# Does longer compulsory schooling affect mental health? Evidence from a British reform *

Mauricio Avendano[a, b], Augustin de Coulon[c, d, e] and Vahé Nafilyan[c, d, f †]

[a]*Global Health and Social Medicine, King's College London, UK*
[b]*Harvard School of Public Health, Boston, MA, USA*
[c]*King's Business School, King's College London, UK*
[d]*Centre for Vocational Education Research LSE, London, UK*
[e]*IZA, Germany*
[f]*Office for National Statistics, Newport, UK*

**Abstract**

In this paper, we examine whether longer compulsory schooling has a causal effect on mental health, exploiting a 1972 reform which raised the minimum school leaving age from age 15 to 16 in Great Britain. Using a regression discontinuity design, we find that the reform did not improve mental health. We provide evidence that extending the duration of compulsory schooling impacts mental health through channels other than increased educational attainment. We argue that these effects may mitigate or offset the health returns to increased educational attainment.

**Keywords**: Mental health, education, compulsory schooling, UK
**JEL**: I12, I21, I28

*We thank the editor Jonah Rockoff and two anonymous referees for helpful comments. We also thank Brian Bell, Tim Bruckner, Arnaud Chevalier, Emilie Courtin, Robynne Davies, Jennifer Dowd, Alice Goisis, Steven McIntosh as well as participants to seminars and conferences at LSE, King's College, UCSF, and ESPE for useful comments. We acknowledge the UK Data Service for liaising on our behalf with the Office for National Statistics (ONS). Access to the Special User License Annual Population Survey was granted by the ONS Microdata Release Panel. This research has also been conducted using the UK Biobank Resource (ID: 47464). Avendano is supported by grant 633666 (Lifepath) from the European Commission Horizon 2020 Programme. Nafilyan acknowledges financial support from the Economic and Social Research Council (ESRC-KISS DTC). Declarations of interest: none.

†Corresponding author at: King's Business School, Bush House, London WC2B 4BG, UK, Email addresses: mauricio.avendano_pabon@kcl.ac.uk, augustin.decoulon@kcl.ac.uk and vahe.nafilyan@kcl.ac.uk

# 1 Introduction

There is a strong, positive relationship between education and health in most countries regardless of their level of development (Cutler and Lleras-Muney, 2012). The strength of this association suggests that education policies may improve health and be more cost-effective than healthcare-based interventions (Woolf et al., 2007, Cohen and Syme, 2013). Following Lleras-Muney (2005), a growing number of studies have exploited changes in compulsory schooling laws to identify the causal effect of increasing educational attainment on mortality and health outcomes.[1] Overall, this literature has yielded contradictory results, suggesting that the effects of education on mortality and health differ depending on the country and context (Galama et al., 2018). Mental health has received less attention in the literature, despite the high prevalence of mental health problems among those with low levels of education.[2]

In this paper we provide new evidence on the mental health effect of extending the duration of compulsory schooling. From a theoretical perspective, it is unclear how extending compulsory schooling would impact mental health. Longer schooling may improve mental health through better labour market outcomes and health inputs (such as more physical exercise, earlier detection, and better access to psychological therapy). However, additional compulsory schooling may imply high opportunity costs for low achievers who would otherwise have gained valuable work experience by leaving school earlier. This may have important consequences for their mental well-being. Remaining in a stressful and competitive environment may also involve substantial emotional and psychological distress for low achievers. The net effect of raising the minimum school leaving age depends on the reasons why adolescents choose to leave school early. If dropping out is the optimal choice for a sub-group, economic theory would predict that extending compulsory schooling will generate negative returns (Galama et al., 2018). The mental health effect of an additional year of education due to an increase in the minimum school leaving age may, therefore, depend on the opportunity costs of leaving school a year later. It may also be very different from the effect of remaining at school for an extra year by choice.

Our paper focuses on the effects of the 1972 Raising of the School Leaving Age Order (ROSLA) on mental health. Whilst Clark and Royer (2013), Davies et al. (2018) and Janke et al. (2018) analysed the health effect of British compulsory schooling laws on mortality and physical health, they did not focus on mental health. We estimate the effect of the reform on a comprehensive range of mental health outcomes, using three datasets: the Annual population Survey (APS), the largest household survey in the UK; Understanding Society, the largest UK

[1] See for instance Oreopoulos (2006), Mazumder (2008), Albouy and Lequien (2009), van Kippersluis et al. (2011), Kemptner et al. (2011), Cipollone and Rosolia (2011), Fischer et al. (2013), Brunello et al. (2013), Clark and Royer (2013), Gathmann et al. (2015), Fletcher (2015), James (2015), Davies et al. (2018).

[2] Mental health conditions are among the leading contributors to the global burden of disease (Vos et al., 2015). In 2015, 17.9 percent of all adults had a mental illness in the United States and 14.2 percent received mental health care in the past 12 months (Substance Abuse and Mental Health Services Administration, 2016). The prevalence of mental health problems is high amongst those with low level of education (Lorant et al., 2003, Mirowsky and Ross, 2003, Sironi, 2012, Stewart-Brown et al., 2015).

longitudinal survey; and the Biobank, a large cohort study with a focus on health.

By raising the minimum school leaving age from 15 to 16, the ROSLA prolonged compulsory schooling by an additional year for a large fraction of cohorts born on or after 1$^{st}$ September 1957 in Great Britain.[3] We use a fuzzy regression discontinuity (RD) design to estimate the effect of an extra year of schooling on a wide range of measures of mental health including depression, anxiety, other common mental health disorders and validated mental health scores predictive of serious mental health illness. Our results suggest that the 1972 ROSLA did not improve mental health, and we find evidence of increased prevalence of mental health disorders, particularly depression and anxiety in the APS data set.

We investigate mechanisms which could explain why such a large increase in schooling duration failed to improve mental health in the long-run. We use additional data from the National Child Development Study (NCDS), a cohort study which follows a sample of individuals who were part of the first cohort affected by the reform. At age 16, they were asked whether they wished they could have left school at age 15. A quarter of them reported that they wished they did, a share that is very close to the increase in participation in education until 16 estimated by Clark and Royer (2013) when investigating the same reform. We estimate whether the desire to leave school at age 15 is associated with mental health problems in adulthood, conditional on an extensive and detailed set of individual and family characteristics. We find that those who had to stay in school until age 16 but wished they could have left earlier have worse mental health than those who left school at 16 by choice. We present a set of placebo tests that indicate that the conditional differences are unlikely to be solely driven by unobserved heterogeneity. Forcing some young people to stay in school may clash with their aspirations to enter the labour market early and involve significant psychological and emotional costs, which could have detrimental consequences on mental health. Some young people, especially low achievers keen to drop out of school, may benefit little from additional formal schooling (Eckstein and Wolpin, 1999) and may be negatively affected by being forced to stay for another year in a stressful academic environment in which they are less likely to succeed than their peers.

We contribute to the existing literature by providing evidence that extending the duration of compulsory schooling may not lead to improvements in mental health and may increase psychological distress for some individuals. Whilst extending the duration of compulsory schooling increases educational attainment, it may also affect young people's aspirations and the mental well-being of those who wanted to leave school earlier. Our findings suggest that the exclusion restriction for the regression discontinuity design may be violated, as changes in schooling laws may affect mental health through channels other than increased educational attainment. Our results have important implications for the design of compulsory schooling laws and, potentially, other interventions with an element of coercion. Whilst compulsory schooling laws can raise educational attainment, improve average labour market outcomes (Card, 1999)

[3]The reform decreased the proportion leaving education before 16 by around 20 percentage points. This group is of particular interest because the prevalence of mental health problems is high amongst those with low level of education (Lorant et al., 2003, Mirowsky and Ross, 2003, Sironi, 2012, Stewart-Brown et al., 2015).

and health (Galama et al., 2018), the coercive element may also have negative effects for some young people. Policies aiming to increase educational attainment should try to minimise these potential adverse effects, for instance by providing incentives rather than being mandatory, or if mandatory, by offering more flexible training routes.

The remainder of this paper is organised as follows. In the next section we briefly discuss the literature examining the effects of compulsory schooling on health. In Section 3 we outline our empirical strategy. Section 4 describes the data used in this paper. In Section 5 we present results on the mental health effects of the 1972 ROSLA. In the next section, we investigate whether longer compulsory schooling may affect mental health directly. The last section concludes.

# 2 Education and Health

The health effects of education have attracted increasing attention within both the economics and public health literature. Since the landmark study on the association between education and mortality by Kitagawa and Hauser (1973), numerous studies have shown that education is strongly associated with better physical (See for instance Huisman, 2004, Meara et al., 2008, Hummer and Lariscy, 2011) and mental health (Lorant et al., 2003, Mirowsky and Ross, 2003, Sironi, 2012, Stewart-Brown et al., 2015).

The canonical model developed by Grossman (1972) highlights two mechanisms that may generate a direct effect of education on health. First, education may help individuals choosing better health inputs by improving knowledge of the relationship between health behaviours and health outcomes. Second, schooling may raise the marginal product of health inputs. Education may also affect health via its impact on labour market outcomes, as increased income may make healthy goods more affordable. More educated people may work in a safer environment (Cutler and Lleras-Muney, 2010) and have more educated and healthier peers (Gaviria and Raphael, 2001, Trogdon et al., 2008).

Yet, there is currently considerable debate on whether education has a causal effect on health. First, the observed correlation between education level and health may be driven by reverse causality: poor health in childhood - a strong predictor of poor health in adulthood - can lead to lower educational attainment (Case et al., 2005, Currie and Stabile, 2006, Cornaglia et al., 2015) and lower socio-economic status (Tyrrell et al., 2016). Another possible explanation for the observed correlation is that education and health are jointly determined by social and genetic endowments (Behrman et al., 2011) or time preferences (Fuchs, 1982).

The main approach in the literature to identify the causal effects of education on health is to exploit quasi-experimental variations in educational attainment generated by changes in compulsory schooling laws. The evidence indicates that the health effects of compulsory schooling laws depend on the country and historical context (Galama et al., 2018). Whereas Lleras-Muney (2005) found very large effects of education on mortality in the United States, other studies found smaller effects using reforms in Italy (Cipollone and Rosolia, 2011), the Netherlands (van Kippersluis et al., 2011), Sweden (Fischer et al., 2013) and seven European countries (Gathmann et al., 2015). Other studies found no effects of education on mortality in France (Albouy and Lequien, 2009), Sweden (Meghir et al., 2018), the UK (Clark and Royer, 2013) and the United States (Mazumder, 2008). There is evidence that education improves self-rated health (Oreopoulos, 2006, Kemptner et al., 2011) and reduces BMI, obesity and diabetes (Kemptner et al., 2011, Brunello et al., 2013, James, 2015, Davies et al., 2018, Janke et al., 2018). Clark and Royer (2013), however, found no effect of education in the UK on a range of health outcomes, including biomarkers, a result also found by Jürges et al. (2012). The evidence on the effects of education on the prevalence of specific health conditions in the US and the UK is also mixed (Mazumder, 2008, Fletcher, 2015, Janke et al., 2018). In their analysis of the effect of compulsory schooling reforms in Sweden, Meghir et al. (2018) found no long-lasting reduction in mortality and hospitalisation.

The few studies that exploit changes in compulsory schooling laws to estimate the effect of schooling on mental health have produced contradictory results. Using data from European countries, Crespo et al. (2014) and Mazzonna (2014) find that additional years of schooling have positive effects on mental health. By contrast, Lager et al. (2016) find that raising the minimum school leaving age in Sweden has an adverse effect on emotional control at age of military conscription. They argue that the reform may lead to a change in the school environment, which may carry negative consequences for the emotional well-being of some students. Dursun and Cesur (2016) examine the impact of a 1997 reform that raised compulsory schooling by three years in Turkey. They find that the increase in school leaving age reduces life satisfaction among men, despite increasing earnings. Courtin et al. (2019) exploit a 1959 two-year extension of compulsory schooling in France and find that increased schooling increases levels of adult depressive symptoms amongst women. The Discrepancies in findings across these studies might arise from diversity in mental health measures used, and heterogeneous effects across countries and reforms. For example, while the ROSLA reform had positive effects on wages, the impact of the French reform on earnings was close to zero (Grenet, 2013). In addition, none of these studies explore mechanisms linking compulsory schooling to mental health. We contribute to this literature by examining the impact of one of the major compulsory schooling reforms in Europe, incorporating a wide range of mental health outcomes, and examining whether aspirations associated with the desire to leave school early by some students might explain observed impacts on mental health.

# 3 Empirical Approach

## 3.1 The 1972 ROSLA

We estimate the long-term effect of an additional year of compulsory schooling on mental health induced by the 1972 ROSLA, which extended the duration of compulsory education by one year in Great Britain. We adopt a regression discontinuity (RD) design (Lee and Lemieux, 2010). This approach is well suited for the analysis of this reform which resulted in a sudden and large increase in the proportion of students taking secondary school exams (Clark and Royer, 2013).

The 1972 ROSLA has a long history, starting with the 1944 Education Act, which raised the minimum school leaving age from 14 to 15 and prepared for a further increase to 16, at the discretion of the Minister of Education. In 1964 the Government announced that the school leaving age would be raised to 16 in September 1970. With a two year delay, the school leaving age was raised to 16 in 1972 by Statutory Instrument 444 in England and Wales, taking effect on 1st September 1972. All individuals who were born on or after the 1st September 1957 had to stay at school until the end of the school year in which they turned 16. In Great Britain there is an almost perfect compliance with the minimum school leaving age and correspondence between the age and the grade, with very few pupils being in a grade other than the one suggested by their month-year of birth. The first school-cohort affected were therefore born between 1st September 1957 and 31st August 1958.

The 1972 ROSLA provides an interesting context to examine the effect of compulsory schooling on mental health. The ROSLA raised the number of years of compulsory education further than most reforms in Europe that occurred in the 1950s and 1960s. It also affected a large proportion of the cohort, since at that time around a quarter of a school cohort left school at 15. In the early 1970s, school leavers had no difficulty in joining the labour market, and therefore a large fraction of young people decided not to stay at school beyond the minimum leaving age. The opportunity cost of an additional year of schooling may have been high for a subset of affected students.

## 3.2 Estimating the effect of the 1972 ROSLA

Following Clark and Royer (2013), we use a fuzzy regression discontinuity design. First, we estimate the impact of the 1972 ROSLA on education attainment. Second, we estimate the mental health effects of the additional years of schooling induced by the reform. Specifically, we use the following reduced-form equation to estimate the overall impact of the reform on educational attainment and mental health outcomes:

$$y_{i,c,t} = \alpha_0 + \alpha_1 D_{i,c} + f(R_{i,c}) + \mathbf{x}_{i,c,t}\alpha_2 + \eta_{i,c,t} \tag{1}$$

where $y_{i,c,t}$ is the outcome of interest (e.g. education, mental health) for an individual $i$ in birth cohort $c$ at time $t$, $D_{i,c}$ is a binary variable indicating whether the individual $i$ belongs to a cohort affected by the ROSLA, $R_{i,c}$ is a measure of the distance between an individual's birth cohort (measured in months) and the ROSLA cutoff (September 1957).[4] We include as controls $\mathbf{x}_{i,c,t}$, a vector of relevant individual characteristics. Our main specification only

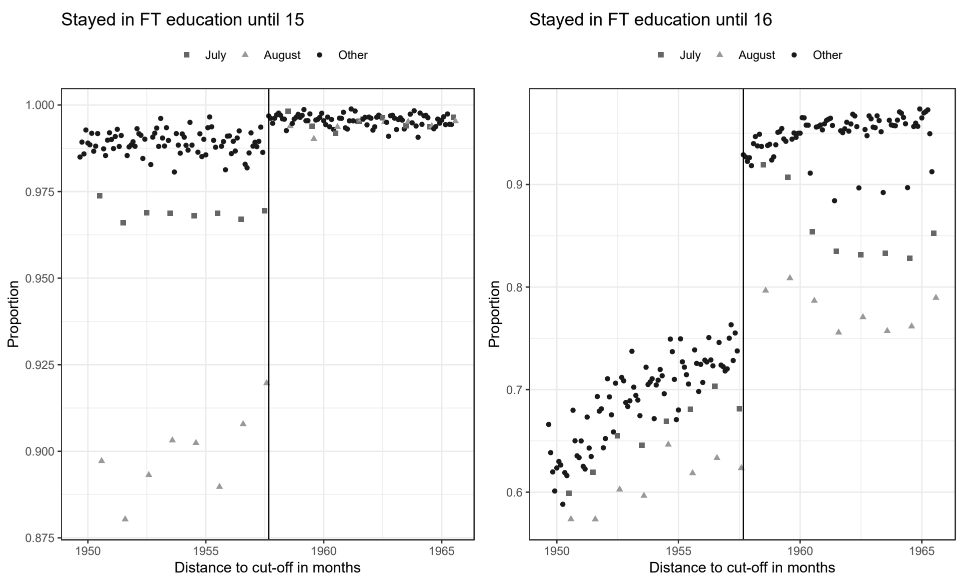
[4]We discuss the functional form of $f(R_{i,c})$ below.

includes indicator variables for calendar month of birth to correct for measurement error in the age when left full-time education.[5]

As can be seen in Figure 1, a large proportion of respondents born in the summer, especially in August, report having left school one year before the minimum school leaving age (15 before the reform, 16 thereafter) whereas the proportion is very low for those born in other months. For instance, 20.4 percent of those born in August 1958 report having left school at 15, compared to 6.2 percent of those who where born in September 1957. This pattern is not driven by respondents born in the summer having completed fewer years of education; instead, it is because they left school before their 16$^{\text{th}}$ birthday, even though they turned 16 before the end of the school year in August. They completed the same number of years of education as those born in other months. A similar pattern can be observed for individuals born in the summer of the pre-reform period.

This measurement error in the age of leaving education could inflate the absolute value of the IV estimate, as it would bias the First Stage downwards. We include a set of calendar month of birth dummies to capture this measurement error. Clark and Royer (2013) included a full set of month of birth dummies interacted with the dummy indicating the post-reform period. However, since misreporting affects respondents born both in the pre-and-post reform periods, this is not necessary. Therefore, in our preferred specification we do not include an interaction between the treatment variable and the month of birth. However, we show that our results are robust to the introduction of an interaction between the treatment variable and being born in the summer months (July and August), or excluding these respondents from our sample.

Figure 1: Age when left full-time education by month of birth)




Note: Outcome by months of birth. Data: APS (2004-2016), grey square are August, grey triangle for July and all other months in black round symbols.

[5] In additional specifications, we include further covariates such as third order polynomial of age in months, gender, country of birth - England and Wales, survey year and proxy response.

We use equation (1) to estimate the effect of the 1972 ROSLA on educational attainment, measured by the age when the respondent left full-time education (i.e. the first stage). We then use the same equation to obtain estimates of the overall effects of the 1972 ROSLA on mental health outcomes (i.e. the reduced-form). These estimates can be interpreted as the Intention to Treat (ITT) parameters, because not all individuals changed their behaviour as a result of the reform. While some individuals stayed at school until age 16 because of the reform (the 'compliers'), other would have stayed at school until age 16 (or more) in the absence of the reform (the 'always-takers').

The average effect of an extra year of education induced by the reform on mental health can be obtained by a Wald estimator (i.e. dividing the estimate of the overall impact of the reform on mental health by the estimate of the effect of the reform on the age when left full-time education). This procedure is equivalent to using predicted age of leaving full-time education from the first stage in a second-stage equation of mental health:

$$mh_{i,c,t} = \beta_0 + \beta_1 \widehat{educ}_{i,c,t} + g(R_{i,c}) + \mathbf{x}_{i,c,t}\beta_2 + \varepsilon_{i,c,t} \tag{2}$$

where $\beta_1$ measures the effects of an additional year of education on a given mental health outcome. It can be obtained by estimating equations (1) and (2) through two-stage least squares (2SLS) using $D_{i,c}$ as the instrument.

In order to obtain unbiased estimates of the discontinuity parameters in equation 1 and 2 it is crucial to capture cohort trends ($f(R_{i,c})$) correctly, as mis-specification of the functional form of the 'forcing' variable is likely to generate biased estimates of the discontinuity parameter. There are two main approaches to estimating treatment effects in an RD design (Lee and Lemieux, 2010). In the global polynomial approach, the equation is estimated using all observations and cohort trends are captured using a parametric function (e.g. n-order polynomial of $R_{i,c}$). In the local linear regression approach, the equation is estimated over a narrow range of data (i.e., in our case, using only individuals born close to the cut-off date of September 1$^{st}$, 1957) and the cohort trends are captured by a linear function of $R$, allowing for different slopes on each side of the cut-off.

We use the second approach, since it generates estimates that are more local to the threshold and less sensitive to mis-specification (Lee and Lemieux, 2010). We estimate local linear regression models on both sides of the cut-off with a rectangular kernel (Imbens and Lemieux, 2008).[6] An important step when estimating local linear regressions is to choose the bandwidth, in our case the number of cohorts born before and after the cutoff. We select the bandwidth so that the pre- and post-reform trends appear to be linear for our main outcomes. We use a bandwidth of 36 months for the data from the Annual Population Survey (APS) and the Biobank data, and 48 months for data from Understanding Society. Whilst the pre- and post-reform trends appear to be linear for a 48-month window in Understanding Society, it is not the case

[6] We thank an anonymous referee for suggesting this approach. We use the Stata command developed by Nichols (2016) and show in Appendix that our results are robust to using a triangular kernel.

in APS and the Biobank. We also report results for bandwidths between 12 and 72 months. We did not use a bandwidth selected via optimal bandwidth algorithm because we need to have a bandwidth of at least 12 months to be able to capture calendar month of birth effects, and bandwidths produced by the algorithm developed by Calonico et al. (2014) were lower than 12 for some outcomes.

Results from the test proposed by McCrary (2008) presented in Figure A.1 in Appendix A.1 indicate that there is no discontinuity in the number of observations around the cut-off for the reform in any of our three datasets. In Table A.1 in Appendix A.1, we check that there is no discontinuity in variables that were determined before exposure to the reform. We find no evidence of discontinuity in predetermined variables except for gender in APS. There appears to be fewer women in our APS sample in the post-reform period compared to the pre-reform period. The difference is only significant at the 5% level and the magnitude is small (one percentage point from a baseline of 50.6).[7]

The main identifying assumption is that in the absence of the reform the conditional expectations of outcomes, with respect to month-year of birth cohorts, would have been continuous at the threshold (i.e. 1st September 1957). This implies that the change in minimum school leaving age was the only 'shock' that affected cohorts born just after the cutoff point for the 1972 reform.[8] Although this assumption cannot be directly tested, one implication is that there should be no discontinuities in outcomes at any other arbitrarily chosen thresholds. We test for discontinuities in outcomes of interest between school cohorts, for every year between 1947 and 1977 in Section 5.3 (Sensitivity analysis).

The estimates of parameter $\beta_1$ in equation 2 are local average treatment effects (LATE) and should be interpreted as the average effects of the additional year of compulsory schooling for those who would have left school at 15 in the absence of the 1972 ROSLA. These effects are likely to be heterogeneous across individuals and different from those of an additional year of education resulting from an individual decision rather than a legal obligation. These effects are also likely to differ from those of an additional year of education at any other point of the education distribution. These effects are nonetheless interesting, since the subpopulation affected by the reform is relatively similar to the young people who leave school early with few or no qualifications, a group that attracts significant policy concern.

## 3.3 Estimating mental health effects of longer compulsory schooling

The RD approach highlighted above identifies the average effect of increasing the minimum school leaving age, but the effects are likely to be heterogeneous. The reform increased the

[7]As women tend to have more mental health problems than men, this could potentially bias our estimates towards overestimating the protective effect of education.

[8]One could argue that differences in macroeconomic conditions at time of labour market could also play an important role. The 1973 oil crisis caused an economic recession but it did not translate into higher unemployment rates until 1975 (Denman and McDonald, 1996). Therefore, members of the first cohort affected by the ROSLA who left school at 16 (in 1974) would have joined a relatively healthy labour market, before unemployment started to rise.

duration of schooling and raised attainment of qualifications on average. As a result, there is evidence that it improved average labour market outcomes (Dickson, 2013, Grenet, 2013). However, staying in school for another year may be sub-optimal for some young people, especially low achievers keen to drop out of school who may benefit little from additional formal schooling (Eckstein and Wolpin, 1999). The reform did not only raise educational attainment, but it also delayed entry on the labour market, which could have reduced earnings for some young people (Buscha and Dickson, 2015). Worsening mental health as a result of a reduction in earnings in this subgroup may offset the mental health gains from improved labour market outcomes for other cohort members. The reform may also carry significant psychological and emotional costs for those who feel forced to stay in school.

Using data from a unique birth cohort, the National Child Development Study (NCDS), we estimate the mental health effect of being forced to stay in school for an extra year as a result of the 1972 ROSLA, 'net' of the effect of higher educational attainment. This can be interpreted as the mental health effects of increasing the duration of compulsory schooling. The NCDS is a longitudinal study that follows 17,415 people born during the week of 3rd to 9th March 1958 in Great Britain. These individuals all belong to the first cohort affected by the 1972 ROSLA. We describe the data in more detail in the following section. At age 16, whilst they were still in school, they were asked a range of questions about educational decisions, including whether they wished they could have left school at age 15 (instead of 16, the new minimum school leaving age). Those who wished they had left school at 15 are likely to be those who would have left school at 15 had they not been required to stay until 16 as a result of the 1972 ROSLA. We restrict our sample to respondents who left school at the minimum school leaving age (16), and compare the mental health outcomes of those who willingly left school at 16 to outcomes of those who wished they could have left school at 15, conditional on a range of observed characteristics:

$$dep_{i,a} = \beta wish15_i + \mathbf{x}_i\gamma + \varepsilon_{i,a} \quad (3)$$

where $dep_{i,a}$ is a measure of depressive symptoms for individual $i$ at age $a$, $wish15_i$ is a binary variable indicating if the individual wished to have left school at 15. $\mathbf{x}_i$ is a vector of an extensive and detailed set of individual and family characteristics (observed at age 16 or earlier) which are likely to be correlated with both wishing to have left school at 15 and depressive symptoms in later life. It includes mother's characteristics at birth (age of mother at birth, intensity of smoking during pregnancy, whether mother had post-compulsory schooling), measures of socioeconomic status (SES) of the family (father's SES at age 7 and 16, region when 16) an indication of the parental level of interest in their child's education, as well as ability tests (reading and maths test score at age 7, 11 and 16, general ability test score at age 11) and educational attainment (number of O-levels and CSEs taken), aspiration on leaving school, aspirations about first job and expectation about marriage and fertility decisions.

To identify the effect of being forced to stay in school for an extra year, we assume that unobserved variables that affect feeling forced to stay in school have no effect on mental health in adulthood ($E(\varepsilon_{i,a}|\mathbf{x}_i, wish15_i) = 0$). We control for a wide range of factors that could influence both feeling forced to stay in school and mental health in adulthood, but there may still remain unobserved factors which could be driving the results. To test the plausibility of this assumption, we estimate whether measures of mental health at age 7 and 11 differ between those who willingly left school at 16 and those who wished they could have left school at 15, conditional on observed characteristics. We can compare the differences in mental health at age 7 and 11 to differences found later in life for the two groups. Finding zero differences at ages 7 and 11 but significant differences at beyond age 16 would be consistent with the hypothesis of mental strain starting after the enforced additional schooling.

To measure the potential bias arising from unobservable factors we also follow the method proposed by Altonji et al. (2005). If observed and unobserved factors are correlated, then the magnitude of the selection effects arising from observed factors gives an indication of the selection that can be caused by unobserved factors. We use the change in coefficient of the treatment and the R-squared resulting from including observed characteristics in the model to derive a lower bound estimate of the treatment effect. Assuming that the observable and unobservable factors are equally related to the treatment, the lower bound coefficient is given by

$$\beta^* = \tilde{\beta} - (\mathring{\beta} - \tilde{\beta})\frac{R_{max} - \tilde{R}}{\tilde{R} - \mathring{R}}$$

where $\tilde{\beta}$ is the coefficient estimate with controls, and $\mathring{\beta}$ is the coefficient estimate without control. $\tilde{R}$ and $\mathring{R}$ are the R-squared of the model with and without controls and $R_{max}$ is the maximum R-squared possible. As suggested by Oster (2019), we set $R_{max} = min(1.3\tilde{R}, 1)$.The multiplicative factor 1.3 was derived from an analysis of results from papers based on randomised experiments and published in leading economics journals between 2008 and 2013.[9]

# 4 Data

To estimate the mental health effects of ROSLA, we use data from two household surveys representative of the UK population, the Annual Population Survey (APS) and Understanding Society, as well as data from the UK Biobank. With these data we estimate the overall impact of the reform on a wide range of mental health outcomes. Additionally, we use data from the National Child Development Study (NCDS) to test the effect of ROSLA on mental health net of the effect of the reform on educational attainment.

[9] Oster (2019) argues that an R-squared of one is unrealistic in many applications and leads to a too conservative test. She proposes a multiplicative factor 1.3 because it allows 90 percent of the results from the analysed randomised experiments to remain different from zero.

## 4.1 The Annual Population Survey

The APS is the largest household survey in the UK and includes approximately 150,000 households and 350,000 respondents per year.[10] In this analysis, we use data from the APS collected between April 2004 and March 2016.

We use the month-year of birth to identify whether respondents belong to a pre- or post-ROSLA cohort. Respondents born before September 1957 were not affected by the 1972 ROSLA and could leave school at age 15, whereas those born in September 1957 or later were legally required to stay at school an additional year. The APS contains detailed information about the educational attainment of respondents, including the age when left full-time education and all qualifications held. We use the age of leaving full-time education as our endogenous variable rather than educational attainment, because the 1972 ROSLA may have increased the age of leaving school but it may not have increased attainment of formal certifications at at the end of high school (eg O levels or CSE).

Whilst the APS does not contain screening tests for mental health problems, all working age respondents (men aged between 16 and 64, and women aged between 16 and 59) are asked whether they have any long-lasting health problems or disabilities.[11] Respondents who answer positively to this question are asked to report the health problems they have based on a list of 17 health conditions and disabilities. Mental health conditions are divided into two categories. Respondents are asked whether they currently suffer from 'depression, bad nerves or anxiety' and whether they have 'mental illness, or suffer from phobias, panics or other nervous disorders'.[12] In addition to analysing the two variables separately, we combine the responses to the two questions to create a variable indicating whether the respondent suffers from a mental health condition. Descriptive statistics are reported in Panel A of Table A.2 in the Appendix A.1.

These questions are likely to capture serious mental health problems rather than short-term emotional and psychological issues, because of the focus on long-term conditions. To check that this is indeed the case, we use data from the 2007 Adult Psychiatric Morbidity Survey (AMPS). We find that self-reports of mental health conditions are strongly correlated with clinical assessments of common mental health conditions (See Figure A.3 in Appendix), such as the Clinical Interview Schedule - Revised (CISR), which is a validated medical instrument used to assess the prevalence of common mental health conditions (Lewis et al., 1992). Respondents who reported suffering from anxiety, depression or other mental health conditions had an average CIS-R score of 15.0, compared to 4.1 for those who did not report having a mental health problem - a score of 12 is used as a cut-off for common mental health conditions. Therefore, self-reports appear to capture clinically-relevant mental health conditions.

[10] Initiated in 2004, the APS combines Wave 1 and Wave 5 of the Labour Force Survey with The Local Labour Force Survey (LLFS) for England, Wales and Scotland.

[11] The precise question posed is: 'Do you have any physical or mental health conditions or illnesses lasting or expecting to last 12 months or more?' - See the Labour Force Survey User Guide - Volume 2: LFS Questionnaire.

[12] Labour Force Survey User Guide - Volume 2: LFS Questionnaire.

We restrict our sample to respondents born in England and Wales. The education system in Northern Ireland and Scotland differs from that of the rest of the UK and respondents born abroad should not have been affected by the 1972 ROSLA, unless they arrived in the UK before they were 15. We only include men aged between 16 and 64, and women aged between 16 and 59 because the health questions are only asked to working age respondents. This restriction has little impact on our estimation sample, since our data are collected up to March 2016 and in our main analysis we focus on respondents born three years before and after the cut off of the reform (1st September 1957).[13] We have a total of 230,882 individuals in our sample born in England and Wales within three years before and after the cut off of the reform.

The APS is a one-stage clustered sample of individuals, with households as the primary sampling unit, since all adults within a household are sampled.[14] As recommended by Kolesár and Rothe (2018), we do not cluster standard errors by the running variable. Instead, we report standard errors clustered at the household level to account for the survey design. In addition, we apply survey weights to our estimator (as recommended by Solon et al. 2015).[15]

## 4.2 Understanding Society

To explore further the effect of the 1972 reform on mental health, we use measures of mental health included in Understanding Society, a longitudinal survey of around 40,000 households. We use data from the first eight waves of the study, which were collected between 2009 and 2018. Whilst the sample size is much smaller compared to the APS, this survey contains two routinely used measures of mental health, the 12-Item Short Form Survey (SF-12) and the General health Questionnaire (GHQ). The SF-12 is a short generic health questionnaire based on the widely used Short-Form 36 Health Survey (SF-36). It contains three of the five items of the Mental Health Inventory, which assesses symptoms of common depressive and anxiety disorders. The mental health component of SF-12 has been found to be a valid measure of mental health and a useful screening tool for both depression and anxiety disorders (Gill et al., 2007). We focus on the SF-12 Mental Component Summary (MCS), which is derived by combining the valid answers to the seven items related to mental health in the SF-12 to a continuous scale with a range of 0 (poor mental health) to 100 (good mental health). The components describe how often in the past four weeks the respondents have experienced different issues or mood states, with a five-option response, ranging from "all the time" to "none of the time". The GHQ is a screening test for identifying minor psychiatric disorders in the general population, and focuses on the inability to carry out normal functions and the appearance of distress (Goldberg and Blackwell, 1970). Respondents in Understanding Society are given the 12-items version

[13] This includes respondents born between September 1st 1954 and 31st August 1959. Women born in September 1954 would be at most 59 unless they were interviewed between September 2014 and 2016. In this case, they would be 60 and therefore not included in our sample. Our results are robust to excluding all respondents interviewed after August 2014.

[14] See Labour Force Survey User Guide - Volume 1: Background and Methodology.

[15] We use weights calibrated to population totals provided with the APS data.

of the test, which is a good proxy for depressive disorder (Lundin et al., 2016). The GHQ-12 consists of 12 questions (six positively phrased and six negatively phrased), with a four-point response indicating if they experienced symptoms more often than usual. We use the likert score in our main analysis.

Understanding Society does not contain information on the age when respondents left full-time education but on the age when left secondary education. This age ranges from 14 to 19. However, as we show in our analysis of the APS data, the reform primarily had an effect on the proportion of young people staying in education until the age of 16 but not beyond. Therefore, using this variable to obtain an IV estimate of the effect of the additional year of education on both the GHQ and SF-12 appears appropriate.

We restrict our sample to respondents born in England and Wales within four years before and after the cut-off for the reform.[16] We further discard individuals with no valid information on the age when they left school. Applying these restrictions, we have 26,416 observations from 5,271 individuals with valid GHQ scores, and 26,639 observations from 5,421 individuals with valid SF-12 mental health scores. Because of the longitudinal nature of the survey, the same individuals are interviewed multiple times. We adjust the survey weights by the inverse of the number of times each individual was surveyed to avoid giving excessive weights to those who responded to the survey more regularly. We cluster standard errors at the individual level.

## 4.3 The UK Biobank

The UK Biobank is a cohort study of about 500,000 participants recruited in the UK between 2006 and 2010. All participants took part in a touchscreen questionnaire at baseline and some also completed an online follow-up questionnaire. Unlike the APS and Understanding Society, the biobank is not a traditional household survey and is not representative of the UK population. Only individuals aged 40–69 years living within 40 kilometres of one of the 22 assessment centres in Great Britain were invited to enter the cohort. Only 5.5% participated in the baseline assessment.[17] The UK Biobank is therefore not representative of the sampling population either, with evidence of a 'healthy volunteer' selection bias (Fry et al., 2017).

The online follow-up questionnaire contains two widely used mental health screening tests routinely used in clinical settings, the General Anxiety Disorder Assessment (GAD-7) and the Patient Health Questionnaire (PHQ-9).

The GAD-7 is a self-administered seven item questionnaire used to assess the severity of generalised anxiety disorder. The respondent is asked to rate the severity of seven symptoms over the past two weeks, using four possible response options: 'not at all', 'several days', 'more than half the days' and 'nearly every day'. The GAD-7 shows excellent internal consistency and

[16] We select respondents born within four years before and after the cut-off for the reform because in Understanding Society the pre- and post-reform trends appear to be linear for a 48-month window. It is not the case in the APS and the Biobank.

[17] The sampling population comprises about 9.2 million individuals.

good test-retest reliability, and therefore is a valid and efficient tool for screening for generalised anxiety disorder in clinical practice and research (Spitzer et al., 2006).

Biobank's participants are also asked to complete the PHQ-9, a self-administered test used for diagnosing depressive disorders (Kroenke et al., 2001). It scores each of the nine criteria from the Diagnostic and Statistical Manual of Mental Disorders manual (DSM-IV) from '0' (not at all) to '3' (nearly every day). PHQ-9 has been validated for use in primary care (Cameron et al., 2008).

We also use a measure of probable major depression derived by Smith et al. (2013). This measure is based on items relating to the lifetime experience of minor and major depression and information on help-seeking for mental health collected in the baseline touchscreen questionnaire. We create a binary variable equal to one if the participant is likely to suffer from either a probable single or recurrent major depression.

After restricting our sample to respondents born in England and Wales within three years before and after the cut-off for the reform, and discarding those with no information about the age when they left school, we have 12,875 participants with valid information about probable major depression and 15,432 with valid GAD-7 and PHQ-9. Descriptive statistics for the Biobank sample are reported in panel C of Table A.2 in Appendix A.1.

## 4.4 The National Child Development Study (NCDS)

To estimate the effect of being forced to stay in school for an extra year as a result of the 1972 ROSLA, net of the effect of higher educational attainment, we analyse data from the NCDS, one of a few birth cohorts that has followed individuals since birth until adult age. NCDS follows 17,415 people born in Great Britain during the week of 3rd to 9th March 1958. Initially set up as a study of perinatal mortality, the study collected a wide range of information on their family background and the circumstances and outcomes of birth. The participants have been followed throughout their life and the scope of the study was expanded to cover many aspects of the health, educational, and social development of cohort members, with interviews taking place at age 7, 11, 16, 23, 33, 42, 46, 50 and 55. For a detailed description of the NCDS, see Power and Elliott (2006).

Interestingly, these individuals belong to the first cohort that was affected by the 1972 ROSLA. The third follow-up took place in the spring of 1974 when the participants were 16, in their final year of compulsory schooling. It retrieved 14,647 of the cohort members. Participants were asked a range of questions about educational decisions, notably when they intended to leave school and whether they wished they could have left school at 15.[18] Just over a quarter of respondents said they wished they could have left school at 15, a proportion which is very close to the increase in participation in education until 16 caused by the 1972 ROSLA (Chevalier et al., 2004, Clark and Royer, 2013). It also contains information about the examinations

[18]The question is 'You will probably know that this is the first year that everybody has had to stay at school until they are 16. In your own case do you wish that you could have left when you were 15?'

they are going to take at the end of the school year (CSE and O-Level). Additional information about educational attainment was collected in the fourth follow-up, conducted when cohort members were 23 and retrieved 12,537 participants. Information on the age when they left school shows that over two-thirds (71.1 percent) left at age 16.

The NCDS contains several measures of mental health. In the first and second follow-ups, when cohort members were aged 7 and 11, the teachers completed the Bristol Social Adjustment Guide (BSAG) for cohort members, a questionnaire designed to assess the prevalence and severity of psychological and behavioural problems in children. Teachers were asked to underline 'items of behaviour' which described the child's behaviour or attitudes, in accordance with the general instructions in the BSAG manual (Stott, 1969). Behaviour items are then combined to give a score for several syndromes, such as unforthcomingness, withdrawal, depression, anxiety, hostility towards adults. A total score is derived by summing the score of the various syndromes.

At ages 23, 33, 42 and 50, participants were administered a questionnaire used to calculate the malaise score, a measure of psychological distress (Rutter, 1970). Designed to identify depression in non-clinical settings, the malaise score is derived from responses to 24 questions on various aspects of well-being and somatic symptoms.[19] A score of 8 or above indicates probable depression. The Malaise Inventory has a good level of internal consistency and its validity is robust across different population groups (Rodgers et al., 1999). Chevalier and Feinstein (2007) used this measure to analyse the effect of education on mental health, but they did not investigate the effect of the extension of the duration of compulsory schooling.

Table 1: Key descriptive statistics - NCDS data

| | Wished left school at 15? | | | | | |
|---|---|---|---|---|---|---|
| | No | | Yes | | | |
| | N | mean | N | mean | Difference | Std Error |
| BSAG at age 7 | 3,752 | 8.170 | 2,012 | 10.92 | -2.754*** | 0.239 |
| BSAG at age 11 | 3,626 | 7.594 | 1,909 | 11.15 | -3.556*** | 0.247 |
| Malaise score at 23 | 3,854 | 2.642 | 2,052 | 3.541 | -0.899*** | 0.0827 |
| Malaise score at 33 | 3,026 | 3.272 | 1,468 | 4.105 | -0.833*** | 0.0976 |
| Malaise score at 42 | 3,043 | 3.387 | 1,475 | 4.358 | -0.971*** | 0.116 |
| Malaise score at 50 | 2,627 | 1.478 | 1,208 | 1.732 | -0.254*** | 0.0692 |

Note: National Child Development Study. Sample restricted to those who left school at 16.

We restrict our sample to participants who were interviewed in the first four follow-ups and answered the educational aspiration module in the third follow-up (8,459) and left school at 16 (5,913). The descriptive statistics for the malaise scores reported in Table 1 indicate that young people who wished they could have left school at 15 instead of 16 have worse mental health throughout adulthood than those who chose to leave school at 16. Additional descriptive

[19] At age 50, a shorter 9-item version of the malaise score was used instead of the full questionnaire.

statistics are reported in Table A.3 in Appendix A.1. We can see that those who wished they could have left school at 15 tend to be more disadvantaged than those who chose to leave school at 16. They come from lower SES families, their mother was more likely to smoke when pregnant, and they have poorer reading and maths score at age 7, 11 and 16.

# 5 The effects of the 1972 ROSLA

## 5.1 The effect of the ROSLA on education

In this section we analyse the effects of the 1972 ROSLA on educational attainment. The impact of the reform is shown graphically in Figure 2, which displays average educational attainment by month-year of birth. As expected, the average age when individuals left full-time education increased substantially following the reform. The proportion of those staying at school until at least 16 rose sharply. Table 2 reports the estimates from regression models based on equation 1. As shown in column one, the 1972 ROSLA had a strong impact on the age when leaving full-time education. Overall, the reform increased the average time spent in education by 0.211 years, from a baseline of 17.1. The coefficient is precisely estimated, with a t-statistic of 8.4. The magnitudes of the t-statistics suggest that the 1972 ROSLA indicator is a powerful instrument for schooling duration. Therefore, results from column 1 can be used as a the First-Stage estimates of the fuzzy regression discontinuity model, as in Clark and Royer (2013).

Table 2: Effect of the 1972 ROSLA on educational achievement

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Age left | Probability to stay in FT education until | | | | CSE | O Level | A Level |
| | FT education | ≥16 | ≥17 | ≥18 | ≥19 | | | |
| 1972 ROSLA | 0.2108*** | 0.1918*** | 0.0120** | 0.0077 | -0.0004 | 0.0900*** | 0.0253*** | -0.0049 |
| | (0.0250) | (0.0038) | (0.0050) | (0.0048) | (0.0042) | (0.0046) | (0.0052) | (0.0047) |
| Observations | 230,882 | 230,882 | 230,882 | 230,882 | 230,882 | 220,294 | 223,738 | 220,410 |
| Mean | 17.112 | 0.715 | 0.396 | 0.307 | 0.202 | 0.197 | 0.497 | 0.266 |

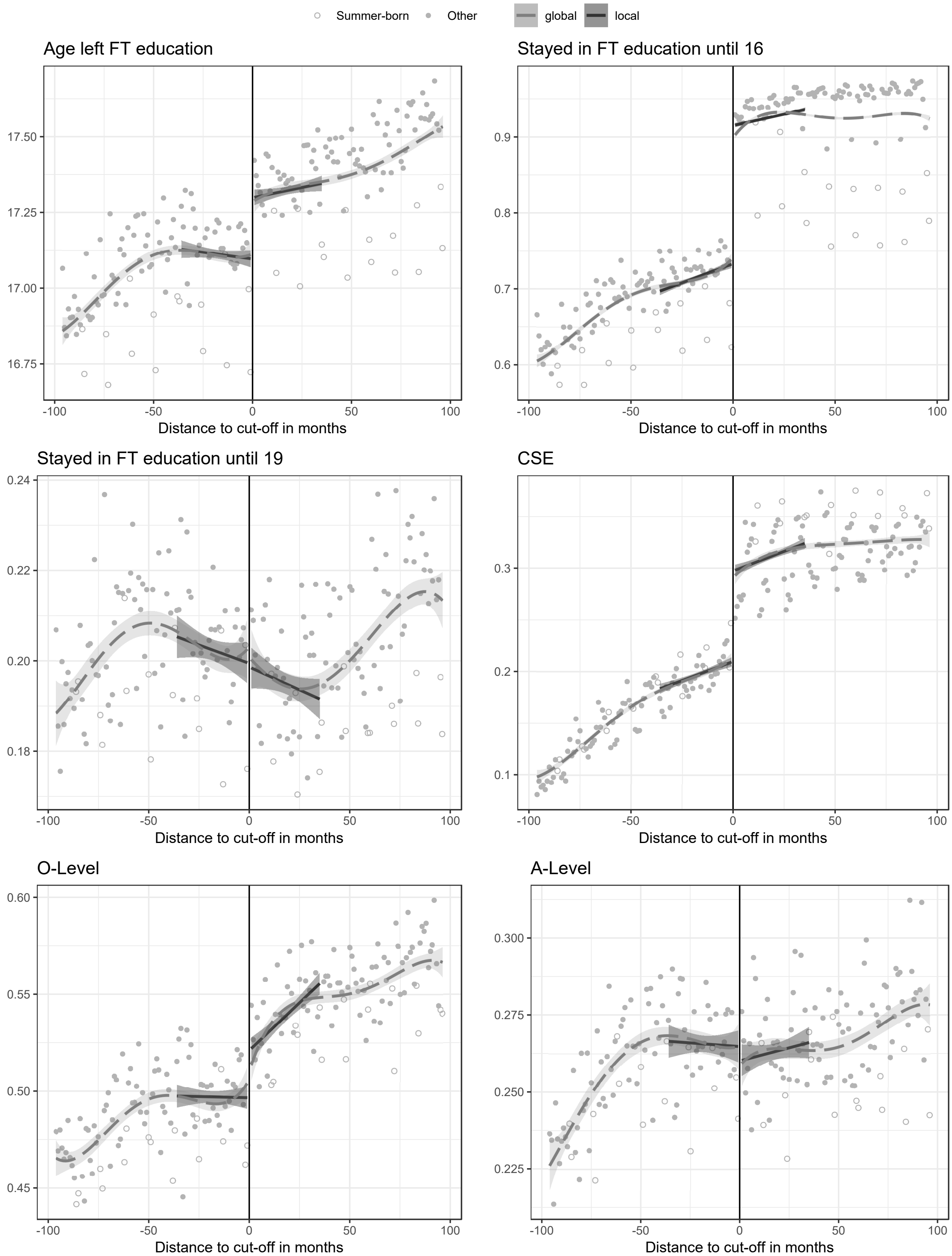
Note: Local linear regression estimated with rectangular kernel and a bandwidth of 36 months. All models include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy, calendar month of birth dummies. Estimates are weighted by the survey weights. Standard errors clustered at household level are reported in parentheses. Reported means are calculated among those who were born within two years before the reform. Data: APS (2004-2016).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Columns two to five report the estimates of the effect of the 1972 ROSLA on the probability to stay in full-time education until at least a given age. We find that the 1972 ROSLA had a strong effect on the probability to stay in education until at least 16 (Column 2). About a fifth of the cohort had to stay an additional year at school. However, it had little impact on the decision to stay in education beyond the minimum school leaving age of 16 (Columns 3-5). Although the estimate of the effect on the probability to stay at school until 17 is statistically significant, the coefficient is very small, about 15 times smaller than the effect on staying until 16. Our results are in line with those of Clark and Royer (2013) obtained with different data sources.

We also find that the reform had a smaller impact on achievement of formal qualifications compared to the large effect on staying at school for another year.[20] Until 1988 there were two types of qualifications that could be obtained at the end of secondary school: first, the General Certificate of Education Ordinary Level, (O- Level), which would open the door to study towards A-Levels, and ultimately to go to University; second, a lower-level qualification, the Certificate of Secondary Education (CSE). Results displayed in Table 2 show that the 1972 reform increased the proportion of people attaining a CSE by 9.0 percentage points from a baseline of 19.7 percent. The reform also had a statistically significant effect on the achievement of the more academic qualification, the O-level, but of a smaller magnitude than the effect on CSE (2.5 percentage points from a baseline of 49.6). A substantial share of those compelled to attend the last grade of secondary school did not gain any qualification, which suggests that they may not have benefited much from this additional year of education. In line with Chevalier et al. (2004), we also find that the 1972 ROSLA had no impact on the achievement of A-Level, the end of upper secondary school diploma, which opens the door to university. The estimates are very close to zero and not statistically significant. This is not surprising, because the reform had little impact on the probability to stay in education beyond 16 and A Level examinations are taken at age 18.

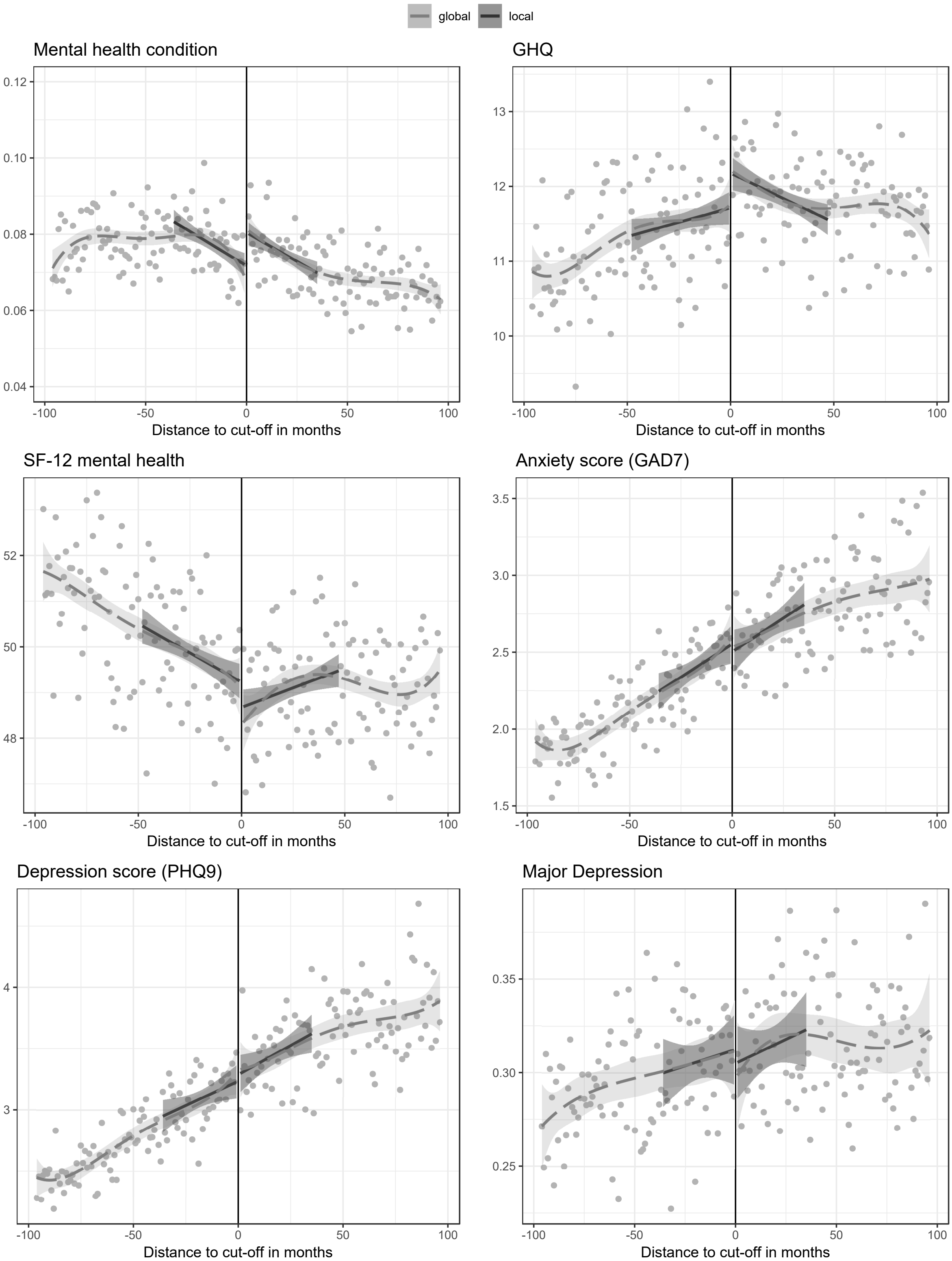
[20]Young people born between September and January could leave school at Easter, and therefore were less likely to sit the examination. This explains the within-school year variation in terms of achievement of CSEs we observe in Figure 2 (Del Bono and Galindo-Rueda, 2007, Braakmann, 2011, Dickson and Smith, 2011)

Figure 2: Educational attainment by month-year of birth




Note: Points show averages by month-year of birth. Fitted lines are adjusted for calendar month of birth, and calculated separately before and after the cut-off for the reform. The local line is a linear fit calculated on a 36 months period. The global line is fourth degree polynomial. 95% confidence intervals are reported in shaded areas. Data: APS (2004-2016)

Figure 3: Mental health outcomes by month-year of birth




Note: Points show averages by bands of two months. Fitted lines are adjusted for calendar month of birth, and calculated separately before and after the cut-off for the reform. The local line is a linear fit calculated on a 36 months period. The global line is fourth degree polynomial. 95% confidence intervals are reported in shaded areas. Data: APS (2004-2016) for mental health condition, depression or anxiety and other mental health conditions; Understanding Society (2009-2018) for GHQ and SF-12 mental health. Biobank for GAD-7, PHQ-9 and major depression

## 5.2 The effects of the 1972 ROSLA on mental health conditions

In Table 3 we present estimates of the effects of the 1972 ROSLA on a range of mental health outcomes. In panel A, we report estimates of the effect on the long-term prevalence of reported mental health conditions based on the APS data. We also report OLS estimates of the association between education on mental health. Although OLS estimates are likely to be biased, and do not reflect the causal effect an extra year of education, they can be viewed as a benchmark against which the IV estimates can be interpreted. The OLS estimates are reported in column 1 of Table 3 and describe the difference in the prevalence of mental health conditions between respondents who left school at 15 and those who left school at 16, conditional on the covariates included in the linear models.[21] The sample only includes cohorts born before the reform and is restricted to those who left full-time education at 15 or 16, so that the coefficients are comparable to the IV estimates reported in column three. Results indicate that an additional year of education is associated with better mental health in adulthood. Individuals who left school at 16 are 4.3 percentage points less likely to report having any mental health condition and 3.9 percentage points less likely to report suffering from depression or anxiety compared to those who left school a year earlier. These estimates are large, considering that 11.6 percent of the individuals who left school at 15 suffer from a mental health condition and 10.4 percent from depression or anxiety. These estimates cannot be interpreted causally as a wide range of factors not included in the model are likely to influence both education decision and mental health, such as parental background, childhood health or other life events in early life.

Column 2 of Table 3 presents the reduced-form (RF) estimates of the 1972 ROSLA, which can be interpreted as the overall effect of the reform on mental health. Figure 3 shows average prevalence of mental health problems by month-year of birth, with local linear fits and global polynomial fits.[22] We can see that there is a discontinuity around the cut-off of the reform in the proportion of individuals suffering from mental health problems, with evidence of downward slopping trends on both sides of the cut-off. The pre- and post-reform trends in the APS outcomes are linear within 36 months of the cut-off, and therefore we use a bandwidth of 36 months for these outcomes. The results appear to be driven by depression and anxiety rather than other mental health conditions (See Figure A.4 in Appendix A.3). RF estimates show that raising the compulsory school leaving age from 15 to 16 significantly and substantially increased the prevalence of mental health conditions. We find that the 1972 ROSLA increased the proportion of people reporting having a mental health condition by 0.8 percentage point, from a baseline of 11.6 percent.[23] It increased the prevalence of depression or anxiety by 0.7

[21] Covariates include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy, a third-order polynomial in age (measured in months) and binary variables for sex, year of survey, country of birth (England and Wales), calendar month of birth, and whether information was collected by a proxy respondent.

[22] We aggregate month-year of birth by bands of two months to reduce the noise. Graphs for depression or anxiety and other mental health conditions are reported in Figure A.4 in Appendix A.3.

[23] The baseline is calculated among those born within three years before the reform who left school at 15, the subpopulation who would have been affected by the reform if they had been born after the cut-off.

percentage point, from a baseline of 10.4 percent, but it had no effect on the prevalence of other mental health conditions.

Table 3: Effect of 1972 ROSLA on mental health

| | OLS | Reduced Form | IV | Mean | *N* |
|---|---|---|---|---|---|
| | | | A. APS | | |
| Mental health condition | -0.043*** | 0.0082*** | 0.0391*** | 0.116 | 230,492 |
| | ( 0.002) | ( 0.0027) | ( 0.0140) | | |
| Depression or anxiety | -0.039*** | 0.0070*** | 0.0334** | 0.104 | 230,492 |
| | ( 0.002) | ( 0.0025) | ( 0.0130) | | |
| Other mental health conditions | -0.021*** | 0.0018 | 0.0084 | 0.050 | 230,492 |
| | ( 0.002) | ( 0.0017) | ( 0.0084) | | |
| | | | B. Understanding Society | | |
| GHQ | -0.723*** | 0.4180 | 1.3498 | 12.151 | 26,416 |
| | ( 0.225) | ( 0.2980) | ( 1.0170) | | |
| SF-12 Mental Component Summary (PCS) | 1.447*** | -0.5569 | -1.8310 | 48.615 | 26,639 |
| | ( 0.411) | ( 0.5279) | ( 1.7989) | | |
| | | | C. Biobank | | |
| GAD-7 Anxiety | -0.683*** | -0.1423 | -0.8141 | 3.285 | 15,432 |
| | ( 0.000) | ( 0.1361) | ( 0.8252) | | |
| PHQ-9 Depression | -0.868*** | -0.1015 | -0.5807 | 4.355 | 15,432 |
| | ( 0.000) | ( 0.1524) | ( 0.8832) | | |
| Probable major depression | -0.012*** | 0.0141 | 0.1722 | 0.315 | 12,875 |
| | ( 0.000) | ( 0.0167) | ( 0.2630) | | |

Note: Local linear regression estimated with rectangular kernel and a bandwidth of 36 months. All models include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy and calendar month of birth dummies. OLS models include a third-order polynomial in age (measured in months) and binary variables for sex, year of survey, calendar month of birth and proxy interview. OLS models are estimated on a sample restricted to those who left school at 15 or 16. Estimates are weighted by the survey weights. Standard errors clustered by household (APS) or individuals (Understanding Society). Reported means are calculated among those who left school aged 15 and were born within three years before the reform.
*** p<0.01, ** p<0.05, * p<0.1

To obtain estimates of the magnitude of the causal effects of an additional year of schooling induced by the reform, we present IV estimates in column 3, which essentially scale up the RF estimates by the inverse of the effects of the reform on the age when left full-time education, shown in column 1 of Table 2. The IV estimates are of the opposite sign to the OLS but of similar magnitude. We find that an extra year of schooling induced by the 1972 ROSLA increased the probability to report a mental health condition by 3.9 percentage points, and the probability to report depression or anxiety by 3.3 percentage points. It had no effect on the prevalence of other mental disorders. These effects are large, as they represent a 30 percent increase in the risk of suffering from these conditions. However, the magnitude of these effects is small compared to the mental health effects of stressful life events such as job loss, financial problems or death of a relative or friend which increase the probability to suffer from depression by 295, 485, and 529 percent respectively (Kendler et al., 1999).

To investigate further the mental health effects of the 1972 ROSLA, we estimate the effect of the reform on widely used mental health screening tests. In panel B of Table 3 we present estimates of the effect of the 1972 ROSLA on two clinically validated mental health scores available in Understanding Society: the SF-12 mental health index and the GHQ index. A high SF-12 mental health component score indicates good mental health, whilst a high GHQ is a sign that individuals are likely to suffer from depressive symptoms. We use a 48-month bandwidth because the pre and post-reform trends are linear within this window (See Figure 3). The point estimates suggest that the reform increased GHQ scores and reduced the SF12 score, but the estimates are not statistically significant. Results are similar for some dimensions of life satisfaction presented in Table A.4 and Figure A.5 in Appendix A.3: the point estimates suggest some decrease in satisfaction with income and leisure time, but the estimates are not statistically significant. The Understanding Society sample is much smaller than the APS sample, resulting in a loss of statistical power. In panel C of Table 3, we present estimates of the effect of the 1972 ROSLA on two widely used screening tests for anxiety (GAD-7) and depression (PHQ-9), as well as a measure for probable major depression. We find that the 1972 ROSLA had no statistically significant effect on these measures of mental health in the Biobank.

Because the measures of mental health conditions in the APS are based on self-reported diagnoses of mental health conditions, one could argue that the effect we find could be driven by reporting bias. More education could improve health knowledge and increase awareness of mental health conditions, which could lead to earlier detection of mental health conditions. The reform could have led to an increase in detection and diagnosis of mental health conditions, rather than a genuine increase in prevalence. This hypothesis is unlikely to explain our results fully because there is some indication that the reforms of compulsory schooling laws in the UK had no impact on health-related knowledge (Johnston et al., 2015). In addition, the results from Understanding Society provide some support for our claim that APS results are not solely driven by reporting bias. However, the discrepancy between the findings from the APS and the Biobank are harder to interpret. Whilst the outcomes of the Biobank may be a more reliable indicator of mental health, the Biobank is not representative of the UK population and therefore findings from the Biobank may not be generalisable to the broader population (Fry et al., 2017).

Overall, our results suggest that, at best, the 1972 ROSLA had no positive effect on mental health, with limited evidence that it may have had a negative effect on some outcomes. At the very least we can conclude that the large increase in educational attainment did not translate into better mental health, despite improving average labour market outcomes in adulthood (Grenet, 2013, Dickson, 2013). In the APS sample, we also find that the reform increased employment (See Table A.5 in Appendix A.3). However, due to substantial measurement error, the effect on earnings are imprecisely estimated and not statistically different from zero. We also find that it had no impact on physical health. In Table A.6 in Appendix A.3 we report estimates of the effects of the 1972 ROSLA on the long-term prevalence of physical health conditions. We use the 13 physical health conditions listed in the Annual Population Survey and also construct a variable indicating if respondents suffer from any physical health condition. Overall, the results suggest that the reform had little effect on physical health, which is in line with the results found by Clark and Royer (2013) and Janke et al. (2018).

## 5.3 Sensitivity analysis

*Different specifications*

In Tables A.7, in Appendix A.4 we present RF and IV estimates based on different specifications for our APS sample. RF and IV estimates based on different specifications are presented in Table A.8 for the Understanding Society sample and in Table A.9 for the Biobank. In Column 1 we show estimates from the preferred specification of our regression discontinuity model, which includes only calendar month dummies as covariates. In Column 2 we show estimates based on a model with no covariates. In column 3 we show results from a model with additional exogenous control variables. The results are very stable to the inclusion or exclusion of exogenous variables such as year of survey, third order polynomial in age in month or gender. To account for misreporting of school leaving age for those born in the summer, we include an interaction of the Post-1972 ROSLA dummy with dummies for being born in the summer months (July and August) in column 4. This does not affect our results. In column 5, we show that our results are robust to control for cohort trends using a polynomial of second-order, allowing for different slopes on each sides of the cut-off. Estimates obtained with a triangular kernel are similar to those obtained with a rectangular kernel (Column 6). In column 7 we present unweighted estimates, and we find that they are very similar to the weighted estimates. In column 8 of Table A.7, we show that results do not change when we restrict the APS sample to non-proxy respondents. Because our main outcomes from APS are binary variables, the local linear regression models could be mis-specified. In column 9 we show marginal effects based on probit models. The marginal effects are very close to the estimates from local linear regression models. We also show that our results are robust to excluding the summer-born respondents (column 10 of Tables A.7; column 8 of Tables A.8). Finally, in column 11, we show that our results are robust to excluding respondents born around the cut-off (6 months before and after).

*Different bandwidths*

In Figure A.6 in Appendix A.4 we report RF estimates of the effect of the 1972 ROSLA using different bandwidth values, ranging from 12 to 72, using multiples of 12. We show results from two specifications: one where the pre- and post-reform trends are linear (in dark grey), another where the trends are modelled as a second-order polynomial (in light grey). As the bandwidth increases, the assumption that the trends are linear becomes more and more unlikely to hold; including a second-order polynomial relaxes this assumption.

The top left graph shows the estimates of the effect of the reform on the school leaving age. The estimates remain statistically significant whichever value of bandwidth is used. The RF estimates of the effect of the reform on the prevalence of any mental health condition, depression and and anxiety, and other mental health conditions are robust to the bandwidth choice, although the magnitude of the estimates obtained with a model controlling for the cohort trends linearly decreases as the bandwidth increases. This is not surprising since increasing the bandwidth is likely to increase the bias of the estimates (Lee and Lemieux, 2010). The estimates generated using models that control for cohort trends using linear terms rely on the assumption that the outcome is linearly related to the month-of-birth cohort. This is more likely to hold if the bandwidth is small. With more flexible trends, the estimates remain of similar magnitude regardless of the bandwidth. The estimates for the outcomes in Understanding Society and the Biobank are also stable, although not significantly different from zero.

*Placebo reforms*

Our approach essentially relies on estimating a discontinuity in outcomes of interest across school cohorts. To make sure that the discontinuity in the prevalence of mental health conditions is indeed due to the simultaneous increase in the school leaving age we estimate the effect of placebo reforms, taking place in September every year within 10 years before and 20 years after the true reform, using reduced form regressions as in equation 1. These placebo reforms should not have any significant effect on either the age when left FT education or the prevalence of mental health conditions or mental health scores. Results from the reduced-form models are reported in Figure A.7 in Appendix A.4. Note that because the bandwidth is 36 months, falsified reforms two years before and after the reforms are likely to be contaminated by the true reform and are therefore not considered as valid placebo tests. We observe some discontinuities in the age when respondents left full-time education for those born in the late 1940s and mid 1970s. The latter can be attributed to the higher education expansion that took place in the 1990s (Devereux and Fan, 2011). No placebo reform is significant at the 1 percent level for mental health condition, depression or anxiety. For the outcomes on which the 1972 had no impact, the placebo reform are not statistically significant either, except for one case for the GHQ.

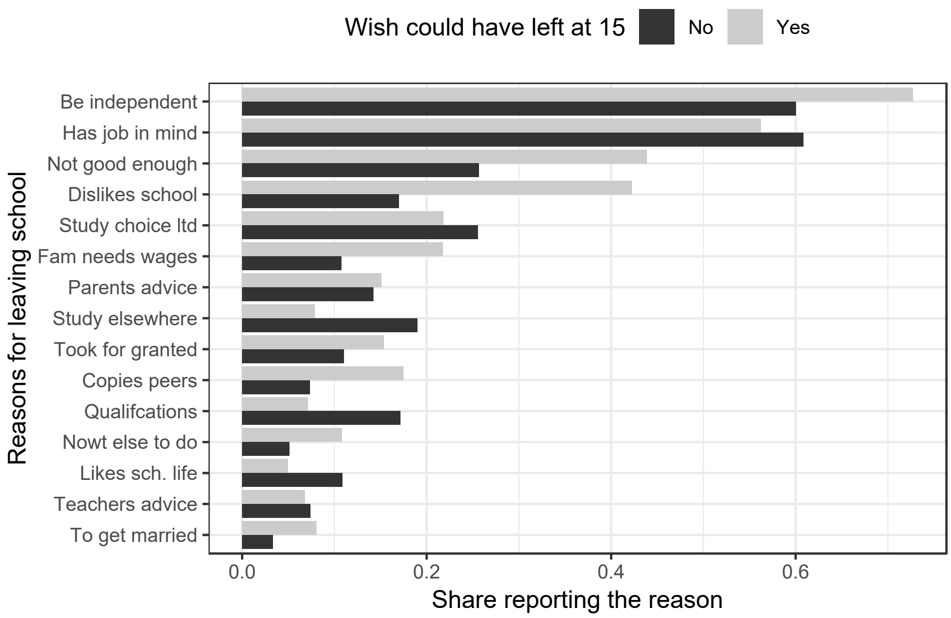
# 6 Estimating direct effects of longer compulsory schooling on mental health

In this section we investigate whether extending the duration of compulsory schooling could affect mental health through channels other than increased educational attainment.

The extra year of schooling caused by the 1972 ROSLA was not a choice but a mandatory constraint and could therefore have been sub-optimal for some individuals. One of the implications of the drop out model developed by Oreopoulos (2007) is that raising the minimum school-leaving age may be detrimental for individuals whose optimal school leaving age is lower than the compulsory one. As highlighted by Eckstein and Wolpin (1999) young people who drop out of high school may have lower school ability and motivation, and consequently lower consumption value of school attendance. In the absence of the reform, these young people would have most probably entered the labour force between ages 15 and 16. This implies that, as a result of the reform, they lost a year of potential work experience, which was replaced by an additional year of schooling. In their analysis of the life-cycle effects of the 1972 ROSLA on earnings, Buscha and Dickson (2015) show that the reform initially had a negative effect on earnings which can be attributed to lower potential labour market experience than those who were not affected by the reform. Some young people may have experienced increased distress from not being able to enter the labour market and attain economic independence.

To investigate whether the 1972 ROSLA could have affected mental health through channels other than increased educational attainment, we analyse data from the NCDS. Just over a quarter of respondents said they wished they could have left school at 15, a proportion which is very close to the estimated increase in participation in education until 16 caused by the 1972 ROSLA (See Clark and Royer (2013) and our estimates in Table 2). Almost all (97.8 percent) of those who wished they could have left school at 15 left school at the earliest opportunity (at age 16), compared to under two thirds of those who did not wish they could have left school at 15 (See Table A.3 in Appendix A.1).

Figure 4: Reasons for leaving school at 16: choice v constraint




Note: NCDS, wave 3 (1974); Sample restricted to respondents who left school at 16

As shown in Figure 4, those who wished they could have left school at 15 report different reasons for leaving school at 16 compared to those who left school at 16 by choice. For instance, 72.6 percent of those who wished they could have left school at 15 reported doing so to be independent and 41.5 percent because they disliked school, compared to 53.8 percent and 17.0 for those who left school at 16 by choice. We restrict our sample to young people who left school at 16 (the new minimum school leaving age) and compare the mental health outcomes of those who reported they wished they could have left at 15 to that of those who willingly chose to leave at 16, conditional on a wide range of pre-determined characteristics (See Section 3.3 for more details). As a measure of mental health, we use the malaise score at age 23, 33 , 42 and 50. We also show that our results are similar if we use a binary variable equal to one if the respondent reports a malaise score of 8 or more.

Table 4 shows estimates of the difference in mental health between those who reported that they wanted to leave school at 15 and those who willingly chose to stay in school until age 16, conditional on mother's characteristics at birth, measures of socioeconomic status (SES) of the family, parental level of interest in their child's education, as well as ability tests and aspiration about first job and expectations about marriage and fertility decisions. We find that those who felt forced to stay at school for an extra year have significantly worse mental health at age 23, 33, 42 and 50, even when conditioning for a wide range of characteristics. At age 23, the conditional difference in malaise score between those who wished they could have left school at 15 and those who did not is 0.216 points which corresponds to 8.3 percent of the mean of the control group. At age 50, the difference is 0.191 points, or 13.3 percent of the control mean. These conditional differences are smaller than the unconditional differences, suggesting that our control vector captures some of the factors that affect both feeling forced to stay in school and mental health in adulthood. Table A.10 in Appendix A.5 shows how estimates vary depending on the model specification.[24] Similar results are obtained when using as a binary variable indicating a malaise score of 8 or above instead of the malaise score (See Table A.11 in Appendix A.5).

Whilst we control for a wide range of factors that could influence both feeling forced to stay in school and mental health in adulthood, there may still be some unobserved factors that drive both. If this is the case, then the conditional differences could be caused by differences in unobserved heterogeneity rather than by having been forced to stay in school. To test whether our findings are likely to be driven by selection on unobserved factors, we first derive coefficients adjusted for selection on the unobservables using the method developed by Altonji et al. (2005) and the proposed implementation by Oster (2019).[25] As shown in Table 4, the bias-adjusted coefficients, which can be interpreted as lower-bound estimates remain positive. This provides reassurance that our results are unlikely to be driven solely by unobserved heterogeneity. Second, we test whether unobserved heterogeneity is likely to drive our results by estimating whether there are differences in measures of mental health at age 7 and 11 between those who willingly left school at 16 to those who wished they could have left school at 15, conditional on observed characteristics. Results presented in columns 1 and 2 of Table 4 show that the un-

[24]We did not use gestation and birth time in our main specification for two reasons: first they are not correlated with wishing to have left school at 15 (See Table A.3). Second, including these results in a lower sample size because of missing values.

[25]For a detailed explanation of the test, see Section 3.3. As suggested by Oster (2019), we set the maximum R-squared as 1.3 times the R-squared from the model with covariates. The test also assumes that selection on the unobservables is as strong as on the observables.

Table 4: Mental health effect of forced education

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | BSAG | BSAG | Depressed (malaise score) | | | |
| | at 7 | at 11 | 23 | 33 | 42 | 50 |
| | Unadjusted | | | | | |
| Wish left at 15 | 2.6653*** | 3.3336*** | 0.8363*** | 0.7767*** | 0.9616*** | 0.3046*** |
| | (0.2611) | (0.2609) | (0.0897) | (0.1063) | (0.1243) | (0.0749) |
| $R^2$ | 0.021 | 0.033 | 0.018 | 0.014 | 0.016 | 0.005 |
| | Adjusted | | | | | |
| Wish left at 15 | -0.3142 | -0.0407 | 0.2162** | 0.3189** | 0.5795*** | 0.1912** |
| | (0.2827) | (0.2870) | (0.1005) | (0.1242) | (0.1452) | (0.0876) |
| $R^2$ | 0.212 | 0.198 | 0.154 | 0.090 | 0.085 | 0.082 |
| N | 4,832 | 4,833 | 4,861 | 3,743 | 3,761 | 3,199 |
| Control mean | 8.010 | 7.548 | 2.618 | 3.269 | 3.341 | 1.433 |
| Bias-adjusted coefficient | - | - | 0.006 | 0.156 | 0.439 | 0.155 |

Note: NCDS. Sample restricted to those who left school at 16. Short malaise scale at age 50. Linear models estimated via OLS. Bias-adjusted coefficient assumes that the maximum $R^2$ is 1.3 times the $R^2$ from the model with covariates (Oster, 2019). Covariates include age of mother at birth, intensity of smoking during pregnancy, whether mother had post-compulsory schooling, father's socioeconomic status at age 7 and 16, parental level of interest in their child's education, reading and maths test score at age 7, 11 and 16, general ability test at age 11, number of O-levels and CSEs taken, region when 16, whether worked part-time at 16, aspirations on leaving school, job aspirations at age 16, and expectations about marriage and fertility at age 16 (interacted with gender). *** p<0.01, ** p<0.05, * p<0.1

conditional and conditional difference in mental health at age 7 and 11 measured by the BSAG total score. Whilst there are unconditional difference in BSAG between the two groups, we find no difference in mental health at age 7 and 11 once we adjust for our control vector. We also find no differences in the depression and anxiety components of the BSAG (See Table A.12 in Appendix A.5). These results further indicate that differences found later in life are unlikely to be due to selection on unobserved factors. However, one potential limitation of this test is that the BSAG may measure different aspects of mental health than the malaise score, which could explain why we find conditional differences in the malaise score in adulthood but not in the BSAG. To mitigate this concern, we show in Table A.14 in Appendix A.5 that the BSAG at age 7 and 11 is a strong and independent predictor of the malaise score at any age in adulthood, both for those who wished they could have left school at 15 and those who did not.

Can the differences in mental health that we observe throughout adulthood be attributed to having been forced to stay in school for an extra year, or could they result from behaviours associated with wishing to have left at 15? One could argue that those who wished they could have left school at 15 may have wished so because they were eager to get a job as soon as possible or wanted to get married and start a family. It could be these behaviours that caused differences in mental health later in life, rather than the long lasting consequences of being forced to stay in a stressful environment. In Table A.13, we show estimates of the conditional differences in the probability of being married, having children and being employed between those who wished they could have left school at 15 and those who did not. We can see that those who felt forced to stay in school are more likely to be married and have children at age 23. However, these differences fade away over time as the proportion of those being married increases. By the age of 33, there are no significant differences between those two groups. The differences in marriage and fertility decisions are short-term only, and therefore are unlikely to explain the differences in mental health problems observed throughout their lives. Similarly, we find no significant differences in the probability to be employed at age 23, 33 and 42. However, we find that those who wished they had left at 15 are significantly less likely to be employed at age 50 compared to those who willingly left school at 16. Since we find differences in mental health at all ages in adulthood but only at age 50 for employment, the differences in mental health are unlikely to be caused by differences in employment rates. Instead, the difference in employment rate at age 50 may be caused by poorer mental health, since poor mental health reduces labour market participation (Frijters et al., 2014).

We present some evidence that forcing young people to stay in school for an extra year may have a persistent negative effect on their mental health. These costs of extending the duration of compulsory schooling may reduce the overall benefits on physical and mental health that a large increase in educational attainment is expected to generate. The magnitude of the average net effects - which are estimated using the RD approach - depends on the extent of these psychological and emotional costs as well as on the returns to the increase in educational attainment caused by the reform. The 1972 ROSLA resulted in a large increase in the proportion of young people staying in school until 16, and increased attainment of end-of-secondary schools diplomas. However, it had no effect on the probability to stay in education beyond the minimum school leaving age. There is some evidence that the reform improved labour market outcomes in adulthood (Grenet, 2013, Dickson, 2013), which may have generated some positive effects on physical health such as reduced diabetes (Davies et al., 2018).

However, we find no improvement in mental health outcomes, which could be due to the direct costs of extending the duration of compulsory schooling. The 1959 reform in France which extended the duration of compulsory schooling by two years had no effect on attainment of qualifications nor labour market outcomes (Grenet, 2013). Courtin et al. (2019) found that the reform led to higher levels of depressive symptoms amongst women in adulthood. As the reform failed to improve young people's lives, it had a negative net effect on mental health.

# 7 Conclusion

Few studies have examined the mental health effects of extending the duration of compulsory schooling. Analysing a reform that raised the minimum school leaving age in Britain in 1972, we provide new evidence on the relationship between schooling and mental health. Using data from two large household surveys and the UK Biobank we find that the additional year of schooling induced by the 1972 reform had no positive effect on mental health and, for some individuals, increased self-reports of diagnosis of depression and anxiety disorder.

We provide evidence that extending the duration of compulsory schooling impact mental health through channels other than increased educational attainment. Extending the duration of compulsory schooling comes at the expense of a year of labour force experience and vocational training. For young people who benefit little from formal schooling, the returns to one extra year of schooling may be lower than the returns to one year of work experience (Eckstein and Wolpin, 1999). As a result, these individuals may be negatively affected by the extension of compulsory schooling. In addition, forcing young people to remain in a competitive academic environment could have psychological and emotional costs for those unlikely to succeed, leading to detrimental effects on the mental health of a subset of the population.

An important implication is that using compulsory schooling laws to estimate the overall health returns to education may be misleading. If compulsory schooling laws affect young people through channels other than increased educational attainment, the exclusion restriction would be violated and the standard regression discontinuity design or instrumental variable approach would yield biased estimates of the health effects of educational attainment.

Our findings suggest that raising the minimum school leaving age may not always be welfare-enhancing. The overall effect of extending the duration of compulsory schooling depends on whether the returns to increased educational attainment offset the psychological and emotional costs of extra schooling.

Our results have important implications for the design of compulsory schooling laws and, potentially, other interventions with an element of coercion. Whilst compulsory schooling laws can raise educational attainment, improve labour market outcomes (Card, 1999) and specific aspects of health (Galama et al., 2018), the coercive element may also have negative effects for some young people. Policies aiming to increase educational attainment should try to minimise these potential adverse effects. Steering early school leavers towards vocational or workplace-based learning best suited to their needs and abilities may raise their economic opportunities while minimising potential psychological and emotional costs (Lerman, 2013). Compelling every young people to stay at school and follow an academic curriculum may entail long-term negative consequences on well-being if it fails to improve their educational attainment and subsequent labour market outcomes.

# References


Albouy, V. and Lequien, L. (2009), 'Does Compulsory Education Lower Mortality?', *Journal of Health Economics* **28**(1), 155–168.

Altonji, J., Elder, T. and Taber, C. (2005), 'Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools', *Journal of Political Economy* **113**(1), 151–184.

Behrman, J. R., Kohler, H.-P., Jensen, V. M., Pedersen, D., Petersen, I., Bingley, P. and Christensen, K. (2011), 'Does more Schooling Reduce Hospitalization and Delay Mortality? New Evidence Based on Danish Twins.', *Demography* **48**(4), 1347–75.

Braakmann, N. (2011), 'The Causal Relationship Between Education, Health and Health Related Behaviour: Evidence from a Natural Experiment in England', *Journal of Health Economics* **30**(4), 753–763.

Brunello, G., Fabbri, D. and Fort, M. (2013), 'The Causal Effect of Education on Body Mass: Evidence from Europe', *Journal of Labor Economics* **31**(1), 195–223.

Buscha, F. and Dickson, M. (2015), 'The Wage Returns to Education over the Life-Cycle : Heterogeneity and the Role of Experience', Discussion Paper No. 9596, IZA.

Calonico, S., Cattaneo, M. D. and Titiunik, R. (2014), 'Robust Data-Driven Inference in the Regression-Discontinuity Design', *Stata Journal* **14**(4), 909–946.

Cameron, I. M., Crawford, J. R., Lawton, K. and Reid, I. C. (2008), 'Psychometric Comparison of PHQ-9 and HADS for Measuring Depression Severity in Primary Care', *British Journal of General Practice* **58**(546), 32–36.

Card, D. (1999), The Causal Effect of Education on Earnings, *in* D. Ashenfelter, Orley C. Card, ed., 'Handbook of Labor Economics', Vol. 3, pp. 1801–1863.

Case, A., Fertig, A. and Paxson, C. (2005), 'The Lasting Impact of Childhood Health and Circumstance', *Journal of Health Economics* **24**(2), 365–389.

Chevalier, A. and Feinstein, L. (2007), 'Sheepskin or Prozac: The Causal Effect of Education on Mental Health', Discussion Paper No. 200715, University College Dublin.

Chevalier, A., Harmon, C., Walker, I. and Zhu, Y. (2004), 'Does Education Raise Productivity, or Just Reflect It?', *The Economic Journal* **114**(499), F499–F517.

Cipollone, P. and Rosolia, A. (2011), 'Schooling and Youth Mortality : Learning from a Mass Military Exemption', *World Bank Policy Research Working Paper Series* **No 5680**. DOI 10.1596/1813-9450-5680.

Clark, D. and Royer, H. (2013), 'The Effect of Education on Adult Health and Mortality', *The American Economic Review* **103**(6), 2087–2120.

Cohen, A. K. and Syme, S. L. (2013), 'Education: a Missed Opportunity for Public Health Intervention', *American Journal of Public Health* **103**(6), 997–1001.

Cornaglia, F., Crivellaro, E. and McNally, S. (2015), 'Mental Health and Education Decisions', *Labour Economics* **33**, 1–12.

Courtin, E., Nafilyan, V., Glymour, M., Goldberg, M., Berr, C., Berkman, L., Zins, M. and Avendano, M. (2019), 'Long-Term Effects of Compulsory Schooling on Physical, Mental and Cognitive Ageing: A Natural Experiment', *Journal of Epidemiology and Community Health* **73**(4).

Crespo, L., López-Noval, B. and Mira, P. (2014), 'Compulsory Schooling, Education, Depression and Memory: New Evidence from SHARELIFE', *Economics of Education Review* **43**, 36–46.

Currie, J. and Stabile, M. (2006), 'Child Mental Health and Human Capital Accumulation: The Case of ADHD', *Journal of Health Economics* **25**(6), 1094–1118.

Cutler, D. M. and Lleras-Muney, A. (2010), 'Understanding Differences in Health Behaviors by Education.', *Journal of health economics* **29**(1), 1–28.

Cutler, D. M. and Lleras-Muney, A. (2012), 'Education and Health: Insights from International Comparisons', NBER Working Paper No. 17738, National Bureau of Economics Research.

Davies, N. M., Dickson, M., Smith, G. D., Van Den Berg, G. J. and Windmeijer, F. (2018), 'The Causal Effects of Education on Health Outcomes in the UK Biobank', *Nature Human Behaviour* **2**(2), 117–125.

Del Bono, E. and Galindo-Rueda, F. (2007), 'The Long Term Impacts of Compulsory Schooling: Evidence from a Natural Experiment in School Leaving Dates', CEE Discussion Papers No. 74, Centre for Economics of Education, LSE.

Denman, J. and McDonald, P. (1996), 'Unemployment Statistics from 1881 to the Present Day', *Labour Market Trends* **104**(1), 5–18.

Devereux, P. J. and Fan, W. (2011), 'Earnings Returns to the British Education Expansion', *Economics of Education Review* **30**(6), 1153–1166.

Dickson, M. (2013), 'The Causal Effect of Education on Wages Revisited', *Oxford Bulletin of Economics and Statistics* **75**(4), 477–498.

Dickson, M. and Smith, S. (2011), 'What determines the return to education: An extra year or a hurdle cleared?', *Economics of Education Review* **30**(6), 1167–1176.

Dursun, B. and Cesur, R. (2016), 'Transforming Lives: The Impact of Compulsory Schooling on Hope and Happiness', *Journal of Population Economics* **29**(3), 911–956.

Eckstein, Z. and Wolpin, K. I. (1999), 'Why Youths Drop Out of High School: The Impact of Preferences, Opportunities, and Abilities', *Econometrica* **67**(6), 1295–1339.

Fischer, M., Karlsson, M. and Nilsson, T. (2013), 'Effects of Compulsory Schooling on Mortality: Evidence from Sweden.', *International journal of environmental research and public health* **10**(8), 3596–3618.

Fletcher, J. M. (2015), 'New Evidence of the Effects of Education on Health in the US: Compulsory Schooling Laws Revisited.', *Social science & medicine (1982)* **127**, 101–7.

Frijters, P., Johnston, D. W. and Shields, M. A. (2014), 'The Effect of Mental Health on Employment: Evidence from Australian Panel Data', *Health Economics* **23**(9), 1058–1071.

Fry, A., Littlejohns, T. J., Sudlow, C., Doherty, N., Adamska, L. et al. (2017), 'Comparison of Sociodemographic and Health-Related Characteristics of UK Biobank Participants With Those of the General Population', *American Journal of Epidemiology* **186**(9), 1026–1034.

Fuchs, V. R. (1982), 'Time Preference and Health: An Exploratory Study', *NBER Chapters* **I**, 93–120.

Galama, T. J., Lleras-Muney, A. and van Kippersluis, H. (2018), ''The Effect of Education on Health and Mortality: A Review of Experimental and Quasi-Experimental Evidence'', *Oxford Research Encyclopedias, Economics and Finance: Health Economics* .

Gathmann, C., Jürges, H. and Reinhold, S. (2015), 'Compulsory Schooling Reforms, Education and Mortality in Twentieth Century Europe', *Social Science and Medicine* **127**, 74–82.

Gaviria, A. and Raphael, S. (2001), 'School-Based Peer Effects and Juvenile Behavior', *The Review of Economics and Statistics* **83**(2), 257–268.

Gill, S. C., Butterworth, P., Rodgers, B. and Mackinnon, A. (2007), 'Validity of the Mental Health Component Scale of the 12-Item Short-Form Health Survey (MCS-12) as Measure of Common Mental Disorders in the General Population.', *Psychiatry Research* **152**(1), 63–71.

Goldberg, D. P. and Blackwell, B. (1970), 'Psychiatric Illness in General Practice. a Detailed Study using a New Method of Case Identification.', *British Medical Journal* **1**(5707), 439–43.

Grenet, J. (2013), 'Is Extending Compulsory Schooling Alone Enough to Raise Earnings? Evidence from French and British Compulsory Schooling Laws', *Scandinavian Journal of Economics* **115**(1), 176–210.

Grossman, M. (1972), 'On the Concept of Health Capital and the Demand for Health', *Journal of Political Economy* **80**(2), 223–55.

Huisman, M. (2004), 'Socioeconomic Inequalities in Mortality among Elderly People in 11 European Populations', *Journal of Epidemiology & Community Health* **58**(6), 468–475.

Hummer, R. A. and Lariscy, J. T. (2011), Educational Attainment and Adult Mortality, *in* R. G. Rogers and E. M. Crimmins, eds, 'International Handbook of Adult Mortality', Springer Netherlands, pp. 241–261.

Imbens, G. W. and Lemieux, T. (2008), 'Regression Discontinuity Designs: A Guide to Practice', *Journal of Econometrics* **142**(2), 615–635.

James, J. (2015), 'Health and Education Expansion', *Economics of Education Review* **49**, 193–215.

Janke, K., Johnston, D. W., Propper, C. and Shields, M. (2018), 'The Causal Effect of Education on Chronic Health Conditions', *IZA Discussion Paper No. 11353* .

Johnston, D. W., Lordan, G., Shields, M. A. and Suziedelyte, A. (2015), 'Education and Health Knowledge: Evidence from UK Compulsory Schooling Reform', *Social Science & Medicine* **127**, 92–100.

Jürges, H., Kruk, E. and Reinhold, S. (2012), 'The Effect of Compulsory Schooling on Health: Evidence from Biomarkers', *Journal of Population Economics* **26**(2), 645–672.

Kemptner, D., Jürges, H. and Reinhold, S. (2011), 'Changes in Compulsory Schooling and the Causal Effect of Education on Health: Evidence from Germany', *Journal of Health Economics* **30**(2), 340–354.

Kendler, K. S., Karkowski, L. M. and Prescott, C. A. (1999), 'Causal Relationship Between Stressful Life Events and the Onset of Major Depression', *American Journal of Psychiatry* **156**(6), 837–841.

Kitagawa, E. M. and Hauser, P. M. (1973), *Differential Mortality in the United States: A Study in Socioeconomic Epidemiology*, Harvard University Press.

Kolesár, M. and Rothe, C. (2018), ‘Inference in Regression Discontinuity Designs with a Discrete Running Variable’, *American Economic Review* **108**(8), 2277–2304.

Kroenke, K., Spitzer, R. L. and Williams, J. B. (2001), ‘The PHQ-9: Validity of a Brief Depression Severity Measure.’, *Journal of General Internal Medicine* **16**(9), 606–13.

Lager, A., Seblova, D., Falkstedt, D. and Lövdén, M. (2016), ‘Cognitive and Emotional Outcomes after Prolonged Education: a Quasi-Experiment on 320 182 Swedish Boys.’, *International Journal of Epidemiology* **46**(1), 303–311.

Lee, D. S. and Lemieux, T. (2010), ‘Regression Discontinuity Designs in Economics’, *Journal of Economic Literature* **48**(2), 281–355.

Lerman, R. (2013), ‘Are Employability Skills Learned in U.S. Youth Education and Training Programs?’, *IZA Journal of Labor Policy* **2**(1), 1–20.

Lewis, G., Pelosi, A. J., Araya, R. and Dunn, G. (1992), ‘Measuring Psychiatric-Disorder in the Community - a Standardized Assessment for Use by Lay Interviewers’, *Psychological Medicine* **22**(2), 465–486.

Lleras-Muney, A. (2005), ‘The Relationship Between Education and Adult Mortality in the United States’, *Review of Economic Studies* **72**(1), 189–221.

Lorant, V., Deliege, D., Eaton, W., Robert, A., Philippot, P. and Ansseau, M. (2003), ‘Socioeconomic Inequalities in Depression: A Meta-Analysis’, *American Journal of Epidemiology* **157**(2), 98–112.

Lundin, A., Hallgren, M., Theobald, H., Hellgren, C. and Torgén, M. (2016), ‘Validity of the 12-Item Version of the General Health Questionnaire in Detecting Depression in the General Population’, *Public Health* **136**, 66–74.

Mazumder, B. (2008), ‘Does Education Improve Health? a Reexamination of the Evidence from Compulsory Schooling Laws’, *Economic Perspectives* **32**(2), 2–16.

Mazzonna, F. (2014), ‘The Long Lasting Effects of Education on Old Age Health: Evidence of Gender Differences’, *Social Science & Medicine* **101**, 129–138.

McCrary, J. (2008), ‘Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test’, *Journal of Econometrics* **142**(2), 698–714.

Meara, E. R., Richards, S. and Cutler, D. M. (2008), ‘The Gap Gets Bigger: Changes in Mortality and Life Expectancy, by Education, 1981-2000’, *Health Affairs* **27**(2), 350–360.

Meghir, C., Palme, M. and Simeonova, E. (2018), ‘Education and Mortality: Evidence from a Social Experiment’, *American Economic Journal: Applied Economics* **19**(2), 234–256.

Mirowsky, J. and Ross, C. E. (2003), *Social Causes of Psychological Distress*, Aldine de Gruyter.

Nichols, A. (2016), ‘RD: Stata Module for Regression Discontinuity Estimation’, Statistical Software Components, Boston College, Department of Economics.

Oreopoulos, P. (2006), ‘Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter’, *American Economic Review* **96**(1), 152–175.

Oreopoulos, P. (2007), ‘Do Dropouts Drop Out Too Soon? Wealth, Health and Happiness from Compulsory Schooling’, *Journal of Public Economics* **91**(11-12), 2213–2229.

Oster, E. (2019), 'Unobservable Selection and Coefficient Stability: Theory and Evidence', *Journal of Business and Economic Statistics* **37**(2), 187–204.

Power, C. and Elliott, J. (2006), 'Cohort Profile: 1958 British Birth Cohort (National Child Development Study)', *International Journal of Epidemiology* **35**(1), 34–41.

Rodgers, B., Pickles, A., Power, C., Collishaw, S. and Maughan, B. (1999), 'Validity of the Malaise Inventory in General Population Samples.', *Social psychiatry and psychiatric epidemiology* **34**(6), 333–41.

Rutter, M. (1970), *Education, Health and Behaviour*, Longman, Harlow.

Sironi, M. (2012), 'Education and Mental Health in Europe', *International Journal of Mental Health* **41**(3), 79–105.

Smith, D. J., Nicholl, B. I., Cullen, B., Martin, D., Ul-Haq, Z. et al. (2013), 'Prevalence and Characteristics of Probable Major Depression and Bipolar Disorder Within UK Biobank: Cross-Sectional Study of 172,751 Participants', *PLoS ONE* **8**(11).

Solon, G., Haider, S. and Wooldridge, J. (2015), 'What are We Weighting For?', *Journal of Human Ressouces* **50**(2), 301–316.

Spitzer, R. L., Kroenke, K., Williams, J. B. W. and Loewe, B. (2006), 'A Brief Measure for Assessing Generalized Anxiety Disorder: The GAD-7', *JAMA Internal Medicine* **166**(10), 1092–1097.

Stewart-Brown, S., Samaraweera, P. C., Taggart, F., Kandala, N. B. and Stranges, S. (2015), 'Socioeconomic Gradients and Mental Health: Implications for Public Health', *British Journal of Psychiatry* **206**(6), 461–465.

Stott, D. H. (1969), *The Social-Adjustment of Children: Manual to the Bristol Social-Adjustment Guides*, University of London Press.

Substance Abuse and Mental Health Services Administration (2016), Key Substance Use and Mental Health Indicators in the United States: Results from the 2015 National Survey on Drug Use and Health. Center for Behavioral Health Statistics and Quality, HHS No. SMA 16-4984, NSDUH Series H-51.

Trogdon, J. G., Nonnemaker, J. and Pais, J. (2008), 'Peer Effects in Adolescent Overweight', *Journal of Health Economics* **27**(5), 1388–1399.

Tyrrell, J., Jones, S., Beaumont, R., Astley, C., Lovell, R., Yaghootkar, H., Tuke, M., Ruth, K., Freathy, R., Hirschhorn, J., Wood, A., Murray, A., Weedon, M. and Frayling, T. (2016), 'Height, Body Mass Index, and Socioeconomic Status: Mendelian Randomisation Study in UK Biobank.', *British Medical Journal* **352**, 1–10.

van Kippersluis, H., O'Donnell, O. and van Doorslaer, E. (2011), 'Long Run Returns to Education: Does Schooling Lead to an Extended Old Age?', *The Journal of Human Resources* **46**(4), 1–33.

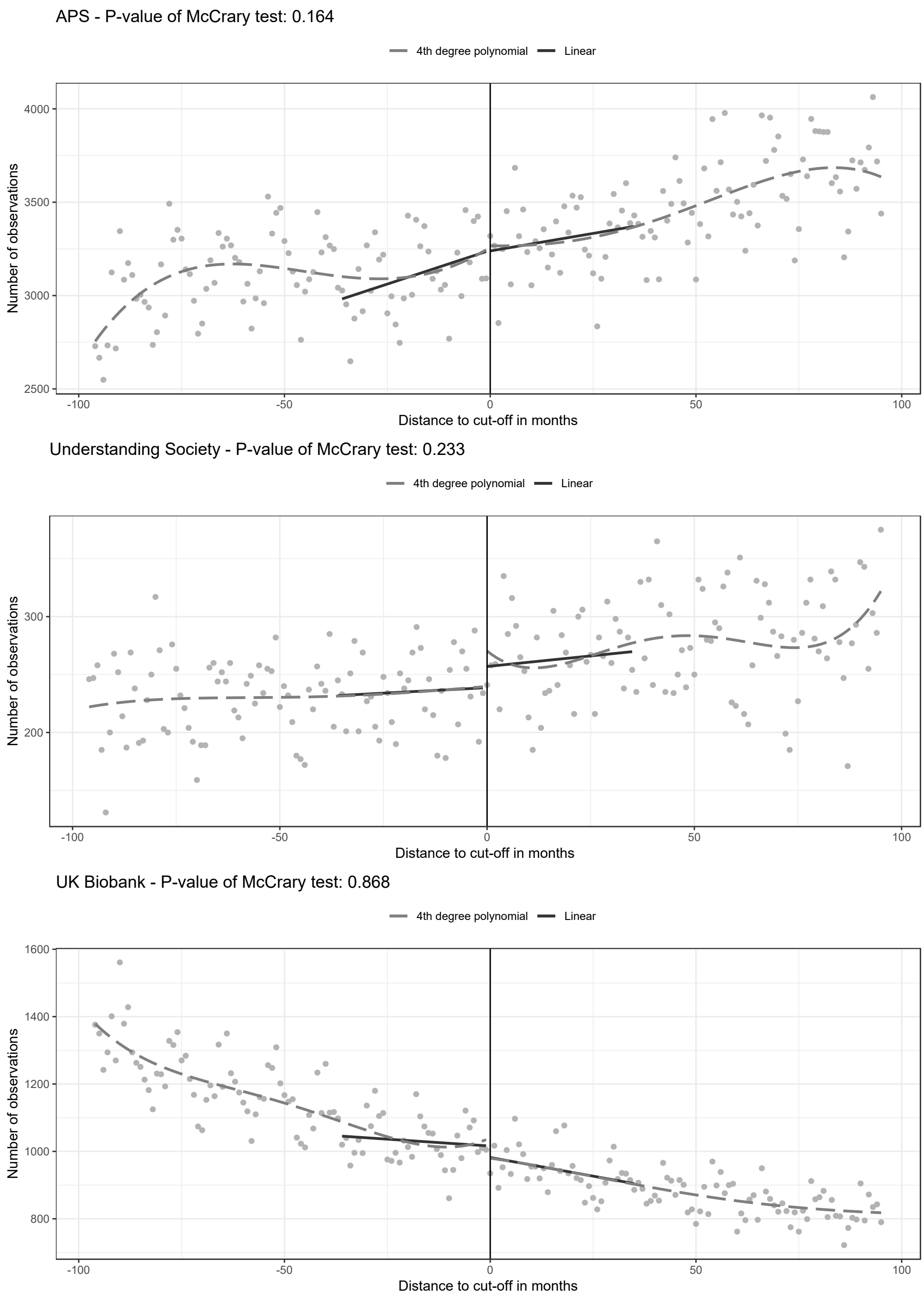
Vos, T., Barber, R. M., Bell, B., Bertozzi-Villa, A. and Biryukov, S. (2015), 'Global, Regional, and National Incidence, Prevalence, and Years Lived with Disability for 301 Acute and Chronic Diseases and Injuries in 188 Countries, 1990-2013: A Systematic Analysis for the Global Burden of Disease Study 2013', *The Lancet* **386**(9995), 743–800.

Woolf, S. H., Johnson, R. E., Phillips, R. L. and Philipsen, M. (2007), 'Giving Everyone the Health of the Educated: An Examination of Whether Social Change Would Save more Lives than Medical Advances', *American Journal of Public Health* **97**(4), 679–683.

# A Appendix

## A.1 Descriptive statistics

Figure A.1: Testing for manipulation around the cutoff




Note: Number of observations by months of birth. Data: APS (2004-2016); USoc (2009-2018); UK Biobank (2006-2010)

Table A.1: Continuity of pre-determined variables

| | Reduced Form | Mean | *N* |
|---|---|---|---|
| | A. APS | | |
| Female | -0.0126** | 0.506 | 230,882 |
| | ( 0.0049) | | |
| Age in months | -0.5179 | 649.8 | 230,882 |
| | ( 0.3939) | | |
| Born in Wales | 0.0024 | 0.061 | 230,882 |
| | ( 0.0017) | | |
| White | -0.0000 | 0.991 | 230,882 |
| | ( 0.0011) | | |
| Year | -0.0407 | 2009.605 | 230,882 |
| | ( 0.0326) | | |
| | B. Understanding Society | | |
| Female | 0.0107 | 0.494 | 21,098 |
| | ( 0.0199) | | |
| Age in months | 0.9329 | 675.3 | 21,095 |
| | ( 1.0916) | | |
| Born in Wales | 0.0042 | 0.055 | 21,098 |
| | ( 0.0082) | | |
| Year | 0.0709 | 2011.970 | 21,095 |
| | ( 0.0906) | | |
| | C. Biobank | | |
| Female | -0.0030 | 0.565 | 45,358 |
| | ( 0.0093) | | |
| Age in months | 0.1058 | 639.708 | 20,913 |
| | ( 0.2591) | | |
| Born in Wales | -0.0009 | 0.053 | 45,358 |
| | ( 0.0041) | | |
| Year | 0.0086 | 2008.969 | 20,913 |
| | ( 0.0235) | | |
| Breastfed as a baby | -0.0087 | 0.379 | 20,913 |
| | ( 0.0214) | | |
| Maternal smoking around birth | -0.0188 | 0.201 | 20,665 |
| | ( 0.0175) | | |

Note: Local linear regression estimated with rectangular kernel and a bandwidth of 36 months. All models include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy. Estimates are weighted by the survey weights. Reported means are calculated among those who were born within three years before the reform. *** p<0.01, ** p<0.05, * p<0.1

Table A.2: Descriptive statistics

| | Pre-ROSLA | | Post-ROSLA | |
|---|---|---|---|---|
| | mean | N | Mean | N |
| | A: APS | | | |
| Mental health condition | 0.078 | 111709 | 0.075 | 118783 |
| | (0.267) | | (0.263) | |
| Depression or anxiety | 0.069 | 111709 | 0.066 | 118783 |
| | (0.254) | | (0.248) | |
| Other mental health conditions | 0.031 | 111709 | 0.030 | 118783 |
| | (0.172) | | (0.171) | |
| Age left FT education | 17.112 | 111913 | 17.326 | 118969 |
| | (2.505) | | (2.278) | |
| CSE | 0.197 | 106375 | 0.311 | 113919 |
| | (0.398) | | (0.463) | |
| O-Level | 0.497 | 108288 | 0.539 | 115450 |
| | (0.500) | | (0.498) | |
| A-Level | 0.266 | 106796 | 0.264 | 113614 |
| | (0.442) | | (0.441) | |
| Age | 53.688 | 111913 | 50.690 | 118969 |
| | (3.524) | | (3.525) | |
| Year | 2009.605 | 111913 | 2009.587 | 118969 |
| | (3.388) | | (3.385) | |
| England | 0.939 | 111913 | 0.942 | 118969 |
| | (0.239) | | (0.234) | |
| Wales | 0.061 | 111913 | 0.058 | 118969 |
| | (0.239) | | (0.234) | |
| Non-proxy | 0.741 | 111913 | 0.734 | 118969 |
| | (0.438) | | (0.442) | |
| | B: Understanding Society | | | |
| Age left school | 16.115 | 13,106 | 16.384 | 15,160 |
| | (1.163) | | (0.936) | |
| SF-12 Mental Component Summary (PCS) | 49.795 | 12,351 | 48.986 | 14,288 |
| | (10.091) | | (10.348) | |
| GHQ | 11.476 | 12,247 | 11.873 | 14,169 |
| | (5.691) | | (5.933) | |
| satisfaction with life overall | 5.037 | 12,263 | 4.931 | 14,163 |
| | (1.550) | | (1.561) | |
| satisfaction with health | 4.633 | 12,269 | 4.594 | 14,163 |
| | (1.787) | | (1.798) | |
| satisfaction with income | 4.545 | 12,256 | 4.364 | 14,166 |
| | (1.752) | | (1.761) | |
| satisfaction with amount of leisure time | 4.641 | 12,257 | 4.398 | 14,161 |
| | (1.668) | | (1.644) | |
| Age | 56.290 | 13,106 | 52.324 | 15,157 |
| | (2.624) | | (2.657) | |
| Year | 2011.982 | 13,106 | 2012.025 | 15,157 |
| | (2.337) | | (2.328) | |
| England | 0.943 | 13,106 | 0.945 | 15,160 |
| | (0.232) | | (0.227) | |
| Wales | 0.057 | 13,106 | 0.055 | 15,160 |
| | (0.232) | | (0.227) | |
| | C: Biobank | | | |
| GAD-7 Anxiety | 2.396 | 13,578 | 2.652 | 11,612 |
| | (3.629) | | (3.798) | |
| PHQ-9 Depression | 3.089 | 13,578 | 3.446 | 11,612 |
| | (4.035) | | (4.230) | |
| Probable major depression | 0.306 | 9,347 | 0.314 | 8,377 |
| | (0.461) | | (0.464) | |
| Age left FT education | 16.792 | 23,589 | 17.015 | 21,769 |
| | (1.943) | | (1.719) | |
| Age | 52.483 | 37,119 | 49.499 | 33,958 |
| | (1.236) | | (1.243) | |
| Year | 2008.623 | 37,119 | 2008.619 | 33,958 |
| | (0.896) | | (0.896) | |
| England | 0.944 | 37,119 | 0.946 | 33,958 |
| | (0.230) | | (0.226) | |
| Wales | 0.054 | 37,119 | 0.051 | 33,958 |
| | (0.225) | | (0.220) | |

Note:Annual Population Survey (2004-2016); Understanding Society (2009-2018); Biobank (2006-2010). Sample is restricted to respondents born in Great Britain within three years before and after September 1957 for APS and Biobank, four years for Understanding Society. Standard deviation are reported in parentheses.

Table A.3: Descriptive statistics - NCDS data

| | Wished left school at 15? | | | | | |
|---|---|---|---|---|---|---|
| | No | | Yes | | | |
| | N | mean | N | mean | Difference | Std Error |
| | All sample | | | | | |
| Left school at 16 | 7,189 | 0.602 | 2,366 | 0.978 | -0.375*** | 0.0102 |
| | Left school at 16 | | | | | |
| Female | 3,857 | 0.534 | 2,056 | 0.447 | 0.0871*** | 0.0136 |
| Mother's age at birth | 3,756 | 27.31 | 2,015 | 27.18 | 0.132 | 0.158 |
| Gestation time (days) | 3,395 | 280.8 | 1,768 | 281.2 | -0.384 | 0.373 |
| Birth weight in ounces | 3,743 | 128.6 | 2,010 | 127.8 | 0.789 | 1.860 |
| Mother during pregnancy: Non smoker | 3,716 | 0.668 | 1,991 | 0.593 | 0.0745*** | 0.0133 |
| Mother during pregnancy: Medium smoker | 3,716 | 0.160 | 1,991 | 0.170 | -0.00991 | 0.0103 |
| Mother during pregnancy: Heavy smoker | 3,716 | 0.112 | 1,991 | 0.161 | -0.0485*** | 0.00929 |
| Mother during pregnancy: Variable smoker | 3,716 | 0.0603 | 1,991 | 0.0763 | -0.0161* | 0.00689 |
| Mother has post-compulsory schooling | 3,747 | 0.203 | 2,013 | 0.111 | 0.0920*** | 0.0103 |
| Father SES at 7: I | 3,857 | 0.0270 | 2,056 | 0.00681 | 0.0202*** | 0.00381 |
| Father SES at 7: II | 3,857 | 0.118 | 2,056 | 0.0550 | 0.0633*** | 0.00801 |
| Father SES at 7: III non-manual | 3,857 | 0.0954 | 2,056 | 0.0545 | 0.0409*** | 0.00744 |
| Father SES at 7: III manual | 3,857 | 0.458 | 2,056 | 0.489 | -0.0314* | 0.0136 |
| Father SES at 7: IV non-manual | 3,857 | 0.0166 | 2,056 | 0.0185 | -0.00189 | 0.00356 |
| Father SES at 7: IV manual | 3,857 | 0.165 | 2,056 | 0.198 | -0.0335** | 0.0104 |
| Father SES at 7: V | 3,857 | 0.0539 | 2,056 | 0.0982 | -0.0443*** | 0.00691 |
| Father SES at 7: Unclear/unknown | 3,857 | 0.0661 | 2,056 | 0.0793 | -0.0132 | 0.00700 |
| Father SES at 16: I | 3,857 | 0.0220 | 2,056 | 0.00438 | 0.0177*** | 0.00341 |
| Father SES at 16: II | 3,857 | 0.145 | 2,056 | 0.0696 | 0.0754*** | 0.00878 |
| Father SES at 16: III non-manual | 3,857 | 0.0783 | 2,056 | 0.0482 | 0.0301*** | 0.00686 |
| Father SES at 16: III manual | 3,857 | 0.364 | 2,056 | 0.406 | -0.0424** | 0.0132 |
| Father SES at 16: IV non-manual | 3,857 | 0.0135 | 2,056 | 0.0117 | 0.00181 | 0.00308 |
| Father SES at 16: IV manual | 3,857 | 0.111 | 2,056 | 0.136 | -0.0255** | 0.00886 |
| Father SES at 16: V | 3,857 | 0.0381 | 2,056 | 0.0579 | -0.0198*** | 0.00566 |
| Father SES at 16: Unclear | 3,857 | 0.229 | 2,056 | 0.266 | -0.0374** | 0.0117 |
| Mother's interest in child's education: Over concerned | 3,857 | 0.0184 | 2,056 | 0.0117 | 0.00673* | 0.00343 |
| Mother's interest in child's education: Very interested | 3,857 | 0.307 | 2,056 | 0.113 | 0.194*** | 0.0114 |
| Mother's interest in child's education: Some interest | 3,857 | 0.341 | 2,056 | 0.324 | 0.0165 | 0.0129 |
| Mother's interest in child's education: Little interest | 3,857 | 0.114 | 2,056 | 0.307 | -0.193*** | 0.0102 |
| Mother's interest in child's education: Cant say | 3,857 | 0.125 | 2,056 | 0.153 | -0.0285** | 0.00931 |
| Mother's interest in child's education: Inapplicable | 3,857 | 0.0135 | 2,056 | 0.0161 | -0.00257 | 0.00325 |
| Mother's interest in child's education: Unknown | 3,857 | 0.0814 | 2,056 | 0.0749 | 0.00651 | 0.00737 |
| Father's interest in child's education: Over concerned | 3,857 | 0.0200 | 2,056 | 0.00681 | 0.0132*** | 0.00336 |
| Father's interest in child's education: Very interested | 3,857 | 0.262 | 2,056 | 0.0807 | 0.181*** | 0.0106 |
| Father's interest in child's education: Some interest | 3,857 | 0.295 | 2,056 | 0.251 | 0.0433*** | 0.0122 |
| Father's interest in child's education: Little interest | 3,857 | 0.127 | 2,056 | 0.312 | -0.184*** | 0.0105 |
| Father's interest in child's education: Cant say | 3,857 | 0.149 | 2,056 | 0.192 | -0.0431*** | 0.0101 |
| Father's interest in child's education: Inapplicable | 3,857 | 0.0283 | 2,056 | 0.0399 | -0.0116* | 0.00483 |
| Father's interest in child's education: Unknown | 3,857 | 0.119 | 2,056 | 0.118 | 0.00130 | 0.00883 |
| Reading test score at age 7 | 3,754 | 23.78 | 2,018 | 20.41 | 3.371*** | 0.190 |
| Reading test score at age 11 | 3,634 | 15.89 | 1,911 | 12.46 | 3.426*** | 0.151 |
| Reading test score at age 16 | 3,822 | 25.75 | 2,017 | 20.57 | 5.173*** | 0.172 |
| Mathematics test score age 7 | 3,759 | 4.895 | 2,016 | 4.226 | 0.669*** | 0.0688 |
| Mathematics test score age 11 | 3,634 | 16.46 | 1,910 | 10.56 | 5.902*** | 0.246 |
| Mathematics test score age 16 | 3,804 | 12.07 | 2,007 | 8.064 | 4.004*** | 0.146 |
| Total score on general ability test | 3,635 | 43.73 | 1,912 | 33.87 | 9.857*** | 0.399 |
| No. of O levels to be taken | 3,713 | 2.125 | 1,951 | 0.621 | 1.504*** | 0.0684 |
| No. of CSEs to be taken | 3,713 | 3.646 | 1,951 | 2.714 | 0.932*** | 0.0774 |
| Highest qualification at 23: Degree or post-secondary | 3,857 | 0.101 | 2,056 | 0.0175 | 0.0839*** | 0.00698 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Highest qualification at 23: A-level or equivalent | 3,857 | 0.160 | 2,056 | 0.0715 | 0.0890*** | 0.00910 |
| Highest qualification at 23: O-level or equivalent | 3,857 | 0.451 | 2,056 | 0.257 | 0.194*** | 0.0130 |
| Highest qualification at 23: Other | 3,857 | 0.0544 | 2,056 | 0.0803 | -0.0258*** | 0.00665 |
| Highest qualification at 23: None | 3,857 | 0.233 | 2,056 | 0.573 | -0.341*** | 0.0123 |
| BSAG at age 7 | 3,752 | 8.170 | 2,012 | 10.92 | -2.754*** | 0.239 |
| BSAG at age 11 | 3,626 | 7.594 | 1,909 | 11.15 | -3.556*** | 0.247 |
| Aspirations on leaving school: Full time studies | 3,857 | 0.183 | 2,056 | 0.0375 | 0.146*** | 0.00906 |
| Aspirations on leaving school: Part study,job | 3,857 | 0.508 | 2,056 | 0.353 | 0.155*** | 0.0134 |
| Aspirations on leaving school: Job,nothing more | 3,857 | 0.156 | 2,056 | 0.399 | -0.242*** | 0.0112 |
| Aspirations on leaving school: Don-t know | 3,857 | 0.149 | 2,056 | 0.200 | -0.0511*** | 0.0102 |
| Hope first job: Artistic, sport | 3,857 | 0.0459 | 2,056 | 0.0263 | 0.0196*** | 0.00529 |
| Hope first job: Prof.managerial | 3,857 | 0.245 | 2,056 | 0.0866 | 0.158*** | 0.0105 |
| Hope first job: Service workers | 3,857 | 0.112 | 2,056 | 0.171 | -0.0585*** | 0.00923 |
| Hope first job: Clerical, office | 3,857 | 0.226 | 2,056 | 0.127 | 0.0994*** | 0.0107 |
| Hope first job: Manual work | 3,857 | 0.200 | 2,056 | 0.375 | -0.175*** | 0.0118 |
| Hope first job: Armed forces | 3,857 | 0.0456 | 2,056 | 0.0520 | -0.00641 | 0.00583 |
| Hope first job: Craftsmen | 3,857 | 0.0124 | 2,056 | 0.0199 | -0.00750* | 0.00332 |
| Hope first job: Unclassifiable | 3,857 | 0.00233 | 2,056 | 0.00146 | 0.000874 | 0.00123 |
| Hope first job: Imprecise | 3,857 | 0.110 | 2,056 | 0.141 | -0.0306*** | 0.00890 |
| Best age to get married: 16 or 17 years | 3,857 | 0.00648 | 2,056 | 0.0331 | -0.0266*** | 0.00338 |
| Best age to get married: 18 or 19 years | 3,857 | 0.108 | 2,056 | 0.185 | -0.0767*** | 0.00927 |
| Best age to get married: 20 or 21 years | 3,857 | 0.369 | 2,056 | 0.376 | -0.00752 | 0.0132 |
| Best age to get married: 22 to 25 years | 3,857 | 0.373 | 2,056 | 0.262 | 0.111*** | 0.0128 |
| Best age to get married: 26 to 30 years | 3,857 | 0.0459 | 2,056 | 0.0482 | -0.00226 | 0.00576 |
| Best age to get married: Over 30 years | 3,857 | 0.00648 | 2,056 | 0.00730 | -0.000814 | 0.00224 |
| Best age to get married: Uncertain | 3,857 | 0.0682 | 2,056 | 0.0447 | 0.0234*** | 0.00648 |
| Best age to get married: No to marriage | 3,857 | 0.0231 | 2,056 | 0.0438 | -0.0207*** | 0.00467 |
| Best age to start family: 16 or 17 years | 3,857 | 0.00285 | 2,056 | 0.0107 | -0.00785*** | 0.00203 |
| Best age to start family: 18 or 19 years | 3,857 | 0.0119 | 2,056 | 0.0569 | -0.0450*** | 0.00443 |
| Best age to start family: 20 or 21 years | 3,857 | 0.137 | 2,056 | 0.202 | -0.0644*** | 0.00997 |
| Best age to start family: 22 to 25 years | 3,857 | 0.520 | 2,056 | 0.448 | 0.0719*** | 0.0136 |
| Best age to start family: 26 to 30 years | 3,857 | 0.183 | 2,056 | 0.142 | 0.0405*** | 0.0102 |
| Best age to start family: Over 30 years | 3,857 | 0.0137 | 2,056 | 0.0175 | -0.00377 | 0.00332 |
| Best age to start family: Uncertain | 3,857 | 0.0998 | 2,056 | 0.0798 | 0.0201* | 0.00792 |
| Best age to start family: No to children | 3,857 | 0.0319 | 2,056 | 0.0433 | -0.0114* | 0.00508 |
| Size of family wants: No children | 3,857 | 0.0290 | 2,056 | 0.0336 | -0.00452 | 0.00470 |
| Size of family wants: One child | 3,857 | 0.0285 | 2,056 | 0.0365 | -0.00796 | 0.00475 |
| Size of family wants: Two children | 3,857 | 0.536 | 2,056 | 0.489 | 0.0476*** | 0.0136 |
| Size of family wants: Three children | 3,857 | 0.203 | 2,056 | 0.203 | 0.000187 | 0.0110 |
| Size of family wants: Four children | 3,857 | 0.0910 | 2,056 | 0.120 | -0.0286*** | 0.00822 |
| Size of family wants: Five children | 3,857 | 0.0101 | 2,056 | 0.0122 | -0.00205 | 0.00283 |
| Size of family wants: Six or more kids | 3,857 | 0.0148 | 2,056 | 0.0185 | -0.00370 | 0.00343 |
| Size of family wants: Dont know | 3,857 | 0.0871 | 2,056 | 0.0880 | -0.000921 | 0.00771 |
| Malaise score at 23 | 3,854 | 2.642 | 2,052 | 3.541 | -0.899*** | 0.0827 |
| Malaise score at 33 | 3,026 | 3.272 | 1,468 | 4.105 | -0.833*** | 0.0976 |
| Malaise score at 42 | 3,043 | 3.387 | 1,475 | 4.358 | -0.971*** | 0.116 |
| Malaise score at 50 | 2,627 | 1.478 | 1,208 | 1.732 | -0.254*** | 0.0692 |
| Depressed (Malaise score ¿=8) at 23 | 3,854 | 0.0636 | 2,052 | 0.126 | -0.0626*** | 0.00759 |
| Depressed (Malaise score ¿=8) at 33 | 3,026 | 0.0803 | 1,468 | 0.144 | -0.0634*** | 0.00954 |
| Depressed (Malaise score ¿=8) at 42 | 3,043 | 0.114 | 1,475 | 0.199 | -0.0846*** | 0.0110 |
| Depressed (Malaise score ¿=8) at 50 | 2,627 | 0.142 | 1,208 | 0.187 | -0.0451*** | 0.0126 |
| SF-36 General health score | 2,392 | 69.21 | 1,103 | 64.28 | 4.930*** | 0.794 |
| In paid work at age 23 | 3,857 | 0.798 | 2,056 | 0.703 | 0.0955*** | 0.0115 |
| In paid work at age 33 | 3,857 | 0.819 | 2,056 | 0.756 | 0.0627*** | 0.0110 |
| In paid work at age 42 | 3,857 | 0.850 | 2,056 | 0.770 | 0.0792*** | 0.0104 |
| In paid work at age 50 | 3,857 | 0.844 | 2,056 | 0.748 | 0.0964*** | 0.0106 |

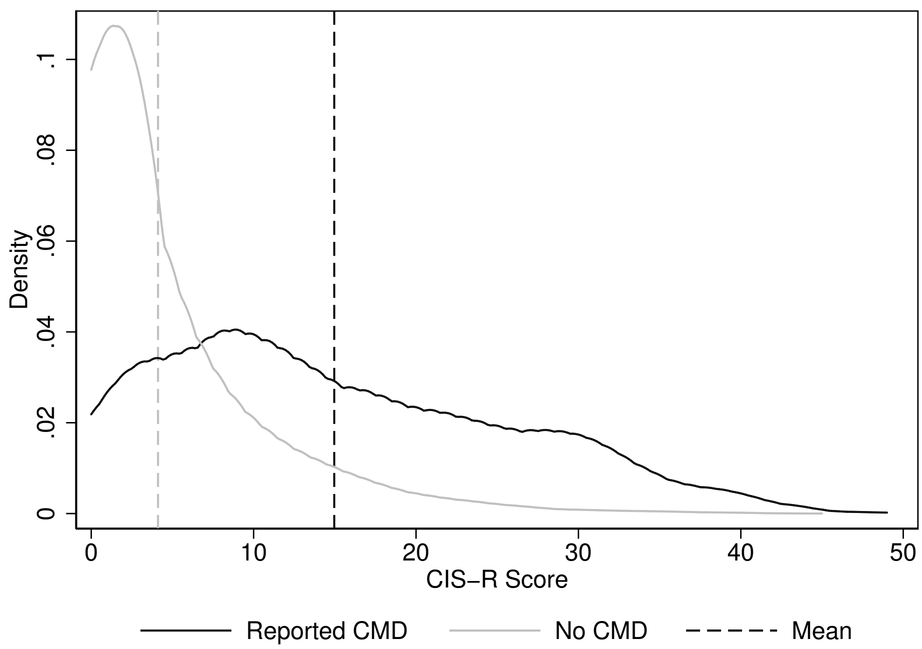
Note: National Child Development Study.

## A.2 Self-reported mental health conditions and clinical measures of mental health

In this section, we show that self-reports of mental health conditions are strongly correlated with clinical assessments of mental health problems. We use data from the 2007 Adult Psychiatric Morbidity Survey, which contains information on whether respondents report to suffer from any mental health condition (as in the APS) and the Clinical Interview Schedule - Revised (CISR). The CISR is a structured validated instrument used to assess the prevalence of common mental health conditions (Lewis et al., 1992). A CISR score of 12 of more indicates the presence of a common mental health condition.

Figure A.2 displays the distribution of the CIS-R score for two groups - those who reported suffering from a mental health condition and those who did not. Respondents who reported suffering from anxiety, depression or other mental health conditions had an average CIS-R score of 15.0, compared to 4.1 for those who did not report having a mental health problem.

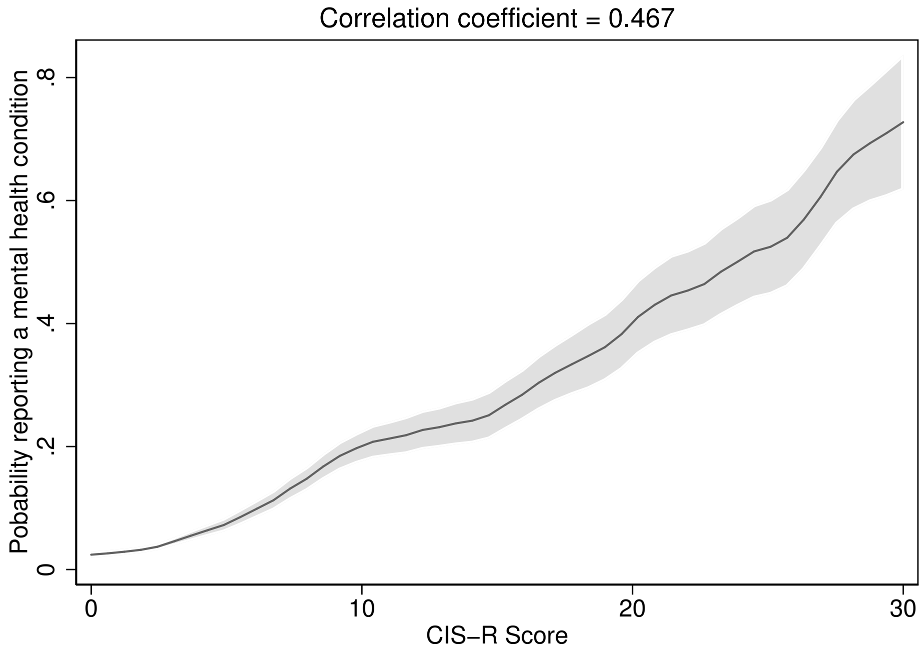
Figure A.2: Distribution of CIS-R score by whether reported having any mental health condition




Notes: 2007 Adult Psychiatric Morbidity Survey. Kernel density estimates obtained with a bandwidth of two. The mean is displayed the dotted lines.

Figure A.3 displays the probability to report having any mental health condition as a function of CIS-R score. Self-reported mental health condition is strongly associated with the CIS-R score. The correlation coefficient is 0.467 and the relationship is almost linear. The magnitude of the correlation between these two variables suggest that self-reported mental health condition may be a good proxy for the 'true' mental health of respondents.

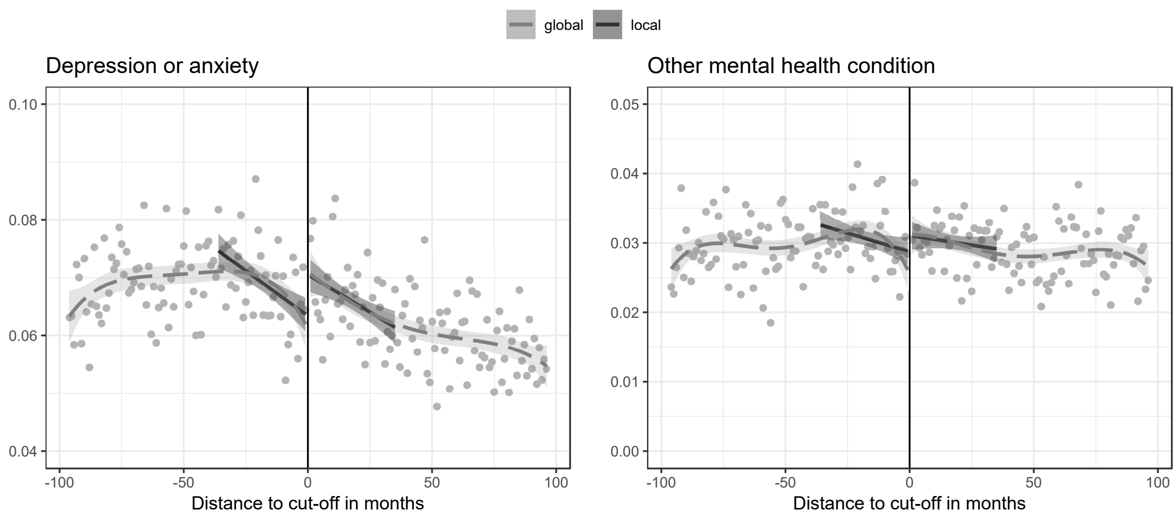
Figure A.3: Probability to report having any mental health condition by CIS-R score




Notes: 2007 Adult Psychiatric Morbidity Survey. The graph is restricted to respondents with a CIS-R score of 30 or less; correlation coefficient calculated over the entire sample. The shaded area represents the 95% confidence interval.

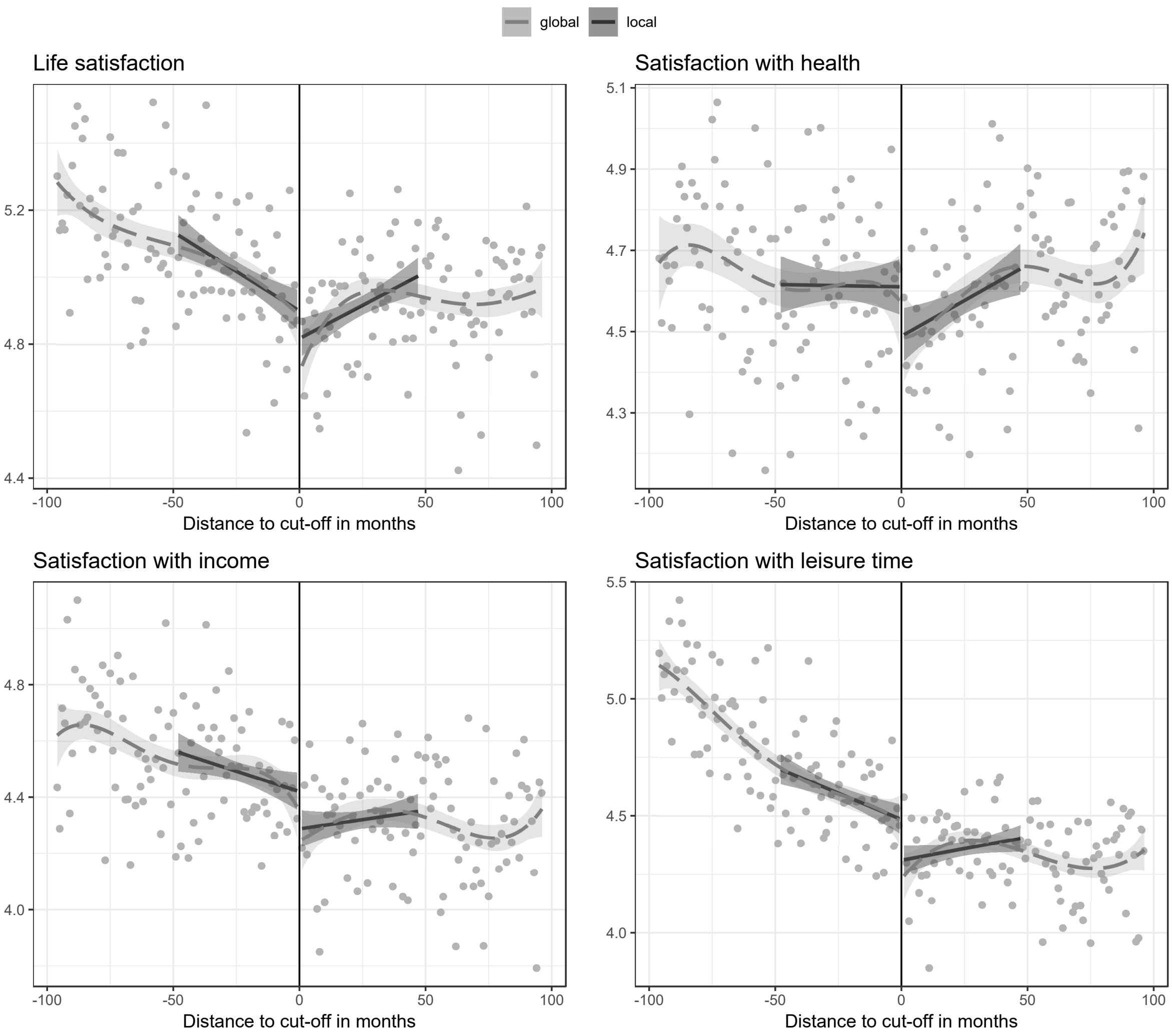
## A.3 Effect of the 1972 ROSLA on other health outcomes

Figure A.4: Other mental health outcomes by month-year of birth




Note: Points show averages by bands of two months. Fitted lines are adjusted for calendar month of birth, and calculated separately before and after the cut-off for the reform. The local line is a linear fit calculated on a 36 months period. The global line is fourth degree polynomial. 95% confidence intervals are reported in shaded areas. Data: APS (2004-2016)

Figure A.5: Life Satisfaction by month-year of birth




Note: Note: Points show averages by bands of two months. Fitted lines are adjusted for calendar month of birth, and calculated separately before and after the cut-off for the reform. The local line is a linear fit calculated on a 48 months period. The global line is fourth degree polynomial. 95% confidence intervals are reported in shaded areas. Understanding Society (2009-2018).

Table A.4: Effect of 1972 ROSLA on life satisfaction

| | OLS | Reduced Form | IV | Mean | *N* |
|---|---|---|---|---|---|
| Satisfaction with life overall | 0.177*** | -0.0549 | -0.1469 | 4.827 | 26,426 |
| | ( 0.062) | ( 0.0766) | ( 0.2087) | | |
| Satisfaction with health | 0.331*** | -0.0970 | -0.2578 | 4.342 | 26,432 |
| | ( 0.066) | ( 0.0854) | ( 0.2362) | | |
| Satisfaction with income | 0.352*** | -0.1141 | -0.3078 | 4.152 | 26,422 |
| | ( 0.067) | ( 0.0871) | ( 0.2504) | | |
| Satisfaction with amount of leisure time | 0.191*** | -0.1066 | -0.2861 | 4.450 | 26,418 |
| | ( 0.057) | ( 0.0780) | ( 0.2189) | | |

Note: Local linear regression estimated with rectangular kernel and a bandwidth of 48 months. All models include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy and calendar month of birth dummies. OLS models include a third-order polynomial in age (measured in months) and binary variables for sex, year of survey, calendar month of birth and proxy interview. OLS models are estimated on a sample restricted to those who left school at 15 or 16. Estimates are weighted by the survey weights. Standard errors are clustered at the individual level. Reported means are calculated among those who left school aged 15 and were born within three years before the reform. Data: Understanding Society (2009 - 2018)
*** p<0.01, ** p<0.05, * p<0.1

Table A.5: Effect of 1972 ROSLA on labour market outcomes

| | OLS | Reduced Form | IV | Mean | *N* |
|---|---|---|---|---|---|
| Employed | 0.078*** | 0.0095** | 0.0458** | 0.698 | 228,924 |
| | ( 0.003) | ( 0.0041) | ( 0.0199) | | |
| Employed (sample restricted to | 0.081*** | 0.0141*** | 0.0638*** | 0.622 | 168,296 |
| those with valid earnings) | ( 0.004) | ( 0.0053) | ( 0.0243) | | |
| Weekly earnings, incl. 0 | 79.846*** | 1.8547 | 8.3648 | 245.517 | 168,296 |
| | ( 3.037) | ( 7.9042) | ( 35.4071) | | |
| Hours worked | 0.106 | -0.0787 | -0.4135 | 36.021 | 176,667 |
| | ( 0.113) | ( 0.1464) | ( 0.7731) | | |
| Gross hourly pay | 1.952*** | -0.1853 | -0.9190 | 11.129 | 122,031 |
| | ( 0.134) | ( 0.2548) | ( 1.3115) | | |
| Log hourly pay | 0.132*** | -0.0086 | -0.0425 | 2.303 | 122,031 |
| | ( 0.005) | ( 0.0083) | ( 0.0446) | | |

Note: Local linear regression estimated with rectangular kernel and a bandwidth of 36 months. All models include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy and calendar month of birth dummies. OLS models include a third-order polynomial in age (measured in months) and binary variables for sex, year of survey, calendar month of birth and proxy interview. OLS models are estimated on a sample restricted to those who left school at 15 or 16. Estimates are weighted by the survey weights. Standard errors clustered by household. Reported means are calculated among those who left school aged 15 and were born within three years before the reform. Data: APS (2004-2016).
*** p<0.01, ** p<0.05, * p<0.1

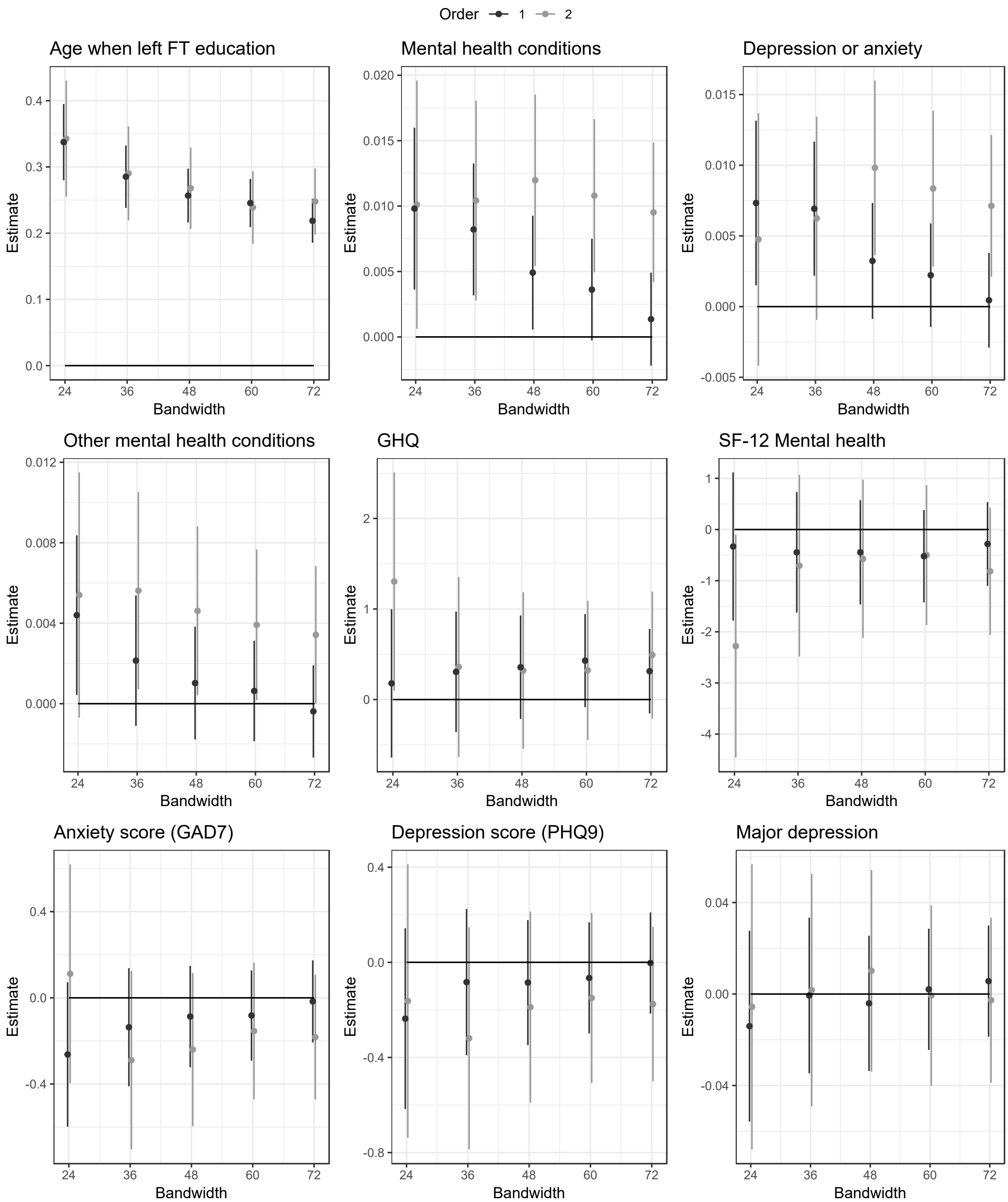
Table A.6: Effect of 1972 ROSLA on prevalence of physical health problems

| Dependent variables | OLS | Reduced Form | IV | Mean | *N* |
|---|---|---|---|---|---|
| Physical health problem | -0.074*** | 0.0008 | 0.0040 | 0.487 | 230,492 |
| | ( 0.004) | ( 0.0050) | ( 0.0239) | | |
| Problems with arms and hands | -0.054*** | -0.0028 | -0.0132 | 0.154 | 230,492 |
| | ( 0.003) | ( 0.0029) | ( 0.0136) | | |
| ....legs or feet | -0.056*** | -0.0026 | -0.0126 | 0.189 | 230,492 |
| | ( 0.003) | ( 0.0033) | ( 0.0155) | | |
| ....back or neck | -0.060*** | 0.0017 | 0.0083 | 0.187 | 230,492 |
| | ( 0.003) | ( 0.0032) | ( 0.0157) | | |
| Difficulty in seeing | -0.008*** | 0.0003 | 0.0016 | 0.027 | 230,492 |
| | ( 0.001) | ( 0.0014) | ( 0.0067) | | |
| Difficulty in hearing | -0.015*** | -0.0025 | -0.0119 | 0.044 | 230,492 |
| | ( 0.002) | ( 0.0017) | ( 0.0080) | | |
| A speech impediment | -0.001** | 0.0006 | 0.0029 | 0.005 | 230,492 |
| | ( 0.001) | ( 0.0006) | ( 0.0029) | | |
| Severe disfigurement, skin conditions, allergies | -0.013*** | 0.0027 | 0.0128 | 0.043 | 230,492 |
| | ( 0.002) | ( 0.0018) | ( 0.0087) | | |
| Chest or breathing problems, asthma, bronchitis | -0.036*** | 0.0001 | 0.0007 | 0.119 | 230,492 |
| | ( 0.002) | ( 0.0027) | ( 0.0131) | | |
| Heart, blood pressure or blood circulation problems | -0.036*** | 0.0005 | 0.0024 | 0.186 | 230,492 |
| | ( 0.003) | ( 0.0035) | ( 0.0166) | | |
| Stomach, liver kidney or digestive problems | -0.025*** | -0.0033 | -0.0157 | 0.085 | 230,492 |
| | ( 0.002) | ( 0.0023) | ( 0.0113) | | |
| Diabetes | -0.010*** | -0.0038* | -0.0184* | 0.065 | 230,492 |
| | ( 0.002) | ( 0.0022) | ( 0.0105) | | |
| Epilepsy | -0.001 | -0.0005 | -0.0023 | 0.010 | 230,492 |
| | ( 0.001) | ( 0.0009) | ( 0.0044) | | |
| Severe or specific learning difficulties | -0.003*** | -0.0015* | -0.0071* | 0.010 | 230,492 |
| | ( 0.001) | ( 0.0008) | ( 0.0039) | | |
| Progressive illness (eg cancer) | -0.004*** | 0.0002 | 0.0011 | 0.032 | 230,492 |
| | ( 0.001) | ( 0.0016) | ( 0.0078) | | |
| Other health problems or disabilities | -0.012*** | -0.0006 | -0.0028 | 0.079 | 230,492 |
| | ( 0.002) | ( 0.0025) | ( 0.0120) | | |

Note: Local linear regression estimated with rectangular kernel and a bandwidth of 36 months. All models include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy. OLS models include a third-order polynomial in age (measured in months) and binary variables for sex, year of survey, calendar month of birth and proxy interview. OLS models are estimated on a sample restricted to those who left school at 15 or 16. Estimates are weighted by the survey weights. Reported means are calculated among those who left school aged 15 and were born within three years before the reform.

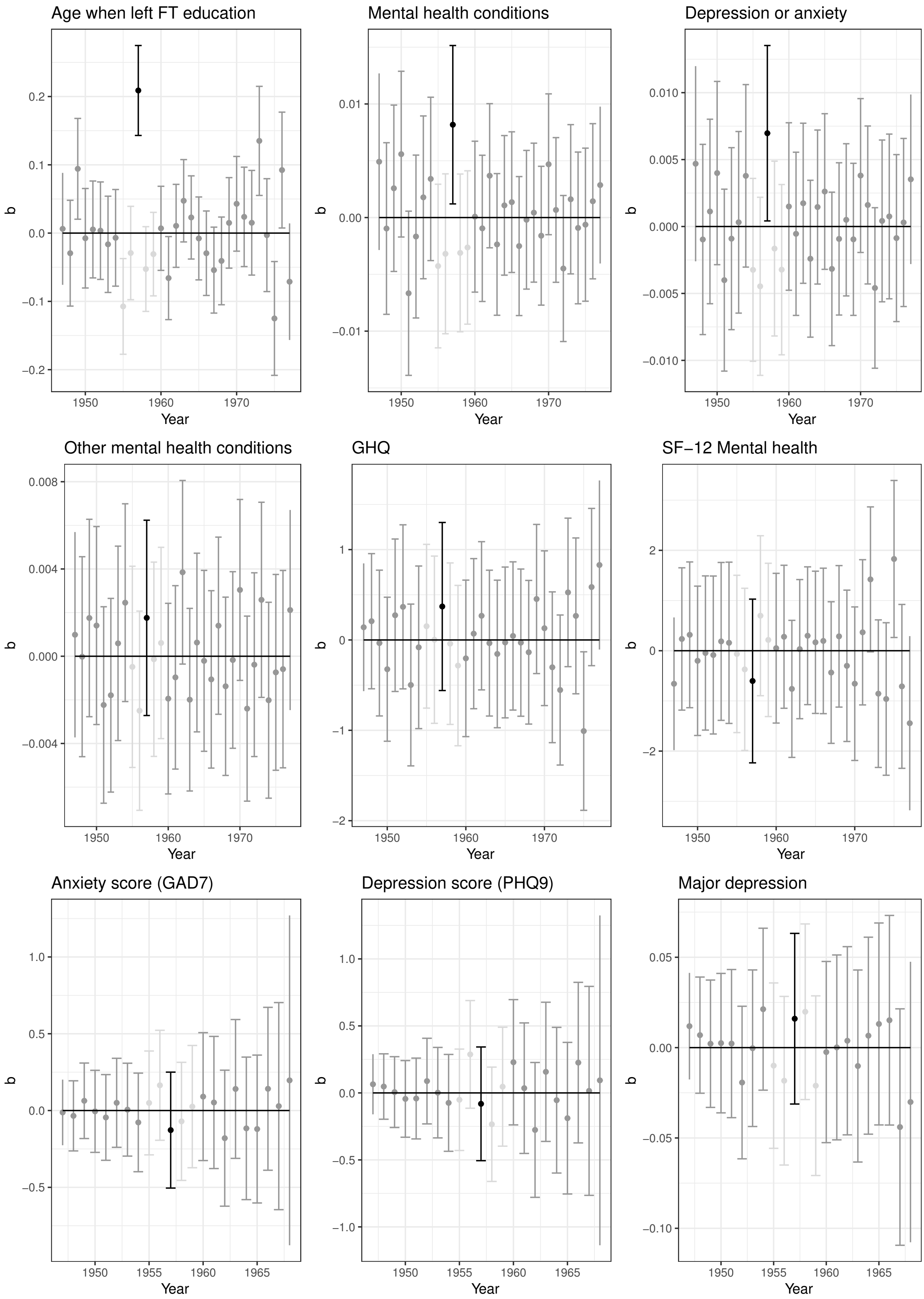
## A.4 Sensitivity analysis - impact of ROSLA

Figure A.6: Reduced-form estimates with different bandwidths




Note: Local linear regression estimated with rectangular kernel. All models include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy and calendar month of birth dummies. Estimates are weighted by the survey weights and standard errors clustered by household (APS) or individuals (Understanding Society). 95 percent confidence intervals.

Figure A.7: Reduced-Form estimates of placebo reforms




Note: 1972 ROSLA in black, placebo reforms in grey. Local linear regression estimated with rectangular kernel and a bandwidth of 36 months (48 for USoc). All models include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy and calendar month of birth dummies. Estimates are weighted by the survey weights and standard errors clustered by household (APS) or individuals (Understanding Society). 99 percent confidence intervals.

## Table A.7: Different specifications - APS

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Age left FT education N = 230,492 | | | | | | | | | | |
| RF | 0.2089*** | 0.2831*** | 0.2077*** | 0.2017*** | 0.2120*** | 0.2178*** | 0.2067*** | 0.2158*** | | 0.1982*** | 0.2324*** |
| | (0.0253) | (0.0243) | (0.0253) | (0.0260) | (0.0372) | (0.0271) | (0.0207) | (0.0292) | | (0.0279) | (0.0356) |
| | Mental health condition,N =230,492 | | | | | | | | | | |
| RF | 0.0082*** | 0.0082*** | 0.0085*** | 0.0076*** | 0.0104*** | 0.0090*** | 0.0065*** | 0.0086*** | 0.0082*** | 0.0084*** | 0.0113*** |
| | (0.0027) | (0.0026) | (0.0027) | (0.0027) | (0.0039) | (0.0029) | (0.0024) | (0.0033) | (0.0027) | (0.0029) | (0.0038) |
| IV | 0.0391*** | 0.0290*** | 0.0410*** | 0.0377** | 0.0490** | 0.0415*** | 0.0316*** | 0.0400** | | 0.0422*** | 0.0488*** |
| | (0.0140) | (0.0096) | (0.0142) | (0.0147) | (0.0211) | (0.0145) | (0.0121) | (0.0167) | | (0.0164) | (0.0184) |
| | Depression or anxiety,N =230,492 | | | | | | | | | | |
| RF | 0.0070*** | 0.0069*** | 0.0073*** | 0.0067*** | 0.0063* | 0.0070*** | 0.0058** | 0.0075** | 0.0070*** | 0.0074*** | 0.0128*** |
| | (0.0025) | (0.0024) | (0.0025) | (0.0026) | (0.0037) | (0.0027) | (0.0022) | (0.0031) | (0.0025) | (0.0028) | (0.0036) |
| IV | 0.0334** | 0.0245*** | 0.0350*** | 0.0333** | 0.0298 | 0.0323** | 0.0279** | 0.0350** | | 0.0373** | 0.0553*** |
| | (0.0130) | (0.0089) | (0.0131) | (0.0137) | (0.0187) | (0.0133) | (0.0114) | (0.0156) | | (0.0152) | (0.0181) |
| | Other mental health conditions,N =230,492 | | | | | | | | | | |
| RF | 0.0018 | 0.0021 | 0.0018 | 0.0018 | 0.0052** | 0.0030 | 0.0024 | 0.0002 | 0.0018 | 0.0018 | -0.0000 |
| | (0.0017) | (0.0016) | (0.0017) | (0.0018) | (0.0025) | (0.0018) | (0.0015) | (0.0021) | (0.0017) | (0.0019) | (0.0025) |
| IV | 0.0084 | 0.0076 | 0.0087 | 0.0089 | 0.0247* | 0.0137 | 0.0118 | 0.0009 | | 0.0090 | -0.0001 |
| | (0.0084) | (0.0059) | (0.0084) | (0.0088) | (0.0129) | (0.0087) | (0.0075) | (0.0099) | | (0.0096) | (0.0106) |
| Calendar month of birth | Yes | No | Yes | Yes | Yes | Yes | | Yes | Yes | Yes | Yes |
| Other covariates | No | No | Yes | No | No | No | No | No | No | No | No |
| Summer months interaction | No | No | No | Yes | No | No | No | No | No | No | No |
| Local polynomial order | One | One | One | One | Two | One | One | One | One | One | One |
| Estimation | R | R | R | R | R | T | R | R | Probit | R | R |
| Weights | Yes | Yes | Yes | Yes | Yes | Yes | No | Yes | Yes | Yes | |
| Sample | All | All | All | All | All | All | All | Non proxy | All | Excl summer born | Excl. 6 months before and after cutoff |

Note: Other covariates include sex, country of birth and response status (proxy/non-proxy). Estimation: R refers to rectangular kernel; T to triangular kernel.

Table A.8: Different specifications - Understanding Society

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Age left school (Understanding Society), N = 26,639 | | | | | | | | |
| RF | 0.3042*** | 0.3653*** | 0.3014*** | 0.2930*** | 0.3217*** | 0.3131*** | 0.2703*** | 0.2666*** | 0.3701*** |
| | (0.0638) | (0.0623) | (0.0639) | (0.0662) | (0.0945) | (0.0683) | (0.0613) | (0.0703) | (0.0827) |
| | GHQ, N = 26,416 | | | | | | | | |
| RF | 0.4180 | 0.3568 | 0.3822 | 0.2972 | 0.3718 | 0.3972 | 0.2939 | 0.2995 | 0.1308 |
| | (0.2980) | (0.2899) | (0.2997) | (0.3065) | (0.4443) | (0.3261) | (0.2872) | (0.3267) | (0.3825) |
| IV | 1.3498 | 0.9602 | 1.2422 | 0.9897 | 1.1184 | 1.2385 | 1.0944 | 1.1060 | 0.3521 |
| | (1.0170) | (0.8070) | (1.0212) | (1.0539) | (1.3970) | (1.0606) | (1.1172) | (1.2510) | (1.0381) |
| | SF12 Mental health, N = 26,639 | | | | | | | | |
| RF | -0.5569 | -0.4456 | -0.5277 | -0.4449 | -0.6853 | -0.6206 | -0.3210 | -0.5085 | 0.1066 |
| | (0.5279) | (0.5167) | (0.5297) | (0.5412) | (0.7944) | (0.5759) | (0.5033) | (0.5764) | (0.6707) |
| IV | -1.8310 | -1.2198 | -1.7512 | -1.5181 | -2.1304 | -1.9819 | -1.1874 | -1.9076 | 0.2880 |
| | (1.7989) | (1.4424) | (1.8159) | (1.8958) | (2.5882) | (1.9052) | (1.9035) | (2.2422) | (1.8092) |
| Calendar month of birth | Yes | No | Yes | Yes | No | No | No | No | No |
| Other covariates | No | No | Yes | Yes | No | Nos | No | No | No |
| Summer months interaction | No | No | No | Yes | No | No | No | No | No |
| Local polynomial order | one | One | One | One | Two | One | One | One | One |
| Estimation | R | R | R | R | R | T | R | R | R |
| Weights | Yes | Yes | Yes | Yes | Yes | Yes | No | Yes | Yes |
| Sample | All | All | All | All | All | All | All | Excl summer born | Excl. 6 months before and after cutoff |

Note: All models include calendar month dummies. Other covariates include sex, country of birth and wave dummies. Estimation: T refers to triangular kernel. Estimation: T refers to triangular kernel. Standard errors clustered at month of birth are presented in parentheses.

Table A.9: Different specifications - Biobank

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Age left FT education | | | | | | | |
| RF | 0.1248* | 0.1771*** | 0.1549** | 0.1541** | 0.3209*** | 0.1802** | 0.0660 | 0.1398* |
| | (0.0680) | (0.0647) | (0.0723) | (0.0723) | (0.0970) | (0.0721) | (0.0963) | (0.0747) |
| | GAD-7,N = 13,457 | | | | | | | |
| RF | -0.1274 | -0.1363 | -0.1481 | -0.1482 | -0.2893 | -0.1732 | -0.0770 | -0.1615 |
| | (0.1451) | (0.1390) | (0.1536) | (0.1536) | (0.2099) | (0.1553) | (0.2034) | (0.1582) |
| IV | -1.0204 | -0.7698 | -0.9563 | -0.9614 | -0.9016 | -0.9609 | -1.1658 | -1.1555 |
| | (1.2710) | (0.8244) | (1.0699) | (1.0759) | (0.7015) | (0.9333) | (3.4570) | (1.2646) |
| | PHQ-9,N = 13,457 | | | | | | | |
| RF | -0.0815 | -0.0832 | -0.1018 | -0.1016 | -0.3194 | -0.1600 | 0.1189 | -0.1003 |
| | (0.1631) | (0.1559) | (0.1739) | (0.1739) | (0.2369) | (0.1748) | (0.2280) | (0.1785) |
| IV | -0.6529 | -0.4699 | -0.6569 | -0.6592 | -0.9954 | -0.8879 | 1.7999 | -0.7175 |
| | (1.3324) | (0.8864) | (1.1435) | (1.1493) | (0.7840) | (1.0146) | (4.4895) | (1.3084) |
| | Major Depression,N = 11,202 | | | | | | | |
| RF | 0.0160 | -0.0006 | 0.0119 | 0.0120 | 0.0017 | 0.0135 | 0.0190 | 0.0105 |
| | (0.0181) | (0.0172) | (0.0218) | (0.0218) | (0.0258) | (0.0193) | (0.0256) | (0.0199) |
| IV | 0.1124 | -0.0030 | 0.0680 | 0.0681 | 0.0060 | 0.0774 | 0.1143 | 0.0651 |
| | (0.1406) | (0.0817) | (0.1290) | (0.1288) | (0.0901) | (0.1163) | (0.1694) | (0.1284) |
| Calendar month of birth | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Other covariates | No | No | Yes | Yes | No | No | No | No |
| Summer months interaction | No | No | No | Yes | No | No | No | No |
| Local polynomial order | One | One | One | One | Two | One | One | One |
| Estimation | R | R | R | R | R | T | R summer born | R before and after cutoff |

Note: All models include calendar month dummies. Other covariates include sex, country of birth and wave dummies. Estimation: T refers to triangular kernel. Estimation: T refers to triangular kernel. Standard errors clustered at month of birth are presented in parentheses.

## A.5 Sensitivity analysis -NCDS

Table A.10: Mental health effect of forced education - different specifications

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | BSAG at age 7 | | | | | | |
| Wished left at 15 | 2.665*** | 2.157*** | 1.689*** | 0.123 | -0.314 | -0.314 | -0.295 |
| | (0.261) | (0.264) | (0.272) | (0.268) | (0.283) | (0.283) | (0.297) |
| R2 | 0.021 | 0.060 | 0.072 | 0.195 | 0.212 | 0.212 | 0.217 |
| N | 4,832 | 4,832 | 4,832 | 4,832 | 4,832 | 4,832 | 4,331 |
| | BSAG at age 11 | | | | | | |
| Wished left at 15 | 3.334*** | 2.790*** | 2.329*** | 0.441 | -0.041 | -0.041 | -0.147 |
| | (0.261) | (0.264) | (0.273) | (0.274) | (0.287) | (0.287) | (0.303) |
| R2 | 0.033 | 0.071 | 0.081 | 0.173 | 0.198 | 0.198 | 0.198 |
| N | 4,833 | 4,833 | 4,833 | 4,833 | 4,833 | 4,833 | 4,330 |
| | Malaise score at 23 | | | | | | |
| Wished left at 15 | 0.836*** | 0.848*** | 0.709*** | 0.413*** | 0.216** | 0.216** | 0.201* |
| | (0.090) | (0.088) | (0.091) | (0.096) | (0.100) | (0.100) | (0.107) |
| R2 | 0.018 | 0.103 | 0.110 | 0.130 | 0.154 | 0.154 | 0.161 |
| N | 4,861 | 4,861 | 4,861 | 4,861 | 4,861 | 4,861 | 4,356 |
| | Malaise score at 33 | | | | | | |
| Wished left at 15 | 0.777*** | 0.760*** | 0.687*** | 0.396*** | 0.319** | 0.319** | 0.317** |
| | (0.106) | (0.108) | (0.112) | (0.118) | (0.124) | (0.124) | (0.130) |
| R2 | 0.014 | 0.050 | 0.054 | 0.069 | 0.090 | 0.090 | 0.100 |
| N | 3,743 | 3,743 | 3,743 | 3,743 | 3,743 | 3,743 | 3,368 |
| | Malaise score at 42 | | | | | | |
| Wished left at 15 | 0.962*** | 0.969*** | 0.944*** | 0.738*** | 0.579*** | 0.579*** | 0.616*** |
| | (0.124) | (0.126) | (0.131) | (0.138) | (0.145) | (0.145) | (0.154) |
| R2 | 0.016 | 0.050 | 0.053 | 0.060 | 0.085 | 0.085 | 0.087 |
| N | 3,761 | 3,761 | 3,761 | 3,761 | 3,761 | 3,761 | 3,386 |
| | Malaise score at 50 | | | | | | |
| Wished left at 15 | 0.305*** | 0.321*** | 0.318*** | 0.238*** | 0.191** | 0.191** | 0.188** |
| | (0.075) | (0.076) | (0.079) | (0.083) | (0.088) | (0.088) | (0.092) |
| R2 | 0.005 | 0.044 | 0.048 | 0.056 | 0.082 | 0.082 | 0.090 |
| N | 3,199 | 3,199 | 3,199 | 3,199 | 3,199 | 3,199 | 2,881 |
| | SF-36 General health score | | | | | | |
| Wished left at 15 | -5.008*** | -4.364*** | -4.070*** | -3.297*** | -2.421** | -2.421** | -2.286** |
| | (0.865) | (0.890) | (0.923) | (0.970) | (1.034) | (1.034) | (1.082) |
| R2 | 0.011 | 0.026 | 0.030 | 0.035 | 0.058 | 0.058 | 0.065 |
| N | 2,912 | 2,912 | 2,912 | 2,912 | 2,912 | 2,912 | 2,633 |
| Gender and region | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Mother's characteristics | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Father SES at 7 and 16 | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Parents' interest in education | No | No | Yes | Yes | Yes | Yes | Yes |
| Reading, Maths & ability scores | No | No | No | Yes | Yes | Yes | Yes |
| Number O-level and CSE taken | No | No | No | No | Yes | Yes | Yes |
| Aspirations | No | No | No | No | No | Yes | Yes |
| Gestation time and birth weight | No | No | No | No | No | No | Yes |

Note: NCDS. Sample restricted to those who left school at 16. BSAG: Bristol Social Adjustment Guides, a measure of child's behaviour, and a high score indicate problematic behaviours. Mother's characteristics include age at birth, whether has some post-compulsory schooling and smoked during pregnancy. Reading and maths scores at age 7, 11 and 16. General ability test at age 11. Aspirations include aspirations on leaving school, job aspirations at age 16, and expectations about marriage and fertility at age 16 (interacted with gender).
*** p<0.01, ** p<0.05, * p<0.1

Table A.11: Mental health effect of forced education - binary variable indicating probable depression

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Malaise score at age | | | |
| | 23 | 33 | 42 | 50 |
| | Unadjusted | | | |
| Wish left at 15 | 0.0533*** | 0.0588*** | 0.0797*** | 0.0539*** |
| | (0.0081) | (0.0104) | (0.0118) | (0.0137) |
| R2 | 0.009 | 0.008 | 0.012 | 0.005 |
| | Adjusted | | | |
| Wish left at 15 | 0.0209** | 0.0259** | 0.0499*** | 0.0388** |
| | (0.0094) | (0.0123) | (0.0140) | (0.0161) |
| R2 | 0.073 | 0.065 | 0.058 | 0.075 |
| N | 4,861 | 3,743 | 3,761 | 3,199 |
| Control mean | 0.060 | 0.081 | 0.109 | 0.135 |
| Bias-adjusted coefficient | 0.010 | 0.014 | 0.039 | 0.034 |

Note: NCDS. Sample restricted to those who left school at 16. Dependent variable is is equal to one of the malaise score is 8 or more (4 or more at age 50, because the short malaise scale was used). Linear probability models estimated via OLS. Covariates include age of mother at birth, intensity of smoking during pregnancy, whether mother had post-compulsory schooling, father's socioeconomic status at age 7 and 16, parental level of interest in their child's education, reading and maths test score at age 7, 11 and 16, general ability test at age 11, number of O-levels and CSEs taken, region when 16, whether worked part-time at 16, aspirations on leaving school, job aspirations at age 16, and expectations about marriage and fertility at age 16 (interacted with gender).
*** p<0.01, ** p<0.05, * p<0.1

Table A.12: Placebo test: differences in BSAG components at age 7 and 11

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | BSAG Depression | | BSAG score Anxiety | |
| | 7 | 11 | 7 | 11 |
| Wish left at 15 | -0.0268 | -0.0285 | -0.0121 | 0.0027 |
| | (0.0527) | (0.0523) | (0.0566) | (0.0427) |
| R2 | 0.139 | 0.132 | 0.051 | 0.050 |
| N | 4,832 | 4,834 | 4,832 | 4,834 |
| Control mean | 0.893 | 0.953 | 0.911 | 0.511 |

Note: NCDS. Sample restricted to those who left school at 16. Probit models. Covariates include age of mother at birth, intensity of smoking during pregnancy, whether mother had post-compulsory schooling, father's socioeconomic status at age 7 and 16, parental level of interest in their child's education, reading and maths test score at age 7, 11 and 16, general ability test at age 11, number of O-levels and CSEs taken, region when 16, whether worked part-time at 16, aspirations on leaving school, job aspirations at age 16, and expectations about marriage and fertility at age 16 (interacted with gender).
*** p<0.01, ** p<0.05, * p<0.1

Table A.13: Effect of forced education

| | (1) 23 | (2) 33 | (3) 42 | (4) 50 |
|---|---|---|---|---|
| | | Married | | |
| Wish left at 15 | 0.0583*** | 0.0033 | 0.0036 | 0.0311* |
| | (0.0170) | (0.0140) | (0.0161) | (0.0180) |
| R2 | 0.071 | 0.056 | 0.034 | 0.046 |
| N | 4,868 | 3,638 | 3,753 | 3,223 |
| Control mean | 0.542 | 0.874 | 0.810 | 0.798 |
| | | Has children | | |
| Wish left at 15 | 0.0639*** | 0.0089 | 0.0102 | -0.0091 |
| | (0.0148) | (0.0171) | (0.0169) | (0.0165) |
| R2 | 0.130 | 0.079 | 0.071 | 0.077 |
| N | 4,868 | 3,614 | 3,772 | 3,220 |
| Control mean | 0.255 | 0.763 | 0.766 | 0.827 |
| | | Paid work | | |
| Wish left at 15 | -0.0206 | -0.0153 | -0.0071 | -0.0252* |
| | (0.0136) | (0.0133) | (0.0126) | (0.0129) |
| R2 | 0.150 | 0.113 | 0.085 | 0.085 |
| N | 4,868 | 4,835 | 4,868 | 4,868 |
| Control mean | 0.802 | 0.824 | 0.856 | 0.852 |

Note: NCDS. Sample restricted to those who left school at 16. Probit models. Covariates include age of mother at birth, intensity of smoking during pregnancy, whether mother had post-compulsory schooling, father's socioeconomic status at age 7 and 16, parental level of interest in their child's education, reading and maths test score at age 7, 11 and 16, general ability test at age 11, number of O-levels and CSEs taken, region when 16, whether worked part-time at 16, aspirations on leaving school, job aspirations at age 16, and expectations about marriage and fertility at age 16 (interacted with gender).
*** p<0.01, ** p<0.05, * p<0.1

Table A.14: Association between malaise score and BSAG

| | Malaise score at age | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | 23 | | 33 | | 42 | | 50 | |
| | A - Sample: Wanted to stay in school until 16 | | | | | | | |
| BSAG at 7 | 0.0252*** | 0.0210*** | 0.0173** | 0.0149* | 0.0302*** | 0.0324*** | 0.0074 | 0.0085 |
| | (0.0066) | (0.0070) | (0.0076) | (0.0085) | (0.0088) | (0.0097) | (0.0054) | (0.0060) |
| R2 | 0.005 | 0.164 | 0.002 | 0.094 | 0.005 | 0.103 | 0.001 | 0.099 |
| N | 2,736 | 2,736 | 2,172 | 2,172 | 2,185 | 2,185 | 1,898 | 1,898 |
| BSAG at 11 | 0.0296*** | 0.0216*** | 0.0226*** | 0.0153* | 0.0334*** | 0.0299*** | 0.0169*** | 0.0223*** |
| | (0.0067) | (0.0070) | (0.0077) | (0.0083) | (0.0088) | (0.0095) | (0.0054) | (0.0059) |
| R2 | 0.007 | 0.164 | 0.004 | 0.095 | 0.007 | 0.105 | 0.005 | 0.105 |
| N | 2,735 | 2,735 | 2,168 | 2,168 | 2,180 | 2,180 | 1,893 | 1,893 |
| Covariates | No | Yes | No | Yes | No | Yes | No | Yes |
| | B - Sample: Wished they could have left school at 15 | | | | | | | |
| BSAG at 7 | 0.0238*** | 0.0212** | 0.0227** | 0.0178 | 0.0110 | 0.0150 | -0.0032 | -0.0011 |
| | (0.0091) | (0.0096) | (0.0109) | (0.0119) | (0.0138) | (0.0152) | (0.0080) | (0.0089) |
| R2 | 0.005 | 0.195 | 0.004 | 0.179 | 0.001 | 0.147 | 0.000 | 0.166 |
| N | 1,432 | 1,432 | 1,038 | 1,038 | 1,049 | 1,049 | 854 | 854 |
| BSAG at 11 | 0.0310*** | 0.0262*** | 0.0342*** | 0.0278** | 0.0406*** | 0.0447*** | 0.0177** | 0.0173** |
| | (0.0088) | (0.0091) | (0.0108) | (0.0115) | (0.0133) | (0.0143) | (0.0079) | (0.0085) |
| R2 | 0.009 | 0.199 | 0.010 | 0.180 | 0.009 | 0.154 | 0.006 | 0.173 |
| N | 1,434 | 1,434 | 1,040 | 1,040 | 1,052 | 1,052 | 857 | 857 |
| Covariates | No | Yes | No | Yes | No | Yes | No | Yes |

Note: NCDS. Sample restricted to those who left school at 16. Linear models estimated via OLS. Covariates include age of mother at birth, intensity of smoking during pregnancy, whether mother had post-compulsory schooling, father's socioeconomic status at age 7 and 16, parental level of interest in their child's education, reading and maths test score at age 7, 11 and 16, general ability test at age 11, number of O-levels and CSEs taken, region when 16, whether worked part-time at 16, aspirations on leaving school, job aspirations at age 16, and expectations about marriage and fertility at age 16 (interacted with gender).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$