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

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Abstract

This paper uses two compulsory schooling reforms in Britain (1947 and 1972) to study the relationship between education and financial behaviours. Employing a regression discontinuity design to analyse nationally representative data from the UK, 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 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; Financial Literacy; Financial Outcomes; Regression Discontinuity.

JEL classification: D14; I20; G11.

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1 Introduction

This study uses a quasi-experimental research design 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 than in the past; 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 Britain 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 (Haliassos and Bertaut, 1995; Rosen and Wu, 2004; Van Rooij et al., 2011; Browning and Lusardi, 1996; Campbell,

[1] Association of British Insurers, 2015.

2006; Gross and Souleles, 2002; Brown and Taylor, 2008).

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, improve financial outcomes at the individual level, then this positive spill-over should be taken into account in a cost-benefit analysis of education policies. Existing evidence on the causality is rather limited; to our knowledge only Cole et al. (2014) provides evidence of a causal relationship between the amount of 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.

Our study proposes the use of a regression discontinuity design (RDD) to investigate the causal effect of education on a set of financial decisions using rich survey data from Britain. We exploit the exogenous variation in the amount of education from two compulsory schooling laws in England and Wales. The first reform implemented in 1947, known as the Butler Act, increased the minimum schooling-leaving age from 14 to 15 – affecting cohorts born after April 1933 – and the second reform, enforced in 1972, increased the school leaving age from 15 to 16 – affecting cohorts born after September 1957. These reforms allow the estimation of the effect of an exogenous change of one additional year of education and, in addition, enable the study of such policy reforms on financial behaviours.

We improve upon the existing literature on four levels. Firstly, the British reforms we exploit 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; Oreopoulos, 2006).[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 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, these reforms are important because they have been proven to affect other outcomes, such as earnings (Devereux and Hart, 2010), cognitive abilities (Banks and Mazzonna, 2012) and risky behaviours (Wilson, 2014) amongst many other outcomes.[3] Thus it is straightforward to assume that the extra year of schooling as a result of the educational reforms might have had an impact on financial behaviours either directly or indirectly.

Third, we employ a RDD framework, 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.

[2]We 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.

[3]See for example, Dickson and Smith (2011), Buscha and Dickson (2012), Oreopoulos (2006) and Jürges et al. (2013) amongst many others.

Fourth, we use a sample from two nationally representative surveys of UK households surveyed between 1991 and 2014, namely, the British Household Panel Study (BHPS) and its follow-up, Understanding Society - the UK Household Longitudinal Study (UKHLS). On the one hand, these surveys offer a rich set of financial variables that provides a more complete picture of each individual's financial position; on the other, this data provides a detailed set of personal information, including the month of birth.[4] The availability of the month of birth allows us to precisely identify the cohort affected by the educational reforms and therefore reduce the potential measurement error. It is worth noting that this is an improvement with respect to both the education-financial behaviour literature and, more generally, most of the papers adopting a RDD strategy, given that the majority uses only the year of birth as a cut-off (Devereux and Hart, 2010; Harmon and Walker, 1995).

Our study highlights significant divergencies between simple regression models and the RDD estimation strategy. The results relating to simple regression estimations indicate that, in line with the existing literature, the level of schooling is highly correlated with individuals' financial decisions. For instance, the higher the educational attainment, the higher the individual's propensity to save 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, 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, although there is no statistically significant effect on the amount saved. These findings do not hold for the later reform.

Finally, the marked difference between the RDD and the OLS results points in the direction of the presence of significant omitted variables - such as ability, family back-

[4]The BHPS and UKHLS have been used by researchers to study a variety of outcomes in a RDD framework, see for example, Brunello et al. (2013). We use a range of financial variables included in the survey, including for example; propensity to save, amount saved, investment income and levels of secured and unsecured debt.

ground 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 provides a background on the schooling laws in England and Wales. Section 4 provides details on the estimation and identification strategy, while Section 5 describes the data used in the analysis. Section 6 presents the results, which are discussed in Section 7 and, finally, Section 8 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.[5] For instance, more educated people are more likely to earn more (Oreopoulos, 2006), 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).

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.,

[5]For a recent review see Oreopoulos and Salvanes (2011).

2001), while more recent work reporting no effect on financial behaviour (Cole et al., 2016).

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 (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).

However, when selection bias is taken into account, these relationships could be also consistent with a completely opposite view if one considers that preferences can shape the amount of education as well as being shaped by it. In sum, if the level of schooling is endogenous; then the results 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 compulsory schooling reforms. These 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. This approach is explained in detail in Section 4.

3 Compulsory Schooling Laws in England and Wales

This paper utilises two reforms that raised the minimum school leaving age in England and Wales 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).

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 after 1st April 1933. 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 (Oreopoulos, 2006). 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 before the age of 17. 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 born after 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.[6] 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 2 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, Oreopoulos, 2006 and 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

[6]See for example, Chevalier et al. (2004) and Dickson and Smith (2011).

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 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 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 after April 1933 or September 1957, 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 2.

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:

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

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. We define "treated" as only those households in which all members are born after the cut-off date and as "untreated" those households in which all members are born before the cut-off date. More precisely, we exclude those households in which at least one member received more education while others did not.[7]

[7] We do this irrespectively to whether the household is considered to be a couple or just individuals leaving together.

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 head of the household and yield quantitatively and qualitatively similar results to those reported hereafter.[8]

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). 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 in the CCT method 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, they add a regularization term that ensures the denominator does not become too small. In practice in our case, the bandwidths without

[8]Results are available upon request.

regularizations are much larger but never infinite.

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 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.[9] 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). Because people enter the data in different periods, their financial behaviour is observed at different age-year cells, we include age, age-squared, year of survey and month of birth.[10]

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 present parametric estimates using a linear regression with clustered standard errors

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

[10] A full explanation of the covariate adjusted RD estimator is presented in Calonico et al. (2016b). These nonparametric models are estimated using the Stata package *rdrobust* by Calonico et al. (2014a) and Calonico et al. (2016a).

at month of birth and four different bandwidths in Appendix B.

4.1 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. One may argue that the content of the education received might have also changed, so that the effect of an extra year of schooling confounds both quantity and quality of education. Every education reform comes with a portfolio of different education interventions, of which raising the schooling minimum age is just one. The first reform for instance 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 schooling in many ways.[11] 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.[12] We are unaware of any other interventions linked to financial decisions that might have changed in correspondence with those laws.

5 Data

This paper draws on a sample of respondents based in England and Wales. This sample is taken from from two large nationally representative household panel surveys of UK households covering the period 1991-2014; these are namely Understanding Society - The

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

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

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. Table 1 provides a list of the variables used and the respective questions asked to the individual or the household.

Understanding Society is a longitudinal survey conducted in the UK, which builds on the BHPS. The first wave was initiated in 2008 and we use data from across 5 waves in the current paper. It 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 and 4 of Understanding Society, 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. Wave 4 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 and 5 of the UKHLS, where they 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?"*[13] 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 of birth, which is a substantial improvement with respect to using just year of birth. The schooling variable included in the surveys is "age left full time education", which is equivalent to completed years of education in Britain (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. More specifically, investment income and secured debt are at the household level. The latter refers to the outstanding debt on the property. Summary statistics of the dependent variables, in addition to the school leaving age, are presented in Table 2.

As explained in Section 4 our sample is composed of only individuals living in households in which every adult member has been either treated (all born after the reform) or control (all born before the reform) to avoid cross-contamination as financial decisions might be made by other members such as heads of household that might not have been affected by the reform.[14]

Prior to the discussion of the results and in order to illustrate the association between

[13]We purposely leave out any questions related to the value of the mortgage and 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.

[14]Analyses using only head of households born either before or after the reform provides similar results but are not reported.

each financial outcome and education, Figure 1 plots the correlation between age individual left school and various financial outcomes. The fitted lines are the predictions from regressions of each financial outcome Y on the age at which the individual left education controlling for 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).[15] 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), an individual that left school at the age of 18 will save on average more than double than an individual that left school at 15.[16]

6 Results

6.1 Compulsory Schooling and 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 3 to Figure 6 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 of individuals leaving before the compulsory age after the cut-off date, increasing the average school

[15]Estimates are reported in Appendix A.

[16]These 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, 1991-2014. 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 by Michael Stepner (https://michaelstepner.com/binscatter/).

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

Table 3 reports estimates of the first stage 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 the second stage.

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 0.5 years for the the 1947 reform and between 0.06 and 0.15 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 British laws is much 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, the first stage 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.

6.2 Compulsory Schooling, Savings and Investments

We now turn our attention to exploring the effects that 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 reform increased the probability that females reported that they save and that they saved regularly by around 18.6 and 19.4 percentage points, respectively. Upon using a large 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 statistically insignificant at the usual confidence level, even when different bandwidths are chosen. 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. Similar patterns are obtained when the parametric approach is implemented (see Table B1).

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.

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. There is 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.

6.3 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 overtime. 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.

7 Discussion

The aim of this section is to reconcile and provide a possible explanation of why the results for Britain 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. In order to estimate the first stage, that is, the amount of schooling brought about by these reforms, he regresses years of schooling on separate dummies that capture the mandate of years of schooling in place in an individual's state of birth when the person turns 14 years of age, that is, eight years or fewer, nine, ten and eleven or more. Table 4 in 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. For instance, compulsory schooling could have increased the amount saved (in the previous month) by £5 to £50. Nevertheless. results related to female 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 compulsory schooling laws. One of the underlining 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 it 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) and 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). 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 *naive* 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, 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.[17] Given that the reforms were 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. 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

[17] Albeit 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.

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 take on board 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.

8 Concluding Remarks

This paper considered the impact of increases in the school leaving age on a range of financial outcomes, analysing large nationally representative UK household surveys, the BHPS and Understanding Society. The paper documents a very strong correlation between education and a range of financial outcomes using simple cross-sectional regression techniques. When we explore causality using an RDD setting, our results showed a strong and positive effect of the level of education on saving behaviours (both saving and being a regular saver) for females relating to 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.

The marked difference between the RDD and the 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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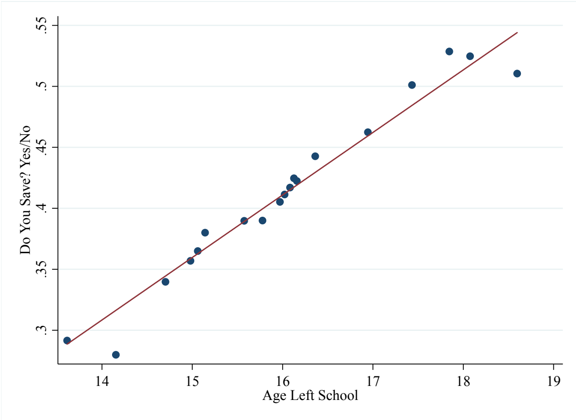

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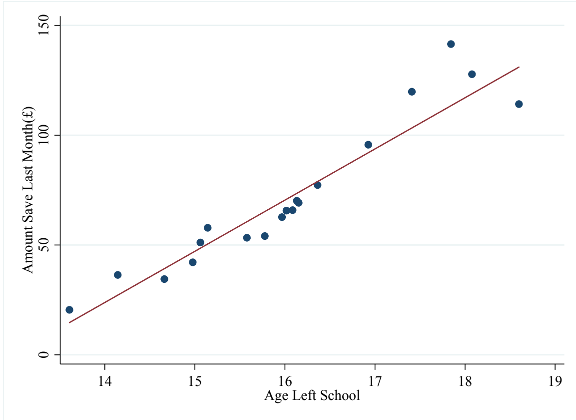

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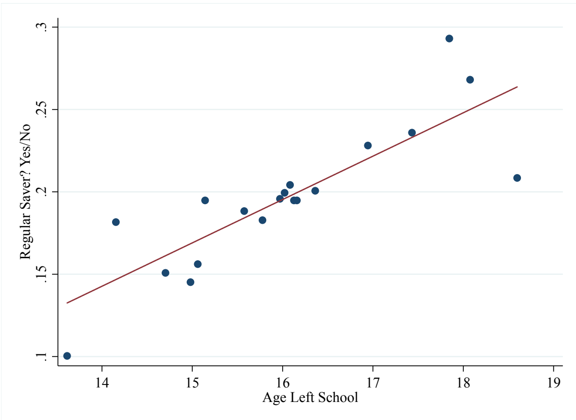

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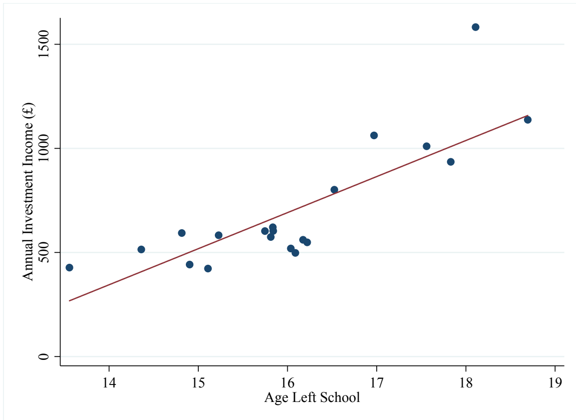

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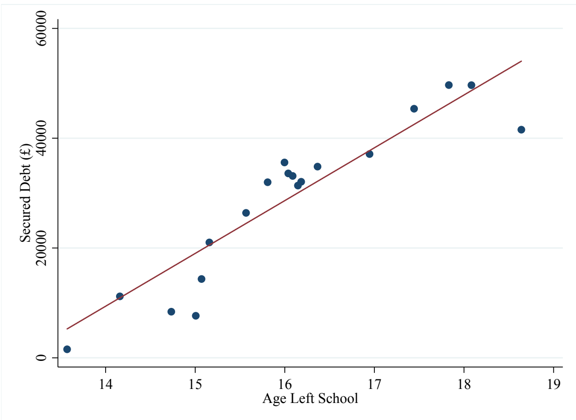

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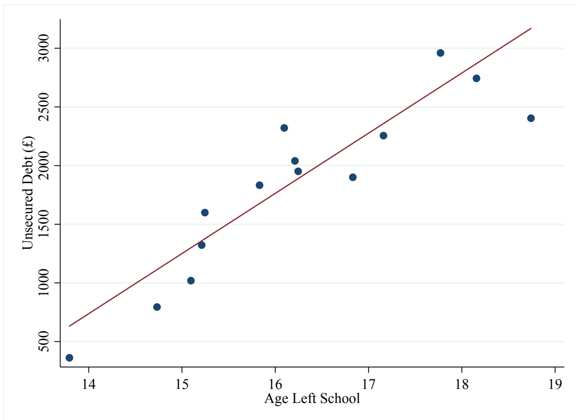

(f)

Figure 1: Correlation between age individual left school 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 controlling for year of survey. Each regression includes individual and household characteristics: age, age squared, log of income of the household, employment status, household size and self-assessed health status. The dots in each graphs are means at each age. The graph has been obtained using the Stata routine -binscatter- by Michael Stepner.

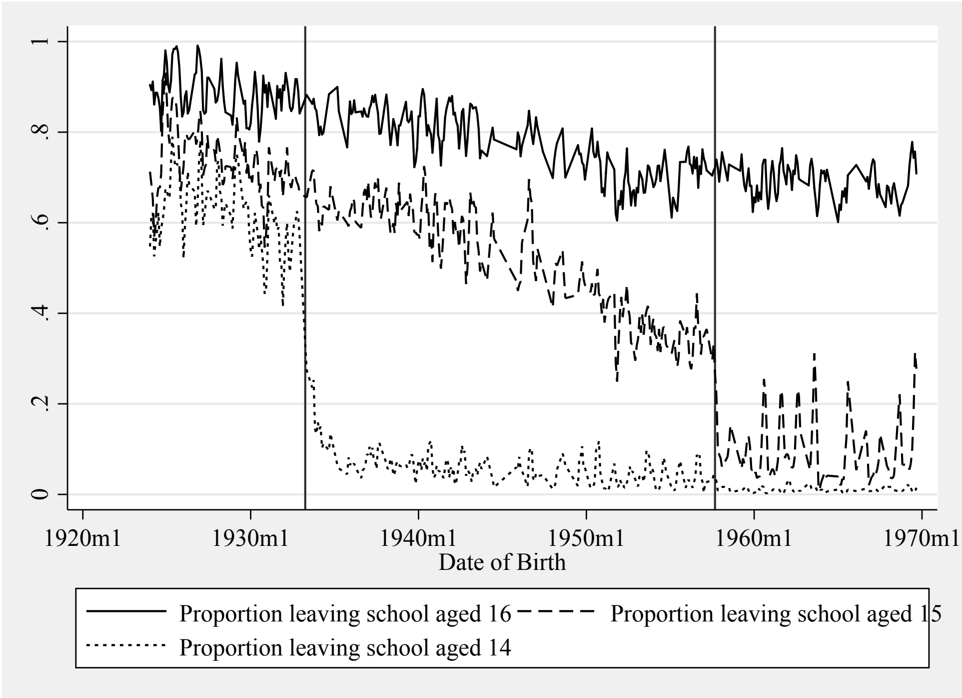

Figure 2: Years of full-time schooling

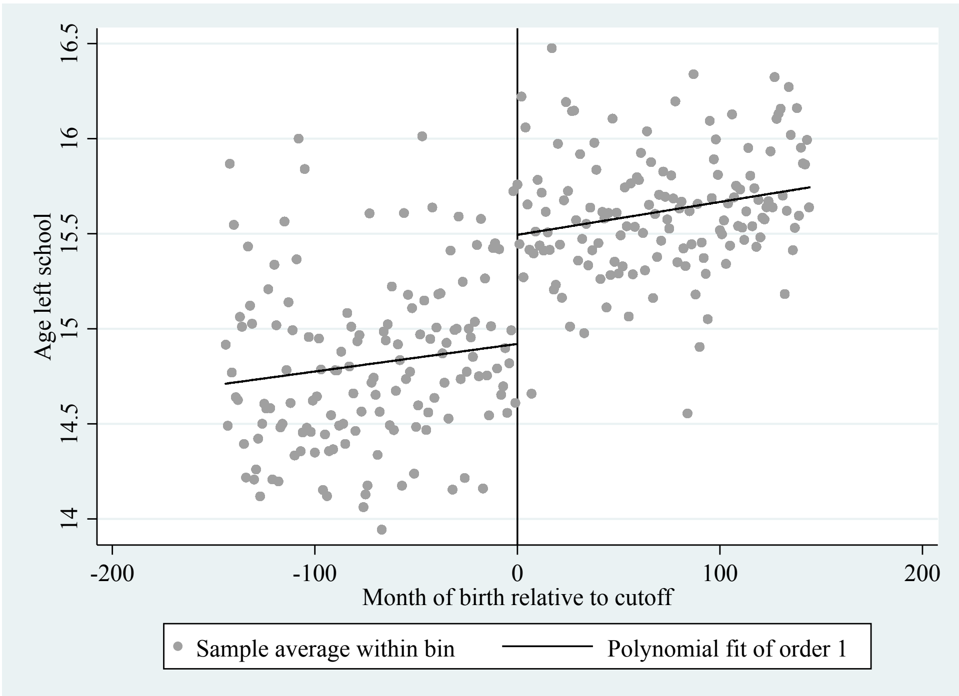

Figure 3: Average School Leaving Age by Month of Birth for Females - 1947 Reform

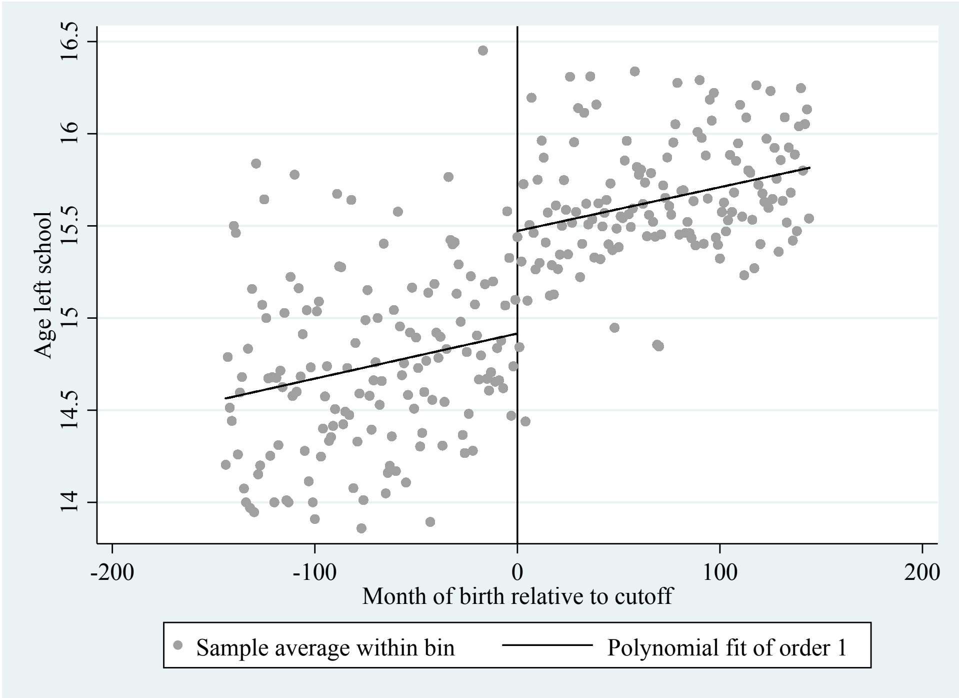

Figure 4: Average School Leaving Age by Month of Birth for Males - 1947 Reform

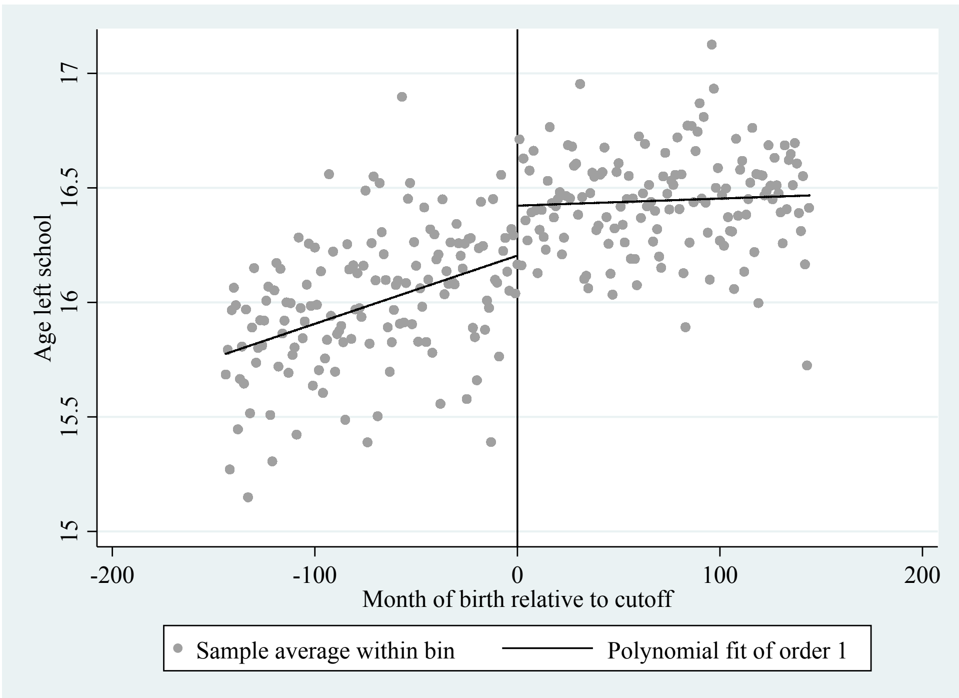

Figure 5: Average School Leaving Age by Month of Birth for Females - 1972 Reform

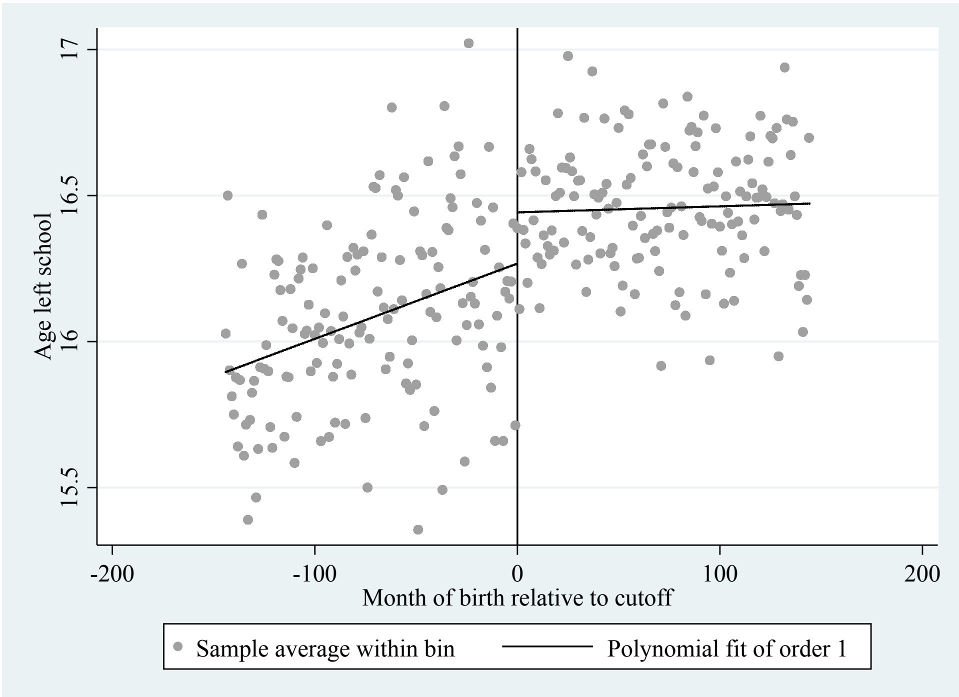

Figure 6: Average School Leaving Age by Month of Birth for Males - 1972 Reform

Table 1: Financial outcomes from BHPS and Understanding Society

Variable	Wording of survey questions and answer' codes	Waves	Level
	Savings and Investment		
Do you save?	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	Individual
Regular Saver	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	Individual
Amount Saved	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	Individual
Investment Income	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	Household
	Debt		
Debt	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 (43)?	- - - - E - - - - J - - - - O - - -, 4	Individual
Unsecured Debt	About how much in total do you owe?	- - - - E - - - - J - - - - O - - -, 4	Individual
Secured Debt	Could I just check, 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	Household

Notes: Waves A (1991) to R (2008) are from the British Household Panel Data, waves $1-5$ (2008 - 2015) are from Understanding society.

Table 2: Summary Statistics

	1947 Reform									
	Female					Male				
Variable	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Do you save?	30740	0.365	0.481	0	1	25909	0.387	0.487	0	1
Regular saver	30738	0.148	0.355	0	1	25904	0.161	0.368	0	1
Amount saved	29063	36.534	126.709	0	10000	24403	58.585	173.234	0	6000
Investment income	38558	926.226	3230.125	0	233920	33558	1133.692	3412.91	0	210000
Debt	7242	0.121513	0.326	0	1	6120	0.137418	0.344	0	1
Amount unsecured debt	7188	226.821	2777.752	0	132000	6071	659.598	7755.760	0	471000
Amount secured debt	26227	4241.992	23219.310	0	1000000	22302	5863.571	37509.980	0	3800000
Year left school	38648	15.322	1.300	8	20	33588	15.335	1.359	7	26
Year	42455	14.165	6.945	1	23	36543	14.111	7.002	1	23
Age	42455	69.131	8.904	44	92	36543	68.794	8.804	46	93
	1972 Reform									
	Fmale					Male				
Variable	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Do you save?	46781	0.455	0.498	0	1	55754	0.434	0.495	0	1
Regular saver	46778	0.229	0.420	0	1	55752	0.224	0.417	0	1
Amount saved	44857	108.907	475.220	0	50000	53883	70.356	246.190	0	30000
Investment income	66107	953.251	17353.840	0	4200000	77510	660.680	4115.797	0	350000
Debt	12063	0.359	0.480	0	1	14580	0.345	0.475	0	1
Amount unsecured debt	11784	2415.841	12265.730	0	590000	14313	1286.742	5939.206	0	400000
Amount secured debt	41161	39157.330	104122.400	0	1.12E+07	47670	34444.550	70007.4	0	5006000
Year left school	63742	16.278	1.195	6	23	75485	16.233	1.176	9	24
Year	69782	15.270	6.683	1	23	82921	15.609	6.583	1	23
Age	69782	47.560	9.6263	22	69	82921	47.734	9.550	22	69

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				
BW= CCT	0.545***	-0.301***	-0.004	-0.002
	(0.081)	(0.021)	(0.031)	(0.0026)
N	35,447	38,927	38,927	38,927
BW	144			
Panel B: 1972 reform				
BW= CCT	0.149***		-0.198***	0.041**
	(0.053)		(0.020)	(0.021)
N	66,995		73,550	73,550
BW	144			

	Male			
	Age Left School	<= 14 Leaving Age	<= 15 Leaving Age	<= 16 Leaving Age
Panel A: 1947 reform				
BW= CCT	0.525***	-0.243***	-0.028	0.005
	(0.096)	(0.023)	(0.034)	(0.028)
N	30,280	32,930	32,930	32,930
BW	144			
Panel B: 1972 reform				
BW= CCT	0.068***		-0.726***	0.123*
	(0.021)		(0.078)	(0.065)
N	55,037		55,037	55,037
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 in 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	0.186**	0.194**	3.162	-1033.8	-0.121	-0.107	51.309	-593.1
	(0.080)	(0.077)	(11.596)	(635.71)	(0.179)	(0.076)	(62.68)	(970.61)
N	4919	4715	6976	6962	5317	4727	5319	4110
BW	26	25	40	29	33	29	34	20
BW=no reg	0.143**	0.182**	4.766	-1044.785	-0.016	-0.043	43.132	353.459
	(0.07)	(0.072)	(9.107)	(617.556)	(0.075)	(0.063)	(38.058)	(465.055)
N	15858	6455	11391	9405	15761	7443	10491	5869
BW	85	35	64	40	95	46	67	28
Panel B: 1972 reform								
BW= CCT	-1.489	1.86	20.65	11927	-0.003	-0.082	34.541	1607.7**
	(4.471)	(44.12)	(190.07)	(16906)	(0.111)	(0.058)	(38.038)	(566.38)
N	7280	5939	8059	7051	12300	9872	6143	8730
BW	43	18	45	16	41	32	20	20
BW=no reg	-0.570	-0.288	23.293	-32.218	0.089	-0.018	21.11	300.94
	(1.012)	(1.025)	(67.942)	(614.84)	(0.099)	(0.058)	(46.06)	(512.64)
N	9475	6941	13991	36805	43325	15618	10331	37799
BW	29	43	43	79	318	53	35	92

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	0.503	-180.88	-1206.3	-.0.089	3980	-18074
	(0.048)	(529.47)	(2144)	(0.181)	(6373.6)	(17787)
N	6491	2064	4465	5536	697	4320
BW	51	48	27	37	19	29
CCT no reg	0.031	-359.09	-2609.9	0.055	-144.47	-5626.6
	(0.031)	(395.01)	(1256.6)	(0.080)	(931.55)	(7553.8)
N	6491	3870	17010	3056	1465	9818
BW	162	89	102	81	39	66
Panel B: 1972 reform						
CCT	0.092	-4254.6	48271	-0.164	-359.97	-9906.9
	(0.261)	(4809.4)	(1.1e+05)	(0.162)	(2253.8)	(6923.4)
N	1761	2196	6551	2220	2321	4487
BW	20	26	23	30	31	17
CCT no reg	0.033	-322.63	-8853.7	-0.228	118.29	-18034
	(0.247)	(1285.5)	(17033)	(0.150)	(2302.9)	(8647)
N	2017	3238	14266	10879	5704	6913
BW	24	48	47	93	79	25

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 A

Table A1: Probit estimates of education on binary financial outcomes

	(1) Do you save?	(2) Regular Saver?	(3) Debt
Age Left School	0.074***	0.063***	0.008
	(0.006)	(0.007)	(0.008)
Age	-0.006***	0.004	0.040***
	(0.002)	(0.002)	(0.003)
Age Squared	0.008***	-0.004	-0.068***
	(0.002)	(0.002)	(0.004)
Female	0.068***	0.071***	-0.088***
	(0.014)	(0.017)	(0.020)
Married	0.315***	0.320***	0.046***
	(0.012)	(0.015)	(0.012)
Ln(Income)	0.500***	0.522***	0.259***
	(0.016)	(0.021)	(0.024)
Employed	0.402***	0.294***	0.178***
	(0.024)	(0.031)	(0.036)
Self-Employed	-0.411***	-0.421***	-0.064
	(0.027)	(0.040)	(0.042)
Unemployed	0.189***	0.244***	-0.078**
	(0.021)	(0.028)	(0.038)
Retired	-0.037***	0.018*	0.081***
	(0.008)	(0.010)	(0.011)
N. of Children	-0.122***	-0.143***	-0.046***
	(0.006)	(0.008)	(0.009)
Household Size	-4.688***	-5.319***	-0.984***
	(0.140)	(0.173)	(0.176)
Observations	182,718	115,714	34,677

Notes: Robust standard errors clustered at individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: OLS estimates of education on continuos financial variables

	(1) Amount Saved	(2) Inv. income	(3) Unsecured debt	(4) Total Debt	(5) Investments
Age Left School	0.011*	0.018***	0.044***	0.812**	0.038***
	(0.006)	(0.004)	(0.013)	(0.323)	(0.002)
Age	0.002*	0.002***	0.003	0.165**	0.009***
	(0.001)	(0.001)	(0.002)	(0.071)	(0.001)
Age Squared	-0.002*	-0.002**	-0.005***	-0.187***	-0.007***
	(0.001)	(0.001)	(0.002)	(0.071)	(0.001)
Female	0.028*	0.021**	0.055**	2.436**	0.025***
	(0.016)	(0.010)	(0.025)	(1.017)	(0.005)
Married	-0.126*	-0.051*	-0.280***	-6.938**	0.052***
	(0.072)	(0.027)	(0.076)	(2.925)	(0.003)
Ln(Income)	0.062*	-0.010	0.117***	3.822**	0.014**
	(0.035)	(0.008)	(0.041)	(1.539)	(0.006)
Employed	0.098*	0.028	0.164***	6.071***	0.020**
	(0.057)	(0.020)	(0.036)	(2.336)	(0.010)
Self-Employed	-0.020*	-0.028***	-0.037	-0.287	-0.022**
	(0.011)	(0.010)	(0.034)	(0.258)	(0.009)
Unemployed	0.013	0.015**	-0.023*	-0.311	0.055***
	(0.008)	(0.007)	(0.014)	(0.213)	(0.009)
Retired	-0.028*	-0.020**	-0.056***	-1.360*	0.012***
	(0.016)	(0.009)	(0.021)	(0.723)	(0.003)
N. of Children	0.023*	0.013*	0.041**	1.196*	-0.024***
	(0.013)	(0.007)	(0.017)	(0.619)	(0.002)
Household Size	0.951*	0.147	1.922***	46.013**	-1.016***
	(0.552)	(0.157)	(0.464)	(19.678)	(0.044)
Observations	188,931	188,931	33,498	30,414	33,710
R-squared	0.005	0.004	0.034	0.030	0.147

Notes: Robust standard errors clustered at individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.All dependent variables in columns 1-7 are deflated by the level of income.

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:

$$S = \gamma + \delta T + g(X - c) + \theta Z + v, \tag{2}$$

$$Y = \alpha + \tau S + f(X - c) + \theta Z + u \tag{3}$$

where T is the indicator that takes the value of 1 if the individual is born after 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 2 and 3. 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 a part, 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?	Amount Saved	Regular Saver?	Investment Income	Do you save?	Amount Saved	Regular Saver?	Investment Income
Panel A: 1947 reform								
BW= 24	0.209*	-0.071	0.258*	-1,204.259	-0.192	15.837	-0.087	500.437
	(0.125)	(16.474)	(0.148)	(847.427)	(0.174)	(43.328)	(0.083)	(686.297)
BW=36	0.100	3.100	0.121**	-1,264.255*	-0.085	56.135	-0.029	1,234.540
	(0.067)	(12.298)	(0.060)	(650.029)	(0.197)	(71.293)	(0.104)	(1,139.464)
BW=72	0.087	-3.357	0.089**	-306.824	0.021	30.518	0.005	986.386**
	(0.058)	(12.572)	(0.041)	(387.672)	(0.079)	(25.310)	(0.043)	(475.644)
BW=108	0.061	-5.719	0.058**	-131.645	-0.053	1.036	-0.015	689.206*
	(0.048)	(9.769)	(0.029)	(311.679)	(0.051)	(16.680)	(0.026)	(355.429)
Panel B: 1972 reform								
BW= 24	-0.323	6.144	-0.125	2,166.654	-0.095	33.181	-0.098	1,078.712
	(0.564)	(103.730)	(0.243)	(2,587.972)	(0.131)	(54.297)	(0.074)	(778.314)
BW=36	-0.066	4.300	0.050	-1,084.282	0.063	18.683	-0.019	990.258
	(0.181)	(70.085)	(0.101)	(1,274.627)	(0.100)	(57.356)	(0.055)	(760.367)
BW=72	0.029	-37.069	0.053	80.629	0.141	48.570	0.043	32.277
	(0.075)	(36.379)	(0.046)	(604.931)	(0.102)	(59.527)	(0.057)	(690.305)
BW=108	0.091	-73.602	0.092	-24.562	0.041	0.773	0.011	-694.532
	(0.087)	(53.611)	(0.058)	(750.881)	(0.096)	(63.811)	(0.057)	(820.520)

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: Compolsory 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	0.107	-773.423	-100.288	-0.007	-18,141.471	745.607
	(0.122)	(2,717.811)	(976.221)	(0.110)	(20,831.627)	(1,066.202)
BW=36	0.097	-1,374.982	-97.189	-0.078	-20,765.735	-790.161
	(0.074)	(1,845.570)	(593.423)	(0.207)	(28,818.307)	(1,913.740)
BW=72	0.003	-3,541.595**	-495.257	0.067	-2,038.188	-481.839
	(0.051)	(1,673.672)	(467.102)	(0.070)	(4,877.850)	(944.920)
BW=108	0.018	-385.980	-3,753.777**	0.033	-1,151.808	-570.559
	(0.042)	(321.958)	(1,792.951)	(0.044)	(2,971.102)	(984.316)
Panel B: 1972 reform						
BW= 24	-0.216	14,466.016	-2,660.224	-0.146	-12,896.732	-1,392.343
	(0.297)	(38,183.192)	(4,246.738)	(0.167)	(16,573.168)	(2,521.409)
BW=36	0.119	-16,718.466	688.315	-0.153	-14,027.480	-345.499
	(0.164)	(19,786.828)	(1,606.562)	(0.154)	(23,915.682)	(3,100.758)
BW=72	-0.043	-8,479.547	96.876	-0.259	14,537.757	383.670
	(0.077)	(10,417.514)	(729.544)	(0.189)	(14,298.760)	(2,806.579)
BW=108	0.034	-5,379.532	1,606.131	-0.269	25,073.424	1,350.718
	(0.101)	(12,642.830)	(990.111)	(0.186)	(17,129.583)	(3,344.787)

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; *** $p<0.01$, ** $p<0.05$, * $p<0.1$.