

Does education improve financial behaviors? Quasi-experimental evidence from Britain*

DANIEL GRAY†
*Department of Economics,
University of Sheffield,
S1 4DT, UK
d.j.gray@sheffield.ac.uk
+44 (0)1142229653*

ALBERTO MONTAGNOLI
*Department of Economics
University of Sheffield,
S1 4DT, UK
a.montagnoli@sheffield.ac.uk*

MIRKO MORO
*Division of Economics
University of Stirling
FK9 4LA, UK
mirko.moro@stirling.ac.uk*

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Abstract

This paper uses a range of exogenous schooling reforms in the UK to explore the relationship between education and a range of financial behaviours. Initially, we exploit two compulsory schooling reforms in Britain (1947 and 1972) and employ a regression discontinuity design to analyse nationally representative data. We find limited evidence that one extra year of schooling led to systematically different financial behaviours. One exception is the promotion of more positive saving behaviours amongst females affected by the 1947 reform. We then go on to explore a large expansion of the higher education sector in the UK, which occurred during the 1980's and 1990's, and confirm that general education does not appear to affect financial behaviours systematically. We argue that, despite clear positive spill-overs of educational reforms, desirable financial behaviours require specific and targeted education policies and we point to the growing research in this field to support this conclusion.

Keywords: Compulsory Schooling Laws; Education Expansion; Financial Behaviours; Regression Discontinuity; Saving Decisions.

JEL classification: D14; I20; G11.

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1 Introduction

This study uses quasi-experimental research designs to investigate the causal effect of education on an individual's financial behaviours, including both saving and borrowing decisions. In the presence of increasingly complex financial markets and products, the ability of consumers to make informed financial decisions is critical to developing sound personal finance, which can contribute to increased saving rates, more efficient allocation of financial resources and greater financial stability. In recent decades, managing credit positions has become more complex; for instance, the Center for Retirement Research at Boston College estimated that the share of USA workers at risk of having insufficient funds to maintain their standard of living during retirement is estimated to have increased from 31% to 53% from 1983 to 2010 (Munnell et al., 2012; Benartzi and Thaler, 2013). In addition, recent changes to pension rules in the UK have allowed more flexibility in how pension funds can be used before the retirement age; this policy has been subjected to criticisms as it might lead to suboptimal decisions by a proportion of the population because of behavioural and cognitive biases.¹ Moreover, the recent financial crisis, which exposed the financial vulnerability of many individuals and households, has generated interest from both researchers and policy makers and, as a result, understanding how to promote more responsible and prudent saving and borrowing behaviours is of increased importance.

Starting from this premise, this paper investigates whether general education policies play a crucial role in improving financial decision making. Education potentially provides agents with the necessary skills to improve how they process information and ultimately make decisions in a variety of fields, including financial behaviours. There is ample evidence of a strong correlation between education and a number of desirable financial behaviours, such as household saving, retirement planning, financial market participation, asset allocation and managing credit positions (see, for example, Haliassos and Bertaut, 1995; Rosen and Wu, 2004; Van Rooij et al., 2011; Browning and Lusardi, 1996; Campbell, 2006; Gross and Souleles, 2002; and Brown and Taylor, 2008 amongst many others).

However, the existence of a simple positive correlation between education and desirable financial behaviours is of little interest from a policy perspective, as both the amount of education and any financial outcome may be endogenously determined. Hence, developing causal estimates is relevant to inform and shape education policies and ultimately to promote financial stability. If educational reforms, such as increases in the school leaving age or an expansion of the higher education (HE) sector, improve financial outcomes at the individual level, then this positive spill-over should be taken into account in a cost-benefit

¹Association of British Insurers, 2015.

analysis of education policies. Existing evidence on the causality is rather limited; Cole et al. (2014) provides evidence of a causal relationship between the amount of general education and financial outcomes (such as investment income, equities ownership, the probability of bankruptcy, foreclosure and loan delinquency) using instrumental variable strategies to exploit the variation in compulsory schooling across states within the USA. Other studies looking at the USA educational system include Cole et al. (2016) and Brown et al. (2016), who explore the effects of personal finance and mathematics courses on financial outcomes and the effect of financial education on debt behaviours, respectively.

This paper draws on rich longitudinal survey data from the UK which includes information on a wide range of financial behaviours, including saving behaviours (whether an individual saves; whether they are a regular saver; and the monthly amount saved) and debt behaviours (whether individuals hold unsecured debt; the amount of unsecured debt held; and the level of secured debt held). We analyse this data in two distinct ways. In the first part of the study we make use of a regression discontinuity design (RDD) to investigate the causal effect of education on a set of financial decisions. We exploit the exogenous variation in the amount of education from two compulsory schooling laws. The first reform implemented in 1947, known as the Butler Act, increased the minimum schooling-leaving age from 14 to 15 in England and Wales – affecting cohorts born from April 1933 – and the second reform, enforced in 1972, increased the school leaving age from 15 to 16 in the UK – affecting cohorts born from September 1957 onwards. In the second part of the paper we take advantage of a large expansion in the HE sector in the UK, that is, post compulsory education, which occurred in the 1980’s and 1990’s. This reform had a dramatic impact on participation in higher education; participation rates increased from 15% to 33% between 1988 and 1994 (Devereux and Fan, 2011). We exploit this variation in a two-stage instrumental variables approach. Together these reforms capture exogenous changes of different levels of education, that we exploit to ascertain the causal impact of general education on a variety of financial behaviours.

We improve upon the existing literature relating the impact of education and financial outcomes in a number of ways. Firstly, the reforms we consider relating to changes in the level of compulsory education affected a significant proportion of relevant cohorts, much more so than compulsory schooling reforms in the USA. The first reform affected 50% of the relevant birth cohorts while the second affected 25%; this compares with a 5% of the population affected by raising of the minimum schooling age in USA (Lleras-Muney, 2005).² These reforms were implemented nationwide at a single point in time, representing a natural experiment that increased the amount of schooling of a large portion of the population who would have otherwise left school (Wilson, 2014). The importance

²We will demonstrate that there is a large and statistically significant difference between cohorts affected and cohorts unaffected when it comes to the amount of schooling.

of these laws documented in the literature enables us to estimate effects that are closer to the population-average instead of the sub-population sampled, that is, more technically, the Local Average Treatment Effect (LATE) is closer to the Average Treatment Effect (ATE) in our case (Clark and Royer, 2013).

Second, the reforms relating to compulsory schooling and the expansion of the HE sector are important because they have been proven to affect other outcomes. For example, changes to compulsory schooling levels have impacted on earnings (Devereux and Hart, 2010), cognitive abilities (Banks and Mazzone, 2012), risky behaviours (Wilson, 2014) and health outcomes (Jürges et al., 2013). Whilst the HE expansion has been found to influence earnings (Devereux and Fan, 2011) and health outcomes (James, 2015). Thus it is straightforward to assume that higher levels of education as a result of these educational reforms might have had an impact on financial behaviours either directly or indirectly.

Third, when considering the raising of the school leaving age, we employ a RDD framework to examine the effect of compulsory schooling, which requires weaker identifying assumptions than a global instrumental variable (IV) approach and therefore offers a more plausible framework to establish causality in this setting (DiNardo and Lee, 2011). RDD's simply require that individual characteristics, including financial behaviour, would have been unaffected around a date that has been set by the Government as a cut-off for a reform many years later. In other words, in the absence of such reform, one would reasonably expect financial outcomes to be continuous around the cut-off (Hahn et al., 2001; Skovron and Titiunik, 2015). Importantly, the use of two reforms is a contribution in itself, in that it enables the study of different cohorts at different points in time to capture potential non-linear effects.

Fourth, this paper exploits different compulsory schooling reforms together with the exogenous expansion of higher education on the same set of outcomes. It is plausible that these margins may have different effects on the ability to make sophisticated financial decisions, and so an important contribution of this study is to explicitly explore both contexts.

Our study highlights significant divergencies between simple regression models and the quasi-experimental estimation strategies. The results relating to simple regression estimations indicate that, in line with the existing literature, the level of education is highly correlated with individuals' financial decisions. For instance, the higher the educational attainment, the higher the individual's propensity to save or be a regular saver and to have a higher investment income. These results are robust to the inclusion of a very rich set of individual characteristics. When looking at the causal effects of compulsory schooling reforms and higher education expansion, we find sparse evidence of a systematic difference in most of the financial outcomes available. The results point to a statistically significant effect of the 1947 reform on the savings behaviour of females. Females that

were born just after the cut-off date are more likely to save and to be regular savers and has a positive impact on the monthly amount saved. These findings do not hold for the later reform relating to compulsory schooling or the HE expansion.

Finally, the results point in the direction of the presence of significant omitted variables - such as ability, family background and time preferences, which may have been overlooked in the existing literature on education and financial behaviours. Education policy is certainly generating positive spill-overs in many areas of our society, nevertheless we argue that desirable financial behaviours require specific targeted education interventions to improve numeracy skills. This is in line with the conclusions drawn by Cole et al. (2016) and Brown et al. (2016) when looking at the USA education provision.

The remainder of the paper is as follows; Section 2 elaborates on the relationship between education and financial behaviour whilst Section 3 describes the data used in the analysis. Section 4 provides the analysis relating to the compulsory schooling reforms whilst Section 5 considers the higher education expansion. The results of these exogenous changes in the level of education are discussed in Section 6 and, finally, Section 7 concludes.

2 Education and Financial Outcomes

There is growing evidence that compulsory schooling laws improve several labour and non-labour market outcomes via different mechanisms.³ For instance, more educated people are more likely to earn more (Devereux and Hart, 2010), be happier (Oreopoulos, 2007) and to participate in democratic elections (Milligan et al., 2004). At the same time they are less likely to become unemployed (Card, 1999) or engage in risky behaviours such as teenage pregnancy (Black et al., 2008; Wilson, 2014) and crime (Berthelon and Kruger, 2011; Jacob and Lefgren, 2003; Lochner and Moretti, 2004). Moreover, there is evidence that increased educational attainment, brought about by an expansion to the higher education sector, also had desirable impacts on a range of outcomes, including earnings (Devereux and Fan, 2011), health outcomes (James, 2015) and crime (Machin et al., 2012).

There is a growing literature relating to education and broader financial reforms. For example, Black et al. (2018) explore the long-term effects of additional primary education on risk taking in financial markets. Analysing administrative data from Sweden, the authors exploit compulsory schooling reforms to explore the causal effect of education on stock market participation and the proportion of financial wealth allocated to these assets. The findings indicate that, for males, higher levels of education increased the likelihood of stock market participation and also resulted in a larger share of wealth being allocated

³For a recent review see Oreopoulos and Salvanes (2011).

to risky assets. There is limited evidence that the reform impacted female financial behaviours. In a related study, Girshina (2019) explores the effect of education on wealth accumulation in Sweden, where the author uses a range of identification strategies to ascertain causality. The results indicate that education has a positive, large, and long-lasting effect on net worth. In contrast, Bingley et al. (2017) report that reforms to secondary schooling in Denmark in 1950's resulted in reduced wealth for males in their 50's, however it was associated with increased pension annuity claims. The authors argue that the reforms to schooling increased job mobility, reduced housing equity and increased leverage of households and improving occupational pension benefits. In a similar spirit Cole et al. (2016) utilize a two-sample instrumental variables strategy which combines Census data for almost 15 million individuals and the Federal Reserve Bank of New York Consumer Credit Panel/Equifax dataset to look at the effect of exogenous variation in state compulsory schooling laws. They show that treated individuals change their saving and investment behaviour, leading to a higher investment income and equities ownership, and a lower probability of bankruptcy.

Following Oreopoulos and Salvanes (2011) and McPeck (1985) we can expect education to have both direct and indirect effects with respect to financial behaviors. Education provides direct knowledge-based skills necessary to make better financial decisions, such as mathematical skills necessary to choose between different mortgage plans (Cole et al., 2016); education might also have some specific personal finance content. In the USA, several states offer this type of education at high school level. The existing evidence is mixed with early studies showing financial courses improving savings (Bernheim et al., 2001), while more recent work reporting no effect on financial behaviour (Cole et al., 2016). In a related study, Carpena et al. (2017) implemented a large scale field experiment in India to explore the link between financial education and financial outcomes. The results indicate that financial education does not lead to improved financial well-being, but does however raise participant's knowledge and attitudes towards financial products. Banks et al. (2019) exploit the 1972 increase in the school leaving age in the UK to explore the effect of education on hypothetical economic decision making in a survey experiment. In the context of incentivized risk choices, the authors find that the policy has no impact on economic decision making ability.

Schooling also potentially improves the way individuals process new information, making complex tasks easier or providing the right tools to make better informed decisions, for example, critical thinking skills. Education has been found to improve intelligence test scores in the USA (Cascio and Lewis, 2006), Norway (Black et al., 2011) and Sweden (Carlsson et al., 2015) and, more generally, cognitive abilities, including, for example: problem solving; planning; abstract thinking; and learning from experience (Hanushek and Woessmann, 2008; Banks and Mazzonna, 2012).

Furthermore, schooling may allow the individual to acquire social skills which may improve financial positions; individuals learn to relate and compare to others and learn to distinguish between acceptable and unacceptable (“reckless”) behaviours, including financial ones. Evidently, these skills acquired at school are useful to achieve other important labour and non-labour market outcomes, which in turn may affect financial positions. For example, higher incomes from education might affect savings (Cole et al., 2014).

Alongside these effects, schooling could have important effects on financial decisions by exerting a change on an individual’s preferences or beliefs. Firstly, schooling focuses one’s attention on the future, lowering discount rates, thereby creating more patient individuals (Becker and Mulligan, 1997). In addition to this, education may attenuate myopic decisions by reducing hyperbolic discounting behaviours. Moreover, schooling may increase self-efficacy, the beliefs in one’s own ability to make successful decisions, including financial ones (Shockey and Seiling, 2004; Lusardi et al., 2014). Finally, Banks and Mazzonna (2012) conjecture that schooling could increase “*the utility derived from more cognitive demanding activities and consumption*” (p. 422), which, we add, may include financial decisions. These explanations are all grounded in human capital theory. The alternative view within economics is that schooling is just a screening mechanism to select more productive people and provide a signal to the job market. Signaling theory would be consistent with better financial outcomes because higher qualified individuals have more cognitively demanding jobs, which in turn help them to make better decisions in financial markets.

The prediction arising from these potential mechanisms is that more educated people are more likely to make sound and rational financial decisions. For instance, more educated individuals would allocate part of their income to saving for retirement or saving for long term objectives, for example, children’s education, rather than overemphasizing present consumption. A variety of studies have shown that a lack of educational attainment is indeed strongly associated with financial mistakes (Campbell, 2006; Calvet et al., 2007).

An alternative explanation is that preferences could shape the amount of education as well as being shaped by it. In sum, if the level of education is endogenous; then the relationships outlined above may be plagued with omitted variable bias. For the purposes of illustration, consider two individuals, L and H , who are identical in everything with the exception of time preferences. Individual L has a very low discount rate (i.e., she is forward-looking) while individual H has a high discount rate (i.e., she is very impatient). This difference in discount rates predicts different schooling choices. It is likely that the more forward-looking individual, L , would choose to study longer than individual H . The observed difference in the education level between individuals L and H in any given data set can be explained by differences in unobserved time preferences.

It is straightforward to see that time preferences may be linked to the financial decisions undertaken by individuals. More forward-looking individuals are more likely to save more. One can imagine other unobserved factors - some innate ability to process analytically or family background - to be also simultaneously correlated with education and financial decisions. It follows that any OLS estimate is prone to omitted variable bias. This example of individuals L and H also suggests the direction of this bias. The omitted factors are more likely to be driving both savings and education in the same direction, so we can expect OLS to produce upward biased estimates.

In an effort to control for such omitted variable bias, this paper proposes the use of exogenous changes to the level of education, namely, changes to compulsory schooling and an expansion of the HE sector. These reforms increased exogenously the amount of education received by some individuals within a population who were born after a specific cut-off date, but left unchanged the amount of education of observationally similar individuals that were born just before the specified date. The empirical strategy relating to each reform is outlined in subsequent sections.

3 Data

This paper draws on a sample of respondents based in the UK. This sample is taken from two large nationally representative household panel surveys of UK households covering the period 1991-2017; these are namely Understanding Society - The UK Household Longitudinal Study (UKHLS) and, its predecessor, the British Household Panel Survey (BHPS). Conducted by the Institute for Social and Economic Research, the BHPS is a nationally representative longitudinal survey of households in Great Britain, where households are interviewed annually. The first wave, conducted in 1991, contained a sample of approximately 5,500 households, corresponding to roughly 10,300 adults. The sample size of the BHPS was increased in 1999 when an additional 1,500 households from Scotland and Wales were included and similarly, in 2001, a further 2,000 households from Northern Ireland were added. In addition, a special wealth module, included in the 1995, 2000 and 2005 waves of the survey, contain a range of information on a variety of assets and debts. This will allow us to explore a range of financial outcomes, including saving decisions in addition to levels of assets and debts. It is important to stress that some financial outcomes are measured at the individual level, such as; saving, regular saver, amount saved, and unsecured debt, whilst other variables, such as investment income and secured debt are measured at the household level.⁴ Table 1 provides a list of the variables used and the respective questions asked to the individual or the household.

⁴We subsequently return to this point when we discuss the definition of treated households.

The first wave of the UKHLS survey builds on the BHPS and was initiated as the BHPS was phased out in 2008. We use data from across 8 waves in the current paper. The UKHLS surveys 40,000 households and includes the existing sample of BHPS households from wave 2 onwards. The UKHLS contains information on a wide variety of demographic and socio-economic characteristics. In addition, information on a variety of financial behaviours is included. Specifically, in waves 2, 4, 6 and 8 of the UKHLS, the survey includes information relating to saving behaviour, including whether the individual saves, whether they save on a regular basis and the amount saved in the last month. In addition, we consider the value of income from any investments including dividends and interest income. These variables are consistent across both the BHPS and the Understanding Society and consequently we analyse data pooled across both data sets.

Furthermore, we consider the liabilities side of the household’s balance sheet, by analysing both whether unsecured debt is held and the amount of unsecured debt held, in addition, to the outstanding level of mortgage debt. Waves 4 and 8 of the UKHLS, contained in the wealth and asset module asks: *“I would now like to ask you about any other financial commitments you may have apart from mortgages. For which, if any, of these items do you currently owe any money?”* The question clearly relates to non-mortgage debt. If the respondent reports having any of these debts, they are asked how much they owe. Information on secured mortgage debt is contained in waves 1, 5, 6, 7 and 8 of the UKHLS, where respondents are asked *“Could I just check, approximately how much is the total amount secured against this property, including your mortgage and any other loans secured on the property?”*⁵ In addition to the absolute level of the continuous variables, we also consider these values relative to households income. These ratios arguable capture, for example, whether the debt accumulated by the household is at a sustainable or an excessive level. Once again these variables are combined with information from the BHPS and pooled data is considered in the subsequent analysis.

Crucially from an RDD perspective, and our identification strategy, these surveys include month and year of birth, which allow us to identify precisely those exposed to the educational reforms. The schooling variable included in the surveys is “school leaving age” and “age left further education”, which allows us to construct an education leaving age which is equivalent to completed years of education (Clark and Royer, 2013). The list of financial behaviour variables and the labels used in the paper - together with a brief description - can be found in Table 1.

⁵We purposely leave out any questions related to the value of the house. It would be difficult to extrapolate the effect of education given that there is a house price effect that the individual cannot control. Migration could be a case where individuals as a group could influence the price behaviour, but this is beyond the aim of this paper.

3.1 Correlations

Prior to the discussion of the results and in order to illustrate the association between each financial outcome and education, Figures 1 and 2 plot the correlation between the age an individual left education and various financial outcomes. The fitted lines are the predictions from regressions of each financial outcome on the age at which the individual left school controlling for the year of survey. These results are robust to the inclusion of numerous individual characteristics (such as gender, age, age-squared, log of household income, employment status, household size and self-assessed health status).⁶ These estimations assume that, conditional on a rich set of demographic and socio-economic characteristics, the coefficient on the education variable measures the effect of schooling. Despite these strong correlations, this does not imply a causal relationship between education and financial outcomes. Consequently, in the subsequent analysis we aim to establish the causal impact of education on a range of financial outcomes. In line with prior expectations, there exists a strong positive correlation between education level and the respective financial measures. For instance in Panel (b) of Figure 1, an individual that left school at the age of 18 will save on average approximately twice as much as an individual that left school at 15.⁷

Moreover it is important to explore whether these associations are present in individuals who achieved lower levels of education, that is, amongst the individuals who were impacted by changes in compulsory schooling. Panel A of Tables A1, A2 and A3 present the association between a range of financial variables and the age left education for the whole sample, whilst Panel B relates to individuals who report leaving school at the age of 16 years old or less.⁸ It is evident that similar associations are observed amongst the full sample and individual who left school aged 16 and below, highlighting that even for those with relatively low levels of education, an additional year of education has a meaningful and desirable impact on a range of financial outcomes. For example, Panel B of Table A1 indicates that an additional year of schooling increase the probability of saving by 2.2 and 3.7 percentage points for females and males respectively, whilst Panel B of Table A2 indicates an additional year of schooling is associated with an increase in the amount saved by 11.1% and 20.4% for female and males, respectively. Table A3 suggests that the level of education has positive association with a range of financial variables relative to

⁶Estimates are reported in Appendix A.

⁷These are binned scatter plots providing a non-parametric visualisation of the relationship between each financial outcome and age left school over the whole period considered in our study, 1991-2017. Each plot results from partitioned regressions between two variables while controlling for year of the survey. A linear fit is then estimated and plotted on top of the scatter points. These graphs were obtained in Stata using the `–binscatter–` command.

⁸We also obtain similar associations when we restrict the sample to include those individuals who left school aged 15 and below, further suggesting that additional schooling at this low level of attainment is correlated with a range of financial outcomes.

household income.

4 Compulsory Schooling and Financial Behaviours

4.1 Background on Compulsory School Laws

Initially, this paper utilises two reforms that raised the minimum school leaving age by an additional year. The attractiveness of these reforms lies in the fact that they offer a clean identification of an extra year of schooling on financial outcomes of similar individuals born just before or after the exogenously determined cut-offs dates. It is worth noting that they changed the amount of schooling by modifying the leaving age but left unaffected school entry and exit rules (Banks and Mazzonna, 2012). Summary statistics of the dependent variables, in addition to the school leaving age and, for reference, a range of demographic and socio-economic characteristics which have previously been shown to influence financial behaviours, for the sample analysed for the compulsory schooling reforms, are presented in Table 2.

The first reform was included in the 1944 Education Act – popularised as the Butler Act – and came into force in 1947. It increased the minimum school leaving age from 14 to 15 years old for individuals born from 1st April 1933 for individuals in England and Wales. In order to avoid confusion, we will refer to this as the ‘1947 reform’ (using the year in which it was implemented). This reform aimed to increase physical and mental adaptability of children and was targeted at lower educated groups (Devereux and Hart, 2010). The 1947 educational reform had far reaching impacts, decreasing by approximately 50% points the proportion of individuals who left education prior to the age of 15 years old. Importantly, this reform allows for the identification of the extra year of schooling separately from qualification attainment as formal school qualifications could not be obtained at this age. This has the consequence of distinguishing between a human capital effect and a signaling effect. One additional year of education arguably does not represent a very strong signal. The success of this reform has been documented by a large literature which has shown that the impact of the increase in schooling has been sizeable and that the extra year systematically changed labour and non-labour outcomes of the affected cohort, while leaving unchanged the unaffected individuals born immediately before the cut-off date. Recent papers using a similar strategy show that these effects are weaker than initially documented. For example, Devereux and Hart (2010) show that the reform did not improve the wages for females while Clark and Royer (2013) show that health outcomes did not significantly differ between “treated” and “untreated” cohorts.

The Butler Act made provision for a further increase in the minimum school leaving age up to 16 but this was not enforced until September 1972, thus affecting cohorts in the

UK born from 1st September 1957. In line with the above, we will refer to this as the ‘1972 reform’, however it is often referred to as the Raising of the School Leaving Age (RoSLA). This delay was as a consequence of shortages in capital and labour in the post-war period. However, following the Crowther Report (1959) there was a move towards increasing the school leaving age, by a further year, to 16 years old (Wilson, 2014). This increase in the school leaving age was part of a more comprehensive educational reform including a revised curriculum, increased scale of teacher-training provision, in addition to increased school building to increase school capacity in order to accommodate the increased number of students. It is documented in different studies that this second reform had less “bite” than the first one, that is, it affected a smaller fraction of the population.⁹ The 1972 reform impacted on individuals in the lower levels of the education distribution and did not influence the propensity of individuals to continue beyond the compulsory leaving age.

Figure 3 shows the proportion of individuals who left full time education by the ages of 14, 15 and 16 by month of birth using a 3 month moving average. The vertical lines indicate cutoffs corresponding to the first cohorts subject to the two compulsory schooling laws (1st April 1933 for the 1947 reform and 1st September 1957 for the 1972 reform). There is a clear declining trend in the proportion of individuals leaving education before the age of 16 as documented in previous papers (see, for example, Devereux and Hart, 2010). These reforms substantially increase the amount of schooling received by individuals born after the cut-off date in both reforms, albeit the effect is stronger for the first reform, which affected children 14 years old, than for the second one, which affected individuals aged 15 years old. The line corresponding to 16 year olds serves as a valid comparison: their amount of schooling has been unaffected by these reforms. This result is consistent with previous studies, for example, Chevalier et al. (2004) and Dickson and Smith (2011), in that the educational reforms did not stimulate an increased proportion of individuals to study beyond the compulsory school leaving age.

One of the most important contributions of our paper is the use of two reforms separately to identify the treatment effect. There are two important advantages of using this setting; firstly, the use of both reforms explores the robustness of our results across different cohorts, and secondly, this method might potentially detect the presence of non-linearities in the relationship between education and financial behaviour.

4.2 Empirical Strategy

The compulsory schooling reforms affected the amount of education of cohorts that were born just a few months apart. The nature of these reforms, together with “incomplete

⁹See for example, Chevalier et al. (2004) and Dickson and Smith (2011).

compliance”, allows for an estimation of the causal effects of schooling on a rich set of financial outcomes using a “fuzzy” RDD. Within this framework, identification of the causal effect of schooling on financial outcomes requires relatively weak assumptions.

Changes in compulsory schooling laws imply that assignment to the treatment (additional schooling) is determined exogenously by the date of birth of each individual. Thus, individuals will be either treated and receive an extra year of schooling if they are born from April 1933 or September 1957 onwards, the cut-off dates for the 1947 and 1972 reforms, respectively. In other words, the RDD models the probability of receiving a treatment as a discontinuous function of a continuous treatment variable, which in our case is the date of birth. The empirical specification will then compare individuals who are born immediately prior and post the cut-off date with the identifying assumption that these individuals are similar in both their observed and unobserved characteristics with the exception that they were born few months apart, and therefore would have behaved similarly with respect to financial decisions in the absence of the reforms. This assumption ensures that individuals unaffected by the reform represent a valid counterfactual and that the reform is “as good as randomly assigned” with respect to date of birth near the discontinuity point.

The treatment in our case does not change from 0 to 1 at the cut-off date, that is, we are under a situation of “incomplete compliance”. On one hand, some individuals would have attended the extra year of schooling regardless of the reform, i.e., “always-takers” (Angrist and Pischke, 2009). On the other hand, although these reforms affected a large number of people, a number of individuals left school before the minimum leaving age, i.e., “never-takers”. Consequently, the probability of receiving the treatment does not jump discontinuously from 0 to 1 at the cut-off date, but the change in the probability is somewhat smaller (Lee and Lemieux, 2010). This is clearly evident from Figure 3.

Formally, let τ denote the causal effect of education on a financial outcome Y ; for small $\epsilon > 0$ a formal representation of the fuzzy RDD can be written as follows:

$$\tau = \frac{\lim_{\epsilon \rightarrow 0} E(Y|X = c + \epsilon) - E(Y|X = c - \epsilon)}{\lim_{\epsilon \rightarrow 0} E(S|X = c + \epsilon) - E(S|X = c - \epsilon)} \quad (1)$$

where X is the month of birth of each sampled individual, i.e., the assignment variable, S is the treatment, i.e., one extra year of schooling, and c is the cut-off date of birth, i.e., April 1933 or September 1957. The treatment effect τ is recovered by dividing the jump in the relationship between the financial outcome Y and birth cohort X around the cut-off date c by the proportion of individuals induced to take-up the treatment S at the cut-off date.

Our analysis does not include all individuals born around these cut-off dates. Individuals live in households with typically two or more members, some of whom might be born

on either side of the cut-off date. In our setting, the inherent problem we have is that individuals are exposed to additional years of education, whilst many of the outcomes we consider are measured at the household level; that is the treatment is at a different level to the outcomes of interest. It is important to acknowledge that, even variables measured at the individual level are likely to be joint decisions of/influenced by other members of the household. Therefore it is important to carefully define the treated units in order to isolate the impact of education on these outcomes and to control for potential spillover effects. We define individuals as being “treated” if all members of a household are born after the cut-off date and we define individuals as being “untreated” if all household members are born before the cut-off date. That is, in order to try and control for potential spillover effects, we exclude households in which at least one member received more education while others did not.¹⁰ This is made necessary to avoid cross-contamination and potential spill-over effects. It is indeed plausible that an individual’s financial behaviour is affected by the spouse living in the same household. As a robustness check we run our RDD specification using only the household reference person (sometimes referred to as head of the household within surveys) and yield quantitatively and qualitatively similar results to those reported hereafter.¹¹

A key consideration when implementing RDD analysis is the choice of window to consider around the discontinuity. In the existing literature there is much debate surrounding which optimal bandwidth to employ. In the choice of bandwidth there is a trade off between statistical power and bias of the estimated coefficients. For example, estimating a small window around the discontinuity will yield an unbiased estimate of the local treatment effect, however, this will rely on a relatively small number of data points and therefore lack statistical precision. In contrast, a wide bandwidth around the discontinuity will include a larger number of observations, however, this will potentially introduce biases by considering observations far away from the discontinuity. We reduce the potential tradeoff between variance and bias by employing a local linear point estimator with an optimal data-driven bandwidth selection procedure developed in Calonico et al. (2014b) and Calonico et al. (2016b) (CCT henceforth). The bandwidth selected is the one that minimises an approximation to the asymptotic mean squared errors (MSE) of the RDD estimator, similarly to that proposed by Imbens and Kalyanaraman (2012). The CCT method produces smaller bandwidths than Imbens and Kalyanaraman (2012).

¹⁰We do this irrespective to whether the household is considered to be a couple or just individuals leaving together. Moreover, the we acknowledge that the potential positive spillovers between household members is an interesting area for future research, however we do not explicitly explore these in this study.

¹¹Analyses using only the household reference person born either before or after the reform provides similar results but are not reported for brevity. The household reference person is defined within each survey (and sometimes referred to as head of household) as the person legally or financially responsible for the accommodation or the elder of two people equally responsible.

In the subsequent analysis, for robustness purposes, we also present results based on larger bandwidths by following CCT optimal bandwidths without regularization as explained in Calonico et al. (2016b). The algorithm to select optimal bandwidths presented in Imbens and Kalyanaraman (2012) trades off variance with bias by using a formula in which, *inter alia*, the estimated variance in the data is divided by the (weighted) estimated bias. If the estimated bias is very close to zero, this would lead to very large (infinite) bandwidths. To avoid this, the CCT method adds a regularization term that ensures the denominator does not become too small. In practice in our case, we present results from small regularized bandwidths and from larger bandwidths that exclude regularization. In Tables we refer to these as CCT and CCT no reg, respectively.¹²

When estimating an RDD, the researcher faces a further decision with respect to the functional form, that is, the shape of the relationship between financial outcomes and age. Imposing a functional form via a regression model in order to explore the impact of a discontinuity on an outcome will only give an unbiased estimate if the functional form is correctly specified. We follow the literature and decide to choose a local linear function for two main reasons. First, the CCT bandwidth selection procedure is optimal, i.e., it reduces the trade-off between bias and variance, *given* the polynomial selected (see, for example, Calonico et al., 2014b and Skovron and Titiunik, 2015). Different polynomials will lead to different bandwidth sizes. Second, a polynomial of order one reduces the potential over-fitting problems associated with much of this literature and criticised by Gelman and Imbens (2014). Local linear regressions are weighted regressions, with weights based on Kernel functions. We estimate linear functions before and after the cut-off by means of triangular Kernel, with closer observations within the bandwidth receiving greater weights.¹³ We also employ cluster-robust standard errors at the month of birth level.

In the standard estimation of a local linear regressions only two variables are used, namely the outcome variable and a continuous running variable which assigns an individual to the treatment. However, in practice pre-intervention controls can be included in order to increase the precision and efficiency of the estimators (see for example, Lee and Lemieux (2010), Calonico et al. (2016b) and Frölich (2007) for a full explanation of the use of additional covariates in RDD models). Given individuals enter the data in different periods, their financial behaviour is observed at different age-year cells, we include age, age-squared, year of survey (as a linear trend) and month of birth.¹⁴

¹²In addition, Tables A4 and A5 present the estimates using fixed bandwidths, that is, 24 months, 36 months and 72 months. The results accord with those obtained using the CCT approach.

¹³For further discussion relating to the estimation of the non-parametric local linear regression, see Fan and Gijbels (1996).

¹⁴A full explanation of the covariate adjusted RD estimator is presented in Calonico et al. (2016b). These non-parametric models are estimated using the Stata package *rdrobust* by Calonico et al. (2014a) and Calonico et al. (2016a). Following Lee and Lemieux (2010), who argue that both the parametric and non-parametric RD approaches should be seen as complements as opposed to substitutes, we also

4.3 Identification Issues

The validity of our empirical strategy rests on two main assumptions. First, subjects should be randomly assigned to the treatment, that is, random assignment to the increase in the minimum school leaving age. Since this is completely determined by date of birth, we can assume that this condition is satisfied. The second condition is that nothing, other than schooling, changed discontinuously around the cut-off dates.¹⁵ We are unaware of any other interventions linked to financial decisions that might have changed in correspondence with those laws. Hence, we are confident that our strategy represent a good effort into estimating causal effects. The first reform introduced free universal secondary education with the opportunity given to everybody to access selective schools (for example, grammar schools) that might have interacted with the amount of schooling in many ways.¹⁶ We conjecture that the emphasis given to numeracy in selective schools, could have led to positive effects on financial decisions. There is no clear a priori assumption on how the second reform could have interacted with the additional year of schooling. It is worth pointing out that these factors would in general bias upward the estimated causal effect.¹⁷

4.4 Results

4.4.1 Compulsory Schooling and Education Leaving Age

We start by visually exploring the effects of both the 1947 and 1972 education reforms on the age individuals left school. The average years of schooling, by birth cohort and gender, are depicted in Figure 4 to Figure 7 where a linear fit is estimated without covariates and is shown in each graph. The figures clearly show jumps in the average level of schooling around both educational reforms, once again depicted by the vertical lines, for both males and females. The figures suggest a substantial drop in individuals leaving before the compulsory age from the cut-off date, increasing the average school leaving age in the post reform periods. These figures provide preliminary evidence of the exogeneity of both the 1947 and 1972 education reforms on the level of education received by individuals. These reforms will allow us to identify the causal effect of education on a

run parametric estimates using a linear regression with clustered standard errors at month of birth and three different bandwidths. Tables B1 and B2 present the results relating the parametric IV approach as opposed to the non-linear approach. Generally the results presented are in line with those discussed in the main text, that is, generally the results fail to have a statistically significant impact on the range of financial behaviours considered.

¹⁵We have also implemented a placebo test by moving the reforms forward and backwards in time by 24 month. As expected there are no systematic differences around these hypothetical reform dates.

¹⁶The abundant literature on the pecuniary and non-pecuniary effects of education seems to overlook this aspect.

¹⁷It is therefore less of an issue for us as our findings do not find any statistical difference between affected cohorts.

range of financial behaviours, if there are no other unobserved changes which might have influenced only the treated cohort.¹⁸

Table 3 reports estimates of a regression on the effect of the reform on the amount of schooling for different school leaving ages. The results suggest that both reforms had a positive impact on the school leaving age, more specifically, they suggest that the proportion of individuals completing less years of schooling than compulsory declined more for females, compared to males, for both the 1947 and 1972 reforms. These estimates are statistically significant at the 1% level and are quantitatively and qualitatively similar to Clark and Royer (2013); this gives us confidence that these reforms are a powerful instrument for our RDD settings.

It should be noted that, despite the fact the educational reforms increased the leaving age by one year, given that we observe incomplete compliance (fuzzy RDD), the observed increase in average school leaving age was approximately 0.6 and 0.5 years, female and males respectively, for the 1947 reform and between 0.13 and 0.17 years for the 1972 reform.

Considering columns two and three of Table 3 for the 1947 and 1972 reforms, respectively, indicates that there is not a statistically significant impact on higher levels of educational attainment. This is taken as evidence that these reforms successfully forced students who would have otherwise left to stay in school for an additional year. It is important to reiterate that the impact of these reforms is more substantial than the impact of the USA compulsory schooling laws, which affected only 5% of the targeted cohort (Lleras-Muney, 2005; Oreopoulos, 2006).

In summary, these results validate our empirical strategy and show that our RDD represents a clear improvement upon the previous instrumental variable strategy used by, among others, Cole et al. (2014) to answer to an identical research question using the USA compulsory schooling laws.

4.4.2 Compulsory Schooling, Savings and Investments

We now turn our attention to exploring the effect of increased levels of education had on a range saving and investment decisions. Table 4 presents the parameter estimates of the non-parametric local regressions for both the 1947 and 1972 educational reforms. The analysis is conducted separately for males and females.

Focusing on the 1947 reform, the results reveal that an additional year of schooling impacted on the financial behaviour of females opposed to males. Specifically, the 1947

¹⁸Given the relatively small sample size, outliers may have a large effect on the results. We confirm that repeating the analysis with winsorized data at the 99th percentile of the relevant financial variables (both dependent and independent) yield consistent results to those presented through out the paper. For brevity these results are not presented however they are available on request.

reform increased the probability that females reported they save and that they saved regularly by around 14.1 and 15.2 percentage points, respectively. Upon using a larger bandwidth, that is, without regularization, the estimates obtained are marginally lower. The 1947 reform failed to have a significant impact on the savings and investment decisions of males. This potentially suggests that the 1947 reform equipped females with additional skills and induced them to have more responsible saving behaviours, which they would otherwise have not gained.

Considering the 1972 reform reveals that the results are generally statistically insignificant at the usual confidence level, even when different bandwidths are chosen. One exception is for males who report a lower propensity to save. We, therefore, cannot accept the hypothesis that this policy produced any impact on the financial outcomes under consideration here, even when we split the sample by gender to account for possible heterogeneity. The lack of statistical significance of evidence for the 1972 reform suggests that it is not simply an extra year of education which influences financial decisions, but perhaps something which happened as part of a wider reform in the post war period.¹⁹

Unfortunately data limitations do not allow us to dig deeper to uncover the driving causes of the effect of the first reform on females rather than males. One potential explanation for females being influenced by the first educational reform is that this is a group which was most influenced by the reform. In particular, as documented previously, the 1947 reform was much wider reaching than the 1972 reform and targeted the lowest educated segments of the population. Consequently, females, given the male dominated culture at the time of the first reform, could have been more influenced by this reform and these reforms could have provided females increased opportunities to acquire sufficient skills to make sound financial decisions. Alternatively, these reforms may have influenced unobservable characteristics, such as risk aversion and discount horizons, which in turn influenced financial behaviours. Unfortunately, these measures are unavailable in our data and so we cannot comment on these mechanisms.

These results indicate that, despite a strong and statistically significant association between individual saving behaviours and an individual's education, there is limited evidence that this relationship is causal, in particular for males. This is in line with the idea that education is endogenous to financial decisions. However for females we document some evidence that an extra year of schooling provided additional skills to make positive saving decisions, specifically relating to the decision to save and making regular saving contributions. In the next section we explore the impact of an additional year of education on a variety of debt measures.

¹⁹Similar patterns are obtained when a parametric approach is implemented; results are available upon request.

4.4.3 Compulsory Schooling and Debt

Table 5 presents the results relating to the effects of the both the 1947 and 1972 reforms, for males and females, on a variety of debt measures. The results indicate that an additional year of education fails to have a statistically significant impact on borrowing decisions at either the individual or household level (i.e., secured debt). For females, across both reforms, there is a consistent positive coefficient on the debt indicator while there is a negative sign on the amount of unsecured debt in every specification. However, these estimates are imprecise with relatively large standard errors.

Turning our attention to males, once again there is no evidence that compulsory schooling had any impact on debt behaviours. The second reform appears to reduce the amount of secured debt, however, this result is not statistically significant. These results can be replicated using a parametric RDD approach (see Table B2).

The lack of a causal effect of education on the levels of a variety of debts is arguably unsurprising. Debt, both unsecured at the individual level and secured at the household level, is a vehicle for households to smooth consumption over-time. Consequently, more educated individuals may make rational financial decisions which involve accumulating debt. Education though, despite not reducing the absolute level of debt, may help reduce the level of “problem” debt, that is, debt which the household cannot repay or causes other indirect consequences.²⁰

5 HE Expansion and Financial Behaviours

In order to gain further insight on the impact of education on individual financial behaviours, this section explores the impact of an exogenous expansion of HE. We exploit a large expansion of educational attainment in the UK, which took place from the late 1980’s through to the early 1990’s. This large increase in HE participation in the UK between 1989 and 1994 impacted cohorts born from 1972, and allows us to explore whether higher levels of education had an impact on a range of financial behaviours.

In this section we explore the effect of this education expansion on educational attainment and subsequently use this exogenous variation in the level of education as an instrument in financial behaviours of the effected cohorts for both males and females.

²⁰Previous empirical evidence and theory tell us that retired individuals could save and accumulate debt differently from individuals that are of working age. Unfortunately, due to insufficient observations around first reform, we are unable to re-estimate our analysis on a sub-sample of non-retired individuals. For the 1972 reform we re-estimate our analysis on a sample of employed individuals and find similar results to those presented in the paper, that is, financial behaviours do not change systematically around the cut-off. One difference we observe is that there is some evidence that the 1972 reform had an adverse impact on saving behaviours of males. In this context, it should be noted that there are complex selection issues that we do not account for. These results are available on request.

This expansion increased the participation rate of higher education and we argue that it is important to explore the impact this expansion had on a range of financial behaviours. Given it is focussed on a higher level of education compared to the compulsory schooling reforms discussed above, this reform may be associated with a development of higher order skills which are required to process complex financial behaviours.²¹

The expansion of higher education could be attributed to a combination of factors. Walker and Zhu (2008) argue that there was a relaxation of limits placed on student recruitment, and in conjunction with a reduction in the grants paid to the institution by the government, this induced universities to increase the number of students enrolled. Another factor was the incorporation of the polytechnic and colleges of higher education into the university sector in 1992. This significantly increased the capacity of higher education that is degree level courses. Moreover, there was a significant increase in the proportion of individuals who continued their studies beyond compulsory level during the late 1980's. This increase is arguably due to the change of the educational system, that is, the removal of the O-levels, and the introduction of GCSE's.²²

As argued in Devereux and Fan (2011) and James (2015), given that there are two distinct policy changes which could have impacted on the educational attainment, we estimate the combined effect of both of these policy changes. In line with the analysis above we estimate the impact of this expansion on a range of financial behaviours.

5.1 Empirical Strategy

We exploit cohort level variation in the level of education attainment and financial behaviours. In line with the regression discontinuity approach described above we assume that in the absence of the educational expansion education levels would have evolved in a manner which can be described using cohort polynomials. Unlike the above, which is a jump for a single cohort, this expansion occurs over several cohorts, and therefore we have to impose stronger assumptions to identify the causal estimate (Devereux and Fan, 2011).

Our estimation specification is as follows:

$$Ed_{ic} = \alpha + \sum_{c=72}^{c=75} \beta_c cohort_c + \delta(after) + f(age_{ic}) + g(cohort_c) + \epsilon \quad (2)$$

where the dependent variable is an individual's education level, as measured by years

²¹In this section we are unable to use this education expansion as an instrument for debt holding and the level of unsecured debt due to a weak first stage in the sample of individual with a valid observations of the dependent variables.

²²The General Certificate of Education - Ordinary Level (O-level), was a subject-based academic qualification, which was introduced in 1951, replacing the 16+ School Certificate (SC). The O-level was then replaced in General Certificate of Secondary Education (GCSE) in 1988.

of education, and the subscript represents individual i in cohort c . We control for age and cohort using cubic polynomials, in addition to year of survey and government office region fixed effects. As in James (2015) the higher education expansion is captured by separate cohort indicators for 1972 to 1975. In addition, we include a variable which captures cohorts after the expansion.²³

In the second stage we link the level of education with the financial behaviours outcome. This takes the form:

$$FinancialBehaviour_{ic} = \alpha + \theta E\hat{d}_{ic} + f(age_{ic}) + g(cohort_c) + \kappa. \quad (3)$$

Assuming that the education expansion had no impact on financial behaviours other than through education we can estimate a 2SLS model where the education specification (Eq. 2) is the first stage; the excluded instruments are the dummy variables for each of the cohorts covered by the expansion period and a dummy variable indicating being in a post-expansion cohort. Eq. 3 represents the second stage equation and θ is the coefficient of interest. This captures the causal effect of higher education on financial behaviours. The causal effect in this setting, as outlined by James (2015), is the LATE for those individuals who obtained a higher level of education as a result of the education reforms. All estimates are clustered at the cohort-age level in line with Machin et al. (2012).

5.2 Results

Table 7 presents the summary statistics of the sample relating to the HE expansion. Initially, prior to the statistical analysis, we graphically explore the impact of the education expansion on individual education level, as measured by the age left education. Figure 8 demonstrates graphically the dramatic increase in level of education of individuals around the expansion of the HE sector. It is clear from the data that there is a significant increase in the level of education for cohorts after the expansion.

We now go on to implement the statistical analysis described in Section 5.1. Panel A of Tables 8 and 9 present the first-stage estimates and indicates that there is a significant increase in the level of education for cohorts after the expansion. The results indicate the F-test of joint significance of the cohort and post education expansion dummy are all above the conventional requirements; suggesting a sufficiently strong first stage. These results are in line with the existing literature.

Turning our attention to the results relating to the second stage of the instrumental variables approach, as presented in Panel B of Tables 8 and 9, reveal similar results to those

²³We include individuals born between 1962 and 1982, and limit the sample analysed to individuals who report an age left education to 30 years old or less, in line with the existing literature. We also control for year of survey, government office region, race, marital status and household income in all specifications.

presented above relating to the raising of the compulsory school leaving age. Generally, the results suggest that education fails to have a statistically significant impact across the financial outcomes considered, once the years of education is instrumented by the HE expansion cohorts. This results is consistent across males and females. One exception is that for males, higher levels of education are associated with lower levels of secured debt holding and the ratio of secured debt to household income.²⁴

This results supports the idea that there is a limited causal relationship between additional years of education and a range of financial behaviours. Moreover, it suggests that this relationship is limited across the spectrum of education attainment. Once again, these results suggest that there are unobservable omitted characteristics which are driving the positive association between observed in many existing studies.

6 Discussion

The aim of this section is to reconcile and provide a possible explanation of why the results for the UK differ from a study that attempts to establish causality between education and financial behaviours in the USA. Our results can be compared to the ones produced by Cole et al. (2014), who analyses the effect of years of schooling on financial outcomes using census data from the USA. Similar to us, he uses compulsory schooling laws to develop causal estimates, but exploits the exogenous variation of these laws across states using an instrumental variable approach. These reforms vary in degree, intensity and time of adoption across different states. Cole et al. (2014) shows that an extra year of education increases the probability of holding investment and retirement savings income by 7-8 percentage and 6 percentage points, respectively. Further, the marginal effect of a year of schooling on the amount of income from investment and from retirement savings is around \$1,800 and \$1,000, respectively. These figures are equivalent to their sample mean, that is, they represent an economically significant change. On the contrary, our estimates are rather imprecise and when it comes to the amount of debt and savings vary greatly according to the specification chosen. Nevertheless, the results relating to a female's propensity to save or to declare to be a regular saver are qualitatively comparable to Cole et al. (2014).

There are some plausible explanations for this discrepancy between our results and some of the results in the current literature. Firstly, one explanation is that differences may be partly generated by the empirical approaches adopted. Studies which exploit data from the USA identify the effect of schooling by using the state-wide variation in the

²⁴As in the analysis above, we have re-run our analysis on winsorized data, censoring the continuous variables at the 99th percentile of the continuous variables, in order to remove the potential effects of outliers. We confirm that when this is done the result are consistent with those presented in the paper, and are available on request.

compulsory schooling laws. One of the underlying assumptions weakening these studies is that the individuals interviewed are assumed to live in the same state where they were born and they studied. As documented by Lleras-Muney (2005) and Oreopoulos (2006), the laws in the USA affect a relatively small proportion of the population, approximately 5%, so that “*higher IV results could occur because they approximate average effects among a small and peculiar group*” (Oreopoulos, 2006, p. 153). This same problem with “global IV estimates” is acknowledged in the returns to schooling literature and is discussed in, for example, Imbens and Angrist (1994), Card (2001) and more recently by Clark and Royer (2013). As demonstrated by Oreopoulos (2006), when the portion affected by the reforms increases – as occurs when using British reforms – the local average treatment effect (LATE) converges to the average treatment effect (ATE), which is the effect on all individuals, not on only a small or specific set of individuals. As reported in numerous studies and confirmed by our data, the UK reforms affected a much larger group of individuals.

Furthermore, our findings are in line with recent studies on earnings by Devereux and Hart (2010), which show that the private returns to schooling in the UK are much lower than suggested by earlier studies. Similar results are also found in Clark and Royer (2013) who find that educational reforms did not have a large effect on health and mortality in Britain. In both cases, similar studies conducted in the USA using “global IV approaches” found larger and statistically significant impacts.

Second, this discrepancy can be due to differences in the nature of the schooling reforms in Britain and the USA. It might be that the USA education reforms captured by the data available put more emphasis on numeracy skills useful to make desirable financial decisions, that is increased saving and lower debt holdings. The importance of improving numeracy rather than general education in order to improve individual’s financial decision making is also supported by Cole et al. (2016) and Brown et al. (2016).²⁵ Our results indicate that changes in compulsory schooling did not have substantial effects on financial behaviours. This is in sharp contrast with the robust positive relationships between financial outcomes and the school leaving age that can be observed in our data and in the literature more generally when running *naïve* regressions. One may argue that one additional year of education is not enough to uncover effects on financial decisions. We cannot rule out this explanation, but at the same time, we point to the vast literature (see Section 2) that finds that an extra year of compulsory education improves labour and non-labour markets outcomes, including cognitive abilities. A potential explanation is that education is endogenous with respect to financial behaviours. As described in our conceptual framework in Section 2, unobservables, such as family characteristics,

²⁵This is in line with Agarwal and Mazumder (2013); they find that individuals with high test scores, and particularly high math scores, are less likely to make financial mistakes.

discount rates, innate ability, may be crucial in driving decisions with respect to the amount of schooling, savings, debts and investments. For instance, recent advances in *genoeconomics* have shown that 33% of the variation in individual saving rates can be explained by genetic differences (see, for example, Cronqvist et al., 2015; Cronqvist and Siegel, 2015; and Cesarini et al., 2010).

There is a further explanation for the statistically insignificant effect of general education on a range financial behaviours.²⁶ When considering the reforms implemented in 1947 and 1972 and we are observing individual's saving behaviours from 1991 onwards, the additional education received at this time arguably has little bearing on the understanding of current financial products available to households. Between the reforms and our observed data, there has been a boom in both the amount and complexity of financial products available, in addition to technological changes and the advent of the internet. Moreover, when we consider higher levels of education, as measured by a substantial expansion in the higher education sector, we still find limited evidence that generally education has a positive impact on financial behaviours. Consequently, additional prior general education received fails to have a significant impact; what is needed, we would argue, is a more tailored, up-to-date and specific education relating to current financial instruments and markets accompanied with numeracy training. For instance, Gaudecker and Martin (2015) find that investment outcomes are better the higher the score in specific financial literacy questions. Our results are also in line with, for example, Miller et al. (2015) who find that financial education interventions can have a positive impact on a range of financial outcomes, whilst, Brown et al. (2016) report that state-mandated financial training makes individuals less prone to accumulate debt and more likely to keep up interest payments. Nevertheless, the literature is far from reaching a definite answer with respect to the importance of financial education. For instance, Cole et al. (2016) provide evidence that financial market participation, investment income and better credit management can be achieved by additional high school math courses.

7 Concluding Remarks

This paper used a range of exogenous schooling reforms in the UK to explore the relationship between education and a range of financial behaviours. The paper documents a strong correlation between education and a range of financial outcomes using simple cross-sectional regression techniques. Initially, when we explore causality using an RDD setting, our results showed a strong and positive effect of the level of education on saving behaviours (saving, being a regular saver and the amount saved) for females relating to

²⁶Albeit this may be marginal in the context of this paper since the survey did not cover in depth use of the various financial assets available on the markets. This is a potential future area of research.

the first educational reform. However there is limited evidence that the reform had any impact on males. Furthermore, we find that the 1972 reform did little to change the financial behaviour of individuals. The results support the recent findings in the literature, see for example, Devereux and Hart (2010), suggesting that one additional year of education had very limited effects on earnings and other outcomes. Moreover, we exploit a large expansion in the HE sector to explore the causal impact of higher levels of educational attainment on financial outcomes. Once more, we find this expansion had a dramatic impact on an individuals level of education, however, generally this increase in education did not translate into improved financial behaviours. This is consistent across males and females.

The marked difference between the quasi-experimental settings and the simple regression results points in the direction of the presence of important omitted variables – such as time preference or other abilities – discussed in the education-financial decision literature. These findings are substantially different from the evidence presented by Cole et al. (2014) analysing the USA education system. These differences could be attributed to differences in the nature and type of reforms in the two countries. There is also the possibility that the differences could be generated by the two statistical approaches. In fact, the instrumental variable approach by Cole et al. (2014) often leads to overestimates of the effects of schooling on a variety of other outcomes. For instance, our findings are in line with Clark and Royer (2013) who find that education did not have a large effect on health in Britain, while previous studies conducted in the USA found a significant impact.

Education policy is certainly generating positive spill-overs in many areas of our society. However, we argue that desirable financial behaviours require specific education interventions to improve financial education and numeracy in particular (see, for example, Cole et al., 2016). We point to the growing literature on financial literacy to support this conclusion. Unfortunately the data used in this analysis does not allow the examination of the impact of financial literacy on these financial outcomes, or of the relationship between compulsory education and financial literacy. Given the increasing complexities faced by households when making financial decisions, fully understanding how to alleviate poor financial behaviours is of utmost importance. This paper therefore highlights the importance for further investigation into the casual impacts and determinants of a range of financial outcomes.

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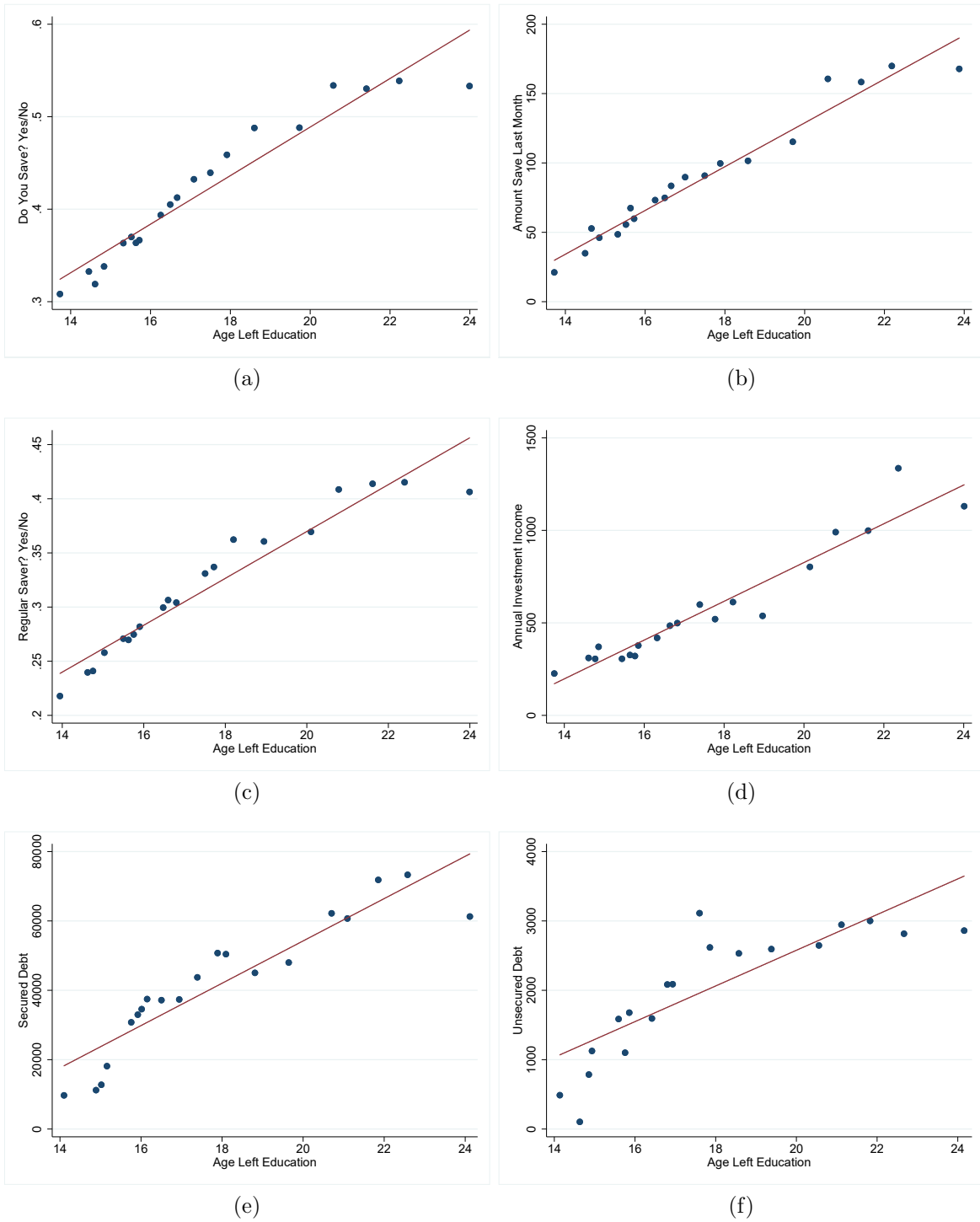


Figure 1: Correlation between age individual left education and various financial outcomes. The fitted line is the prediction from a regression of financial outcome Y on age at which the individual left education. Each regression includes individual and household characteristics: age, age squared, log of income of the household, employment status, household size, self-assessed health status, survey wave and government office regions fixed effects. The dots in each graphs are means at each age. The graph has been obtained using the Stata routine *binscatter*.

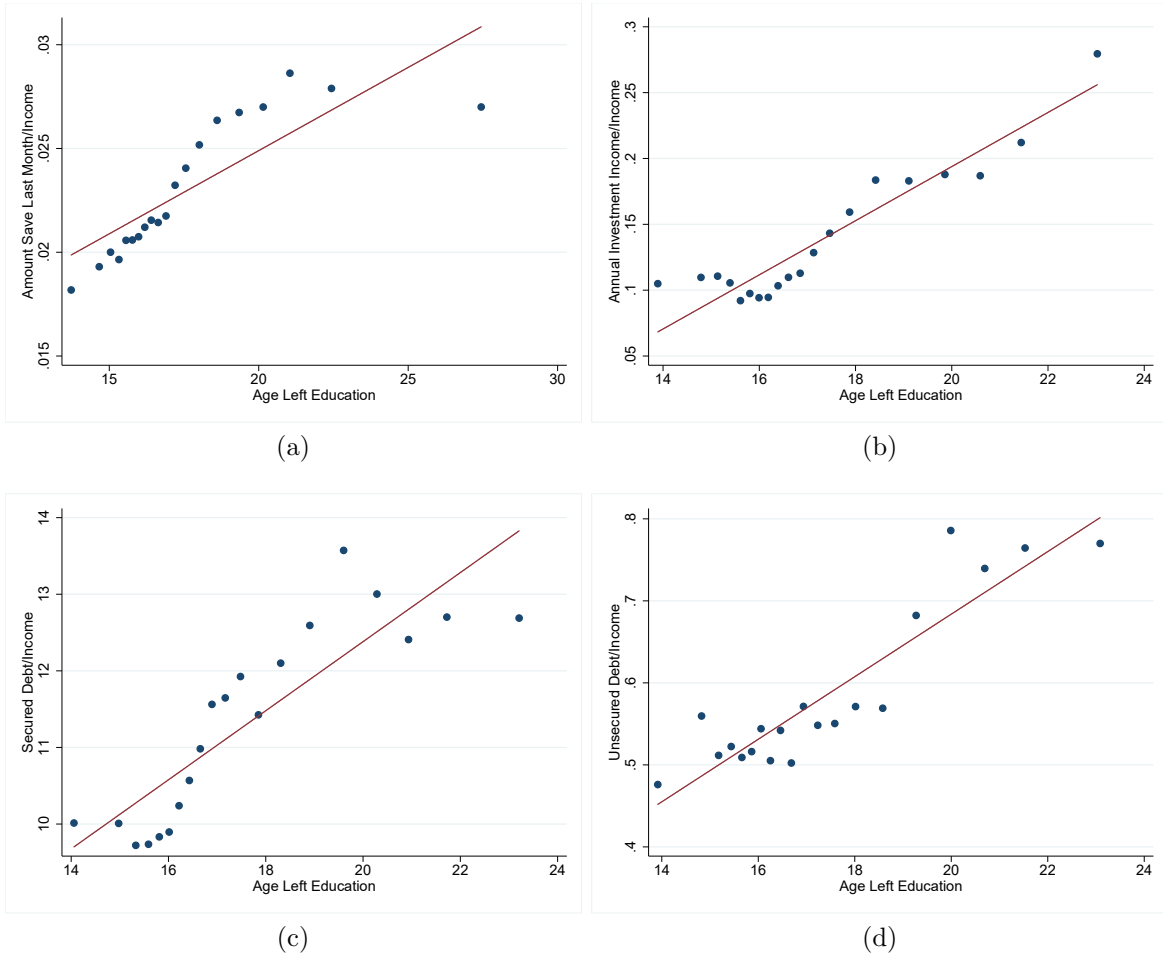


Figure 2: Correlation between age individual left education and various financial outcomes. The fitted line is the prediction from a regression of financial outcome Y on age at which the individual left education. Each regression includes individual and household characteristics: age, age squared, log of income of the household, employment status, household size, self-assessed health status, survey wave and government office regions fixed effects. The dots in each graphs are means at each age. The graph has been obtained using the Stata routine *binscatter*.

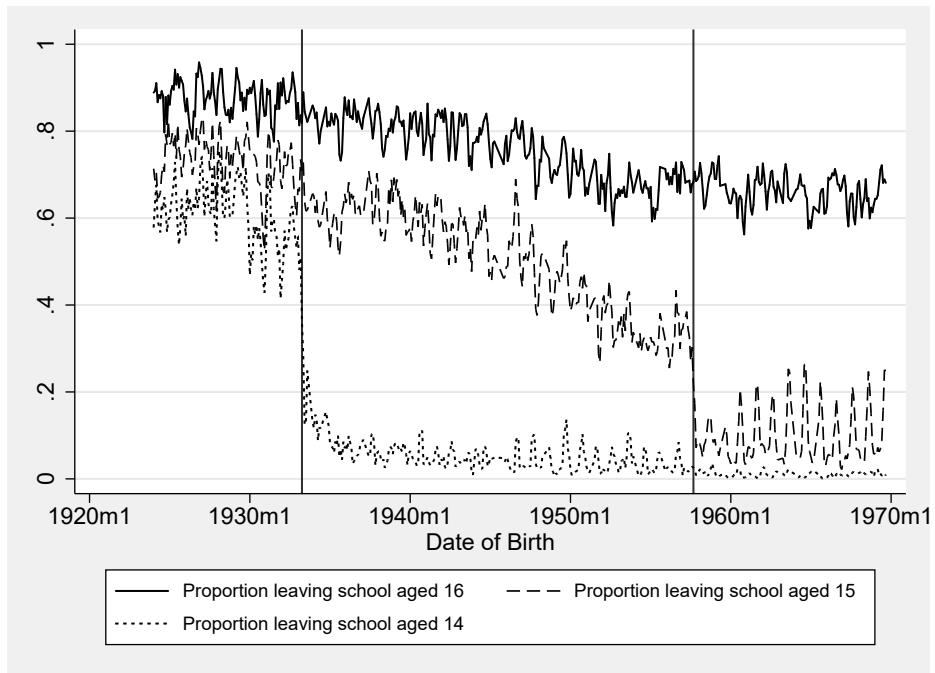


Figure 3: Years of full-time schooling

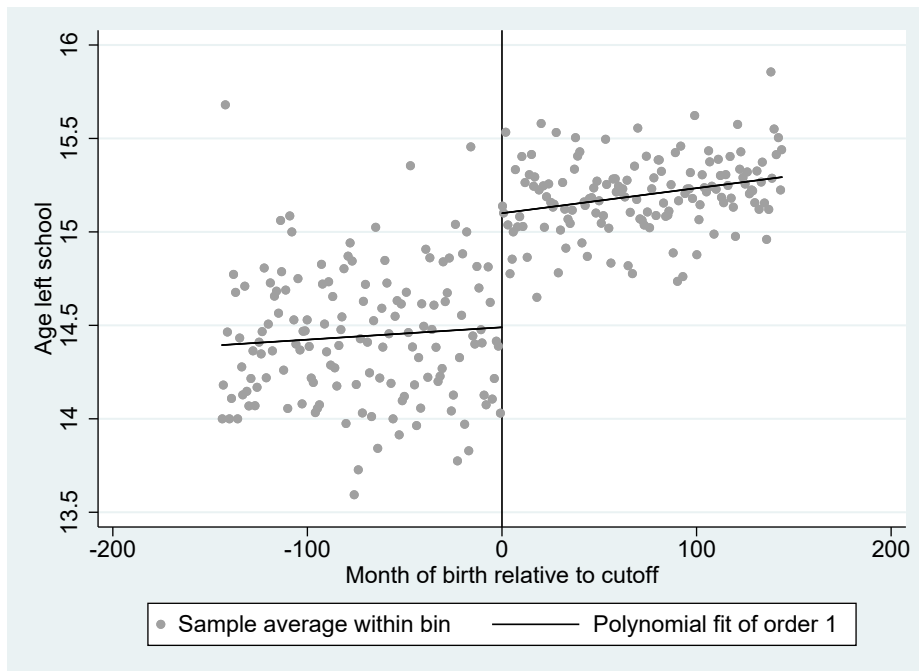


Figure 4: Average School Leaving Age by Month of Birth for Females - 1947 Reform

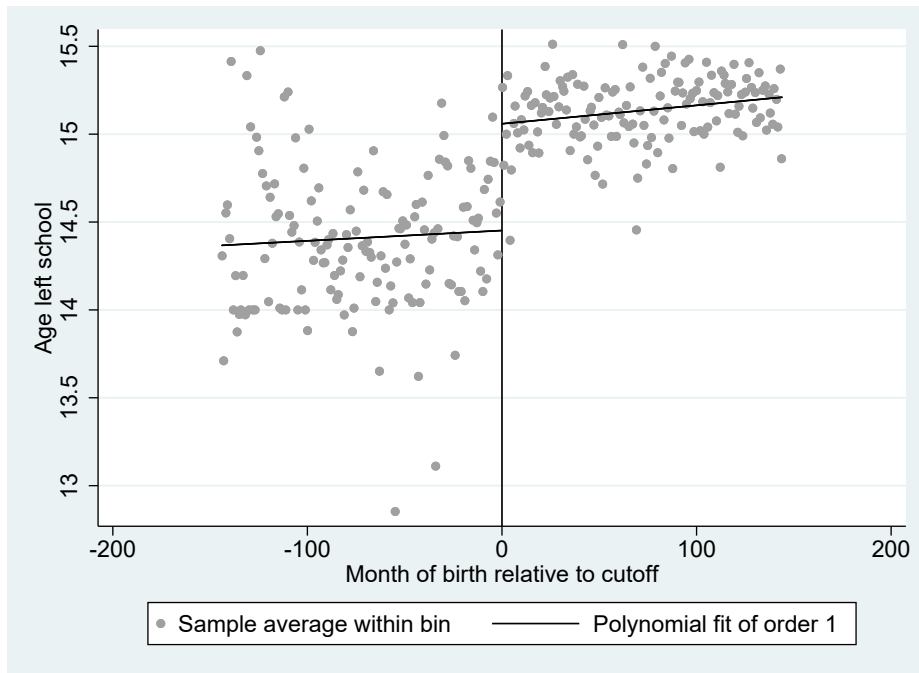


Figure 5: Average School Leaving Age by Month of Birth for Males - 1947 Reform

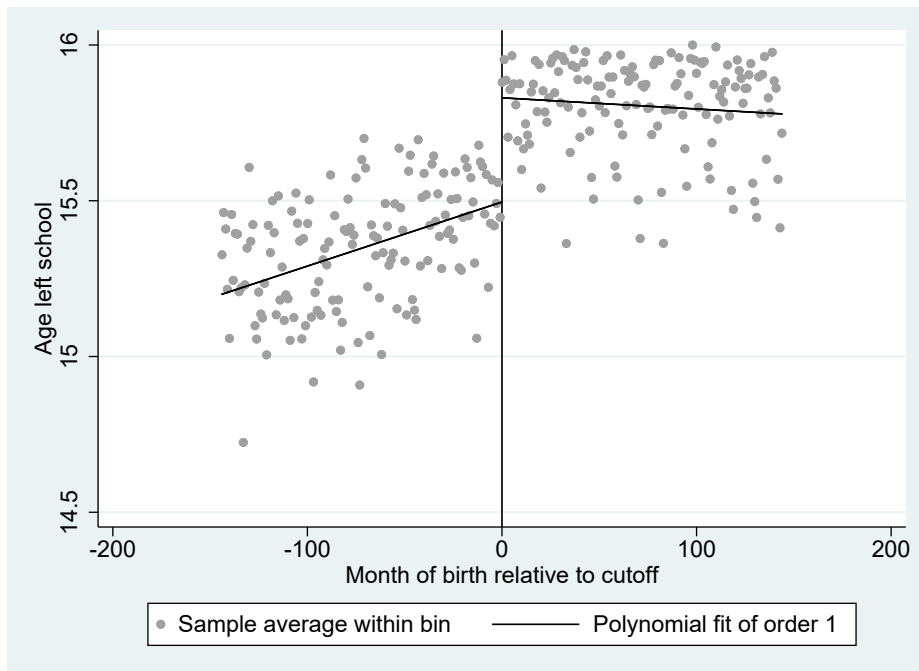


Figure 6: Average School Leaving Age by Month of Birth for Females - 1972 Reform

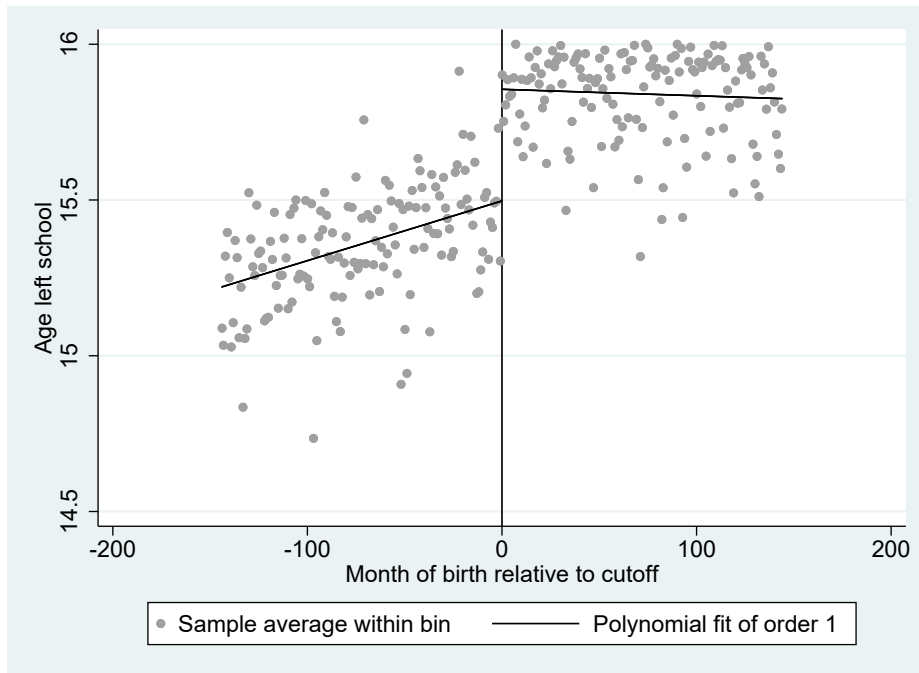


Figure 7: Average School Leaving Age by Month of Birth for Males - 1972 Reform

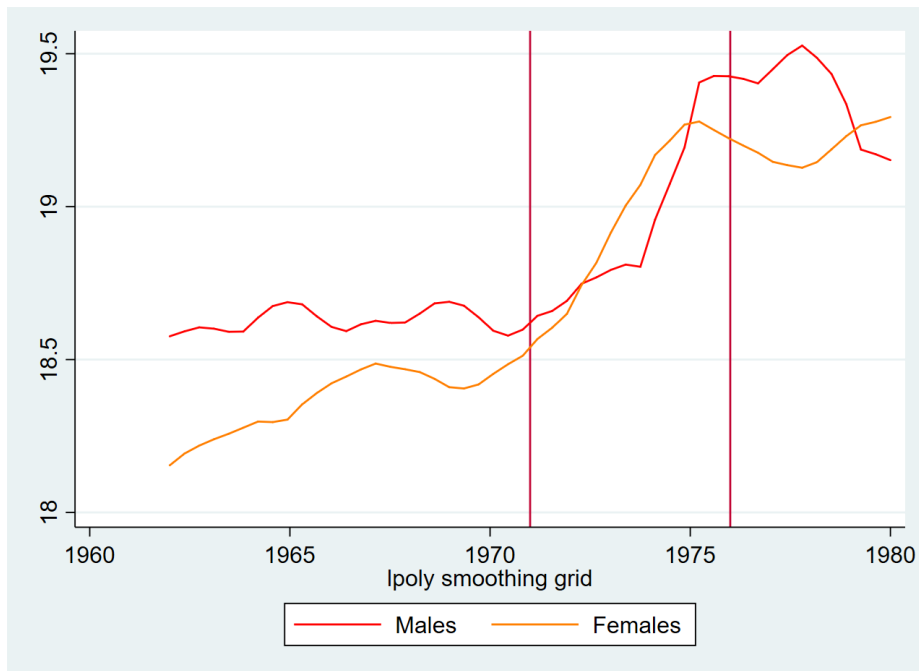


Figure 8: Average Years of Schooling - Education Expansion

Table 1: Financial outcomes from BHPS and Understanding Society

Variable	Wording of survey questions and answer codes	Waves	Level
Savings and Investment			
<i>Do you save?</i>	Do you save any amount of your income for example by putting something away now and then in a bank, building society, or Post Office account other than to meet regular bills? Please include share purchase schemes ISA's and Tessa accounts.	A, B, C, D, E, F, G, H, I, J, K, L, M, N, O, P, Q, R, 2, 4, 6, 8	Individual
<i>Regular Saver</i>	Do you save on a regular basis or just from time to time when you can? =1 if Saves on a regular basis.	J, K, L, M, N, O, P, Q, R, 2, 4, 6, 8	Individual
<i>Amount Saved</i>	About how much on average do you personally manage to save a month?	A, B, C, D, E, F, G, H, I, J, K, L, M, N, O, P, Q, R, 2, 4, 6, 8	Individual
<i>Amount Saved/Income</i>	The ratio of amount saved in previous month to household monthly income	A, B, C, D, E, F, G, H, I, J, K, L, M, N, O, P, Q, R, 2, 4, 6, 8	Individual
<i>Investment Income</i>	Derived Variable: Annual household investment income. This variable sums the values of annual investment income in the reference year, that is the twelve months prior to the start of the interview period (1st Sept.) for individuals in the household.	A, B, C, D, E, F, G, H, I, J, K, L, M, N, O, P, Q, R, 1, 2, 3, 4, 5, 6, 7, 8	Household
<i>Investment Income/Income</i>	The ratio of investment income to monthly income in previous month	A, B, C, D, E, F, G, H, I, J, K, L, M, N, O, P, Q, R, 1, 2, 3, 4, 5, 6, 7, 8	Household
Debt			
<i>Debt</i>	I would like to ask you now about any other financial commitments you may have apart from mortgages and housing related loans. Do you currently owe any money on the things listed on this card?	E, J, O, 4, 8	Individual
<i>Unsecured Debt</i>	About how much in total do you owe?	E, J, O, 4, 8	Individual
<i>Unsecured Debt/Income</i>	The ratio of unsecured debt to monthly household income	E, J, O, 4, 8	Individual
<i>Secured Debt</i>	Approximately how much is the total amount of your outstanding loans on all the property you (or your household) own, including your current home?	C, D, E, F, G, H, I, J, K, L, M, N, O, P, Q, R, 1, 5, 6, 7, 8	Household
<i>Secured Debt/Income</i>	The ratio of secured debt to household income	C, D, E, F, G, H, I, J, K, L, M, N, O, P, Q, R, 1, 5, 6, 7, 8	Household

Notes: Waves A (1991) to R (2008) are from the British Household Panel Data, waves 1 – 8 (2008 - 2018) are from Understanding society.

Table 3: The effect of compulsory schooling laws on schooling leaving age

		Female			
		Age Left School	<= 14 Leaving Age	<= 15 Leaving Age	<= 16 Leaving Age
Panel A: 1947 reform					
		0.599*** (0.097)	-0.292*** (0.021)	-0.052 (0.037)	-0.034 (0.030)
N		32,747	32,974	32,974	32,974
BW		144			
Panel B: 1972 reform BW= CCT					
		0.129*** (0.045)		-0.194*** (0.017)	0.043** (0.018)
N		79,283		79,816	79,816
BW		144			
		Male			
		Age Left School	<= 14 Leaving Age	<= 15 Leaving Age	<= 16 Leaving Age
Panel A: 1947 reform					
		0.509*** (0.090)	-0.267*** (0.023)	-0.051 (0.033)	0.002 (0.026)
N		40,236	40,508	40,508	40,508
BW		144			
Panel B: 1972 reform					
		0.173*** (0.015)		-0.828*** (0.075)	0.051 (0.062)
N		91,468		91,869	91,869
BW		144			

Notes: Table 3 gives the estimated effect of compulsory schooling law change on various outcomes. All regressions estimated using pooled waves of the BHPS and Understanding Society. All regressions include a linear function of month of birth and a linear interaction of month of birth and a dummy variable for being born after the relevant threshold, and robust standard errors clustered by month of birth are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Compulsory schooling effects on savings and Investments using local linear regression models

	Female				Male			
	Do You Save?	Regular Saver?	Amount Saved	Investment Income	Do You Save?	Regular Saver?	Amount Saved	Investment Income
Panel A: 1947 reform								
BW= CCT	0.141*** (0.043)	0.152*** (0.047)	0.520*** (0.198)	-0.439 (0.503)	-0.191 (0.198)	-0.162 (0.167)	-0.636 (1.237)	-0.243 (1.610)
N	5518	3534	6653	10472	5806	3750	4964	5672
BW	23	22	31	32	28	28	26	20
BW=no reg	0.067* (0.040)	0.084*** (0.042)	0.313* (0.179)	-0.348 (0.398)	-0.075 (0.153)	-0.032 (0.165)	0.246 (0.542)	0.108 (1.066)
N	24764	15303	15678	16724	15582	6045	14041	11173
BW	105	98	49	52	94	47	75	28
Panel B: 1972 reform								
BW= CCT	0.097 (0.384)	0.578 (1.328)	0.646 (1.560)	-3.940 (6.246)	-0.383*** (0.131)	-0.302* (0.172)	-0.732 (0.549)	-0.609 (0.625)
N	11964	8749	11909	15417	7868	6951	9219	14005
BW	23	22	23	20	17	21	21	20
BW=no reg	0.001 (0.245)	0.111 (0.173)	0.082 (0.479)	-4.243 (4.764)	-0.216* (0.111)	-0.241* (0.143)	-0.123 (0.543)	-0.317 (0.629)
N	20996	18155	48782	26531	12007	9216	23552	20150
BW	39	45	97	33	25	28	55	29

Notes: Table 4 gives the estimated effect of compulsory schooling law change on various outcomes. All local linear regressions estimated with triangular Kernels using pooled waves of the BHPS and Understanding Society. Robust standard errors clustered at date or birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Compulsory schooling effects on debts using local linear regression models

	Female			Male		
	Debt	Amount Unsecured Debt	Amount Secured Debt	Debt	Amount Unsecured Debt	Amount Secured Debt
Panel A: 1947 reform						
CCT	0.035 (0.043)	0.141 (0.268)	-1.624 (1.141)	-0.007 (0.111)	-0.387 (1.058)	0.014 (0.461)
N	2328	2262	2300	2131	1803	2199
BW	38	37	21	40	34	23
CCT no reg	0.026 (0.035)	0.072 (0.211)	-0.437 (0.351)	0.033 (0.030)	-0.063 (0.418)	0.391 (0.747)
N	4081	3953	9838	7674	4894	4322
BW	66	63	87	72	92	43
Panel B: 1972 reform						
CCT	0.153 (0.358)	-0.911 (2.780)	6.047 (33.419)	-0.067 (0.165)	-0.979 (1.318)	-0.039 (0.564)
N	3354	3506	7459	3438	3303	8364
BW	22	23	17	26	24	20
CCT no reg	0.237 (0.231)	0.322 (0.785)	-3.069 (14.864)	-0.098 (0.133)	-0.283 (1.628)	-0.536 (0.659)
N	7395	22806	10178	14058	7155	17537
BW	46	144	24	107	54	43

Notes: Table 5 gives the estimated effect of compulsory schooling law change on various outcomes. All regressions estimated with local linear regressions with triangular kernel weights using pooled waves of the BHPS and Understanding Society. Robust standard errors clustered at date of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Compulsory schooling effects on savings and financial ratios using local linear regression models

	Female				Male			
	Am. Saved/ Income	Inv. Inc/ Income	Unsecured Debt/ Income	Secured Debt/ Income	Am. Saved/ Income	Inv. Inc/ Income	Unsecured Debt/ Income	Secured Debt/ Income
Panel A: 1947 reform								
CCT	0.006 (0.004)	-0.109 (0.127)	0.041 (0.092)	1.572 (2.445)	-0.028 (0.047)	-0.044 (0.197)	-0.140 (0.360)	-3.276 (3.437)
N	5629	9537	2199	2738	4042	6754	1340	2329
BW	26	28	36	26	22	23	25	24
CCT no reg	0.004 (0.004)	-0.077 (0.100)	-0.021 (0.065)	-0.230 (1.510)	0.010 (0.019)	-0.006 (0.180)	-0.363 (0.309)	-1.536 (2.028)
N	25823	13960	4728	7045	7259	18410	3435	9360
BW	68	42	75	64	40	64	65	91
Panel B: 1972 reform								
CCT	0.057 (0.057)	-0.702 (1.144)	0.468 (1.240)	-20.592 (75.717)	-0.002 (0.010)	-0.042 (0.117)	-0.993*** (0.373)	-4.499 (2.871)
N	10435	12894	3659	9302	10535	15927	3018	7928
BW	20	16	24	21	24	23	22	19
CCT no reg	0.005 (0.013)	-0.527 (0.477)	0.474 (0.303)	-3.525 (7.483)	0.003 (0.009)	0.035 (0.123)	-0.519 (0.466)	-6.053* (3.098)
N	24700	27986	17223	18296	34129	37094	4630	13776
BW	48	36	108	41	80	54	34	35

Notes: Table 6 gives the estimated effect of compulsory schooling law change on various financial ratios. Dependent variables are winsorized at the 99 percentile to remove the potential effect of outliers. All local linear regressions estimated with triangular kernels using pooled waves of the BHPS and Understanding Society. Robust standard errors clustered at date or birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Summary Statistics - Education Expansion

Variable	Obs	Female				Male			
		Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Do you save?	32,419	0.411	0.492	0	1	0.471	0.499	0	1
Regular saver	28,941	0.327	0.469	0	1	0.369	0.482	0	1
Amount saved	31,484	78.545	231.165	0	10000	123.791	456.217	0	40000
Amount saved / Income	31,484	0.0249	0.3945	0	44.977	0.1761	15.408	0	2000
Investment income	59,081	243.174	3175.006	0	460000.2	60,653	6906.751	0	1000000
Investment income/income	59,081	.0553774	.6400708	0	103.4168	.1217988	3.250888	0	782.2686
Debt	12,245	0.403	0.490	0	1	0.424	0.494	0	1
Amount unsecured debt	12,245	2300.886	8356.672	0	350000	3192.218	10831.480	0	600000
Amount secured debt	30,662	63788.390	129685.500	0	1.12E+07	74226.140	138496.500	0	13000000
Amount secured debt/Income	30,662	21.5052	312.996	0	36428.57	199.8339	11697.09	0	1529500
Age left Education	59,081	18.680	3.148	15	30	18.817	3.172	15	30
Wave of Survey	59,081	20.494	4.942	1	26	18.775	6.116	1	26
Age	59,081	40.41663	6.978274	16	56	39.21384	7.352779	16	56
Married	59,081	.6769689	.4676384	0	1	.8318467	.3740054	0	1
Ln(household income)	59,081	8.023679	.7866716	.0953102	14.06126	8.05857	.7671571	.0861777	14.06126
Retired	58,752	.0013787	.0371052	0	1	.0018192	.0426133	0	1
Household size	59,081	3.307899	1.362721	1	14	3.21135	1.430475	1	16
Self-assessed health	58,667	2.556872	1.068249	0	4	2.723264	.9940194	0	4

Table 8: Higher Education Expansion on Age Left Education and Financial Behaviours

	Female									
	Do you Save?	Regular Saver	Amount Saved	Investment Income	Secured Debt	Do you Save?	Regular Saver	Amount Saved	Investment Income	Secured Debt
Panel A: First stage										
Cohort 72	0.0918 (0.0751)	0.136 (0.0848)	0.104 (0.0834)	0.120** (0.0571)	0.169* (0.0976)	0.447*** (0.0867)	0.498*** (0.0951)	0.453*** (0.0897)	0.484*** (0.0614)	0.533*** (0.0872)
Cohort 73	0.306*** (0.0797)	0.310*** (0.0877)	0.356*** (0.0823)	0.341*** (0.0707)	0.497*** (0.115)	0.492*** (0.0820)	0.565*** (0.0883)	0.513*** (0.08870)	0.522*** (0.0585)	0.543*** (0.0815)
Cohort 74	0.280*** (0.0951)	0.292*** (0.103)	0.326*** (0.0959)	0.214*** (0.0778)	0.418*** (0.120)	1.037*** (0.119)	1.155*** (0.124)	1.048*** (0.124)	1.107*** (0.0769)	1.153*** (0.110)
Cohort 75	0.860*** (0.145)	0.913*** (0.150)	0.923*** (0.145)	0.830*** (0.107)	1.191*** (0.146)	1.068*** (0.129)	1.186*** (0.139)	1.059*** (0.138)	1.130*** (0.0895)	1.122*** (0.140)
Post Expansion	1.155*** (0.138)	1.183*** (0.149)	1.255*** (0.138)	0.878*** (0.112)	1.201*** (0.176)	0.854*** (0.163)	0.934*** (0.175)	0.882*** (0.166)	0.910*** (0.122)	0.920*** (0.175)
F-test	18.170	16.859	21.174	20.969	19.150	20.948	23.471	18.089	56.104	27.170
Panel B: Second stage										
Age Left Education	-0.00702 (0.0194)	0.00660 (0.0195)	-0.0886 (0.101)	0.0874 (0.0790)	-0.514*** (0.144)	-0.0155 (0.0165)	-0.00700 (0.0149)	-0.0766 (0.0811)	-0.0363 (0.0571)	-0.281* (0.166)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36,674	29,817	35,201	60,653	34,985	32,419	28,941	31,484	59,081	30,664

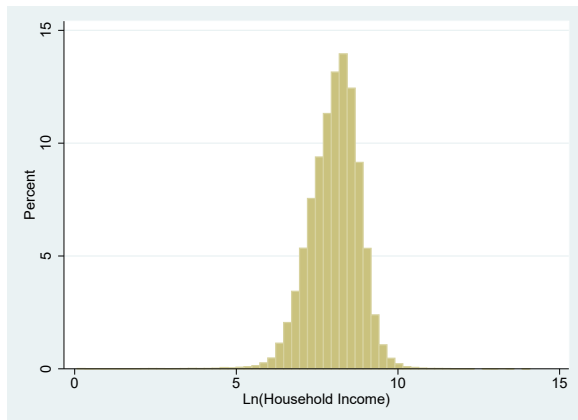
Notes: F-test of joint significance of cohort and post education expansion dummies. Robust standard errors clustered at cohort/age level; *** p<0.01, ** p<0.05, * p<0.1; All specifications include a cubic in age and cohort, year of survey, government office region, race, marital status and household income.

Table 9: Higher Education Expansion on Age Left Education and Financial Ratios

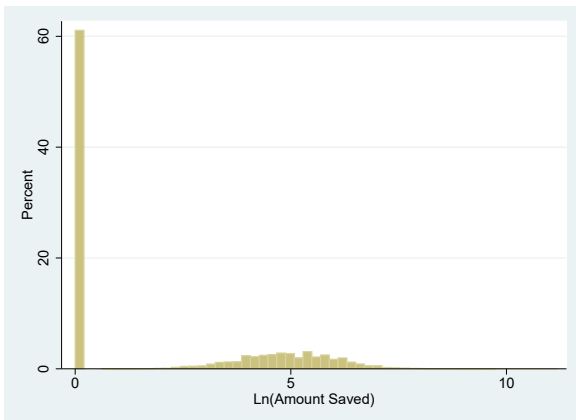
	Male			Female		
	Amount Saved/Income	Investment Income/Income	Secured Debt/Income	Amount Saved/Income	Investment Income/Income	Secured Debt/Income
Panel A: First stage						
Cohort 72	0.104 (0.0834)	0.120** (0.0571)	0.172* (0.0983)	0.453*** (0.0897)	0.484*** (0.0614)	0.533*** (0.0872)
Cohort 73	0.356*** (0.0823)	0.341*** (0.0707)	0.500*** (0.117)	0.513*** (0.0887)	0.522*** (0.0585)	0.543*** (0.0814)
Cohort 74	0.326*** (0.0959)	0.214*** (0.0778)	0.422*** (0.121)	1.048*** (0.124)	1.107*** (0.0769)	1.153*** (0.110)
Cohort 75	0.923*** (0.145)	0.830*** (0.107)	1.199*** (0.147)	1.059*** (0.138)	1.130*** (0.0895)	1.122*** (0.140)
Post Expansion	1.255*** (0.138)	0.878*** (0.112)	1.208*** (0.178)	0.882*** (0.166)	0.910*** (0.122)	0.919*** (0.176)
F-test	21.17	20.97	19.17	18.09	56.10	27.19
Panel B: Second stage						
Age Left Education	-0.00277 (0.00200)	-0.0137* (0.00798)	-1.354** (0.638)	-0.00103 (0.00138)	0.00827 (0.00504)	0.00977 (0.606)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	35,201	60,653	34,820	31,484	59,081	30,662

Notes: F-test of joint significance of cohort and post education expansion dummies. Dependent variables are winsorized at the 99 percentile to remove the potential effect of outliers. Robust standard errors clustered at cohort/age level; *** p<0.01, ** p<0.05, * p<0.1; All specifications include a cubic in age and cohort, year of survey, government office region, race, marital status and household income.

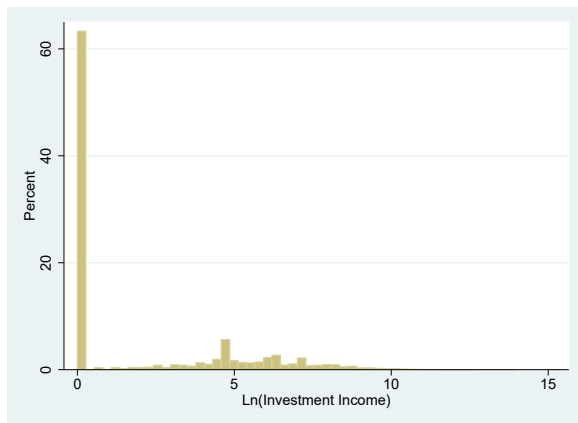
Appendix



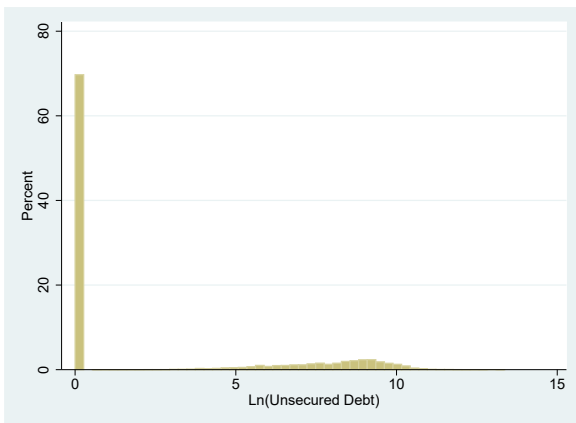
(a)



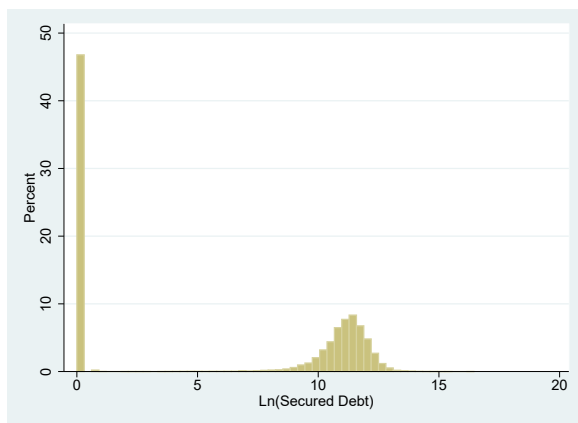
(b)



(c)



(d)



(e)

Figure A1: Histograms of key financial variables.

Table A1: Random effects probit estimates of education on binary financial outcomes: Marginal effects

	Do you save?		Regular Saver?		Debt	
	Females	Males	Females	Males	Females	Males
Panel A - All Education Levels						
Age Left Education	0.024*** (0.002)	0.026*** (0.002)	0.016*** (0.002)	0.019*** (0.002)	-0.008*** (0.002)	0.004* (0.002)
Observations	97,589	117,738	82,436	86,340	35,391	34,684
Panel B - Left School at 16 years or less						
Age Left Education	0.022*** (0.004)	0.037*** (0.005)	0.015*** (0.004)	0.029*** (0.005)	0.017*** (0.005)	0.021*** (0.005)
Observations	69,532	83,397	57,410	59,572	24,207	23,564

Notes: Standard errors presented in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional controls include: age, gender, marital status, household income, labour force status, household size, number of children, self-assessed health, and year and region fixed effects.

Table A2: Estimates of linear model with individual random effects of education on continuous financial variables

	Amount Saved		Inv. income		Unsecured debt		Secured Debt	
	Females	Males	Females	Males	Females	Males	Females	Males
Panel A - All Education Levels								
Age left Education	0.152*** (0.009)	0.183*** (0.010)	0.379*** (0.008)	0.393*** (0.010)	0.003 (0.018)	0.061*** (0.019)	0.318*** (0.021)	0.107*** (0.021)
Observations	92,579	110,689	170,944	181,955	35,391	34,684	83,324	91,896
Panel B - Left School at 16 years or less								
Age Left Education	0.111*** (0.018)	0.204*** (0.021)	0.296*** (0.016)	0.364*** (0.020)	0.143*** (0.035)	0.155*** (0.041)	0.419*** (0.042)	0.344*** (0.046)
Observations	66,346	78,509	119,230	125,982	24,207	23,564	55,721	60,495

Notes: All dependent variables are log transformed. Standard errors presented in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional controls include: age, gender, marital status, household income, labour force status, household size, number of children, self-assessed health, and year and region fixed effects.

Table A3: Estimates of linear model with individual random effects of education on ratios of continuous financial variables to income

	Amount Saved/Income		Inv. income/Income		Unsecured debt /Income		Secured Debt/Income	
	Females	Males	Females	Males	Females	Males	Females	Males
Panel A - All Education Levels								
Age left Education	0.003*** (0.000)	0.004*** (0.000)	0.051*** (0.002)	0.050*** (0.002)	0.073*** (0.008)	0.078*** (0.009)	1.405*** (0.074)	1.093*** (0.077)
Observations	92,579	110,689	170,944	181,955	35,391	34,684	83,324	91,896
Panel B - Left School at 16 years or less								
Age Left Education	0.002*** (0.000)	0.005*** (0.000)	0.040*** (0.003)	0.045*** (0.004)	0.072*** (0.016)	0.063*** (0.019)	1.379*** (0.143)	1.876*** (0.169)
Observations	66,346	78,509	119,230	125,982	24,207	23,564	55,721	60,495

Notes: Standard errors presented in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional controls include: age, gender, marital status, household income, labour force status, household size, number of children, self-assessed health, and year and region fixed effects.

Table A4: Compulsory schooling effects on savings and Investments using local linear regression models - Fixed Bandwidths

		Female			Male				
		Do You Save?	Regular Saver?	Amount Saved	Investment Income	Do You Save?	Regular Saver?	Amount Saved	Investment Income
Panel A: 1947 reform									
	BW = 24	0.142*** (0.044)	0.154*** (0.050)	0.418** (0.189)	-0.542 (0.567)	-0.225 (0.218)	-0.218 (0.201)	-0.824 (1.540)	0.154 (1.039)
	BW = 36	0.128*** (0.046)	0.156*** (0.056)	0.461** (0.194)	-0.465 (0.495)	-0.096 (0.170)	-0.055 (0.154)	0.125 (1.000)	0.096 (1.079)
	BW = 72	0.082** (0.041)	0.095** (0.042)	0.314* (0.178)	-0.244 (0.359)	-0.014 (0.098)	0.003 (0.115)	0.233 (0.575)	0.082 (0.705)
Panel B: 1972 reform									
	BW = 24	0.080 (0.405)	0.568 (1.383)	0.558 (1.599)	-3.872 (6.452)	-0.230** (0.115)	-0.228 (0.143)	-0.558 (0.515)	-0.578 (0.638)
	BW = 36	0.013 (0.274)	0.151 (0.275)	-0.118 (1.354)	-3.861 (3.815)	-0.148 (0.127)	-0.218 (0.149)	-0.308 (0.560)	0.132 (0.641)
	BW = 72	0.025 (0.104)	0.097 (0.098)	0.065 (0.536)	-1.493* (0.823)	-0.046 (0.102)	-0.068 (0.106)	0.033 (0.473)	0.567 (0.598)

Notes: Table 4 gives the estimated effect of compulsory schooling law change on various outcomes. All local linear regressions estimated with triangular Kernels using pooled waves of the BHPS and Understanding Society. Robust standard errors clustered at date or birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Compulsory schooling effects on debts using local linear regression models - Fixed Bandwidths

	Female			Male		
	Debt	Amount Unsecured Debt	Amount Secured Debt	Debt	Amount Unsecured Debt	Amount Secured Debt
Panel A: 1947 reform						
BW = 24	-0.013 (0.034)	-0.124 (0.223)	-1.488 (1.076)	0.064 (0.100)	0.408 (0.772)	-0.001 (0.462)
BW = 36	0.033 (0.043)	0.136 (0.271)	-0.650 (0.631)	-0.014 (0.117)	-0.444 (1.009)	0.184 (0.695)
BW = 72	0.024 (0.034)	0.042 (0.206)	-0.496 (0.392)	0.023 (0.065)	-0.092 (0.524)	0.148 (0.497)
Panel B: 1972 reform						
BW = 24	0.105 (0.358)	-1.192 (3.033)	-3.079 (14.934)	-0.102 (0.173)	-1.049 (1.351)	-0.201 (0.518)
BW = 36	0.245 (0.325)	0.611 (2.010)	-1.329 (3.256)	0.001 (0.197)	-0.079 (1.554)	-0.635 (0.590)
BW = 72	0.071 (0.101)	0.397 (0.795)	0.052 (0.874)	-0.069 (0.169)	-0.659 (1.385)	-0.687 (0.845)

Notes: Table 5 gives the estimated effect of compulsory schooling law change on various outcomes. All regressions estimated with local linear regressions with triangular kernel weights using pooled waves of the BHPS and Understanding Society. Robust standard errors clustered at date of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Compulsory schooling effects on financial ratios using local linear regression models - Fixed Bandwidths

	Female			Male		
	Saving/Inc.	Secured/inc.	Unsec/inc	Saving/Inc.	Secured/inc.	Unsec/inc
Panel A: 1947 reform						
BW = 24	0.008* (0.005)	-3.365 (11.539)	0.052 (0.212)	0.082 (0.069)	2.580 (22.724)	-0.830 (0.955)
BW = 36	0.021** (0.011)	-5.210 (8.246)	0.007 (0.189)	-0.569 (0.669)	20.431 (25.757)	-1.194 (1.042)
BW = 72	0.009 (0.007)	-5.768 (7.042)	-0.073 (0.140)	0.248 (0.263)	1.220 (13.391)	0.019 (1.355)
Panel B: 1972 reform						
BW = 24	-8.614 (9.042)	1,378.304 (5,111.459)	1.632 (2.612)	-0.368** (0.178)	-681.799** (279.817)	-95.703 (58.686)
BW = 36	-7.078 (5.691)	-25.478 (256.124)	0.999 (1.345)	-0.364* (0.203)	-739.302** (312.773)	-12.363 (54.558)
BW = 72	-2.278 (1.781)	-115.900 (121.799)	0.800 (0.564)	-0.369* (0.219)	-748.656* (438.648)	60.297 (69.865)

Notes: Table 5 gives the estimated effect of compulsory schooling law change on various outcomes. All regressions estimated with local linear regressions with triangular kernel weights using pooled waves of the BHPS and Understanding Society. Robust standard errors clustered at date of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Appendix B

As a robustness check to the non-parametric approach presented in the main body of the text, we present the equivalent parametric results. Following Hahn et al. (2001) the estimation of the treatment effect τ on financial outcome Y proceeds using Two Stage Least Square (2SLS). Firstly, we estimate the jump in the amount of schooling, S , induced by the reforms, i.e., the first stage. The second stage estimates the change in financial outcome, Y , as a result of the change in the amount of schooling, S . Formally, the 2SLS is estimated by running these two equations:

$$S = \gamma + \delta T + g(X - c) + \theta Z + v, \quad (4)$$

$$Y = \alpha + \tau S + f(X - c) + \theta Z + u \quad (5)$$

where T is the indicator that takes the value of 1 if the individual is born from the cut-off date and 0 otherwise ($T = 1[X \geq c]$), $(X - c)$ is birth cohort measured in month relative to each cut-off date. In order to capture flexibly the relationship between birth cohort, amount of schooling and financial decisions, each model includes functions g and f , that is, different polynomial orders. We employ two different polynomials – linear and quadratic, but we report only linear models in what follows. The same polynomial function is used for both equations 4 and 5. To allow for different functional forms on either side of the cutoff, our model includes interaction terms between the indicator T and function g . Z represents a pre-determined set of controls such as calendar month and survey year fixed effects. We also include a vector of controls (interaction between year of survey and age, age-squared, calendar month of birth) to improve the precision of the estimates. As a consequence of the local randomisation assumption, the inclusion of controls should not affect the estimates but only improve their precision (Lee and Lemieux, 2010).

Our estimation proceeds by adopting different bandwidths, the window of observations around the cut-off dates, namely in our case the number of months. Higher order polynomials might overestimate the effects because of over-fitting and are not used in the paper, see, for example, Gelman and Imbens (2014). Finally, in order to overcome the concerns relating to the inclusion of individual fixed effects in RDD, we follow the recommendation set out by Lee and Lemieux (2010) by considering that the source of identification is the local randomisation exerted by individuals being born few months apart, we ignore the panel structure of the data, and carry out the estimation with a single cross-section. Controlling for clustering is thus particularly important as there are potentially two sources for serial correlation: over time within the same individual or across individuals within the same month of birth. We present results using standard errors clustered at month birth level. For robustness purposes we re-estimated all the models using standard errors

clustered at the household level too. Results are similar to the ones presented here and are available upon request.

Table B1: Compulsory schooling effects on savings and investments using RDD parametric models

		Female						Male							
		Do you save?	Regular Saver?	Amount Saved	Investment Income	Do you save?	Amount Saved	Regular Saver?	Amount Saved	Investment Income	Do you save?	Amount Saved	Regular Saver?	Amount Saved	Investment Income
Panel A: 1947 reform															
BW=24		0.144 (0.116)	0.176 (0.109)	0.329 (0.438)	-0.807 (0.694)	-0.252 (0.321)	-0.031 (0.220)	0.149 (1.381)	-0.031 (0.220)	1.528 (1.059)					
BW=36		0.092 (0.064)	0.105* (0.059)	0.357 (0.278)	-0.473 (0.440)	0.038 (0.316)	0.590 (1.161)	2.246 (2.917)	0.590 (1.161)	1.261 (1.375)					
BW=72		0.087 (0.056)	0.079 (0.050)	0.323 (0.257)	-0.109 (0.335)	0.020 (0.103)	0.057 (0.115)	0.369 (0.551)	0.057 (0.115)	-0.136 (0.638)					
Panel B: 1972 reform															
BW=24		0.418 (1.486)	-0.028 (0.784)	-0.062 (4.054)	-15.184 (43.234)	-0.266 (0.197)	-0.186 (0.185)	-0.801 (0.877)	-0.186 (0.185)	-1.111 (1.032)					
BW=36		-0.170 (0.523)	0.001 (0.283)	-1.310 (2.453)	-5.985 (4.828)	0.042 (0.162)	-0.027 (0.167)	0.531 (0.875)	-0.027 (0.167)	0.743 (0.959)					
BW=72		-0.032 (0.115)	0.012 (0.099)	-0.172 (0.599)	-1.928** (0.877)	0.055 (0.123)	0.078 (0.098)	0.559 (0.669)	0.078 (0.098)	0.630 (0.714)					

Notes: Table gives the estimated effect of compulsory schooling law change on various outcomes. All regressions estimated with linear regressions using pooled waves of the BHPS and Understanding Society. Robust standard errors clustered at date of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Compulsory schooling effects on debts using RDD parametric models

		Female			Male		
	Debt	Amount Unsecured Debt	Amount Secured Debt	Debt	Amount Unsecured Debt	Amount Secured Debt	
Panel A: 1947 reform							
BW=24	0.094 (0.078)	-0.528 (0.664)	0.416 (0.465)	0.068 (0.137)	-0.307 (0.982)	0.150 (1.100)	
BW=36	0.094* (0.052)	-0.288 (0.379)	0.507 (0.317)	0.028 (0.318)	-0.897 (2.161)	-1.805 (4.707)	
BW=72	0.084** (0.043)	-0.439 (0.319)	0.316 (0.257)	0.073 (0.072)	-0.363 (0.501)	0.414 (0.569)	
Panel B: 1972 reform							
BW=24	-0.521 (1.391)	1.663 (12.758)	-7.498 (18.677)	0.213 (0.197)	1.338 (0.849)	1.806 (1.526)	
BW=36	0.322 (0.440)	1.519 (2.315)	1.329 (2.607)	0.211 (0.181)	1.322 (0.995)	1.599 (1.425)	
BW=72	0.047 (0.100)	1.118 (0.949)	0.280 (0.754)	0.135 (0.124)	0.795 (1.026)	1.010 (1.039)	

Notes: Table gives the estimated effect of compulsory schooling law change on various outcomes. All regressions estimated with linear regressions using pooled waves of the BHPS and Understanding Society. Robust standard errors clustered at date of birth level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.