture these possibilities we modify the expression for individual returns to wealth and write average returns to individual wealth as

$$r_{it}^{w} = r_i^F - d_i + \eta_t + \alpha_i \epsilon_t$$

where $d_i$ is an individual specific function measuring the distance of the average return an individual earns from the frictionless return. We assume that this distance is affected by two general factors: the *knowledge* capital an individual is endowed with, $k_i$, and the *accessibility* to investment opportunities that an individual faces, $z_i$. Thus

$$d_i = d(k_i, z_i)$$

with the distance decreasing in knowledge capital and accessibility, and converging to zero as $k_i$ and $z_i$ approach their frictionless values $k^F$and $z^F$, respectively. Hence, $d(k^F, z^F) = 0$.

In Appendix A we illustrate several mechanisms, operating either through knowledge capital,[7] or through accessibility to investment opportunities,[8] for explaining heterogeneity in returns to net worth across individuals even when they have the same risk tolerance. Which scenario prevails depends on the specific friction assumed. Knowledge capital may differ across individuals because of differences in skills, due to either education or ability.[9] To reflect this dependence we write $k_i = k(E_i, a_i^k)$ - a function of education $E$ and ability $a^k$, which we allow to be specific to $k$-returns. In a world with frictions, returns are sometimes also affected by the level of wealth of the individual through the accessibility channel. The simplest case is when participation in an asset market - such as the stock market or investment in a private business - entails a fixed cost, implying that only those with wealth $w_i$ above a given threshold may be able to invest in the asset. To capture this, we can write $z_i = z(E_i, a_i^k, w_i)$, where education and ability affect accessibility, for example because they affect participation costs. The key point is that, regardless of the specific mechanism at work, the return to net wealth captures *all channels* through which education and ability influence the financial performance of an individual. Hence, the return to net worth is a sufficient statistic for an individual's financial performance.

In the next section we propose a general empirical model meant to capture these mechanisms and discuss the challenges that the identification of the effect of education on returns to wealth poses.

## 3 The Empirical Model and Identification

Following the example above we formalize the empirical model as:

$$r_{it}^w = \beta E_i + f_{w,t-1} + f_m + f_{b,t} + u_{it} \tag{1}$$

The left hand side is the return to net worth of individual $i$ in year $t$ born in year $b$ in municipality $m$, reflecting the panel nature of our data. $E_i$ is a measure of educational attainment, measured either by the number of years of schooling (as is often done in the $l$-returns to education literature) or by a set of educational attainment dummies. We also let $k$-returns depend on lagged wealth percentiles fixed effects ($f_{w,t-1}$) to reflect scale effects on returns. In addition, returns may depend on a common time-varying component per birth year$f_{b,t}$, a common component depending on the municipality where the individual was born$f_m$, and a random component $u_{it}$ capturing unobserved persistent heterogeneity in wealth returns as well as "good or bad luck".[10] Controlling for lagged wealth is crucial, since education affects returns to wealth *directly* as well as *indirectly*, by increasing labor income and hence savings. It is this net-of-$l$-returns effect of education that we

[7] For example because of costly information collection (Peress, 2003; Kacperczyk, Nosal and Stevens, 2019; Best and Dogra, 2019; Lei, 2019), endogenous acquisition of financial capabilities (Jappelli and Padula, 2017; Lusardi, Michaud and Mitchell, 2017), costly financial advice (Gennaioli, Shleifer and Vishny, 2015), or the presence of search frictions in the debt and safe asset markets (Fagereng et al., 2020).

[8] For example because of costly stock market participation or limited access to to private business investment (Luttmer, 1999).

[9] We use the term skills to denote an individual capacity to manage his/her assets; these skills may increase with education or may reflect individual ability or talent.

[10] Note that the time-varying component for birth year controls implicitly for age due to the well-known collinearity between age, time and cohort.

are (mostly) interested in.

The identification of the parameter $\beta$ poses a major challenge, since education may be correlated with unobserved heterogeneity in wealth returns. The endogeneity concerns in equation (1) apply equally well to the wealth term, since wealth is likely correlated with unobserved ability. In this case, however, the panel dimension of the data is helpful, since the position in the wealth distribution varies over time. To deal with these challenges we consider a two-step strategy. In the first step we obtain a consistent estimate of the lagged wealth percentile fixed effects by exploiting the panel dimension of our data. Specifically, we consider the first differenced version of (1) to eliminate unobserved fixed heterogeneity and estimate:

$$\Delta r_{it}^{w} = \Delta f_{w,t-1} + \Delta f_{b,t} + \Delta u_{it} \tag{2}$$

This allows us to obtain a consistent estimate of the scale effect (under the assumption that the source of inconsistency in equation (1) is the potential correlation of $f_{w,t-1}$ with the persistent component of $u_{it}$). We can then construct a "scale-adjusted" return measure: $\tilde{r}_{it}^{w} = r_{it}^{w} - \tilde{f}_{w,t-1}$. In the second step, we estimate:

$$\tilde{r}_{it}^{w} = \beta E_i + f_m + f_{b,t} + u_{it} \tag{3}$$

To deal with the endogeneity of education, we rely on an IV strategy that uses the Norwegian school reform of the 1960s discussed in detail in the next section. Hence, the identifying variation comes from comparing individuals born in the same municipality $m$ but in a different years $b$. In a robustness check, we also consider a twins design, in which we make specific assumptions about the nature of unobserved heterogeneity.

# 4 The Data

## 4.1 Data

Our analysis is based on several administrative registries maintained by Statistics Norway, which we link through unique identifiers for individuals and households. In this section, we discuss the broad features of these data; Fagereng et al. (2020) provide more details. We start by using a rich longitudinal database that covers every Norwegian resident from 1967 to 2015. For each year, it provides relevant demographic information (gender, age, marital status, educational attainment) and geographical identifiers (the "demographic database"). Our focus will be on the Norwegian population born between 1943 and 1963, which includes individuals who were potentially affected by the school reform as well as adjacent cohorts that can be used as control group. The demographic database is linked with social security earnings records to form an "*l*-return database", which we use to estimate *l*-returns to education. As traditionally done in the labor economics literature, this database includes individuals during the working phase of their life cycle, i.e., aged 25 to 62, with no restriction on their marital status. However, we look separately at *l*-returns for men and women. The estimation of *k*-returns to education uses a smaller sample (the

"$k$-return database") due to both data and conceptual constraints. Estimation of returns to wealth uses information from tax records containing detailed information about the individual's sources of income from labor and capital, the value of asset holdings and liabilities, a housing transaction registry, a shareholder registry with detailed information on listed and unlisted shares owned, and balance sheet data for the private businesses owned by the individual. These data are only available for the period 2004-2015.[11] The value of asset holdings and liabilities is measured as of December 31. In most of our analyses, we use wealth data in 2004 as the initial condition, and the period 2005-2015 to obtain the relevant estimates. Since our focus is on individuals born between 1943 and 1963, the sample for the estimation of $k$-returns includes individuals aged 41-61 in 2004, the first year we can compute returns, and 52-72 in our last sample year (2015). The age span over which we compute individual returns comprises the years of the life cycle where individuals have already accumulated substantial assets and make relevant investment decisions; hence if education matters the observed sample is ideal to detect its effects. The second data restriction is more conceptual. Both wealth and returns to wealth are household-level variables (unlike labor earnings which are person-level variables). For singles, there is no difference between person and household. For couples, however, it is impossible to disentangle whose education is relevant for the household's return. In order to trace the effect of an individual's education on the return to wealth, we focus on single individuals and study the effect of education separately for males and females.[12]

Administrative data of the type we assemble have several noteworthy advantages relative to survey data, at least for the purpose of our study. First, income and wealth data cover all individuals in the population who are subject to income and wealth tax, including people at the very top of the wealth distribution. The availability of population data is important, since our focus is - by design - on the birth cohorts who were of school age during the 1960s (when the school reform took place). Despite this sample selection and the fact that accurate measures of wealth returns are only available for the 2004-15 period, the availability of population data means that we can still count on a large set of observations in our empirical analyses. Second, because most components of income and wealth are reported by a third party (e.g., employers, banks, and financial intermediaries) and recorded without any top- or bottom-coding, the data do not suffer from the standard measurement errors that plague household surveys or confidentiality considerations that lead to censorship of asset holdings. Third, the Norwegian data have a long panel dimension, which is crucial to obtain a consistent estimate of scale-adjusted returns and thus be able to identify the effect of education on the return to wealth. The long individual panel data dimension is also crucial to obtain reliable measures of individual average returns to wealth and returns volatility. Because of the administrative nature of the data, there is no attrition, except

[11]Before 2004 no shareholder registry is available. The shareholder registry is necessary to identify each stock in the portfolio and be able to obtain accrual measures of annual returns on stocks.

[12]In general, the return to wealth for married couples depend on the education level of both partners. However, the weight played by the education of each spouse depends on who is in charge of wealth management within the family. Because the allocation of the responsibility of household wealth management between spouses varies from household to household (possibly as a function of education itself as well as factors that are unobserved, at least to the econometrician) tracing the effect of education on returns without knowledge of who makes decisions is complicated. We discuss this issue in Section 6.

the (unavoidable) one arising from mortality and emigration. Finally, unique identifiers allow us to match parents with their children. This allows us to pin down where people lived at the time of the school reform in the 1960s when they were of school age. This is crucial to establish who was treated and who was not by the school reform.

Following Fagereng et al. (2020), we impose some minor sample selection aimed at reducing errors in the computation of returns. First, we drop people with less than USD 500 in financial wealth (about NOK 3000). These are typically observations with highly volatile beginning- and end-of-period reported stocks, which tend to introduce large errors in computed returns. Second, we trim the distribution of returns in each year at the top and bottom 1% and drop observations with trimmed returns.

Below, we describe how we construct our measures of wealth and wealth returns.

### 4.2 Wealth aggregates

We measure individual and household wealth by net worth, the most comprehensive measure of household wealth, defined as gross wealth $w_{it}^{g}$ net of outstanding debt $b_{it}$:

$$w_{it} = w_{it}^{g} - b_{it}$$

To obtain a measure of gross wealth we compute the sum of its two main components - financial wealth $w_{it}^{f}$ and non-financial (real) wealth $w_{it}^{r}$. The first is the sum of safe and risky financial assets,[13] the second is the sum of housing and private business wealth. Our data allow us to construct detailed measures of these aggregates. All the components of financial wealth, as well as the value of liabilities, are measured at market value. Private business wealth is obtained as the product of the equity share held in the firm (available from the shareholder registry) and the fiscally-relevant "assessed value" of the firm. The latter is the value reported by the private business to the tax authority to comply with the wealth tax requirements. Every year, private business owners are required by law to fill in a special tax form, detailing the balance sheet of the firm's asset and liability components, most of which are required to be evaluated at market value. The assessed value is the net worth of the firm computed from this form. In principle it corresponds to the "market value" of the company, i.e., what the company would realize if it were to be sold in the market. There are, however, some components of the firm's net worth that are missing, such as the value of intangible capital and residual goodwill. In general, the firm may have an incentive to report an assessed value below the true market value. On the other hand, the tax authority has the opposite incentive and uses control routines designed to identify firms that under-report their value.[14]

[13]Safe financial assets are obtained by summing : (a) cash/bank deposits (in domestic or foreign accounts), (b) money market funds, bond mutual funds, and bonds (government and corporate), and (c) outstanding claims and receivables. Risky financial assets are the sum of: (a) the market value of listed stocks held directly, (b) the market value of listed stocks held indirectly through mutual funds, and (c) the value of other (non-deposit) financial assets held abroad.

[14]Since private business wealth is an important component of wealth, especially for people at the top of the distribution, we have used also alternative measurements of its value. In particular we have used book to market multipliers

The stock of housing includes both the value of the principal residence and of secondary homes. To obtain an estimate of these values, we merge official transaction data from the Norwegian Mapping Authority ("Kartverket"), the land registry, and the population Census, which allows us to identify ownership of each single dwelling and its precise location. Following tax authority methodology (described in Fagereng, Holm and Torstensen, 2020), we estimate a hedonic model for the price per square meter as a function of house characteristics (number of rooms, etc.), time dummies, location dummies and their interactions. The predicted values are then used to impute housing wealth for each year between 2004 and 2015.

The outstanding level of debt from the tax records is the sum of student debt, consumer debt, and long-term debt (mortgages and personal loans).

### 4.3 Measuring returns to wealth

The tax records contain detailed information on all sources of income from capital, which we combine with the data on wealth aggregates to obtain measures of returns to wealth. Our reference measure of return is the return to net worth, defined as:

$$r_{it}^{w} = \frac{y_{it}^{f} + y_{it}^{r} - y_{it}^{b}}{w_{it}^{g} + F_{it}^{g}/2} \tag{4}$$

The numerator is the sum of income from financial assets, $y_{it}^{f}$, and from real assets, $y_{it}^{r}$, minus the interest cost of the debt, $y_{it}^{b}$, all measured as flows accrued in year $t$ (i.e., realized and unrealized components). The denominator follows Dietz (1968), and is defined as $w_{it}^{g} + F_{it}^{g}/2$, the sum of beginning-of-period stock of gross wealth and net flows of gross wealth during the year (assuming they occur on average in mid-year). The second term on the denominator accounts for the fact that asset yields are generated not only by beginning-of-period wealth but also by additions/subtractions of assets during the year.[15]

In equation (4), we express the dollar yield on net worth as a share of *gross* wealth (or total assets) instead of net worth itself. We choose to do so to avoid two practical problems that arise from scaling by net worth. First, for people with net worth close to zero and positive net capital income the measured rate of return on wealth would approach either plus or minus infinity, depending on the sign of the denominator. This would introduce large outliers in the sample. Second, for people with negative net worth and negative net income from wealth, the measured rate of return would be positive. Both problems are relevant in practice. In our sample, around 14% have negative net worth, 4.5% have positive but close to zero net worth (less than $10,000), and almost 10% have negative net worth and negative net income from wealth. Scaling net capital income

for listed companies to obtain an alternative estimate of the value of private business wealth (see Fagereng et al. (2020) for details). Results using this alternative measure are similar.

[15] Without this adjustment, estimates of returns would be biased. The bias is most obvious in the case in which beginning-of-period wealth is "small" but capital income is "large" due to positive net asset flows occurring during the period. Ignoring the adjustment would clearly overstate the return. The opposite problem occurs when assets are sold during the period. Fagereng et al. (2020, Online Appendix) describe how to use information on asset stocks at the beginning and end of period, together with information on the income that is capitalized into wealth, to obtain an estimate of $F_{it}^{g}$. We follow their methodology.

by gross wealth takes care of both problems. Our measure captures the two channels that affect people's returns - the asset allocation channel (the share of assets across asset classes with different returns) and the return picking one (investing in an instrument that pays off more within an asset class, e.g. obtaining a mortgage at a lower rate, investing in higher yield stocks, or depositing money in a bank that remunerates deposits more). In this sense it is a comprehensive measure of individual returns to wealth. Because it captures all sources of heterogeneity in returns to wealth, it also captures all channels through which education may affect $k$-returns.[16] In the accounting literature, (4) is known as the return on assets (*roa*), measuring how much net income the individual generates on \$1 worth of assets.[17] Our main focus will be on the comprehensive measure of returns to wealth in (4), but we will also provide results for the return to individual components (e.g., deposits, housing, risky assets, etc.) on each individual's balance sheet.

The yield from financial wealth is the sum of income earned on all safe assets (interest income on domestic and foreign bank deposits, bond yields, and outstanding claims),[18] as well as yields from mutual funds, from directly held listed shares (the sum of dividends, available from the Shareholder Registry, and accrued capital gains and losses), and from risky assets held abroad. The yield on housing is estimated as: $y_{it}^h = d_{it}^h + g_{it}^h$, where $d_{it}^h$ is the imputed rent net of ownership and maintenance cost and $g_{it}^h$ the capital gain/loss on housing. We follow Eika, Mogstad and Vestad (2020) and assume that the imputed rent is a constant fraction of the house value (which they estimate to be 2.88%); finally, we obtain the capital gain on housing as the change in housing wealth net of housing transactions. The change in housing wealth follows from the hedonic regressions described above (Section 4.2). Consequently, the capital gain on housing exhibits not only variation over time and space, but also across households, since households own dwellings that differ in size (e.g., square footage), composition (number of bedrooms and bathrooms), and

[16] Previous contributions in the area of education and financial performance typically ignore the effect on returns. For instance Christiansen, Joensen and Rangvid (2008) shows that obtaining an economics degree raises stock market participation; Cole, Paulson and Shastry (2014) and Black et al. (2018) show a causal effect of education on financial market participation. Though presumably participating in a broader set of financial markets may result in higher returns on wealth, neither paper shows that education has indeed a causal effect on returns to financial investments following participation, nor that the overall return to wealth increases as a consequence.

More recent research focuses on specific (field-targeted) education. Hvidberg (2023) estimates the causal effect of admission to university-level business and economics programs in Denmark and finds that such targeted education improves financial behavior and substantially reduces the risk of default and delinquency. Altmejd, Jansson and Karabulut (2024) similarly exploit university admission discontinuities and show that enrolling in business or economics programs increases stock market participation and leads to higher risk-adjusted portfolio returns. These findings complement our analysis by highlighting that while general education does not appear to affect returns to wealth, targeted business or economics education can improve specific financial outcomes, including portfolio returns.

[17] Scaling net income from wealth with net worth results in what is known as *roe* (return on equity). Denote with $y_{it}^a = y_{it}^f + y_{it}^r$ the income from assets. Then $roa_{it} = y_{it}^a / w_{it}^g - y_{it}^b / w_{it}^g = r_{it}^a - r_{it}^b l_{it}$, where $r_{it}^a$ is the rate of return on assets, $r_{it}^b$ the interest rate on debt and $l_{it} = b_{it}/w_{it}^g$ is the household leverage (the ratio of debt to assets; we have ignored for the sake of illustration the Dietz adjustment). Clearly, *roa* is a decreasing linear function of leverage. It also increases linearly with the weight in total assets of the assets components. Hence, it treats leverage and the portfolio composition symmetrically. In contrast, the return on equity $roe_{it} = y_{it}^a/(w_{it}^g - b_{it}) - y_{it}^b/(w_{it}^g - b_{it}) = \frac{r_{it}^a - r_{it}^b l_{it}}{1 - l_{it}} = \frac{roa_{it}}{1-l_{it}}$. Holding leverage constant, *roa* and *roe* go hand in hand. But when leverage flips from slightly below to slightly above 1, *roe* flips from extremely positive to extremely negative levels, as long as $roa > 0$; and vice versa from extremely negative to extremely positive if $roa < 0$ due a combination of high leverage and relatively low return on assets $r_{it}^a$. Furthermore if $roa < 0$ and $l_{it} > 1$ , $roe > 0$ - the puzzling finding of a positive rate of return even when the dollar value of the net return on wealth is negative.

[18] Since households rarely report receiving interest payments on outstanding claims and receivables, we impute the return using the rate charged by banks on corporate loans.

type (apartment, semi-detached, detached house, etc.). The income from private businesses is the sum of distributed dividends, available from the Shareholder Registry, and the individual share of the private business's retained profits, which we interpret as a measure of the capital gains on the value of the private business.[19] Lastly, the cost of debt $y_{it}^b$ is the sum of interests paid on all outstanding loans. We define measures of returns on components of net worth (real and financial assets and debt) similarly to equation (4), by scaling the income corresponding to specific assets with their beginning of period stock plus half of the net annual flows, i.e.

$$r_{it}^x = \frac{y_{it}^x}{w_{it}^x + F_{it}^x/2} \tag{5}$$

where $x = (f, r, b)$ stands for "financial", "real" and "debt", respectively, and denotes the component of net worth.

All return measures are net of inflation (using the 2011 CPI) and gross of taxes/subsidies. Because net worth includes all assets and all liabilities, and because we have information on the incomes generated by all its components, the return to net worth captures all sources of heterogeneity in returns to wealth across individuals. Hence, it reflects all potential channels through which education and ability may affect individual wealth management performance.

Table 1 shows summary statistics of the demographic variables (Panel *A*), net worth and its components (Panel *B*) and measures of returns on net worth and several wealth aggregates (Panel C). Statistics are reported for our estimation sample of single individuals who are either treated or non-treated by the school reform, and that are present in all years between 2005 and 2015. Returns to net worth average around 2.7% but are very heterogeneous with a standard deviation of almost 11%.

# 5 The Norwegian school reform

## 5.1 The school reform

Our instrument relies on a compulsory school reform legislated in 1959 by the Norwegian Parliament. The reform mandated an increase in the minimum length of studies from 7 to 9 years.[20] Black, Devereux and Salvanes (2005) provide a detailed description of the reform and we refer to them for a summary of its salient features. The reform was implemented at the municipality level - the highest level of administrative decentralization in Norway. To reduce the burden imposed on the local authorities, the law mandated that all municipalities must complete implementation of the reform by 1973. Implementation took place in a staggered way, implying that for over 10 years, schools in some Norwegian municipalities were run according to the pre-reform rules while schools in other municipalities had already switched to the new rules. Hence, members of

[19] In the absence of information on private firms' market prices and assuming corporate tax neutrality, retained profits can be interpreted as an estimate of the private business's capital gains or losses (see Fagereng et al. (2020) who also show that corporate tax neutrality holds in Norway during our sample period).

[20] Besides raising compulsory schooling, the reform standardized the curriculum with the goal of improving average school quality. Hence, in so far as the reform also increased school quality, our estimates can be interpreted as reflecting both the increase in the number of years of education and the improvement in school quality .

Table 1: Summary statistics

| | Non-treated | | | | Treated | | | |
|---|---|---|---|---|---|---|---|---|
| | **Mean** | **SD** | **Median** | **N** | **Mean** | **SD** | **Median** | **N** |
| *A. Demographic variables:* | | | | | | | | |
| Age | 62.56 | 4.41 | 63 | 660,744 | 52.95 | 4.74 | 53 | 784,638 |
| Male | 0.41 | 0.49 | 0 | 660,744 | 0.46 | 0.50 | 0 | 784,638 |
| Family Size | 1.20 | 0.46 | 1 | 660,744 | 1.47 | 0.74 | 1 | 784,638 |
| Less than High School | 0.32 | 0.47 | 0 | 660,744 | 0.24 | 0.42 | 0 | 784,638 |
| High School | 0.40 | 0.49 | 0 | 660,744 | 0.43 | 0.50 | 0 | 784,638 |
| College | 0.27 | 0.45 | 0 | 660,744 | 0.33 | 0.47 | 0 | 784,638 |
| Years of education | 11.80 | 3.20 | 12 | 660,744 | 12.51 | 2.90 | 12 | 784,638 |
| *B. Earnings, Assets, and Liabilities:* | | | | | | | | |
| Labor earnings | 39,887 | 48,270 | 27,465 | 660,744 | 66,948 | 59,275 | 68,763 | 784,638 |
| Financial wealth | 94,822 | 238,598 | 37,949 | 660,744 | 76,109 | 228,987 | 25,239 | 784,638 |
| Risky assets | 43,261 | 592,979 | 0 | 660,744 | 55,820 | 1,892,562 | 0 | 784,638 |
| Private equity | 25,334 | 532,900 | 0 | 660,744 | 39,145 | 1,787,815 | 0 | 784,638 |
| Housing wealth | 695,452 | 841,731 | 541,349 | 660,744 | 665,998 | 674,764 | 529,452 | 784,638 |
| Gross wealth | 815,607 | 1,128,589 | 624,260 | 660,744 | 781,253 | 2,079,403 | 595,758 | 784,638 |
| Debt | 78,379 | 143,892 | 35,217 | 660,744 | 119,679 | 359,511 | 75,657 | 784,638 |
| Net worth | 737,127 | 1,096,910 | 553,991 | 660,744 | 660,649 | 1,972,290 | 481,329 | 784,638 |
| *C. Returns on wealth:* | | | | | | | | |
| Financial wealth | 0.88 | 5.22 | 0.83 | 653,040 | 0.82 | 5.36 | 0.78 | 773,748 |
| Deposits | 0.66 | 1.30 | 0.58 | 608,267 | 0.55 | 1.32 | 0.50 | 712,659 |
| Risky assets | 3.23 | 22.53 | 5.26 | 239,406 | 3.29 | 21.46 | 3.95 | 297,481 |
| Listed shares | 4.26 | 24.63 | 7.25 | 236,646 | 4.42 | 23.69 | 7.24 | 293,450 |
| Stock funds | 4.46 | 22.83 | 8.40 | 209,712 | 4.73 | 22.34 | 7.27 | 265,587 |
| Housing | 3.37 | 10.96 | 1.36 | 596,663 | 3.30 | 10.79 | 1.28 | 699,891 |
| Private equity | 4.02 | 14.77 | 0.54 | 48,726 | 4.92 | 14.24 | 0.91 | 59,121 |
| Gross wealth | 3.16 | 9.83 | 1.48 | 652,299 | 3.20 | 9.85 | 1.46 | 774,134 |
| Debt | 2.16 | 2.16 | 2.13 | 514,461 | 2.30 | 1.95 | 2.19 | 638,641 |
| Long-term debt | 2.05 | 2.07 | 2.05 | 515,827 | 2.20 | 1.87 | 2.11 | 663,397 |
| Consumer debt | 8.34 | 8.10 | 6.86 | 112,807 | 8.34 | 7.90 | 6.88 | 173,324 |
| Net worth | 2.72 | 10.51 | 1.23 | 660,728 | 2.53 | 10.68 | 1.07 | 784,422 |

*Notes:* The table shows summary statistics for the estimation sample. This includes all Norwegian male and female cohorts born between 1943 and 1963 that were potentially exposed to the school reforms, that are single as of 2005 and remain as such over the entire 2005-2015 period (where we measure returns to wealth). Data refer to the balanced 2005-2015 panel. Panel *A* reports summary statistic on demographics; Panel *B* on earnings, assets, and liabilities; Panel *C* on returns on net worth and its components. "Treated" denote individuals that were affected by the reform; "Non-Treated" those who were not.

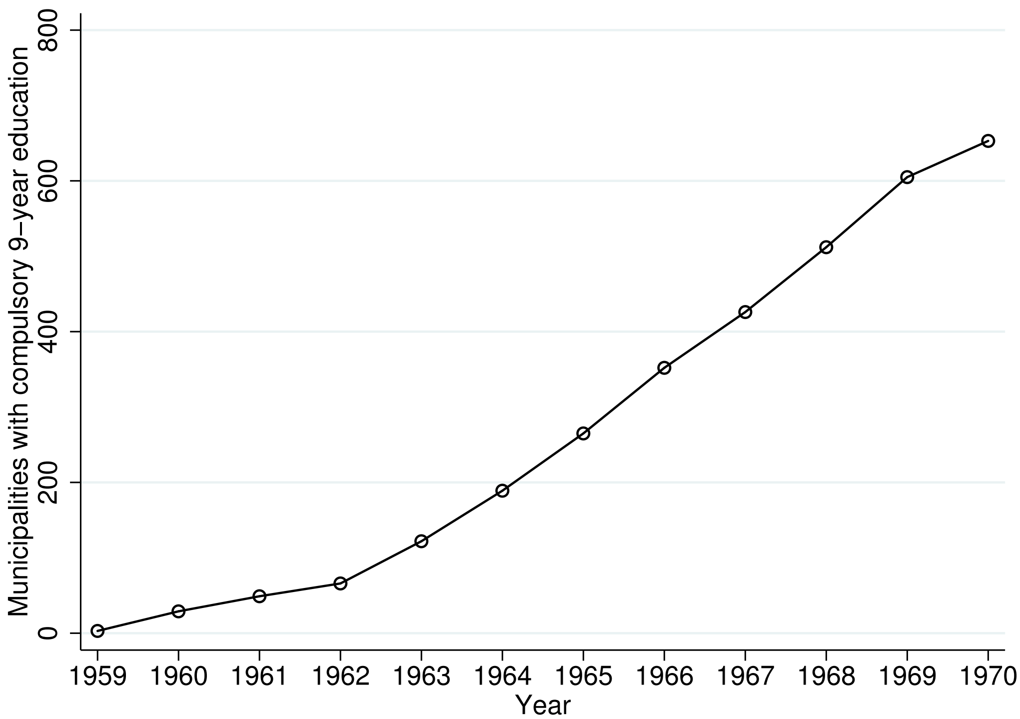
the same cohort of Norwegians were either affected or unaffected by the reform, depending on their municipality of residence at the time they were of school age.

The first cohort that *could* have been exposed to the reform was the one born in 1946. This cohort started school in 1953, and some members either (*i*) finished the pre-reform compulsory school in 1960 if they lived in a municipality that by 1959 had not adopted the reform; or (*ii*) went to primary school from 1953 to 1959 and then had to complete two extra years of schooling if they lived in an early-adopter municipality. The last cohort that could have gone through the old system was the cohort born in 1958, who started school in 1965 and finished compulsory school in 1972.[21] For comparability with previous work and to gain precision in the estimation of the municipality-fixed effects, we follow Bhuller, Mogstad and Salvanes (2017) and include individuals born between 1943 and 1963 in our estimation sample.

The implementation of the reform was financed by the government, based on a plan presented

[21] Although the 1958 cohort was the last cohort that in principle could have gone through the old system, there are still a few municipalities that implemented the reform later, as revealed by the share of non-treated individuals after 1958 in Table 2.

Figure 1: Number of municipalities with compulsory 9-year education by year




*Notes:* The figure shows the cumulative number of municipalities that implemented the compulsory 9-year education reform by year.

by the municipality. A committee set by the Ministry of Education was in charge of verifying the acceptability of the plan and proposing its approval. The reform affected the 732 municipalities existing in 1960. We are able to identify 655 of them from official administrative records. Figure 1 shows the cumulative number of municipalities that implemented the shift from 7 to 9-years of compulsory education for each year. By 1966 half of the municipalities had implemented the reform; and by the end of 1972, the entire country had made the switch.

We use the reform dates listed in Ness (1971). Other authors expand the set of municipalities, by either using additional data sources (e.g. Bhuller, Mogstad and Salvanes, 2017) or identifying the reform date from changes in the share of individuals in a municipality with less than 9 years of schooling (Brinch and Galloway, 2012). While we use the directly observed reform dates in our main specifications, we show in the Appendix B that the alternative approaches mostly agree on the treatment years (see Figure B.1). We also show that our main results are robust to alternative definitions of the reform instrument (Table B.1 in Appendix B).

### 5.2 Properties of the instrument

Table 2 shows the distribution of the number of individuals in our sample belonging to the various reform cohorts, distinguishing, among these, between those affected (treated) and those not affected (non-treated) by the reform. The number of treated individuals trends upward as we move towards the younger cohorts, while that of the non-treated shows the opposite pattern. Our identification will come from variation within a cohort between children living in municipalities

Table 2: Number of treated and non-treated individuals in each reform cohort

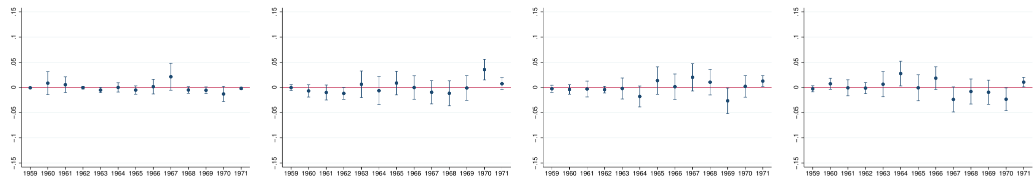
| | | Non-treated | | | Treated | | |
|---|---|---|---|---|---|---|---|
| **Year** | **Observ.** | **N** | **%** | **Years of education** | **N** | **%** | **Years of education** |
| 1943 | 5,666 | 5,666 | 100.00 | 11.24 | 0 | 0.00 | - |
| 1944 | 6,361 | 6,361 | 100.00 | 11.35 | 0 | 0.00 | - |
| 1945 | 6,761 | 6,761 | 100.00 | 11.43 | 0 | 0.00 | - |
| 1946 | 7,543 | 7,503 | 99,47 | 11.51 | 40 | 0.53 | 11.13 |
| 1947 | 7,182 | 6,762 | 94.15 | 11.61 | 420 | 5.64 | 11.86 |
| 1948 | 7,067 | 6,241 | 88.31 | 11.63 | 826 | 11.69 | 11.95 |
| 1949 | 6,745 | 5,818 | 86.26 | 11.73 | 927 | 13.74 | 12.11 |
| 1950 | 6,932 | 5,622 | 81.10 | 11.88 | 1,310 | 18.90 | 12.18 |
| 1951 | 6,680 | 4,820 | 72.16 | 11.95 | 1,860 | 27.84 | 12.36 |
| 1952 | 7,012 | 4,155 | 59.26 | 12.11 | 2,857 | 40.74 | 12.31 |
| 1953 | 7,406 | 3,472 | 46.88 | 12.17 | 3,934 | 53.12 | 12.37 |
| 1954 | 7,121 | 2,036 | 28.59 | 12.08 | 5,085 | 71.41 | 12.35 |
| 1955 | 7,155 | 1,214 | 16.97 | 12.20 | 5,941 | 83.03 | 12.48 |
| 1956 | 7,485 | 582 | 7.78 | 12.09 | 6,903 | 92.22 | 12.41 |
| 1957 | 7,381 | 113 | 1.53 | 11.44 | 7,268 | 98.47 | 12.43 |
| 1958 | 7,316 | 12 | 0.16 | 12.42 | 7,304 | 99.84 | 12.43 |
| 1959 | 7,153 | 15 | 0.22 | 11.06 | 7,137 | 99.78 | 12.24 |
| 1960 | 7,066 | 13 | 0.18 | 12.23 | 7,053 | 99.82 | 12.25 |
| 1961 | 7,151 | 14 | 0.20 | 12.21 | 7,137 | 99.80 | 12.35 |
| 1962 | 6,901 | 1 | 0.01 | - | 6,900 | 99.99 | 12.45 |
| 1963 | 7,153 | 0 | 0.00 | - | 7,153 | 100.00 | 12.46 |

*Notes:* The table shows the distribution of the number of individual in our sample belonging to each reform cohorts (identified by year of birth). "Treated" are individuals that were affected by the reform; "Non-Treated" those who were not.

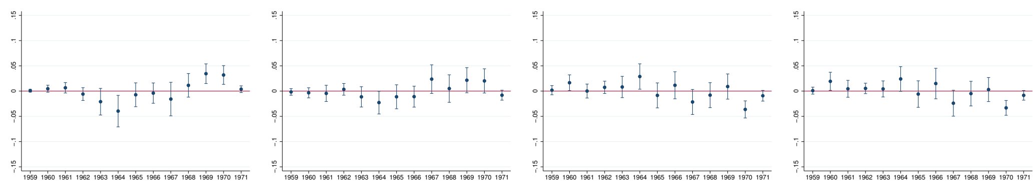
that had already adopted the reform at the time they finished their seventh grade, and those living in municipalities that had not yet complied with the new legislation.

Black, Devereux and Salvanes (2005) show that there is very little predictability in the timing of adoption of the reform on the basis of municipality characteristics; that is, the timing of the reform appears to be fairly random. We achieve the same conclusion in our sample as can be seen by the balancing plots shown in Figure 2, where we test whether a set of municipality characteristics at the time of the reform (population size, share of married residents, male-female ratio, labor force participation, share of registered church members, share of citizen with less than high school, high school and college) predict adoption time. For each characteristic, we run regressions where the dependent variable is a dummy equal to 1 if a municipality adopts the reform in a given year and zero otherwise, and the controls are the characteristics above (one at a time) interacted with a full set of year dummies covering the reform years. The balancing plots show the 95% confidence intervals of the coefficients of these interaction terms. With very few exceptions, there is little or no predictability in the timing of the reforms based on these observables. In addition to these variables, Black, Devereux and Salvanes (2005) show that there is no systematic relationship between the timing of implementation and the teenage birth rate, parents' average earnings, education levels, average age, urban/rural status, industry or labor force composition, municipality unemployment rates in 1960, and the share of individuals who were members of the Labour

Figure 2: Balancing plots



(a) Municipality size (b) Share married (c) Male-female ratio (d) Labor force



(e) Church members (f) < High school (g) High school (h) College

*Notes:* The figures show the standardized coefficients from a regression of a dummy that is 1 if the municipality implemented the reform in the year and zero otherwise on municipality characteristics for each year in the sample.

Party (the most pro-reform and largest political party at that time). To account for predictability of the timing of the reform by unobservables, we will control for municipality fixed effects in all regressions.

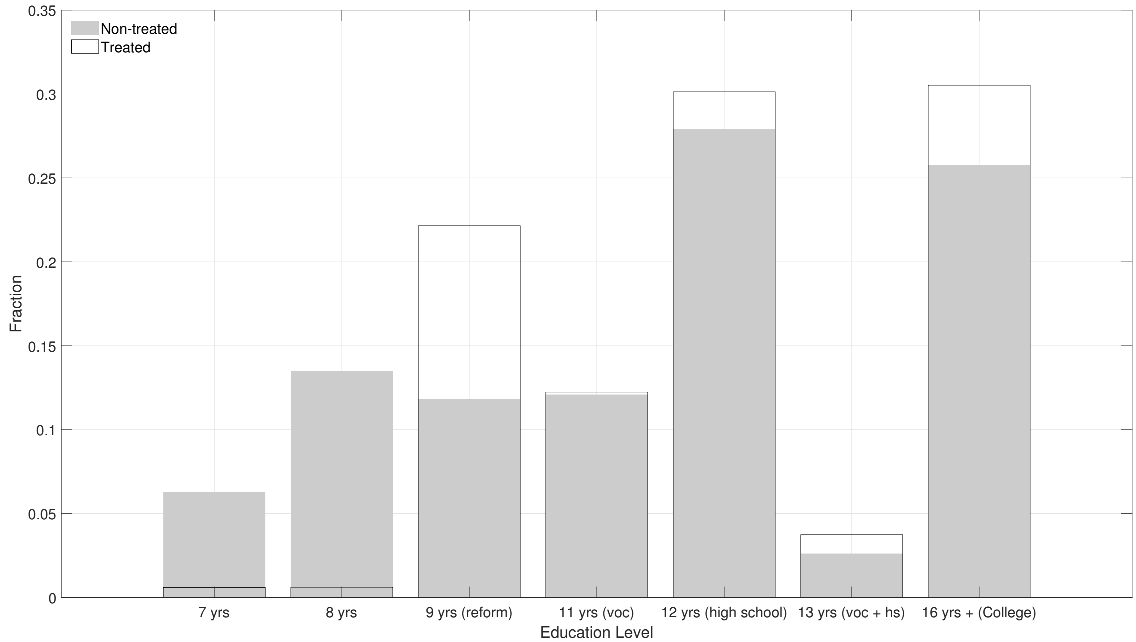
We complete this section by showing that the reform not only raised the years of compulsory school among those who otherwise would have stopped at 7 years of education without the new regime, but also shifted the whole distribution of educational attainment. Figure 3 compares the distribution of the years of schooling for the treated and non-treated cohorts, pooling all reform cohorts together. It shows that while there is a marked upward shift in the probability mass at 9 years of education among the treated, the whole distribution is shifted to the right. For instance, the share of individuals with 16 or more years of education is 25.8% among the non-treated cohorts and increases to 30.5% in the treated sample. This suggests that the reform encouraged those treated to undertake investments in education beyond what they would have done otherwise, perhaps because of changes in school quality or because children were pushed to make decisions at a more mature age.[22] Figure B.2 in the Appendix shows that this is true for all reform cohorts. Aakvik, Salvanes and Vaage (2010) provide evidence that the shift is causally induced by the reform.

To get a sense of the power of the instrument, Table 3 shows regressions of years of education on a treatment dummy equal to 1 if an individual belongs to a cohort affected by the reform. Regressions are reported separately for males, females, and for the pooled sample. All regressions include full controls for municipality and cohort dummies. In all estimates the treatment dummy is highly statistically significant (p-values < 0.1% in all samples). The treatment increases the average years of schooling by about 0.23 years in the pooled sample with a similar impact for males and females.[23] Overall, this provides *prima facie* evidence that the IV regressions that use

[22]If one considers the distribution of education across individuals who later hold risky assets, the pattern is similar: treated cohorts have higher education, mostly shifting individuals from 7 or 8 years to 9, 12, or 16 years. The main difference, however, is that holders of risky financial assets tend to have a higher level of education on average.

[23]This effect is similar to the one estimated by Bhuller, Mogstad and Salvanes (2017).

Figure 3: Distribution of years of schooling for treated and non-treated individuals




*Notes:* The figure shows the distribution of the number of years of education for the pooled "Treated" and "Non-Treated" cohorts reform. "Treated" are individual that were affected by the reform; "Non-Treated" those who were not.

the school reform as an instrument do not suffer from a weak instrument problem; moreover, there is gain in power when pooling the female and male samples.

### 5.3 The causal effect of education on *l*-returns

Before showing the estimates of the effect of education on *k*-returns (our main focus), we discuss OLS and IV estimates of the effect of education on *l*-returns. Panel A in Table 4 focuses on the cohorts born between 1943 and 1963, and reports the results of regressing log earnings on years of schooling and a set of controls for years, municipalities, and birth cohorts. We restrict the sample to working-age male and female adults (aged 25-62). In the OLS regressions, log earnings are positively correlated with education with an estimated return of 6.3% per each additional year of schooling when pooling males and females; *l*-returns are somewhat higher for women. When we run the IV regressions, the estimated return declines to around 3% per each additional year of schooling in the pooled sample (consistent with the presence of an omitted ability bias); *l*-returns for males are now slightly higher at 4.5% - close to the 5% return estimate cited by Aakvik, Salvanes and Vaage (2010, footnote 16).[24] As is typical with IV, the standard error of the estimate is larger, but the estimate is still highly significant. This suggests that the treatment is powerful enough to identify the causal effect of education on *l*-returns with high precision.[25] The *F*-statistics

[24]Our estimate is smaller than that by Aakvik, Salvanes and Vaage (2010) most likely because they restrict the sample to workers in the age bracket between 37 and 48 years of age in 1995, where returns to education tend to be higher than the average estimated over a wider age range. Bhuller, Mogstad and Salvanes (2017) illustrate this age-variation in effects of education on earnings.

[25]It is worth noting that in most papers in the literature IV estimates tend to be higher than OLS estimates. The interpretation given to this finding is that measurement error bias (which biases OLS estimates downward) is larger than the omitted ability bias (which biases OLS estimates upward). In our case, the measurement error bias is absent

Table 3: The effect of the reform on the number of years of schooling

| | *Years of education* | | |
|---|---|---|---|
| | **(1)** **Male** | **(2)** **Female** | **(3)** **Pooled** |
| Treatment | 0.231 | 0.231 | 0.228 |
| | (0.049) | (0.043) | (0.032) |
| Observations | 629,915 | 815,467 | 1,445,382 |

*Notes:* The table shows regressions of the effect of the reform treatment on the number of years of schooling in the sample of male and female reform cohorts and in the pooled sample. Treatment is a dummy=1 if the individuals was affected by the reform; zero otherwise. All regressions include cohort fixed effects and a full set of municipality dummies for where parents were located in 1960. Robust standard errors are clustered at the individual level and reported in brackets.

in Panel A provide direct evidence about power of the instruments.

To further check the robustness of our findings we consider an alternative empirical strategy, namely using twins data to eliminate the effect of unobserved fixed heterogeneity (e.g., ability). The advantage of this strategy is that we do not need to make any assumption regarding the validity of an instrument. As is common in the literature (see Ashenfelter and Krueger, 1994), the key identifying assumption is that the ability component is common within an identical twin pair. This strategy thus identifies the causal effect of education on earnings by exploiting variation in education within same-sex twin pairs. We identify identical twins by selecting individuals of the same gender who were born in the same month from the same mother.[26] Figure 4 shows the distribution of the difference in years of education within twin pairs. Around 40% of the twins have the same level of education but for the remaining 60% the number of years of education differs on a range between 1 and 8 years.

Panel B of Table 4 presents the results of using the twins sample. We first run an OLS regression, obtaining estimates that are similar to those obtained for the whole sample. Next, we consider a within-twin version of the Mincer regression which partials out genetic ability. Estimates of the effect of education on $l$-returns for the pooled twins sample are similar to the IV results commented above for the whole sample.[27]

Table 4: The effects of education on *l*-returns

| | Male | | Female | | Pooled | |
|---|---|---|---|---|---|---|
| *A. Full sample:* | | | | | | |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **IV** | **OLS** | **IV** | **OLS** | **IV** |
| Years of education | 0.052 | 0.043 | 0.074 | 0.021 | 0.063 | 0.028 |
| | (0.000) | (0.012) | (0.000) | (0.011) | (0.000) | (0.009) |
| First-stage *F*-test | | 150.36 | | 278.44 | | 398.76 |
| Observations | 22,104,057 | 22,104,057 | 21,763,966 | 21,763,966 | 41,356,521 | 41,356,521 |
| *B. Twins sample:* | | | | | | |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **Twins FE** | **OLS** | **Twins FE** | **OLS** | **Twins FE** |
| Years of education | 0.022 | 0.020 | 0.047 | 0.038 | 0.030 | 0.028 |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.001) | (0.002) |
| Observations | 300,234 | 300,234 | 237,416 | 237,416 | 537,650 | 537,650 |

*Notes:* The table shows regressions of log earnings on years of education for the cohorts born between 1943 and 1963. IV regressions use the reform treatment as an instrument for education. Treatment is a dummy=1 if the individual was affected by the reform; zero otherwise. All regressions in Panel A include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. All regressions in Panel B include cohort-time fixed effects. Robust standard errors are clustered at the individual level and reported in brackets.

Table 5: The effects of education on *k*-returns

| | Male | | Female | | Pooled | |
|---|---|---|---|---|---|---|
| *A. Full sample:* | | | | | | |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **IV** | **OLS** | **IV** | **OLS** | **IV** |
| Years of education | 0.177 | 0.040 | 0.152 | -0.016 | 0.163 | -0.021 |
| | (0.008) | (0.481) | (0.007) | (0.361) | (0.005) | (0.297) |
| First-stage *F*-test | | 22.13 | | 29.40 | | 49.96 |
| Observations | 629,915 | 629,915 | 815,467 | 815,467 | 1,445,382 | 1,445,382 |
| *B. Twins sample:* | | | | | | |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **Twins FE** | **OLS** | **Twins FE** | **OLS** | **Twins FE** |
| Years of education | 0.161 | 0.068 | 0.165 | 0.019 | 0.147 | 0.035 |
| | (0.096) | (0.201) | (0.090) | (0.141) | (0.067) | (0.115) |
| Observations | 1,878 | 1,878 | 2,790 | 2,790 | 4,668 | 4,668 |

*Notes: Panel A* shows OLS and IV regression coefficients. The dependent variable is the (scale adjusted) returns to net worth. The sample is single individuals belonging to the cohorts born between 1943 and 1963. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects and a full set of municipality dummies for where parents were located in 1960. In the IV regressions the instrument for years of education is a dummy =1 if the individual was affected by the school reform. In *Panel B* the sample is twins belonging to the cohorts born between 1943 and 1963. Regressions are shown for male and female twins and for the pooled sample. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects. In all regressions robust standard errors are clustered at the individual level and reported in brackets.

# 6 *K*-returns to Education

## 6.1 OLS estimates

To estimate *k*-returns to education we again focus on the sample of Norwegians born between 1943 and 1963. These individuals were aged between 42 and 62 in 2005, the first year for which we can obtain accurate estimates of returns to net worth. One important issue is that married individuals report wealth and capital income jointly, and hence we observe returns on *household* net worth. Identification of the effect of education of the two spouses on returns to household wealth is complex. This is because the relation between education and ability and households returns depends on how the decisions about the management of household wealth are shared between the two spouses. A lack of association between education of one spouse and the household return could reflect either a genuine lack of a causal effect or the fact that that spouse's characteristics have no influence on the management of household wealth. For these reasons, we focus on the population of individuals born in the 1943-1963 period who were not married as of 2005. To make sure that we have enough data to estimate differences in average returns to wealth we focus on the balanced panel of single individuals who are observed in all years between 2005 and 2015. Obviously, this selection implies that estimates of the relation between education and *k*-returns, while valid for our sample, may not necessarily extend to the whole population. Summary statistics on this sample are reported in Table 1.

Panel A of Table 5 shows the results of the OLS estimates of the effect of years of education on *k*-returns to net worth (columns (1), (3) and (5), respectively for men, women, and the pooled sample). In all estimates, the left hand side is $r^w_{it} - \tilde{f}^w_{i,t-1}$ - the return to net worth net of the scale effect. To allow for a flexible functional form in the scale effect we insert the first differences of a full set of initial wealth percentile dummies and then retrieve the estimated vector of parameters to correct for scale. All regressions include a set of time dummies to account for aggregate variation in returns and a full set of municipality dummies where the parents of the individual were located at the time of the reform in 1960 to capture any local feature that may affect returns. They also include a full set of cohort fixed effects which capture the trend in schooling in Norway. We run estimates separately on the sample of about 630,000 observations on males and 815,000 observations on female individuals to allow for gender-specific effects of education on *k*-returns; we also report results for the pooled sample.[28] The OLS regressions show a very precisely estimated positive association between education and returns to net worth. The association is also sizable:

due to the administrative nature of the data on school attainment.

[26]Of course, this selection procedure will also include fraternal twins of the same gender. By the Bayes' theorem, the probability that twins of the same gender are identical is 50% - assuming that the fraction of twins that are of the identical type is about 1/3 (as it appears to be the case in most countries).

[27]If ability is differenced out only for identical twins but not for fraternal twins (at least not entirely), the within-twin regressions will still be biased. However, under some realistic assumptions the bias of the within-twin estimate is smaller than the bias of the OLS estimate, implying that the former is a more credible upper bound to the true effect of schooling on earnings.

[28]Given that never-married people are more likely to be male (55%), the fact that we observe more single females than single males may look surprising at first. However, for two other categories of singles (widows/widowers and divorced), the fraction of females is much higher (81% and 61%, respectively). Given the age span of our sample, the latter effect dominates.

16.3 basis points for each additional year of education. It is larger among males (17.7 basis points) although the gender gap is only 2.5 basis points. Using the estimate for the pooled sample, an individual with a four-year college degree would earn on average a 65 basis points higher return on net worth than a similar individual with just a high school degree. If one were to capitalize this return difference over the entire working life cycle (say from age 25 to age 65), individuals with a college degree would arrive at retirement (*ceteris paribus*) with roughly 29% higher net worth than an otherwise equal individual with a high school diploma (and 56% higher net worth than someone with the compulsory (post reform) level of schooling).[29]

The association between education and wealth returns extends to the broad components of net worth - gross assets and liabilities - as well as their sub-components (real and financial wealth, and mortgage and consumer debt, respectively). Estimates are shown in the Appendix Tables B.3 and B.4. OLS regressions show that education correlates positively with returns on gross assets, real assets and financial assets, particularly in the pooled sample. It correlates negatively with the interest rate paid on total debt and with the cost of mortgage and consumer loans. The marginal effect of an extra year of education is particularly large for consumer loans. Hence, the correlation between education and net worth reflects both higher returns on assets among individuals with higher education as well as a lower cost of debt.

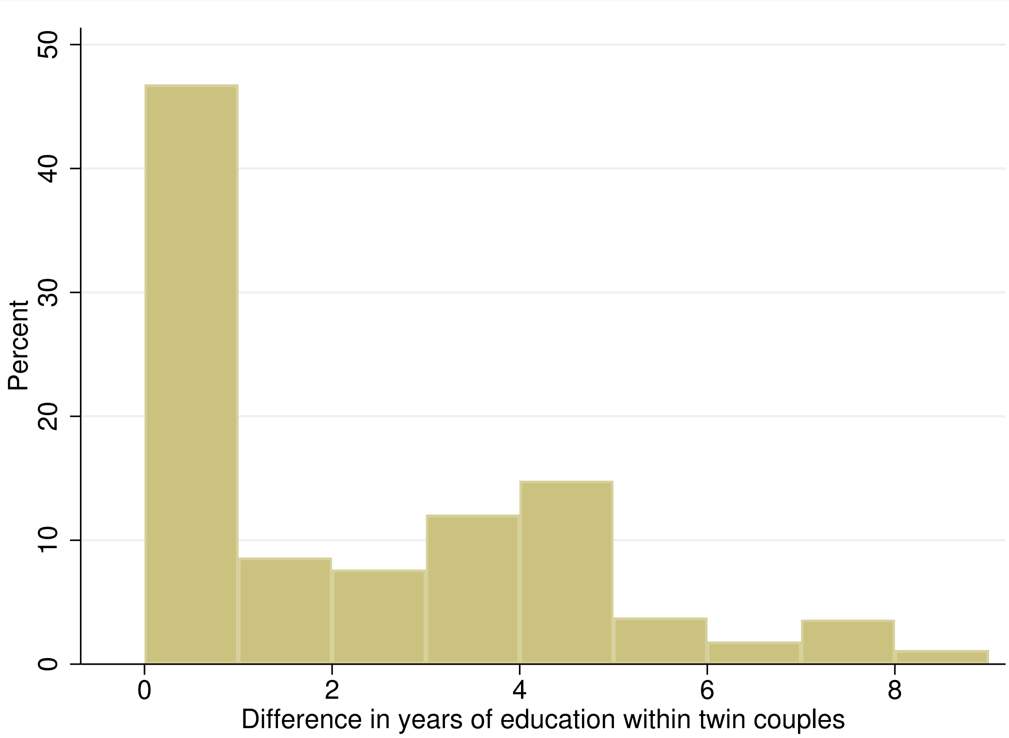
### 6.2 IV estimates

In this section we discuss instrumental variable estimates of $k$-returns to education using the differential exposure of various birth cohorts to the 1960s reform as a source of exogenous variation in education. Columns (2), (4) and (6) in Panel A of Table 5 report the IV estimates for the returns to net worth for males, females, and the pooled sample. In all cases the estimated coefficients are lower than the OLS estimates, dropping to values closer to zero, and statistically insignificant, suggesting that education has no causal effect on returns to net worth.[30] We can rule out that absence of a significant effect of education is just due to lack of power of the instrument that results in high standard errors. First, the discussion in Section 5 suggests that the instrument does indeed shift the distribution of the number of years of education. Statistically, the $F$-statistic on the excluded instrument in the first stage regression (22.1 in the males sample, 29.4 in the females sample and 50.0 in the pooled sample) implies that the estimates do not suffer from a weak instrument problem, particularly for the pooled sample. Second, while the reform raised compulsory schooling from 7

[29]The Appendix Table B.2 shows results when years of schooling are replaced with a set of educational attainment dummies, the excluded group being those with less than 8 years of schooling. The estimates show that returns to net worth are monotonically increasing with educational attainment and the correlation is strong: compared to someone with less than 8 years of schooling, an individual with post-college schooling (21 years of education) earns on average 245 basis points higher annual return on net worth and the move from after reform compulsory school (9 years) to a high school diploma (12 years) is associated with a 66 basis points higher return on net worth.

[30]One potential issue with the IV estimates is that because we use a sample of singles, the instrument may be invalid due to selection if: a) exposure to the reform affected the marriage market opportunities of the relevant cohorts and b) marital status matters for performance in the labor and capital market. If both are true than the instrument would fail to be orthogonal to the error term in equation (1). If we run a regression for the probability of being single among the cohorts affected by the reform we find no evidence that the treatment predicts singlehood (Table (B.5)), reassuring about its validity. One reason why this is so is that the reform had exactly the same effect on the education of males and females (Table 3), suggesting that shocks to human capital left marriage patterns unaffected (Low, 2024).

Figure 4: Differences in years of education within twin couples




*Notes:* The figure shows the sample distribution of the differences in years of education within the twin couples in our sample.

to 9 years, it resulted in a shift of the entire distribution of educational attainment, implying that the treatment had not only a local effect (raising the education of those that would have stopped after seven years of schooling in the absence of the reform) but affected also subsequent education decision (for example, because people made decisions about whether to accumulate further human capital at a more mature age). Finally, as discussed in Section 5, the treatment does affect $l$-returns implying that it is not the instrument that fails to predict returns but rather the *nature* of the return that makes the difference: formal, general education has a causal effect on $l$-returns but it appears to have no significant causal effect on $k$-returns.

Appendix Table 5 shows that this finding holds when we look at returns on total assets and its components, real and financial assets respectively, as well as for the interest rate paid on total debt and on its two components (mortgages and consumer loans, see Appendix Table B.4). Contrary to the OLS estimates (which produce statistically significant positive effects on returns to assets and negative effects on the cost of debt), the IV estimates imply at face value no effect of education on asset returns: the point estimates are in both cases negative and statistically insignificant.[31]

Our results are also robust to changing the definition of the instrument and either using that of Bhuller, Mogstad and Salvanes, 2017 or that of Brinch and Galloway (2012), see Table B.1 in Appendix B.1.

[31] The same conclusion holds if instead of IV regressions we run reduced form regressions of returns on the treatment dummy. While the latter predicts $l$-returns, as documented in Table 4, Panel A, it has no effect on $k$-returns to net worth and all its components. See the Appendix, Tables B.6, B.7, and B.8.

## 6.3 Within-twin estimates

To further check the robustness of our findings, we replicate what done above for the estimation of $l$-returns and use a sample of twins where the bias induced by unobserved fixed heterogeneity can be eliminated without the need of relying on an instrumental variable procedure. As before, this strategy exploits variation in education within same-sex twin pairs to identify the causal effect of education.[32] Given the data restrictions, estimation of $k$-returns is based on a smaller sample of twins than estimation of $l$-returns. In particular, we are able to identify 290 twin pairs in our baseline sample (with both individuals unmarried and present in all years between 2005 and 2015). Panel B of Table 5 shows results for male and female twins and for the pooled sample of twins, first for OLS regressions of returns to net worth, and then adding twin fixed effects to separate the effect of education from that of unobserved ability. OLS estimates are similar to those in the whole sample in Panel A, that is, they show a positive and similarly sized effect of years of education on returns to net worth. Not surprisingly, the twins sample OLS estimates are less precise given the smaller sample size and for males are not statistically significant. But in the pooled sample the association between education and returns to net worth is precisely estimated. When twin fixed effects are added, the effect of education shrinks in size (from 0.147 to 0.035 in the pooled sample) and loses its statistical significance.[33]

Recall that when this strategy is used instead to identify the causal effect of education on $l$-returns, results are in line with those using the variation induced by the reform in Table 4, Panel B. In the twins sample, OLS estimates of the effect of education on earnings are positive and highly statistically significant as in the whole sample; and within-twins estimate are comparable in both magnitude and statistical significance to IV estimates. In sum, the twins strategy further suggests that while education appears to exert a positive causal effect on $l$-returns, it has no discernible effect on returns to wealth.

One important remark is about power: $k$-return estimates that correct for potential unobserved heterogeneity (IV estimates and within-twins estimates) display substantially larger standard errors than the corresponding OLS estimates, implying that (in principle) one could not reject the null that they are not different from the OLS estimates themselves. However, economic significance is equally, if not more important. The finding that is common across all the experiments we run (for the return to net worth and for the return to all other assets and liabilities; for IV estimates as well as within-twin estimates) is that estimates uniformly and substantially decline in magnitude, in some cases becoming close to zero. Holistically, this appears to support our interpretation that the economic effect of general education on $k$-returns is small or absent, as such uniformity would be hard to reconcile with pure statistical power issues.

[32] Sandewall, Cesarini and Johannesson (2014) argue that the within-pair variation in education we use here may be directly related to ability. In our context, it means that the estimated effects of education on returns from within-pair variation may be upward biased. Since we find that there is no effect of education on returns in the within-pair specification, the presence of a bias would only buttress our main findings.

[33] Interestingly, using a panel of Swedish twins and Swedish financial data from administrative records, Calvet and Sodini (2014) find that education is not significantly correlated with risky asset market participation and the risky share of financial assets once they control for the stock of wealth and yearly twin fixed effects. This implies that education is unlikely to *cause* higher returns to financial wealth by inducing investors to participate more intensively in the stock market through a channel that is not the scale of wealth.

### 6.4 Age restrictions and samples alignment

In this section we address two potential concerns about our findings. The first is that the effects of increased education on returns to wealth are measured significantly later in life (median age in the treated sample is 52 and in the non-treated sample, 62). One may argue that even if formal education initially improves financial knowledge and returns on capital, it is unclear how long-lasting these effects might be. As Lusardi, Michaud and Mitchell (2017) argue, financial knowledge depreciates over time due to decay and obsolescence as new financial products emerge. To keep the stock of financial knowledge up to date and possibly increase it, new investment is required. It is reasonable that the investment in financial knowledge is increasing in formal education (e.g., because it is less costly for the high educated to do so). If this is the case, the "advantage" in financial knowledge due to higher general education need not dissipate over time, but it may actually even increase as people age.[34] Guiso and Jappelli (2020) provide empirical support for this conjecture. They show that investment in financial information is strongly increasing in general, formal education. Using survey data on the time people devote to gathering financial information to guide portfolio decisions, they find that education strongly predicts investment in financial information, even after controlling for wealth and income – a finding consistent with education reducing the costs of collecting and processing such information. In turn, they find that individuals who invest more in information collection earn higher average and risk-adjusted portfolio returns. Our findings generalize these correlations to the entire portfolio and—importantly—show that it is not education *per se*, but the underlying individual ability for which it serves as a proxy that causes more educated and better-informed individuals to earn higher returns.

The second concern is that we estimate $l$-returns and $k$-returns on samples that are not fully aligned along the age scale, which may raise doubts about our finding of different effects of education on the two types of returns. We first stress that, for both $k$-estimates and $l$-estimates, the initial sampling is identical: people born between 1943 and 1963, i.e., individuals who are either directly affected by the schooling reform or in adjacent cohorts (and hence constitute a more reliable control group). However, data constraints prevent us from completely aligning the analyses for $l$-returns and $k$-returns: wealth returns are available – in their full form - only after 2005. This means we can only study the portfolio decisions of those exposed to the reforms when they are aged 42-62. As already discussed above and in Section 5, this timing is not problematic for studying $k$-returns. On the other hand, studying labor market returns on a sample of workers in this same age range may seriously bias estimates of $l$-returns. The labor market literature identifies several reasons why excluding young workers from a study that aims to estimate returns to education may create biases. First, individuals who invest more in human capital have low initial earnings but faster earnings growth than people who don't invest in human capital.[35] If one runs a regression of earnings on education using only the later part of the life cycle, the estimate will be

[34] As Lusardi, Michaud and Mitchell (2017) write, "Obtaining knowledge in the form of investment has a cost [. . . ]; we assume that this cost function is convex, reflecting decreasing returns in the production of knowledge. Relatively little is known about how this cost might vary across individuals; for instance, it could either rise or fall depending on the level of education. Clearly the opportunity cost of time is higher for higher earners, but education might be a complement in the production of knowledge, making it easier for the better educated to learn."

[35] See Card (1999).

Table 6: Education and returns on deposits: OLS and IV

| | *Returns to deposits* | | | | | |
|---|---|---|---|---|---|---|
| | **Male** | | **Female** | | **Pooled** | |
| | **(1) OLS** | **(2) IV** | **(3) OLS** | **(4) IV** | **(5) OLS** | **(6) IV** |
| Years of education | 0.043 | 0.067 | 0.029 | -0.040 | 0.035 | 0.002 |
| | (0.001) | (0.056) | (0.001) | (0.047) | (0.001) | (0.036) |
| First-stage *F*-test | | 23.70 | | 30.37 | | 53.27 |
| Observations | 539,361 | 539,361 | 742,874 | 742,874 | 1,282,235 | 1,282,235 |

*Notes:* The table shows OLS and IV regressions of returns bank deposits on years of education for the of individuals belonging to the cohorts born between 1943 and 1963. Regressions are shown for single males and female and for and pooled sample. Regressions are run on the balanced panel covering the years 2005-2015 with deposits lower than the threshold for the deposit insurance scheme. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. In the IV regression the instrument for years of education is a dummy = 1 if the individual was affected by the school reform. Robust standard errors are clustered at the individual level and reported in brackets.

biased upward because it ignores the fact that in the early years of the life cycle the high educated have lower earnings while they are investing in human capital.

Second, there are important selection issues by age. Labor market participation rates decline substantially after age 50 to 55 and this decline is stronger for people with lower levels of schooling and lower level of skills. Once more, focusing on older workers in the presence of these selection effects by age will produce a larger premium for schooling than one would obtain in a more representative sample. Indeed, there are much smaller differences in participation rates between younger workers of different education levels. Third, omitting the early phase of the life cycle may create biases pushing in the opposite direction, since more educated workers experience strong spurs of earnings growth through movements across firms, career progression, etc., which are events that are typically more frequently observed when workers are relatively young.

To assess the robustness of our results to age misalignment we re-estimate the $l$-returns on a sample that has an age range closer to the one used to estimate $k$-returns. In keeping with the concerns above, however, we avoid trimming the early-age portion of the sample excessively. Specifically, we run estimates for three different age samples: 25-62 (the original sample), 30-62, and 30-52 (where 52 is the midpoint of the 42-62 age range we use for estimating $k$-returns. The results, shown in Table B.9 in the Appendix B.2 , are essentially unchanged, qualitatively and quantitatively.

# 7 Interpretation

The evidence above leaves us with a key question: why does education predict $k$-returns to wealth in OLS regressions while the correlation is absent when we control for unobserved heterogeneity? Our results imply that $k$-returns are fundamentally affected by preferences for risk and/or wealth-management ability (unobserved heterogeneity in returns), while formal education - differently from what happens for labor market returns - does not pay off in capital markets. Additionally,

Table 7: The effects of education on $k$-returns, OLS and IV, volatility-adjusted

| | *Returns to net worth* | | | | | |
|---|---|---|---|---|---|---|
| | **Male** | | **Female** | | **Pooled** | |
| | **(1)** **OLS** | **(2)** **IV** | **(3)** **OLS** | **(4)** **IV** | **(5)** **OLS** | **(6)** **IV** |
| Years of education | 0.157 | 0.301 | 0.115 | 0.026 | 0.134 | 0.124 |
| | (0.008) | (0.452) | (0.006) | (0.338) | (0.005) | (0.277) |
| First-stage $F$-test | | 22.54 | | 29.64 | | 50.78 |
| Observations | 629,914 | 629,914 | 815,467 | 815,467 | 1,445,381 | 1,445,381 |

*Notes:* The table shows OLS and IV regressions of returns to net worth on years of education for the male, female and pooled sample of single individuals belonging to the cohorts born between 1943 and 1963. The regressions include a control for volatility in individual returns to gross wealth. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. The instrument for years of education is a dummy =1 if the individual was affected by the school reform. Robust standard errors are clustered at the individual level and reported in brackets.

in order to explain why education predicts $k$-returns in OLS estimates, risk tolerance and/or ability to navigate capital markets must be positively associated with educational attainment. Thus, the key question is whether unobserved heterogeneity in $k$-returns reflects risk tolerance, wealth-management ability, or a combination of the two.

To test whether it is *only* preferences for risk that can rationalize the results we follow two strategies. First, we focus on returns to deposits. Because deposits up to 2 million NOK (approximately $260,000) are fully insured by the government through the Banks' Guarantee Fund, they bear no risk. Hence, heterogeneity in returns on fully insured deposit accounts cannot reflect unobserved risk tolerance. It follows that if one finds a positive correlation between education and returns on fully insured deposits in OLS regressions it cannot be due to uncontrolled-for individual risk tolerance. Results in Table 6 show that in the OLS regressions, education has a positive and significant relationship with returns on fully insured deposits in all samples (males, females, and pooled) although the marginal effect is small (3.5 basis points for each additional year of schooling in the pooled sample). However, the IV estimates result in a smaller effect of education. The effect is actually negative in the female sample and positive in the male and pooled samples, but not statistically significant in all cases. Because deposits are risk-free, these results suggest that unobserved heterogeneity in risk tolerance does not appear to be the main driver behind our baseline findings.

Secondly, we run OLS and IV regressions of returns to net worth on years of education controlling for individual volatility in returns to capture differences in risk tolerance across investors. For each individual, we measure volatility with the variance of individual returns on net worth over the 2005-2015 sample period. Results in Table 7 show that in OLS regressions the marginal effect of education is only slightly reduced when controlling for return volatility (0.134 basis points instead on 0.163 for each year of education in the pooled sample). This is consistent with education being only mildly correlated with appetite for risk, suggesting that compensation for risk taking is not the only reason for the positive correlation between returns and education. However, in the

IV estimates the hypothesis that education has no causal effect on returns is never rejected despite explicit controls for portfolio risk.

While this evidence does not rule out that education *also* captures heterogeneity in risk tolerance when we look at returns to net worth, it does imply that education captures individuals' specific ability to manage their own wealth. In turn, this ability must encourage investment in education, which, *per se*, does not enhance $k$-returns. Put differently, while one can acquire at school a set of skills that have a payoff in the labor market, school-acquired skills do not seem to make an individual better at managing her savings. What matters for the latter appears to be mostly heterogeneity in ability.

This interpretation is consistent with recent research by Barth, Papageorge and Thom (2020), who find that genetic endowment - a measure of ability/preferences - strongly predicts wealth at retirement besides predicting education attainment. They show evidence that genetic endowment affects wealth accumulation beyond the effect that it has through education and labor income. Rather, as they argue, it acts through a variety of additional channels including "a facility with complex financial decision making". Our evidence is consistent with Barth, Papageorge and Thom (2020), but compared to them we move one step forward in unveiling the mechanism that links ability to wealth. Ability simultaneously causes education and returns to capital and labor. However, while education contributes to wealth accumulation because it affects $l$-returns, according to our estimates its contribution through $k$-returns is absent: returns to wealth are by and large affected by pre-education ability.

Our evidence is also consistent with Black et al. (2018), who study the causal effect of education on stock market participation using Swedish data on a school reform analogous to the Norwegian one. They find a causal effect of education on participation and on the risky financial portfolio share but only in regressions that do not control for the *scale* of wealth. Once the latter is controlled for, the effect vanishes implying that education has a causal effect on stock investment only because it affects $l$-returns and thus the stock of savings. However, eliminating this channel would leave the financial portfolio - and so $k$-returns - unaffected.

To further corroborate this interpretation, we follow Fagereng et al. (2020) and run OLS regressions of returns to net worth (filtered to account for wealth-scale effects) on our sample. We run three sequential specifications: first controlling for education, demographics and the other controls used in Table 5, then adding a rich set of controls for the composition of individual net worth interacted with time dummies in order to capture differences in returns reflecting compensation for risk exposure to individuals with greater risk tolerance.[36] Finally, we consider a third specification that includes a set of individual fixed effects. The latter capture all fixed cross-sectional variation included in the previous specifications (in particular, the effect of education and the persistent component of the wealth allocation) plus unobserved sources of heterogeneity, including

[36] We include the shares of mutual funds, directly held stocks, bonds, foreign wealth shares, outstanding claims, private business wealth and housing all as shares of gross assets; on the liability side, we control for the share of mortgage debt, student loans and consumption loans again scaled by gross assets. All these shares are interacted with time dummies to capture differential responses to aggregate risk. To further control for compensation for risk exposure, following Fagereng et al. (2020) who also include controls for the average individual $\beta$ of the stock portfolio, private business wealth and housing wealth, again interacted with time dummies. See Fagereng et al. (2020) for a full description of these variables.

Table 8: Education on returns to net worth: OLS and Fixed Effects.

| | (1) | (2) | (3) |
|---|---|---|---|
| Years of education | 0.085 | 0.096 | |
| | (0.005) | (0.005) | |
| Male | 0.049 | 0.153 | |
| | (0.035) | (0.034) | |
| Demographics | Y | Y | Y |
| Year effects | Y | Y | Y |
| Shares x year effects | N | Y | Y |
| Fixed effects | N | N | Y |
| Adjusted R-squared | 0.098 | 0.104 | 0.248 |
| Observations | 1,154,825 | 1,154,825 | 1,154,825 |

*Notes:* The table shows OLS (first and second column) and fixed effects (third column) regressions of scale adjusted returns to net worth on education and detailed controls for demographics (sex, 4th order polynomial in age, cohort, municipality of mother), year, and risk exposure (portfolio composition and portfolios $\beta's$). Robust standard errors are clustered at the individual level and reported in brackets.

individual ability and risk tolerance.

Our main interest is in the change in the fit of the regression as measured by the adjusted $R^2$ as we move from the first to the second specification, and from this to the third. The change in the adjusted $R^2$ moving from the first to the second specification speaks about the contribution to returns to wealth due to compensation for risk; the change from the second to the third reveals the additional explanatory power of unobserved heterogeneity due to ability to process and use financial information or other persistent individual traits (such as inter-temporal discounting) that may be relevant for investment decisions. These features affect the average return that individuals extract from their net worth *conditioning* on risk exposure and the scale of their portfolio.

Table 8 shows the results of these regressions run on the pooled male and females sample (results are similar for the two sub-samples). In column (1) the effect of education on wealth returns is 0.085, which is smaller than what we found in Table 5 because we are using a much richer set of controls. This regression explains slightly less than 10% of the variation in returns. In moving from the first to the second specification (where we add controls for risk compensation) the adjusted $R^2$ of the OLS estimates increases from 0.098 to 0.104. This suggests that some part of the observable heterogeneity in returns to net worth reflects compensation for risk, consistent with the indirect tests above. At the same time the marginal effect of education increases to 0.096 implying that risk exposure has limited importance for our results. The last column of Table 8 adds the individual fixed effects. Obviously, the effect of time-invariant characteristics (including education) is absorbed by the fixed effects. The important result is that the individual fixed effects improve the fit further and considerably: compared to column (2), the adjusted $R^2$ of the regression increases from 0.10 to 0.25. Since risk exposure and education were already accounted for in column 2, the increase in explanatory power is all due a persistent unobserved individual component consistent with the ability interpretation of the IV estimates in Table 5.

A final possibility is that it is "specific" rather than "general" education that matters for $k$-returns. Testing for this poses several challenges. The first is that it is not obvious how to measure "specific" education. Measures of financial literacy of the type used by Lusardi, Michaud and

Table 9: The effects of education on *k*-returns, twins sample

| | Returns to net worth | | | |
|---|---|---|---|---|
| | **(1)** **OLS** | **(2)** **Twins FE** | **(3)** **OLS** | **(4)** **Twins FE** |
| Years of education | 0.147 | 0.035 | 0.125 | 0.033 |
| | (0.067) | (0.115) | (0.066) | (0.115) |
| Econ./Bus. education | | | 0.757 | 0.850 |
| | | | (0.444) | (0.396) |
| Observations | 4,668 | 4,668 | 4,668 | 4,668 |

*Notes:* The table shows regressions of (scale adjusted) returns to net worth on years of education and a dummy for economics/business degree for the sample of twins belonging to the cohorts born between 1943 and 1963. Regressions are shown only for pooled male/female sample. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects. Robust standard errors are clustered at the individual level and reported in brackets.

Mitchell, 2017 are unavailable in administrative data of the type we are using. Second, specific measures of human capital are also potentially correlated with unobserved heterogeneity, and finding valid instruments is difficult. One final set of regressions we propose involve the use of a dummy for whether the individual completed an economics/business degree in college or high school ("mercantile high school"). We interpret this variable as a rough proxy for financial knowledge, i.e., for "specific" human capital. To avoid endogeneity issues, and since we do not have a convincing instrument for type of school degree, we consider the within-twins specification. The results are reported in Table 9 for the pooled sample only; columns 1-2 replicate the regressions of Table 5 (Panel B) for comparison, while columns 3-4 add the economics/business education dummy. Two interesting findings emerge from these regressions. First, adding the "specific" human capital proxy does not affect the basic result neither qualitatively nor quantitatively: "general" human capital becomes unimportant once we control for unobserved fixed heterogeneity, implying that the omission of "specific" human capital is not responsible for our main finding. Second, the "specific" human capital proxy is marginally significant and economically sizable (implying that people with economics/business education have on average 85 basis point higher return to net worth than those who do not). These findings, while based on a small sample, suggest that "specific" human capital may play an important role in explaining heterogeneity in returns to wealth in addition to non-learned ability, consistent with recent research using university admission cutoffs showing that university-level business and economics education improves portfolio performance (Altmejd, Jansson and Karabulut, 2024).

## 8 Conclusions

In this paper, we study whether formal general education generates returns in capital markets comparable to those observed in labor markets. Using Norway's compulsory schooling reform to obtain exogenous variation in years of schooling, we find that while education predicts returns to wealth in OLS regressions, this relationship disappears once we account for unobserved heterogeneity using either an instrumental variables strategy or a twins research design. Various indirect tests we conduct suggest that the observed correlation between education and wealth returns operates entirely through confounding factors—specifically, risk tolerance and ability appear to be the primary drivers of heterogeneity in individual returns to capital. This contrasts with the evidence for labor earnings, where general education has a statistically significant causal effect.

This asymmetry raises an important question: why does general education matter for labor earnings but not for capital returns? We consider two possible explanations. The first is that general education matters for labor earnings because it is a signal for unobserved productivity. Signaling may matter in the labor market due to labor demand considerations.[37] However, signaling is irrelevant for returns on self-managed wealth, which may partially explain our findings. The second possibility is that while the skills relevant for labor market success may be effectively acquired through general education, investing skills may be fundamentally different in nature—less amenable to acquisition through general education and more dependent on specialized financial training or learning-by-doing. Understanding these issues is critical for the debate on the benefits of financial education and more generally for assessing whether formal education is an effective policy to contain wealth inequality. We offer some suggestive evidence that specific education in finance or business may improve people's financial decisions. However, pinning down the effect of specific education requires exogenous variation in the field of study (see the discussion in Section 7) and represents an important avenue for further research.

[37]Empirical tests of the signaling hypothesis yield mixed findings. Bedard (2001) notes that in the signaling model, an environment in which some individuals are constrained from attending college will be characterized by increased "bunching" at the high school graduation level, relative to an environment with greater university access in which high-ability students can more easily separate from low-ability students. She finds evidence supporting the signaling hypothesis. Clark and Martorell (2014) use a regression discontinuity design to test for a signaling effect of education, by comparing wages of individuals just below and just above the grade to obtain a high school diploma. They find no evidence of a signaling effect. However, this may be because firms observe not only the diploma but also the passing grade and can thus infer that an individual just above the threshold is no different, in terms of ability, from an individual just below. Put differently, their test rests on strong assumptions about what information firms observe.

## References


Aakvik, Arild, Kjell G Salvanes and Kjell Vaage. 2010. "Measuring heterogeneity in the returns to education using an education reform." *European Economic Review* 54(4):483–500.

Altmejd, Adam, Thomas Jansson and Yigitcan Karabulut. 2024. "Business education and portfolio returns." *Working paper* .

Aoki, Shuhei and Makoto Nirei. 2017. "Zipf's Law, Pareto's Law, and the Evolution of Top Incomes in the United States." *American Economic Journal: Macroeconomics* 9(3):36–71.

Arrow, Kenneth J. 1987. "The demand for information and the distribution of income." *Probability in the Engineering and Informational Sciences* 1(1):3–13.

Ashenfelter, Orley and Alan Krueger. 1994. "Estimates of the Economic Return to Schooling from a New Sample of Twins." *American Economic Review* 84(5):1157–1173.

Bach, Laurent, Laurent E. Calvet and Paolo Sodini. 2020. "Rich Pickings? Risk, Return, and Skill in Household Wealth." *American Economic Review* 110(9):2703–47.

Barth, Daniel, Nicholas W Papageorge and Kevin Thom. 2020. "Genetic endowments and wealth inequality." *Journal of Political Economy* 128(4):1474–1522.

Becker, Gary S and Barry R Chiswick. 1966. "Education and the Distribution of Earnings." *American Economic Review* 56(1/2):358–369.

Bedard, Kelly. 2001. "Human Capital versus Signaling Models: University Access and High School Dropouts." *Journal of Political Economy* 109(4):749–775.

Benhabib, Jess and Alberto Bisin. 2018. "Skewed wealth distributions: Theory and empirics." *Journal of Economic Literature* 56(4):1261–91.

Benhabib, Jess, Alberto Bisin and Mi Luo. 2017. "Earnings inequality and other determinants of wealth inequality." *American Economic Review* 107(5):593–97.

Benhabib, Jess, Alberto Bisin and Shenghao Zhu. 2011. "The distribution of wealth and fiscal policy in economies with finitely lived agents." *Econometrica* 79(1):123–157.

Best, J and K Dogra. 2019. "Capital Management and Inequality." *Working Paper* .

Bhuller, Manudeep, Magne Mogstad and Kjell G Salvanes. 2017. "Life-cycle earnings, education premiums, and internal rates of return." *Journal of Labor Economics* 35(4):993–1030.

Black, Sandra E, Paul J Devereux and Kjell G Salvanes. 2005. "Why the apple doesn't fall far: Understanding intergenerational transmission of human capital." *American Economic Review* 95(1):437–449.

Black, Sandra E, Paul J Devereux, Petter Lundborg and Kaveh Majlesi. 2018. "Learning to take risks? The effect of education on risk-taking in financial markets." *Review of Finance* 22(3):951–975.

Brinch, Christian N and Taryn Ann Galloway. 2012. "Schooling in adolescence raises IQ scores." *Proceedings of the National Academy of Sciences* 109(2):425–430.

Calvet, Laurent E and Paolo Sodini. 2014. "Twin picks: Disentangling the determinants of risk-taking in household portfolios." *Journal of Finance* 69(2):867–906.

Card, David. 1999. Chapter 30 - The Causal Effect of Education on Earnings. Vol. 3 of *Handbook of Labor Economics* Elsevier pp. 1801–1863.
**URL:** *https://www.sciencedirect.com/science/article/pii/S1573446399030114*

Card, David. 2001. "Estimating the return to schooling: Progress on some persistent econometric problems." *Econometrica* 69(5):1127–1160.

Card, David and Alan B Krueger. 1992. "Does school quality matter? Returns to education and the characteristics of public schools in the United States." *Journal of Political Economy* 100(1):1–40.

Christiansen, Charlotte, Juanna Schröter Joensen and Jesper Rangvid. 2008. "Are economists more likely to hold stocks?" *Review of Finance* 12(3):465–496.

Clark, Damon and Paco Martorell. 2014. "The signaling value of a high school diploma." *Journal of Political Economy* 122(2):282–318.

Cole, Shawn, Anna Paulson and Gauri Kartini Shastry. 2014. "Smart money? The effect of education on financial outcomes." *The Review of Financial Studies* 27(7):2022–2051.

De Nardi, Mariacristina and Giulio Fella. 2017. "Saving and wealth inequality." *Review of Economic Dynamics* 26:280–300.

Dietz, Peter O. 1968. "Components of a measurement model: rate of return, risk, and timing." *Journal of Finance* 23(2):267–275.

Duflo, Esther. 2001. "Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment." *American Economic Review* 91(4):795–813.

Eika, Lasse, Magne Mogstad and Ola Vestad. 2020. "What can we learn about household consumption from information on income and wealth." *Journal of Public Economics* 189.

Fagereng, Andreas, Luigi Guiso, Davide Malacrino and Luigi Pistaferri. 2020. "Heterogeneity and Persistence in Returns to Wealth." *Econometrica* 88(1):115–170.

Fagereng, Andreas, Martin B Holm and Kjersti N Torstensen. 2020. "Housing wealth in Norway, 1993-2015." *Journal of Economic and Social Measurement* 45(1):65–81.

Foà, Gabriele, Leonardo Gambacorta, Luigi Guiso and Paolo Emilio Mistrulli. 2019. "The supply side of household finance." *Review of Financial Studies* 32(10):3762–3798.

Gabaix, Xavier, Jean-Michel Lasry, Pierre-Louis Lions and Benjamin Moll. 2016. "The dynamics of inequality." *Econometrica* 84(6):2071–2111.

Garbinti, Bertrand, Jonathan Goupille-Lebret and Thomas Piketty. 2021. "Accounting for wealth inequality dynamics: Methods, estimates and simulations for France." *Journal of the European Economic Association* 19(1):620–663.

Gennaioli, Nicola, Andrei Shleifer and Robert Vishny. 2015. "Money doctors." *Journal of Finance* 70(1):91–114.

Girshina, Anastasia. 2019. "Wealth, Savings, and Returns Over the Life Cycle: The Role of Education." *Working Paper* .

Guiso, Luigi, Andrea Pozzi, Anton Tsoy, Leonardo Gambacorta and Paolo Emilio Mistrulli. 2022. "The cost of steering in financial markets: Evidence from the mortgage market." *Journal of Financial Economics* 143(3):1209–1226.

Guiso, Luigi and Fabiano Schivardi. 2011. "What determines entrepreneurial clusters?" *Journal of the European Economic Association* 9(1):61–86.

Guiso, Luigi and Tullio Jappelli. 2020. "Investment in financial information and portfolio performance." *Economica* 87(348):1133–1170.

Hastings, Justine S, Brigitte C Madrian and William L Skimmyhorn. 2013. "Financial literacy, financial education, and economic outcomes." *Annual Review of Economics* 5(1):347–373.

Heckman, James J, Lance J Lochner and Petra E Todd. 2006. "Earnings functions, rates of return and treatment effects: The Mincer equation and beyond." *Handbook of the Economics of Education* 1:307–458.

Hvidberg, Kristoffer Balle. 2023. "Field of study and financial problems: How economics reduces the risk of default." *The Review of Financial Studies* 36(11):4677–4711.

Jappelli, Tullio and Mario Padula. 2017. "Consumption growth, the interest rate, and financial sophistication." *Journal of Pension Economics & Finance* 16(3):348–370.

Kacperczyk, Marcin, Jaromir Nosal and Luminita Stevens. 2019. "Investor sophistication and capital income inequality." *Journal of Monetary Economics* 107:18–31.

Lei, Xiaowen. 2019. "Information and inequality." *Journal of Economic Theory* 184:104937.

Low, Corinne. 2024. "The human capital–reproductive capital trade-off in marriage market matching." *Journal of Political Economy* 132(2):552–576.

Lucas, Jr, Robert E. 1978. "On the size distribution of business firms." *Bell Journal of Economics* pp. 508–523.

Lusardi, Annamaria, Pierre-Carl Michaud and Olivia S Mitchell. 2017. "Optimal financial knowledge and wealth inequality." *Journal of Political Economy* 125(2):431–477.

Luttmer, Erzo G. J. 1999. "What Level of Fixed Costs Can Reconcile Consumption and Stock Returns?" *Journal of Political Economy* 107(5):969–997.

Merton, Robert C. 1975. Optimum consumption and portfolio rules in a continuous-time model. In *Stochastic Optimization Models in Finance*. Elsevier pp. 621–661.

Ness, Erik. 1971. *Skolens Årbok 1971 (The Primary School Yearbook 1971)*.

Oreopoulos, Philip. 2006. "Estimating average and local average treatment effects of education when compulsory schooling laws really matter." *American Economic Review* 96(1):152–175.

Peress, Joel. 2003. "Wealth, information acquisition, and portfolio choice." *Review of Financial Studies* 17(3):879–914.

Psacharopoulos, George and Harry Anthony Patrinos. 2018. "Returns to Investment in Education: A Decennial Review of the Global Literature." *Education Economics* 26(5):445–458.

Rosenzweig, Mark R. 1995. "Why are there returns to schooling?" *American Economic Review* 85(2):153–158.

Saez, Emmanuel and Gabriel Zucman. 2016. "Wealth inequality in the United States since 1913: Evidence from capitalized income tax data." *Quarterly Journal of Economics* 131(2):519–578.

Sandewall, Örjan, David Cesarini and Magnus Johannesson. 2014. "The co-twin methodology and returns to schooling – testing a critical assumption." *Labour Economics* 26:1–10.

# A Examples of Departure from the Frictionless Case

## Examples of $z_i$

Assume first $k_i = k^F$, the frictionless value and focus on cases that lead $z_i$ to fall short of $z^F$.

**Costly stock market participation.** The friction faced by the investor is a fixed participation cost to hold stocks. The investor portfolio solution will then be a wealth threshold $\overline{w_i}$, below which the investor stays out of the stock market. Let $I(w_{i,}\overline{w_i}) = 1$ if $w_i > \overline{w_i}$ and zero otherwise. Then $d_i = z_i = \alpha_i r^e(1 - I(w_i - \overline{w_i}))$ so that the return to wealth will be $r_{it}^w = r_i^F - \alpha_i r^e(1 - I(w_i - \overline{w_i})) + \eta_t + \alpha_i \epsilon_t$. The individual return to wealth will be positively correlated with current wealth and with any variable that affects the threshold $\overline{w_i}$; education and ability may affect returns through this channel if high education or high ability investors face a lower cost of participating in the market.

**Limited access to investment in private business.** Some people's portfolio can include investment in a business that is individual-specific and not accessible by other investors, such as a private business. Assume all people have access to public equity. For private equity investors let $r_{i,p}^e$ and $\sigma_{i,p}^2$ denote the private business equity premium and the variance of private equity returns, respectively. Both are individual specific. To illustrate, assume private equity returns are independent from public equity returns and investors have mean-variance preferences with risk tolerance parameter $\tau_i$ . Let $\alpha_{i,p}$ denote the share in private equity and $\alpha_{i,l}$ the share in listed stocks (public equity). Let $I(F_i, \overline{F_i})$ denote an indicator function $= 1$ if the state variable $F_i > \bar{F}_i$ (a threshold) is such that the individual has access to a private business. Variable $F_i$ can be for instance a minimum capital requirement to set up a business in the presence of borrowing constraints or a minimum managerial ability to run a business (as in Lucas, 1978) in the presence of a set up cost (Guiso and Schivardi, 2011) Then $d_i = z_i = (\alpha_i r^e - \alpha_{i,l}^e r^e - \alpha_{i,p} r_{i,p}^e) I(F_i, \overline{F_i})$ and the observed return on wealth will be:

$$r_{it}^w = r_i^F - (\alpha_i r^e - \alpha_{i,l}^e r^e - \alpha_{i,p} r_{i,p}^e) I(F_i, \overline{F_i}) + \eta_t + \alpha_{i,l}\epsilon_t + \alpha_{i,p}\zeta_t.$$

Notice that, in this case, the return to wealth is affected by an individual specific component $r_{i,p}^e$ ; the expression also includes a time varying shock to private business returns $\zeta_t$. Returns to wealth will depend on variables that affect access to private business as well as on the specific return the investor obtains from the business, including possibly the level of education and experience in the business as well as specific managerial ability.

## Examples of $k_i$

We now assume $z_i = z^F = 0$ and focus on cases that cause $k_i$ to depart from $k^F$.

**Endogenous information collection (Arrow, 1987; Peress, 2003; Kacperczyk, Nosal and Stevens, 2019; Best and Dogra, 2019).** As in Peress (2003) and Kacperczyk, Nosal and Stevens (2019), assume individuals can obtain, at a cost, a private signal about stock market returns. The cost of acquiring information differs across individuals and may depend on the level of education of the individual as well as his experience with the stock market. Denote by $g_i$ the individual specific signal, which is uncorrelated with the signals received by other individuals. The signal has the following properties:

$g_i = \widetilde{r^e} + \varsigma_i$

with $E(g_i) = r^e, var(g_i) = \sigma^2_{i,\varsigma}$. Thus the signal is undistorted and carries precision $1/\sigma^2_{i,\varsigma}$. Investors who acquire more information obtain a more informative signal and can obtain a more precise prediction of the stocks return and its variance. This results in a modified allocation of the optimal share to stocks. Conditional on the signal, the investors' optimal share is:

$\alpha_{i,g} = \alpha_i + \frac{g_i}{\sigma^2_{i,\varsigma}}$,

Hence, compared to the share with equally informed investors, $\alpha_i$, when private signals can be obtained the investor will twist the allocation towards stocks or towards the safe asset depending on whether he receives an "optimistic" or a "pessimistic" signal. How much she departs from $\alpha_i$ depends on the precision of the signal. The more precise the signal the larger the departure. On average (across signals), the investor will invest in stocks a share $\alpha_{i,g} = \alpha_i + \tau_i \frac{r^e}{\sigma^2_{i,\varsigma}}$. Hence $d_i = k_i = -\tau_i \frac{r^e}{\sigma^2_{i,\varsigma}}$ and the return to wealth will be:

$$r^w_{it} = r^F_i - d_i = r^F_i + \tau_i \frac{r^e}{\sigma^2_{i,\varsigma}}$$

In turn, the informativeness of the signal $\frac{1}{\sigma^2_{i,\varsigma}}$ will depend on the experience, the education and the ability of the investors - as all may lower the cost of acquiring and processing information. It will also depend on the wealth of the individual and her risk tolerance because both increase the size of stock investments and the incentive to acquire information. That is, $\frac{1}{\sigma^2_{i,\varsigma}} = h(E_i, x_i, a_i, w_i, \tau_i)$, implying that $k$-returns increase with education ($E_i$), experience ($x_i$) and ability ($a_i$) as well as with the level of individual wealth (a scale effect). With endogenous information-acquisition, risk tolerance also has an extra effect on returns to wealth because the more risk-tolerant invest more in stocks and have a stronger motive to acquire information.

**Costly advice (Gennaioli, Shleifer and Vishny, 2015).** Suppose that people who lack the sophistication needed to invest in the stock market abstain altogether from buying stock. One reason is that unsophisticated investors would feel too much anxiety investing in stocks, as in Gennaioli, Shleifer and Vishny (2015). Another is that the stock market is ambiguous for them, and they drop out of it to avoid dealing with the ambiguity involved (Guiso et al., 2022). In the absence of financial advisers, there would be heterogeneity in returns simply because - independently of risk tolerance - low $k_i$ investors do not invest in stocks while high $k_i$ ones do. Hence, for the former $r^w_{it} = r_f$, while the for the latter $r^w_{it} = r^F_i$, with the difference reflecting heterogeneity in $k_i$. Advisers can bridge this gap because they can lift the anxiety or eliminate the ambiguity that investors face. Only unsophisticated investors will rely on advice, and with limited trust in advisers, they will be charged a fee by the trusted advisers. Hence, their return on stocks will be $r^e - f_j$ where $f_j$ is the fee charged by adviser $j$. Let $I(E_i, x_i, a_i)$ an indicator function equal to 1 if the investor is sophisticated and zero otherwise. Then $d_i = k_i = r_i^F - r_i^F I(E_i, x_i) - (r^f + \alpha_i(r^e - f_i))(1 - I(E_i, x_i, a_i)$ and the return to wealth will be:

$$r_{it}^w = r_i^F - d_i = r_i^F I(E_i, a_i x_i) + (r^f + \alpha_i(r^e - f_i))(1 - I(E_i, x_i, a_i)),$$

a function of education, ability and experience. In Gennaioli, Shleifer and Vishny (2015), advice is costly but undistorted. In more general models, advice can be distorted (e.g., Guiso et al. 2022; Foà et al. 2019), resulting not only in fees, but also in a different composition of the portfolio, which is skewed towards high-fees instruments, and a departure of the return on equity from the market return $r^e$ and of the return on net worth from its frictionless value.

**Search ability and returns to safe assets.** Sophistication eases individuals' access to information about the set of rates offered by financial intermediaries on investment products or the rates charged on debt instruments. This can affect returns because individuals can search among a broader set of rates. Interestingly, being aware of a broader set of rates can induce heterogeneity in returns to safe assets as well. In the standard portfolio model, there is only one safe asset and all people can access it. A close representation are T-bills for which there is a single market and return. For other safe assets, such as bank deposits, rates differ across intermediaries often reflecting local market power.

In the Norwegian data Fagereng et al. (2020) document that: a) banks differ persistently in the returns they offer for the same type of deposit; b) there is an important individual return heterogeneity component (even conditioning on deposit size); c) high-return individuals tend to match with high-return banks; and d) individuals with more schooling tend to select deposit accounts at banks offering higher returns. They take this as evidence that some market power, reflecting segmentation in local banking markets, generates return differences for the same financial instrument and better informed/more sophisticated individuals seem to be able to spot the better rates. Differences in investors' sophistication can result in access to different information sets about available alternatives and thus different returns on safe assets. Suppose sophisticated investors are aware of a wider sets of rates on deposits and on debts such as mortgages or consumer loans in their local markets, with the size of the set increasing in sophistication. Investors choose the highest rate on deposits in their set (alternatively, the lower rate on debt), which clearly results in heterogeneity in returns on safe assets and net worth. Let $\tilde{r}_i^f$ be the distribution of safe rates faced by investor $i$. Assume this is uniforms in the interval $r_i^{max} = r^{max} \times h(E_i, a_i, x_i)$ and $r_i^{min} = r^{min} \times h(E_i, a_i, x_i)$, where $h(E_i, a_i, x_i)$ is increasing in education, ability and experience. Because the investor will choose the minimum rate she is aware of, we can set $d_i = k_i = r^f - r^{min} \times h(E_i, a_i, x_i)$. The return on wealth will thus be:[38]

[38] Fagereng et al. (2020) find evidence of this channel. They show that individuals who earn higher than average returns on bank deposits do so because they match with banks that pay higher then average interest on deposits. High-rate individuals have in turn higher education.

$$r^w_{it} = r^F_i - d_i = r^F_i - r^f + r^{min} \times h(E_i, a_i, x_i)$$

# B Robustness

In this section, we first present results for alternative definitions of the instrument. Next, we present additional results on returns to specific asset and liability categories.

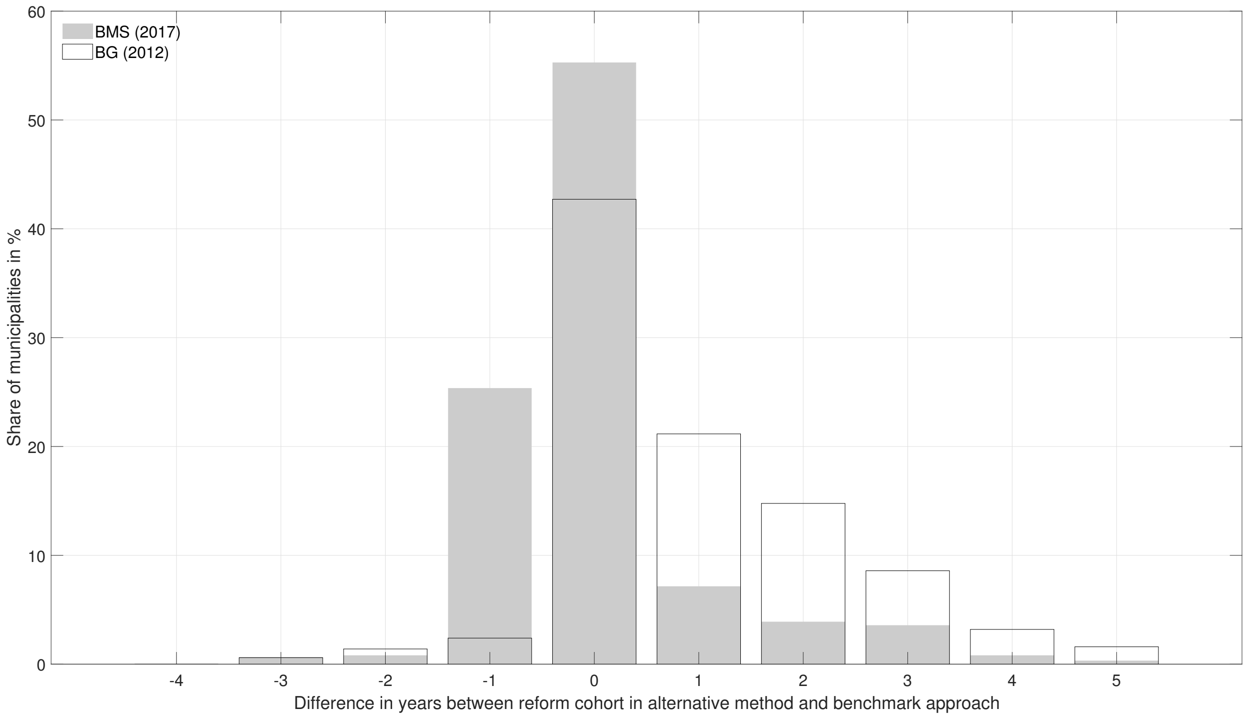
## B.1 Robustness to instrument definition

In the body of the paper, we rely on the municipalities where we directly observe the reform cohort in Ness (1971). There are two alternative approaches in the literature. First, Bhuller, Mogstad and Salvanes (2017) supplement the information from that source and are able to obtain information on reform cohorts for more municipalities. Second, Brinch and Galloway (2012) define cohorts within municipalities as treated by observing when the share of individuals with less than 9 years of schooling in that municipality dropped significantly. In addition, the definition of treated cohorts may differ. Intuitively, an individual in a cohort is treated if (s)he was still in the compulsory schooling system when the reform was implemented. Formally, an individual is defined as treated if their cohort was young enough to have been affected by the reform's implementation. This corresponds to an age of 13 or younger in the reform year for municipalities with a 6-year pre-reform system, and 14 or younger for those with a 7-year pre-reform system. Further, some municipalities did not implement the reform for all classes at the identified implementation year. In that case, we use the variable "includes the following classes" ("omfatter følg. klassetrinn" in Ness, 1971) to identify the treated cohorts. Our definition of treated cohorts differ from that in Bhuller, Mogstad and Salvanes (2017), which means that the two methods in a few cases disagree on the treated cohort in the same municipality.

While the approaches differ, the definition of treated cohorts within municipalities mostly agree. Figure B.1 presents the difference in reform cohorts between our main definition and the two alternative approaches. For both alternative approaches, the methods agree with our main definition in between 40 and 60% of the municipalities. Further, more than 80% of municipalities are within $\pm 2$ years for both methods. While the exact definition of treated cohorts differ, the three approaches should yield similar results, but since the instrument contains measurement error in all cases, the standard errors and estimated coefficients may differ between methods.

Table B.1 shows the effect of education on returns to net worth in IV regressions using the three instruments. Across the instruments, we find that the effect of education on returns to net worth is statistically insignificant in all specifications. The main difference is on the size of the coefficients. While the coefficients tend to be small and stable across genders when we use our preferred instrument, they tend to be more volatile for the two alternative instruments. However, as the sample size increases, as in the pooled sample, the coefficient tend to converge towards zero also for the alternative instruments.

Figure B.1: Comparison of alternative reform instruments




*Notes:* The figure compares the instrument used in Bhuller, Mogstad and Salvanes (2017) and the instrument constructed using the method of Brinch and Galloway (2012) with our benchmark instrument. The figure presents the distribution of the difference in years between the cohort reform computed using alternative methods and our benchmark approach.

Table B.1: The causal effects of education on *k*-returns: IV estimates. Robustness.

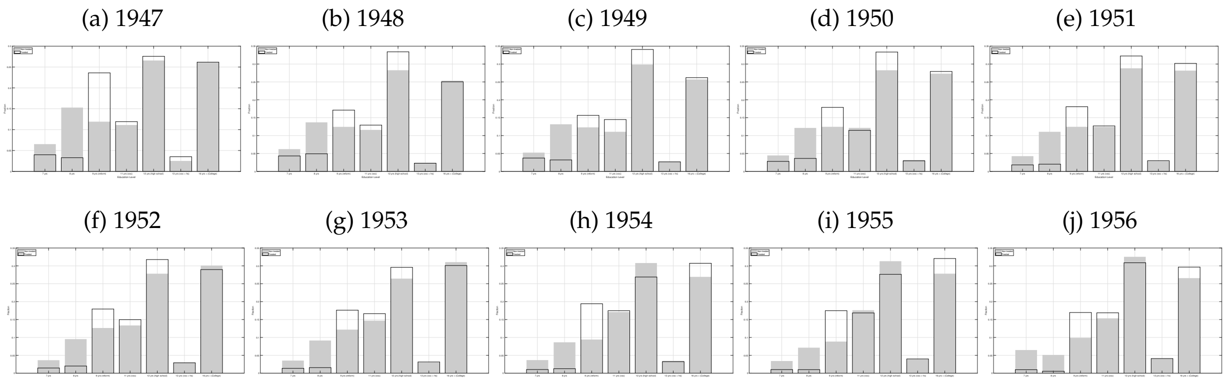
| | *Baseline:* | | | *Instrument from BMS (2017):* | | | *Instrument from BG (2012):* | | |
|---|---|---|---|---|---|---|---|---|---|
| | **(1) Male** | **(2) Female** | **(3) Pooled** | **(1) Male** | **(2) Female** | **(3) Pooled** | **(1) Male** | **(2) Female** | **(3) Pooled** |
| Years of education | 0.040 | -0.016 | -0.021 | -0.187 | -0.337 | -0.297 | 0.399 | -0.234 | -0.002 |
| | (0.481) | (0.361) | (0.297) | (0.500) | (0.399) | (0.330) | (0.574) | (0.444) | (0.352) |
| First-stage *F*-test | 22.13 | 29.40 | 49.96 | 21.23 | 26.03 | 41.71 | 16.29 | 20.99 | 36.89 |
| Observations | 629,915 | 815,467 | 1,445,382 | 542,888 | 885,723 | 1,428,611 | 565,705 | 735,289 | 1,300,994 |

*Notes:* The table shows IV regressions of (scale adjusted) returns to net worth on years of education for the male, female and pooled sample of single individuals belonging to the reform cohorts. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. The instrument for years of education is a dummy =1 if the individual was affected by the school reform. Robust standard errors are clustered at the individual level and reported in brackets.

## B.2 Additional Tables and Figures

This section presents additional results. Figure B.2 presents education levels by cohorts in our sample. Table B.3 and B.4 show results for the effects of education on asset and debt categories. Table B.5 predicts the probability of being in the sample (i.e., being a single person) as a function of the reform instrument. Table B.6, B.7, and B.8 present results for net worth, asset classes, and liabilities when we estimate using reduced-form regressions. Finally, Table B.9 considers the robustness of *l*-return estimates when we change sample age restrictions.

Figure B.2: Education histogram by cohorts



(a) 1947 (b) 1948 (c) 1949 (d) 1950 (e) 1951

(f) 1952 (g) 1953 (h) 1954 (i) 1955 (j) 1956

*Notes:* The figure shows the distribution of years of schooling for each treated and non-treated cohort generation. "Treated" are all individuals that were affected by the school reform; "non-treated" all members of the reform cohorts unaffected by the reform.

Table B.2: Education attainment and *k*-returns: OLS estimates

| | *Returns to net worth* | | |
|---|---|---|---|
| | **(1)**<br>**Male** | **(2)**<br>**Female** | **(3)**<br>**Pooled** |
| Compulsory schooling pre ref (8 years) | 0.498<br>(0.209) | 0.286<br>(0.175) | 0.401<br>(0.136) |
| Compulsory schooling post ref (9 years) | 0.633<br>(0.194) | 0.639<br>(0.158) | 0.654<br>(0.124) |
| Vocational education (11 years) | 0.966<br>(0.194) | 0.876<br>(0.161) | 0.944<br>(0.126) |
| High school diploma (12 years) | 1.470<br>(0.182) | 1.151<br>(0.153) | 1.308<br>(0.119) |
| Vocational education incl. general<br>high school diploma (13 years) | 1.547<br>(0.221) | 1.595<br>(0.189) | 1.567<br>(0.146) |
| College (16 years) | 1.845<br>(0.186) | 1.597<br>(0.152) | 1.725<br>(0.119) |
| Masters (18 years) | 2.185<br>(0.191) | 1.982<br>(0.164) | 2.077<br>(0.126) |
| Graduate school degree (21 years or more) | 2.647<br>(0.318) | 2.284<br>(0.262) | 2.445<br>(0.209) |
| Observations | 629,915 | 815,467 | 1,445,382 |

*Notes:* The table shows OLS regressions of (scale adjusted) returns to net worth on education attainment dummies for the male, female and pooled sample of single individuals belonging to the cohorts born between 1943 and 1963. The excluded group are individuals with less than 8 years of schooling. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. Robust standard errors are clustered at the individual level and reported in brackets.

Table B.3: The effects of education on $k$-returns to different asset types

| | | | | | | |
|---|---|---|---|---|---|---|
| | *A. Return to gross wealth* | | | | | |
| | **Male** | | **Female** | | **Pooled** | |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **IV** | **OLS** | **IV** | **OLS** | **IV** |
| Years of education | 0.063 | -0.286 | 0.065 | -0.065 | 0.062 | -0.165 |
| | (0.004) | (0.223) | (0.003) | (0.169) | (0.002) | (0.137) |
| First-stage $F$-test | | 21.70 | | 30.03 | | 50.16 |
| Observations | 621,105 | 621,105 | 805,328 | 805,328 | 1,426,433 | 1,426,433 |
| | *B. Return to real wealth* | | | | | |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **IV** | **OLS** | **IV** | **OLS** | **IV** |
| Years of education | 0.003 | -0.290 | 0.026 | 0.009 | 0.014 | -0.119 |
| | (0.004) | (0.253) | (0.004) | (0.194) | (0.003) | (0.156) |
| First stage F-test | | 17.83 | | 26.28 | | 42.60 |
| Observations | 556,713 | 556,713 | 739,841 | 739,841 | 1,296,554 | 1,296,554 |
| | *C. Return to financial wealth* | | | | | |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **IV** | **OLS** | **IV** | **OLS** | **IV** |
| Years of education | 0.068 | -0.050 | 0.053 | -0.055 | 0.059 | -0.051 |
| | (0.002) | (0.092) | (0.001) | (0.076) | (0.001) | (0.060) |
| First stage F-test | | 22.21 | | 28.28 | | 48.87 |
| Observations | 619,992 | 619,992 | 806,796 | 806,796 | 1,426,788 | 1,426,788 |

*Notes:* The table shows OLS and IV regressions of (scale adjusted) returns to Gross Assets (Panel A), Real Assets (Panel B) and Financial Assets (Panel C) on years of education for the male, female and pooled sample of single individuals belonging to the cohorts born between 1943 and 1963. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. The treatment dummy =1 if the individual was affected by the school reform, zero otherwise. Robust standard errors are clustered at the individual level and reported in brackets.

Table B.4: The effects of education on interest rate on debt: IV

| | Male | | Female | | Pooled | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | OLS | IV | OLS | IV | OLS | IV |
| *A. Interest on total debt* | | | | | | |
| Years of education | -0.094 | -0.308 | -0.040 | -0.032 | -0.064 | -0.152 |
| | (0.003) | (0.166) | (0.002) | (0.144) | (0.002) | (0.109) |
| First-stage *F*-test | | 20.34 | | 21.87 | | 41.17 |
| Observations | 511,327 | 511,327 | 672,656 | 672,656 | 1,183,983 | 1,183,983 |
| *B. Interest on mortgages* | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | OLS | IV | OLS | IV | OLS | IV |
| Years of education | -0.071 | -0.283 | -0.022 | -0.007 | -0.044 | -0.132 |
| | (0.003) | (0.153) | (0.002) | (0.134) | (0.002) | (0.101) |
| First stage F-test | | 20.25 | | 21.72 | | 41.08 |
| Observations | 508,115 | 508,115 | 671,109 | 671,109 | 1,179,224 | 1,179,224 |
| *C. Interest on consumption loans* | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | OLS | IV | OLS | IV | OLS | IV |
| Years of education | -0.416 | -0.733 | -0.383 | -0.675 | -0.389 | -0.734 |
| | (0.015) | (0.742) | (0.014) | (0.844) | (0.010) | (0.597) |
| First stage F-test | | 9.10 | | 7.91 | | 15.31 |
| Observations | 130,656 | 130,656 | 155,475 | 155,475 | 286,131 | 286,131 |

*Notes:* The table shows OLS and IV regressions of the interest rate on total debt and its components on years of education for the male, female and pooled sample of single individuals belonging to the reform cohorts. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. The instrument for years of education is a dummy =1 if the individual was affected by the school reform. Robust standard errors are clustered at the individual level and reported in brackets.

Table B.5: The effects of treatment on being in the sample (single), probit model.

| | Dependent variable: in sample | | |
|---|---|---|---|
| | **Male** | **Female** | **Pooled** |
| | (1) | (2) | (5) |
| Treatment | 0.0069 | -0.0022 | 0.0019 |
| | (0.0099) | (0.0093) | (0.0068) |
| Observations | 4,887,092 | 5,030,936 | 9,918,028 |

*Notes:* The table shows the coefficients on treatment from probit regressions of being in the estimation sample for males, females, and the pooled population. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960.

Table B.6: The effects of education on returns to net worth: Reduced-form

| | *Returns on net worth* | | |
|---|---|---|---|
| | **(1)** **Male** | **(2)** **Female** | **(3)** **Pooled** |
| Treatment | 0.009 | -0.004 | -0.005 |
| | (0.112) | (0.084) | (0.068) |
| Observations | 629,915 | 815,467 | 1,445,382 |

*Notes:* The table shows reduced form regressions of (scale adjusted) returns to net worth one the treatment dummy for the male, female and pooled sample of single individuals belonging to the reform cohorts. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. The treatment dummy =1 if the individual was affected by the school reform, zero otherwise. Robust standard errors are clustered at the individual level and reported in brackets.

Table B.7: The effects of education on returns to assets: Reduced-form

| | **(1)** **Male** | **(2)** **Female** | **(3)** **Pooled** |
|---|---|---|---|
| *A. Returns on gross wealth* | | | |
| Treatment | -0.065 | -0.015 | -0.038 |
| | (0.048) | (0.039) | (0.031) |
| Observations | 621,105 | 805,328 | 1,426,433 |
| *B. Returns on real wealth* | | | |
| Treatment | -0.063 | 0.002 | -0.026 |
| | (0.053) | (0.044) | (0.034) |
| Observations | 556,713 | 739,841 | 1,296,554 |
| *C. Returns on financial wealth* | | | |
| Treatment | -0.012 | -0.012 | -0.011 |
| | (0.021) | (0.017) | (0.013) |
| Observations | 619,992 | 806,796 | 1,426,788 |

*Notes:* The table shows reduced form regressions of (scale adjusted) returns to the assets components of net worth on the treatment dummy for the male, female and pooled sample of single individuals belonging to the reform cohorts. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. The treatment dummy =1 if the individual was affected by the school reform, zero otherwise. Robust standard errors are clustered at the individual level and reported in brackets.

Table B.8: The effects of education on interest on debt: Reduced-form

| | (1) Male | (2) Female | (3) Pooled |
|---|---|---|---|
| *A. Rate on total debt* | | | |
| Treatment | -0.073 | -0.007 | -0.034 |
| | (0.038) | (0.031) | (0.024) |
| Observations | 511,327 | 672,656 | 1,183,983 |
| *B. Rate on mortgages* | | | |
| Treatment | -0.067 | -0.001 | -0.029 |
| | (0.035) | (0.028) | (0.022) |
| Observations | 508,115 | 671,109 | 1,179,224 |
| *C. Rate on consumption loans* | | | |
| Treatment | -0.184 | -0.139 | -0.158 |
| | (0.190) | (0.175) | (0.130) |
| Observations | 130,656 | 155,475 | 286,131 |

*Notes:* The table shows reduced form regressions of interest rate on debt and its components on the treatment dummy for the male, female and pooled sample of single individuals belonging to the reform cohorts. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. The treatment dummy =1 if the individual was affected by the school reform, zero otherwise. Robust standard errors are clustered at the individual level and reported in brackets.

Table B.9: The effects of education on *l*-returns - Changing age restrictions

*A. Full sample (25-62 years old):*

| | Male | | Female | | Pooled | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **IV** | **OLS** | **IV** | **OLS** | **IV** |
| Years of education | 0.052 | 0.043 | 0.074 | 0.021 | 0.063 | 0.028 |
| | (0.000) | (0.012) | (0.000) | (0.011) | (0.000) | (0.009) |
| First *F*-test | | 150.36 | | 278.44 | | 398.76 |
| Observations | 22,104,057 | 22,104,057 | 21,763,966 | 21,763,966 | 41,356,521 | 41,356,521 |

*B. Restricted sample (30-62 years old):*

| | Male | | Female | | Pooled | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **IV** | **OLS** | **IV** | **OLS** | **IV** |
| Years of education | 0.066 | 0.041 | 0.076 | 0.026 | 0.073 | 0.029 |
| | (0.000) | (0.013) | (0.000) | (0.011) | (0.000) | (0.010) |
| First *F*-test | | 144.04 | | 268.42 | | 390.23 |
| Observations | 18,486,729 | 18,486,729 | 16,478,502 | 16,478,502 | 34,965,231 | 34,965,231 |

*C. Restricted sample (30-52 years old):*

| | Male | | Female | | Pooled | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **OLS** | **IV** | **OLS** | **IV** | **OLS** | **IV** |
| Years of education | 0.062 | 0.044 | 0.074 | 0.022 | 0.069 | 0.028 |
| | (0.000) | (0.012) | (0.000) | (0.012) | (0.000) | (0.010) |
| First *F*-test | | 159.65 | | 280.92 | | 418.92 |
| Observations | 15,261,271 | 15,261,271 | 13,565,487 | 13,565,487 | 28,826,758 | 28,826,758 |

*Notes:* The table shows regressions of log earnings on years of education for the cohorts born between 1943 and 1963. IV regressions use the reform treatment as an instrument for education. Treatment is a dummy=1 if the individual was affected by the reform; zero otherwise. All regressions in Panel A include cohort-time fixed effects, and a full set of municipality dummies for where parents were located in 1960. All regressions in Panel B include cohort-time fixed effects. Robust standard errors are clustered at the individual level and reported in brackets.