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Does more education always improve mental health? Evidence from a British compulsory schooling reform*

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Abstract

In this paper, we test whether education has a causal effect on mental health by exploiting a compulsory schooling reform in 1972, which raised the minimum school leaving age from 15 to 16 years old in Great Britain. Using a regression discontinuity design, we provide robust evidence that although the reform increased educational attainment, it also increased the prevalence of depression and other mental health conditions in adulthood. Our results do not imply that more schooling *per se* leads to poorer mental health, but rather suggest that forcing low achieving teenagers to remain in an academic environment may have long-term unintended consequences on their mental health.

Keywords: Mental health, education, compulsory schooling, UK

JEL: I12, I21, I28

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1 Introduction

There is a strong, positive relationship between education health in most countries regardless of their level of development (Cutler & Lleras-Muney 2012). The strength of this association suggests that education policies may have the potential to improve health and also be more cost-effective than health care based interventions (Woolf et al. 2007, Cohen & Syme 2013). Yet, there is considerable debate on whether this relationship is causal. 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 other health outcomes.[1] Overall, the results from this literature suggests that the health effect of additional years of compulsory schooling may be relatively small and inconsistent across a wide range of outcomes and populations (Karas Montez & Friedman 2015).

A particular domain of health that has received less attention but may nonetheless be crucial to understanding the effects of compulsory schooling laws is mental health. There are two important reasons why the mental health effects of education deserve increased focused. First, mental health conditions are among the leading contributors to the global burden of disease (Vos et al. 2015). In the United Kingdom, mental and behavioural disorders are the second largest contributor to years lost to disability, accounting for 21.5% of all years lost to disability. Around one in six adults met the criteria for a common mental health condition in 2014, with the proportion of women having mental health conditions increasing steadily since 2000 (McManus et al. 2016). Mental health conditions are also a burden to the economy, costing GBP 70 billion a year, equivalent to 4.5% of GDP, through lost productivity, transfers and health expenditure (OECD 2014).

A second compelling reason to examine the mental health effects of education policy refers to the particular nature of compulsory schooling laws, which impose a legal obligation on students to stay longer at school. For some young people, this obligation may clash with individual preferences to enter the labour market at an earlier age. In addition, low achievers may experience sustained psychosocial stress arising from the obligation to remain in a competitive academic environment where they are less likely to succeed than their peers. Even if the laws led to better educational and labour market outcomes, stressful events early in life are known to increase the risk of future episodes of mental illness in adult life (Kessler et al. 2005, Copeland et al. 2009, Patton et al. 2014). It is therefore plausible that a legal obligation to remain longer at school could have unintended, long-term consequences on the mental well-being of some individuals. As subjective well-being is increasingly seen as a key policy outcome (Layard & O'Donnell 2015), measuring mental health effects is crucial to understand the welfare implications of compulsory schooling. These effects on mental health could counterbalance the potential effects on earnings and physical health reported in earlier studies, and may explain why studies examining self-reported health have yielded contradictory findings.

This paper examines the causal effect of education on mental health in Great Britain. In recent years, a handful of studies have started to address this question, yielding inconsistent

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

results. A recent study by Crespo et al. (2014) found that education reduces depression and improves memory. Using data from European countries, Mazzonna (2014) found that additional years of schooling had positive effects on the mental health of men. By contrast, raising the minimum school leaving age in Sweden appears to have adversely affected emotional control measured at military age conscription, despite increasing IQ (Lager et al. 2016). Likewise, the 1997 increase in school leaving age in Turkey had a negative effect on life satisfaction among men, despite increasing earnings (Dursun & Cesur 2016).

In this paper we provide new evidence on the causal relationship between compulsory schooling and mental health. We estimate the mental health effects of the 1972 Raising of the School Leaving Age Order (ROSLA), which increased the minimum school leaving age in Britain from 15 to 16. Using a fuzzy regression discontinuity design, we exploit the sharp increase in school enrollment resulting from the reform to estimate the effect of an extra year of schooling on prevalence of reported depression and other common mental health conditions in adulthood. The reform sharply increased the average age of leaving full-time education and the proportion of students staying at school until age 16, but had a small effect on the probability of obtaining a high quality end-of-secondary-school diploma (O-level).

Our main contribution is to show that the extra year of schooling induced by the 1972 ROSLA increased the prevalence of self-reported depression and common mental health conditions in adult life.[2] We argue that the increase in minimum school leaving age may have led some low achievers to over-invest in formal schooling, at the expense of early entry in the labour market, which could have been more beneficial to their well-being. A second possible story compatible with our results relates to mismatched expectations: the 1972 reform may have raised young people's aspiration but failed to translate into significant improvements for their labour market performance, which could have led to increased level of depression. Our results do not imply that increased educational attainment *per se* leads to poorer mental health. They rather suggest that forcing low achieving teenagers to remain in an a formal academic environment may have unintended consequences on their mental health.

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 our main results and discuss potential channels for the mental health effect of the 1972 ROSLA in Section 6. The last section concludes.

2 Education and Health

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

[2]Using data from the 2007 Adult Psychiatric Morbidity Survey we show that self-reports of mental health conditions are strongly correlated with clinical assessments of common mental health conditions; We use data from the Annual Population Survey, a large scale household survey, for our main analysis.

2012, Stewart-Brown et al. 2015).

There are several mechanisms through which education may influence health. In the canonical model developed by Grossman (1972), there are two mechanisms that may generate a direct effect of education on health. First, education may help individuals to choose 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 and more educated people may work in a safer environment (Cutler & Lleras-Muney 2010). More educated people may also have more educated and healthier peers (Gaviria & 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 & 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 could be 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. Following Lleras-Muney (2005), a growing number of studies have exploited changes in compulsory schooling laws enacted in the 20th Century. Overall, this literature suggests that the causal effect of education on health may be smaller than suggested by correlations. Whereas Lleras-Muney (2005) found very large effects of education on mortality in the United States, other studies found much smaller and localised effects using reforms in Italy (Cipollone & Rosolia 2011), the Netherlands (van Kippersluis et al. 2009) Sweden (Meghir et al. 2012, Fischer et al. 2013) and seven European countries (Gathmann et al. 2015). However, some studies have found no effects of education on mortality in France (Albouy & Lequien 2009), the UK (Clark & Royer 2013) and the United States (Mazumder 2008).

The evidence on other health outcomes is also mixed. Some studies found evidence that education improves self-rated health (Oreopoulos 2006, Kemptner et al. 2011) and reduces BMI and obesity (Kemptner et al. 2011, Brunello et al. 2013). By contrast, Clark & Royer (2013) found no effect of education in the UK on a range of health outcomes, including biomarkers, a finding that is confirmed by Jürges et al. (2012). In addition, examining the effects of education on the prevalence of specific health conditions Mazumder (2008) and Fletcher (2015) find inconclusive evidence for the US. In their analysis of the effect of compulsory schooling reforms in Sweden Meghir et al. (2012) find no long-lasting reduction in mortality and hospitalisation.

Comparatively few studies exploit changes in compulsory schooling laws to estimate the effect of schooling on mental health. Exploiting changes in the compulsory schooling age across a six of European countries, Crespo et al. (2014) found that education has a protective effect on mental health and memory. Following a similar approach and using data from the same countries, Mazzonna (2014) found that education reduces depression and improves memory of men but not women. The effect of schooling on cognitive abilities in older age had previously been highlighted by Glymour et al. (2008) in the US and Banks & Mazzonna (2012) in England.

By contrast, more recent studies have found negative effects of compulsory schooling laws on mental health. Analysing the the effects of the 1997 increase in minimum schooling age in Turkey, Dursun & Cesur (2016) find a negative impact on subjective well-being and life satisfaction for men, despite positive effects on earnings and IQ. Lager et al. (2016) show that the increase in school leaving age in Sweden in the 1950s had an adverse effect on emotional control at time of military conscription. They argue that students from a low socioeconomic status affected by the reform were not able to leave school at age 15, which may have resulted in reduced emotional control. They also found a negative effect on emotional control for students whose parents were non-manual, entrepreneurs or professionals, even if the reform had no impact on the IQ or schooling years for this group. They argue that the reform may have led to a change in the school environment, which may have carried negative consequence for the emotional well-being of students from privileged background.

Overall, evidence on the causal impact of education on health has been mixed and inconsistent, with particularly little attention paid to mental health. The studies examining impacts on mental health have been inconclusive, with more recent studies showing potentially negative effects on emotional well-being at the time of leaving full-time education. 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. Yet, it 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, especially for low achievers.

3 Empirical Approach

3.1 Approach

We estimate the long-term effect of an additional year of compulsory schooling on mental health induced by the 1972 ROSLA, which raised the minimum school leaving age the compulsory schooling from 15 to 16 in Britain. We adopt a regression discontinuity (RD) design framework(Lee & Lemieux 2010), since the 1972 ROSLA resulted in a sudden and large increase in the proportion of students finishing secondary school.

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 made provisions 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, and extensive preparations were started. With a two year delay, the school leaving age was raised to 16 in 1972 by Statutory Instrument 59 in Scotland[3] and Statutory Instrument 444 in England and Wales [4], taking effect on 1st September 1972. All individuals who were born on or after the 1st September 1957 had

[3] http://www.legislation.gov.uk/uksi/1972/59/pdfs/uksi_19720059_en.pdf

[4] http://www.legislation.gov.uk/uksi/1972/444/pdfs/uksi_19720444_en.pdf

to stay at school until 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.

Following Clark & 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 an 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).[5] $\mathbf{x}_{i,c,t}$ is a vector of relevant individual characteristics (third order polynomial of age, indicators for calendar month of birth, gender, proxy response and survey year).[6] In theory, including the vector $\mathbf{x}_{i,c,t}$ should not affect the estimated coefficients, since its elements should be uncorrelated with being affected by the reform but may result in a gain in precision. The parameter of interest is α_1, which denotes the effect of the 1972 ROSLA on the outcome of interest y.

First 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. In a second step, we use the same reduced-form equation to obtain estimates of the overall effects of the 1972 ROSLA on mental health outcomes. These estimates can be interpreted as the Intention to Treat (ITT) parameter, because not all individuals changed their behaviour as a result of the reform. While some individuals stayed at school until 16 because of the reform (the 'compliers'), other would have stayed at school until 16 (or more) in the absence of the reform. The average effect of an extra year of education induced by the reform on mental health can be obtained by dividing the estimate of the overall effect 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 combining the first-stage equation 1 with a structural equation which describes the relationship between mental health and education:

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

β_1 measures the effects of an additional year of education on the prevalence of mental health condition and can estimated by combining equations 1 and 2 via two-stage least squares (2SLS), using $D_{i,c}$ as the excluded instrument.

[5]We discuss about the functional form of $f(R_{i,c})$ in the next section.

[6]Unlike Clark & Royer (2013), we do not include a full set of month of birth dummies interacted with the treatment. This would reduce the external validity of the estimate of the discontinuity parameter, which would be local to individuals born in the reference month (i.e. the month for which the dummy was excluded). However, we show that our results are robust to the introduction of an interaction of the treatment variable with being born in the summer months.

3.2 Estimation and Interpretation

In order to obtain unbiased estimates of the discontinuity parameters in equation 1 and 2 it is crucial to correctly capture the cohort trends ($f(R_{i,c})$). Using an incorrect functional form can have serious consequences in RD design, since 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 estimate the treatment effects in an RD design (Lee & 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 cutoff date - September 1957) and the cohort trends are captured by a linear function of R.

We use local linear regressions to estimate the effect of the 1972 ROSLA on health outcomes, since the local linear approach generates estimates that are more local to the threshold (Lee & Lemieux 2010) and are less sensitive to mis-specification. The cohort trends are modelled as a linear function of R with different slopes on both sides of the cutoff.[7] The crucial step when estimating local linear regressions is to choose the bandwidth, in our case the number of cohorts born before and after the cutoff. Optimal bandwidth is selected using the method proposed by Imbens & Kalyanaraman (2012). Because the 2SLS estimator is the ratio of the reduced-form estimate of the effect of the 1972 ROSLA on mental health outcomes and the first-stage estimate of the effect of reform on education, we use the optimal bandwidths for the reduced-form estimate and the first-stage estimate, obtained separately (Clark & Royer 2013). The 2SLS standard errors are then calculated using the Delta method. In Appendix we show that our results are robust to the choice of alternative bandwidths.

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 (1st September 1957). This implies that the change in minimum school leaving age was the only 'shock' that affected the cohorts born just after the cutoff point for the 1972 reform.[8] Although this assumption cannot be directly tested, there should be no discontinuities in outcomes at any other arbitrarily chosen thresholds. We test this by estimating discontinuities in outcomes of interest between school cohorts, from 1947 to 1967, 10 years before and after the cutoff of the 1972 ROSLA.

The estimates of parameter β_1 in equation 2 are local average treatment effects (LATE) and should be interpreted as the 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 may be very different from the effects of an additional year of education resulting from an individual decision rather than a legal obligation. These effects are also very likely to differ from the effects of an additional year of education at any other point of the education distribution. These effects are

[7]More formally, $f(R_{i,c}) = \lambda_0 R_{i,c} + \lambda_1 D_{i,c} R_{i,c}$

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

nonetheless interesting, since the subpopulation affected by the reform is relatively similar to the young people who leave school early with little or no qualification, a group attracting mounting policy concern in the UK and elsewhere. This analysis sheds light on whether increasing the minimum school leaving age and keeping young people at school has long-term effects on mental health for this group.

4 Data

We use data from the Annual Population Survey (APS) collected between April 2004 and March 2016. The APS is the largest household survey in the UK[9], and includes approximately 150,000 households and 350,000 respondents per year.

The APS collects information about a wide range of demographics, including month and year of birth, which are crucial to correctly identify whether the 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 15, whereas those born in September 1957 or later had to stay at school until 16. In addition, the APS contains detailed information about the educational attainment of the respondents, including the age when left full-time education and all qualifications held by the respondents. In this paper we primarily use the age when left full-time education in order to estimate the impact of increasing the school leaving age on adult mental health. The 1972 ROSLA increased the school leaving age but this may not have resulted in a comparable increase in attainment of formal certifications sat at the end of high school (eg O levels or CSE). However, using this variable may be problematic for respondents born in the summer. Respondents born in the summer are likely to report having left school at 15 even if they have completed the last year of high school. If they consider that they left school before the summer holiday they may very well have been 15 by the time they last went to school, but have nonetheless finished high school. A way to account for this is to include a interaction term between the treatment and a set of binary variables indicating whether the respondents are born during the summer months (June, July and August). However, doing this implies that the effect of the reform is estimated only for those born between September and May, discarding those born in the summer. In our main specification, we do not include interaction terms between the treatment and being born summer months. However, we show that the results are robust to including these interaction terms (See Table 7 in Appendix).

The APS does not contain traditional measures of mental health, such as the Center for Epidemiological Studies Depression (CESD) scale. The main survey used to monitor the prevalence of mental health conditions in England is the Adult Psychiatric Morbidity Survey, a survey of around 7,000 individuals run every seven years. This survey cannot be used to estimate the health effects of compulsory education owing to its very small sample size. A large sample size is a crucial requirement for RD design, as the estimation sample is restricted to individuals born just before and after the cut-off date for the reform.

In the APS all working age respondents (men aged between 16 and 64, and women aged be-

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

tween 16 and 59) are asked whether they have any long-lasting health problems or disabilities.[10] The respondents who answer positively to that question are subsequently 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'. 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 Table 5 in the Appendix.

Using 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 6 in Appendix), such as the Clinical Interview Schedule - Revised (CISR), which is a validated 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 CISR score of 12 of more indicates the presence of a common mental health condition. The correlation coefficient between reporting a mental health condition and the CIS-R score in the 2007 APMS is 0.467.

The APS may be considered as a one-stage clustered sample of people, with the primary sampling units being the households, since all adults within a household are sampled.[11] Therefore, standard errors should be clustered at the household level. However, in the regression estimates we cluster the standard errors at the level of the forcing variable, i.e. month-year of birth, as suggested by Lee & Card (2008). In addition, we apply the sampling weights to our estimator (Solon et al. 2013).

5 Results

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 1. 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 1 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 0.22 year, with a baseline of 17.2. The coefficient is precisely estimated, with a t-statistic of 8.7. Women appear to have been affected more strongly by the reform than men, as the effect is slightly larger for women (0.23) than for men (0.19). The magnitudes of the t- and F-statistics suggest that the 1972 ROSLA indicator is a powerful instruments for educational attainment. Therefore, results from column 1 can be used as a the First-Stage estimates of the

[10]'Do you have any physical or mental health conditions or illnesses lasting or expecting to last 12 months or more?', cf. Labour Force Survey User Guide - Volume 2: LFS Questionnaire

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

fuzzy regression discontinuity model, as in Clark & Royer (2013).

Table 1: Effect of the 1972 ROSLA on education leaving age

	(1) Age left FT education	(2) Probability to stay in FT education until ≥16	(3) ≥17	(4) ≥18	(5) ≥19
	All (Bandwidth=26, N= 198,405)				
1972 ROSLA	0.217***	0.192***	0.012**	0.012***	0.003
	(0.027)	(0.007)	(0.005)	(0.004)	(0.004)
F-test	62.7	681.8	5.1	7.5	0.5
Mean	17.2	0.724	0.400	0.308	0.205
	Men (Bandwidth=20, N=73,582)				
1972 ROSLA	0.189***	0.182***	0.008	0.004	-0.002
	(0.043)	(0.010)	(0.009)	(0.008)	(0.005)
F-test	19.6	364.0	0.6	0.3	0.2
Mean	17.3	0.730	0.394	0.316	0.223
	Women (Bandwidth=24, N=96,297)				
1972 ROSLA	0.232***	0.191***	0.010	0.011	0.008
	(0.039)	(0.010)	(0.009)	(0.009)	(0.006)
F-test	35.9	363.0	1.2	1.6	2.2
Mean	17.1	0.724	0.403	0.300	0.189

Note: All linear regression models 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, calendar month of birth and proxy interview. Estimates are weighted by the survey weights and standard errors clustered at month of birth are presented in parentheses. F-test on 1972 reform dummy variable. Reported means are calculated among those were born in the period before the reform (based on the bandwidth). Optimal bandwidth obtained by the bandwidth selector proposed by Imbens & Kalyanaraman (2012).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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. While the 1972 ROSLA had a strong effect on the probability to stay in education until at least 16 (Column 2), it had little impact on the decision to stay in education beyond the minimum school leaving age of 16 (Columns 3-5). Although the effects are statistically significant for pooled respondents, the coefficients are very small, about 15 to 20 smaller than the effect on staying until 16.

We also find that the reform had a much smaller impact on achievement of formal qualifications compared to the large effect on staying at school for another year. Until 1988 there were two types of qualifications that could be obtained at the end of secondary school: the General Certificate of Education Ordinary Level, (O- Level), which would open the door to study towards A-Levels, and a more vocational qualification, the Certificate of Secondary Education (CSE). Results displayed in Table 2 show that the 1972 reform significantly increased the proportion of people attaining a CSE, and had no statistically significant effect on the achievement of the more academic qualification, the O-level.[12] While the 1972 reform increased the proportion of

[12]Because the formal certification framework is different in Scotland, we focus on individuals born in England

people staying at school until 16 (i.e. completing secondary school) by 19.2 percentage points, it increased the probability to obtain a CSE by only by 7.9 percentage points and had no effect on a O-level. This suggests that a large share of those compelled to attend the last grade of secondary school may not have benefited much from this additional year of education.

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, as shown in Table 1.

Table 2: The effects of the 1972 RoSLA on qualification achievement

	CSE	O-level	A-level
		All	
1972 ROSLA	0.0788***	0.00729	-0.00741
	(0.00731)	(0.00798)	(0.00485)
Mean	0.204	0.492	0.265
Bandwidth	21	23	42
Observations	128,142	142,148	251,470
		Men	
1972 ROSLA	0.0847***	0.00394	-0.00477
	(0.00815)	(0.0114)	(0.00731)
Mean	0.200	0.486	0.269
Bandwidth	18	26	38
Observations	52,309	76,448	110,133
		Women	
1972 ROSLA	0.0828***	0.0122	-0.0103
	(0.00997)	(0.00769)	(0.00746)
Mean	0.208	0.499	0.261
Bandwidth	30	21	30
Observations	93,930	67,844	93,968

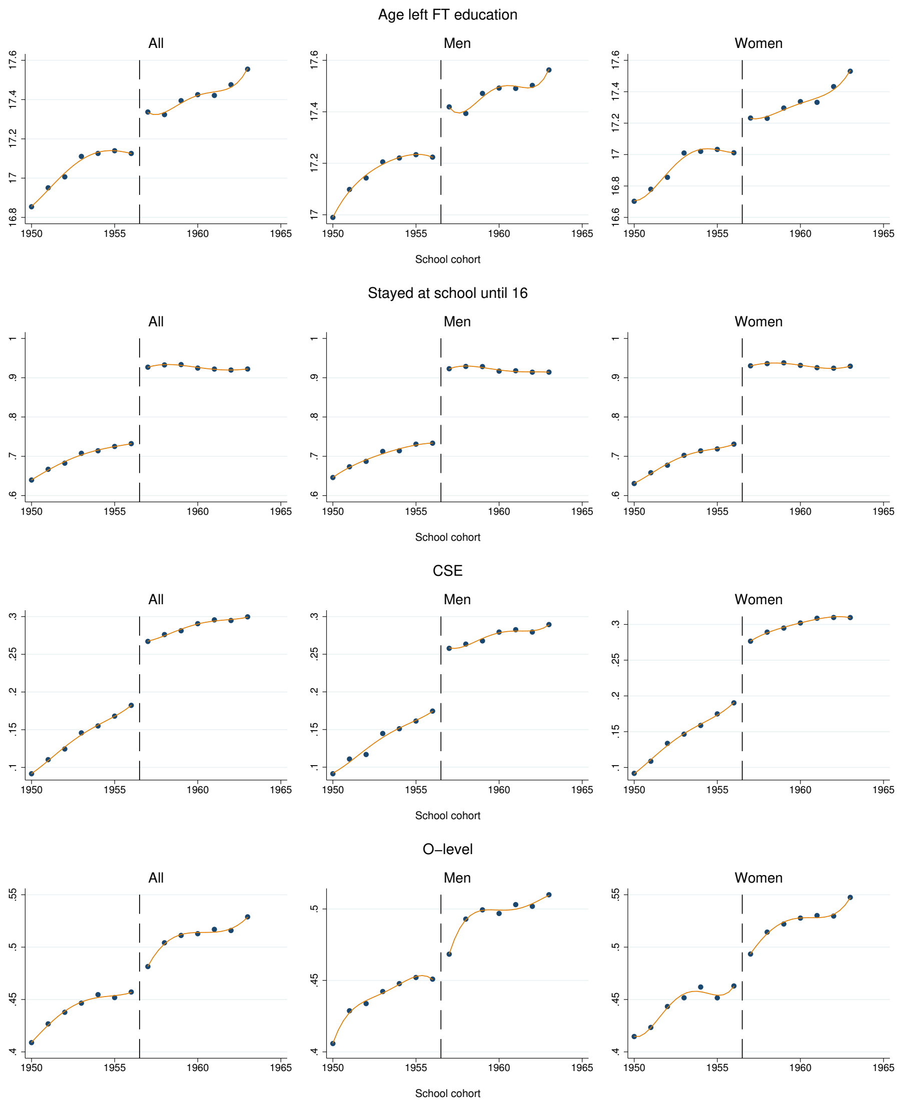
Note: Sample restricted to individuals born in England and Wales only. All linear regression models include a linear function of month-year of birth, a linear interaction of month-year of birth and the 1972 ROSLA dummy, a third-order polynomial in age (measured in months) and binary variables for sex, year of survey, calendar month of birth and proxy interview. Estimates are weighted by the survey weights and standard errors clustered at month of birth are presented in parentheses. Reported means are calculated among those were born in the period before the reform (based on the bandwidth). Optimal bandwidth obtained by the bandwidth selector proposed by Imbens & Kalyanaraman (2012).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

5.2 The effects of the 1972 ROSLA on mental health conditions

In this section we present estimates of the effects of the 1972 ROSLA on the long-term prevalence of reported mental health conditions. We also report OLS estimates of the effect of education on mental health. Although OLS estimates are likely to be biased, 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

and Wales only.

Figure 1: The effect of the 1972 ROSLA on education



Note: Average outcomes by school cohorts. adjusted for age, sex, calendar month of birth and proxy interview.

covariates included in the linear models.[13] 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 4.1 percentage points less likely to report having any mental health condition and 3.7 percentage points less likely to report suffering from depression or anxiety compared those who left school a year earlier. These effects are very large, considering that 11.8 percent of the individuals who left school at 15 suffer from a mental health condition and 10.5 percent from depression or anxiety.

Table 3: Effect of 1972 ROSLA on mental health

	OLS	Reduced Form	IV	Mean	Bandwidth	*N*
			All			
Mental health condition	-0.041***	0.011***	0.050***	0.118	24	198,405
	(0.003)	(0.003)	(0.016)			
Depression or anxiety	-0.037***	0.008***	0.037***	0.105	29	220,448
	(0.003)	(0.003)	(0.012)			
Other mental health conditions	-0.018***	0.006***	0.026***	0.050	25	198,405
	(0.003)	(0.001)	(0.008)			
			Men			
Mental health condition	-0.032***	0.009**	0.048**	0.097	28	101,886
	(0.005)	(0.004)	(0.021)			
Depression or anxiety	-0.032***	0.005	0.028	0.087	30	109,170
	(0.004)	(0.004)	(0.019)			
Other mental health conditions	-0.012***	0.001	0.007	0.041	44	160,630
	(0.002)	(0.002)	(0.010)			
			Women			
Mental health condition	-.048***	0.013**	0.054**	0.137	27	107,163
	(0.004)	(0.005)	(0.025)			
Depression or anxiety	-0.044***	0.011**	0.048**	0.124	25	99,759
	(0.005)	(0.005)	(0.024)			
Other mental health conditions	-0.025***	0.008***	0.037**	0.059	23	96,297
	(0.004)	(0.003)	(0.017)			

Note: All models 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, calendar month of birth and proxy interview. Models are estimated by OLS. Estimates are weighted by the survey weights and standard errors clustered at month of birth are presented in parentheses. Reported means are calculated among those who left school aged 15 and were born in the period before the reform (based on the bandwidth). Optimal bandwidth obtained by the bandwidth selector proposed by Imbens & Kalyanaraman (2012).
*** p<0.01, ** p<0.05, * p<0.1

Column 2 of Table 3 presents the reduced-form (RF) estimates of the 1972 ROLA, which can be interpreted as the overall effect of the reform on mental health. Figure 2 provides a graphical representation of these estimates. RF estimates show that raising the compulsory school leaving age from 15 to 16 significantly and substantially increased the prevalence of depression and other

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

mental health conditions. We find that the 1972 ROSLA increased the proportion of people reporting having a mental health condition by 1.1 percentage point, from a baseline of 11.8 percent.[14] It increased the prevalence of depression and anxiety by 0.8 percentage point, from a baseline of 10.5 percent. We also present results of models estimated separately for men and women. The estimates for women are larger than the pooled estimates and are significant at the five percent level. The coefficients for men are also positive but smaller and less precisely estimated.

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 1. The IV estimates are of the opposite sign to the OLS but of the same magnitude. We find that an extra year of schooling induced by the 1972 ROSLA increased the probability to report having a mental health condition by 4.9 percentage points and suffering from depression by 3.7 percentage points. The IV estimates can be interpreted as a Local Average Treatment Effects (LATE), that is the effects of the additional year of compulsory schooling for those who would have left school at 15 in the absence of the 1972 ROSLA. This implicitly assumes that the reform did not indirectly affect the mental health of those who did not change their schooling decision because of the reform (the 'non-compliers'). To check this assumption, we estimate the RF and IV parameters for those who left FT education at 19 or below, and those who stayed in FT education until 20 or above separately. As shown in Table 1, the 1972 reform had no effect on the proportion of individuals who stayed in full-time education until 20 or above. Therefore these individuals were not directly affected by the reform and in the absence of indirect effects, there should be no effect on their mental health. Results reported in Table 6 in Appendix A.2 show that the reform had no impact on the prevalence of reported depression for those who stayed in FT education until at least 20. This rules out indirect effects and suggests that the IV estimates on depression can be interpreted as Local Average Treatment Effects. The IV estimates for those who left FT education at 19 or below are very similar to the main estimates presented in Table 3.

5.3 Sensitivity analysis

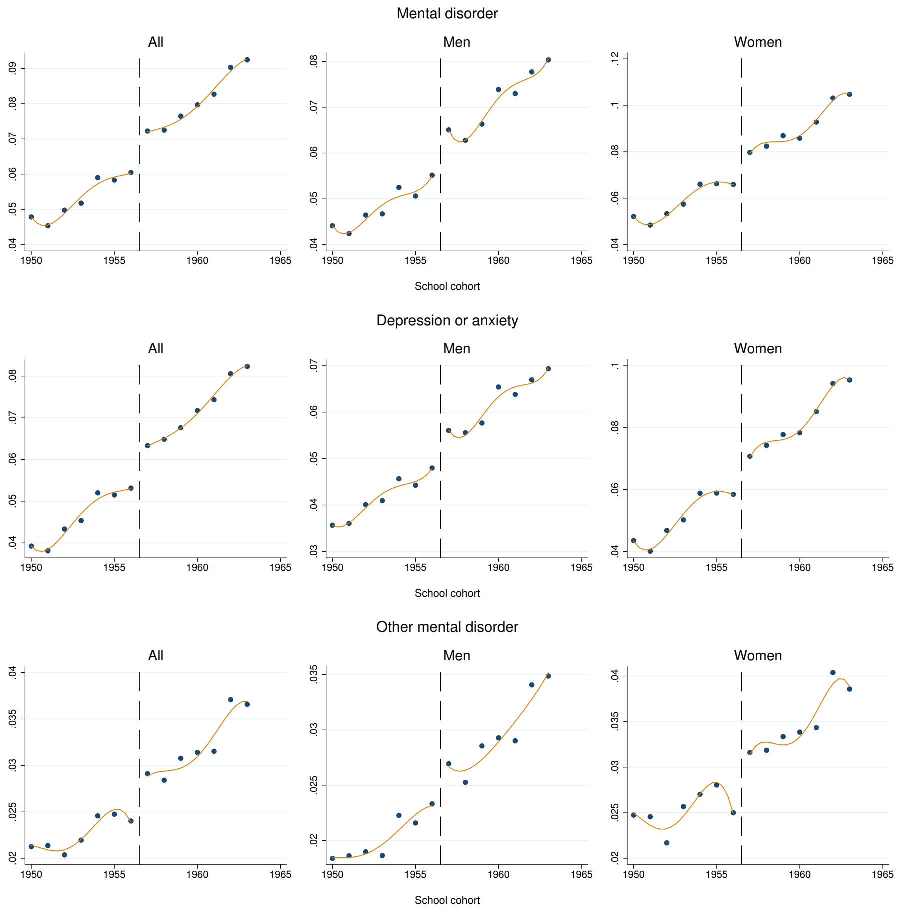
Different specifications

In Table 7 in Appendix A.2 we present RF and IV estimates based on different specifications. Column 1 to 5 show estimates based on models which include different sets of exogenous control variables. Column 5 is our preferred specification. 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 being born in the summer months (June, July and August) in column 6. The results are very stable and are not sensitive to a change in the covariates included in the model.

Columns 7 to 10 show results from models that control for cohort trends with different functional forms. In our main specification, the cohort trends are modelled as a piecewise linear function of the month-year of birth, allowing for different slopes before and after the cutoff. We

[14]The baseline is calculated among those born before the reform who left school at 15, the subpopulation who was affected by the reform

Figure 2: The effect of the 1972 ROSLA on mental health conditions



Note: Average outcomes by school cohorts. adjusted for age, sex, calendar month of birth and proxy interview.

test whether the estimates are sensitive to a change in the functional form by including in the model polynomials of order two and three instead of the piecewise linear function in column 7 and 8. In Columns 9 and 10 we use quadratic and cubic spline (i.e. interacting each term of the polynomial with the ROSLA dummy). The estimates remain of the same magnitude, although they loose precision with the higher-order splines, due to the small bandwidth we use.[15]

Results presented in columns 1 to 10 are based on linear models estimated via Ordinary Least Square. Because our main outcomes are binary variables, the models could be mis-specified. In column 11 we show marginal effects based on probit models. The marginal effects are very close to the estimates from linear probability models. Column 12 reports estimates obtained using local linear regressions with a triangular kernel. Estimates are very similar to those obtained via OLS, which implicitly use a rectangular kernel.

Different bandwidth

In Figure 3 in Appendix A.2 we report RF estimates of the effect of the 1972 ROSLA based on the same specification as our main results, but using different bandwidth values, ranging from 12 to 60. The top panel shows the estimates of the effect of the reform on the education leaving age. The estimates remain statistically significant whichever value of bandwidth is used. However, increasing the bandwidth leads to lower estimates. This is not surprising since increasing the bandwidth is likely to increase the bias of the estimates (Lee & Lemieux 2010). The estimates are generated using regression models that control for cohorts trends using linear terms and the assumption that the age left full-time education is linearly related to the month-of-birth cohort (with different slopes on both sides of the cutoff point) is more likely to hold if the bandwidth is small. The bottom panels show 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. As for the education leaving age, the estimated coefficients tend to get smaller as the bandwidth is increased, although this is more marked for other mental health conditions than depression and anxiety. The estimates remain statistically significant for nearly all values of bandwidth when all respondents are pooled together as well as for women only. For men, the coefficients are less robust to a change in bandwidth.

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 after the true reform, using reduced form regressions as in equation 1. These placebo reforms should not have any significant effect on neither the age when left FT education nor the prevalence of mental health conditions. Results from the reduced-form models are reported in Figure 4 in Appendix A.2.[16] While most estimates are not statistically significant several placebo reforms

[15]The estimates are significant at the 5% level if the bandwidth is increased to for instance 60. As noted by Gelman & Imbens (2014), inference based on high-order polynomials (above two) is often poor.

[16]Note that because the bandwidth used exceeds 24 months, falsified reforms 2 years before and after the reforms are likely to be contaminated by the true reform and are therefore not considered as placebo tests.

yield significant reduced-form coefficients. Given the number of regressions that are estimated, it is expected that some estimates will be statistically significant. We would expect five percent of the coefficient to be statistically significant at the five percent level. Four out of 48 (8.4 percent) coefficients are statistically significant at the five percent level for the age when left full-time education, but only two out of 48 (4.2 percent) for mental health condition and one out of 48 (2.1 percent) for depression or anxiety, which indicates that our main estimates likely capture a genuine discontinuity in these outcomes.

6 Discussion

We find that the 1972 ROSLA increased the probability of reporting suffering from depression and other mental health conditions in adulthood, especially among women. In this section we discuss the potential mechanisms that could explain this finding.

Detection

Because our measures of mental health conditions are self-reported, one could argue that the 1972 ROSLA effect is driven by an increase in detection and diagnosis of mental health conditions, rather than a genuine increase in prevalence. Improved education could improve health knowledge and increase awareness of mental health conditions, which could lead to an improvement in the detection of mental health conditions. Prolonged education could result in improved cognitive abilities in adulthood and older age, as shown by Banks & Mazzonna (2012), who used the 1947 change to the minimum school leaving age to analyse the effect of education on memory and executive functioning at older ages. By improving the ability to process information, increased education may help individuals better detect symptoms, or increase the probability that they seek medical care for these symptoms. We provide two pieces of evidence that offer little support for this explanation. First, there is some indication that the reforms of compulsory schooling laws in the UK had no impact on health-related knowledge. For example, Johnston et al. (2015) shows that raising the minimum schooling age in the UK in 1947 and 1972 did not significantly improve health knowledge. Second, we use data from the 2007 Adult Psychiatric Morbidity Survey to estimate whether educational attainment is associated with the probability of self-reporting having a mental health condition, conditional on symptoms for mental disorders as measured by the Clinical Interview Schedule-Revised (CIS-R) scale, which is based on a structured interview applied by trained interviewers to assess psychiatric problems in the community. If education has no effect on detection, we would expect education to be unrelated with reporting suffering from a mental health condition, conditional on the CIS-R scale. Results are reported in Table 8 in Appendix A.4. We find that while educational attainment is associated with the unconditional probability to report having a mental health condition, there is no positive association between years of education and the probability of reporting having a mental health condition, conditional on symptoms for mental disorders as measured by the CIS-R scale. This indicate that educational attainment is unlikely to increase awareness and detection of depression.

Psychological and opportunity costs of compulsory schooling

The 1972 ROSLA made it compulsory to attend the last year of secondary school. This extra year of schooling was not the result of 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 et al. (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 & Wolpin (1999) young people who choose to drop out from high school may have lower school ability and motivation, and therefore have lower consumption value of school attendance. In addition, the reform had only a small impact on achievement of formal qualifications which suggests that a large share of those compelled to attend the last year of secondary school may not have benefited much from this additional year of education.

Yet, this extra year of compulsory schooling may have entailed large psychological costs. For low achievers, an additional year of compulsory education may have been a stressful experience, especially as exams were taken at the end of that year. Being out of school and having a job could have promoted better mental health, as work may have provided a less stressful environment. This explanation is proposed by Lager et al. (2016) to account for their finding that prolonged compulsory schooling in Sweden had a negative effect on emotional control at younger ages. In addition, being compelled to stay at school may have been felt by young people as an external constraint resulting in a loss of control over their life. The psychology literature suggests that a lack of control is associated with depression, stress, and anxiety-related disorders (Shapiro et al. 1996, Mirowsky & Ross 2003).

To see whether this hypothesis is likely to explain our results, we analyse data from the third wave of the National Child Development Study (NCDS), a longitudinal study that follows 17,415 people born in Great Britain during the week of 3-9 March 1958. These individuals belong to the first cohort that was affected by the 1972 ROSLA and in the third wave, which took place in spring 1974, they are asked a range of questions about educational decisions. We find that 26.2 percent of respondents say they wish they could have left school at 15, a proportion which is very close to the estimated effect of the 1972 ROSLA presented in Table 1. These individuals can be considered as the 'compliers', that is those who changed their behaviour because of the reform. We examine the reasons for leaving school at 16 reported by the respondents who would have liked to leave school at 15. We find that 72.7 percent of respondents mention 'be independent' as one of their reasons for not continuing full-time education beyond the compulsory schooling age and this proportion is slightly higher for women than men. In addition, 42.3 percent reported disliking school as a reason to choose to leave full-time education. In comparison, only 17.0 percent of those who decided to leave school at 16 but would not have wanted to leave school at 15 report disliking school. These findings suggest that being compelled to stay an extra year in school may have delayed access to independence and forced them to remain in an environment in which they are less likely to succeed than their peers, which may have led to increased stress, anxiety and depression.

In addition, the extra year of compulsory schooling came at the expense of competing opportunities, such as vocational education, training or employment. Those who stayed at school until 16 lost a year of full-time work experience. Some evidence supports this interpretation.

Table 4: Reasons for leaving school at 16: choice v constrainst

	Wished could have left at 15?					
	Yes			No		
	All	Men	Women	All	Men	Women
Family needs wages	0.218	0.210	0.228	0.107	0.104	0.110
Be independent	0.727	0.697	0.767	0.600	0.597	0.603
Dislikes school	0.423	0.427	0.417	0.170	0.169	0.171
Has job in mind	0.562	0.576	0.544	0.608	0.600	0.616
To get married	0.080	0.052	0.118	0.033	0.017	0.048
Not good enough	0.439	0.419	0.465	0.257	0.264	0.251
N	3,058	1,740	1,318	4,457	2,142	2,315

Note: NCDS, wave 3 (1974); Sample restricted to respondents who intend to leave school at 16, broken down by their answer to the following question: '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 could have left when you were 15?'

While Grenet (2013) found that the reform had a positive impact of earnings, Clark & Royer (2013) found no evidence of earnings returns. In addition, in their analysis of the life-cycle effects of the 1972 ROSLA on earnings, Buscha & 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.[17] At best, the 1972 reform seems to have led to no net positive gains in earnings, at the expense of increased stress from compulsory attendance of an extra year of schooling.

Unrealised expectations

Another potential explanation for the mental effects is that the 1972 ROSLA raised expectations that were not realised. A number of theories from the social psychology literature conceptualise why unrealised expectations may have detrimental consequences for mental health. For instance, the self-discrepancy theory (Higgins 1987) predicts that gaps between the ideal self (i.e. ambition and aspirations) and the actual self (i.e. actual achievement) may generate negative emotions, including depression. The relative deprivation theory (Walker & Pettigrew 1984) posits that individuals may experience mental distress when deprived from reward or status they feel entitled to. By allowing everybody to complete the last year of high school the 1972 reform may have raised young people's educational aspiration. However, the reform had no impact on the probability to achieve O-Level, the more academic end-of-secondary school diploma. This may have created a sense of relative deprivation, as young people failed to achieve an educational level that they had been led to feel entitled to. The 1972 reform may also have created unrealised expectations about labour market outcomes. As discussed above, the earnings effects of the 1972 ROSLA are uncertain and there is evidence that the reform did not increase the

[17]The authors conclude that RDD-based models cannot capture the earnings effect of education 'net' of the loss of additional work experience. They propose a method to 'correct' for the difference in potential work experience between those affected by the 1972 ROSLA and those who were not affected and find that prolonged schooling has a positive effect on earnings. However, in our case, what matters is the overall effect of the reform on earnings, as this is what is likely to affect mental health. We are not interested in earnings returns to education net of work experience.

rate of intergenerational social mobility (Sturgis & Buscha 2015). Analysing the the effects of the 1997 increase in minimum schooling age in Turkey Dursun & Cesur (2016) find a negative impact on subjective well-being for men, in spite of positive effects on earnings. They find that the reform reduced satisfaction with a number of dimensions of economic and social life, such as earnings, marriage, health and housing. Their analysis suggests that a discrepancy between aspirations and attainments may explain this result.

7 Conclusion

Few studies have examined the causal effect of education on mental health. Using a reform that raised the minimum school leaving age in Britain in 1972 we provide new evidence on the causal relationship between schooling and mental health. We find that the additional year of schooling induced by the 1972 reform increased the prevalence of depression and other mental health conditions in adulthood. This finding complements the analysis of the effects of reforms raising the school leaving age in Britain by Clark & Royer (2013) who found no effect on mortality and a wide range of physical health outcomes.

Our paper is among the first to show that prolonged compulsory schooling may have detrimental effects on mental health that persist into adulthood. Our findings resonate with recent studies showing that longer compulsory schooling reduced emotional control measured at time of military conscription in Sweden (Lager et al. 2016) and lowered subjective well-being among young men in Turkey (Dursun & Cesur 2016).

We propose two potential mechanisms that could explain the adverse effect of additional schooling on mental health. First, those likely to drop out from school may benefit little from additional formal schooling (Eckstein & Wolpin 1999) and may incur significant psychological and emotional costs by being compelled to stay at school. For some low achievers, being out of school and gaining a year of full-time work experience could have been more beneficial to their well-being and mental health in the long-run. A second explanation is that the 1972 reform may have raised young people's aspiration but failed to deliver significant changes to their life (Higgins 1987, Walker & Pettigrew 1984). We find that the reform had a comparatively small effect on achievement of end-of-secondary school, which is consistent with previous evidence that the reform had no impact on social mobility (Sturgis & Buscha 2015).

While longer education often leads to higher earnings and better labour market outcomes (Card 1999), our results show that the 1972 reform in Britain had a long-lasting negative effect on mental health. Given recent calls that Governments should be concerned with subjective well-being (Layard & O'Donnell 2015), our results raise doubts on whether extending compulsory schooling is always a way to increase the overall well-being of populations. Our results do not implying that higher educational attainment *per se* leads to poorer mental health. They rather suggest that forcing low achieving teenagers to remain in an a formal academic environment may have unintended consequences on their mental health. Compelling everybody to stay at school and follow an academic curriculum may not be the most efficient policy to improve the skills of low achievers. For them, vocational pathways may be more beneficial than formal schooling and academic education (Goux et al. 2016). Steering young people towards different pathways that are best suited to their needs may raise their skills while minimising potential psychological and emotional costs.

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A Appendix

A.1 Descriptive statistics

Table 5: Descriptive statistics

	All		Men		Women	
	Pre-ROSLA	Post-ROSLA	Pre-ROSLA	Post-ROSLA	Pre-ROSLA	Post-ROSLA
Mental health condition	0.077	0.075	0.065	0.063	0.088	0.087
	(0.266)	(0.263)	(0.247)	(0.243)	(0.284)	(0.281)
Depression or anxiety	0.068	0.066	0.058	0.055	0.079	0.078
	(0.252)	(0.249)	(0.233)	(0.228)	(0.270)	(0.268)
Other health condition	0.030	0.031	0.026	0.027	0.034	0.034
	(0.171)	(0.173)	(0.160)	(0.163)	(0.182)	(0.182)
Age left FT education	17.171	17.356	17.260	17.428	17.078	17.285
	(2.518)	(2.297)	(2.639)	(2.425)	(2.384)	(2.162)
Age	53.949	50.161	54.174	50.160	53.716	50.162
	(3.477)	(3.613)	(3.623)	(3.607)	(3.302)	(3.619)
England	0.830	0.836	0.834	0.838	0.827	0.834
	(0.375)	(0.370)	(0.372)	(0.368)	(0.378)	(0.372)
Wales	0.053	0.052	0.053	0.051	0.053	0.052
	(0.224)	(0.221)	(0.223)	(0.221)	(0.225)	(0.222)
Scotland	0.117	0.112	0.114	0.111	0.120	0.114
	(0.321)	(0.316)	(0.317)	(0.314)	(0.325)	(0.318)
Non proxy respondnet	0.740	0.735	0.691	0.671	0.790	0.798
	(0.439)	(0.441)	(0.462)	(0.470)	(0.407)	(0.402)
N	171,137	191,178	84,201	91,265	86,936	99,913

Note:Annual Population Survey, 2004-2015. Sample is restricted to respondents born in Great Britain within five years before and after the cutoff date for the 1972 ROSLA, i.e. between September 1952 and August 1962. Standard deviation are reported in parentheses.

A.2 Sensitivity analysis

Table 6: Effect of education on mental health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Age left FT education < 19			Age left FT education $\geq$ 19			
	RF	IV	N	RF	IV	N	Bandwidth
				All			
Mental health condition	0.015***	0.058***	160,588	-0.006	0.142	37,817	24
	(0.004)	(0.014)		(0.006)	(0.215)		
Depression	0.011***	.042***	178,363	-0.005	.116	42,085	29
	(0.003)	(0.012)		(0.005)	(0.156)		
Other mental health conditions	0.007***	0.028***	160,588	-0.001	0.020	37,817	25
	(0.001)	(0.006)		(0.003)	(0.073)		
				Men			
Mental health condition	0.014***	0.055***	80,643	-0.009	-0.338	21,243	28
	(0.005)	(0.018)		(0.005)	(0.919)		
Depression	0.009**	0.034**	86,449	-0.008	-0.285	22,721	30
	(0.004)	(0.016)		(0.005)	(0.776)		
Other mental health conditions	0.003	0.011	126,953	-0.004	-0.150	33,677	44
	(0.002)	(0.009)		(0.003)	(0.406)		
				Women			
Mental health condition	0.015***	.062**	88,472	0.002	-.043	18,691	27
	(0.005)	(0.024)		(0.009)	(0.197)		
Depression	0.016***	0.064***	82,394	-0.007	0.145	17,365	25
	(0.005)	(0.023)		(0.010)	(0.304)		
Other mental health conditions	0.009***	0.038**	79,514	0.007	-.149	16,783	23
	(0.003)	(0.015)		(0.004)	(0.165)		

Note: All models 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, calendar month of birth and proxy interview. Models are estimated by OLS. Estimates are weighted by the survey weights and standard errors clustered at month of birth are presented in parentheses. Reported means are calculated among those who left school aged 15 and were born in the period before the reform (based on the bandwidth). Optimal bandwidth obtained by the bandwidth selector proposed by Imbens & Kalyanaraman (2012).
*** p<0.01, ** p<0.05, * p<0.1

Figure 3: Reduced-form estimates with different bandwidth

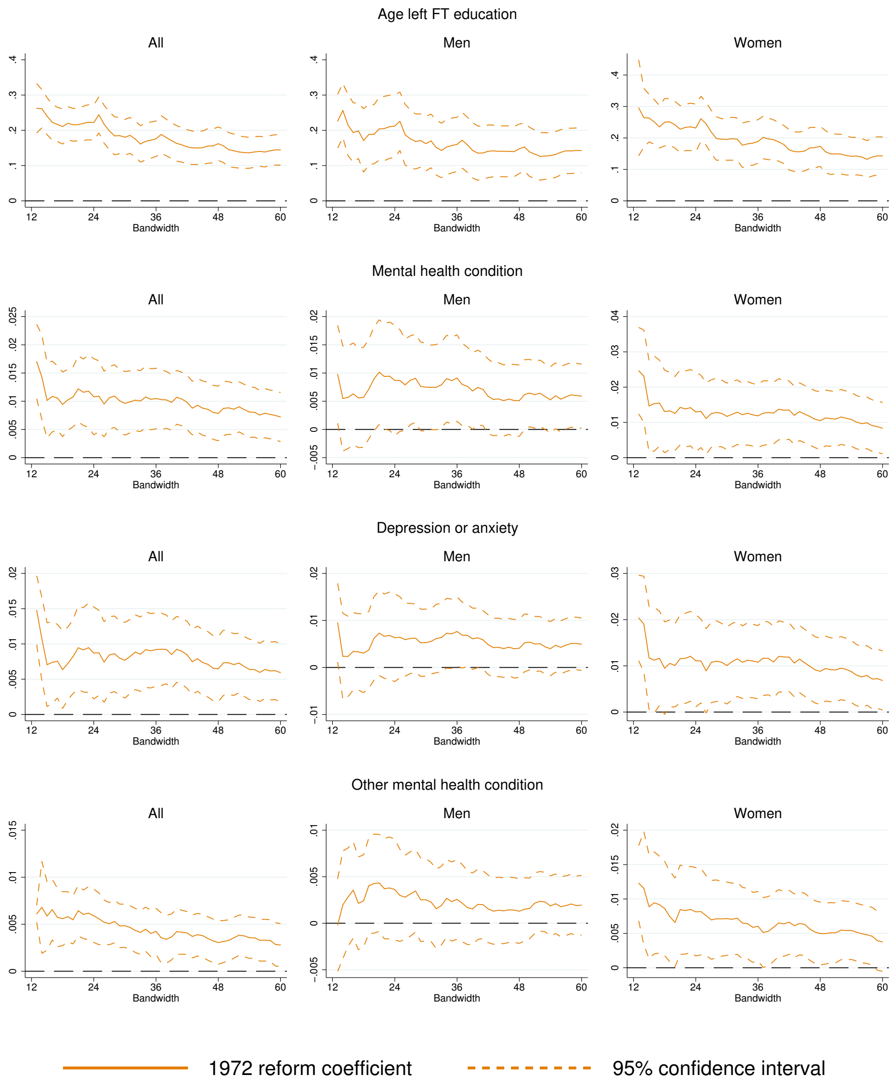
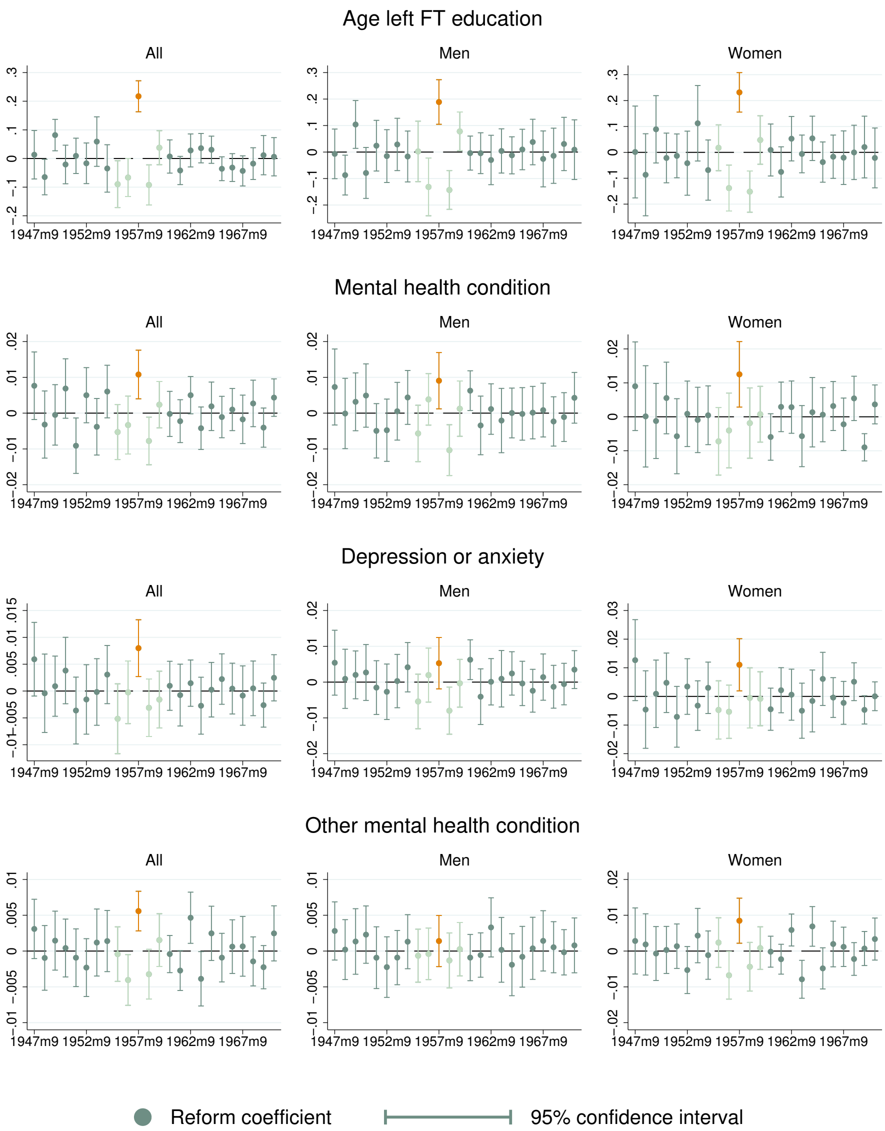


Figure 4: Reduced-Form estimates of placebo reforms



Note: 1972 ROSLA in orange.

Table 7: Different specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Age left FT education (N=198,405 bandwidth=26)											
RF	.2359***	.2364***	.2196***	.2197***	.2173***	.228***	.218***	.2247***	.2232***	.2113***	–	. 2229***
	(.0464)	(.0462)	(.0276)	(.0276)	(.0274)	(.0271)	(.0276)	(.0289)	(.0327)	(.0465)	-	(.0303)
	Mental health condition, All (N= 183,434 bandwidth=26)											
RF	.01***	.0103***	.0106***	.0107***	.0108***	.0085**	.0108***	.0127***	.0134***	.0111	.0110***	.0114***
	(.0038)	(.0038)	(.0036)	(.0036)	(.0034)	(.0034)	(.0034)	(.0037)	(.0043)	(.0086)	(.0034)	(.0034)
IV	.0425***	.0434***	.0482***	.0487***	.0497***	.0375**	.0494***	.0567***	.0599***	.0525	-	.0475***
	(.0162)	(.0162)	(.0169)	(.0169)	(.0163)	(.0151)	(.0163)	(.0178)	(.0216)	(.0419)	-	(.0155)
	Depression and anxiety, All (N=220,113 bandwidth=29)											
RF	.0073**	.0076**	.0077***	.0078***	.008***	.007**	.008***	.0091**	.0092**	.0078	.0081***	.0084***
	(.0033)	(.0033)	(.0028)	(.0028)	(.0027)	(.0028)	(.0027)	(.0036)	(.0039)	(.0053)	(.0027)	(.0028)
IV	.0312**	.0322**	.0349***	.0356***	.0368***	.0309***	.0365***	.0407**	.0412**	.037	-	.0372***
	(.0129)	(.0128)	(.0125)	(.0125)	(.012)	(.0115)	(.0119)	(.0159)	(.0179)	(.0254)	-	(.0135)
	Other mental health conditions, All (N=198,405 bandwidth=25)											
RF	.0096**	.0097***	.0086***	.0086***	.0085***	.0083**	.0082**	.0075*	.0082*	.0181***	.009***	.0087***
	(.0038)	(.0037)	(.0032)	(.0032)	(.0032)	(.0033)	(.0032)	(.0043)	(.0048)	(.0066)	(.0033)	(.0032)
IV	.035**	.0352**	.0376**	.0376**	.0366**	.0313**	.0353**	.0297	.0326	.1388	-	.0374**
	(.0143)	(.0141)	(.0169)	(.0169)	(.0168)	(.0141)	(.0169)	(.0188)	(.0218)	(.1254)	-	(.0152)
Cohort trends	Piecewise	Piecewise	Piecewise	Piecewise	Piecewise	Piecewise	Quadratic	Cubic	Quadratic Spline	Cubic Spline	Piecewise	Piecewise
Year of Survey	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month of birth	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age in month cubic	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Summer months interaction	No	No	No	No	No	Yes	No	No	No	No	No	NO
Estimation	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	Probit	Triangular Kernel

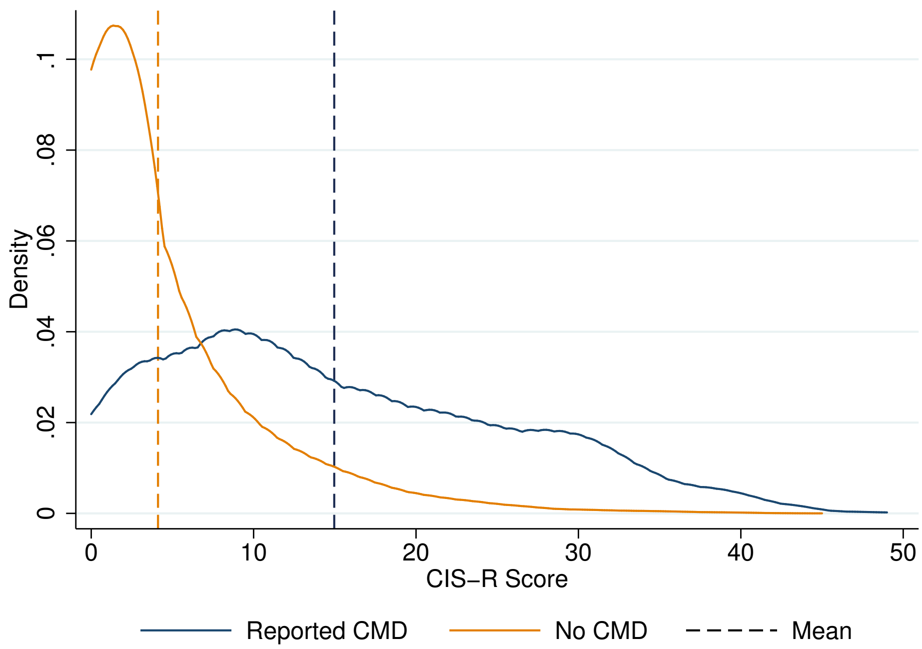
Note: Piecewise refers to a linear function of month-year of birth and a linear interaction of month-year of birth and the 1972 RSLAO dummy. Additional covariates include sex, year of survey, and response status (proxy/non-proxy). Estimates are weighted by the survey weights and standard errors clustered at month of birth are presented in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

A.3 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 contain information on whether respondents report to suffer from any mental health condition (as in the APS) and the Clinical Interview Schedule - Revised (CISR), which is 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 5 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.

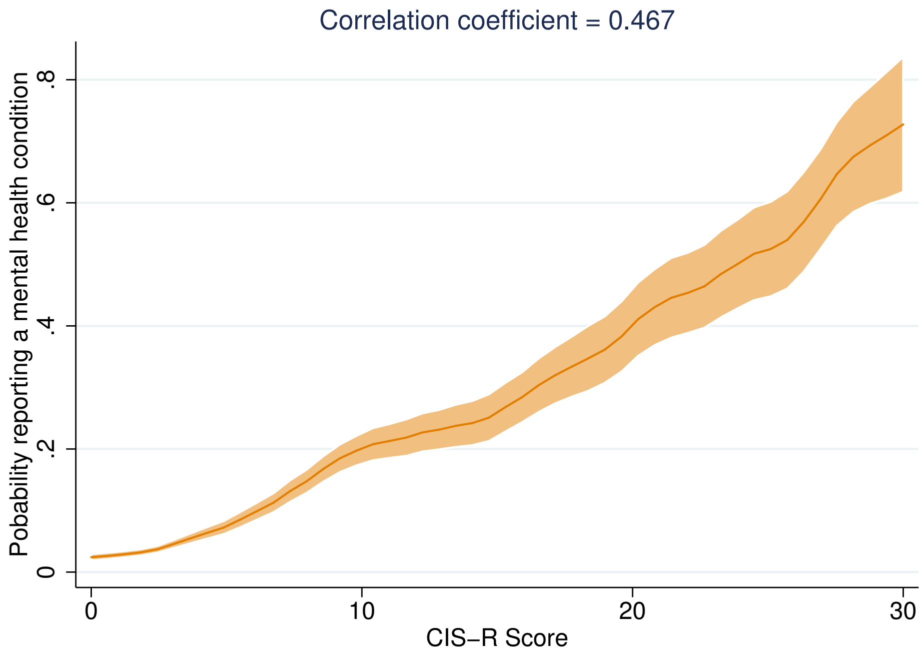
Figure 5: Distribution of CIS-R score by whether reported having any mental health condition



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

Figure 6 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 6: 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.4 Education and awareness of mental health conditions

The measures of mental health conditions in the APS consist of self-reported conditions. Education may not only have an effect on the mental health of the respondents but could very well affect the awareness of mental health conditions. If education affects awareness and detection of mental health conditions, then it is possible the increase in the propensity to report suffering from mental health conditions caused by the 1972 ROSLA could be driven by an increase in diagnosis of mental health conditions, rather than an increase in prevalence. In order to test whether education is likely to affect detection of mental health conditions, we investigate whether education is associated with self-reported mental health conditions, conditioning on mental disorder symptoms. If education has no effect on awareness and detection, then we expect that education should not be positively associated with reporting suffering from a mental health condition, conditional on mental disorder symptoms. We use data from the 2007 Adult Psychiatric Morbidity Survey, which contain information on whether respondents report to suffer from mental health condition (as in the APS) and the Clinical Interview Schedule - Revised (CISR), which is a structured validated instrument used to assess the prevalence of common mental health conditions (Lewis et al. 1992). We estimate the association between educational attainment using probit models estimated via Maximum Likelihood. In our first specification, we include age, gender as covariates. The marginal effects reported in column 1 of Table 8 show that education is associated with a lower probability to report having a common mental health condition. We then include the CIS-R score as a control variable in the model. If education has a effect on awareness and detection of common mental health condition, then the coefficient of years of education should be positive and significant. Results reported in column 2 of Table 8 the coefficient is negative and statistically significant at the 10% level, but lower than in column one. This indicates that education is unlikely to be associated with improved detection and higher awareness of mental health conditions. On the contrary, these results suggest that, with similar symptoms, more educated people are less likely to report having a mental health condition compared to less educated individuals. This analysis does not address the endogeneity of education, and should therefore be interpreted with caution.

Table 8: Unconditional and conditional association of education and self-reported depression

	(1)	(2)
Years of education	-0.00885***	-0.00299*
	(0.00187)	(0.00162)
CIS-R score		0.0169***
		(0.00110)
CIS-R score squared		-0.000182***
		(3.57e-05)
Observations	6,668	6,668

Note: Adult Psychiatric Morbidity Survey, 2007; Years of educations are calculated based on the highest qualification held by the respondent. All models include age, gender and ethnicity as covariates.
*** p<0.01, ** p<0.05, * p<0.1