The Effects of Compulsory Schooling on Health and Hospitalizations over the Life-Cycle

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Abstract

We leverage both survey data and administrative health records to estimate the causal effects of education on health. We specifically exploit the 1972 compulsory schooling reform in Britain that resulted in a large part of the population receiving an additional year of high school education. While survey data provide little evidence of an education-health gradient, our administrative records suggest that reform led to significantly lower hospitalization rates for men. These benefits primarily stem from improvements in digestive health, cardiovascular health, and lower rates of alcohol abuse. Consistent with theoretical predictions that the greatest health productivity gains takes place early in the life-cycle, we document that - while heterogeneous across conditions - the largest education-induced health improvements occur at a younger age and persist into mid-life.

Keywords: Health; Education; Compulsory Schooling; Life Cycle; Gender Differences;
JEL: I10; I12; I14; I20

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1 Introduction

A prominent theoretical prediction in the health economics literature involves the positive association between education and health. As shown in the Grossman (1972) model of the demand for health, a considerable empirical literature finds that an increase in schooling is associated with better health and other quality-of-life improvements. This literature also notes some of the identification challenges in estimating the causal effect of education on health due to the presence of unobservable factors (e.g., time preferences) and potential simultaneity bias – with no clear consensus within this body of work even when employing nearly identical identification strategies and even when assessing the effects within the same country (Galama et al., 2018; Grossman, 2015). For example, several studies exploit changes in compulsory schooling laws such as the twentieth century raising of school leaving age (ROSLA) reforms within the United Kingdom (UK) (Oreopoulos, 2006; Clark and Royer, 2013; Barcellos et al., 2018; Davies et al., 2018). Evidence from these studies suggests that these reforms led to almost half of the population receiving an additional year of education, but come to different conclusions regarding the causal effects of this increase in schooling on health. Clark and Royer (2013) find no evidence that the UK ROSLA reforms led to any significant changes in self-reported health for a large sample drawn from two English survey data sets. This stands in contrast to a more recent study by Davies et al. (2018) who find that education reduces mortality and the probability of reporting a diabetes diagnosis. They also use mostly self-reported data but on a very large volunteer sample of more than 500,000 individuals who provide information on whether they had ever been diagnosed with a variety of conditions. Barcellos et al. (2018) and **?** (2022) use the same data source, the UK Biobank project, and find that education has heterogeneous effects on obesity. That is, the least healthy individuals and those with a high genetic predisposition towards obesity experience the largest health benefits from an additional year of schooling. These studies are also notable in that they are the first to leverage a set of objective measures of health obtained by health professionals on participants' body size, blood pressure, and lung function.

Our paper reconciles at times conflicting empirical results from this literature. We exploit exogenous variation from similar UK ROSLA reforms, generally studied in England, in Scotland. As part of the UK, these reforms also applied to Scottish schools by raising the minimum school leaving age from 14 to 15 in April 1947 and from 15 to 16 in September 1972. We implement a regression discontinuity (RD) design based on these reforms as these empirical designs produce estimates close to the population average (Oreopoulos, 2006), avoid the pitfalls of weak instruments (Bound et al., 1995), and rests on the credible identifying assumption that the ROSLA reforms serve as the sole event driving variation in educational attainment for individuals born only one month apart. Using this design, we find that these reforms increased educational attainment among the affected cohorts by a magnitude that is strikingly similar to well-documented first-stage estimates from studies based on the same laws in England and Northern Ireland. Because the 1947 ROSLA reform was accompanied with several other changes to the education system, such as the construction of new schools and hiring of new teachers, and because mortality in the affected cohorts results in much smaller sample sizes, we focus our analysis on cohorts who were born around the threshold date of the 1972 ROSLA reform.[1]

Scotland is also uniquely suited for studying the education-health gradient as they track a 5.3% (semi-)randomly selected, nationally representative population sample that can be linked to administrative data on a variety of health outcomes – including information within their cancer registry, health care utilization records, and the Scottish Drug Misuse database. These data allow for us to construct comprehensive, nationally representative and reliable health measures for over 100,000 men and women. We also obtain nationally representative survey data in order to compare evidence from official health records to more subjective self-reported health measures. Based on these data, we show that an increase in years of schooling reduces the number of hospital episodes and stays. These reductions occur primarily for inpatient episodes involving diseases related to the digestive system, gastrointestinal

[1]For completeness, we report all results pertaining to the 1947 reform in the Appendix of this article.

tract, and the cardio-vascular system. Moreover, education reduces the probability of a pre or post-mortem cancer diagnosis. We also document large gender differences with the effects of schooling on health primarily concentrated among men. We use the unique panel structure of our data to show that most health benefits occur early on in their life-cycle, which is consistent with recent theoretical work predicting that education leads to large initial efficiency gains in the production of health and a convergence in health stocks later in life. Consequently, we find no evidence for a significant relationship between education and mortality, suggesting that the health benefits we document are unlikely to suffer from survivorship bias. While a socio-economic channel is plausible, our results also support to a behavioral explanation for education-induced health improvements given that we document significant reductions in harmful alcohol consumption, alcohol poisoning, and drug abuse among men who were affected by the compulsory schooling reform.

In contrast to our evidence based on objective health measures, we find no evidence of an effect of the ROSLA reforms on self-reported health behaviors (e.g., smoking and drinking) or illnesses. Similar divergences in empirical findings based on estimation involving subjective and objective health measures are well-documented in the economics literature (Bound 1991; Mackenbach et al. 1996; Bound et al. 1999). Subjective measures often remain susceptible to both measurement error and non-random differences in responses that challenge the interpretation of the empirical estimate of interest. An important case in point in our context is a study of hypertension in the UK by Johnston et al. (2009). They find that the percentage of survey participants who self-report having hypertension is 7% when objectively measured 35% of respondents suffered from the illness. Because this misreporting is systematic, a large and statistically significant income/health gradient disappears when self-reported rather than measured health data are used - even after controlling for an extensive set of control variables. It is conceivable that previous studies of the education/health gradient - the focus of our study - may have been subject to similar issues and consequently have failed to pick up an actual relationship between schooling and health. Moreover, responses

to survey questions will be largely driven by differences in interactions with the health care system at various points in the life cycle.

Our study contributes to the existing literature on education and health in several important ways. It is among the first to analyze the causal relationship between schooling and health using detailed administrative data on health care utilization. We are also in the unique position to explore this relationship over individuals' life-cycles. Our comparison of subjective and objective health information reconciles strands in the literature which produce conflicting results. This is important because analyses of the education-health gradient that use school entry policies for identification have received immense attention, not least because the correlation between education and health, by way of comparison, dwarfs the effects of income or race. The issue has been studied in Denmark (Arendt, 2008), France (Albouy and Lequien, 2009), the Netherlands (Van Kippersluis et al., 2011), Romania (Malamud et al., 2021), Sweden (**?**), the UK (Barcellos et al. 2018; Clark and Royer 2013; Davies et al. 2018) and the United States (Lleras-Muney 2005; Mazumder 2008) with contradictory findings.[2] Our findings are also of interest to policy makers, especially in the UK where the school leaving age has been increased to 17 in 2013 and 18 in 2015.

Our paper is organized as follows. Section 2 examines theoretical evidence on the relationship between education and health over the life cycle. Section 3 provides an overview of the institutional setting in Scotland and the nature of the ROSLA reforms in Scottish schools. In this section, we also outline our identification strategy. In Section 4, we introduce our data and discuss how these data differ from other sources used in this literature. We present our results in Section 5 along with robustness checks. Section 6 discusses the channels through which education affects health while Section 7 concludes.

[2]Furthermore, McCrary and Royer (2011) show that education has little bearing on fertility and infant health in the US, whereas Black et al. (2008) find reductions in teenage motherhood, and Wilson (2017) documents that compulsory schooling reform-induced increases in education led to postponements of births in the UK. There is also a growing literature on the effects of college education on health (see Buckles et al. (2016)).

2 Health and Education over the Life Cycle

In this section we provide some background on the theoretical underpinnings of the relationship between education and health, in order to derive hypotheses and predictions that are then explored in our empirical analysis. Building on Grossman's (1972) original model of the demand for health, a theoretical literature on the relationship between education and health has emerged. Recent examples of this literature address the extent to which this relationship evolves over the life cycle (Galama and Van Kippersluis, 2019; Kaestner et al., 2020). Although not explicitly focusing on education, Galama and Van Kippersluis (2019) construct a model acknowledging the dynamic complementarities between skill and health capital over the life cycle. Extending the Grossman (1972) demand for health model, Kaestner et al. (2020) argue that education affects health over the life cycle through both changes in the productivity and quantity of health investment in the presence of depreciation. Noting that individuals within the Grossman framework make optimal health investment such that the marginal utility of this investment (including utility from higher income) equals the marginal cost, Kaestner et al. (2020) derive an expression for the cumulative effect of education (E) on health (H) as follows:

$$\frac{\partial H_t}{\partial E} = \prod_{k=18}^{t-2} [(\frac{\partial \alpha_k}{\partial E} I_k + \alpha_k \frac{\partial I_k}{\partial E}) \prod_{j=k+1}^{t-1} (1 - \delta_j] + \frac{\partial \alpha_{t-1}}{\partial E} I_{t-1} + \frac{\partial I_{t-1}}{\partial E} \alpha_{t-1} \tag{1}$$

Here α_k denotes the effect of investment I_k on health stock which depreciates at rate δ_j. We assume that education raises the productivity of health care investments and, within the context of the ROSLA reforms discussed in this paper, will generate different predictions regarding these effects across cohorts. One key prediction of this model, for which Kaestner et al. (2020) provide empirical evidence, involves a convergence in health stock at later ages (e.g., ages 60 and above) with the greatest productivity gains being made within the intermediate age range (e.g., ages 40-50). More specifically, individuals with higher levels

of education initially experience additional efficiency gains from health investment. However, these gains become less pronounced as the multiplicative effect of depreciation becomes more salient. These predictions also remain consistent with other empirical evidence within this literature suggesting that individuals with higher levels of education experience higher allocative efficiency. That is, they potentially process health information more efficiently, manage chronic conditions or diseases in a way that prevents serious hospitalization, practice better behavioral health, or are more likely to adopt new medical technologies or innovations (Lleras-Muney and Lichtenberg, 2002; **?**; Glied and Lleras-Muney, 2008).

The above model can also inform any gender differences in the health returns to education. For example, considerable evidence exists within the health literature establishing that women enjoy longer life expectancy than men although the exact mechanisms driving these differences are still not fully understood (Lawlor et al., 2001; Barford et al., 2006; Beltrán-Sánchez et al., 2015). The extended Grossman model offers some additional insight into these gender differences primarily through through two channels. First, women might possess distinct biological advantages which allow for them to enjoy either higher levels of initial health stock or health stock which depreciates more slowly (Austad, 2006; Cullen et al., 2016). The "female advantage" could also emerge through differential responses to public health interventions or differences in other environmental exposures (Goldin and Lleras-Muney, 2019). These differences, in turn, are likely to translate into gender differences in the education-health gradient.

Second, expanded opportunities for women in the labor market could also lead to differences in the health returns to education for younger and older cohorts–allowing for higher wages and health investment. Altogether, the aforementioned model predicts substantial heterogeneity regarding the effects of ROSLA across cohorts and demographic groups.

3 Empirical Strategy

3.1 Minimum School Leaving Age in Scotland

We test the predictions of our theoretical model and analyze the effect of schooling on health by exploiting exogenous changes in the minimum age at which students are allowed to drop out of high school in Scotland. Although the Scottish school system always possessed some distinguishing features from the English institutional setting, British compulsory schooling laws apply to both nations. Therefore, our empirical strategy can exploit the same 20th century compulsory schooling reforms examined in similar work on England and Wales.

Given the similarity in policies, we only briefly outline the two raising of school leaving age (ROSLA) reforms.[3] The minimum school leaving age in Scotland (as in the rest of Britain) was raised from 14 to 15 in accordance with the 1944 Education Act. The same law was again invoked to raise the school leaving age from 15 to 16 in September 1972. As a result of the first reform, students who were born after 1 April 1933 (and thus less than 14 years before the first change) were required to stay in school for one more year than students born before April 1933. Similarly, students born in September 1957 or after were pushed into more education relative to those born just before the cut-off date.

Figure 1 illustrates the effect of these changes on educational attainment in Scotland. Since children typically start school upon reaching five years of age, the 1947 reform (indicated by the first vertical line) pushed the affected cohorts into obtaining at least 10 years of education. As a result, Figure 1 shows that the percentage of students who obtained nine years of education or less was cut by more than half. In the same vein, the 1972 reform dramatically reduced the number of students leaving high school with ten or fewer years of education. Figure 1 breaks our data down by quarter of birth. Because September is part of the third quarter only one third of the 1957Q3 cohort was affected and explains why the drop in the percentage of individuals who obtain ≤ 10 years of education only fully materializes for the

[3] See Clark and Royer (2013) for a more detailed overview.

1957Q4 cohort.[4] Our first stage results are also strikingly similar to estimates provided by Clark and Royer (2013) and other studies documenting the effects of these reforms in other parts of the UK (see, e.g., Braakmann (2011) and Janke et al. (2020)).

Throughout this paper, we focus our analysis on the 1972 ROSLA reform and report results for the 1947 ROSLA reform in the appendix. We do so for two reasons. First, a focus on the 1972 reform ensures a sufficiently large sample size. As will be described in more detail in Section 4, our data run up until 2016, at which point mortality will have affected the size of cohorts born in the 1920s and 1930s whereas cohorts born in the 1970s are less likely to be subject to non-negligible sample attrition. Second, the earlier ROSLA reform was accompanied by school construction and teacher hiring drives as well as the abolition of school fees for secondary education. For instance, the 1947 ROSLA was accompanied by the HOSLA program ("Hutting Operation for the Raising of the School-Leaving Age") which resulted in the construction of what were often make-shift school buildings. While the majority of any reduced form effect is likely to operate through an increase in the number of schooling years, we cannot rule out that changes in the quality of education may have had an independent effect on health. The 1972 ROSLA reform, on the other hand, had a much longer lead-up time and was by all accounts unaffected by space or staffing constraints.

3.2 Compulsory Schooling and Education

We exploit the natural experiment created by the 1972 compulsory schooling reform using a regression discontinuity design. Intuitively, our approach compares the health outcomes of individuals born in cohorts so that they were just about affected by the reform (i.e. born on or after 1 September 1957) with individuals born in cohorts just before the same cutoff date. Any differences in outcomes are then rescaled by the average increase in the years of schooling to obtain a local average treatment effect (LATE) of education on health. Oreopoulos (2006)

[4]Note that in order to demonstrate that Scotland was subject to the same compulsory schooling reform as the rest of Britain (as also shown by Buscha and Dickson (2018)), we consciously use a very similar design for our tables and figures as Clark and Royer (2013) who document the ROSLA impact on educational attainment for England.

provides a detailed overview of this method and shows that a LATE will closely resemble the population average treatment effect (ATE), thus allowing for generalizations and external validity.

The first key assumption underpinning this identification strategy requires that the reform had a strong effect on educational attainment. Figure 2 establishes that this is indeed the case. Panel B suggests that the 1972 ROSLA increased the average number of years of schooling by slightly less than half a year for those affected by the reform. Much of this appears to be driven by fewer individuals dropping out after receiving only ten years of education (see Panel A). Recall that the 1972 reform increased the minimum schooling age to 16, thus obliging students to stay in school well into 11th grade. Figure 2 suggests that compliance with the law was strong.

Table 1 provides numerical estimates of the jumps and drops in educational attainment as shown in Figure 2. The 1972 reform increased average educational attainment by 0.35 years for men and had a very similar effect for women. Years of education are calculated as the age at which a person left full-time education minus five. All estimates are obtained from local polynomial regression discontinuity (RD) estimation with automated bandwidth selection and bias-corrected inference (Calonico et al., 2014). The following equation outlines all elements of this estimation:

$$Educ_{ict} = \alpha_0 + \alpha_1 D_{ic} + f(Run_{ic}) + \alpha_2 \mathbf{X}_{ict} + \varepsilon_{ict} \tag{2}$$

where $Educ_{ict}$ is a measure of educational attainment and D_{ic} is an indicator for whether a person was born on or after the cutoff-date. The running variable, Run_{ic}, is birth month-year and is centered around each of the respective cut-offs. The vector of covariates $\mathbf{X}_{ict}$ includes flexible controls for age, sex, birth month and survey year.

The resulting first stage estimates provided in Table 1 confirms that increases in educational attainment are driven by individuals who stay in school for just one additional year. The 1972 change prevented students from dropping out after ten years of formal education

and thus decreased the odds of dropping out by about 27 percentage points. However, the reform had no effect in terms of completing more than 11 years of education and no statistically significant effect on the probability of leaving school with less than 13 years of education. In other words, the reform prompted students to stay in school until they had completed 11th grade but not beyond. These findings suggest that most individuals who were affected by the reform (i.e., "compliers") would have dropped out of school otherwise. Our findings also confirm the strength of our first stage. By way of comparison, both average levels of education, and first-stage effects are also very similar to those in Clark and Royer (2013), thus indicating that the reform was implemented in a similar fashion in both England and the rest of the UK.[5] For completeness, Appendix Figure A1 and Table A2 show the corresponding estimates for the 1947 ROSLA which again closely resemble the first stage estimates documented by the previous literature.

3.3 Compulsory Schooling and Health

In addition to a strong first stage, our methodological approach rests on the assumption that individuals are unable to sort onto either side of the reform cutoffs. Because the changes in ROSLA were imposed on students who were already in school, this assumption is likely to be met. The retrospective implementation of the reforms also eliminates the possibility that parents' fertility choices or selective deferral of school starting dates might violate the exogeneity assumption. Our covariate balancing test of pre-determined characteristics provides further support for this notion. For example, Appendix Figure A3 shows that there are no changes around the threshold dates in the percentage of individuals who were raised Protestant.[6] Balancing tests on ethnicity and gender composition also show no evidence of discontinuities around the ROSLA cutoff dates.

[5]The UK Biobank project, another frequently used data source, on the other hand, tends to over-sample more educated participants, thus resulting in a different first stage.

[6]Past (and to some extent contemporary) ethnic conflict in Scotland proceeded primarily along religious lines in which Protestants of the Church of Scotland are the majority and Roman-Catholics, often of Irish descent, are a sizeable minority.

Exogeneity is a precondition for obtaining consistent reduced-form effect of the compulsory schooling reform on our health outcomes of interest. The following equation outlines our approach for estimating these effects:

$$H_{ict} = \beta_0 + \beta_1 D_{ic} + f(Run_{ic}) + \beta_2 \mathbf{X}_{ict} + \eta_{ict} \tag{3}$$

where H_{ict} is a vector of our subjective and objective measures of health (e.g., self-reported health status or the number of inpatient hospital episodes) for each individual i in month-year of birth cohort c at time t. All other variables have the same interpretation as in equation (2). The effect of an additional year of schooling – as induced by the ROSLA reform – on our outcomes of interest is then typically estimated using a "fuzzy" regression discontinuity design (RDD), which, in turn, is operationalized by two stage least squares (2SLS) where equation (2) constitutes the first stage and the second stage is given by the following equation:

$$H_{ict} = \delta_0 + \delta_1 \widehat{Educ_{ic}} + f(Run_{ic}) + \delta_2 \mathbf{X}_{ict} + \upsilon_{ict} \tag{4}$$

where $\widehat{Educ_{ic}}$ are the predicted values from our first stage. Intuitively, any second stage result is equivalent to dividing the main effect from equation (3) by the corresponding first stage estimate. By re-scaling the reduced-form effect of the ROSLA reform on health by the proportion of individuals who were pushed into an additional year of education due to the same reform, we obtain an estimate of the effect of an additional year of schooling on our health outcomes of interest.[7] Section 4 will show that we draw on data from different sources – all of which contain birth dates but some of which are missing individuals' years of schooling. In these cases, we can nonetheless obtain estimates of the effect of an additional year of education on health by re-scaling the reduced form effect by our nationally representative first-stage estimates.

[7] In fact, Abraham Wald (1940) famously showed that with a binary treatment variable and no control variables, δ_1 will be exactly equal to β_1/α_1.

4 Data

4.1 Scottish Longitudinal Study (SLS)

Our main data source is the Scottish Longitudinal Study (SLS) which links census data to administrative records. The census in Scotland takes place every ten years and uses the same methodology developed by the Office for National Statistics (ONS) for the rest of the United Kingdom. The last census took place in 2011 and collected information on education, ethnic identity, religious identity, and housing for the entire Scottish population.

In order to create a longitudinal dimension for a representative part of the population, the SLS was set up in 2006 and links a sub-sample of individuals across the 1991, 2001, and 2011 census waves. These individuals were selected by (semi-)randomly picking 20 of 366 possible birthdays. Any census participant born on one of these dates is automatically included in the SLS–yielding a 5.3% sample (approximately 270,000 individuals) of the entire Scottish population.

For our project, detailed information on each participant's health and National Health Service (NHS) utilization have been matched to the SLS sample. In particular, we have information on all inpatient hospital admissions and discharges since 1981 including the type of admission. For each admission we also have information on the number of episodes. For instance, a patient might be admitted as part of a consulting episode and then transferred into a surgical unit, thus triggering a second episode within the same spell. The duration of each hospitalization episode in days is also recorded. For each admission, our data set contains information on the main and secondary diagnoses as indicated by International Classification of Diseases (ICD) codes. Most codes follow the 10th ICD revision (ICD-10). Cases in which the older 9th revision (ICD-9) was still used were converted to ICD-10. As we are primarily analyzing disease categories rather than specific conditions, this conversion

is straightforward.[8]

We use these ICD codes to categorize hospital episodes by disease and injury type – including diseases of the circulatory system (ICD-codes I00-I99), respiratory system (J00-J99), digestive system (K00-K93), several sub-categories such as heart disease (I00-I52), and liver disease (K70-K79). Figure 5 contains a full breakdown of categories. Our SLS participants are also linked to the Scottish Cancer Registry in which all cancer diagnoses (including post-mortem) are recorded. In addition, we separately analyze hospitalizations related to risky health behavior such as episodes related to alcohol poisoning (ICD-10 codes: T51, X45, X65, and Y15), intoxication and harmful use (ICD-10 codes: F10.0 and F10.1), alcohol dependency and withdrawal (ICD-10 codes: F10.2 to F10.9), and drug abuse related episodes (ICD-10 codes: T40 and T43.6; F11-F19).

The SLS data are unique in several ways. First, our nationally representative sample avoids many of the self-selection concerns present in other studies based on volunteers. Second, we are able to work with objective health measures as per the diagnosis of health care professionals. Such measures are arguably more reliable than commonly used subjective, self-reported health indicators. Third, the data allow for us to track the same individual over her life cycle and serve a considerable improvement upon the cross-sectional snapshots seen elsewhere in the literature.

The SLS also takes several precautions with respect to selective sample attrition and retention. For example, immigration could present important challenges to our study design. Individuals who emigrated to Scotland later in life, but by virtue of their birth-year and -month would have been affected by the ROSLA reforms, could falsely be classified as "treated." Fortunately, the SLS is regularly cross-checked with NHS registrations. Since all residents are required, and possess a strong incentive to register with the NHS upon relocating in order to receive free universal health care, we can reliably determine if/when a person arrived in the UK. In other words, left-censoring is not an issue and throughout the study we

[8]See Appendix B for additional details.

limit our sample to individuals who were either born in the UK or arrived in the UK before turning 14 years old. Similarly, selective emigration could also lead to important empirical hurdles. However, emigrants are required to notify the NHS when they move abroad and these cases will be accounted for in the SLS data. Rare cases of emigration without notification can be detected by virtue of the census. Dedicated SLS staff look into the whereabouts of all participants who show up in one census wave but not the next. Deaths of individuals who were born on one of the 20 semi-randomly selected birthdays are also transmitted to the SLS by mortality registries. Sections 5.4.1 and 5.4.2 of this paper analytically investigate these issues and finds no evidence for selective mortality or migration.

In addition to basic demographic characteristics typically surveyed in a census (e.g., age, sex, ethnicity, occupation, and post-code level of deprivation), the SLS also contains information on each participant's highest educational qualification such as O-Grades (equivalent to English O-Levels) or a university degree. However, these data do not contain information on the exact number of years of schooling, which motivates why we supplement them with a second source.

4.2 Scottish Health Survey (SHeS)

Our second data source is the Scottish Health Survey (SHeS). The SHeS is the Scottish equivalent to the Health Survey of England (HSE) which has been widely used in the previous literature. Although life expectancy in Scotland is about two years lower than in England, health profiles in Scotland are only marginally worse than in England and broadly comparable (Bromley and Shelton, 2010). The SHeS is a nationally representative health survey and is primarily based on a personal interview. The SHeS asks respondents to self-assess their health on a 5 point-scale ranging from "very good" to "very bad." We group the bottom three categories of "fair", "bad", and "very bad" health into a single indicator of "poor health." Survey participants are also asked to indicate whether they suffer from any long-standing illnesses which are coded up as a dummy variable. In addition, respondents

report whether they regularly drink alcohol, currently smoke, or smoked in the past.

Crucially, the SHeS also contains information about the age at which a person left full-time education. This information allows us to calculate the number of years of schooling for each respondent. The before-mentioned Figures 1 and 2 pooled data from the 1995, 1998, 2003, 2008-2011, and 2012-2016 waves. In other words, our first-stage analysis is based on SHeS data whereas large parts of our reduced form analysis will draw on SLS data. Since both datasets are nationally representative, we can approximate the LATEs by re-scaling our reduced form estimates by our first-stage results.

To illustrate the representativeness of both SHeS and SLS, we compare the one common education measure contained within both the SLS and SHeS describing whether a participant left full-time education without any qualification. Appendix Figure A2 demonstrates that in both the SLS and SHeS data the 1972 ROSLA reduced the percentage of the population that leaves school without any formal educational qualification by about four percentage points.[9] This is comforting as it suggests comparability across both data sets.

5 Results

5.1 Education and Self-Reported Health

As a first step, we pool our SHeS data from all waves to investigate the relationship between years of schooling and health as (self-)reported in the survey. Table 2 shows the well-known positive correlation between education and health. A simple OLS regression suggests that an additional year of schooling reduces the probabilities of reporting poor health by 3.9 percentage points and the prevalence of long-standing illnesses by about 2.1 percentage points.

[9]The same Figure shows that the 1947 ROSLA reform, on the other hand, had no statistically significant effect. This difference is not surprising as the 1947 reform merely pushed most students into a 10th year of formal education, but provided less of an incentive to stay for a 11th year at the end of which one can obtain a high school degree.

We re-run the specification with dummy indicators for whether an individual obtained more than 11 years of education and again find large and statistically significant effects. There is also a negative and highly statistically significant relationship between education and both present and past smoking habits. We also find a positive relationship between alcohol consumption and education as shown elsewhere in the literature.[10]

While all of our OLS estimates are statistically and economically significant, they are arguably biased due to many of the endogeneity issues mentioned earlier. In order to determine whether a causal relationship exists between schooling and self-reported health, we therefore conduct a reduced form analysis of the effect of the ROSLA reform as outlined in equation (3). Intuitively, this analysis investigates whether the large increase in schooling that was induced by the compulsory schooling reform was accompanied by improvements in health for the affected cohorts. The results of Table 2 suggest that this was not the case. With the exception of alcohol drinking behavior, none of our reduced form coefficients are statistically significant at the five percent level. Indeed, some of our results suggest a negative association between education and health. For instance, the results in column (1) of Table 2 suggest that the 1972 ROSLA increased the incidence of poor health by 0.7 percentage points although we cannot convincingly reject the null hypothesis of no effect. All coefficients in the Table are also fairly precisely estimated so that we can all but rule out large undetected beneficial effects of schooling on health.

Figure 3 also gives no indication of either a clear-cut jump or a drop in the incidence of poor health and long-standing illness around the cut-off dates. As such, our results confirm the findings of the prominent study by Clark and Royer (2013) and other work concluding that the observed positive relationship between education and health turns out to be a spurious correlation when estimated based on plausibly exogenous sources of variation. However, we will argue in the next section that these findings are specific to self-reported health measures obtained from cross-sectional data while estimates based on more objective

[10]For an overview of this literature, see Huerta and Borgonovi (2010) who argue that in the UK education promotes social positions, occupations, and circumstances in which alcohol consumption is more widespread.

health measures over time reveal a positive causal relationship as predicted by our theoretical model introduced in Section 2.

5.2 Education and Objective Health

5.2.1 Inpatient Hospitalizations

In Table 3, we present our results for inpatient hospitalizations for the time period from 1981 to 2016 and broken down by sex. Panel A shows male cohorts that were born in or just after September 1957 experienced, on average, about 1.6 fewer inpatient hospital episodes than cohorts born just before the cutoff date. Our point estimate implies about a 0.16 standard deviation drop in inpatient episodes and the effect is statistically significant at the five percent level. We find a similarly sized effect for the number of hospital stays and a slightly smaller effect on the number of days in inpatient care. For the corresponding female cohorts, our results are also suggestive of small improvements in health although the standard errors are too large to rule out a null effect.

It is worth reiterating that the coefficients presented in Table 3 are reduced form coefficients. Given that both previous studies and the results of Table 1 indicate that the 1972 ROSLA reform increased the years of schooling by about 0.35 years, our results indicate that an additional year of education reduces the number of inpatient episodes by almost half a standard deviation which represents a large, economically significant reduction. Our effect sizes are not directly comparable to most of the existing literature, as our outcome here are hospitalizations over a long period rather than a continuous or categorical cross-sectional health measure. A deterioration in health typically does not manifest itself in a single hospitalization. Rather, one hospitalization episode tends to lead to another, either within the same stay or as part of a new spell, thus right-skewing our outcome measure. As such the estimated effect size is plausible even if there is no literature to directly compare it to.[11]

[11] An exception is the study by **?** who, among other things, consider the effect of a Swedish compulsory schooling reform on the number of hospital days over a 27-year period. While their outcome mean is similar to ours, we cannot compare point estimates as they find no effect.

Also note that all effects are estimated conditional on a person being alive as per the 1991 census date and are evaluated for the full period from 1981 to 2016 for which hospitalization records have been linked.[12]

While our flexible control for birth year-month should account for any differences in age across cohorts, we also explicitly test for age-specific effects of education by limiting our sample to individuals born between September 1951 and August 1963. That is, the youngest person in this sub-sample will be 53 years old in 2016 when we stop observing them and the oldest person in this sub-sample will be 29 years old in 1981 when our inpatient data begin. In other words, we observe hospitalization records between ages 29 and 53 for every person in this sub-sample, allowing us to asses the effect of the ROSLA reform on inpatient admissions for a fixed age range.

Panel B of Table 3 shows that our results are robust to these age-specific sample restrictions. The reform reduced the number of inpatient episodes and stays experienced by men between ages 29 and 53 by about 0.16 standard deviations. The point estimates for women are again smaller and only borderline statistically significant. Taken at face value, these results suggest that the compulsory schooling reform reduced the number of inpatient episodes between ages 29 and 53 by about 0.1 standard deviations for women.[13]

Overall, our analysis of inpatient hospitalizations suggests that education reduces the number of inpatient hospitalization episodes with men in their younger years showing the largest and most significant improvements in health. The graphical evidence in Figure 4 supports our regression results. Panels (a), (c), and (e) show a clear drop for the number of inpatient episodes, stays, and days for men who were just about affected by the 1972 reform. For women of the same cohort, on the other hand, these drops are much less clear and indicative of very small effects at best.

We further break down our inpatient hospitalization data by main diagnosis. We estimate

[12]In section 5.4.1 we will show that potentially selective mortality is not driving these results and if anything works against us finding an effect.

[13]The corresponding results for the 1947 ROSLA cohorts are shown in Appendix Tables A3 and A4 as well as Figures A4 and A5 and are also suggestive of health improvements for men.

the effect of the compulsory schooling reforms on 21 conditions/categories which are listed in Figure 5. The reduced form estimates shown in the forest plot are re-scaled in order to interpret them as effects in standard deviations. All standard errors are robust to Bonferroni multiple hypotheses tests. Figure 5 shows that the reduction in overall inpatient hospitalizations due to the 1972 ROSLA reform are primarily driven by improvements in internal health conditions such as health issues related to the digestive system, intestines, gall, biliary and pancreas, and the stomach as well as issues related to the cardiovascular system, in particular heart disease. The same chart also confirms the substantial gender differences indicated by both the results of Table 3 and as predicted by our theoretical model. None of the 21 estimates for women is negative and statistically significant at the five percent level. Appendix Figures A7 and A8 show the corresponding results for the number of inpatient stays and days. These figures corroborate the above finding that the reduced form effects of the Scottish compulsory schooling reform are concentrated among men.

5.2.2 Cancer Diagnoses

We also analyze our data on cancer diagnoses. Table 4 shows that for this outcome, effects are again concentrated among men who were exposed to the 1972 ROSLA reform. Panel B suggests that the effect on the probability of being diagnosed with any type of cancer is significantly lower for this group. The size of the effect is similar to those for inpatient hospitalization. That is, exposure to the late reform reduced cancer diagnoses by nearly 0.17 standard deviations. We show the corresponding graphical evidence in Appendix Figure A10 which confirms that the reduction in cancer diagnoses is concentrated among men who were affected by the 1972 reform. Given that this cohort is only around 60 years old by the end of our observed sample period, these results are best interpreted as a reduction in the early onset of cancer.

A concern with cancer diagnoses is that education might lead to changes in the compo-

sition of health care inputs. For instance, more educated individuals could demand more cancer screenings and check-ups. However, this behavior would imply a higher rather than lower incidence of cancer diagnoses to the right of the cut-off. Furthermore, the Scottish Cancer Registry also includes post-mortem cancer diagnoses.[14] Of course, cancer diagnoses are arguably more prone to measurement error than hospitalizations so these results are best interpreted as suggestive evidence.

5.2.3 Alcohol and Drug-Related Hospitalizations

Health behaviors, such as alcohol consumption and smoking, are of particular interest in the context of our study as they can help identify the channel through which education affects overall health. Our analysis of survey data (see Table 2) suggested that an increase in educational attainment does not lead to either lower smoking or drinking behavior. In fact, these results suggested that there might be a positive causal effect of education on alcohol consumption, although the SHeS only contains crude measures that fail to distinguish between moderate and excessive alcohol consumption.

Table 5 leverages our hospitalization data in order to closely investigate the nature of this relationship within the context of the 1972 ROSLA reform. We specifically group inpatient hospitalizations due to alcohol poisoning, intoxication and harmful use, or alcohol dependency and withdrawal into a single category–using the number of episodes, stays, and days of inpatient care that are related to this category as our outcome. Our findings suggests that the health benefits of education in the form of lower alcohol abuse rates accrue primarily to men. An additional year of education is associated with approximately a 0.29 standard deviation reduction in the number of inpatient episodes related to excessive alcohol consumption. Again, the differences across gender are striking as we fail to find any such pattern for women. The graphical evidence of Figure 6 corroborates this finding. Panel (a)

[14]Post-mortem cancer diagnoses account for just 1.4% of entries. All results are robust to excluding these detections.

displays a drop in the number of alcohol-related inpatient episodes for men born around the September 1957 cut-off date whereas no such drop can be found for women.

We also analyze the effect of education on inpatient hospitalizations that are indicative of acute drug abuse. However, we note that these hospitalizations are very rare and this subsequently influences the precision of our estimates. With this caveat in mind, columns (5) and (6) document a negative relationship between ROSLA exposure and drug-related hospitalizations for men. While this estimate is not statistically significant, we view this finding as potentially suggestive evidence of a negative effect of education on drug abuse.[15]

5.3 Life-Cycle Effects

We next leverage the inherent panel dimension of the SLS data to explore how education affects health over the life-cycle. The theoretical model we put forward in Section 2 implies that education-induced productivity gains in health production are largest early on in the life-cycle whereas the compounding of health stock depreciation will eliminate differences in health stock in the long run. In order to explore these predictions, we re-code our hospitalization outcomes into age-specific outcomes. That is, rather than estimating the effect of the 1972 ROSLA reform on the total number of hospitalizations over more than two decades, we analyze the reduced form effect on the number of hospitalizations by a particular age (e.g. number of inpatient episodes by age 40). In Figure 7 we then plot the corresponding life-cycle profiles, in particular the standardized point estimates and 95% confidence intervals, separately for men and women.

Panel (a) of Figure 7 presents effects of the 1972 ROSLA reform on the number of inpatient hospitalization episodes. The blue dots represent our estimates for men, red triangles refer to our results for women. It is apparent that the health-enhancing effect of education is largest in men's late 30s and early 40s. Throughout their 40s and early 50s, men who

[15] Given that drug abuse related hospital admissions are very rare, Appendix Figure A11 which shows this relationship graphically had to group men and women together for disclosure reasons.

were affected by the 1972 ROSLA continue to enjoy lower hospitalization rates although this health advantage stops to increase. What is more, the gender differences are again striking with women enjoying substantially smaller health benefits and a flatter life-cycle profile then men.

Life-cycle patterns show some variation across diagnoses. For instance, a comparison of Panel (b) and (c) reveals that the largest effects of education on cardio-vascular health occur to men in their mid to late-30s whereas the largest drops in hospitalizations for conditions related to the digestive system are visible in their mid to late 40s. This pattern is consistent with aggregate NHS data. In 2018/19, for instance, the English National Health Service (NHS) recorded about twice as many hospital admissions due to diseases of the circulatory system for individuals aged 40-44 than for individuals aged 30-34 (NHS, 2019). The corresponding admissions number for diseases of the digestive system show no difference across these age categories, but are 50% higher for individuals aged 50-54 than for those aged 40-44.

In other words, the education-induced reductions in hospitalizations occur at a point in individuals' life-cycles when they tend to first develop the corresponding conditions. This is when a more efficient production of health may have the largest impact. Panel (d) of Figure 7 is another case in point. The figure shows our estimates for the effect of schooling on admissions due to injuries, poisoning, and other external causes. Naturally, these kinds of admissions are less related to investments and depreciation of one's health stock (i.e. productive efficiency improvements) and more related to allocative efficiency. It is thus not surprising that the life-cycle profile in this sub-figure is flat as health stock depreciation is unlikely to yet play an important role for the age range we analyze.[16] At the same time, there is a clear level effect which is consistent with an education-induced improvement in allocative efficiency.

[16] Aggregate hospital admission numbers are all but constant across our age range. In 2018/19, there were 61,399 injury-related admissions for patients aged 30-34, 57,276 for 35-39 year-olds, 52,489 for 40-44 year olds, 62,929 for 45-49 year olds, and 69,053 for 50-54 year olds (NHS, 2019).

5.4 Robustness Checks

5.4.1 Education and Mortality

A potential concern for our analysis involves the possibility of selective mortality affecting our results. For example, the lower number of inpatient episodes among men affected by the 1972 ROSLA relative to women could be due to higher male mortality. This would induce a survivorship bias in that hospitalizations for men are lower because only the healthiest men survive. It may also be that differences in hospitalization outcomes across cohorts are affected by education-induced mortality reductions. Under the assumption that education helps the longevity of the least healthy cohort members, we would underestimate the effects of the compulsory schooling reform on health.

We investigate this issue using population data from the Scottish Census of 1991 as well as population mortality records from National Records of Scotland. Both data sources contain counts by month and year of birth, allowing us to combine them to create an individual-level panel. This allows us to adopt a two-step approach as in Clark and Royer (2013) who in turn adopted it from Sullivan and Von Wachter (2009).

In a first step, we estimate a panel logit model where we model the probability of dying in each month t from 1991 to 2016 (conditional on being alive at the beginning of month t) for individual i of cohort c in month t as a function of a full set of month-year cohort dummies, θ_c as well as age fixed effects δ_a:

$$P(Death_{ict}|\theta_c, \delta_a) = F(\theta_c + \delta_a) \tag{5}$$

We then obtain the coefficients for our month-year cohort dummies which become the outcome for step 2. In this step we use these $\hat{\theta_c}$ as the independent variable in a local linear model of the following form:

$$\hat{\theta_c} = \pi_0 + \pi_1 D_c + f(Run_c) + \gamma_m + \epsilon_c \tag{6}$$

where γ_m is a set of calendar-month fixed effects. Intuitively, our set of $\hat{\theta}_c$ measure the effect of being born in a particular cohort on the log odds of death. Equation (6), in turn, explores whether there is a break in this effect, in particular for cohorts who are born around the time of the schooling reform. Such a discontinuous change in mortality would be picked up by π_1. Our models are estimated using weighted least squares where the weights are commensurate to the inverse of the standard errors in step 1. All standard errors are adjusted for clustering at the cohort level. The results of this analysis are summarized in Figure 8 which reveals that there is no discontinuous jump in the log odds of death by year-month of birth for the cohorts affected by the 1972 reform. The point estimates are small, -0.019 for men and -0.002 for women, and statistically insignificant at any standard level of significance. The sign of these estimates also suggests that any effects on mortality would work in the opposite direction of any survivorship bias in our main health estimates.

5.4.2 Selective Migration

A similar concern involves the potential for selective migration to bias our results. SLS records include country of birth as well as the year of immigration (if applicable) which allows us to reliably identify immigrants and thus exposure to the compulsory education reforms. However, this does not rule out the possibility that education selectively increases the propensity to emigrate. As mentioned in Section 4, UK residents are required to notify the NHS when they move abroad.[17] This allows us to analyze whether the ROSLA reforms increased the probabilities of reporting such a relocation and provides a useful proxy for emigration in general (both observed and unobserved).

Appendix Table A7 shows that there is no indication that increases in education have led to increases in emigration. All point estimates are statistically insignificant at the five percent level and of a size that suggests that any bias arising from selective migration should be negligible. In particular, the coefficient for men in the cohorts which were potentially

[17]Pre-Brexit, pensioners moving to southern Europe, in particular, had a strong incentive to do so in order to claim entitlement to reciprocal healthcare in their host country.

exposed to the 1972 ROSLA is positive. Assuming that healthier individuals are more likely to emigrate (as suggested by Farré (2016), among others), we would thus, expect any bias in our estimates to lead to an underestimation of the health benefits of education.

5.4.3 Placebo Tests

Finally, we conducted a series of falsification tests. For this exercise, we selected SLS participants who were born between September 1958 and August 1972. By virtue of their late birth years, neither of these cohorts was affected by changes in compulsory schooling laws. For our test, we create a "fake" (placebo) treatment indicator for a compulsory schooling reform. This indicator is equal to one for all SLS participants born in or after September 1965. As a hypothetical intervention, one would expect this placebo regression to yield a null result, whereas a health-enhancing effect would suggest issues with our approach or specification. Appendix Table A8 indeed confirms that we find no statistically significant effect of our placebo treatment on education nor on the number of inpatient episodes – be it general, through alcohol misuse, or for issues related to the digestive system.

6 Discussion

There are several mechanism through which education can affect health. Our results for alcohol and drug misuse provide some evidence for a behavioral channel, and as such our study highlights the importance of information and behavioral changes in the production of health. There is, of course, also scope for socioeconomic mechanisms. A large literature links the ROSLA reform effects to increased earnings. In particular, the gender differences we uncover are consistent with results by Devereux and Hart (2010) who document that the first British compulsory schooling reform yielded no returns for women and boosted male

earnings by 3-7%.[18]

While neither the SLS nor the SHeS contain information on earnings, the SLS records neighbourhood characteristics. In particular, our data contain information on the Scottish Index of Multiple Deprivation (SIMD). To calculate the SIMD, Scotland is divided into 6,976 small areas (so called data zones) which are then ranked by levels of deprivation. Income is an important input for these calculation along with the domains of employment, education, health, access to services, crime and housing. Put differently, SIMD is a comprehensive measure of respondents' socioeconomic environment which may well have a larger impact on health than a pure earnings measure. In Table 6 we define as our outcomes of interest whether an individual resided in one of the 10% most deprived ("Most Deprived"), in one of the 10% most affluent ("Least Deprived") data zones as per the SIMD in either the 1991, 2001, or 2011 census. We also define a measure for whether a respondent resided in a data zone with below-median levels of deprivation. We find no meaningful relationship between exposure to the ROSLA reform and neighbourhood quality for men and only suggestive evidence for women who are about 4.2 percentage points more likely to live in a neighbourhood with below-median levels of deprivation. The compulsory schooling reform does not seem to have meaningfully affected the chances of relocating to a substantially more affluent neighborhood later in life.

The census-component of the SLS also contains information on occupations. We categorize respondents who report in any of the three census waves to work in professional, managerial and technical, and skilled manual or non-manual occupations (as opposed to partly skilled or unskilled occupations) into a single "skilled" category. The first column of Table 6 shows little in the way of an effect of the ROSLA reform on occupational choice.

Admittedly, both our deprivation and occupational proxies are too crude to fully discount the role of a socioeconomic channel.[19] At the same time, our study takes place in a context

[18]Recent work by Barcellos et al. (2021) documents further heterogeneity in wage gains by socio-economic background.

[19]Another mechanism, studied by Hener and Wilson (2018) and Anderberg et al. (2019), operates through the marriage market.

in which one of the most important inputs in the health production function, health care, is freely and universally accessible by way of the National Health Service (NHS). Of course, financial resources nonetheless matter in the acquisition of other health inputs. But it is notable that a lot of the health inequalities occur at points in the life-cycle when workers are either in or entering their prime-earning years.

Similar to previous literature, our reduced-form estimates also conclude that the compulsory reforms generally did not lead to meaningful changes in subjective health. Individuals who experienced the reform were statistically no less likely to report being in poor health, experience a longstanding illness, or possess current (or past) smoking habits. These findings are very much consistent with the subjective health estimates from Clark and Royer (2013) and suggest that the U.K. ROSLA reforms did not improve self-reported health or health behaviors. The sole exception among our subjective health estimates involves individuals receiving an additional year of education also being 3.4% more likely to report being a current drinker. In contrast, men who were assigned to an additional year of education experienced a 0.11 standard deviation reduction in alcohol-related inpatient episodes. Given that regular consumption of alcohol alone does not immediately imply poor health behavior, one possibility could be that men who experienced an additional year of schooling were simply more likely report their drinking status and less likely participate in excessive alcohol consumption behavior that might require future hospitalization.

How exactly should we reconcile the differences in self-reported health and our findings on health care utilization? Our findings on alcohol-related consumption and hospitalization suggests that systematic differences might exist in how survey participants report their drinking behaviors along the line the lines of education. Unlike self-reported mental health measures, which are congruent with clinical diagnostic procedures that already rely upon self-reported behavior, self-reported physical health measures entertain a variety of perceived health improvements–including changes in overall well-being (Finkelstein et al., 2012). Thus, an alternative interpretation of these findings could involve compulsory schooling reforms not

producing meaningful changes in overall well-being rather than any demonstrable improvements in actual physical health. A notable example of divergence in subjective and objective health measures comes from Johnston et al. (2009) who compares self-reported measures of hypertension with blood pressure measurements taken by a nurse practitioner. The authors find that 85% of sample engaged in false negative reporting of hypertension with lower income households being significantly more likely to provide a false negative report. Systematically biased measures of self-reported health would similarly result in attenuated estimates of the effects of education on health (Bound, 1991).

7 Conclusion

In this paper we combine longitudinal, nationally representative administrative data with a natural experiment generated by the 1972 British compulsory schooling reform in order to shed new light on the relationship between education and health. Our regression discontinuity design also reconciles some of the conflicting results within this literature. Similar to previous studies, we find no effect of additional schooling on self-reported health measures as obtained from health surveys. In contrast, we show that education reduces the hospitalizations that result in inpatient stays, lower cancer indidence, and fewer hospitalizations due to alcohol or drug abuse. These health benefits are most pronounced for men and for conditions related to digestive and cardiovascular diseases.

We also document that the largest education-induced improvements in health occur early on in the life-cycle during men's thirties and forties and persist until their early fifties. Higher mortality or selective out-migration do not drive our results. While we certainly acknowledge other avenues through which education might improve health, we also provide evidence that the positive effect of education may operate by way of a behavioral channel (e.g., through lower alcohol and drug misuse).

We also note that our study identifies a local average treatment effect (LATE) of an addi-

tional year of high school education for those individuals who were prompted to stay in high school for an additional year. While strong compliance ensures that our LATE resembles the average population effect, it is not informative with respect to health effects of, say, an additional year of tertiary education. This seems acceptable not least because recent increases in compulsory schooling age affected predominantly high school education. Therefore, our results inform the greater policy discussion surrounding ROSLA reforms with the caveat that we cannot distinguish between the effects of increases in education quality or even (potentially health-related) content covered in additional schooling time.

Finally, our research points to additional points of emphasis for future research. We show that the use of arguably more objective, administrative data can lead to substantially different conclusions than those drawn from survey data. This issue as well as the potential for non-response bias in surveys was recently highlighted by Dutz et al. (2021). We also specifically highlight the substantial and arguably under-explored gender differences within in the relationship between education and health. Our study also shows that this relationship may differ throughout the life cycle. This is consistent and adds a causal interpretation to the findings of **?** who document that the gradient of education on health peaks around ages 50–60. Life cycle effects are thus a phenomenon that deserves further study in future research by leveraging high quality panel data.

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Tables and Figures

Table 1: First Stage Results: Effect of 1972 Compulsory Schooling Reforms on Education

	Years of Education	≤9 years	≤10 years	≤11 years	≤12 years	≤13 years
	All (N=42,872; bandwidth = 50 months)					
Estimate	0.365***	−0.017***	−0.274***	−0.008	−0.010	−0.013
	(0.090)	(0.004)	(0.013)	(0.019)	(0.018)	(0.017)
Outcome Mean	11.98	0.0165	0.370	0.595	0.709	0.776
	Men (N=18,999; bandwidth = 74 months)					
Estimate	0.348***	−0.021***	−0.269***	0.003	−0.018	−0.014
	(0.102)	(0.006)	(0.020)	(0.026)	(0.024)	(0.018)
Outcome Mean	11.96	0.0233	0.381	0.600	0.703	0.777
	Women (N=23,873; bandwidth = 48 months)					
Estimate	0.382***	−0.015***	−0.277***	−0.016	−0.001	−0.011
	(0.129)	(0.006)	(0.015)	(0.026)	(0.023)	(0.025)
Outcome Mean	12.00	0.0111	0.360	0.590	0.714	0.774

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the first stage effects of compulsory schooling reforms on educational attainment as outlined in equation (2), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, sex (unless stratified by sex), month of birth, and survey year. All data come from the Scottish Health Survey (SHeS) with all waves from 1995 to 2016 pooled together.

Table 2: Effect of Education on Self-Reported Health - 1972 ROSLA

	Poor Health	Longst. Illness	Current Drinker	Current Smoker	Ever Smoked
Mean	0.208	0.380	0.818	0.269	0.614
(SD)	(0.406)	(0.485)	(0.386)	(0.443)	(0.487)
Reduced Form	0.007	0.016	0.034**	−0.011	0.010
	(0.015)	(0.013)	(0.017)	(0.015)	(0.017)
OLS (years of schooling)	−0.039***	−0.021***	0.009***	−0.045***	−0.036***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
OLS (>11 years)	−0.169***	−0.081***	0.044***	−0.197***	−0.159***
	(0.006)	(0.007)	(0.005)	(0.007)	(0.008)
N	42,869	42,858	43,949	44,021	44,229
Bandwidth	69.71	59.01	62.96	59.48	74.46

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the the effects of education on self-reported health outcomes. OLS estimates refer to the effect of an additional year of schooling or of having more than 11 years of education. Reduced from effects yield the effect of the compulsory schooling reforms on educational attainment as outlined in equation (2) and are obtained using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, sex (unless stratified by sex), month of birth, and survey year. All data come from the Scottish Health Survey (SHeS) with all waves from 1995 to 2016 pooled together.

Table 3: Effect of 1972 Compulsory Schooling Reform on Inpatient Hospitalizations

Panel A: All Hospitalizations

	Men			Women		
	Episodes	Stays	Days	Episodes	Stays	Days
Mean	5.164	4.449	13.939	6.137	5.561	14.282
(SD)	(9.828)	(8.496)	(57.567)	(10.205)	(8.984)	(48.675)
Coef	-1.607^{**}	-1.322^{**}	-5.187^{***}	-0.632	-0.515	-1.566
(SE)	(0.714)	(0.605)	(1.799)	(0.438)	(0.406)	(2.138)
N	32,164	32,164	32,164	32,103	32,103	32,103
Bandwidth	14.61	15.21	15.77	28.42	23.99	25.97

Panel B: Hospitalizations between Ages 29 and 53

	Men			Women		
	Episodes	Stays	Days	Episodes	Stays	Days
Mean	2.773	2.508	5.620	3.456	3.250	6.568
(SD)	(6.484)	(5.912)	(27.181)	(6.165)	(5.625)	(21.115)
Coef	-1.026^{**}	-0.870^{**}	-2.871^{***}	-0.553^{*}	-0.538^{*}	-0.703
(SE)	(0.399)	(0.349)	(0.824)	(0.307)	(0.279)	(0.667)
N	30,869	30,869	30,869	31,094	31,094	31,094
Bandwidth	16.16	17.10	14.37	20.65	23.52	15.65

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1972 compulsory schooling reform on the number of inpatient hospitalization as outlined in equation (3), using local polynomial regression discontinuity estimation. In Panel A, the outcome is the number of hospitalizations at any age between 1981 and 2016. In Panel B, the outcome is the number of hospitalizations between ages 29 to 53. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table 4: Effect of 1972 Compulsory Schooling Reform on Cancer Diagnoses

	Men				Women			
	Any Cancer	Lung Cancer	Skin Cancer	Urin. Cancer	Any Cancer	Lung Cancer	Skin Cancer	Genital Cancers
Mean	0.080	0.007	0.025	0.019	0.142	0.007	0.024	0.028
(SD)	(0.272)	(0.081)	(0.157)	(0.138)	(0.350)	(0.084)	(0.153)	(0.164)
Coef	-0.048^{***}	0.004	-0.011	-0.008^{**}	-0.02	-0.006^{*}	-0.005	-0.011^{*}
(SE)	(0.013)	(0.003)	(0.007)	(0.004)	(0.019)	(0.003)	(0.007)	(0.006)
N	31,776	31,776	31,776	31,776	31,720	31,720	31,720	31,568
Bandwidth	15.24	27.90	28.75	20.11	23.24	16.47	34.80	24.05

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1972 compulsory schooling reform on cancer diagnoses as outlined in equation (3), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table 5: Effect of 1972 ROSLA on Alcohol and Drug-Related Hospitaliations

	Alcohol-Related Inpatient Admissions				Drug-Related Inpatient Admissions			
	Men		Women		Men		Women	
	Episodes	Days	Episodes	Days	Episodes	Days	Episodes	Days
Mean	0.311	1.03	0.128	0.412	0.094	0.034	0.068	0.028
(SD)	(2.019)	(9.638)	(1.029)	(4.968)	(2.114)	(0.467)	(1.543)	(0.352)
Coef	-0.213^{**}	-0.779^{**}	-0.004	0.051	-0.035	-0.037	-0.002	0.039
(SE)	(0.103)	(0.326)	(0.036)	(0.198)	(0.027)	(0.084)	(0.012)	(0.051)
N	32,164	32,164	32,103	32,103	32,164	32,164	32,103	32,103
Bandwidth	27.22	16.68	12.1	19.04	19.17	18.17	17.14	13.69

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1972 compulsory schooling reforms on alcohol and drug related hospital admissions, as outlined in equation (3), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

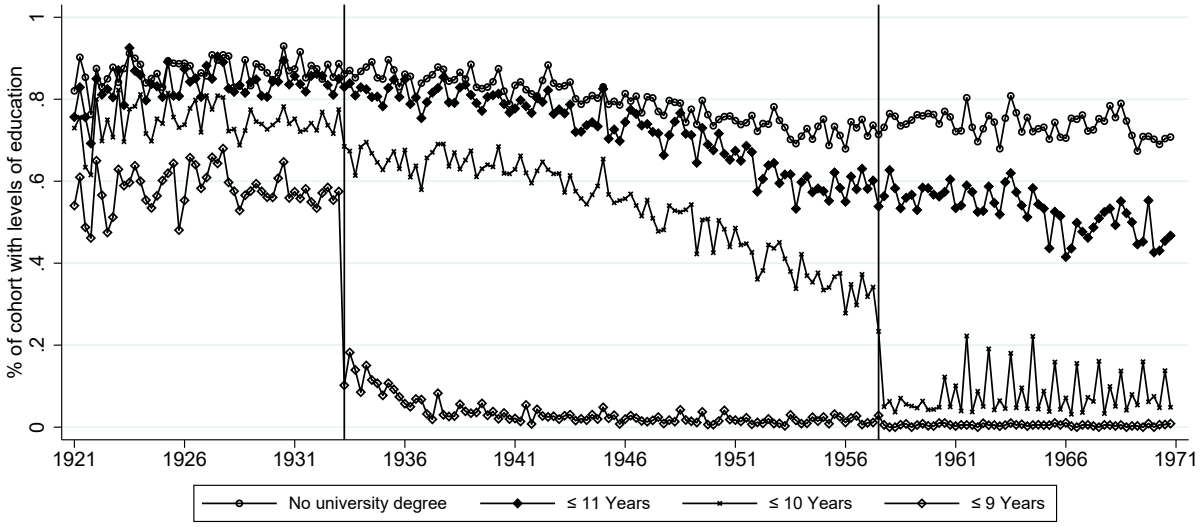
Table 6: Effect of 1972 ROSLA on Socio-Economic Outcomes

	Skilled Occ.	Most Deprived	Above Median	Least Deprived
Panel A: Men				
Mean	0.755	0.095	0.489	0.108
(SD)	(0.430)	(0.294)	(0.500)	(0.310)
Coef	0.001	0.007	−0.007	−0.015
(SE)	(0.012)	(0.011)	(0.024)	(0.011)
N	31,776	31,745	31,745	31,745
Bandwidth	16.67	28.73	18.54	21.76
Panel B: Women				
Mean	0.728	0.099	0.488	0.111
(SD)	(0.445)	(0.299)	(0.500)	(0.314)
Coef	0.021	−0.007	0.042***	−0.010
(SE)	(0.020)	(0.008)	(0.012)	(0.010)
N	31,720	31,711	31,711	31,711
Bandwidth	21.75	17.25	22.49	28.82

Notes: ∗ ∗ ∗/ ∗ ∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of compulsory schooling reforms on a set of socio-economic outcomes, in particular whether a SLS participant reports to be employed in either a professional, managerial, technical, or other skilled occupation; whether a SLS participant reports living in one of the 10% most deprived neighborhoods, in a neighborhood above deprivation median, and one of the 10% least deprived neighborhoods (as measured by the Scottish Index of Multiple Deprivations (SMID). Estimation is outlined in equation (3) and uses local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of a ROSLA cut-off date were included in the analysis.

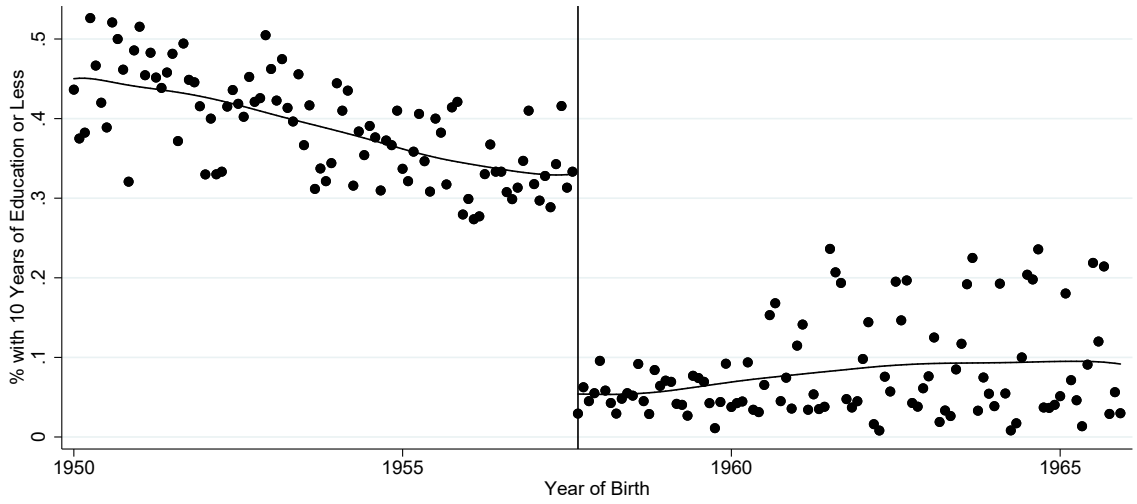
Figure 1: Years of Full-Time Education by Quarter of Birth



Notes: Each dot is a quarter of birth cohort. Vertical lines indicate ROSLA reforms. The 1947 reform is indicated by the first vertical line and increased the minimum schooling age from 14 to 15 for all cohorts born in the 2nd quarter of 1933 or after. The second reform increased the minimum school leaving age from 15 to 16 for all cohorts born in or after September 1957.
Data Source: Scottish Health Surveys of 1995-2016

Figure 2: Effect of 1972 ROSLA Reform on Educational Attainment

(a) ≤10 Years of Education



(b) Years of Education



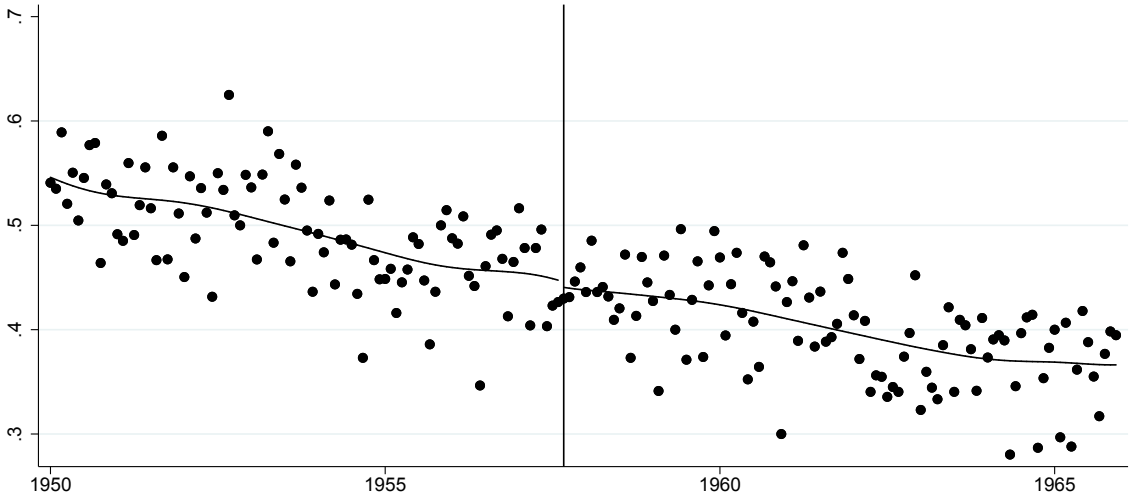
Notes: Each dot is the average of a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reform. Data Source: Pooled data from 1995-2016 waves of the Scottish Health Survey (SHeS).

Figure 3: Effect of 1972 ROSLA Reform on Self-Reported Health

(a) 1972 Reform: Poor Health



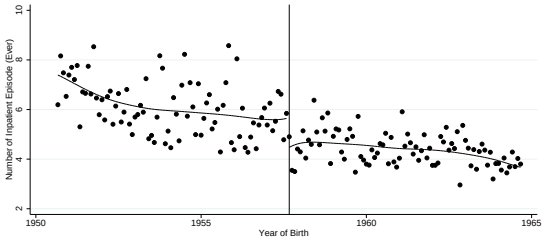
(b) 1972 Reform: Long-Standing Illness



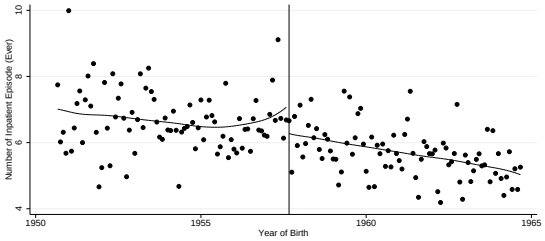
Notes: Each dot shows the fraction of a month-year birth cohort who reported to be in fair, bad, or very bad health and who reported having a long-standing illness, respectively. Horizontal lowess lines provide a flexible fit. Vertical lines indicate 1972 ROSLA reform. All figures are constructed by pooling the 1995-2016 waves of the Scottish Health Survey (SHeS).

Figure 4: Effect of 1972 ROSLA Reform on Inpatient Hospitalizations

(a) Men - Inpatient Episodes



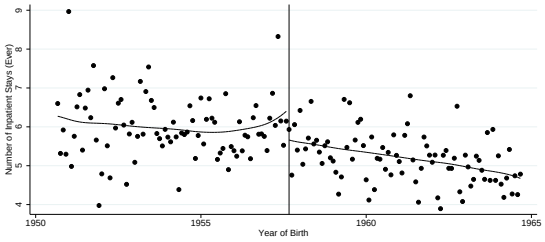
(b) Women - Inpatient Episodes



(c) Men - Inpatient Stays



(d) Women - Inpatient Stays



(e) Men - Inpatient Days



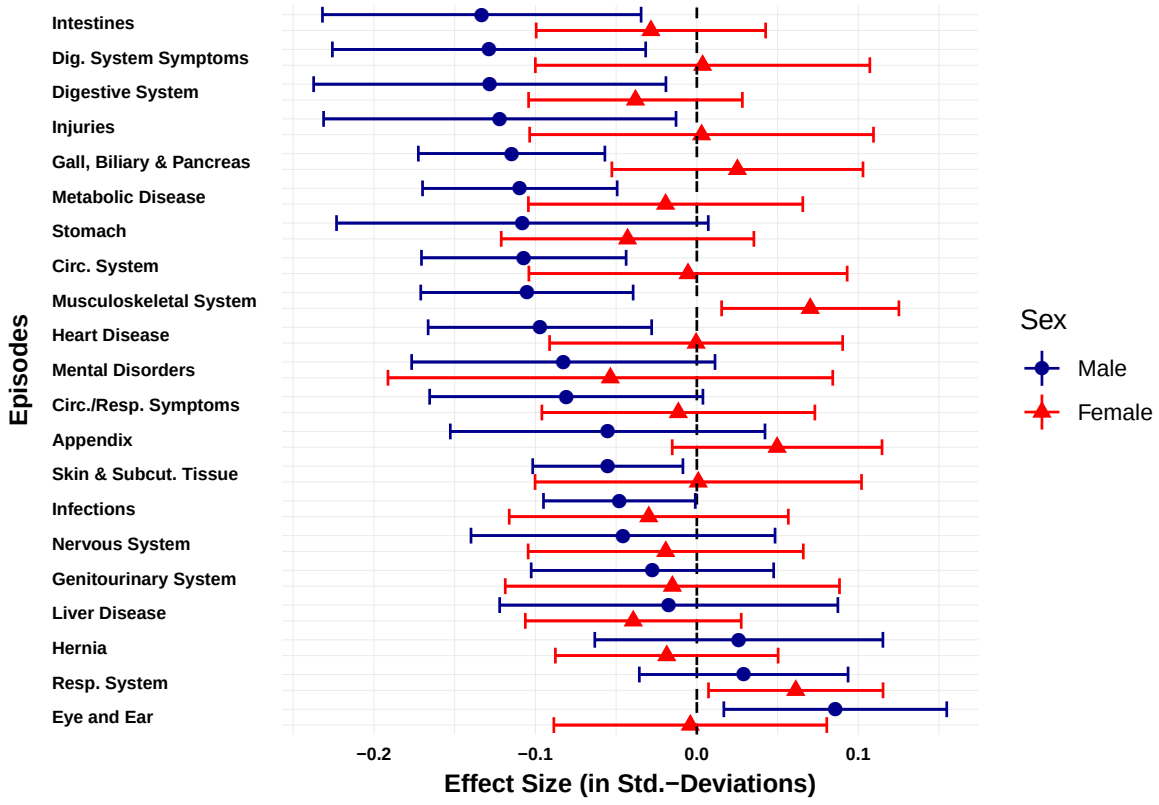
(f) Women - Inpatient Days



Notes: Each dot shows the the average number of inpatient episodes, stays, and days for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

Figure 5: Effect of 1972 ROSLA on Inpatient Episodes by Diagnosis

(a) Effect on Inpatient Care Episodes



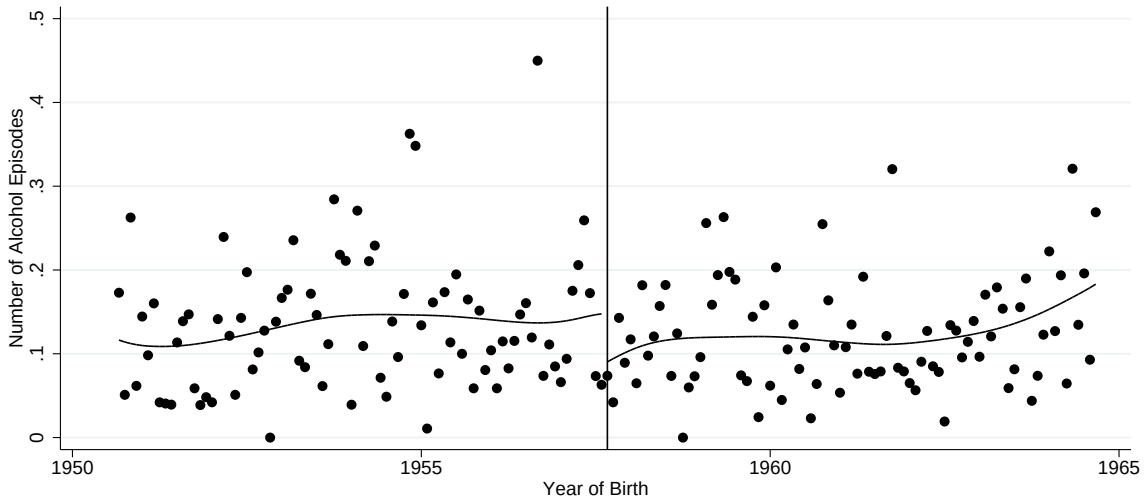
Notes: Forrest plot shows reduced form point estimates and 95% confidence intervals for the effect of the 1972 ROSLA reform on the number of inpatient hospitalization episodes. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex.

Figure 6: Effect of 1972 ROSLA Reform on Alcohol-Related Hospitalizations

(a) Number of Episodes - Men

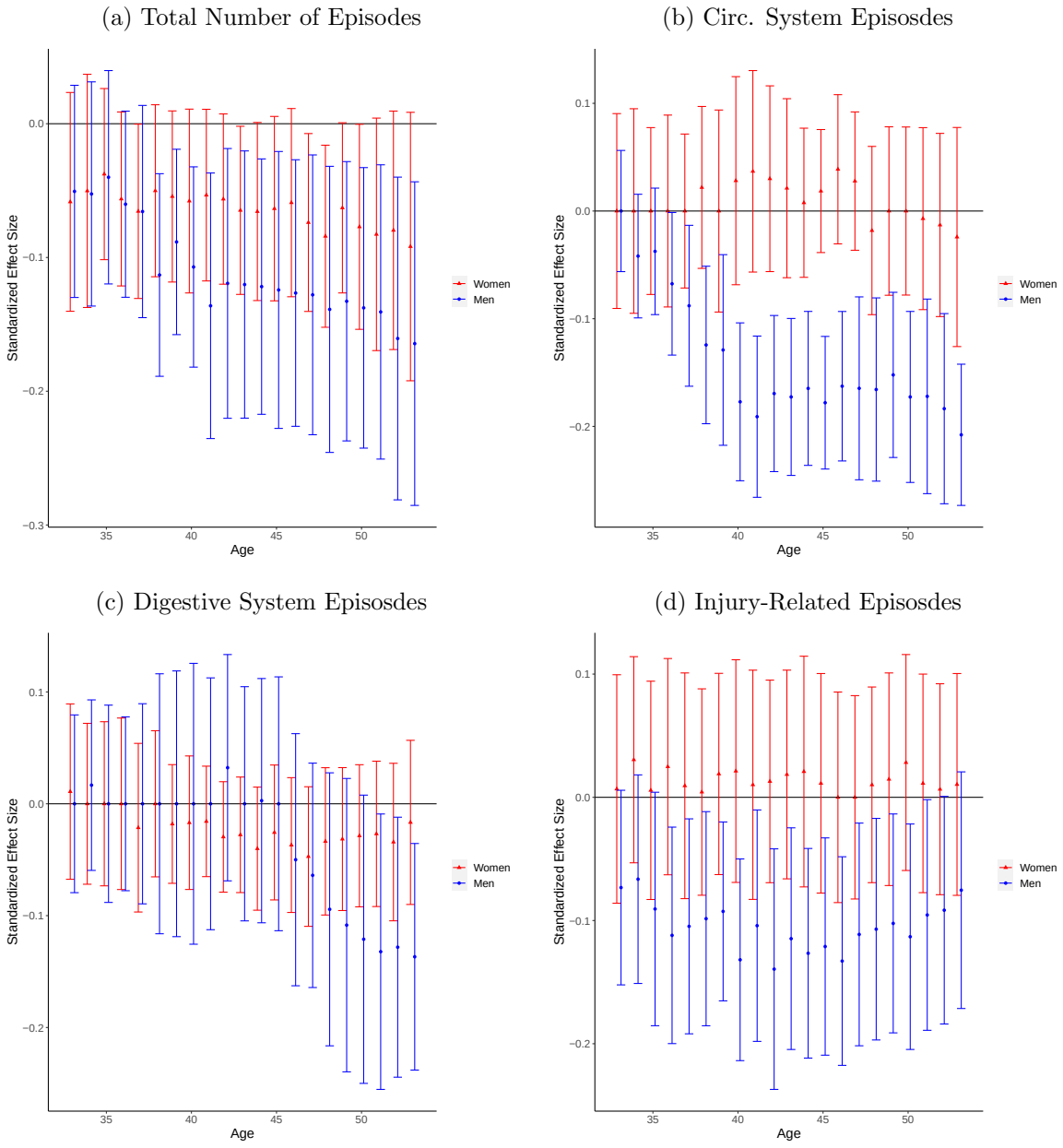


(b) Number of Episodes - Women



Notes: Each dot shows the average number of inpatient episodes due to alcohol poisoning, intoxication, harmful use, or dependency/withdrawal for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate 1972 ROSLA reform. Data Source: Scottish Longitudinal Study (SLS).

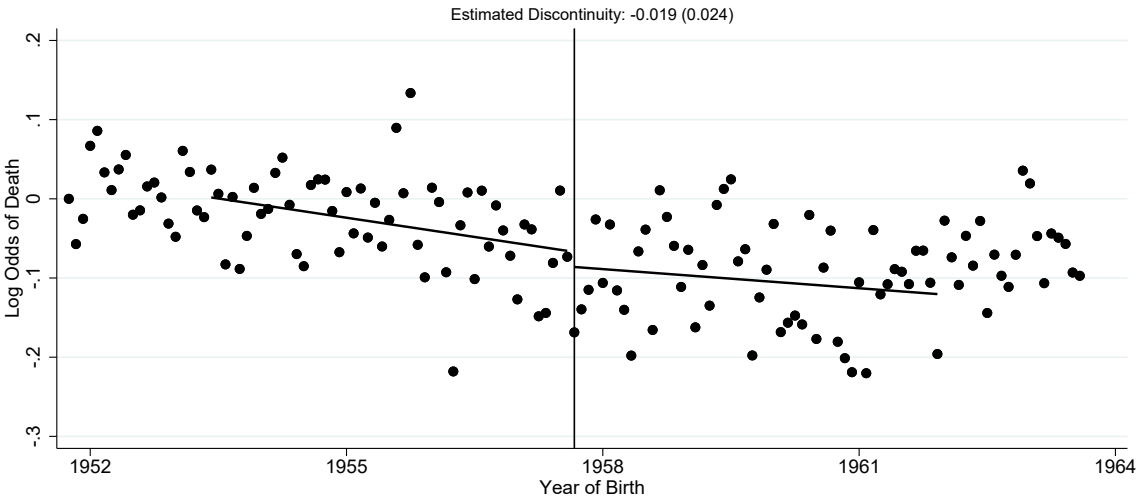
Figure 7: Life-Cycle Effects of 1972 ROSLA Reforms on Hospitalizations



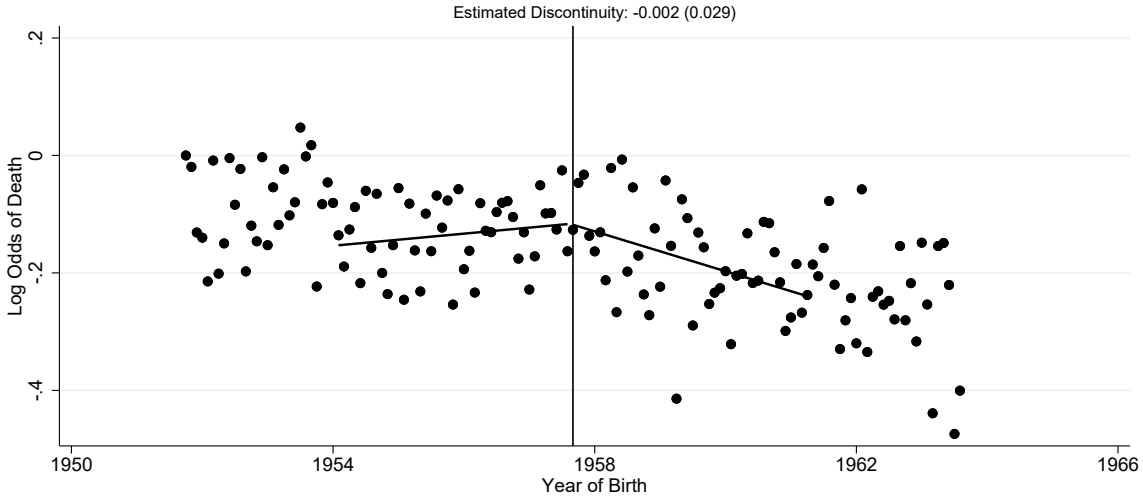
Notes: Charts shows reduced form point estimates and 95% confidence intervals for the effect of the 1972 ROSLA reform on the number of inpatient hospitalization episodes in general and by select conditions. Each estimate describes the effect of the 1972 ROSLA on the number of hospitalizations by a particular age. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex. Data Source: Scottish Longitudinal Study (SLS).

Figure 8: Effect of 1972 ROSLA Reform on Mortality

(a) Men



(b) Women



Notes: Each dot shows the log odds death ratio for a month-year birth cohort. All estimates should be interpreted relative to the September 1950 cohort. Data Source: Scottish Census and Death Registry.

Appendix A

Table A1: Table of Means for SLS Samples

Panel A: Sample for 1947 ROSLA	Men		Women	
	April 1926 - March 1933	April 1933 - March 1940	April 1926 - March 1933	April 1933 - March 1940
Pre-Defined Characteristics				
% White	0.998	0.997	0.996	0.997
% Non-White	0.002	0.003	0.004	0.003
% Catholic (Raised)	0.089	0.115	0.104	0.131
% Church of Scotland (Raised)	0.450	0.496	0.469	0.502
Education Outcomes				
% No Qualification	0.746	0.663	0.764	0.677
%At Least O-Grades	0.254	0.337	0.236	0.323
% Degree	0.115	0.140	0.104	0.128
Post-Defined Characteristics				
% Unskilled	0.273	0.243	0.350	0.338
% Skilled	0.439	0.443	0.398	0.395
% Professional	0.288	0.315	0.252	0.267
% Ever Married	0.914	0.912	0.912	0.937
Deprivation Index	−0.056	−0.146	−0.027	−0.150
N	9,910	11,240	11,408	11,801

Panel B: Sample for 1972 ROSLA	Men		Women	
	Sep. 1950 - August 1957	Sep. 1957 - August 1964	Sep. 1950 - August 1957	Sep. 1957 - August 1964
Pre-Defined Characteristics				
% White	0.998	0.997	0.998	0.998
% Non-White	0.002	0.003	0.002	0.002
% Catholic (Raised)	0.139	0.149	0.16	0.173
% Church of Scotland (Raised)	0.447	0.386	0.475	0.427
Education Outcomes				
% No Qualification	0.371	0.296	0.377	0.269
% At Least O-Grades	0.629	0.704	0.623	0.731
% Degree	0.245	0.239	0.256	0.255
Post-Defined Characteristics				
% Unskilled	0.193	0.240	0.259	0.242
% Skilled	0.447	0.474	0.418	0.457
% Professional	0.36	0.287	0.324	0.301
% Ever Married	0.843	0.665	0.894	0.759
Deprivation Index	−0.486	−0.155	−0.332	0.024
N	14,641	17,135	14,774	16,946

Notes: This table shows the means for all observable demographic characteristics of SLS participants who were born within seven years of either ROSLA cut-off date. Data Source: Scottish Longitudinal Study (SLS).

Table A2: First Stage Results: Effect of 1947 Compulsory Schooling Reform on Education

	Years of Education	≤9 years	≤10 years	≤11 years	≤12 years	≤13 years
Panel A: Impact of 1947 ROSLA						
	All (N=24,001; bandwidth = 48 months)					
Estimate	0.515***	−0.424***	−0.065***	−0.002	0.001	−0.007
	(0.085)	(0.023)	(0.022)	(0.016)	(0.014)	(0.014)
Outcome Mean	10.15	0.587	0.741	0.843	0.898	0.927
	Men (N=10,627; bandwidth = 56 months)					
Estimate	0.274*	−0.376***	−0.003	0.028	0.041	0.009
	(0.152)	(0.038)	(0.032)	(0.029)	(0.027)	(0.023)
Outcome Mean	10.16	0.578	0.749	0.844	0.893	0.924
	Women (N=13,374; bandwidth = 51 months)					
Estimate	0.696***	−0.450***	−0.114***	−0.032	−0.033**	−0.019
	(0.120)	(0.031)	(0.034)	(0.021)	(0.016)	(0.017)
Outcome Mean	10.14	0.593	0.736	0.842	0.901	0.929

Notes: ∗ ∗ ∗/ ∗ ∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the first stage effects of compulsory schooling reforms on educational attainment as outlined in equation (2), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, sex (unless stratified by sex), month of birth, and survey year. All data come from the Scottish Health Survey (SHeS) with all waves from 1995 to 2016 pooled together.

Table A3: Effect of Education on Self-Reported Health - 1947 ROSLA

	Poor Health	Longst. Illness	Current Drinker	Current Smoker	Ever Smoked
Panel A: Impact of 1947 ROSLA					
Mean	0.356	0.592	0.758	0.230	0.651
(SD)	(0.479)	(0.491)	(0.429)	(0.421)	(0.477)
Reduced Form	0.025	0.011	−0.012	−0.004	−0.020
	(0.022)	(0.028)	(0.025)	(0.020)	(0.027)
OLS (years of schooling)	−0.165***	−0.060***	0.105***	−0.082***	−0.038***
	(0.011)	(0.010)	(0.008)	(0.008)	(0.010)
OLS (>10 years)	−0.044***	−0.014***	0.026***	−0.023***	−0.009***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
N	24,377	24,377	24,350	24,353	24,353
Bandwidth	48.93	59.82	83.02	55.44	50.98

Notes: ∗ ∗ ∗/ ∗ ∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the the effects of education on self-reported health outcomes. OLS estimates refer to the effect of an additional year of schooling or of having more than 10 years of education. Reduced from effects yield the effect of the compulsory schooling reform on educational attainment as outlined in equation (2) and are obtained using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, sex (unless stratified by sex), month of birth, and survey year. All data come from the Scottish Health Survey (SHeS) with all waves from 1995 to 2016 pooled together.

Table A4: Effect of 1947 Compulsory Schooling Reform on Inpatient Hospitalizations

Panel A: All Hospitalizations

	Men			Women		
	Episodes	Stays	Days	Episodes	Stays	Days
Mean	11.27	8.792	57.457	10.562	8.273	58.961
(SD)	(13.124)	(10.837)	(111.037)	(12.007)	(9.624)	(112.2)
Coef	−2.151**	−1.849**	−7.462	0.410	0.308	4.152
(SE)	(0.905)	(0.728)	(5.467)	(0.770)	(0.561)	(6.348)
N	21,311	21,311	21,311	23,407	23,407	23,407
Bandwidth	17.38	16.68	20.27	33.90	34.73	26.84

Panel B: Hospitalizations between Ages 53 and 77

	Men			Women		
	Episodes	Stays	Days	Episodes	Stays	Days
Mean	6.302	5.263	18.395	5.310	4.535	18.174
(SD)	(8.882)	(7.448)	(48.075)	(7.533)	(6.258)	(47.285)
Coef	−1.195	−1.374*	−3.601	0.130	0.149	−1.392
(SE)	(0.886)	(0.801)	(2.766)	(0.464)	(0.358)	(2.843)
N	12,813	12,813	12,813	16,796	16,796	16,796
Bandwidth	19.09	17.48	18.49	32.25	32.50	20.14

Notes: ∗∗∗/ ∗∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effect of the 1947 compulsory schooling reform on the number of inpatient hospitalizations as outlined in equation (3), using local polynomial regression discontinuity estimation. In Panel A the outcome is the number of hospitalizations at any age between 1981 and 2016. In Panel B, the outcome is the number of hospitalizations between ages 53 to 77. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table A5: Effect of 1947 Compulsory Schooling Reform on Cancer Diagnoses

	Men				Women			
	Any Cancer	Lung Cancer	Skin Cancer	Urin. Cancer	Any Cancer	Lung Cancer	Skin Cancer	Genital Cancers
Mean	0.396	0.074	0.115	0.103	0.343	0.054	0.093	0.034
(SD)	(0.489)	(0.262)	(0.319)	(0.304)	(0.475)	(0.225)	(0.290)	(0.180)
Coef	−0.005	0.017	0.01	−0.042***	0.015	0.008	0.008	−0.001
(SE)	(0.025)	(0.016)	(0.016)	(0.013)	(0.032)	(0.014)	(0.020)	(0.010)
N	21,150	21,150	21,150	21,150	23,209	23,209	23,209	23,209
Bandwidth	32.07	22.53	29.07	18.26	33.56	24.18	29.61	21.69

Notes: ∗ ∗ ∗/ ∗ ∗/∗ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1947 compulsory schooling reform on cancer diagnoses as outlined in equation (3), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table A6: Effects of 1947 ROSLA on Alcohol and Drug-Related Hospitalizations

	Alcohol-Related Inpatient Admissions				Drug-Related Inpatient Admissions			
	Men		Women		Men		Women	
	Episodes	Days	Episodes	Days	Episodes	Days	Episodes	Days
Mean	0.283	2.187	0.127	0.975	0.033	0.007	0.049	0.008
(SD)	(1.325)	(15.832)	(0.844)	(8.824)	(0.983)	(0.113)	(1.229)	(0.134)
Coef	0.029	−0.729	0.114*	0.877	0.005	0.042	0.005	0.026
(SE)	(0.106)	(1.053)	(0.062)	(0.703	(0.004)	(0.047)	(0.004)	(0.037)
N	21,311	21,311	23,407	23,407	21,311	21,311	23,407	23,407
Bandwidth	24.49	37.72	12.63	14.56	16.37	20.58	18.51	17.59

Notes: ***/**/* indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of the 1947 compulsory schooling reform on alcohol and drug related hospital admissions, as outlined in equation (3), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of the ROSLA cut-off date were included in the analysis.

Table A7: Effect of Compulsory Schooling Reforms on Emigration

	1947 ROSLA		1972 ROSLA	
	Men	Women	Men	Women
Mean	0.022	0.019	0.048	0.039
(SD)	(0.146)	(0.138)	(0.213)	(0.193)
Coef	0.009*	0.007	0.009	−0.008
(SE)	(0.005)	(0.005)	(0.008)	(0.009)
N	21,484	23,492	35,228	34,831
Bandwidth	11.18	21.38	22.63	14.36

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

This table reports the reduced form effects of compulsory schooling reforms on observed emigration. Unobserved emigration by definition cannot be assessed. The regression follows equation (3), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS), only individuals who were born within seven years of a ROSLA cut-off date were included in the analysis.

Table A8: Placebo Regressions: 1958 to 1972 Birth Cohorts

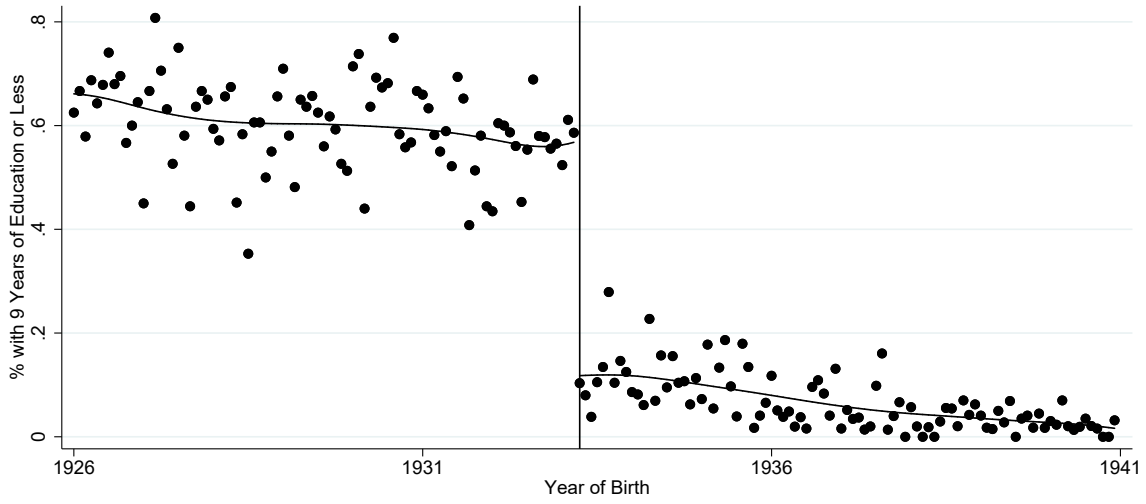
	No Qual.	Inpatient Epis. (all)	Inpatient Epis. (Digest. Syst.)	Alcohol-related Epis.
Panel A: Men				
Mean	0.287	3.876	0.750	0.244
(SD)	(0.452)	(7.438)	(2.440)	(1.756)
Coef	−0.019	−0.022	−0.066	0.038
(SE)	(0.016)	(0.284)	(0.082)	(0.068)
N	33,451	33,794	33,794	33,794
Bandwidth	26.66	25.60	26.88	26.34
Panel B: Women				
Mean	0.244	5.080	0.841	0.112
(SD)	(0.430)	(8.716)	(2.541)	(1.000)
Coef	−0.008	−0.527	0.003	−0.007
(SE)	(0.028)	(0.363)	(0.085)	(0.044)
N	33,564	33,802	33,802	33,802
Bandwidth	15.58	18.98	27.50	20.15

Notes: $***/**/*$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors adjusted for clustering at the running variable are reported in parentheses.

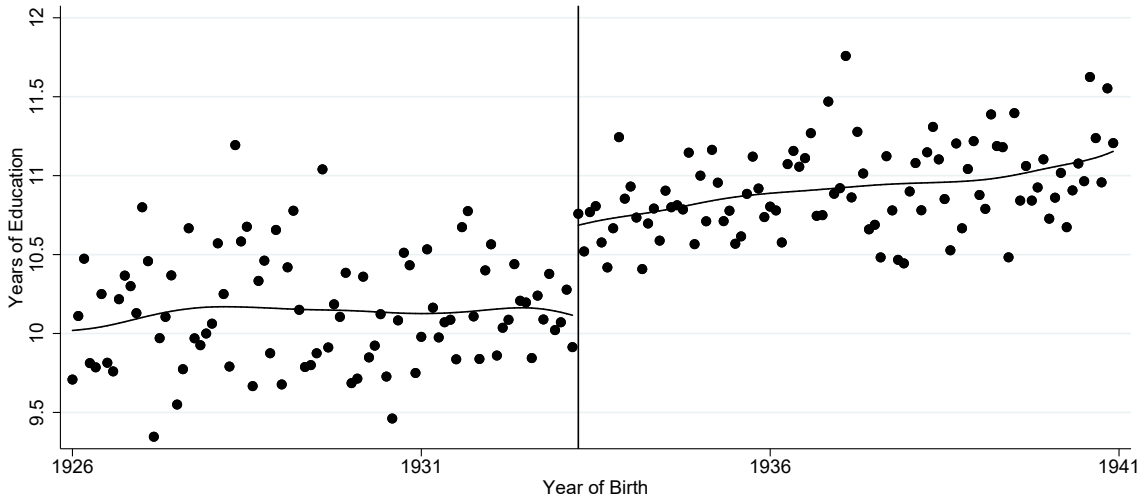
This table reports the results from a placebo regression in which only SLS respondents who were born between August 1958 and September 1972 were used. These cohorts were not subject to any ROSLA changes. The table calculates reduced form effects of a hypothetical ROSLA reform affecting everyone born after September 1965, as outlined in equation (3), using local polynomial regression discontinuity estimation. All regressions control flexibly for birthyear-month and control for age, age-squared, and a third-order polynomial for age, and month of birth. All data come from the Scottish Longitudinal Study (SLS).

Figure A1: Effect of 1947 ROSLA Reform on Educational Attainment

(a) ≤9 Years of Education



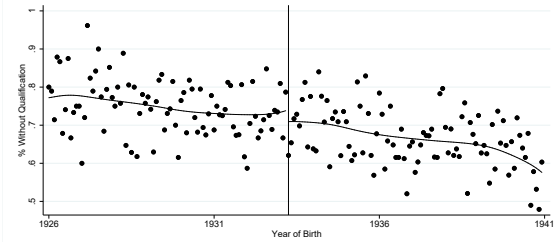
(b) Years of Education



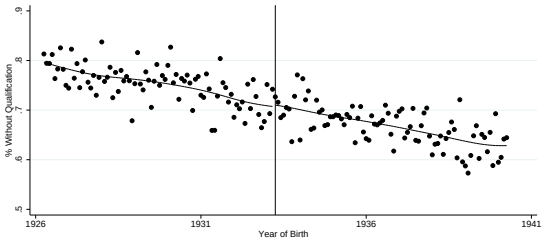
Notes: Each dot is the average of a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reform. Data Source: Pooled data from 1995-2016 waves of the Scottish Health Survey (SHeS).

Figure A2: Comparison SHeS and SLS Data

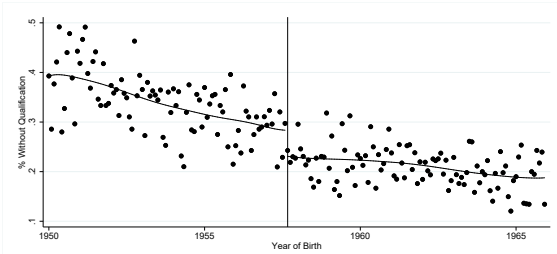
(a) SHeS - 1947 Reform: % No Qualification



(b) SLS - 1947 Reform: % No Qualification



(c) SHeS - 1972 Reform: % No Qualification



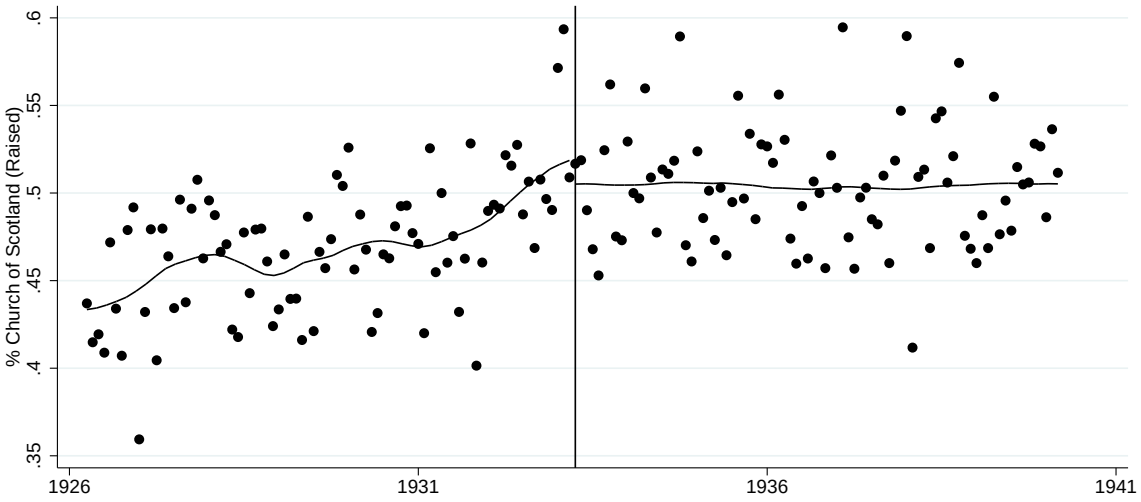
(d) SLS - 1972 Reform: % No Qualification



Notes: Each dot shows the fraction of a month-year birth cohort without formal qualification ($\approx$ high school dropouts). Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Panels (a) and (c) are constructed by pooling the 1995-2016 waves of the Scottish Health Survey (SHeS). Panels (b) and (d) are constructed using each individual's most recent information in the Scottish Longitudinal Study (SLS).

Figure A3: Balanced Covariates: Religious Denomination

(a) 1947 Reform: Fraction Church of Scotland



(b) 1972 Reform: Fraction Church of Scotland



Notes: Each dot is the average of a month-year birth cohort that was raised Protestant (Church of Scotland). Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

Figure A4: Effect of 1947 ROSLA Reform on Self-Reported Health

(a) 1947 Reform: Poor Health



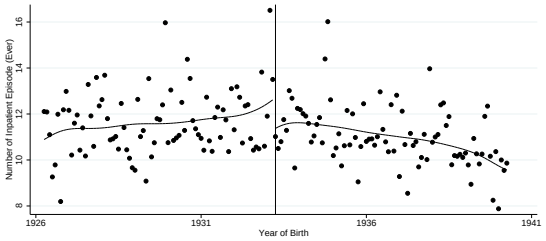
(b) 1947 Reform: Long-Standing Illness



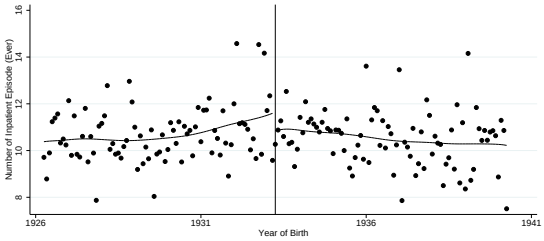
Notes: Each dot shows the fraction of a month-year birth cohort who reported to be in fair, bad, or very bad health and who reported having a long-standing illness, respectively. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. All figures are constructed by pooling the 1995-2016 waves of the Scottish Health Survey (SHeS).

Figure A5: Effect of 1947 ROSLA Reform on Inpatient Hospitalizations

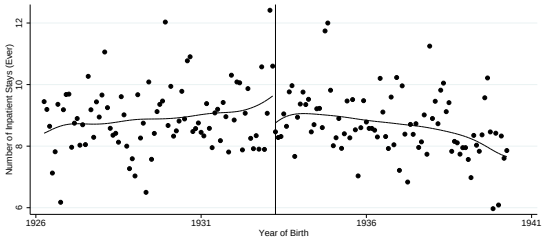
(a) Men - Inpatient Episodes



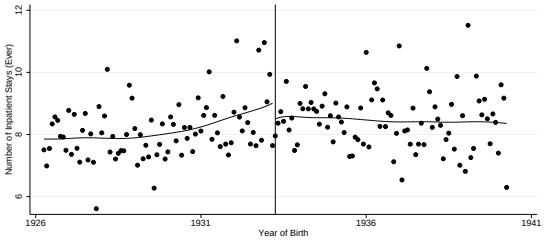
(b) Women - Inpatient Episodes



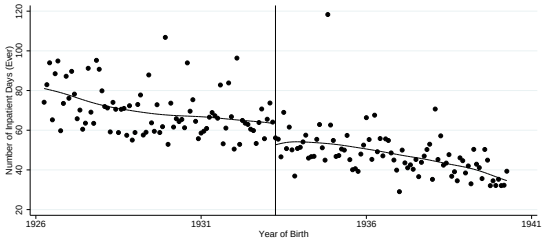
(c) Men - Inpatient Stays



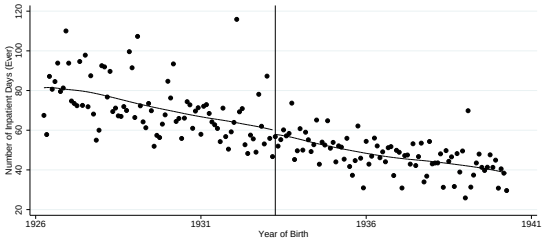
(d) Women - Inpatient Stays



(e) Men - Inpatient Days



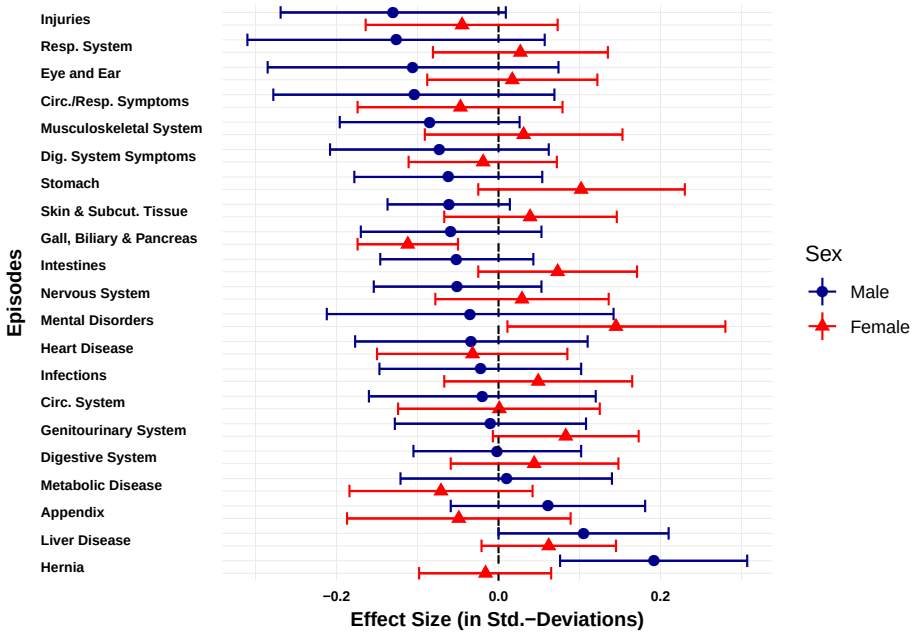
(f) Women - Inpatient Days



Notes: Each dot shows the the average number of inpatient episodes, stays, and days for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

Figure A6: Effect of 1947 ROSLA on Inpatient Episodes by Diagnosis

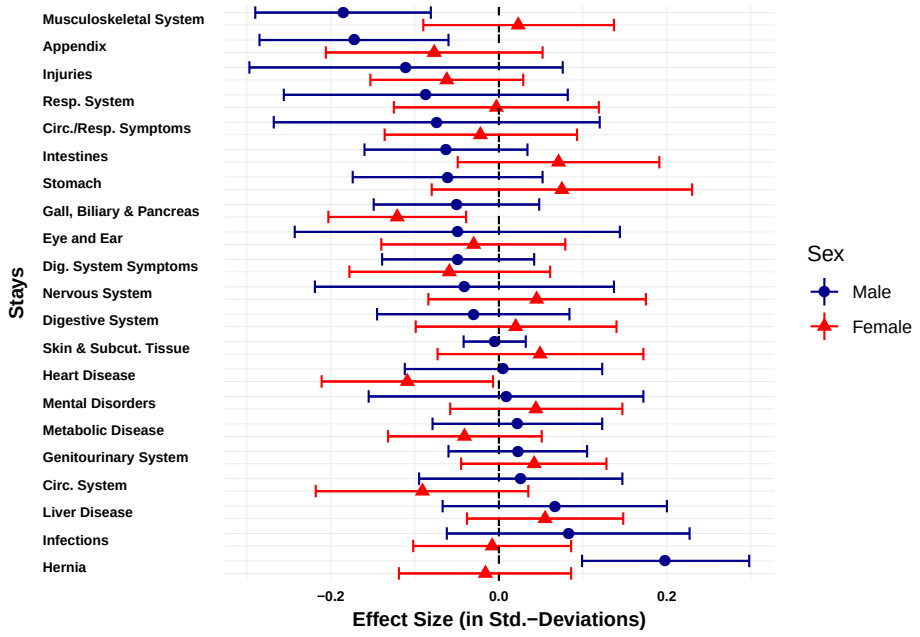
(a) Effect on Inpatient Care Episodes



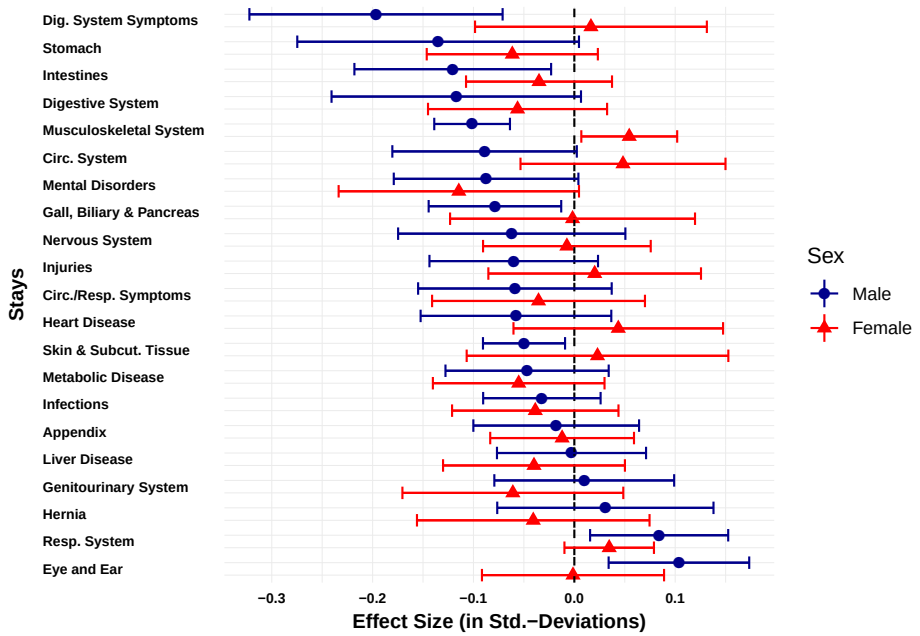
Notes: Forrest plot shows reduced form point estimates and 95% confidence intervals for the effect of the 1947 ROSLA reform on the number of inpatient hospitalization episodes. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex.

Figure A7: Effect on Inpatient Stays by Diagnosis

(a) 1947 Reform: Effect on Stays in Inpatient Care



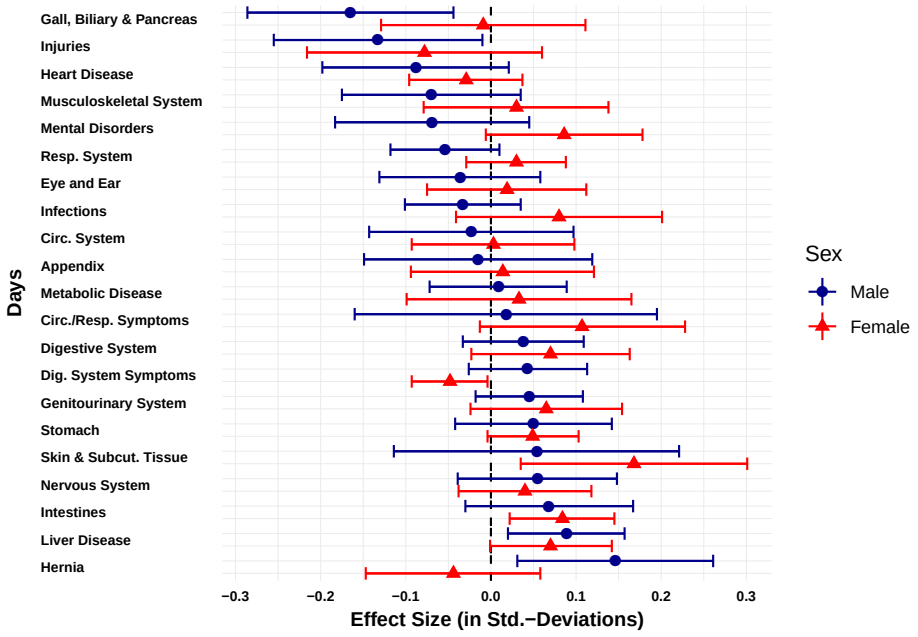
(b) 1972 Reform: Effect on Stays in Inpatient Care



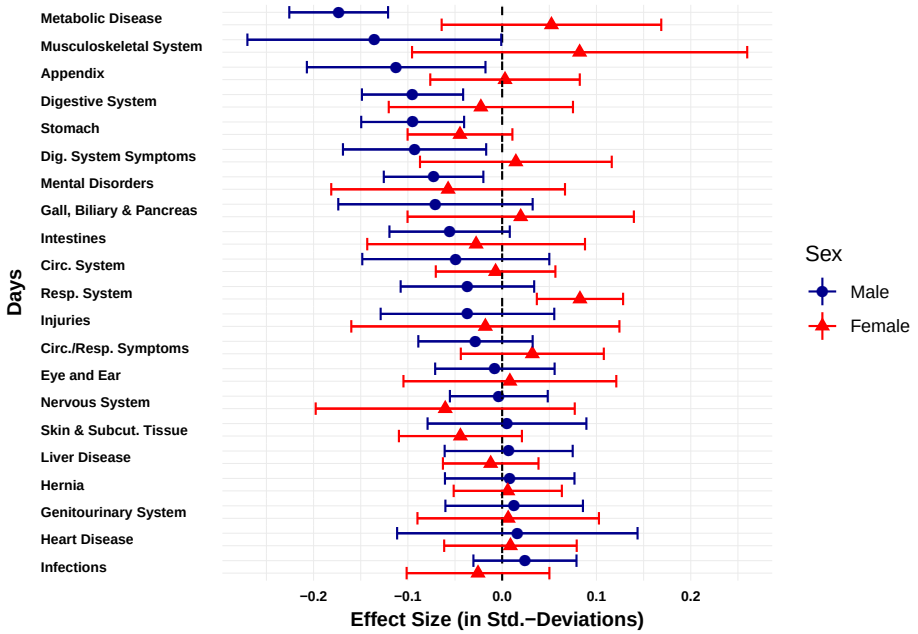
Notes: Forrest plots show reduced form point estimates and 95% confidence intervals for the effect of the ROSLA reforms on inpatient hospitalizations. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex.

Figure A8: Effect on Inpatient Days by Diagnosis

(a) 1947 Reform: Effect on Days in Inpatient Care



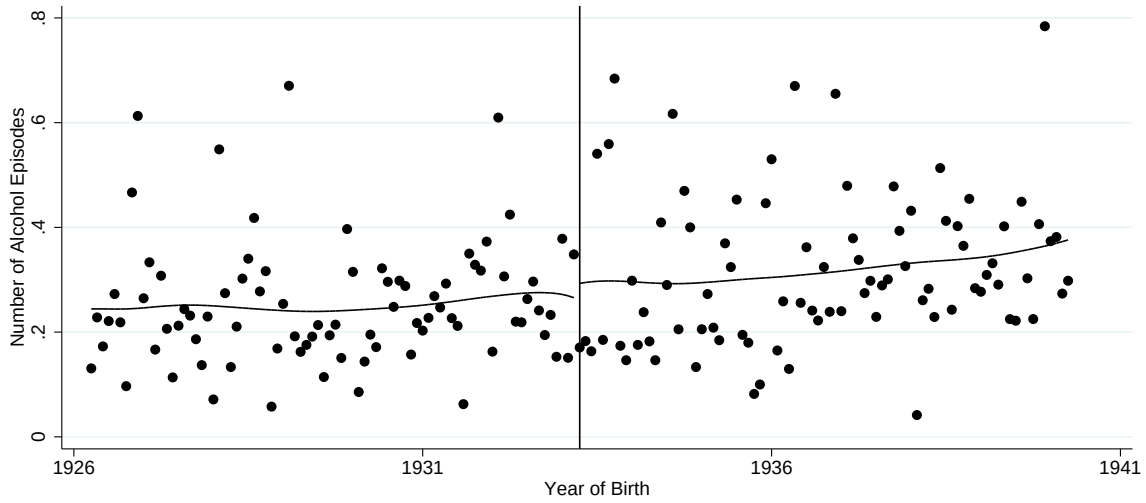
(b) 1972 Reform: Effect on Days in Inpatient Car



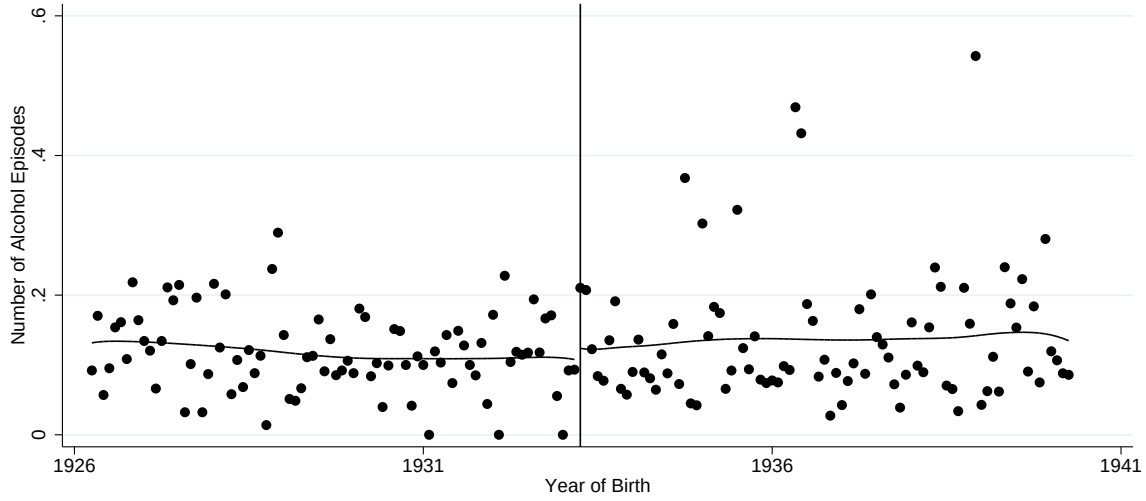
Notes: Forrest plots show reduced form point estimates and 95% confidence intervals for the effect of the ROSLA reforms on inpatient hospitalizations. Effects have been rescaled to reflect changes in standard deviations, and are shown separately by sex.

Figure A9: Effect of 1947 ROSLA Reform on Alcohol-Related Hospitalizations

(a) Number of Episodes - Men



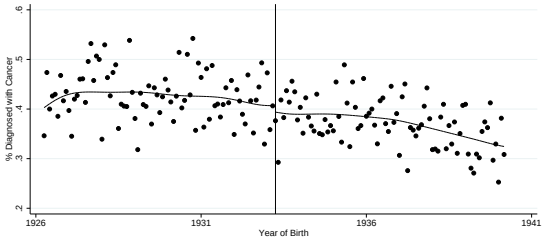
(b) Number of Episodes - Women



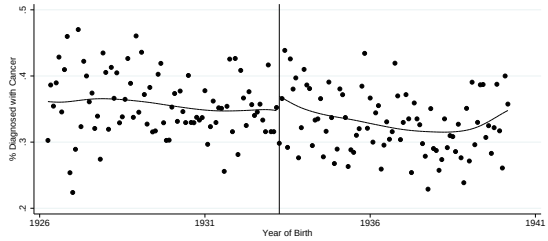
Notes: Each dot shows the average number of inpatient episodes due to alcohol poisoning, intoxication, harmful use, or dependency/withdrawal for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate 1947 ROSLA reform. Data Source: Scottish Longitudinal Study (SLS).

Figure A10: Effect of ROSLA Reforms on Cancer Prevalence

(a) 1947 Reform: Any Cancer Diagn. (Men)



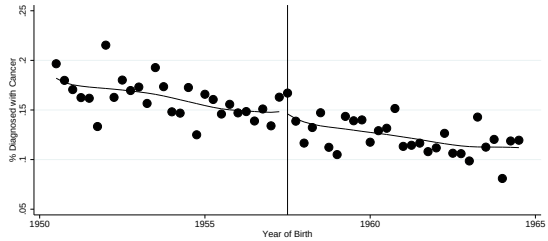
(b) 1947 Reform: Any Cancer Diagn. (Women)



(c) 1972 Reform: Any Cancer Diagn. (Men)



(d) 1972 Reform: Any Cancer Diagn. (Women)



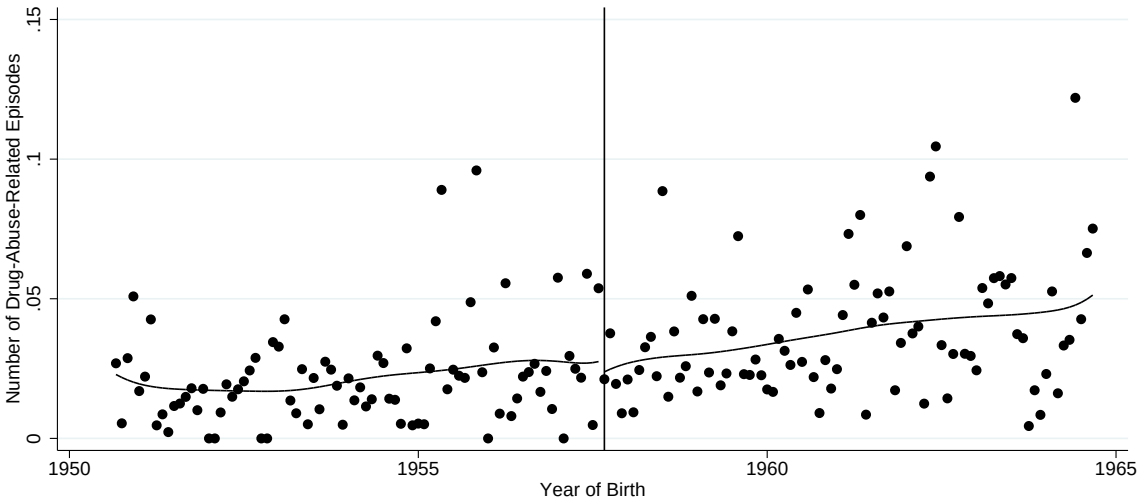
Notes: Each dot is the average of a month-year birth cohort that has been diagnosed with cancer. Because cancer diagnoses are rare for these groups, we show quarterly aggregates for later cohorts due to disclosure control reasons. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

Figure A11: Drug Abuse Related Inpatient Hospital Episodes

(a) 1947 Reform: Drug Abuse Related Admissions



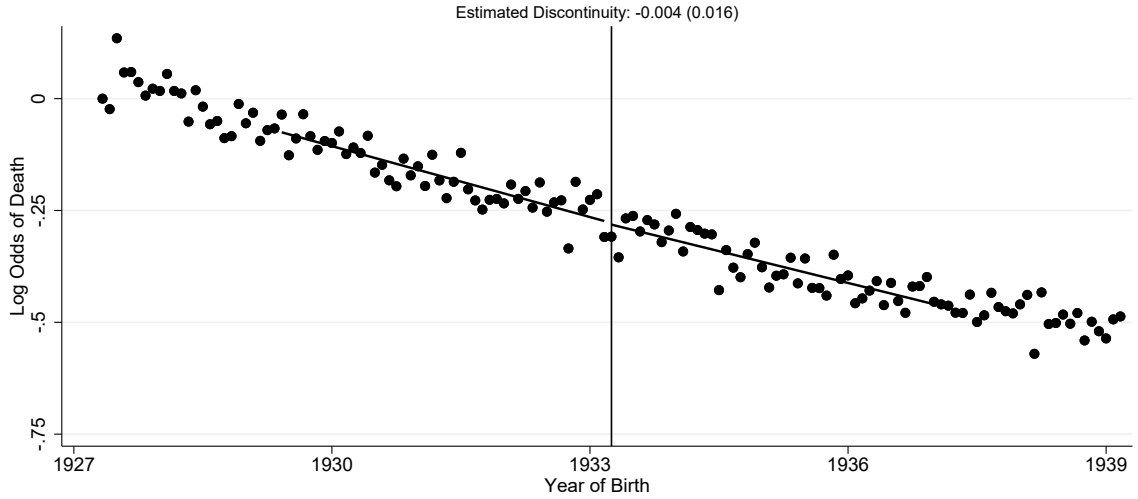
(b) 1972 Reform: Drug Abuse Related Admissions



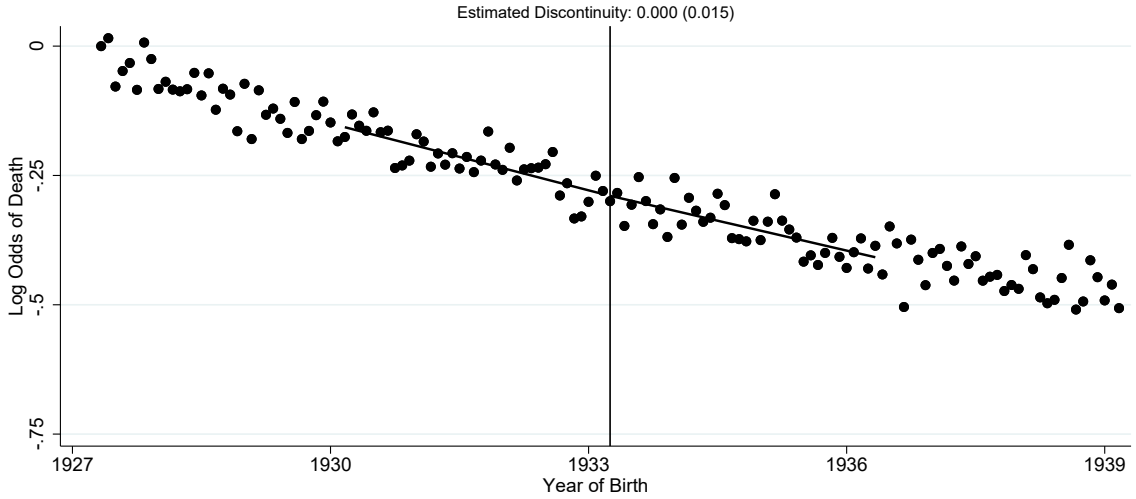
Notes: Each dot is the average number of drug abuse related admissions for a month-year birth cohort. Horizontal lowess lines provide a flexible fit. Vertical lines indicate ROSLA reforms. Data Source: Scottish Longitudinal Study (SLS).

Figure A12: Effect of 1947 ROSLA on Mortality

(a) Men



(b) Women



Notes: Each dot shows the log odds death ratio for a month-year birth cohort. All estimates should be interpreted relative to the April 1926 cohort. Data Source: Scottish Census and Death Registry.

Appendix B

.1 Documentation Hospitalization Categories

In this appendix, we describe and provide detailed definitions of diagnosis subcategories. These categories are listed in Figure 5. The basis of our classification is International statistical classification of diseases and related health problems in its 10th revision, 5th edition (2016). Analyzed group categories are in bold, analyzed subcategories in italics:

Certain infectious and parasitic diseases - ICD10 Codes A00-B99 ("Infections") include: Intestinal infectious diseases (A00-A09); Tuberculosis (A15-A19); Certain zoonotic bacterial diseases (A20-A28); Other bacterial diseases (A30-A49); Infections with a predominantly sexual mode of transmission (A50-A64); Other spirochetal diseases (A65-A69) Other diseases caused by chlamydiae (A70-A74); Rickettsioses (A75-A79); Viral and prion infections of the central nervous system (A80-A89); Arthropod-borne viral fevers and viral hemorrhagic fevers (A90-A99);Viral infections characterized by skin and mucous membrane lesions (B00-B09); Other human herpesviruses (B10); Viral hepatitis (B15-B19); Human immunodeficiency virus [HIV] disease (B20-B20); Other viral diseases (B25-B34); Mycoses (B35-B49); Protozoal diseases (B50-B64); Helminthiases (B65-B83); Pediculosis, acariasis and other infestations (B85-B89); Sequelae of infectious and parasitic diseases (B90-B94); Bacterial and viral infectious agents (B95-B97); Other infectious diseases (B99);

Endocrine, nutritional and metabolic diseases - ICD10 Codes E00-E89 ("Metabolic Diseases") include: Disorders of thyroid gland (E00-E07); Diabetes mellitus (E08-E13); Other disorders of glucose regulation and pancreatic internal secretions (E15-E16); Disorders of other endocrine gland (E20-E35); Intraoperative complications of endocrine system (E36); Malnutrition (E40-E46); Other nutritional deficiencies (E50-E64); Overweight, obesity and other hyperalimentation (E65-E68); Metabolic disorders (E70-E88); Other postprocedural

endocrine and metabolic complications and disorders (E89).

Mental, Behavioral and Neurodevelopmental disorders - ICD10 Codes F01-F99 ("Mental Disorders") include: Mental disorders due to known physiological conditions (F01-F09); Mental and behavioral disorders due to psychoactive substance use (F10-F19); Schizophrenia, schizotypal, delusional, and other non-mood psychotic disorders (F20-F29); Mood [affective] disorders (F30-F39); Anxiety, dissociative, stress-related, somatoform and other nonpsychotic mental disorders (F40-F48); Behavioral syndromes associated with physiological disturbances and physical factors (F50-F59); Disorders of adult personality and behavior (F60-F69); Intellectual Disabilities (F70-F79); Pervasive and specific developmental disorders (F80-F89); Behavioral and emotional disorders with onset usually occurring in childhood and adolescence (F90-F98); Unspecified mental disorder (F99-F99).

Diseases of the nervous system - ICD10 Codes G00-G99 ("Nervous System") include: Inflammatory diseases of the central nervous system (G00-G09); Systemic atrophies primarily affecting the central nervous system (G10-G14); Extrapyramidal and movement disorders (G20-G26); Other degenerative diseases of the nervous system (G30-G32); Demyelinating diseases of the central nervous system (G35-G37); Episodic and paroxysmal disorders (G40-G47); Nerve, nerve root and plexus disorders (G50-G59); Polyneuropathies and other disorders of the peripheral nervous system (G60-G65); Diseases of myoneural junction and muscle (G70-G73); Cerebral palsy and other paralytic syndromes (G80-G83); Other disorders of the nervous system (G89-G99);

Diseases of the eye and adnexa & Diseases of the ear and mastoid process - ICD10 Codes H00-H95 ("Eye and Ear") include: Disorders of eyelid, lacrimal system and orbit (H00-H05); Disorders of conjunctiva (H10-H11); Disorders of sclera, cornea, iris and ciliary body (H15-H22); Disorders of lens (H25-H28); Disorders of choroid and retina

(H30-H36); Glaucoma (H40-H42); Disorders of vitreous body and globe (H43-H44); Disorders of optic nerve and visual pathways (H46-H47); Disorders of ocular muscles, binocular movement, accommodation and refraction (H49-H52); Visual disturbances and blindness (H53-H54); Other disorders of eye and adnexa (H55-H57); Intraoperative and postprocedural complications and other disorders of eye and adnexa (H59); Diseases of external ear (H60-H62); Diseases of middle ear and mastoid (H65-H75); Diseases of inner ear (H80-H83); Other disorders of ear (H90-H94); Other intraoperative and postprocedural complications and disorders of ear and mastoid process (H95).

Diseases of the circulatory system - ICD10 Codes I00-I99 ("Circ. System") include: *Heart Disease: Acute rheumatic fever (I00-I02); Chronic rheumatic heart diseases (I05-I09); Hypertensive diseases (I10-I16);Ischemic heart diseases (I20-I25); Pulmonary heart disease and diseases of pulmonary circulation (I26-I28); Other forms of heart disease (I30-I52);* Cerebrovascular diseases (I60-I69); Diseases of arteries, arterioles and capillaries (I70-I79); Diseases of veins, lymphatic vessels and lymph nodes, not elsewhere classified (I80-I89); Other and unspecified disorders of the circulatory system (I95-I99);

Diseases of the respiratory system - ICD10 Codes J00-J99 ("Resp. System") include:

Acute upper respiratory infections (J00-J06); Influenza and pneumonia (J09-J18); Other acute lower respiratory infections (J20-J22); Other diseases of upper respiratory tract (J30-J39); Chronic lower respiratory diseases (J40-J47); Lung diseases due to external agents (J60-J70); Other respiratory diseases principally affecting the interstitium (J80-J84); Suppurative and necrotic conditions of the lower respiratory tract (J85-J86); Other diseases of the pleura (J90-J94); Intraoperative and postprocedural complications and disorders of respiratory system, not elsewhere classified (J95); Other diseases of the respiratory system (J96-J99);

Diseases of the digestive system - ICD10 Code: K00-K95 ("Digestive System") include: Diseases of oral cavity and salivary glands (K00-K14); *Diseases of esophagus, stomach and duodenum (K20-K3) ("Stomach"); Diseases of appendix (K35-K38) ("Appendix"); Hernia (K40-K46) ("Hernia"); Noninfective enteritis and colitis (K50-K52); Other diseases of intestines (K55-K64) ("Intestines");* Diseases of peritoneum and retroperitoneum (K65-K68); *Diseases of liver (K70-K77) ("Liver Disease")*; *Disorders of gallbladder, biliary tract and pancreas (K80-K87) ("Gall, Biliary & Pancreas")*; Other diseases of the digestive system (K90-K95);

Diseases of the skin and subcutaneous tissue - ICD10 Codes L00-L99 ("Skin & Subcut. Tissue") include: L00-L08Infections of the skin and subcutaneous tissue Bullous disorders (L10-L14); Dermatitis and eczema (L20-L30); Papulosquamous disorders (L40-L45); Urticaria and erythema (L49-L54); Radiation-related disorders of the skin and subcutaneous tissue (L55-L59); Disorders of skin appendages (L60-L75); Intraoperative and postprocedural complications of skin and subcutaneous tissue (L76); Other disorders of the skin and subcutaneous tissue (L80-L99)

Diseases of the musculoskeletal system and connective tissue - ICD10 Codes M00-M99 ("Musculoskeletal System") include: Arthropathies (M00-M25); Dentofacial anomalies [including malocclusion] and other disorders of jaw (M26-M27); Systemic connective tissue disorders (M30-M36); Dorsopathies (M40-M54); Soft tissue disorders (M60-M79); Osteopathies and chondropathie (M80-M94); ther disorders of the musculoskeletal system and connective tissue (M95); Other intraoperative and postprocedural complications and disorders of musculoskeletal system (M96); Periprosthetic fracture around internal prosthetic joint (M97); Biomechanical lesions, not elsewhere classified (M99);

Diseases of the genitourinary system - ICD10 codes N00-N99 ("Genitourinary System") include: Glomerular diseases (N00-N08); Renal tubulo-interstitial diseases (N10-N16); Acute kidney failure and chronic kidney disease (N17-N19); Other disorders of kidney and ureter (N20-N23); Urolithiasis (N25-N29); Other diseases of the urinary system (N30-N39); Diseases of male genital organs (N40-N53); Disorders of breast (N60-N65); Inflammatory diseases of female pelvic organs (N70-N77); Noninflammatory disorders of female genital tract (N80-N98); Other intraoperative and postprocedural complications and disorders of genitourinary system (N99)

Injury, poisoning and certain other consequences of external causes - ICD10 Codes S00-T88 ("Injuries") include: Injuries to the head (S00-S09); Injuries to the neck (S10-S19); Injuries to the thorax (S20-S29); Injuries to the abdomen, lower back, lumbar spine, pelvis and external genitals (S30-S39); Injuries to the shoulder and upper arm (S40-S49); Injuries to the elbow and forearm (S50-S59); Injuries to the wrist, hand and fingers (S60-S69); Injuries to the hip and thigh (S70-S79); Injuries to the knee and lower leg (S80-S89); Injuries to the ankle and foot (S90-S99); Injury, poisoning and certain other consequences of external causes (T07-T88);

Symptoms and signs involving the circulatory and respiratory systems - ICD10 Codes R00-R09 ("Circ./Resp. Symptoms")

Symptoms and signs involving the digestive system and abdomen - ICD10 Codes R10-R19 ("Dig. System Symptoms")