The Causal Effect of Schooling on Smoking Behavior*

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Abstract

This paper, using data for Great Britain and Northern Ireland, examines the hypothesis that there is a causal relationship between schooling and cigarette smoking. Compulsory schooling laws are exploited to isolate for causation. Cohorts who were teenagers before and after the health consequences of smoking were widely known are used to compare the effects of additional schooling in the presence and absence of widespread exposure to health-related information. Although the results for Great Britain indicate no causal role for education either before or after the consequences of smoking for health were widely known, the results for Northern Ireland suggest that, at least among men, schooling affected smoking decisions prior to the public dissemination of knowledge on the dangers of smoking for health.

Keywords: education, health, endogeneity bias.

JEL Classification: I21

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1. Introduction

Cigarette smoking is the single leading cause of preventable disease and premature mortality in the United Kingdom as well as the United States. It accounts for approximately one in five of all deaths and is the leading avoidable risk factor associated with coronary heart disease and cancer (Department of Health 1998). Numerous empirical studies have documented a strong association between schooling and health (see, for a review, Grossman 2006). This article investigates the explanation that schooling creates systematic health differences through differences in the prevalence of cigarette smoking. We also explore the hypothesis that the schooling-smoking gradient reflects different amounts of health-related information. This hypothesis is consistent with the theory of the production of health developed by Grossman (1972) which suggests that more educated individuals may be better able to access and process new health information[1].

To examine the hypothesis that the schooling-smoking gradient stems from differences in health information, we focus our attention on how the first major advance in medical information in the United Kingdom affected the smoking decisions of those who would have been teenagers at the time it was published. In 1962 the British Royal College of Physicians published a report titled "Smoking and Health" which provided compelling evidence of the adverse effects of smoking for health. This report was extensively covered in the popular press at the time and was followed by government policies aimed at informing and educating society about the risks associated with smoking. We would

[1] The allocative efficiency hypothesis assumes that more educated individuals respond more rapidly to new knowledge.

expect the schooling-smoking gradient to be more pronounced in cohorts who initiated smoking before the 1962 report was widely publicized. In these cohorts, there should be more people who smoke because they lacked information concerning the risks associated with smoking. In an advanced-information environment, we would expect the schooling-smoking gradient to substantially drop as the knowledge levels of the less educated should converge to that of the more educated.

The present paper makes a new contribution to the literature to be the first to evaluate the impact of health information on the decision to smoke among individuals who were age 16 before and after the publication of the 1962 report by the British Royal College of Physicians on Smoking and Health [2]. Prior research in this area for the United States found that the link between schooling and smoking only appeared after the 1964 Surgeon General's Report on the health hazards of smoking (Farrell and Fuchs 1982).

In the health economics literature credible estimates of the causal effect of schooling on smoking are difficult to ascertain. It is well known that an observed correlation between education and health in general may arise due to an unobserved characteristic (e.g., the rate of time discount) that causes the same individuals to invest more in education and take greater care of their health. An extensive literature has found evidence of a causal relationship between education and self-evaluations of health status, blood pressure and mortality for a number of countries including the United Kingdom (Aizer and Stroud 2010; Kemptner et al., 2011; Lleras-Muney 2005; Mazumder 2008; Oreopoulos 2006;

[2] In the UK, over the course of our data, the legal minimum age to purchase cigarettes was age 16. The legal minimum age was raised to 18 in October 2007.

Powdthavee 2010; Silles 2009; Van Kippersluis et al. 2011). A number of previous studies for the United States have also found evidence of a causal relationship between education and cigarette smoking (see, for example, De Walque 2007, 2010; Grimard and Parent 2007; Kenkel et al. 2006; and Sanders 1995). This paper isolates the causal effect of schooling on smoking decisions using changes in mandatory schooling laws for Great Britain and Northern Ireland to generate exogenous variation in schooling. In this respect, our study is closest to Clark and Royer (2013) for Great Britain who found no causal link between education and current tobacco use. This paper adds to the literature by focusing on three dependent variables: the probability of smoking initiation, current smoking and the probability of cessation, allowing us to estimate the effect of schooling for multiple smoking behaviors. Furthermore, this paper is the first to examine smoking behavior in Northern Ireland. Our focus on Northern Ireland as well as Great Britain is important as its schooling reforms occurred at different times and its smoking patterns are different from the rest of the UK.

For Great Britain, our IV estimates suggest that schooling has no causal impact on the probability of smoking initiation, smoking at the time of the interview, and conditional on prior smoking, the probability of cessation. However, for Northern Ireland for male cohorts who reached age 16 before but not after the publication of the 1962 Report, we find that more schooling decreases the probability of smoking, and among those who started smoking, increases the probability of stopping. Therefore, the results for Northern Ireland support the hypothesis that, at least for males, education in an era characterized by limited access to health information aids an individual in selecting a healthier lifestyle

by its dissuasive effects on smoking. The results for women suggest that education plays no causal role in smoking behavior either before or after the widespread increase in health information.

The structure of this paper is as follows. Section 2 presents a description of the compulsory schooling law changes used for identification. Section 3 describes the data and provides some background to the 1962 Report. Section 4 presents the estimation strategy. The results are contained in section 5. The last section concludes.

2. Background

Changes in the minimum school leaving age are thought to provide a particularly credible source of exogenous variation in schooling. There were two changes to the school-leaving age laws in Great Britain and Northern Ireland in the second half of the twentieth century. The 1944 Education Act (The Butler Act) in England and Wales and the Education (Scotland) Act 1946 provided the legislative framework which resulted in the raising of the minimum school leaving age in 1947 from 14 to 15 in Great Britain. In this paper compulsory education laws are assigned based on the year the individual was born. Thus, the first law change would have affected individuals who were born in 1933 or later. The 1947 Education Act (Northern Ireland) was closely modeled on the British 1944 Education Act. However, the schooling leaving age was not raised from 14 to 15 years of age until 1957, 10 years after it was first advocated in the 1947 Act. This law would have affected individuals who were born in 1943. Following the recommendations of the Robbins Report, the minimum school leaving age increased again in 1973 to 16

years of age in both Great Britain and Northern Ireland. This change occurred in the same year for both countries, and would have affected individuals who were born in 1958.

The next two figures illustrate the experiment that this paper exploits. These figures present data from the GHS for Great Britain and the CHS for Northern Ireland. As preliminary evidence of the effects of these laws on education, figure 1 visually represents the effect that this legislation had on raising participation in education in Great Britain. Prior to 1947 a very large number of individuals left school at age 14. Within two years of the 1947 change in legislation the fraction of 14-year-olds leaving school at age 14 fell from about 60 percent to less than 11 percent. The 1947 legislation appears to have very little effect on the fraction of students dropping out at age 15 (or less) – virtually everyone who wanted to drop out at age 14 now dropped out at age 15. By 1971 the fraction of students leaving school at age 15 or less is about 35 percent. Two years following the rising of compulsory education from 15 to 16, the fraction drops to about 7 percent. For Northern Ireland, figure 2 demonstrates equally clear breaks in response to the increases in the minimum schooling leaving age in 1957 and again in 1973.

To set the stage for our empirical work, it is also important to look at changes in smoking rates. Changes in the starting rates of smoking initiation by birth cohort are reported in figure 3 for both countries. There are two important features to note at this point. The first is that despite the changes in the minimum school leaving age laws dramatically impacting education, essentially no remarkable connection with smoking initiation is visually apparent at least. The second point to note is that smoking prevalence is steadily

declining by birth cohort in Great Britain. Although the plot is somewhat less smooth for Northern Ireland, given the smaller sample size, a downward trend in smoking initiation is also apparent in these data, though this trend is much less steep than for Great Britain. The release of the 1962 report by the British Royal College of Physicians on smoking and health may have exerted an important influence on these downward trends which sharply drop in both countries for those who were born in the late 1940s[3]. Individuals born in the late 1940s would have turned age 16 around the same time the 1962 report on the adverse effects of smoking on health was first published. In the analysis below, the data is split into two samples to examine the hypothesis that changes in medical information affected the impact of schooling on starting smoking.

Figure 4 plots current smoking rates by birth cohorts. Although figure 3 clearly shows that historically people in Northern Ireland have been less likely to smoke than in the rest of the United Kingdom, current smoking rates are actually quite close for both countries. Another important observation is that the current smoking rates appear to be slightly higher for progressively younger cohorts in both countries. This pattern is likely to be linked to the age distribution of cohorts. Age controls are added to all regressions in order to absorb age-related differences in smoking rates.

Figure 5 shows how quitting rates have evolved by birth cohort. This figure clearly illustrates that the proportion quitting smoking appears to have steadily declined in Great Britain. This declining trend reflects the fact that the average fraction initiating smoking

[3] Davy (2006) using the General Household Survey showed that for individuals born after 1950 the prevalence of cigarette smoking is markedly less than for those born before due to an increase in the number of young people who had never smoked.

also declined among successively younger birth cohorts. As stopping rates for Great Britain have declined from a relatively high level, the cessation rates for Northern Ireland have slightly trended upwards from a relatively low base. The difference between quitting rates across the two countries has been driven down primarily by the rapid decline in smoking initiation in Great Britain. In general the strikingly different patterns in smoking initiation and quitting behaviors that are apparent for Great Britain and Northern Ireland motivate the use of separate regression analysis for the two regions.

3. Data

Two large scale household surveys comprise the data sources for this study: the General Household Survey (GHS) for Great Britain (England, Scotland and Wales) and the Continuous Household Survey (CHS) for Northern Ireland. The data used for Great Britain is derived from combining the household surveys between 1978 and 2004 and for Northern Ireland between 1983 and 2004 for the years when smoking information was collected. It is important to note that data on smoking behavior is not available for all years of these surveys[4]. Yet, the combined sample for Great Britain, which has been obtained from pooling the cross sections possessing complete data with which to estimate the empirical models presented below, is still relatively sizeable and amounts to 169,937 observations. For Northern Ireland, our working sample is 34,927 observations. This sample contains information for those at least age 23 who were born between 1923 and 1981. Summary statistics for the variables used in this study can be found in table 1.

[4] The GHS contains fifteen smoking supplements as part of the surveys conducted in 1980, 1982, 1984, 1986, 1988, 1990, 1992, 1994, 1996, 1998, 2000, 2002, 2003, 2004 and 2005. The CHS contains ten smoking supplements in 1983, 1984, 1988, 1990, 1992, 1994, 1998, 2000, 2002 and 2004.

There are two particularly fortuitous characteristics of these data. The first is that years of schooling are recorded precisely as the age the individual completed full-time continuous education, which includes college education. This variable has the advantage that it can be matched closely with changes in the minimum school leaving age, the instrumental variable used in this study. For Great Britain and Northern Ireland, the data in table 1 show that the average number of years of schooling is similar for men and women at approximately 11 years. For Great Britain, the tabulations also show that 15 percent faced a minimum school leaving age of 14, while 56 percent faced a minimum school leaving age of 15, and 29 percent faced a minimum school leaving age of 16. For Northern Ireland, the equivalent numbers are 34 percent for the MSLA14, 35 percent for MSLA15, and 31 percent for MSLA16.

The second important feature of these data is the detail with which current and past smoking behavior is recorded which can be used to construct retrospective histories of smoking status. Three dichotomous variables are derived to capturing the respondent's smoking decisions which are incorporated into our analysis. Respondents to the GHS/CHS surveys were asked if they had ever regularly smoked. From this information, the first dependent variable is created which focuses on the decision to start smoking with a comparison between those who ever regularly smoked and those who never smoked. An important feature to notice in the summary statistics presented in table 1 is that the probability of having ever smoked is much higher in Great Britain than in Northern Ireland, a result which was already apparent in figure 3. The data for the whole sample indicates that approximately 72 percent of people in Great Britain and 60 percent of

people in Northern Ireland are classified in these data as having regularly smoked at some point in time. Smoking initiation rates are also much higher for men than for women in both regions.

Those who were categorized in these data as having ever regularly smoked were asked if they were currently smoking. From the answers to this question, the second dependent variable is created which takes a value equal to one if a respondent is a current smoker and zero otherwise. The summary statistics presented in table 1 show that about 32 percent of men and women are classified as current smokers which is similar for both Great Britain and Northern Ireland.

A third dependent variable is constructed which focuses on the decision to quit smoking among those who ever regularly smoked. The cessation variable is coded to equal a value of one for individuals who had quit smoking at the time of the interview and a value of zero for those who had not stopped. In our data approximately 56 percent of men and women in Great Britain who were former smokers had stopped by the time of the interview in comparison with just 46 percent in Northern Ireland.

The 1962 report of the Royal College of Physicians on Smoking and Health provided a robust account of scientific evidence on the adverse effects of smoking including detrimental effects on mortality due to heart disease, cancer and chronic obstructive pulmonary diseases. The report received extensive coverage in the popular press and propelled a momentous change in public policy on smoking. Half a century prior to the

publication of this report, the harmful effects of smoking for health were suspected as death rates among men from lung cancer were notably increasing at the same time as cigarette smoking had spread[5]. In 1950 a set of landmark research studies in cancer epidemiology appeared, one in the UK and four in the US, directly linking smoking to lung cancer and cardiovascular disease (Peto et al. 2000). Although the consensus regarding the health damaging effects of smoking in the medical profession was achieved in the early 1950s, it was not until the publication of the 1962 report that the health consequences of smoking were massively disseminated to a wider public. Differences across schooling levels would not necessarily be expected to influence smoking decisions before the 1962 Report if most people were unconvinced about the serious health risks associated with smoking. However, in the years leading up to the publication of the 1962 report, the risks associated with smoking had received substantial press attention. For example, there were a total of twelve articles on the health consequences of smoking in the period from 1950 to 1959 in the *Reader's Digest* (Viscusi 1992). Although 1962 marked the beginning of serious efforts to improve consumer information about the impact of smoking, from the early 1950s onwards information on the harmful effects of smoking was gradually emerging. Thus, there was a substantial amount of information available in the ten years prior to the public of the 1962 report. As this information was not widespread, it would seem reasonable to expect that more educated individuals would have had better access to that information. Moreover, we also expect that more educated individuals would have reacted more quickly to the arrival of new information about the health-damaging effects of smoking.

[5] See, for a historical literature review on smoking and disease, White (1990).

The regression analysis will carefully look at the decision to smoke with a comparison between those who were age 16 before and after the 1962 report. For this reason, survey respondents are assigned to the "pre-health concern" generation if they were born in the calendar year 1946 or before, and to the "post-health concern" generation if they were born in 1947 or later. Mindful of the problem that some individuals began smoking earlier and some later, the 1946 cut point is chosen because individuals born in this year would have been age 16 in 1962 which has been the legal age at which individuals are first permitted to purchase cigarettes in the United Kingdom since 1908[6]. The 1962 report may have crucially affected those who were looking at the decision to quit smoking. Table 1 also gives the percentage of smokers who quit smoking before and after the widespread increase in consumer information. For Great Britain, the summary statistics reveal evidence of a dramatic difference in quitting rates from 60 percent before 1962 to 52 percent after 1962. The analogous statistics for Northern Ireland show that quitting rates decrease after the widespread diffusion of health information to 42 percent, down from 50 percent before the 1962 Report. The large reduction in cigarette consumption for men and women who were age 16 prior to the publication of the 1962 Report is consistent with previous findings by Atkinson and Skegg (1973) using aggregate data on tobacco consumption to examine the effects of the 1962 report.

The effect of schooling on smoking may operate through a number of mechanisms. Particularly at low levels of education, the compulsory education reforms by lengthening the minimum school leaving age should help deter young people from smoking in the short term; while individuals are attending classes they do not have an opportunity to

[6] The legal minimum age for the purchase of cigarettes in the UK was raised to 18 in October 2007.

smoke cigarettes[7]. As already noted, an additional year of education should improve an individual's ability to better understand new information on the consequences of smoking for health. Moreover, since education improves human capital it increases expectations about future labor market prospects, which may lead to a higher demand for health and a reduction in the demand for cigarettes. Schooling may also be related to smoking decisions for reasons unrelated to health concerns. For example, schooling effects may reflect differences in the acceptability or the stigma attached to smoking across social groups.

4. Methodology

The econometric model for the relationship between education and smoking behaviors can be written as:

$$S_i = \beta_1 + \beta_2 E_i + g(B_i) + \beta_3 X_i + \varepsilon_i \qquad (1)$$

where S_i is a dichotomous 0-1 variable describing smoking behavior (either current smoking or cessation), E_i is the number of years of schooling, and $g(.)$ is a quartic polynomial in year-of-birth (B) to account for any differences in the consumption of smoking across cohorts in our data[8]. X_i represents other covariates including age and gender. The error term is denoted by ε_i. Huber-White standard errors are computed from clustering by birth cohort and country.

If the residual is correlated with years of schooling due to omitted variables, least squares

[7] Jacob and Lefgren (2003) discuss an "incarceration effect" in the context of the effect of education on teenage criminal behaviour. A similar "incarceration effect" explanation may apply to the effects of compulsory schooling on smoking.
[8] In practice, the estimates are very similar if a slightly lower or higher order polynomial is used.

estimates of the schooling coefficient, β_2, will be biased. Social scientists have long recognized that there are many individual characteristics such as the rate of time preference which could give rise to biased estimates. For example, individuals with a high discount rate of time, presumably from more disadvantaged backgrounds, tend to acquire less schooling and are more likely to smoke cigarettes. To the extent that variation in unobserved characteristics is important, OLS estimates which do not account for these characteristics could overstate in absolute value the true effect of schooling on smoking.

One potential solution to this problem is to find an instrumental variable that induces exogenous variation in schooling but is uncorrelated with discount rates and other characteristics which affect both smoking behavior and schooling. In the present article changes over time in the number of years of compulsory education that countries mandate serve as an instrument for schooling. Accordingly, the following model is estimated as the first-stage equation:

$$S_i = \alpha_1 + \alpha_2 MSLA15_i + \alpha_3 MSLA16_i + f(B_i) + \alpha_4 X_i + \nu_i \qquad (2)$$

where $MSLA15_i$ and $MSLA16_i$ are indicator variables for whether individual i was subject to the minimum school leaving age of 15 or 16, respectively. These law variables are assigned to individuals on the basis of country of birth and the year of birth as follows. Individuals who were born in Great Britain before 1932 and in Northern Ireland before 1942 would have faced a minimum school leaving age of 14. MSLA15 is defined equal to one for those born after these dates but before 1957 who were impacted by the first law reform which raised the minimum school leaving age to 15 in both countries, and zero

otherwise. The MSLA16 is likewise set equal to one for individuals born in both countries after 1958 who were impacted by the second law reform which compelled them to remain in school until age 16, and zero otherwise. The maintained identification assumption is that conditional on year of birth the timing of the changes in compulsory attendance laws within each country should be orthogonal to unmeasured characteristics of individuals that affect smoking rates. Since the error term in equation (1) might be correlated with education, we re-estimate it with IV, instrumenting education using these reforms. If increased education lowers the probability of becoming a smoker and/or increases the probability of stopping having started, education would have significant consequences in terms of improving the health of the population.

While there are good reasons to pool the data for the Great Britain and Northern Ireland together, this approach assumes that cohort effects and age effects are the same in the two countries. This may be quite a strong assumption given that smoking behaviors are observed to differ in level and trend across cohorts in Great Britain and Northern Ireland. Thus, separate regressions are estimated for the two regions.

5. Results

Before turning to the OLS and IV estimates, the first-stage estimates in which years of schooling is regressed on the compulsory schooling dummies are presented for both countries in table 2. The results for Great Britain are displayed in the first three columns of the table. In column (1) for the entire sample, the jump in average educational attainment corresponding to the first law reform which increased the age at which an

individual was permitted to leave school from 14 to 15 in 1947 is 0.52 years. This coefficient yields the average effect of the reform across all individuals including those with a college education. The coefficient on the second reform associated with raising the minimum school leaving age to from 15 to 16 implies an average increase of 0.57 years of schooling. Given that the objective of using this relationship is as the basis of the instrumental variable strategy, it is important to record the F-statistic for the joint significance of the excluded instruments in the first-stage equation. The F-statistic for this sample is 168, which implies that the instruments perform well in explaining variation in educational attainment.

In order to investigate whether the aggregate analysis obscures the impact of education on smoking rates following the release of the 1962 report, the data is split to identify cohorts born before and after 1946. In column (2) for the cohorts born before 1946, the rise in educational attainment associated with the first schooling reform is 0.67 of a year with a standard error of 0.043. This is a precise estimate with a corresponding F-statistic of 248, suggesting that the first-stage has good power and the instrument is not weak. For cohorts born after 1947, the estimate of the education effect of the second compulsory attendance law reveals that this constraint extended education on average by a further 0.35 of a year. The associated F-statistic is 70.57, which implies that the compulsory education laws are a good predictor of years of schooling. In the lower panels of the table, breaking out the data by gender reveals that men and women responded similarly to changes in compulsory education.

Beyond looking at the statistical significance of the excluded instruments, we also examine the Stock and Yogo (2005) test for weak instruments. Using their criteria a set of identifying instruments is considered weak if the bias of the IV relative to that of the OLS estimator exceeds a certain threshold. In all subsamples for Great Britain, the F statistics in the first stage of the IV exceed the 10% threshold level in Stock and Yogo (2005)[9]. These results strongly reject the null that our instrument is weak.

Columns (4) through (6) of table 2 present parallel results for Northern Ireland. Although the first raising of the minimum school leaving age from 14 to 15 occurred in 1957 in Northern Ireland, a full decade after it was raised in Great Britain, the results show that this change was also remarkably effective in raising educational attainment. For the entire sample, individuals who were compelled to remain at school until age 15 have approximately 0.62 years of schooling more than those who faced a minimum school leaving age of 14. The coefficient on the second schooling reform shows that those who were constrained to stay in school until age 16 attained 0.73 years higher schooling compared to those who faced the lowest drop out age of 14. Although the parameters are less precisely estimated than for Great Britain, they are still statistically significant at the 1 percent level. The F-statistic on the excluded instruments indicates that there is ample explanatory power in the first-stage.

Focusing on those who were age 16 prior to 1962, schooling attainment increased on average by 0.804 years as a result of the raising the minimum school leaving age from 14

[9] In the case of a single endogenous regressor, as we have here, the Kleibergen-Paap rk Wald F-statistic reduces to the standard F-statistic on the exclusion of the instruments from the first stage.

to 15. For those age 16 after 1962, the coefficient on the second compulsory attendance law indicates that individuals who faced a drop out age of 16 achieved 0.222 more years of schooling than those who were eligible to leave at age 15. The associated F-statistic is 7.69, which is above the 20% critical value for the maximal relative (to OLS) bias in the IV coefficient proposed by Stock and Yogo (2005). This implies that our IV estimates should be successful in removing most of the bias in the OLS estimates.

For the extract of men, the point estimates on the reform dummies are roughly similar to those for the aggregated sample. For the cohorts that preceded the 1962 report, the coefficient of 0.94 on the first reform indicates that men in Northern Ireland facing a minimum school leaving age of 15 attained almost a full year more of schooling than those who were compelled to stay until age 14. The F-statistics associated with these results point to no weak instrument problem using the Stock-Yogo criteria. For the cohorts that followed the 1962 report, the second reform that raised the minimum school leaving age to 16 entailed an average increase in schooling of 0.36 years. In this case, the associated F-statistic of 8.76 is above the Stock-Yogo 20% critical value.

For all cohorts of women, the coefficients on the two schooling reforms look very similar to those for the aggregated sample over the same range of data. For those age 16 prior to 1962, the first reform which raised mandatory schooling from 14 to 15 in 1957 increased schooling by approximately 0.69 years. The associated F-statistic is 14.23, which is above the 15 percent critical value tabulated by Stock and Watson (2005). For the later birth cohorts, the effect of the 1973 reform is small and lacks precision: the coefficient

(standard error) is 0.123(0.110) and the F-statistic is only 1.24. The last compulsory education law appears not to have prolonged schooling for more recent cohorts of women in Northern Ireland. Thus the 1973 reform does not provide a useful instrument in the subsample restricted to women born between 1947 and 1981. The insignificant effect of compulsory schooling for this subgroup of women is obviously very different from the highly statistically significant equivalent estimate for men. IV results from models where the instrument is weak should be interpreted cautiously.

For Great Britain, table 3 presents the OLS and IV results of the impact of schooling on the probability of smoking initiation. The OLS specification assumes that schooling is exogenous and the estimates indicate a negative association between schooling and the probability of starting smoking. For the total sample, the results suggest that one extra year of schooling reduces the probability of smoking by 1.7 percentage points. Given that the percentage of the sample who ever smoked prior to the 1962 report is 77 percent, this coefficient implies that the effect of increasing the number of years of schooling by one is to decrease the probability of smoking by just 2.21 percent. IV coefficients are virtually identical in magnitude to the OLS coefficients, though imprecisely estimated. To explore the impact of schooling on starting smoking in response to the release of 1962 report, estimates for the two cohort groups that permit a before and after information comparison are presented. The OLS coefficients reveal that an extra year of education reduces the probability of smoking before the spread of health information by 1.0 percentage points rising to 2.1 percentage points after the increase in information. Since 67.5 percent of individuals smoked after the 1962 report, the estimate of 0.021 (0.001) translates into a

3.1 percent decrease in the likelihood of smoking as a result of an additional year of education. IV coefficients are imprecisely estimated before as well as after the dissemination of health knowledge on the consequences of smoking, though similar in magnitude to the OLS coefficients prior to the publication of the 1962 report. The results disaggregated by gender reveal estimates that are broadly similar for men and women.

The OLS results for Northern Ireland, presented in columns (1) through (3) of table 4, are comparable to those found for Great Britain. These results show a strong association between schooling and smoking for the total sample, for the subsamples before and after the widespread increase in information, and among both men and women. The IV coefficients on schooling are typically not statistically different from zero at any conventional level of significance. The one important exception occurs with the cohort of men age 16 before the 1962 report. For this group, the IV coefficient on schooling is -0.104(0.051) which is statistically significant at the 5 percent level. With 73.3% of men having ever smoked, the decrease in the probability is 14%. IV results for the post-1962 cohorts show that the gap in smoking initiation between men with different schooling levels does not remain after the widespread increase in health knowledge. These results are in line with what one would expect if schooling's influence on smoking decisions was primarily through the acquisition and processing of health information. In contrast, for women the IV results are entirely statistically insignificant both before and after the widespread dissemination of health information.

The OLS and IV results for a discontinuity sample presented in the last three columns of

the full sample. Past the massive increase in information in 1962, the IV estimates essentially show no relationship between schooling and smoking. Thus, the results from the discontinuity sample lead to similar conclusions as that for the full sample.

In addition to looking at smoking initiation and current smoking behaviors, we also examine the impact of schooling on the probability of stopping smoking, conditional on being a current or former smoker. Thus the composition of the sample for the analysis of cessation is restricted to those who ever smoked as quitting behavior for individuals who never smoked cannot be observed. De Walque (2010) points out that as the decision to quit smoking is usually taken when education is completed, it may provide a clearer margin for analysis than smoking initiation, which generally occurs before schooling differences can arise. One potential problem in the interpretation of this set of findings is the risk of sample selection bias in the relationship between schooling and stopping smoking. Given that this analysis results in dropping individuals who never smoked (and thus are on average better educated), the most likely concern is that our schooling estimates will be biased upwards in absolute terms. Keeping this in mind, table 7 presents the results for Great Britain. For those who were age 16 before knowledge of the harmful consequences of smoking was widely diffused, the OLS estimates show that one extra year of education raises the probability of quitting smoking by 3.8 percentage points. For those who were age 16 following the widespread dissemination of smoking information, the associated point estimate is 4.6 percentage points. The relative consistency of the OLS results seem to imply that the probability of quitting smoking increases with years of schooling and the dramatic shift in public emphasis has no large differential impact on

the education gradient.

The compulsory education laws are again used as instruments to identify the causal effect of schooling on quitting smoking, conditional on having ever initiated smoking. For those who were age 16 both before and after the health consequences of smoking became widely known, the IV coefficients are small and statistically insignificant. The results for men and women are similar and reveal no association between schooling and stopping smoking.

The results for Northern Ireland are presented in the first three columns of table 8. For all birth cohorts, the OLS estimates appear to suggest that increases in education are associated with a higher probability of cessation. However, the IV estimates suggest that there is no causal effect of schooling on smoking cessation except for men who were age 16 prior to the widespread increase in health information. Among these men, the IV results indicate that those who attended school longer because of the 1957 compulsory schooling law are more likely to quit smoking as a result of their increased schooling. The estimated effect of an additional year of schooling for this subsample is 10 percentage points. Given that the proportion of men stopping smoking is 55.7%, the IV estimate implies that an additional year of schooling due to the first reform causes an increase in the probability of stopping by 18%. For the later born cohort who would have been widely exposed to health information on the dangers of cigarette smoking, the analogous estimates are neither statistically distinguishable from zero nor correctly signed.

Once again to check whether the estimated schooling-smoking relationship is truly a result of education for the subsample of men who attended school prior to the massive increase in information about smoking risks, we restrict the data to the narrow discontinuity sample in columns (4) through (6) of table 8. The results of this exploration are similar to those for the full sample. In particularly for men in the earlier cohorts, the IV estimate on schooling is positive, statistically significant (at the 10 percent level) and actually slightly greater than the corresponding IV estimate in column (2). As a consequence, this strengthens our interpretation that the IV estimate reflects a causal role for schooling in stopping smoking for men in school prior to the 1962 report.

Our robustness strategy of narrowing the sample to those born just three years before and after the reforms relies on the assumption that all of the difference in smoking behavior is attributable to the difference in education. Although this seems plausible given the very narrow discontinuity sample, one might still worry that the IV estimated schooling effect is still picking up something else. Given that the IV estimates are only statistically significant for the cohort of men born between 1923 and 1946 (that is, age 16 prior to the widespread increase in information), we construct a placebo education reform by falsely assuming that the reform took place either before or after the actual reform was implemented. If we uncover a statistically significant effect of the placebo, our main results from the actual reform are likely to be spurious. In work that is not reported, we tried placing the 1957 compulsory education instrument for Northern Ireland in 1947, and exclude all real post-reform cohorts. That is, we assume that Northern Ireland had raised

the minimum school leaving age from 14 to 15 at the same time as the rest of the UK. For all three measures of smoking behavior, the schooling estimates generated using the placebo reform are statistically insignificant. We also tried placing the first reform for Northern Ireland just two years before and after the true reform. The results of this analysis were also statistically insignificant. Thus, our placebo analysis supports our initial findings that education plays an important role for men in Northern Ireland in reducing cigarette smoking.

To sum up our main findings, for Great Britain the causal results suggest no link between increases in schooling and a reduction in cigarette smoking among both men and women. However, the Northern Ireland results for men (but not for women) indicate a steep decrease in the probability of smoking associated with one more year of schooling before rather than after the rise in public information. This contrast in results by region is interesting and naturally raises the question of why there is no evidence of a reduction in smoking due to additional education in Great Britain prior to the widespread increase in health information. A likely answer is that the first school reform instrument, which was implemented a decade apart in the two countries, essentially compares individuals from very different cohorts in Great Britain and Northern Ireland. The fact that the education reform in Northern Ireland was delayed until 1957 meant that its implementation occurred only five years before the publication of the 1962 report. Individuals in Northern Ireland in this five-year interval were attending school for longer in an environment where information was gradually accumulating on the risks associated with smoking. According to the theory of the production of health developed by Grossman

(1972), education improves the acquisition and use of medical knowledge in health related decisions. This theory sees that the more educated would obtain information about the damaging effects of smoking even during a time when medical knowledge about smoking related illnesses was not widely accessible, such as during the five-years prior to the publication of the 1962 report. The fact that the schooling-smoking gradient is present in Northern Ireland at roughly the same time as the gradual arrival of new health-related information prior to the publication of the 1962 report is consistent with a causal role for schooling through health information[10]. This role is further supported by the evidence that the gradient entirely disappears following the 1962 report when information was widely diffused to the less educated.

For Great Britain, the IV estimates for Great Britain based on the 1947 reform suggest that education did not exert an important influence on smoking prior to1962 in this country. The earlier implementation of the education reform in 1947 in Great Britain was before the consensus in the medical literature on the predominant role of tobacco use as the main cause of lung cancer in 1950. One reason for the difference in results between Northern Ireland and Great Britain is that a large fraction of individuals in Great Britain had received an additional year of education as a result of the 1947 reform ahead of knowledge about the harmful effects of smoking on health. The effect of education would be unlikely to be present in an environment prior to the late1950s when little information about the risks associated with tobacco was available in the media (Viscusi 1992). This

[10] Direct measures of differences in health knowledge would be required to empirically test the hypothesis that schooling reduced smoking by improving knowledge of the relationship between smoking and health (Kenkel 1991).

reasoning may explain why we do not observe the same effect of education in Great Britain that we observed for Northern Ireland prior to 1962.

For men in Northern Ireland, on balance it seems reasonable to assume that there was a causal pathway for schooling to smoking. However, the results for women are entirely statistically insignificant for the early birth cohorts. The difference in results for men and women in Northern Ireland is surprising given that women would have been exposed to the same information as men prior to the 1962 report. However, gender disparities in smoking by education may be attributable to two factors. First, schooling increases the returns to work, which makes any time spent out of the labor market due to illness more costly. This channel is likely to raise the value of good health for men more than women as long as schooling increases the marginal return to work more for men than women (Becker 1993; Ross et al. 2012; Ross and Mirowsky 2006). Second, one of the main reasons that women have historically smoked less than men is externally imposed normative constraints that discouraged women from smoking (Bird and Rieker 2008; Pampel 2002). If men engaged in smoking more than women in part because they face fewer external constraints, then education may have more of an effect on the smoking decisions of men than women as men would be more self-directed (Mirowsky and Ross 2005). For this reason, more educated men might be more open to messages about the adverse effects of smoking and thus more likely to select a healthier lifestyle compared to more educated women. Through both these channels educated men and women are unlikely to experience a similar response to knowledge about the adverse consequences of smoking for health.

Our finding that the education gradient in smoking past the publication of the 1962 report did not persist in either Great Britain or Northern Ireland is particularly noteworthy. One possible explanation for the absence of a statistically significant education effect following the 1962 report is that the rapid improvement of knowledge about the health hazards of smoking led to the elimination of the knowledge gap between education categories as everyone became exposed to the same information. The widespread improvement in information on the harmful health effects of smoking appears to have removed educational disparities in smoking that existed in earlier cohorts of men in Northern Ireland.

It is also worth bearing in mind in the interpretation of our results that the compulsory education laws affected a large fraction of individuals who would have left school earlier had they not been compelled to stay an additional year by the raising of the minimum school leaving age. Imbens and Angrist (1994) have shown that IV and OLS estimates are usually difficult to compare as OLS seeks to estimate the average treatment effect (ATE) across the population, while IV identifies the local average treatment effect (LATE) for the subsample whose education was raised as a result of the reforms. For men in Northern Ireland who were age 16 before the 1962 report, our IV estimates, which are larger in absolute terms than our OLS estimates, may be picking up the effect of a lower discount rate for those who were redirected at age 14 to stay in school for one more year

than for those who left earlier[11].

6. Conclusion

For economists interested in health, the relationship between schooling and smoking is particularly important as smoking is the leading preventable cause of premature mortality in all advanced industrial countries. The goal of this paper is to test the hypothesis that education has a casual effect on smoking. Compulsory education laws in Great Britain and Northern Ireland are exploited as instruments for years of schooling as these laws should have no bearing on an individual's smoking habits except through increased schooling. The analysis was undertaken separately for men and women given the possibility that males and females may have reacted differently to the increases in education.

For both countries, the OLS results are consistent with the hypothesis that education causes individuals to be healthier through a reduction in smoking. However, for Great Britain, IV estimates are typically not statistically significant either before or after the widespread public dissemination of information on the dangers of smoking for health. These patterns are consistent with previous work by Clark and Royer (2013) for Great Britain, but contrast with what is found for Northern Ireland. For Northern Ireland, our pattern of results for males is consistent with the idea that schooling has a causal pathway leading to better health through education helping individuals become better informed about the health implications of smoking. This interpretation is reinforced by the finding

[11] Becker and Mulligan (1997) suggest that changes in education can lower the discount rate of time.

that schooling differences did not remain once the harmful effects of smoking became widely known. The difference in our results for men between the two regions may be linked to the timing difference of the instrumental variable that generates these IV estimates. Unlike the rest of the United Kingdom, the raising of the minimum school leaving age from 14 to 15 for Northern Ireland was roughly coincident with the gradual diffusion of new information on the health hazards of smoking a half decade before this information widely publicized following the 1962 report. For both countries for women we do not find any significant causal effect of schooling on smoking either before or after the widespread improvement in health information. The difference in estimated schooling effects found for men and women in Northern Ireland may indicate that education and health-related information mediate lifestyle choices differently for men in comparison to women. Subsequent research should aim to understand these differences by gender.

The conclusions drawn from this article are subject to the caveat that the compulsory education reforms extended education at the lower tail of the distribution. Thus, the estimates of the effect of schooling on smoking are specific to the sub-population who would have presumably left school earlier in the absence of the law changes. It remains an open question whether policy changes that increase participation throughout the education distribution would be successful in reducing smoking. For example, de Walque (2010) for the United States finds that college educated women were less likely to smoke and faster to respond to the diffusion of information on the health consequences of smoking. In future research it would be interesting to examine the causal effect of schooling on smoking at different schooling margins for the United Kingdom.

Acknowledgement

I would like to thank two anonymous referees for helpful comments.

Figure 1. Fraction who left full-time education by age 14 and 15 for Great Britain

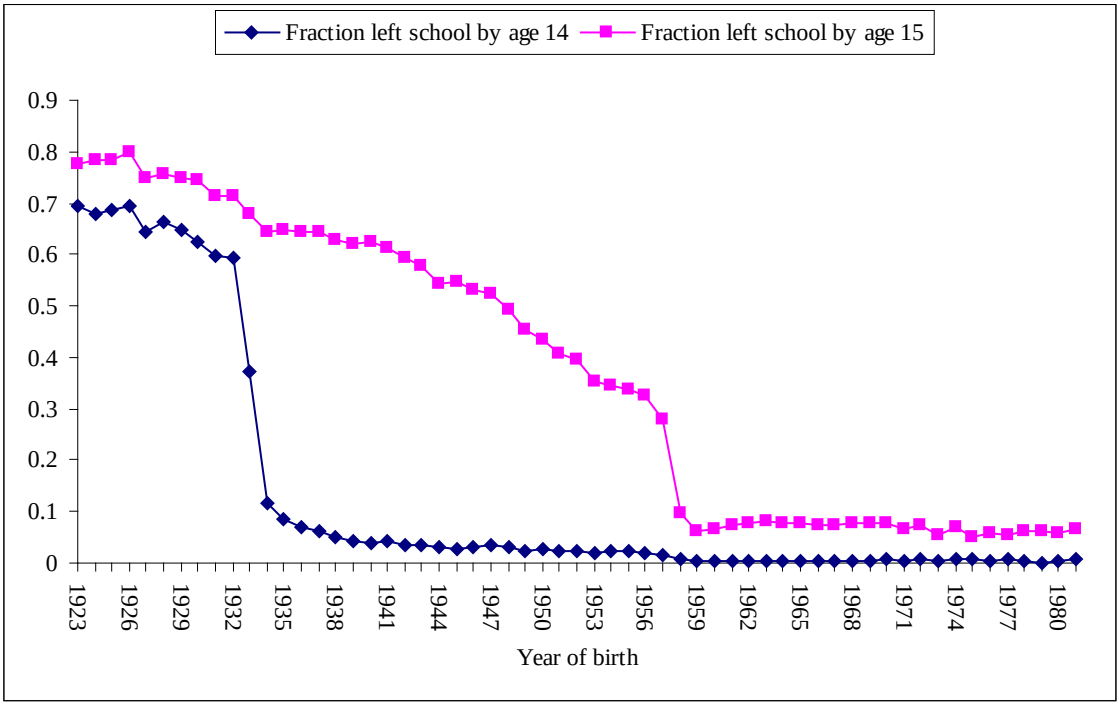

Figure 2. Fraction who left full-time education by age 14 and 15 for Northern Ireland

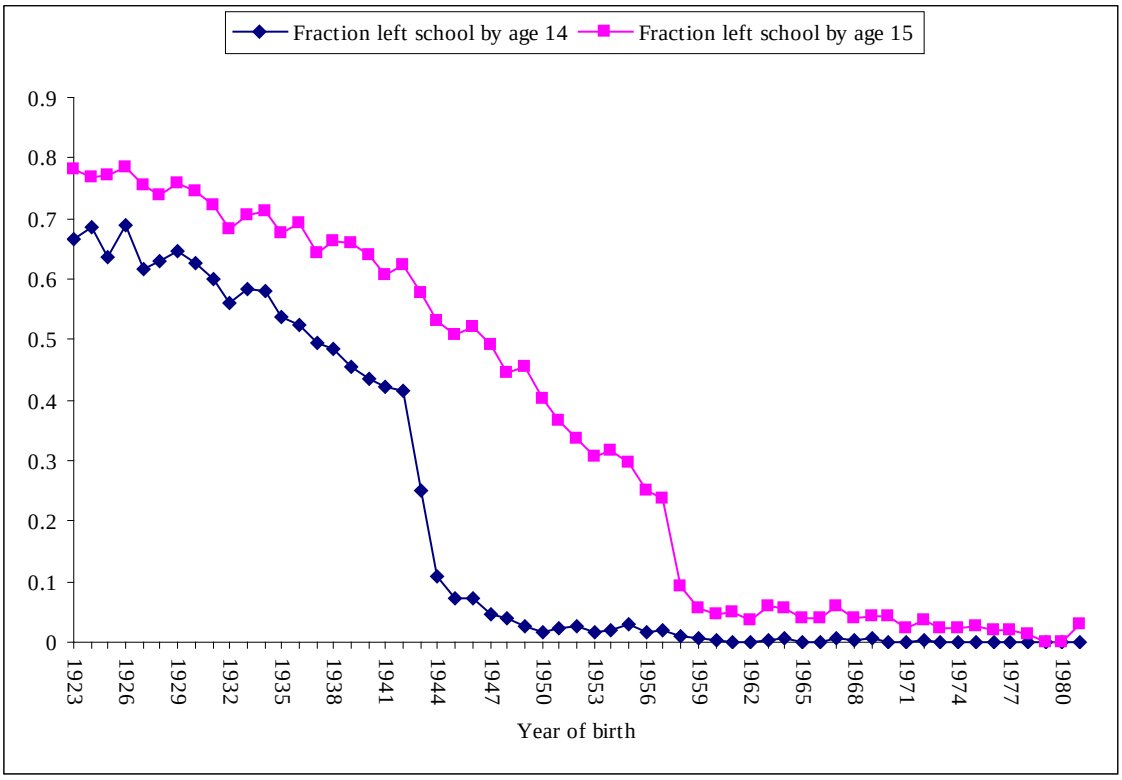

Figure 3 Fraction who reported ever smoking in Great Britain and Northern Ireland

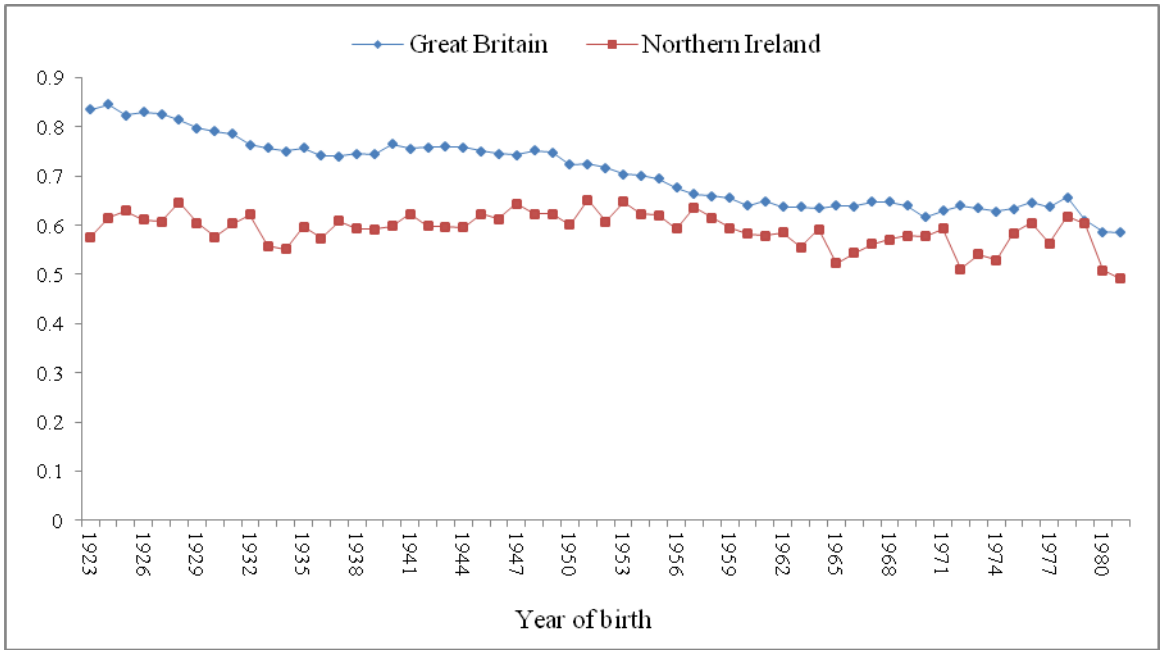

Figure 4 Fraction who are current smokers in Great Britain and Northern Ireland

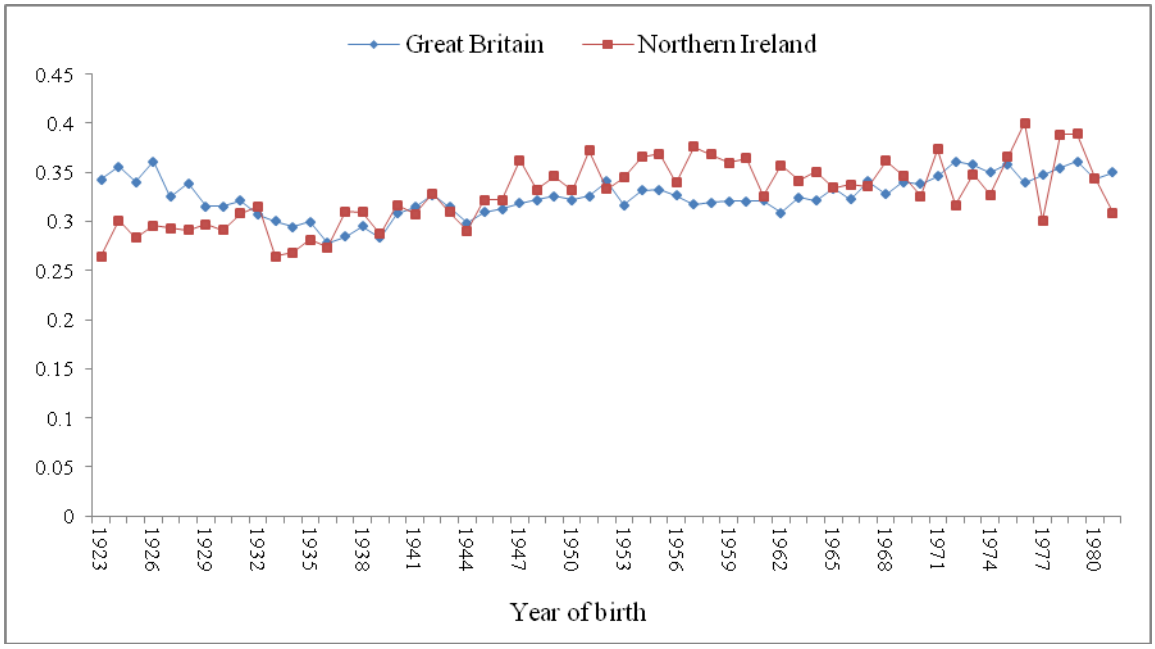

Figure 5. Fraction who quit smoking in Great Britain and Northern Ireland

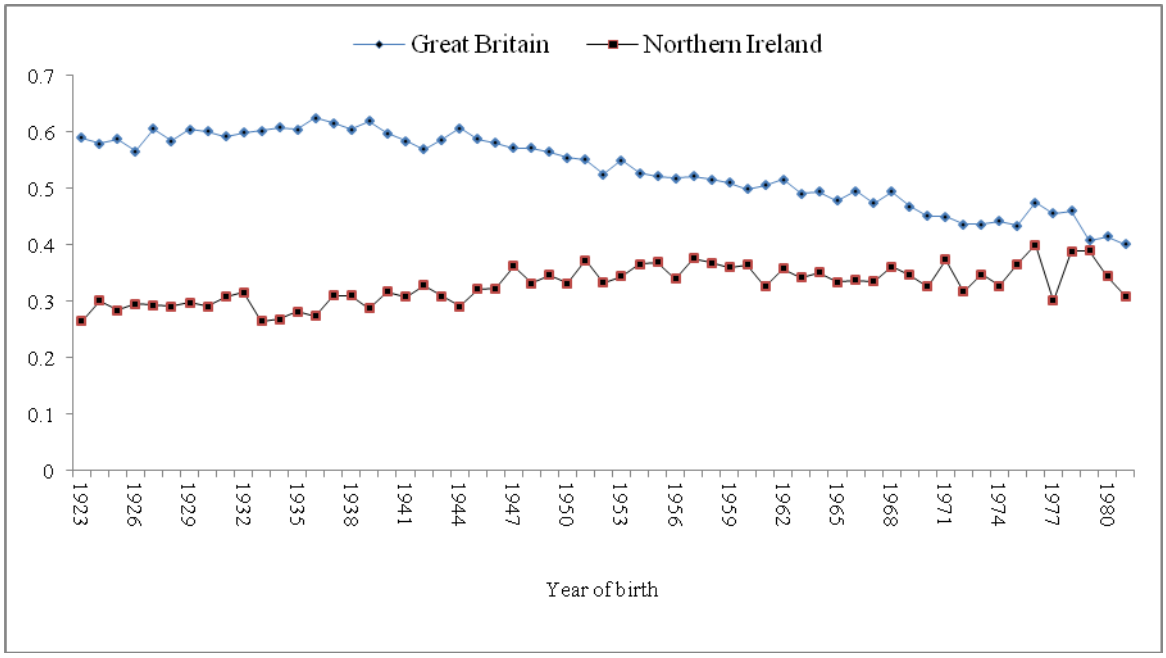

Table 1. Summary statistics, Great Britain and Northern Ireland.

	Great Britain			Northern Ireland		
	(1)	(2)	(3)	(4)	(5)	(6)
	1923-1979	1923-1946	1947-1979	1923-1979	1923-1946	1947-1979
Total sample						
Years of schooling	11.569	10.674	12.220	11.450	10.432	12.164
	(2.231)	(1.890)	(2.235)	(2.236)	(2.005)	(2.110)
MSLA 14	0.149	0.355	0.000	0.344	0.834	0.000
	(0.356)	(0.479)	(0.000)	(0.475)	(0.372)	(0.000)
MSLA 15	0.559	0.645	0.496	0.349	0.166	0.478
	(0.497)	(0.479)	(0.500)	(0.477)	(0.372)	(0.500)
MSLA 16	0.292	0.000	0.504	0.307	0.000	0.522
	(0.455)	(0.000)	(0.500)	(0.461)	(0.000)	(0.500)
Ever smoked	0.716	0.773	0.675	0.597	0.599	0.595
	0.451	0.419	0.468	(0.490)	(0.490)	(0.491)
Current smoker	0.315	0.309	0.319	0.324	0.297	0.342
	(0.464)	(0.462)	(0.466)	(0.468)	(0.457)	(0.474)
Quit smoking	0.559	0.599	0.524	0.455	0.504	0.420
	(0.497)	(0.490)	(0.499)	(0.498)	(0.500)	(0.494)
Observations	169,937	71,465	98,472	34,927	14,396	20,531
Male sample						
Years of schooling	11.566	10.667	12.234	11.315	10.330	12.021
	(2.265)	(1.911)	(2.276)	(2.202)	(1.991)	(2.071)
MSLA 14	0.149	0.351	0.000	0.348	0.834	0.000
	(0.357)	(0.477)	(0.000)	(0.476)	(0.373)	(0.000)
MSLA 15	0.566	0.649	0.505	0.355	0.166	0.491
	(0.496)	(0.477)	(0.500)	(0.479)	(0.373)	(0.500)
MSLA 16	0.284	0.000	0.495	0.297	0.000	0.509
	(0.451)	(0.000)	(0.500)	(0.457)	(0.000)	(0.500)
Ever smoked	0.781	0.857	0.727	0.676	0.733	0.636
	(0.413)	(0.350)	(0.446)	(0.468)	(0.442)	(0.481)
Current smoker	0.326	0.314	0.336	0.335	0.325	0.343
	(0.469)	(0.464)	(0.472)	(0.472)	(0.468)	(0.475)
Quit smoking	0.581	0.634	0.535	0.502	0.557	0.456
	(0.493)	(0.482)	(0.499)	(0.500)	(0.497)	(0.498)
Observations	79,271	33,787	45,484	15,298	6,385	8,913

Table 1. Summary statistics, Great Britain and Northern Ireland (concluded).

	Great Britain			Northern Ireland		
	(1)	(2)	(3)	(4)	(5)	(6)
	1923-1979	1923-1946	1947-1979	1923-1979	1923-1946	1947-1979
Female sample						
Years of schooling	11.572	10.679	12.207	11.556	10.515	12.274
	(2.202)	(1.870)	(2.200)	(2.257)	(2.012)	(2.133)
MSLA 14	0.149	0.359	0.000	0.341	0.835	0.000
	(0.356)	(0.480)	(0.000)	(0.474)	(0.371)	(0.000)
MSLA 15	0.552	0.641	0.488	0.344	0.165	0.467
	(0.497)	(0.480)	(0.500)	(0.475)	(0.371)	(0.499)
MSLA 16	0.299	0.000	0.512	0.315	0.000	0.533
	(0.458)	(0.000)	(0.500)	(0.465)	(0.000)	(0.499)
Ever smoked	0.658	0.697	0.631	0.535	0.492	0.564
	(0.474)	(0.460)	(0.483)	(0.499)	(0.500)	(0.496)
Current smoker	0.305	0.305	0.304	0.315	0.275	0.342
	(0.460)	(0.461)	(0.460)	(0.464)	(0.447)	(0.474)
Quit smoking	0.535	0.562	0.514	0.408	0.441	0.388
	(0.499)	(0.496)	(0.500)	(0.492)	(0.497)	(0.487)
Observations	90,666	37,678	52,988	19,629	8,011	11,618

Table 2. The first-stage results: The effect of the minimum school leaving age law changes on years of schooling, Great Britain and Northern Ireland.

	Great Britain			Northern Ireland		
	(1)	(2)	(3)	(4)	(5)	(6)
	1921-1981	1921-1946	1947-1981	1921-1981	1921-1946	1947-1981
Total sample						
MSLA = 15 (v. 14)	0.518***	0.672***		0.615***	0.804***	
	[0.029]	[0.043]		[0.065]	[0.138]	
MSLA = 16 (v. 14)	0.572***		0.345***	0.725***		0.222***
	[0.038]		[0.041]	[0.096]		[0.080]
F-statistic	168.31§	247.63§	70.57§	44.74§	34.02§	7.69§§§
Observations	169,937	71,465	98,472	34,927	14,396	20,531
Male sample						
MSLA = 15 (v. 14)	0.463***	0.659***		0.636***	0.942***	
	[0.043]	[0.061]		[0.097]	[0.207]	
MSLA = 16 (v. 14)	0.477***		0.351***	0.809***		0.360***
	[0.056]		[0.061]	[0.143]		[0.122]
F-statistic	61.22§	115.64§	32.74§	21.91§	20.77§	8.76§§§
Observations	79,271	33,787	45,484	15,298	6,385	8,913
Female sample						
MSLA = 15 (v. 14)	0.567***	0.685***		0.598***	0.698***	
	[0.040]	[0.059]		[0.088]	[0.185]	
MSLA = 16 (v. 14)	0.656***		0.330***	0.663***		0.123
	[0.051]		[0.054]	[0.130]		[0.110]
F-statistic	109.95§	132.75§	37.26§	23.39§	14.23§§	1.24
Observations	90,666	37,678	52,988	19,629	8,011	11,618

Notes: Control variables include quartic polynomial in year-of-birth and age at time of survey, and a dummy variable for gender. Huber-White's robust standard errors from clustering by region and birth cohort are in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%. Test of IV strength is above Stock and Yogo (2005) critical values: § = 10%; §§ = 15%; § §§ = 20%.

Table 3. OLS and IV estimates of the effect of years of schooling on ever smoking by birth cohort, Great Britain.

	(1) 1923-1981	(2) 1923-1946	(3) 1947-1981
Total sample			
Schooling (OLS)	-0.017***	-0.010***	-0.021***
	[0.001]	[0.001]	[0.001]
Schooling (IV)	-0.016	-0.009	-0.009
	[0.012]	[0.015]	[0.026]
Observations	169,937	71,465	98,472
Male sample			
Schooling (OLS)	-0.015***	-0.011***	-0.017***
	[0.001]	[0.001]	[0.001]
Schooling (IV)	0.011	-0.012	-0.010
	[0.017]	[0.018]	[0.036]
Observations	79,271	33,787	45,484
Female sample			
Schooling (OLS)	-0.019***	-0.008***	-0.024***
	[0.001]	[0.001]	[0.001]
Schooling (IV)	-0.035**	-0.007	-0.010
	[0.017]	[0.022]	[0.038]
Observations	90,666	37,678	52,988

Notes: Control variables include quartic polynomial in year-of-birth and age at time of survey, and a dummy variable for gender. Huber-White's robust standard errors from clustering by region and birth cohort are in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

Table 4. OLS and IV estimates of the effect of years of schooling on ever smoking by birth cohort, Northern Ireland.

	Full Sample			Discontinuity Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
	1921-1981	1921-1946	1947-1981	1921-1981	1921-1946	1947-1981
Total sample						
Schooling (OLS)	-0.025***	-0.013***	-0.033***	-0.030***	-0.019***	-0.041***
	[0.001]	[0.002]	[0.002]	[0.002]	[0.003]	[0.003]
Schooling (IV)	0.034	-0.019	-0.127	0.005	-0.010	-0.155
	[0.025]	[0.039]	[0.091]	[0.025]	[0.045]	[0.117]
Observations	34,927	14,396	20,531	10,190	4,570	5,620
Male sample						
Schooling (OLS)	-0.021***	-0.016***	-0.024***	-0.028***	-0.022***	-0.034***
	[0.002]	[0.003]	[0.003]	[0.004]	[0.005]	[0.005]
Schooling (IV)	-0.015	-0.104**	-0.097	-0.019	-0.109*	-0.076
	[0.033]	[0.051]	[0.085]	[0.035]	[0.065]	[0.084]
Observations	15,298	6,385	8,913	4,517	2,032	2,485
Female sample						
Schooling (OLS)	-0.029***	-0.011***	-0.040***	-0.032***	-0.016***	-0.047***
	[0.002]	[0.003]	[0.002]	[0.003]	[0.005]	[0.004]
Schooling (IV)	0.004	0.070	-0.197	0.025	0.077	-0.553
	[0.038]	[0.067]	[0.254]	[0.035]	[0.072]	[1.106]
Observations	19,629	8,011	11,618	5,673	2,538	3,135

Notes: For the full sample, control variables include quartic polynomial in year-of-birth and age at time of survey, and a dummy variable for gender. For the discontinuity sample, control variables include a linear variable in year of birth, age at time of survey, and a dummy variable for gender. Huber-White's robust standard errors from clustering by region and birth cohort are in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

Table 5. OLS and IV estimates of the effect of years of schooling on current smoking by birth cohort, Great Britain.

	(1) 1923-1981	(2) 1923-1946	(3) 1947-1981
Total sample			
Schooling (OLS)	-0.037***	-0.032***	-0.039***
	[0.000]	[0.001]	[0.001]
Schooling (IV)	-0.010	-0.009	-0.010
	[0.014]	[0.016]	[0.026]
Observations	169,937	71,465	98,472
Male sample			
Schooling (OLS)	-0.034***	-0.030***	-0.036***
	[0.001]	[0.001]	[0.001]
Schooling (IV)	0.012	-0.008	-0.007
	[0.023]	[0.024]	[0.038]
Observations	79,271	33,787	45,484
Female sample			
Schooling (OLS)	-0.039***	-0.034***	-0.042***
	[0.001]	[0.001]	[0.001]
Schooling (IV)	-0.023	-0.011	-0.015
	[0.017]	[0.022]	[0.035]
Observations	90,666	37,678	52,988

Notes: Control variables include quartic polynomial in year-of-birth and age at time of survey, and a dummy variable for gender. Huber-White's robust standard errors from clustering by region and birth cohort are in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

Table 6. OLS and IV estimates of the effect of years of schooling on current smoking by birth cohort, Northern Ireland.

	Full Sample			Discontinuity Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
	1921-1981	1921-1946	1947-1981	1921-1981	1921-1946	1947-1981
Total sample						
Schooling (OLS)	-0.040***	-0.031***	-0.046***	-0.045***	-0.036***	-0.054***
	[0.001]	[0.002]	[0.001]	[0.002]	[0.003]	[0.003]
Schooling (IV)	-0.018	-0.076**	-0.115	-0.039*	-0.076*	-0.036
	[0.024]	[0.037]	[0.086]	[0.023]	[0.044]	[0.102]
Observations	34,927	14,396	20,531	10,190	4,570	5,620
Male sample						
Schooling (OLS)	-0.036***	-0.033***	-0.038***	-0.044***	-0.039***	-0.048***
	[0.002]	[0.003]	[0.002]	[0.003]	[0.004]	[0.004]
Schooling (IV)	-0.090**	-0.117**	-0.079	-0.079**	-0.148**	0.002
	[0.035]	[0.050]	[0.080]	[0.036]	[0.068]	[0.082]
Observations	15,298	6,385	8,913	4,517	2,032	2,485
Female sample						
Schooling (OLS)	-0.043***	-0.029***	-0.052***	-0.046***	-0.034***	-0.058***
	[0.001]	[0.002]	[0.002]	[0.003]	[0.004]	[0.003]
Schooling (IV)	0.041	-0.038	-0.187	-0.007	-0.014	-0.209
	[0.034]	[0.056]	[0.237]	[0.031]	[0.060]	[0.539]
Observations	19,629	8,011	11,618	5,673	2,538	3,135

Notes: For the full sample, control variables include quartic polynomial in year-of-birth and age at time of survey, and a dummy variable for gender. For the discontinuity sample, control variables include a linear variable in year of birth, age at time of survey, and a dummy variable for gender. Huber-White's robust standard errors from clustering by region and birth cohort are in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

Table 7. OLS and IV estimates of the effect of years of schooling on smoking cessation by birth cohort, Great Britain.

	(1) 1923-1981	(2) 1923-1946	(3) 1947-1981
Total sample			
Schooling (OLS)	0.043***	0.038***	0.046***
	[0.001]	[0.001]	[0.001]
Schooling (IV)	0.009	0.011	0.007
	[0.014]	[0.019]	[0.029]
Observations	121,192	55,188	66,004
Male sample			
Schooling (OLS)	0.038***	0.032***	0.041***
	[0.001]	[0.001]	[0.001]
Schooling (IV)	0.000	0.006	0.002
	[0.021]	[0.026]	[0.042]
Observations	61,775	28,927	32,848
Female sample			
Schooling (OLS)	0.049***	0.045***	0.052***
	[0.001]	[0.002]	[0.001]
Schooling (IV)	0.014	0.016	0.012
	[0.019]	[0.029]	[0.041]
Observations	59,417	26,261	33,156

Notes: Control variables include quartic polynomial in year-of-birth and age at time of survey, and a dummy variable for gender. Huber-White's robust standard errors from clustering by region and birth cohort are in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

Table 8. OLS and IV estimates of the effect of years of schooling on smoking cessation by birth cohort, Northern Ireland.

	Full Sample			Discontinuity Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
	1921-1981	1921-1946	1947-1981	1921-1981	1921-1946	1947-1981
Total sample						
Schooling (OLS)	0.047***	0.043***	0.051***	0.052***	0.046***	0.059***
	[0.002]	[0.003]	[0.002]	[0.003]	[0.004]	[0.005]
Schooling (IV)	0.062*	0.125**	0.027	0.071**	0.155**	-0.062
	[0.034]	[0.055]	[0.061]	[0.033]	[0.079]	[0.091]
Observations	20,739	8,629	12,110	6,114	2,780	3,334
Male sample						
Schooling (OLS)	0.042***	0.040***	0.043***	0.049***	0.044***	0.054***
	[0.002]	[0.004]	[0.003]	[0.005]	[0.006]	[0.007]
Schooling (IV)	0.124**	0.100*	0.023	0.104**	0.158*	-0.078
	[0.049]	[0.056]	[0.089]	[0.044]	[0.088]	[0.106]
Observations	10,304	4,682	5,622	3,014	1,470	1,544
Female sample						
Schooling (OLS)	0.053***	0.046***	0.058***	0.056***	0.048***	0.064***
	[0.002]	[0.004]	[0.003]	[0.004]	[0.006]	[0.006]
Schooling (IV)	-0.005	0.183	0.028	0.036	0.153	-0.039
	[0.050]	[0.131]	[0.084]	[0.050]	[0.151]	[0.164]
Observations	10,435	3,947	6,488	3,100	1,310	1,790

Notes: For the full sample, control variables include quartic polynomial in year-of-birth and age at time of survey, and a dummy variable for gender. For the discontinuity sample, control variables include a linear variable in year of birth, age at time of survey, and a dummy variable for gender. Huber-White's robust standard errors from clustering by region and birth cohort are in brackets. * denotes statistical significant at 10%; ** denotes statistical significant at 5%; *** denotes statistical significant at 1%.

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