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K-Returns to Education

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Abstract

We exploit a school reform that increased the length of compulsory schooling in Norway in the 1960s to study the causal effect of formal general education on returns on wealth (*k*-returns). OLS estimates reveal a strong, positive and statistically significant correlation between education and returns on individual net worth. This effect disappears in IV regressions, implying that general education has no causal effect on individual performance in capital markets, whose heterogeneity largely reflects non-acquired ability. On the contrary, we find that education causes higher returns in the labor market (*l*-returns). We speculate about possible rationales for this important asymmetry.

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1 Introduction

Prompted by the need to understand the substantial differences in individual incomes, the second half of the 20th century has witnessed a huge research effort on the determinants of the returns to human capital. The seminal paper by Mincer (1958), significantly titled "Investment in Human Capital and Personal Income Distribution," provided an analytically-founded contribution to the causes of income inequality, bringing attention to the labor market returns to education, which we label l-returns.

The second decade of the 21st century is witnessing a reversal in attention towards returns to wealth. The reversal is called for by renewed interest in the determinants of wealth concentration, worries about its rising dynamics in some western countries and the conclusion, after many years of research, that inequality in labor earnings (and thus in l-returns to human capital) are simply unable to explain the large concentration in wealth (for a review see De Nardi and Fella, 2017). In fact, a new strand of literature has shifted attention from heterogeneity in returns to labor to heterogeneity in returns to financial and physical capital, which 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 and (to a contained extent) across generations can generate a steady-state distribution of wealth with a thick right tail, which resembles what is observed in reality. Moreover, persistent heterogeneity in returns, coupled with a positive correlation between wealth returns and wealth levels ("type dependence" and "size dependence" in Gabaix et al. (2016) terminology), can potentially account for rapid transitions in wealth concentration at the top, like 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, 2017).

While theoretical developments have been leading this strand of research, very little was known to support it empirically, mostly due to a lack of comprehensive and reliable data on returns to wealth, and because heterogeneity in returns to wealth was thought to be contained (e.g. Saez and Zucman, 2016). Using population data on Norwegian households Fagereng et al. (forthcoming) document that individuals differ systematically and persistently in returns to wealth.[1] Part of these differences reflect heterogeneity in people's risk exposure, which is compensated with higher returns. However, risk compensation alone does not explain all the difference in individual returns to wealth. This is consistent with the idea that returns to wealth reflect systematic differences in peoples' ability to manage one's savings. Such abilities may arise due to formal education and knowledge accumulated with experience, or, alternatively, to non-learned ability in making investment decisions or in gathering information about the available investment opportunities.

The drivers of heterogeneity in returns to wealth are still largely unknown. In this paper we study whether formal education, besides causally increasing l-returns as established by a large

[1] In a related paper, Bach, Calvet and Sodini (forthcoming) provide analogous evidence of systematic heterogeneity in returns to wealth using population data for Sweden.

literature,[2] also has a causal positive effect on returns to capital. We do so by exploiting a school reform in Norway, which tookplace in the 1960s and raised compulsory schooling years by two years, from 7 to 9 years. Because the reform was implemented at different times in different municipalities for random reasons, it provides exogenous variation across cohorts in compulsory schooling. The Norwegian reform has been used by Aakvik, Salvanes and Vaage (2010) to study the effect of compulsory schooling on school attainment and on *l*-returns to education. They document that the reform encouraged treated individuals to undertake more education beyond the compulsory level, which in turn has caused labor earnings to rise. More recently, Bhuller, Mogstad and Salvanes (2017) have relied on the Norwegian reform to study the causal effect of education on lifetime labor earnings, using the almost career-long wage histories for individuals who wereaffected and not-affected by the reform. They find that reform-induced additional schooling causes higher lifetime earnings as well as steeper age-earnings profiles. Black, Devereux and Salvanes (2005) rely on the reform to study the causal effect of education on inter-generational transmission of human capital. In all these instances, the reform treatment proves powerful in identifying causal effects of education on *l*-returns or on parents' investment in offspring's human capital.[3] No one has yet studied the casual effect of education on *k*-returns; this paper is the first attempt in this direction.

Following Fagereng et al. (forthcoming), we rely on administrative records for the whole population of Norway to construct measures of annual returns to net worth and its components. Because net worth reflects all sources of wealth, 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 returns. That is, the return on net worth is a sufficient statistic for an individual's performance in managing their own savings. In order to trace the effect of an individual's education on the return on their own wealth, we focus on single individuals allowing for separate effects of education across males and females.[4]

In OLS regressions, we find that education, measured by years of schooling, has a positive, large and significant association with returns to net worth.[5] This is true for both males and females with small differences in the slope parameter. In the pooled male-female sample, an additional year of schooling is associated with 16 basis points higher returns on wealth. Hence, an individual

[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 up to they comprehensive review).

[3] The reform has proved useful as a source of exogenous variation to study the effect of educational attainment on women decision to delay childbearing (Black, Devereux and Salvanes, 2008), the effect of education on workers mobility (Machin, Salvanes and Pelkonen, 2012) and that on fertility (Monstad, Propper and Salvanes, 2008).

[4] Returns on wealth of married couples in general depend on the education of both, but the importance of the education of each spouse varies depending on who is in charge of wealth management within the family, whether the husband, the wife or both with specific weights. 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 unobserved - to the econometrician not to the spouses - ability) tracing the effect of education without knowledge of who makes decisions is hard. We discuss this issue in Section 7.

[5] Girshina (2019) also finds a positive association between education and returns in Swedish data in OLS and sibling specifications.

with 18 years of schooling (a college degree) earns an average annual return on net worth that is 64 basis points higher than that earned by an individual with a high school diploma (16 years of schooling) and 144 basis points higher than that of someone who just completed the post reform compulsory schooling. Over a working life of 40 years, these differences in returns would result in differences a higher wealth at retirement of 17% for a college graduate compared to a high-school graduate, and of 46% for a college graduate compared to someone who completed compulsory schooling.

We find a positive and significant correlation between education and returns on assets (and their components, both real and financial) and a negative correlation with the rate on debt, with a larger effect on the interest rate on mortgages. This implies that the correlation between education and the return on net worth results not only from differences in wealth composition but also from differences in individual returns within each asset or liability component of net worth.

Of course, the positive effect on *k*-returns we observe in OLS regressions may arise because education happens to be correlated with unobserved wealth management ability. 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 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 on net worth in OLS regression on the sample of twins but the effect vanishes when we control for twin fixed effects.

This result is specific to *k*-returns. If we estimate standard Mincerian OLS regressions of log wages on years of education we find a positive and highly statistically significant correlation between education and wages; the effect increases slightly in size and remains highly significant in IV regressions using exposure to the reform as instrument for education. We obtain the same results if the twins sample design is used. Thus, the gap between the OLS and IV regressions when estimating *k*-returns is not a reflection of lack of power of the instrument. Rather, our estimates suggest that general skills learned at school do not pay off in terms of efficient management of own savings though they do pay off in the labor market. What matters for individual performance in capital markets is non-acquired skills, which are also an important input for investment in education (hence the correlation between returns and education in OLS regressions).

This interpretation is consistent with the findings of a recent paper by Barth, Papageorge and Thom (2018). Focusing on a sample of US investors, they find that genetic endowment - a measure of pre-existing ability - strongly 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 direct support to their interpretation and, importantly, pin down one key channel through which financial capability affects wealth accumulation: by enhancing returns on 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 or innate (we will refer to the latter as ability or talent), are key in explaining heterogeneity in returns to wealth and thus wealth inequality (Peress, 2003;

Best and Dogra, 2017; Kacperczyk, Nosal and Stevens, 2019; Lei, 2019). It 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, but as Hastings, Madrian and Skimmyhorn (2013) argue in their thoughtful review of this literature, the causality of the effects still needs to be established. As they also argue, there is a need for more causal evidence in the debate on the effect of financial education on financial literacy. 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 add some skepticism to 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 attention on the effect of education on *k*-returns in the next section we offer an illustrative example. In Section 3 we set up an analytical framework of the determinants of returns on wealth; we start from a friction-less 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 4 we lay down the empirical model and discuss the identification challenges. Section 5 describes our data sources; Section 6 illustrates the Norwegian reform and shows properties of the instrument. We also discuss here estimates of the effect of education on *l*-returns. Section 7 shows the results of the estimates of *k*-returns, first for the OLS regressions, then for the IV estimates. Section 8 discusses interpretations of the gap between the two. Section 9 concludes.

2 The Importance of Return Heterogeneity on Wealth: an Example

Skill-induced heterogeneity in *k*-returns can potentially be as important as *l*-returns to education in causing large differences in levels of wealth at retirement. To appreciate this point consider an illustrative example. Consider two individuals, A and B, that each earn the same labor income of, say EUR 100,000 per year. They both start saving 20% of their labor income at age 25, income is constant over age and both retire at age 65. The only difference between A and B is their return on wealth. A earns persistently a return of 3.5%, B a persistent return of 6%. This return difference is roughly consistent with the difference in returns to net worth between an individual with 20 years of schooling and one with 11 years of formal education implied by the OLS estimates in Section 6 (Table 6). Under these assumptions, at retirement (age 65) B would have accumulated assets worth 3.5 million euros; A's assets would instead amount to 1.8 million euros, almost only half of A's retirement assets. Let us now ask: holding unchanged his propensity to save and the permanent return on wealth, how much labor income should A have to earn in order to be able to have the same assets at retirement as B? To match B's wealth at retirement A should save almost twice - 39,000 Euros per year - and thus need to earn 195,000 Euros of labor income. Put simply, *k*-returns can generate differences in people's asset accumulation, more dramatic than those generated by even remarkable differences in returns to human capital.[6] Yet, while substantial attention has been

[6] A *l*-return to education of 6.7% for each additional year of schooling would double labor income if the education gap was 15 years of schooling. Hence, another way to appreciate the importance of heterogeneity in returns to wealth

given to understanding the latter; serious research has thus far ignored the former. We are the first to study the causal effect of general education on returns to wealth.

3 Analytical Framework

In classical models of portfolio allocations the only driver 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 α_i invested in risky assets by an individual with relative risk tolerance τ_i facing a risky assets premium r^e and variance of risky assets returns σ^2 is $\alpha_i = \tau_i \frac{r^e}{\sigma^2}$. Investors have the same information about returns and they all have access to the available risky assets and thus face the same returns distributions. If the return on the safe asset is r^f - the same for all individuals - the average return on individual wealth will be

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

and the 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. The observed heterogeneity in returns would be only a reflection of individual preferences for risk. 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. [7] We call this return to wealth the friction-less return and label it $r_i^F = r^f + \alpha_i r^e$. It measures the return on wealth an individual would earn on average if the market were friction-less and individuals were 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 friction-less 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. (forthcoming) implies that this representation fails to fit the

for differences in wealth at retirement is to notice that, assuming A and B earn the same return on wealth of 3.5%, B would need 15 years more schooling than A and an annual l-return of 6.7% to have at retirement (almost) twice as much wealth as A.

[7] 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, $\frac{r_i^w - r^f}{s_i \sigma} = \frac{r^e}{\sigma}$ is the same for all individuals, and thus unrelated to any individual observable characteristic, and the same as the market Sharpe ratio.

data. Fagereng et al. (forthcoming) document substantial heterogeneity in returns to wealth even after controlling for the portfolio composition. This component 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 investments decisions. Recent theoretical papers give support to this idea 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 (2017) 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 friction-less return to wealth. We assume that this distance is affected by two general factors: the *knowledge* capital that and an individual has, k_i and the *accessibility* to investment opportunities that an individual faces, z_i. Thus

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

with distance decreasing in knowledge capital and accessibility and converging to zero as k_i and z_i approach their friction-less values $k^F$and z^F respectively, i.e. $d(k^F, z^F) = 0$.

In Appendix A we illustrate several mechanisms, operating either through knowledge capital[8] or through accessibility to investment opportunities[9] depending on the specific friction that is assumed. All these mechanisms give rise to heterogeneity in returns to net worth across individuals even when they have the same risk tolerance. All entail a role of knowledge capital which differs across individuals, reflecting differences in skills due to either education or ability[10]. To reflect this dependence we write $k_i = k(E_i, a_i^k)$ - a function of education E and ability a^k, which

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

[9] For example because of costly stock market participation or limited access to to private business investment.

[10] We use the word skill to denote an individual capacity to manage his/her assets; this skill may increase with education or may reflect innate ability.

we allow to be specific to k-returns. In a world with frictions, returns are sometimes 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 that sets a wealth threshold to invest in the asset. To capture this, we can write $z_i = z(E_i, a_i^k, w_i)$, where ability and talent 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* of influence of education and ability on the financial performance of an individual. That is, the return to net worth is a sufficient statistic of 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.

4 The Empirical Model and Identification

Following the example above we formalize the empirical model as:

$$r_{it}^w = \beta E_i + \gamma g(age_{it}) + \delta w_{it-1} + \mathbf{x}_{it}\grave{} + f_t + f_i^k + u_{it}$$

The left hand side is the return to net worth of individual i in year t, reflecting the panel nature of our data. E_i is a measure of education 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 education attainment dummies. We capture experience and learning over the life cycle with a polynomial in age $g(age_{it})$. We also let k-returns depend on previous period wealth to reflect scale effects and a vector of either time varying or time invariant individual controls $\mathbf{x}_{it}$. In addition, returns may be affected by a common time varying component f_t, unobserved individual heterogeneity captured by the fixed effects f_i^k reflecting both systematic differences in wealth management ability, a_i^k, and preferences for risk, and a random component measuring for instance "luck". Controlling for wealth is crucial to ascertain whether education affects returns to wealth *directly* in addition to affecting k-returns indirectly, because education increases labor income, thereby increasing savings and wealth scale. It is this net-of-l-returns effect of education that we are mostly interested in.

The identification of β poses two major challenges. First, as in the estimation of l-returns to education, E_i may be correlated with unobserved ability or even risk tolerance, both reflected in f_i^k. Because (completed) education is time-invariant, unobserved heterogeneity cannot be controlled for exploiting the panel dimension of the data. To deal with this issue we rely on an IV strategy that exploits the Norwegian school reform of the 1960s discussed in detail in the next section.

Second, as mentioned above, education may affect k-returns just because it affects wealth through its effects on labor income and savings. This effect of education, though unnoticed in the literature and potentially important given the evidence of "scale dependence" documented in Fagereng et al. (forthcoming) and Bach, Calvet and Sodini (forthcoming), would not be a novelty.

It would be a channel through which l-returns to education end up affecting k-returns. However, all the mechanisms described in Section 3 imply that education and/or ability may affect returns holding the scale of wealth constant. Indeed, Fagereng et al. (forthcoming) show that there is considerable persistent heterogeneity in returns, which is not due to size dependence. To shut down the size dependence channel, we need a consistent estimate of δ. In fact, just controlling for wealth in the above equation is not enough because wealth is likely to be correlated with unobserved ability, a potential source of wealth endogeneity which would produce a biased estimate of δ. To deal with this problem we exploit the panel dimension to consistently estimate δ and then plug in the estimated coefficient in the k-returns regression. Specifically, we take first differences of (1) to eliminate unobserved heterogeneity and estimate

$$\Delta r_{it}^{w} = \delta \Delta w_{it-1} + \gamma \Delta g(age_{it}) + \Delta \mathbf{x}_{it}` + \Delta u_{it}$$

to retrieve an estimate of $\hat{\delta}$ and use it to compute "scale adjusted returns $r_{it}^{\tilde{w}} = r_{it}^{w} - \hat{\delta} w_{it-1}$. Then, we estimate

$$r_{it}^{\tilde{w}} = \beta E_i + \gamma g(age_{it}) + \mathbf{x}_{it}` + f_t + f_i^k + u_{it}$$

where we instrument education using the exogenous variation in the length of studies created by the reform.

5 The Data

5.1 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. (forthcoming) 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. For the period 1993-2015 we can link this database with several additional administrative registries: tax records containing detailed information about the individual's sources of income from labor and capital and the value of asset holdings and liabilities as well as a housing transaction registry. For the shorter period 2004-2015 we also have access to a shareholder registry with detailed information on listed and unlisted shares owned, as well as balance sheet data for the private businesses owned by the individual. The value of asset holdings and liabilities is measured as of December 31.

The data we assemble have several noteworthy advantages for the purpose of our study. First, our 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. This allows us to retrieve the data on returns on wealth for all relevant school-reform cohorts who survived up to the 2004-2015 period (the time interval over which we observe returns to wealth, as we discuss below). The availability of population data is also essential for us to be able to focus attention on single adult males and females and still count on a large set of observations. 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 *l*-returns-free effect of education on returns to wealth. The long individual panel data dimension is also crucial to obtain reliable measures of average return on wealth and measures of individual returns volatility. Because the data cover the whole relevant population, they are free from attrition, except the (unavoidable) one arising from mortality and emigration. Fourth, unique identifiers allow us to match parents with their children which allows us to pin down where the current adult individuals in our sample were located at the time of the reform in the 1960s when they were school age. This is crucial to establish who was treated and who was not by the school reform.

For the purpose of this paper, we use data starting in 2004, as the shareholder registry data is not available for previous years. The shareholder registry is necessary to identify each stock in the portfolio and be able to obtain accrual measures of annual returns on stocks. In most of our analyses, we use wealth data in 2004 as the initial condition, and the period 2005-2015 as our sample period. Following Fagereng et al. (forthcoming), 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. Finally, we focus on the Norwegian population belonging to the cohorts born between 1943 and 1963 which are potentially affected by the school reform, as we discuss below. Hence, our sample will include individuals aged between 41 and 61 in 2004, the first year we compute returns, and between 52 and 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.

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

5.2 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,[11] 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.[12]

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, 2018), 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).

5.3 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:

$$r_{it}^{w} = \frac{y_{it}^{f} + y_{it}^{r} - y_{it}^{b}}{w_{it}^{g} + F_{it}^{g}/2} \tag{1}$$

The numerator is the sum of income from financial assets, y_{it}^{f}, and from real assets, y_{it}^{r}, minus the

[11] 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.

[12] 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 for listed companies to obtain an alternative estimate of the value of private business wealth (see Fagereng et al. (forthcoming) for details). All results using this alternative measure are unaffected.

cost of debt, y_{it}^{b}, all measured as flows accrued in year t. 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.[13]

In equation (1) we express the dollar yield on net worth as a share of *gross* wealth (or total assets). The sign of the return depends only on the sign of the yield (and not on that of net worth), thus avoiding assigning positive returns to individuals with negative net worth and debt cost exceeding income from assets, or infinite returns to people with zero net worth and positive net capital income.

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) as well as[14] 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 (2017) 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 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.[15] 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 (1), by scaling the income corresponding to specific assets with their beginning of period stock plus half of the net annual flows, i.e.

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

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 returns to net worth captures all sources of hetero-

[13] 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. (forthcoming, 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.

[14] 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.

[15] 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. (forthcoming) who also show that corporate tax neutrality holds in Norway during our sample period).

Table 1: Summary statistics

	Non-treated				Treated			
	Mean	**SD**	**Median**	**N**	**Mean**	**SD**	**Median**	**N**
A. Demographic variables:								
Age	61.55	4.81	62	817,380	51.94	5.10	52	975,348
Male	0.41	0.49	0	817,380	0.46	0.50	0	975,348
Family Size	1.20	0.48	1	817,380	1.48	0.76	1	975,348
Less than High School	0.32	0.48	0	817,380	0.23	0.42	0	975,348
High School	0.40	0.49	0	817,380	0.42	0.49	0	975,348
College	0.28	0.45	0	817,380	0.34	0.47	0	975,348
Years of education	11.79	3.20	12	817,380	12.50	2.89	12	975,348
B. Assets and Liabilities:								
Financial wealth	89,967	232,273	34,873	817,380	72,081	267,750	23,183	975,348
Risky assets	41,195	555,784	0	817,380	53,399	1,900,000	0	975,348
Private equity	23,419	496,227	0	817,380	36,874	1,790,000	0	975,348
Housing wealth	662,813	813,332	516,520	817,380	625,631	652,612	497,595	975,348
Gross wealth	776,199	1,080,000	594,241	817,380	734,587	2,080,000	558,667	975,348
Debt	77,715	151,926	35,448	817,380	116,836	478,832	73,559	975,348
Net worth	698,352	1,050,000	524,219	817,380	616,713	1,920,000	445,303	975,348
C. Returns on wealth:								
Financial wealth	0.99	5.13	1	737,813	0.91	5.22	1	877,902
Deposits	0.58	1.28	0	688,150	0.48	1.31	0	810,148
Risky assets	4.68	23.39	7	271,120	4.58	22.53	6	334,977
Listed shares	5.78	25.49	9	268,042	5.78	24.75	8	330,551
Stock funds	5.42	22.12	9	237,290	5.49	21.77	9	298,652
Housing	3.75	11.13	2	663,184	3.68	10.97	2	777,040
Private equity	6.70	20.21	1	54,066	9.27	22.35	2	65,070
Gross wealth	3.56	10.03	2	734,946	3.58	10.06	2	876,297
Debt	2.12	2.15	2	578,365	2.26	1.97	2	716,945
Long-term debt	2.01	2.05	2	580,961	2.16	1.88	2	747,100
Consumer debt	8.59	9.12	7	124,681	8.61	8.80	7	191,463
Net worth	3.18	10.90	2	741,299	3.01	11.10	1	881,937

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

geneity in returns to wealth across individuals. This 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 3% but are very heterogeneous with a standard deviation around 11%.

6 The Norwegian reform and the instrument

The 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 raising it from 7 to 9

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

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

years. Black, Devereux and Salvanes (2005) provide a detailed description of the reform and we refer to them to summarize its salient features. The reform was implemented at the municipality level - the highest decentralization level of administrative power in Norway. To ease municipalities' job, the law mandated that all municipalities must have implemented 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 the schools in other municipalities followed the new rules. Hence, members of 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 followed two extra years of schooling if they lived in a 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.[16]

The implementation of the reform was financed by the government based on a plan presented 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 concerned 732 municipalities

[16]Besides raising compulsory schooling the reform standardized the curriculum with the goal of improving average school quality. It then follows that, in so far the reform also increased school quality, our estimates will reflect both the increase in the number of years of education and the improvement in the quality .

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

		Non-treated			Treated		
Year	**Observe.**	**Non-treated**	**%**	**Years of education**	**Treated**	**%**	**Years of education**
1943	24,108	24,108	100.00	11.59	0	0.00	-
1944	27,568	27,568	100.00	11.72	0	0.00	-
1945	29,025	29,025	100.00	11.74	0	0.00	-
1946	33,213	33,061	99.54	11.79	152	0.46	11.61
1947	32,049	30,219	94.29	11.86	1,830	5.71	12.29
1948	31,602	27,893	88.26	11.92	3,709	11.74	12.31
1949	30,761	26,516	86.20	12.04	4,245	13.80	12.42
1950	30,650	24,731	80.69	12.10	5,919	19.31	12.46
1951	30,108	21,533	71.52	12.26	8,575	28.48	12.61
1952	31,786	18,784	59.10	12.36	13,002	40.90	12.56
1953	32,165	15,003	46.64	12.47	17,162	53.36	12.69
1954	32,047	9,408	29.36	12.36	22,639	70.64	12.71
1955	32,830	5,980	18.22	12.49	26,850	81.78	12.77
1956	33,398	2,679	8.02	12.38	30,719	91.98	12.74
1957	33,148	460	1.39	12.13	32,688	98.61	12.78
1958	33,012	73	0.22	12.31	32,939	99.78	12.72
1959	33,116	66	0.20	11.58	33,050	99.80	12.61
1960	32,763	71	0.22	11.92	32,692	99.78	12.65
1961	33,463	81	0.24	12.79	33,382	99.76	12.77
1962	33,398	11	0.03	14.09	33,381	99.97	12.83
1963	34,163	0	0.00	-	34,163	100.00	12.88

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 individual that were affected by the reform; "Non-Treated" those who were not.

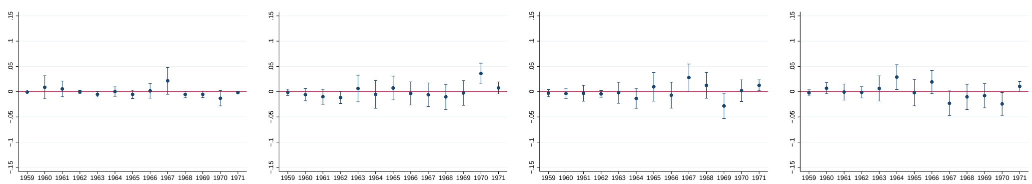
existing in 1960. We are able to identify 655 of them from official administrative records. Figure 1 shows the number of municipalities that has implemented compulsory 9-year education for each year; by 1966 half of the municipalities has adopted; and by the end of 1972, the reform spread over all municipalities.

We stick to the reform dates listed in Ness (1971). Other authors expand the set of municipalities, by either using additional 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 defintions of the reform instrument (Table B.1 in Appendix B.

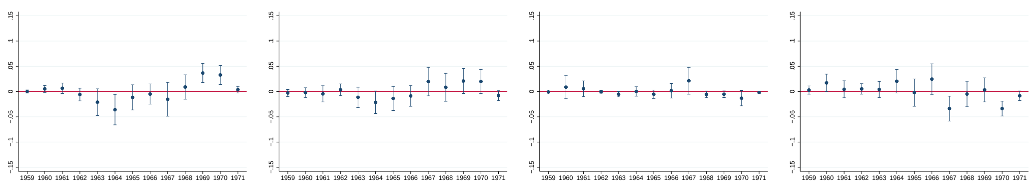
Properties of the instrument

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 trends upward as we move towards the younger cohorts, while that of the non-treated shows an opposite pattern. Our identification will come from variation within a cohort between children living in municipalities 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.

Figure 2: Balancing plots



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



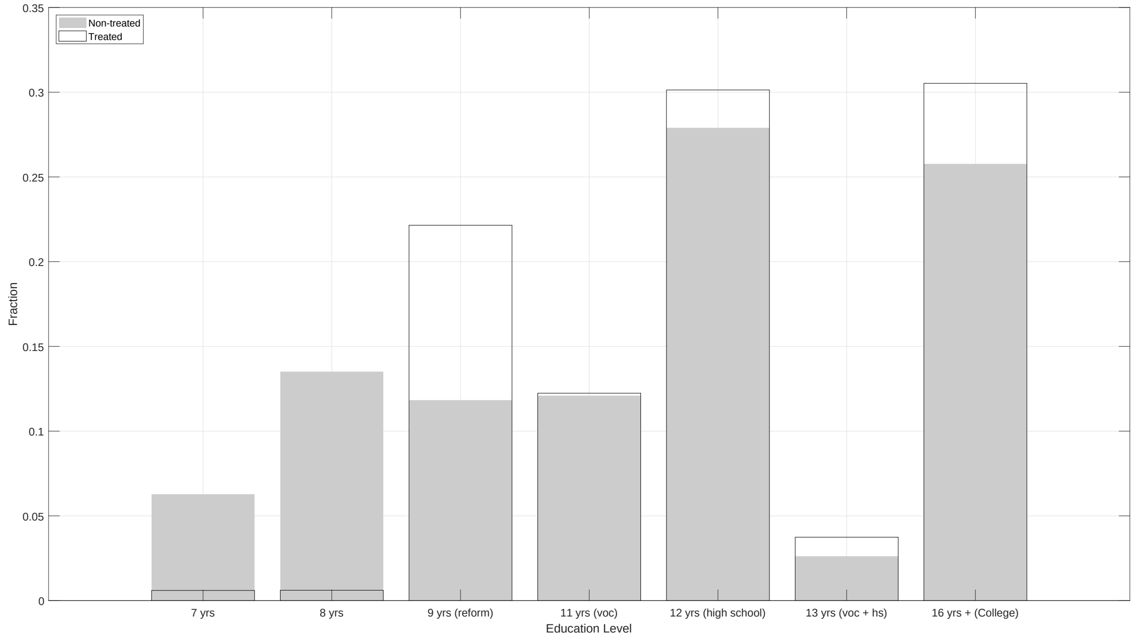
(e) Church members (f) < High school (g) High school (h) College

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.

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 a regression of a dummy equal to 1 if a municipality adopts the reform in a given year and zero otherwise, and regress it on the characteristic 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 time 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, parent 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 Party (the most pro-reform and largest political party). 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 without the new regime, but also shifted the whole distribution of education 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

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.

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 has encouraged those treated to undertake investment in education beyond what they would have done otherwise. 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 determined by the reform.

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

	Years of education		
	(1) Male	**(2) Female**	**(3) Pooled**
Treatment	0.229 (0.049)	0.227 (0.043)	0.224 (0.032)
Observations	705,581	908,018	1,613,599

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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. Robust standard errors are clustered at the individual level and reported in brackets.

To get a sense of the power of the treatment, 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

Table 4: The effects of education on l-returns

A. Full sample:	Male		Female		Pooled	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	OLS	**IV**	**OLS**	**IV**	**OLS**	**IV**
Years of education	0.052	0.043	0074	0.024	0.063	0.028
	(0.000)	(0.012)	(0.000)	(0.011)	(0.000)	(0.009)
First F-test		150.39		271.22		398.76
Observations	24,212,041	22,104,057	21,079,796	19,252,464	45,291,837	41,356,521
B. Twins sample:						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	OLS	**Twins FE**	**OLS**	**Twins FE**	**OLS**	**Twins FE**
Years of education	0.029	0.022	0.054	0.042	0.040	0.030
	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)
Observations	320,706	320,706	268,591	268,591	589,297	589,207

Notes: The table shows regressions of the male log earnings on years of education (first and last column) and on the reform treatment (second column) in the sample of male adults belonging to the reform cohorts. Treatment is a dummy=1 if the individual was affected by the reform; zero otherwise. The IV regression uses as instrument the treatment dummy. All regressions in Panel A include time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. All regressions in Panel B include time fixed effects and a fourth-order polynomial in age. Robust standard errors are clustered at the individual level and reported in brackets.

include 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 0.22 of a year in the pooled sample with a similar impact in the males and females samples.[17] Overall, this suggests that the IV regressions that we will run will not suffer from a weak instrument problem and that there is gain in power when pooling the females and males sample together.

6.1 The causal effect of education on l-returns

Before showing the estimates of education on k-returns, we discuss OLS and IV estimates of the effect of education on l-returns. Panel A in Table 4 shows results of estimates of log earnings for the population belonging to the cohorts born in any year between 1943 and 1963 on years of education and a set of local controls for years, municipalities, and cohorts. We restrict the sample to working age male and female adults (18-62 years). In the OLS regressions, log earnings are positively correlated with education with an estimated return of 4.3% per each additional year of schooling when pooling males and females, and is somewhat higher for females. When we run IV regressions the estimated return is around 3% per year of education, a somewhat lower estimate than the OLS in the pooled sample. In the males sample it is higher at around 4% - close to the 5%

[17] This is the same effect estimated by Bhuller, Mogstad and Salvanes (2017).

Table 5: Years of education and k-returns: OLS estimates

	Returns to net worth		
	(1)	**(2)**	**(3)**
	Male	**Female**	**All**
Years of education	0.176	0.151	0.162
	(0.008)	(0.006)	(0.005)
Observations	693,076	892,908	1,585,984

Notes: The table shows OLS 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. Regression are run on the balanced panel covering the years 2005-2015. All regressions include time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. Robust standard errors are clustered at the individual level and reported in brackets.

return estimate cited by Aakvik, Salvanes and Vaage (2010, footnote 16).[18] As is typical with IV, the standard error of the estimate is greater, but the estimate is still highly significant (t-stat = 3.9). This suggests that the treatment is powerful enough to identify the causal effect of education on l-returns with high precision.

Table 4 also presents results in a twin sample where we first run an OLS for the whole twin sample, and next control for the twin fixed effects to partial out in-born ability. Estimates of the effect of education on l-returns are similar to the IV specification: 4% in the OLS specification which drops to 3% when twin-fixed effects are not accounted for.

7 *K*-returns to Education

7.1 OLS estimates

To estimate k-returns to education we focus of the sample of Norwegian individuals belonging to the 21 cohorts born between 1943 and 1963, which were potentially affected by the school reform. These individuals are aged between 42 and 62 in 2005, the first year in our sample for which we can obtain complete estimates of returns to net worth. For married individuals belonging to the 1943-1963 cohorts we obviously observe returns on *household* net worth. Identification of the effect of education of the two spouses on returns to household wealth is very hard. 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. Fagereng, Guiso and Pistaferri (2019) show that decision power is granted to both spouses but with a much larger weight to the spouse with the highest pre-marriage return to wealth and a lower weight to the

[18]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.

Table 6: Education attainment and k-returns: OLS estimates

	Returns to net worth		
	(1) **Male**	**(2)** **Female**	**(3)** **Pooled**
Compulsory schooling pre ref (8 years)	0.456	0.258	0.364
	(0.196)	(0.161)	(0.126)
Compulsory schooling post ref (9 years)	0.614	0.579	0.609
	(0.182)	(0.147)	(0.116)
Vocational education (11 years)	0.982	0.840	0.914
	(0.181)	(0.149)	(0.117)
High school diploma (12 years)	1.428	1.083	1.249
	(0.171)	(0.141)	(0.111)
Vocational education incl. general high school diploma (13 years)	1.501	1.535	1.508
	(0.208)	(0.177)	(0.137)
College (16 years)	1.822	1.545	1.681
	(0.174)	(0.141)	(0.112)
Masters (18 years)	2.144	1.923	2.023
	(0.180)	(0.153)	(0.118)
Graduate school degree (21 years or more)	2.573	2.200	2.364
	(0.301)	(0.245)	(0.197)
Observations	693,076	892,908	1,585,984

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 reform cohorts. The excluded group are individuals with less than 8 years of schooling. Regression are run on the balanced panel covering the years 2005-2015. All regressions include time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. Robust standard errors are clustered at the individual level and reported in brackets.

spouse with the lowest return. If pre-marriage returns depend on individual education and ability, this allocation rule introduces non-linearities between the education of the two spouses and household returns on wealth which makes identification of the k-returns to education hard.[19] Accordingly, we focus on the population of male and female Norwegians belonging to one of the cohorts born in any one of the years 1943-1963 that are 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 that are observed in all years between 2005 and 2015. Summary statistics on this sample are reported in Table 1.

[19]In the case of married couples the return to household wealth can be specified as $r^w_{it} = \beta(\omega Max[\rho_1(E_1, a^k_1), \rho_2(E_2, a^k_2)] + (1-\omega)Min[\rho_1(E_1, a^k_1), \rho_2(E_2, a^k_2)]) + \gamma g(age_{it}) + \delta w_{it-1} + \mathbf{x}_{it}\grave{} + f_t + f^k_i + u_{it}$ where ρ_1 and ρ_2 are the pre-marriage returns to wealth of spouse 1 and 2 respectively and ω and $1-\omega$ the weights of the spouse with the maximum and minimum pre-marriage returns in the management of post-marriage household wealth. Pre-marriage returns are a function of each spouse education and ability; if this function was linear and we knew the pre-marriage returns (and thus be able to trace the spouse with the maximum and minimum return), we could run simple linear OLS regressions. However, we do not observe pre-marriage returns, implying that estimation of household returns entail a complex non-linear function of the education and ability of the two spouses.

Table 7: Education and returns to assets, OLS

	(1) Male	(2) Female	(3) Pooled
A. Returns to gross wealth			
Years of education	0.066	0.067	0.065
	(0.004)	(0.003)	(0.002)
Observations	690,651	886,974	1,577,625
B. Returns to real wealth			
Years of education	0.002	0.025	0.013
	(0.004)	(0.003)	(0.003)
Observations	613,039	809,812	1,422,851
C. Returns to financial wealth			
Years of education	0.069	0.054	0.060
	(0.002)	(0.001)	(0.001)
Observations	694,613	894,257	1,588,870

Notes: The table shows OLS 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 reform cohorts. Regression are run on the balanced panel covering the years 2005-2015. All regressions include time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. Robust standard errors are clustered at the individual level and reported in brackets.

Table 5 shows the results of the OLS estimates of the effect of years of education on k-returns to net worth. In all estimates, the left hand side is $r_{it}^{w} - \hat{\delta} w_{it-1}$ - the return to net worth net of the scale effect - where $\hat{\delta}$ is obtained from a first difference regression of returns to wealth on the first difference of beginning of period wealth and controls. 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 693,000 observations on males and 893,000 observations on female individuals to allow for differences in the effects of education on k-returns based on gender; we also report results for the pooled sample.[20] The OLS regressions document a very precisely estimated positive association between education and returns to net worth. The association is also sizable - 16 basis points for each additional year of education - and is larger among males (17.6 basis points) but the gender gap is only 2.4 (s.e. 1.0) basis points. Using the estimate for the pooled sample, an individual with

[20]In our sample, we observe more single females than single males. This may sound surprising in light of the fact that for individuals that have never married, males are more prevalent at about 55%. However, for the two additional categories of singles that are in our sample - widows/widowers and divorced - females are more prevalent at 81% and 61%, respectively. Given the age span of our sample the later effect dominates.

Table 8: Education and the cost of debt: OLS regressions

	(1) Male	(2) Female	(3) Pooled
A. Interest on total debt			
Years of education	-0.095	-0.043	-0.066
	(0.003)	(0.003)	(0.002)
Observations	570,882	740,768	1,311,650
B. Interest on mortgages			
Years of education	-0.070	-0.025	-0.045
	(0.003)	(0.002)	(0.002)
Observations	567,343	739,159	1,306,502
C. Interest on consumption loans			
Years of education	-0.410	-0.393	-0.391
	(0.014)	(0.013)	(0.010)
Observations	148,716	174,062	322,778

Notes: The table shows OLS regressions of interest on Total Debt (Panel A), Mortgages (Panel B) and Consumption Loans (Panel C) on years of education for the male, female and pooled sample of single individuals belonging to the reform cohorts. Regression are run on the balanced panel covering the years 2005-2015. All regressions include time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. Robust standard errors are clustered at the individual level and reported in brackets.

a four-year college degree would earn on average a 64 basis points higher return on net worth than a similar individual with a high school degree. Assuming that one dollar is saved each year and capitalizing this one dollar worth of savings with the 64 basis extra return over a working life of 40 years would result in a net worth at retirement 17% higher for an individual with a college degree compared to an otherwise equal individual with a high school diploma. Wealth at retirement wold be 44% higher than that of someone with the compulsory (post reform) level of education.

Table 6 shows results when years of education is replaced by a set of education 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 education attainment and correlation is strong: compared to someone with no education, an individual with post-college schooling (21 years of education) earns on average 237 basis point higher annual return on net worth and the move from after reform compulsory school (nine years) to a high school diploma (12 years) is associated with a 800 basis points higher return on net worth.

The correlation between education and 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 Tables 7 and 8. Education correlates positively with returns on gross assets (Table 7, A), real assets (Table 7, B) and financial assets (Table 7, C), particularly in the pooled men-women sample. It correlates negatively with

Table 9: The causal effects of education on k-returns: IV estimates

	Returns to net worth		
	(1) Male	**(2) Female**	**(3) Pooled**
Years of education	0.040	-0.016	-0.021
	(0.481)	(0.361)	(0.297)
First-stage F-test	22.14	29.41	49.97
Observations	629,915	815,467	1,445,382

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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. 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.

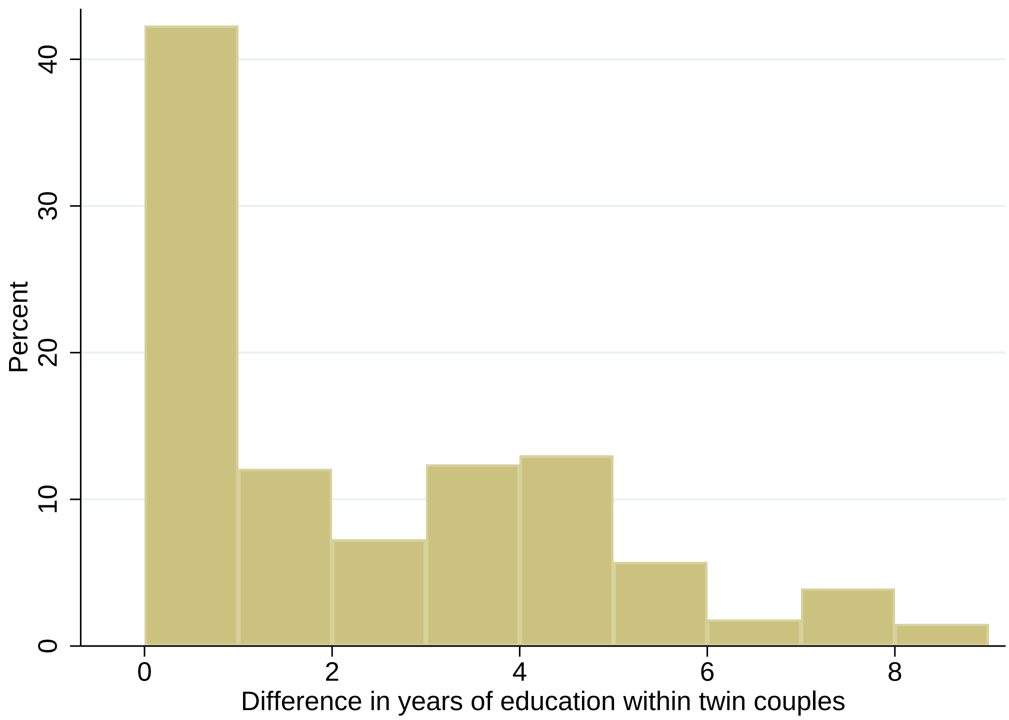
the interest rate paid on total debt (Table 8, A) and that of mortgages and consumer loans (Table 8, B and C, respectively). The marginal effect of an extra year of education is particularly large for consumer loans. Hence, the correlation between education and net worth reflects both higher returns on assets among individuals with higher education as well as a lower cost of debt.

7.2 IV estimates

In this section we discuss instrumental variable estimates of k-returns to education using the differential exposure of various cohorts to the 1960s reform as a source of exogenous variation in education. Table 9 shows the IV estimates for the returns to net worth for males, females and the pooled sample. In all cases the estimated coefficients are much lower than the OLS estimates, dropping to values close to zero. The point estimate is slightly positive in the sample of males and slightly negative in the sample of females and in the pooled sample. In all cases the effect is not statistically significant, suggesting that education has no casual effect on returns to net worth. 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 6 suggests that the instrument does indeed shift the distribution of the number of years of education. Second, while the reform raised compulsory schooling from 7 to 9 years, it has shifted the whole distribution of education attainment, implying that the treatment has not only a local effect (just raising the education of those that would have stopped after seven years of schooling without the reform) but affects also their subsequent education decision. 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 in the pooled sample) implies that the estimates do not suffer from a weak instrument problem, particularly for the pooled sample. Second, as discussed in Section 6, 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 casual effect on l-returns but it has no casual effect on k-returns.

As shown in Appendix B, this finding holds when we look at returns on total assets and its

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.

components, real and financial assets respectively (Table B.3), as well as for the rate on interest paid on total debt and on its two components (mortgages and consumer loans, Table B.4). Contrary to the OLS estimates that predict a positive and significant effect on returns on assets and negative on the cost of debt the IV estimates imply no effect on returns on assets: point estimates are *negative* and not statistically significant. The IV estimates of the effect of education on the cost of debt is negative but statistically not significant. [21]

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).

To further check the robustness of our finding we also follow another strategy: we use twins data to eliminate the effect of unobserved fixed heterogenity (e.g. in ability or risk tolerance) assumed to be the same for twins, and exploit variation in education within same sex twin pairs to identify the causal effect of education. Since we do not observe whether two siblings are twins, we identify the latter by classifying sons/daughters of a mother that were born in the same month as twins. We are able to identify 290 twin couples in our baseline sample where both individuals in the twin couple are single and are present in all years between 2005 and 2015 over which we measure returns. 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%

[21] 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 prediction on k-returns on net worth and all its components. See the Appendix, Tables B.5, B.6, and B.7.

Table 10: The effects of education on k-returns, twins sample

	Returns to net worth					
	Male		**Female**		**Pooled**	
	(1) **OLS**	**(2)** **Twins FE**	**(3)** **OLS**	**(4)** **Twins FE**	**(5)** **OLS**	**(6)** **Twins FE**
Years of education	0.161	0.031	0.163	0.034	0.146	0.032
	(0.093)	(0.203)	(0.089)	(0.148)	(0.066)	(0.119)
Observations	1,928	1,928	2,826	2,826	4,754	4,754

Notes: The table shows regressions of (scale adjusted) returns to net worth on years of education for the sample of twins belonging to the reform cohorts. Regressions are shown for male and female twins and for the pooled sample. Each time the table shows OLS and Twins fixed effects regressions. Regressions are run on the balanced panel covering the years 2005-2015. All regressions include time fixed effects and a fourth-order polynomial in age. Robust standard errors are clustered at the individual level and reported in brackets.

the number of years of education differs on a range between 1 and 8 years. It is on this variation that we rely on to identify the casual effect of education on returns to wealth in this sample. Table 10 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 Table 5, that is, they show a positive and similarly sized effect of years of education on returns to net worth. Not surprisingly, OLS estimates are less precise given the smaller sample size and for males it is not statistically significant. But in the larger female samples and in the pooled sample the correlation is precisely estimated. When twin fixed effects are added, the effect of education shrinks in size (from 0.227 to 0.026 in the pooled sample) and loses its statistical significance.[22]

Recall that if this strategy is used instead to identify the causal effect of education on l-returns, we obtain results that are in line with those using the variation induced by the reform in Table 4, Panel B. The OLS estimates show a positive and highly statistically significant relation with education as in the whole sample, though the estimated return is higher in the twins sample. The IV estimates produce a slightly higher and precisely estimated effect of education; and this is true in the male, female, and pooled samples. Thus, this strategy furhter suggests that while education has a positive causal effect on l-returns, it has none on returns on wealth.

In sum, once unobserved fixed heterogeneity is eliminated, education still significantly affects labor market returns, with marginal effects similar to those obtained in OLS estimates, which is consistent with a large literature on l-returns to education (Psacharopoulos and Patrinos, 2018). On the other hand, when unobserved heterogeneity is removed, the effect of education on returns to wealtgh found in OLS estimates vanishes, implying that formal education has no causal effect on k-returns.

[22]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.

Table 11: Education and returns on deposits: OLS and IV

	Returns to deposits					
	Male		**Female**		**Pooled**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	OLS	**IV**	**OLS**	**IV**	**OLS**	**IV**
Years of education	0.044	0.055	0.030	-0.041	0.036	-0.002
	(0.001)	(0.056)	(0.001)	(0.048)	(0.001)	(0.036)
First-stage *F*-test		23.74		29.64		52.44
Observations	604,709	548,995	824,057	751,761	1,428,766	1,300,756

Notes: The table shows OLS and IV regressions of returns bank deposits on years of education for the of individuals belonging to the reform cohorts. Regressions are shown for single males and female and for and pooled sample. Each time the table shows OLS and IVs regressions. 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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. 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.

8 Interpretation

This leaves us with the question: why does education predict *k*-returns to wealth in OLS regressions while the correlation vanishes once we control for unobserved heterogeneity? Our results imply that *k*-returns are fundamentally affected by either preferences for risk or by capital-management ability, or both, but formal education - differently from what happens for labor market returns - does not pay off in capital markets. Additionally, in order to explain why education predicts *k*-returns in OLS estimates, ability to navigate in capital market or risk tolerance must have a causal effect on education attainment.

To test whether it is *only* preferences for risk that can rationalize the results we follow two strategies. First, we focus on returns on 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 deposits 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 individual risk tolerance. Results in Table 11 show that in 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 (3.6 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 and pooled sample and positive in the males sample, but not statistically significant in all cases. Because deposits are risk-free, this result cannot be due to unobserved heterogeneity in risk tolerance.

Secondly, we run OLS and IV regressions of returns to net worth on years of education controlling for the individual volatility in returns to capture differences in risk tolerance across investors. For each individual, we measure the latter with the variance of individual returns on net

Table 12: 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	0.133	-0.075	0.134	-0.046	0.132	-0.089
	(0.007)	(0.434)	(0.006)	(0.335)	(0.005)	(0.271)
First-stage F-test		22.62		29.55		50.85
Observations	693,070	629,908	892,900	815,460	1,585,970	1,445,368

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 reform cohorts. 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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. 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.

worth over the 2005 and 2015 sample years. Results in Table 12 show that in OLS regressions the marginal effects of education is only slightly reduced when controlling for returns volatility (0.133 basis points instead on 0.166 for each year of education in the pooled sample). This is consistent with education being only mildly correlated with risk appetite, 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 education is never rejected.

While this evidence does not rule out that education also captures heterogeneity in risk tolerance when we look at returns on 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 skills that have a payoff in the labor market, school-acquired skills do not seem to make an individual better at managing his savings. What matters for the latter is only heterogeneity in non-acquired ability.

This interpretation is consistent with recent research by Barth, Papageorge and Thom (2018) 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 fully consistent with Barth, Papageorge and Thom (2018), but compared to them we move a 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 nil: returns to wealth are only affected by pre-education ability.

It is also consistent with the evidence in Black et al. (2018) who study the causal effect of

Table 13: Education on returns to net worth: OLS and Fixed Effects.

	(1)	(2)	(3)
Years of education	0.153	0.099	
	(0.005)	(0.005)	
Male	-0.089	0.277	
	(0.032)	(0.030)	
Demographics	Y	Y	Y
Year effects	Y	Y	Y
Shares x year effects	N	Y	Y
Fixed effects	N	N	Y
Adjusted R-squared	0.057	0.114	0.268
Observations	1,583,881	1,583,881	1,583,881

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.

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, banning this channel would leave the financial portfolio - and so k-returns - unaffected.

To further corroborate this interpretation, we follow Fagereng et al. (forthcoming) and run OLS regressions of returns on 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.[23] The third specification adds 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 heterogeneity, including individual ability.

Our main interest is in the change in the fit of the regression as measured by the R^2 as we move from the first to the second specification, and from this to the third. The change in the 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 unobserved ability to process and use financial information, or heterogeneity in the cost of accessing investment opportunities and other persistent individual traits (such as inter-temporal discounting) that may be relevant for investment

[23]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. (forthcoming) with also include controls for the average individual β of the stock portfolio, private business wealth and housing wealth, again interacted with time dummies. See Fagereng et al. (forthcoming) for a full description of these variables.

decisions. These features affect the average return that individuals extract from their net worth *conditioning* on the risk exposure and the scale of their portfolio.

Table 13 shows the results of these estimates run on the pooled male and females sample (results are similar for the two sub-samples). Together with the other controls education attainment captures part of the variation, as shown by the R^2 of the first column. The estimated correlation, 0.154, is essentially the same as that in Table 5. In moving from the first to the second specification the adjusted R^2 of the OLS estimates increases from 0.05 to 0.11. This suggests that an important part of the observable heterogeneity in returns to net worth reflects compensation for risk. At the same time the marginal effect of education falls to 0.10 implying that education also captures risk exposure as already documented in Table 12, for example because highly educated individuals face lower costs of entering the stock market; but it retains its significance implying that compensation for risk is not the sole reason why education correlates with k-returns. The last column of Table 13 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 0.11 to 0.27. 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 9.

9 Conclusions

In this paper we have studied whether formal general education pays off in capital markets as it does in the labor market. Using a compulsory school reform in Norway to obtain exogenous variation in years of schooling we find that, while education predicts returns to wealth in OLS estimates, it has no casual effect in IV regressions or when unobserved heterogeneity is taken care of using a twins design. General education predicts returns only because it is correlated with ability and risk tolerance, and the latter seem to be the relevant drivers of heterogeneity in individual returns on capital. This is at odds with the evidence on labor earnings where general education has a casual effect on returns. This raises the question of why is there this asymmetry? One possibility is that general education matters for labor earnings because it signals ability and, while signaling is relevant in the labor market,[24] it is clearly irrelevant for returns on self-managed wealth. Another possibility is that while labor market skills may be acquired through formal general education and added to pre-existing abilities, skills that matter for investments are hard to obtain through general education and may, instead, require specific training that enhances individual investment skills. An understanding of this issue 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. Pinning down the effect of specific education requires exogenous variation in the field

[24] 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 identification strategy rests on a strong restriction on what firms observe.

of study.[25] We are undertaking this task in a dedicated project.

[25] Fagereng et al. (forthcoming) show that having a degree in Economics of Finance correlates positively with returns to net worth in OLS regressions that control for years of education. Obviously, the correlation may just reflect a choice to specialize in Economics and Finance by individuals with a talent for it.

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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 $d_i = z_i = s_i r^e (1 - I(w_i - \overline{w_i}))$ so that the return to wealth will be $r^w_{it} = r^F_i - s_i r^e (1 - I(w_i - \overline{w_i}) + \eta_t + s_i \epsilon_t$. 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, like a private business. Assume all people have access to public equity. For private equity investors let $r^e_{i,p}$ and $\sigma^2_{i,p}$ 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 τ_i . Let $s_{i,p}$ denote the share in private equity and $s_{i,l}$ the share in listed stocks (public equity). Let $I(F_i, \overline{F_i})$ 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 $d_i = z_i = (s_i r^e - s^e_{i,l} r^e - s_{i,p} r^e_{i,p}) I(F_i, \overline{F_i})$ and the observed return on wealth will be:

$$r^w_{it} = r^F_i - (s_i r^e - s^e_{i,l} r^e - s_{i,p} r^e_{i,p}) I(F_i, \overline{F_i}) + \eta_t + s_{i,l}\epsilon_t + s_{i,p}\zeta_t.$$

Notice that, in this case, the return to wealth is affected by an individual specific component $r^e_{i,p}$; the expression also includes a time varying shock to private business returns ζ_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, 2017).** 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 market. Denote 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, 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 he departs from α_i depends on the precision of the signal, the more precise the larger the departure. On average (across signals), he 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 on 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 his 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, experience and ability 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 acuire information. Notice that controlling for the share in risky assets absorbs also this effect. However, because people who invest in information face lower conditional uncertainty, they will have higher Sharpe ratios. Indeed, endogenous information acquisition predicts heterogeneous Sharpe ratios correlated with individual education and experience, ability, wealth and risk aversion.

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 to avoid dealing with the ambiguity(Gambacorta et al., 2017). 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, the first have $r^w_{it} = r_f$, while the second have $r^w_{it} = r^F_i$, 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 $I(E_i, x_i)$ be an indicator function equal to 1 if the investor is sophisticated and zero otherwise. Then $d_i = k_i = r_i^F - r_i^F I(E_i, x_i) - (r^f + \alpha_i(r^e - f_i))(1 - I(E_i, x_i)$ and the return on wealth will then be

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

hence 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. Gambacorta et al. 2017; 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 friction-less value.

Search ability and returns on safe assets. Sophistication eases individuals' access to information about the set of rates offered by financial intermediaries on investment products or charged on debt instruments. This can affect returns because individuals can search on a broder set of rates. Interestingly, being aware of a broader set of rates can induce heterogeneity in returns on 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 treasury 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. (forthcoming) 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.[26] 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 (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 educa-

[26] Fagereng et al. (forthcoming) document systematic differences in rates on deposits across Norwegian banks. The website bankrate.com provides some indirect evidence about the importance of local market power among US banks. Comparing and homogeneous financial product - a 12-month, $25,000 CD - banks offer systematically different rates in the same local US market. The financial institutions with the lowest rates (HSBC, Bank of America and Wells Fargo) have undoubtedly more market power than those at the top of the rate scale (typically, online banks).

tion, ability and experience. Because the investor will choose the minimum rate he is aware of, we can set $d_i = k_i = r^f - r^{min} \times h(E_i, a_i, x_i)$. And the return on wealth will be[27]

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

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

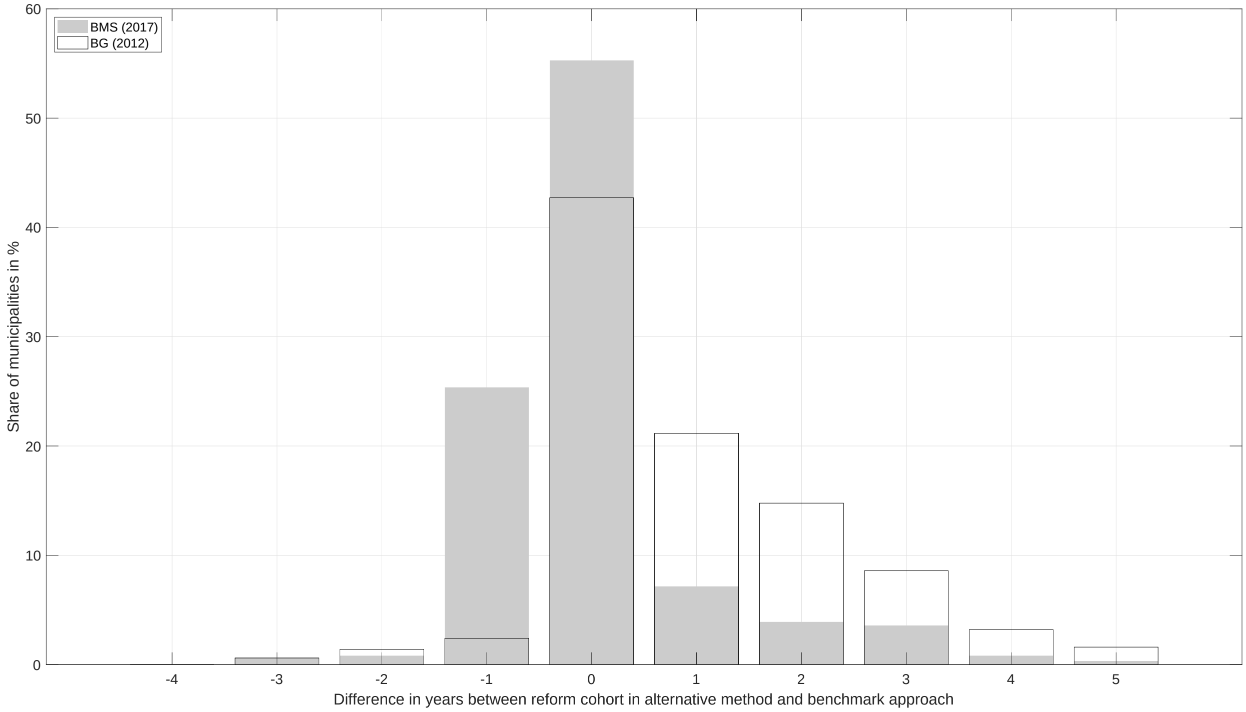
In the body of the paper, we rely on the municipalities where we directly observe the reform cohort in Ness (1971). There are two alternative approaches in the literature. First, Bhuller, Mogstad and Salvanes (2017) supplement the information from that source and are able to obtain information on reform 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 of (s)he was still in the compulsory schooling system when the reform was implemented. Formally, we define an individual as treated if (s)he belongs to a cohort that was less than or equal to 13 or 14 years in the reform year, where it is 13 if the compulsory system was 6 years of schooling prior to the reform and 14 if it was 7. 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 identifiy 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 ± 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 wealth 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 specications. 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

[27] Fagereng et al. (forthcoming) 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.

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.

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.

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

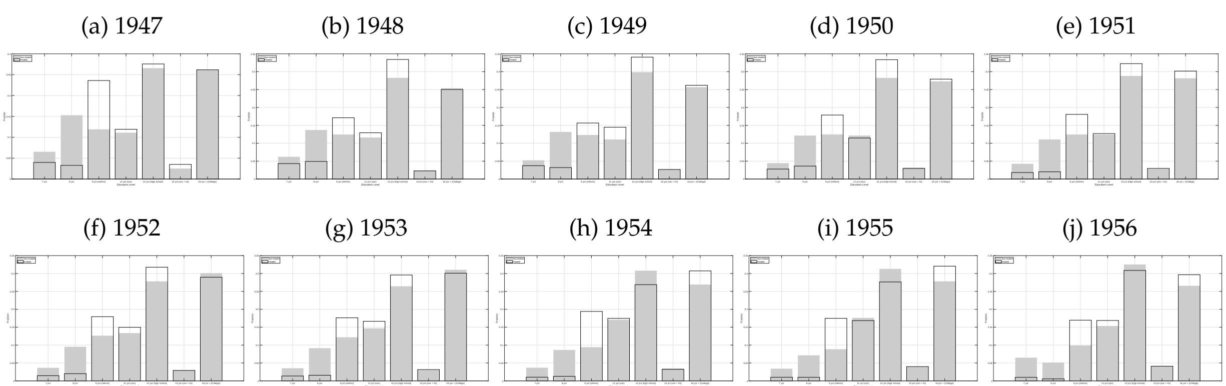
	Baseline:			*Instrument from BMS (2017):*			*Instrument from BG (2012):*		
	(1)	**(2)**	**(3)**	**(1)**	**(2)**	**(3)**	**(1)**	**(2)**	**(3)**
	Male	**Female**	**Pooled**	**Male**	**Female**	**Pooled**	**Male**	**Female**	**Pooled**
Years of education	0.041	-0.017	-0.021	0.111	-0.097	-0.062	0.3984	-0.233	-0.003
	(0.481)	(0.361)	(0.297)	(0.472)	(0.334)	(0.287)	(0.574)	(0.444)	(0.352)
First-stage *F*-test	22.14	29.41	49.97	22.51	35.07	52.51	16.29	21.00	36.90
Observations	622,915	815,467	1,445,382	519,707	810,948	1,330,655	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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. 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. Table B.2 shows results for the l-returns to education for the same sample as in our paper. Table B.3 and B.4 show results for the effects of education on asset and debt categories. Table B.5, B.6, and B.7 present results for net worth, asset classes, and liabilities when we estimate using reduced-form regressions. Figure B.2 presents education levels by cohorts in our sample.

Figure B.2: Education histogram by cohorts



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: The effects of education on *l*-returns, same sample as in returns to wealth regressions

A. Full sample:

	Male		Female		Pooled	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	OLS	**IV**	**OLS**	**IV**	**OLS**	**IV**
Years of education	0.081	0.017	0.083	0.019	0.081	0.018
	(0.000)	(0.026)	(0.000)	(0.020)	(0.000)	(0.017)
First-stage *F*-test		67.33		108.448		169.37
Observations	3.477.296	3.177.285	3,295,903	3,011,329	6,773,199	6,188,614

B. Twins sample:

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	OLS	**Twins FE**	**OLS**	**Twins FE**	**OLS**	**Twins FE**
Years of education	0.083	0.066	0.084	0.078	0.083	0.072
	(0.003)	(0.005)	(0.003)	(0.005)	(0.002)	(0.004)
Observations	41,857	41,857	41,171	41,171	83,028	83,028

Notes: The table shows regressions of the log earnings on years of education estimated on a balanced sample of individuals belonging to the reform cohorts in the time period between 2005 and 2015. Treatment is a dummy=1 if the individual was affected by the reform; zero otherwise. The IV regression uses as instrument the treatment dummy. All regressions in Panel A include time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. All regressions in Panel B include time fixed effects and a fourth-order polynomial in age. Robust standard errors are clustered at the individual level and reported in brackets.

Table B.3: The effects of education on returns to assets, IV

	(1) Male	(2) Female	(3) Pooled
A. Returns to gross wealth			
Years of education	-0.313	-0.067	-0.180
	(0.227)	(0.172)	(0.139)
First-stage *F*-test	21.66	29.33	49.45
Observations	627,918	810,039	1,437,957
B. Returns to real wealth			
Years of education	-0.275	-0.018	-0.131
	(0.253)	(0.195)	(0.157)
First-stage *F*-test	17.89	26.27	42.59
Observations	557,384	740,350	1,297,734
C. Returns to financial wealth			
Years of education	-0.060	-0.067	-0.063
	(0.093)	(0.078)	(0.061)
First-stage *F*-test	22.04	27.35	47.67
Observations	631,336	816,639	1,447,975

Notes: The table shows IV regressions of (scale adjusted) returns to the assets components of 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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. 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.4: The effects of education on interest rate on debt: IV

	(1) Male	(2) Female	(3) Pooled
A. Interest on total debt			
Years of education	-0.331	-0.043	-0.168
	(0.172)	(0.147)	(0.112)
First-stage *F*-test	20.03	21.46	40.44
Observations	520,374	679,553	1,199,927
B. Interest on mortgages			
Years of education	-0.275	-0.018	-0.133
	(0.156)	(0.136)	(0.103)
First-stage *F*-test	19.91	21.18	40.14
Observations	517,174	678,233	1,195,407
C. Interest on consumption loans			
Years of education	-0.418	-0.381	-0.429
	(0.776)	(0.900)	(0.628)
First-stage *F*-test	8.41	6.86	13.69
Observations	135,989	159,880	295,869

Notes: The table shows 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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. 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 education on returns to net worth: reduced-form

	Returns on net worth		
	(1) Male	(2) Female	(3) Pooled
Treatment	0.008	-0.001	-0.003
	(0.110)	(0.084)	(0.067)
Observations	640,915	825,544	1,465,459

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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. 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.6: The effects of education on returns to assets: reduced-form

	(1) Male	(2) Female	(3) Pooled
A. Returns on gross wealth			
Treatment	-0.059	-0.009	-0.032
	(0.048)	(0.039)	(0.031)
Observations	638,893	819,089	1,457,982
B. Returns on real wealth			
Treatment	-0.055	-0.003	-0.026
	(0.053)	(0.044)	(0.034)
Observations	561,111	743,640	1,304,751
C. Returns on financial wealth			
Treatment	-0.014	-0.013	-0.013
	(0.021)	(0.017)	(0.013)
Observations	642,327	825,707	1,468,034

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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. 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 interest on debt: reduced-form

	(1) **Male**	(2) **Female**	(3) **Pooled**
A. Rate on total debt			
Treatment	-0.081	-0.005	-0.035
	(0.039)	(0.031)	(0.024)
Observations	523,108	682,140	1,205,248
B. Rate on mortgages			
Treatment	-0.068	-0.002	-0.029
	(0.035)	(0.029)	(0.022)
Observations	519,874	680,805	1,200,679
C. Rate on consumption loans			
Treatment	-0.112	-0.062	-0.085
	(0.190)	(0.174)	(0.129)
Observations	136,552	160,412	296,964

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 time fixed effects, a full set of municipality dummies for where parents where located in 1960, a fourth-order polynomial in age, and individual cohort dummies. 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.