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The lasting health impact of leaving school in a bad economy: Britons in the 1970s recession

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The lasting health impact of leaving school in a bad economy: Britons in the 1970s recession[1]

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Abstract: This paper investigates whether leaving school in a bad economy deteriorates health in the long run. It focuses on low-educated individuals in England and Wales – specifically, individuals who left full-time education in their last year of compulsory schooling – who entered the labour market immediately after the 1973 oil crisis. Unemployment rates sharply increased in the wake of this crisis, such that between 1974 and 1976, each school cohort faced worse economic conditions at labour-market entry than did the previous cohort. Our identification strategy relies on the comparison of very similar pupils – born in the same year and having a similar quantity of education – whose school-leaving behaviour in different economic conditions was exogenously implied by compulsory schooling laws. We provide evidence that, unlike school-leavers who did postpone their entry into the labour market during the recessions of the 1980s and 1990s, pupils' decisions to leave school at compulsory age immediately after the 1973 oil crisis were not endogenous to the contemporaneous economic conditions at labour-market entry. We use a repeated cross section of individuals over the period 1983-2001 from the General Household Survey (GHS) and adopt a lifecourse perspective, from 7 to 26 years after school-leaving. Our results show that poor economic conditions at labour-market entry are particularly damaging to women's health. Women who left school in a bad economy are more likely to report poorer health and to consult a general practitioner over the whole period under study (1983-2001). Additional evidence suggests that they are also more likely to suffer from a longstanding illness/disability over the whole period. For men, the health impact of poor economic conditions at labour-market entry is less obvious and not robust to all specifications.

Keywords: health, school-leaving, macro-economic shocks

JEL Classification: C010, I10.

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Les effets persistants sur la santé de finir ses études quand les conditions économiques se dégradent : les britanniques face à la crise du début des années 1970

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Abstract : Nous nous intéressons à l'entrée sur le marché du travail lorsque la conjoncture économique se dégrade, et aux conséquences sur la santé que cela engendre à long terme. Il y a en effet de bonnes raisons de supposer que sortir du système scolaire dans une conjoncture dégradée ait un effet négatif et persistant sur la santé. Pour tester cette hypothèse, nous mobilisons des données Britanniques issues de la General Household Survey (GHS) sur la période 1983-2001. Nous considérons des individus quittant le système scolaire à l'âge minimum de scolarité obligatoire. Afin de créer un choc exogène sur le timing d'entrée sur le marché du travail (et sur les conditions économiques prévalentes à ce moment-là), nous exploitons (i) la forte hausse des taux de chômage générée par la crise pétrolière de 1973 et (ii) des règles de scolarité obligatoire en Angleterre et au Pays de Galles. Nos résultats montrent que les femmes ayant quitté le système scolaire lorsque la conjoncture s'était fortement dégradée sont en plus mauvaise santé sur toute la période 1983-2001, c'est-à-dire de 7 à 26 ans après l'entrée sur le marché du travail. Nos résultats pour les hommes vont dans le même sens, mais sont plus mitigés, car moins robustes selon les spécifications.

Mots-clefs : santé, sortie du système scolaire, chocs macroéconomiques

JEL Classification : C010, I10.

1 Introduction

Recent research in health economics shows that socioeconomic circumstances during infancy and early childhood have a bearing on health outcomes and mortality later in life (Almond, 2006; Kesternich et al., 2014; Lindeboom et al., 2010; Van den Berg et al., 2006). There is growing evidence there are critical periods for health – not only in utero and early infancy but also during childhood and young adulthood – when environmental exposure may do more damage to health and long-term health potential than they would at other times (WHO, 2000). This paper investigates whether leaving full-time education in a bad economy is such a critical period for health. This is an important question from a policy perspective, as youths suffered disproportionately during the Great Recession (Bell and Blanchflower, 2011). Young cohorts who left full-time education in the late 2000s faced historically high unemployment rates and have experienced difficulties in accessing employment. To the extent that leaving school in a bad economy entails a lasting and negative impact on health, this situation will most likely generate important health disparities in the future.

The idea that poor economic conditions at school-leaving[1] may lead to lower health in the long run is grounded in two empirical patterns. First, empirical evidence shows that leaving school in a bad economy has a negative and somewhat persistent effect on labour-market outcomes – as captured by wages[2], employment prospects[3] or inactivity patterns[4]. In essence, those who graduate in bad economies suffer from underemployment and are more likely to experience job mismatching because they have fewer jobs from which to choose (Kahn, 2010). They may initially be placed in lower-level jobs with less important tasks and fewer promotions (Gibbons and Waldman, 2006) and be persistently locked into low-quality jobs.[5] Second, there is both theoretical and empirical evidence that labour-market outcomes and job quality influence health. Income and higher life-time earnings are generally thought to improve health.[6] Job loss is associated with lower health, adverse health behaviours and higher mortality rates[7], while other job-quality dimensions – such as job stress, occupational injury risk, perceived job insecurity, long working hours, harmful working conditions, and downward occupational mobility – have been shown to deteriorate health.[8] As a result, one may expect that leaving school in a bad economy may have a negative and lasting impact on health through the cumulative impact of worse career outcomes. Beyond labour-market outcomes, another channel whereby poor economic conditions at labour-market entry may

[1]We use the phrase "school-leaving" or "leaving school" in this paper to mean leaving full-time education.

[2]See Kahn (2010) and Oreopoulos et al. (2012). The authors show that graduating from college during a recession has a large, negative and persistent effect on men's wages in the USA and Canada. According to Kahn (2010), the catch-up process for wages in the USA can be as long as 15 years. Similar evidence for the USA is provided in Kondo (2007). Interestingly, her results indicate less persistence for low-skilled or disadvantaged workers.

[3]See Gaini et al. (2012); Genda et al. (2010); Stevens (2007). Graduating during an economic downturn decreases men's probability of being employed, especially among the low-educated – although this effect generally fades out over the next few years.

[4]See Hershbein (2012). Female (but not male) US high school graduates who graduated in an adverse labour market are less likely to be in the workforce in the next four years, but the longer term effects are minimal.

[5]See Schoar and Zuo (2011) and Leombruni et al. (2015) for empirical evidence on worse career perspectives (occupational prestige and mobility) and increased occupational injury risk due to poor economic conditions at labour-market entry.

[6]See Currie (2009); Duleep (1986); Galama and Van Kippersluis (2010); Gardner and Oswald (2007); Grossman (1972).

[7]See Browning and Heinesen (2012); Deb et al. (2011); Eliason and Storrie (2009a); Salm (2009); Sullivan and Von Wachter (2009).

[8]See Caroli and Godard (2014); Fischer and Sousa-Poza (2009); Fletcher et al. (2011); Llena-Nozal (2009); Robone et al. (2011).

affect subsequent health is family formation.[9]

In this paper, we examine the impact of leaving full-time education in a bad economy on long-term health in England and Wales. We focus on low-educated individuals – specifically, individuals who left full-time education at the earliest opportunity (i.e. at compulsory age) – who entered the labour market immediately after the 1973 oil crisis. The proportion of pupils who left full-time education at compulsory age was remarkably high in the UK during the 1970s – as high 50% (Micklewright et al., 1989). We employ an innovative identification strategy, which builds on two sources. First, we rely on the comparison of very similar individuals – born in the same year and having a similar quantity of schooling (in months) – whose school-leaving behaviour in different economic conditions was exogenously induced by compulsory schooling laws. Specifically, within a given birth cohort, the earliest opportunity to leave school for pupils born at the end of the calendar year (September to December) was almost a year later than pupils born earlier in the year (January to August). Second, we exploit the sharp increase in unemployment rates generated by the 1973 oil crisis. Between 1974 and 1976, each school cohort faced worse economic conditions at labour-market entry than its predecessor.[10] As a consequence, unlucky pupils born in September-December faced higher unemployment rates at labour-market entry than pupils born in January-August of the same calendar year.

Of course, a potential selection issue concerns the fact that pupils' decisions to leave school at compulsory age between 1974 and 1976 may have been endogenous to the contemporaneous economic conditions at labour-market entry. Prior research links schooling choice to decreased labour-market opportunities (Betts and McFarland, 1995; Card and Lemieux, 2001; Clark, 2011; Gustman and Steinmeier, 1981) and shows that individuals tend to remain in school during economic downturns. We provide evidence, however, that this is not the case in our context. Unlike school-leavers who postponed their entry into the labour market during the recessions of the 1980s and 1990s, we show that pupils' decisions to leave school at the compulsory age between 1974 and 1976 were not endogenous to the contemporaneous economic conditions at labour-market entry. We argue that the 1973 oil crisis was highly unexpected and that pupils who were in their last year of schooling at that time did not anticipate the adverse career effects of leaving school when unemployment rates were high.

We use a repeated cross section of individuals over the period 1983-2001 from the General Household Survey (GHS) and adopt a lifecourse perspective[11], from 7 to 26 years after school-leaving. We investigate the medium- to long-term impact of leaving school in a bad economy on health status, health care and health behaviour. Our results show that poor economic conditions at labour-market entry are particularly damaging to women's health. Women are more likely to report poorer health and have a higher probability of consulting a general practitioner over the whole period (1983-2001). Additional

[9] See Gustman and Steinmeier (1981); Gutiérrez-Domènech (2008); Hashimoto and Kondo (2012); Kondo (2012); Maclean et al. (2015) and Wolbers (2007) for empirical evidence on the relationship between economic conditions at labour-market entry and subsequent family formation.

[10] We focus on pupils who left school at compulsory age between 1974 and Easter 1976 – e.g., the 1958 and 1959 birth cohorts. We do not consider older individuals, as we intend to abstract from the effect of the increase in school-leaving age from 15 to 16 from September 1972 onwards. In our setup, all individuals are affected by the 1972 reform, and hence our identification strategy does not rely on the comparison of pre-reform cohorts with post-reform cohorts.

[11] The GHS employs a new cross-section in each year, and hence, although we cannot track any particular individual over time, we can track birth cohorts.

results suggest that they have a higher propensity to suffer from a longstanding illness or disability. For men, the health impact of poor economic conditions at labour-market entry is less obvious and not robust to all specifications. We also examine the potential mechanisms by which adverse economic conditions at school-leaving may affect subsequent health. We find scarce evidence that leaving school in a bad economy has a persistent impact on labour-market, marriage and fertility outcomes 7 to 26 years after school-leaving. A notable exception is that women who left school in a bad economy have a higher probability of being single.

This paper relates to several strands of literature. First, it contributes to the emerging literature investigating the long-term health consequences of graduating in a bad economy. To our knowledge, only a very limited number of studies (Cutler et al., 2015; Hessel and Avendano, 2013; Maclean, 2013) have addressed this question. The results obtained thus far have been mixed. Maclean (2013) uses the National Longitudinal Survey of Youth 79 (NLSY79) and exploits the variation in school-leaving state unemployment rates over the period 1976-1992 to identify the effect of leaving school in a bad economy on health at age 40. As the time or location of school-leaving may be endogenous to the contemporaneous unemployment rate, she uses instrumental-variable (IV) methods to address selection problems related to endogenous sorting. Her findings show that men who left school when the state unemployment rate was high have a higher probability of reporting poor or fair health and depressive symptoms and have lower physical functioning at age 40. Surprisingly, she finds that women leaving school in a bad economy tend to have fewer depressive symptoms at age 40. Hessel and Avendano (2013) consider individuals aged 50 and over from the Survey of Health, Aging and Retirement in Europe (SHARE). According to their results, leaving school when the country unemployment rate was high predict worse health status among women and better health status among men. However, the authors acknowledge that both selection into higher education and causation mechanisms may explain this association. Using a similar strategy, Cutler et al. (2015) use Eurobarometer data and consider economic fluctuations over 50 years across 31 countries. They show that higher unemployment rates at graduation are associated with lower income, lower life satisfaction, greater obesity, and more smoking and drinking later in life, for both men and women. In a series of recent papers Maclean (2014a,b,c) specifically tests whether leaving school in an economic downturn persistently affects drinking behaviour, body weight and the probability of having access to employer-sponsored health insurance.

Overall, the evidence provided by the literature is rather mixed. Of course, differences in the age groups considered may account for these conflicting results. Differences in terms of labour markets, social security schemes and social policies between the US and Europe may also play a role. Nevertheless, additional evidence is needed to understand the long-term health consequences of leaving school in a bad economy – and in particular its heterogeneous impact with respect to gender. We contribute to this literature in several ways. First, we develop an innovative identification strategy – quite different in spirit from those previously employed in the literature. We do not consider long periods of economic fluctuations and do not exploit the variation in country (or state) school-leaving unemployment rates as previous studies do. This ensures that our results cannot be biased by country-specific (or state-specific) cohort effects. Our case-study approach takes the post 1973 oil crisis period as an ideal setup, in which the economic conditions faced by early school-leavers quickly and strongly deteriorated. We rely on a comparison of very similar individuals whose school-leaving behaviour in different economic conditions was exogenously induced by compulsory schooling laws. We provide evidence that endogeneous timing at the end of compulsory schooling over the 1974-1976 period is not a threat to our identification. There is thus no need to address problems related to endogenous sorting of school-leaving, and our results do not rely on the (untestable) exogeneity assumptions when implementing instrumental-variables models. Second, our data allow us to adopt a lifecourse perspective, which is only considered in the paper by Cutler et al. (2015). Finally, we focus on low-educated individuals. There are good reasons to focus on pupils leaving school at the compulsory age : first, they represent a sizable proportion of pupils in England and Wales in the mid-1970s (approximately 50%). Second, whether they should be more affected than highly educated individuals by high unemployment rates at labour-market entry – i.e., whether education plays a protective role – is not clear. On the one hand, economic theory predicts less persistence of poor economic conditions at school-leaving for low-skilled workers and those with weak attachment to the labour force (Kondo, 2007). On the other hand, education has been hypothesised to increase one's ability to cope with negative shocks and uncertainty (Cutler et al., 2015; Cutler and Lleras-Muney, 2006; Smith, 2004). Overall, whether and to which extent low-educated individuals' health should be affected by poor economic conditions at career entry remains an open question.

Our study is indirectly related to the literature on the short-term health effect of contemporaneous economic fluctuations. Surprisingly, most studies in this literature indicate that health and health behaviours are countercyclical, at least in the short run (Buchmueller et al., 2007; Gerdtham and Ruhm, 2006; Neumayer, 2004; Ruhm, 2000, 2003, 2004, 2005). At first glance, these findings seem to challenge the idea that leaving school in an economic downturn deteriorates health. However, we argue that, for a number of reasons, these findings can easily be reconciled with our own results. First, we are interested in the long-run consequences of contemporaneous economic fluctuations – as opposed to short-run consequences. Second, we focus on a critical period – the first entry into the labour market.[12] Finally, the impact of contemporaneous macroeconomic conditions is highly heterogeneous across worker's ex ante employment probabilities. There is evidence that individuals at a high risk of unemployment are more likely to adopt risky health behaviours and to suffer from depressive symptoms in bad times (Charles and DeCicca, 2008; Dave and Kelly, 2012; Dee, 2001). As early school-leavers are typically at a high risk of unemployment, these results can easily be reconciled with the idea that leaving school in an economic downturn is bad for one's health, in both the short and the long run.

The remainder of the paper is organised as follows. Section 2 presents the institutional framework

[12] In our setup, pupils in the treatment group (i.e., those who left school in a bad economy) and pupils in the control group are born in the same year. As a result, they face identical contemporaneous economic conditions throughout their lives. The only difference between the two groups is the timing of school-leaving – and hence the economic conditions faced at labour-market entry. Even if the 1973 oil recession was good for these pupils' health, the impact of leaving school in a bad economy may nevertheless be negative, and our setup will allow us to measure it.

and Section 3 the empirical approach. Section 4 describes the data that we use. Section 5 reports our results and Section 6 concludes.

2 Institutional framework

This section describes the compulsory schooling laws in England and Wales (see section 2.1) and provides graphical evidence of the sharp increase in unemployment rates after the 1973 oil crisis (see section 2.2).

2.1 Compulsory schooling in England and Wales

The British compulsory schooling laws specify the maximum age at which pupils are required to begin school and the minimum age at which pupils are allowed to leave school.

The official school-starting age is the beginning of the term starting after the child's fifth birthday. Hence, entry rules dictate that a school cohort consists of children born between the first day of September and the last day of August in the following calendar year (Del Bono and Galinda-Rueda, 2007). In other words, due to the discontinuity introduced by the school-entry rule, students within a single birth cohort belong to different school cohorts. There is evidence that compliance with the school-entry requirement is nearly perfect and that grade repetition (or grade skipping) is nearly non-existent in England and Wales (Grenet, 2013; Sharp et al., 2002).

Two increases were made to reach the current school leaving age of 16 in England and Wales[13], from age 14 to 15 in 1947 and from age 15 to 16 in 1972.[14] The proportion of children leaving education at the first legal opportunity in the UK is high by the standards of other industrialised countries (Micklewright et al., 1989). In our data, this proportion amounts to 50% in the mid-1970s. After the 1972 Raising Of the School-Leaving Age (ROSLA), students in their last year of compulsory schooling were normally attending secondary school (Year 11), while the less academically inclined were in vocational training. Two types of qualifications could be obtained at the end of Year 11 : the General Certificate of Education Ordinary Level (GCE O level) or the Certificate of Secondary Education (CSE). Both credentials were awarded at the end of junior secondary school, after an examination (Grenet, 2013).

Unlike other countries – such as the USA – the implementation of compulsory schooling in England and Wales differs in that a student is not allowed to leave school on the exact date (birthday) on which she reaches the school-leaving age. Between school years 1963-1964 and 1996-1997, (see the Education Act of 1962, Appendix E), the rules governing school exit implied that pupils who reached age 16 between the 1^{st} of September and the 31^{st} of January had to complete their education until the following Easter. Students who reached the age of 16 between the 1^{st} of February and the end of August were forced to leave school at the end of the summer term, typically in May/June. Pupils born between the end of the summer term and August – i.e., pupils born in July or August – were thus allowed to leave school before

[13] The education system in Scotland is different and not considered here.

[14] Several studies use these changes in minimum school-leaving age to identify the returns to education on labour-market outcomes and health (Clark and Royer, 2013; Devereux and Hart, 2010; Grenet, 2013; Harmon and Walker, 1995; Oreopoulos, 2006). Note that in our setup, however, all individuals are affected by the 1972 ROSLA reform. Our identification strategy does not rely on a comparison of pre-reform cohorts and post-reform cohorts.

their 16^{th} birthday, i.e., at age 15.

To show how these exit rules support our identification strategy, we present in Figure 1 the earliest opportunity to leave school with respect to students' month-year of birth. The figure makes it clear that students born in the same calendar year belonged to different school cohorts due to the discontinuity introduced by the school-entry rule (see column 3). It provides evidence that, within the same birth cohort, the oldest pupils – born between January and August – were allowed to leave school at Easter or in May/June of year *t*, whereas the youngest – born between September and December – were not allowed to leave school until the following Easter of year *t+1*. Figure 2 provides an illustration of how the compulsory schooling rules operate by taking the 1958 birth cohort as an example. Note that, due to the discontinuities introduced by both school-entry and school-exit rules, pupils born in different months had a similar quantity of schooling (in months) at the end of full-time education.[15]

2.2 Unemployment rates

The sharp and unprecedented increase in the oil price from three to ten dollars per barrel in October 1973 had serious effects on the balance of payments of the industrial nations, which were oil-importing countries. This first world-wide recession had strong effects on unemployment rates in a number of industrialised countries, including the UK (Bhattarai, 2011).[16]

To provide a sense of the shock, the number of 16- and 17-year-olds out of work rose markedly from 33,000 in July 1974 to 104,000 in July 1975 and 199,000 in July 1976 (Brown, 1990). Note that figures taken in July are inevitably higher than at other times of the year because they include school-leavers. Between 1974 and 1976, unemployment rates for individuals under 18 (including school-leavers) rose from 5.2% to 19.1% (for males) and from 3.4% to 18.3% (for females).[17] This marked increase in the youth unemployment rate was much larger than the increase observed in the unemployment rates for all ages over the period 1974-1976 – from 3.3% to 6.9% (males) and 1% to 3.4% (females). Besides, while only 28.15% of those aged 16-17 had sought employment for more than 3 months in the spring quarter of 1975, this proportion amounted to 39% in 1977.[18] Overall, this provides evidence that school-leavers experienced greater difficulties accessing employment throughout the period under study. This situation was all the more drastic because new school-leavers had typically not worked enough to be entitled to unemployment benefits (although insurance rights could begin to be acquired at age 16). They did

[15] A maximum difference of three months of education upon reaching the final year of schooling was induced by the existence of two specific school-leaving dates (Easter or the end of the summer term). We find it highly unlikely, however, that this three-month difference in compulsory schooling should have an impact on health – especially as Clark and Royer (2013) show that the additional year of schooling induced by the 1972 ROSLA reform had no effect on health whatsoever.

[16] In this context, it can reasonably be argued that the 1973 crisis was not endogenous to health in the UK.

[17] Increases in unemployment rates under 18 excluding school-leavers lay in the same range of magnitude (from 3.1% in 1974 to 9.8% in 1976 (for males) and 2.0% in 1974 to 9.4% in 1976 (for females)). Unemployment rates are borrowed from Wells (1983). To compute unemployment rates, numbers of unemployed under 18 were expressed as a percentage of the sum of the number of employees in employment under 18 and the unemployed under 18. The estimated numbers of registered unemployed under 18 exclusive of school-leavers are obtained by substracting the *mid-year count* (i.e. July) of unemployed school-leavers from the appropriate figures. The figures inclusive of school-leavers are obtained by adding the *annual average* of unemployed school-leavers to the exclusive figures. Further details on the method used to calculate unemployment rates including and excluding school-leavers can be found in Wells (1983).

[18] These figures are based on our own computations from the 1975 and 1977 waves of the UK Labour Force Survey (LFS). As the LFS survey was conducted in the spring quarter (March/May), these unemployment rates reflect the state of the labour market at the time school-leavers entered the labour market.

fall within the eligibility criteria for the Supplementary Benefit, which was a means-tested benefit but intended only to "top-up" other benefits (Brown, 1990).

Figure 3 provides graphical evidence of the sharp increase in youth unemployment after the 1973 oil crisis. The 1973 oil crisis – which occurred in October 1973 – is represented by the vertical dark red line on the left-hand side. The Figure shows the unemployment rates for individuals under 18 on a yearly basis over the 1971-1980 period. The blue (green) line represents the unemloyment rates inclusive of school-leavers for men (women), while the black (red) line represents the unemployment rates exclusive of school-leavers for men (women). As shown in Figure 3, male and female unemployment rates increased sharply between 1974 and 1977-1978 – when the economy recovered – with the sharpest increase occurring between 1974 and 1976. As made clear by the graph, each school cohort entering the labour market faced significantly higher unemployment rates than did the previous school cohort.

3 Empirical approach

Section 3.1 presents our main identification strategy and the model we estimate. Section 3.2 discusses the validity of this identification strategy, and Section 3.3 presents a placebo test and introduces a difference-in-differences strategy.

3.1 Identification strategy and model

Our identification strategy considers pupils (i) who left school at the minimum school-leaving age (ii) and who entered the labour market between Easter 1974 and Easter 1976, i.e., the 1958 and 1959 birth cohorts. Note that in our setup, all individuals are affected by the 1972 reform, and hence our identification strategy does not exploit the increase in school-leaving age from 15 to 16. We rely on the comparison of similar individuals – born in the same year and having a similar amount of education (in months) – whose school-leaving behaviour in different economic conditions was exogeneously induced by compulsory schooling laws. Specifically, in each birth cohort, pupils born at the end of the calendar year (September to December) were allowed to leave school almost a year later than pupils born earlier in the year (January to August) – and faced worse economic conditions at labour-market entry. Note that our identification strategy does not rely on the comparison on individuals who left school *before* and *after* 1973. In our setup, all individuals are affected by the 1973 oil crisis. However, some pupils (the "treated") left school in worse conditions than otherwise similar pupils.[19]

We use a repeated cross-section of individuals over the period 1983-2001 to estimate the following equation by standard probit[20], for men and women separately :

$$H_i^* = \alpha + \gamma T_i + BirthYear_i + f(BirthMonth_i) + InterviewYear_i + \epsilon_i \tag{1}$$

[19]The 1973 oil crisis had a disproportionate impact on some regions – typically in Wales and the North of England. However, we do not exploit this additional regional variation. First, we do not have reliable data on UK regional unemployment rates at a sufficiently disaggregated level for the 1970s. Second, we do not have information on the actual region in which the individual lived at age 16.

[20]All health outcomes are binary variables. Further details are available in the data section.

where H_i^* denotes the latent health status of individual i and is only observed as:

$$H_i = \mathbb{1}_{\{H_i^* > 0\}} \tag{2}$$

and where T_i is a dummy variable taking value 1 if individual i is treated, i.e., born between the 1^{st} of September and the 31^{st} of December, and value 0 if non-treated, i.e., born between the 1^{st} of January and the 31^{st} of August. $BirthYear_i$ is a dummy variable for individual i's year of birth. $InterviewYear_i$ is a dummy variable for individual i's interview year.[21] $f(BirthMonth_i)$ is a linear function of age in months within a birth year.[22] We define it as $(12 - BirthMonth_i)$, where $BirthMonth_i$ denotes the month of birth of respondent i and varies from 1 (January) to 12 (December). We include this linear function of age in Equation (1) to account for the fact that within each birth cohort, treated pupils (born September-December) are younger than non-treated pupils (born January-August).[23] As age and health are negatively correlated, failing to account for this age difference – which is a difference in months within a birth cohort – may lead us to underestimate the negative impact of leaving school in a bad economy.[24] Finally, ϵ_i denotes the error term.

Equation (1) estimates the average effect of leaving school in a bad economy on health over the whole period ($\widehat{\gamma}$). However, our empirical approach also allows us to adopt a lifecourse perspective. To do so, we compute the marginal effects of the treatment associated with each interview year over the period 1983-2001.[25] This allows us to investigate whether the impact of poor economic conditions at labour-market entry on health is driven by medium- or long-term effects.

3.2 Validity of the identification strategy

A key assumption is that pupils in their last year of compulsory schooling do not strategically remain in school when the economy deteriorates (see Section 3.2.1). A second identifying assumption is that apart from school-exit rules, no other institutional differences generates differences in health between the treated and the control within each birth cohort (see Section 3.2.2). Finally, to the extent that individuals born between January and August and individuals born between September and December are identical in all observable and unobservable characteristics (see Section 3.2.3) we can safely attribute observed differences in health to the impact of labour-market conditions at labour-market entry.

[21] We control for *InterviewYear* to account for the fact that we observe individuals at different points in time. We choose to include a dummy indicating the year in which an individual is interviewed rather a dummy indicating the survey wave because a survey wave can be conducted over several years – usually two.

[22] Note that when allowing for more flexible specifications of $f(BirthMonth_i)$ (e.g., a change in the slope of $f(BirthMonth_i)$ at the August/September discontinuity; a quadratic specification), the results go in the same direction (results are available upon request).

[23] One might be concerned that introducing simultaneously the variables T_i, $BirthYear_i$ and $(12 - BirthMonth_i)$ in Equation (1) could lead to multicollinearity issues. When estimating Equation (1), we find that the VIF (Variance Inflation Factor) criterion is lower than 10 for all variables, suggesting inconsequential multicollinearity (see the rule of thumb provided by Hair et al. (1995)).

[24] As expected, estimating Equation (1) without the linear function of age in months yields very similar estimates, although of lower magnitude and less significant (results are not reported but available upon request).

[25] Specifically, we estimate Equation (1) and substitute the interaction term $T_i * InterviewYear_i$ for T_i. Interview-year-specific marginal effects correspond to the estimated marginal effects associated with the interaction terms.

3.2.1 Endogenous timing of school-leaving

Time of school-leaving may be endogenous to the contemporaneous economic conditions. The sign of the bias arising from endogenous timing, however, is difficult to predict. On the one hand, school-leavers who avoid leaving school in a bad economy may have unobserved characteristics (e.g., financial resources, other parental characteristics) that allow them to postpone their entry into the labour market. On the other hand, it is likely that only the most capable and hardworking are able to leave school during a bad economy because their abilities allow them to secure desirable jobs regardless of the economic conditions. These characteristics may be correlated with subsequent health, in which case our estimates will be biased.

Whether pupils in their last year of compulsory schooling strategically remain in school when the economy deteriorates is an empirical question. For each birth cohort, Figure 4 shows the proportion of pupils who left school at the compulsory age among the treated and non-treated groups. It also depicts the one-year growth in school-leaving unemployment rates (calculated for the March-June period) faced by the youngest school cohort (treated) – relative to the previous school cohort (non-treated).[26] When considering the 1958 and 1959 birth cohorts, Figure 4 shows that within each birth cohort, the proportion of pupils who left school at the compulsory age among the treated and the non-treated group is equal, indicating that school-leaving behaviour in last year of compulsory schooling was not shaped by the sharp increase in unemployment rates generated by the 1973 oil crisis.[27] When considering younger birth cohorts, however, we do find that a sharp increase in the unemployment rate (e.g., the recessions of the 1980s and 1990s) is associated with a significant decrease in the proportion of treated pupils leaving school at the compulsory age.[28] When regressing the individual probability of leaving school at the compulsory age on the series of birth dummies and the series of interaction terms *treatment*birth dummy*, the coefficients associated with the interaction terms for the 1958 and 1959 birth cohorts are not significant. In contrast, a number of interaction terms are significant for birth cohorts graduating during recessions of the 1980 and 1990s (the 1963-1964 and 1973-1974 cohorts, specifically).

In summary, we find no evidence that school-leavers born in 1958-1959 – the cohorts that we consider – exhibit endogenous timing in their school-leaving behaviour. It can be hypothesised that pupils in their last year of compulsory schooling in 1974-1976 did not anticipate the adverse consequences of high unemployment rates at labour-market entry – contrary to school-leavers during the recessions of the 1980s and 1990s. Moreover, as a large share of pupils was leaving school at the earliest opportunity in the 1970s, and it can be hypothesised that the compulsory age was still binding at that time.

[26] Unfortunately, unemployment rates for individuals under 18 are not available in a consistent way after 1981 from Wells (1983). We use instead unemployment rates on a monthly basis for all ages from administrative unemployment statistics – namely, the monthly "registrant count" (borrowed from Denman and McDonald (1996)).

[27] One might argue that even if the *proportion* of pupils who left school at the compulsory age is equal across the treated and non-treated groups, the *composition* of each group might be different. Due to the lack of information on individual characteristics at age 16, we cannot test this assumption in a proper way. Although the GHS includes information on father's occupation, this variable is not reliable and has many missing values over the study period, and we cannot use it.

[28] Interestingly, this result suggests that rather than high unemployment rates or even increasing unemployment rates, it is a sharp increase in unemployment rates – typically occurring during recessions – that induces endogenous timing among pupils in their last year of education.

3.2.2 School-entry rules – Differential incentives to take GCE O-level/CSE examinations

School-entry rules introduce a discontinuity between August-born and September-born children. This discontinuity implies that within a given birth cohort, students belong to different school cohorts. This institutional feature may generate differences in health between treated and untreated pupils by means of age-relative rank, school-cohort size or job-experience effects. A detailed discussion of this issue can be found in the online appendix (see Appendix C). Overall, the combined outcome of age-relative rank, school-cohort size or job-experience effects (i.e., whether we should over or underestimate the true health impact of poor economic conditions at career entry) is far from clear.

Differential incentives to take GCE O-level/CSE examinations. Depending on their date of birth (before or after January 31^{st}), individuals within a given school cohort were allowed to leave school only after one of two specific dates (Easter or the end of the summer term) upon reaching their final year of schooling. Pupils who left school at the end of the summer term, however, had greater incentives to take the exam at the end of Year 11 (O-Level/CSE qualifications) in which they could be awarded nationally recognised qualifications.[29] In this context, the January/February discontinuity might introduce a bias into our analysis : treated pupils (born between September and December) allowed to leave at Easter have less of an incentive to take the exam at the end of the year. It might impact their educational achievement and their adult labour-market and health outcomes.
In the robustness section, we verify that this differential incentive for taking the exams at the end of Year 11 is not likely to bias our results.

3.2.3 Season-of-birth effects

Our identification strategy assumes that individuals born between January and August and individuals born between September and December are identical in all observable and unobservable characteristics. Yet, a growing body of literature has shown the importance of season-of-birth effects on subsequent labour and health outcomes (Bound and Jaeger, 1996) – see for instance Kestenbaum (1987) or Doblhammer and Vaupel (2001). A detailed discussion of that matter can be found in Appendix C.

3.3 Placebo test and difference-in-differences approach

In this section, we check that our estimates are not systematically biased by differences between September-December-born children and January-August-born children. As a first step, we run a placebo test on the 1953-1954 birth cohorts. The 1953-54 birth cohorts faced very similar school-leaving unemployment rates at the end of compulsory schooling. Moreover, the same schooling rules applied for these cohorts (see the 1962 Education Act, Appendix E), except that the minimum school-leaving age was then 15.

[29] Del Bono and Galinda-Rueda (2007) exploit this January/February discontinuity in a regression discontinuity design and estimate the impact of three additional months of compulsory schooling on educational attainment and longer term labour-market outcomes. In this paper, we do not exploit this January/February discontinuity for two reasons : (i) unemployment rates do not vary enough between Easter and the end of the summer term, (ii) and our sample would probably be too small to detect any effect.

School-leaving unemployment rates (averaged over March-June) varied from 2.475 to 2.675 over a three-year period (1968-1970).[30] Importantly, all pupils born in 1953-1954 who left school at the compulsory age did so after the major events of 1968. As a sensitivity check, we run additional placebo tests on alternative cohorts (see Section 5.3).

As a second step, we implement a difference-in-differences analysis. This strategy allows us to eliminate any systematic differences between September-December-born children and January-August-born children (e.g., job experience, season-of-birth, or any other time-invariant characteristic). We use the 1953-1954 cohorts as a "control" group and estimate the following equation by a linear probability model :

$$H_i = \alpha + \chi T_i + \delta D_i + \beta T_i \times D_i + BirthYear_i + f(BirthMonth_i) + InterviewYear_i + \epsilon_i \qquad (3)$$

where D_i is an indicator variable taking value 1 if individual i is born in 1958-1959 and value 0 if born in 1953-1954. $\widehat{\beta}$ is the difference-in-differences estimator. It corresponds to the difference in health between the treated and untreated individuals across the 1958-59 and 1953-54 cohorts. We assume that if the treated had not been subjected to the treatment (i.e., an increase in unemployment rates at school-leaving relative to the previous school cohort), both treated and untreated groups would have experienced the same trend in health (Lechner, 2010).

4 Data

We use data from the General Household Survey (GHS). The GHS is an annual survey of over 13,000 households and a nationally representative survey of private households in Great Britain.[31] It ran from 1972 to 2011 as a repeated cross-sectional survey.

Among other data, it includes information on demographics (including month-year of birth from 1983 to 2001[32], the survey waves that we use), education (including the age at which the individual left full-time education and the highest qualification obtained), labour-market characteristics (including earnings and employment status) and health (including health status, health care and health behaviours). Importantly, a number of the GHS respondents left full-time education immediately after the 1973 oil crisis.

We restrict our sample to all individuals born in 1958 and 1959 and who left full-time education at the earliest opportunity. By doing so, we consider individuals who entered the labour market between Easter 1974 and Easter 1976. We further exclude truands, i.e., pupils who left full-time education before the compulsory age, and pupils born in July/August.[33] We also exclude individuals reporting that they never

[30] Unemployment rates are computed from Denman and McDonald (1996), see footnote 26.

[31] See Data Appendix D.1 for more information on the sampling procedures and sample sizes in GHS.

[32] Month and year of birth in 1983-1985 are only available for women who completed the Family Information section. They are available for all respondents over the period 1986-2001.

[33] Patterns of school leaving seem to be quite different for pupils born in July/August. In particular, the proportion of pupils who left full-time education at the compulsory age (i.e., at age 15) among those pupils is significantly lower than the proportion of pupils who left full-time education at compulsory age (i.e., at age 16) among other pupils. This may be because employers were reluctant to hire individuals under age 16. Being a 15-year-old at labour-market entry can be interpreted as a negative signal (e.g., being a truand). By excluding all individuals born in July/August from our sample, leaving school at age 16 becomes equivalent to leaving school at the compulsory age. Results are robust to re-introducing

went to school or individuals whose highest qualification was equivalent to Year 12 or more.[34] Finally, we restrict our sample to individuals who lived in England and Wales at the time the survey was conducted because we examine school-leaving rules operating in these countries.[35] As the outcomes of interest are not collected consistently over the period (see Data Appendix D.2), we include all possible observations for each outcome to maximise sample size. Overall, our sample consists of a maximum of 1096 men over the 1986-2001 period and 1921 women over the period 1983-2001.

In addition to the variables mentioned above – month-year of birth, the age at which the individual left full-time education, the highest degree obtained and the region in which she lives – we use some information on health status, health care and health behaviour. The GHS contains several health indicators that are comparable over the 1983-2001 period. They include self-reported health status, dichotomised as poor (fair or bad health) versus good health, the presence of a longstanding illness or disability and whether the respondent restricted his activity during the two weeks preceding the interview due to illness or injury. In addition, we create several dummy variables indicating whether the respondent consulted a General Practitioner (GP) during the two weeks preceding the interview or whether she went to hospital as an outpatient/inpatient during the twelve months preceding the interview. The GHS also includes self-reported health behaviours such as smoking and drinking (which are measured in alternate years). Finally, the GHS provides information on the economic status of the individual – employed or self-employed, unemployed according to the ILO definition, keeping house, and other statuses, e.g., other unemployed, permanently disabled, student, retired or other. For those currently employed or self-employed, it indicates the time with the present employer. Finally, we use the usual gross weekly earnings from the main job (available and measured in a consistent way over the 1984-2001 period) as our main measure of earnings.[36] Finally, we use some information regarding marital history and fertility : marital status, age at first marriage (conditional on having been married), number of children (only for women) and age at first birth (only for women and conditional on having at least one child).

Summary statistics for all the variables mentioned above are shown separately by gender in Tables 1, 2 and 3. We also provide a breakdown of our sample by survey wave and birth cohort in Table 4.

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these observations.

[34]Obviously, individuals who left full-time education at the compulsory age could have engaged in further education and obtained higher degrees in adulthood. However, the proportion of individuals reporting Year 12-equivalent or higher degrees amounts to 20% in our sample, which is surprisingly high. One concern could be that the reported age at which individuals left full-time education suffers from measurement error. To minimise measurement error in this variable, we exclude individuals whose highest qualification was equivalent to Year 12 or more. Results are robust to re-introducing these observations (although the results for men become marginally significant in the main specification).

[35]Our data do not allow us to take into account migration patterns from Scotland or Northern Ireland, which is likely to generate some noise.

[36]Usual gross weekly earnings are measured in pounds. When running regressions, we take the logarithm of earnings as the outcome variable.

5 Results

5.1 The impact of leaving school in a bad economy on health

In this section, we successively present our main results (see Section 3.1), the placebo test and the results obtained when implementing a difference-in-differences approach (see Section 3.3).

5.1.1 Main results

To draw general conclusions regarding the impact of leaving school in a bad economy, we first present findings for summary indices that aggregate information over multiple treatment effect estimates. We then present the estimates for specific health outcomes. The aggregation improves statistical power to detect effects that go in the same direction within a domain (Kling et al., 2007). For each sex, we create an index of "poor health" that averages the five dichotomous measures of health (i.e., self-rated health, the presence of a longstanding illness, whether the respondent restricted his activity due to illness/injury, whether she consulted a GP and whether she went to hospital as an outpatient/inpatient). A similar index is computed for health behaviour. Following Kling et al. (2007), each summary index is an equally weighted average of the z-scores of the components of the index, with the signs of the measures oriented such that more detrimental outcomes have higher index scores. For each sex, the z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation, and hence the value of the index has mean zero and standard deviation one by construction for the control group. The estimate shows where the mean of the treatment group is in the distribution of the control group in terms of standard deviation units. Estimates of Equation (1) are presented in Table 5 for men and women separately. Each line presents the marginal effect (resp. standard error and number of observations used in the model) of having left school in a bad economy for a different health outcome. The first two lines present our results for the two summary indices,[37] and the remaining lines display the results for specific health outcomes.[38] All of our models include dummy variables for interview and birth years and a linear function of age – see Equation (1). Regarding the summary indices, our results suggest that poor economic conditions at labour-market entry are particularly damaging to women's health over study period (1983-2001). The estimated coefficient in Table 5 implies that leaving school in a bad economy increases women's poor health index by 0.86 standard deviations (at the 5% significance level) relative to women in the control group. This result is confirmed when considering specific health outcomes for women. The marginal effects in Table 5 imply that women who left school in a bad economy have an 11 percentage-point higher probability of reporting poor self-rated health (at the 10% significance level). Consistently, women are also more likely to consult a GP during the last two weeks (a 12 percentage-point probability increase, at the 5% significance level). In contrast, leaving school in a bad economy does not seem to affect their propensity to restrict their activities due to illness or injury, to suffer from a longstanding illness/disability, or to go to the hospital during the 12 months preceding the interview. Leaving school in an economic downturn does not seem to be particularly harmful to women's health

[37] Note that in these cases, Equation (1) is estimated by OLS.
[38] In these cases, Equation (1) is estimated by standard probit.

behaviour, either. For men, our results show that over the whole period (1983-2001), those who left school in a bad economy face a 17 percentage-point increase in the probability of having ever smoked (at the 5% significance level). Regarding the other health outcomes, the marginal effects for men do not appear to be statistically significant at conventional levels.

Figures 5 and 6 present the impact of having left school in a bad economy on health outcomes from a lifecourse perspective. While the estimates in Table 5 provide the average impact of poor economic conditions at labour-market entry over the full period (1983-2001), Figures 5 and 6 investigate whether this impact is driven by medium- or long-term effects. For a given health outcome, the corresponding figure pictures interview-year-specific treatment effects over the period 1983-2000, for men and women separately. For the sake of conciseness, these figures are presented only for the health index and for health outcomes previously found to be significant in Table 5. For instance, Figure 5a (resp. Figure 5b) shows, for the period 1986-2000, the interview-year-specific treatment effect on men's health index (resp. on men's probability of having ever smoked). Correspondingly, Figure 6a (resp. Figure 6b and Figure 6c) shows the interview-year-specific treatment effect on women's health index (resp. on women's probability of reporting poor health and on women's probability of consulting a GP).
Overall, these figures show that the average impact on health of leaving school in a bad economy does not seem to be particularly driven by medium- or long-term effects – for each figure, the majority of the treatment effects lies above the zero line. This suggests that men's smoking behaviour and women's health seem to be negatively and persistently affected by poor economic conditions at labour-market entry over the whole period under study.

5.1.2 Placebo test and difference-in-differences approach

In this section, we show that we can safely attribute the observed health differences between the treated and control to the labour-market conditions at labour-market entry, as opposed to unobservable differences between September-December- and January-August-born children (e.g. age-relative rank, season-of-birth effects, etc.).
As a first step, we re-estimate our probit models on the 1953-1954 cohorts (who faced very similar school-leaving unemployment rates at the end of compulsory schooling). The results are presented in Table 6. As expected, we find no significant effect of the treatment (being born between September and December) on any health outcome. All coefficients are not statistically significant at conventional levels. The same pattern emerges from a lifecourse perspective (see Figures 7a and 7b for men's and women's health index, respectively). If anything, treated individuals born in 1953-1954 seem to be in better health than untreated ones over the study period, as for each figure, the majority of the treatment effects lies below the zero line.

The placebo test has provided initial evidence that our main results were not likely to be biased by any systematic (unobservable) differences between September-December- and January-August-born children. To further investigate this matter, we implement a difference-in-differences (DiD) strategy. This strategy uses the 1953-54 cohorts as a "control" group. It controls for any systematic differences between September-December-born children and January-August-born children. Estimates of Equation (3) are presented in Table 7 separately for men and women. Our results show that poor economic conditions at labour-market entry decrease women's health status by 0.42 standard deviations (at the 15% significance level) relative to women in the control group. In particular, the marginal effects in Table 7 imply that women who left school in a bad economy face a 6 percentage-point increase in the probability of reporting poor self-rated health (at the 10% significance level) over the whole period (1983-2001). Correspondingly, poor economic conditions at labour-market entry increase women's probability of consulting a GP during the previous two weeks by 7.5 percentage points (at the 1% significance level). When controlling for any systematic differences between September-December- and January-August-born children, we find that women are also more likely to suffer from a longstanding illness/disability (a 6 percentage-point increase, significant at the 5% level) over the whole period. Overall, the results obtained for women when implementing a DiD strategy confirm our findings from the main analysis. In particular, the DiD estimates are in the same range of magnitude as those presented in Table 5. Our main results for men, however, are not robust to the DiD specification. The results presented in Table 7 show that the effect of poor economic conditions at labour-market entry on men's smoking behaviour is no longer significant.

Overall, our findings when implementing the placebo test and the DiD strategy make us confident that our main estimates capture the true effect of poor economic conditions at labour-market entry – as opposed to any systematic differences between September-December- and January-August-born children. In particular, women's health seems to be much more affected by poor economic conditions at labour-market entry than men's.

5.2 The impact of leaving school on labour-market, marriage and fertility outcomes

Labour-market, marriage and fertility characteristics can be viewed as mechanisms whereby poor economic conditions at labour-market entry affect health in the long run. To investigate this, we regress labour-market, marriage and fertility proxies on the treatment variable T_i, on year-of-birth and interview dummies and on the linear function of age. Models are estimated by OLS or probit – depending on the nature of the dependent variable (continuous or dichotomous). This analysis deserves an important caveat, however, as we consider potential outcomes at best seven years after the treatment. In that sense, the short-term effects of poor economic conditions at career entry will not be observed in our data.

Table 8 presents the effect of leaving school in a bad economy on labour-market outcomes for men and women separately. We find no effect on unemployment, inactivity patterns or earnings[39] over the study period, neither for men nor for women. While women who left school in a bad economy do not seem to have been in their current job for a shorter period of time, men have a higher probability of being in their current job for less than one month (coeff : 0.074, significant at the 5% level). This result is consistent

[39] Our results hold when estimating Tobit models for earnings (results not reported but available upon request)

with the idea that poor economic conditions at labour-market entry have a negative effect on job tenure. It may also reflect the additional year of job experience among the untreated (who entered the labour market one year earlier relative to treated individuals). This job-experience effect should be captured by our DiD estimates. Indeed, when implementing the DiD model on labour-market proxies, the effect on job tenure (i.e., being in the current job for less than one month) for men vanishes, suggesting that our previous result was mostly driven by job-experience effects. Other DiD estimates (not reported) are very similar to those presented in Table 8.

Overall, we find scarce evidence that leaving school in a bad economy has a strong and lasting impact on labour-market outcomes 7 to 26 years after school-leaving. This is not surprising, however, as we consider potential outcomes at best seven years after the treatment. The catch-up process in earnings or employment prospects may have already taken place by then. This is especially likely to be the case because we consider low-educated individuals. Economic theory indeed predicts less persistence of poor economic conditions at school-leaving for low-skilled workers' subsequent labour-market outcomes. Correspondingly, empirical work shows that the effect of poor economic conditions on unemployment vanishes after a few years (usually four or five) when considering low-educated individuals in Germany, France and the USA – see Stevens (2007), Gaini et al. (2012) and Genda et al. (2010).

Table 9 presents the effect of leaving school in a bad economy on marriage and fertility outcomes for men and women separately. Our results show that leaving school in a bad economy does not seem to have a significant impact on men's marital status or age at first marriage. Women's marital status, however, seems to be significantly affected by poor economic conditions at labour-market entry. In particular, treated women have a higher probability of being single (coeff : 0.081, s.e : 0.044, significant at the the 10% level) and a lower probability of being divorced if once married (coeff :-0.050, s.e :0.026, significant at the the 10% level). The latter effect, however, is no longer found to be significant when implementing DiD models (not reported). Other DiD estimates are otherwise very similar to those presented in Table 8. There is a number of reasons why poor economic conditions at labour-market entry may affect female marriage formation. As Gutiérrez-Domènech (2008) suggests, it may be the case that potential mothers who left school in a bad economy are concerned about future employment opportunities after child bearing and chose to postpone or even skip marriage.

5.3 Robustness Checks

This section performs several robustness checks using our main specification (see Equation (1)).

5.3.1 Differential incentives to take GCE O-level/CSE examinations

One might be concerned that treated pupils have a lesser incentive to take examinations at the end of Year 11. This might impact their educational achievement and subsequent health outcomes. To control for this potential bias, we re-estimate our regressions while controlling for a dummy variable indicating whether the individual holds a Year-11-equivalent qualification (O-level, CSE, etc.). Our results are virtually unchanged.

5.3.2 Alternative empirical approach

Thus far, our treatment variable has been a dummy variable indicating whether an individual was born at the end of the calendar year or earlier in the year (see Equation (1)). A possible disadvantage of this approach is that it linearises the impact of the treatment across the 1958 and 1959 birth cohorts − which may be problematic to the extent that in each birth cohort, treated pupils do not face the same increase in school-leaving unemployment rates relative to non-treated pupils (increases of 8.5 (8.2) percentage points for treated men (women) born in 1958 and increases of 5.4 (6.7) percentage points for treated men (women) born in 1959).

To address this potential problem, we first run a separate regression for each birth cohort. Overall, the results go in the same direction (results not shown). When considering the 1958 birth cohort, our model implies that leaving school in a bad economy increases women's poor health index by 0.69 standard deviations relative to women in the control group (the coefficient, however, is not significant at conventional levels (p-value: 0.20)). The corresponding figure for the 1959 cohort is 1.07 standard deviations, and significant at the 5% level. Figure A1 shows the impact of leaving school in a bad economy on women's health index from a lifecourse perspective, for each cohort separately (see Figures A1a and A1b). Whether we consider the 1958 or the 1959 birth cohort, a simple look at each graph confirms that poor economic conditions at labour-market entry are harmful to women's health over the whole period under study.

As a second step, we estimate the impact of school-leaving unemployment rate on subsequent health. More formally, we estimate the following equation by linear regression or standard probit − depending on the nature of the dependent variable (continuous or dichotomous) :

$$H_i^* = \lambda + \pi UR_i + BirthYear_i + f(BirthMonth_i) + InterviewYear_i + \eta_i \tag{4}$$

where H_i^* denotes the latent health status of individual i and is either fully observed or only observed as a dummy variable. UR_i stands for the school-leaving unemployment rate faced by individual i, and the other variables are presented in section 3.

Estimates of Equation (4) are presented in Table A1. Our estimates in Table A1 imply that a one-point increase in school-leaving unemployment rates leads to a 0.12 standard-deviation increase in women's poor health index (at the 5% level). In particular, a one-point increase in school-leaving unemployment rates leads to a 1.4 percentage-point increase in women's probability of reporting poor health (at the 10% significance level), and an 1.9 percentage-point increase in women's probability of consulting a GP (at the 1% level) among women. For men, a one-point increase in school-leaving unemployment rates leads to a 2.7 percentage-point increase in the probability of having ever smoked (at the 5% significance level).

The results obtained when implementing an instrumental-variable strategy are very similar to the ones presented in Table A1. In the first stage, school-leaving unemployment rate is regressed on the treatment variable (i.e. the instrumentral variable), dummy variables for interview and birth year as well as a linear function of age in months. In the second stage, Equation (4) is estimated by 2SLS and school-leaving unemployment rate is replaced with its predicted value from the first stage. The covariance matrix of $\hat{\pi}$ is corrected accordingly. Not surprisingly, the first-stage results (combined with F-statistics of the excluded instrument above the standard requirement of 10 (Bound et al., 1995)) show that being born between January and August is associated with higher school-leaving unemployment rates in the 1958-1959 birth cohorts (coeffs: 7.2 (7.4) for men (women), significant at the 1% level). The second-stage results are displayed in Table A2 and confirm the harmful impact of poor economic conditions at labour-market entry on women's health.

5.3.3 Additional placebos

Until now, we have used the 1953 and 1954 birth cohorts in our placebo exercise. This section considers alternative placebo cohorts. For the sake of conciseness, it mainly presents graphical evidence of the treatment over the lifecourse using the health index. We first present the results obtained when considering the 1949 and 1950 cohorts, which faced very similar school-leaving unemployment rates at the end of compulsory schooling (see Figures A2a and A3a for men and women, respectively). Consistently with our previous placebo analysis, treated individuals seem to be in better health than untreated ones over the study period, as for each figure, the majority of the treatment effects lies below the zero line.

One may worry, however, that although the same schooling rules applied for the 1949-1959 and 1953-1954 cohorts (see the 1962 Education Act, Appendix E), the minimum school-leaving age was then 15. If the impact of poor economic conditions at labour-market entry interacts with the earliest age at which individuals have the opportunity to leave school, the 1949-1959 or 1953-1954 birth cohorts may not be valid comparison groups. As a sensitivity check, we run placebo tests on the 1960-1961 and 1961-1962 cohorts, for which the minimum school-leaving age was 16. School-leaving unemployment rates were not fully stable over the 1976-1978 and 1977-1979 periods – in fact they were slightly decreasing as the economy was recovering. For men, school-leaving unemployment rates varied from 19.1% in 1976 to 20.2% in 1977, 18.4% in 1978 and 15.3% in 1979. For women, the corresponding figures were 9.8% , 9.7% , 8.7% and 7.1%. Figures A2b and A2c (Figures A3b and A3c) show the interview-year-specific treatment effect on the health index over the study period for men (women). Consistently with the previous placebo analyses, September-December-born children tend to be in better health than those born between January and August over the study period.

Overall, these additional placebos provide evidence that the impact of the treatment observed in the 1958 and 1959 cohorts is driven by worse economic at career entry, as opposed to by systematic differences between September-December- and January-August-born children. When implementing DiD models and using alternatively the 1949-1950, 1960-1961 and 1961-1962 cohorts as "control" groups, the results are similar to the ones obtained when using the 1953-54 birth cohorts as a "control" group (results not shown but available upon request).

6 Conclusion

In this paper, we investigate the impact of leaving school in a bad economy on long-term health status, health care consumption and health behaviour. We consider pupils in England and Wales who left school in their last year of compulsory schooling immediately after the 1973 oil crisis and whose school-leaving behaviour in worse economic conditions was exogeneously induced by compulsory schooling laws. We provide evidence that these pupils did not engage in endogenous timing. We use a repeated cross section of individuals over the period 1983-2001 from the General Household Survey (GHS) and adopt a lifecourse perspective. We find that poor economic conditions at labour-market entry are particularly damaging to women's health. Women who left school in a bad economy are more likely to report poorer health and to consult a general practitioner over the whole period under study (1983-2001). Additional evidence suggests that they are also more likely to suffer from a longstanding illness/disability over the whole period. For men, the health impact of poor economic conditions at labour-market entry is less obvious. Men who left school in a bad economy have a higher probability of having ever smoked, but this effect is not robust to all specifications. Finally, we find scarce evidence that leaving school in a bad economy has a persistent impact on labour-market, marriage and fertility outcomes 7 to 26 years after school-leaving. A notable exception is that poor economic conditions at labour-market entry decrease women's probability of being single.

Overall, our results show that women's health seems to be strongly affected by poor economic conditions at labour-market entry. In contrast, men's health is virtually not affected. This gender heterogeneity has numerous potential explanations. First, our subsample of men is smaller than that of women.[40] In this context, the rather imprecise results obtained for the subsample of men could simply be due to a power problem. This is likely to be the case for a limited number of health outcomes – namely self-rated health or the probability of restricting one's activity due to illness/injury – for which the estimated marginal effects for men and women lie in the same range of magnitude (see Table 5). In this context – and for a limited number of health outcomes – our results could not be interpreted as ruling out any damaging impact of poor economic conditions at labour-market entry on men's health.
Marriage formation could be an important channel whereby poor economic conditions at labour-market entry may deteriorate women's health. Being single (rather than married) is indeed associated with lower health and longevity (see Roelfs et al. (2011) for a meta-analysis). Once again, our data do not allow us to go beyond mere speculation on this matter, and this is only suggestive evidence of the mechanisms at play. A promising avenue for research would consist in a structural model of work, marriage and health, in which multiple mechanisms and their cumulative long-term effects would be studied over the lifecourse.

A potential extrapolation of our findings is that the Great Recession will have lasting and negative health effects among lower-educated individuals. However, the external validity of our findings depends on the similarity between the 1958 and 1959 GLS cohorts and current cohorts of school-leavers. In

[40]This is because men are observed over a shorter period of time (from 1986 onwards).

the mid-1970s, 50% of pupils left school at the compulsory age, while less than 20% do so at present. Moreover, there is evidence that the 1973 oil crisis and the current Great Recession did not have the same effects on unemployment rates, wages and working conditions in the UK (Gregg and Wadsworth, 2011). In this context, the extent to which our results can be generalised to young people who entered the labour market during the Great Recession is a open issue.

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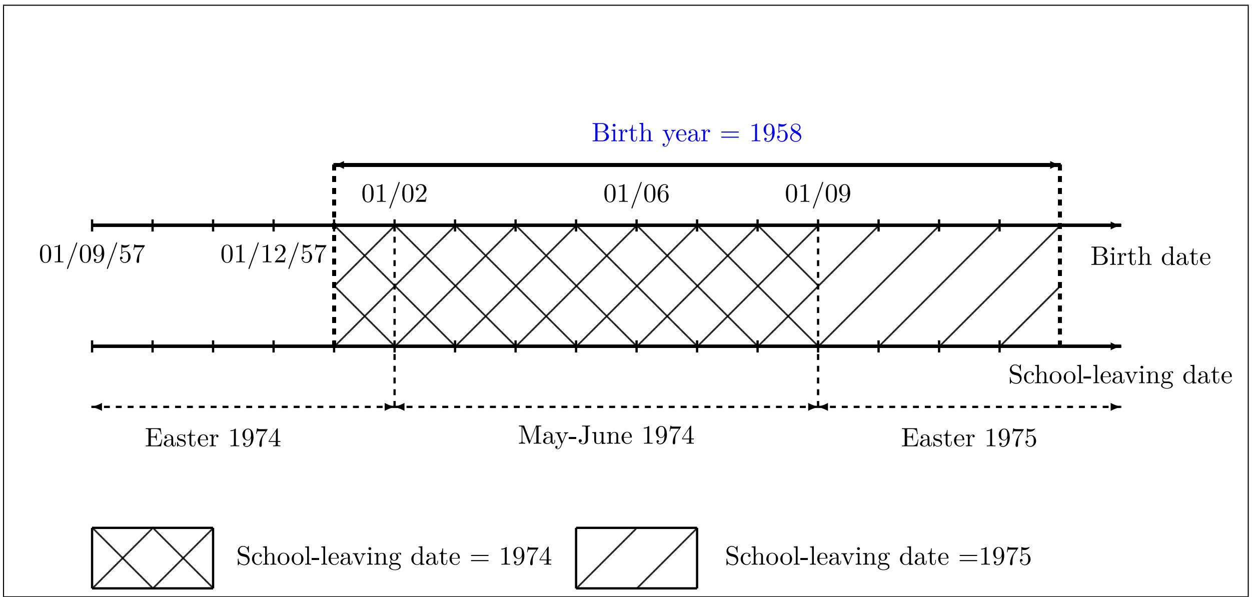
Figure 1: Compulsory schooling rules by month-year of birth

Birth year (1)	Month of birth (2)	School starting date (3)	Earliest opportunity to leave school (4)
1958	January	Sept. 1963	Easter 1974
1958	February	Sept. 1963	May/June 1974
1958	March	Sept. 1963	May/June 1974
1958	April	Sept. 1963	May/June 1974
1958	May	Sept. 1963	May/June 1974
1958	June	Sept. 1963	May/June 1974
1958	July	Sept. 1963	May/June 1974
1958	August	Sept. 1963	May/June 1974
1958	September	Sept. 1964	Easter 1975
1958	October	Sept. 1964	Easter 1975
1958	November	Sept. 1964	Easter 1975
1958	December	Sept. 1964	Easter 1975
1959	January	Sept. 1964	Easter 1975
1959	February to August	Sept. 1964	May/June 1975
1959	September to December	Sept. 1965	Easter 1976

Figure 2: Focus on the 1958 birth cohort

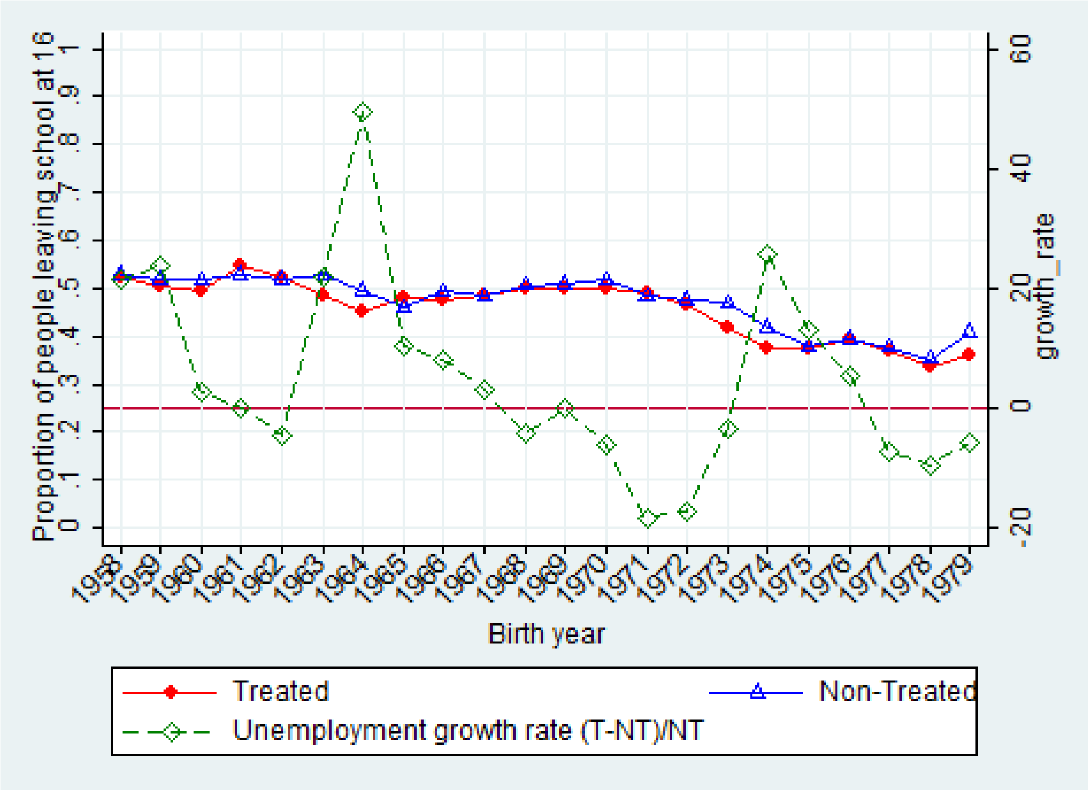


Reading : A pupil born between the 1^{st} of September 1958 and the 31^{st} of December 1958 is allowed to leave school at Easter 1975.

Figure 3: Unemployment rates for individuals under 18 over the 1971-1980 period.

Source : Wells (1983). Note: To compute unemployment rates, numbers of unemployed under 18 were expressed as a percentage of the sum of the number of employees in employment under 18 and the unemployed under 18. The estimated numbers of registered unemployed under 18 exclusive of school-leavers are obtained by substracting the *mid-year count* (i.e. July) of unemployed school-leavers from the appropriate figures. The figures inclusive of school-leavers are obtained by adding the *annual average* of unemployed school-leavers to the exclusive figures. See Wells (1983) for more details.

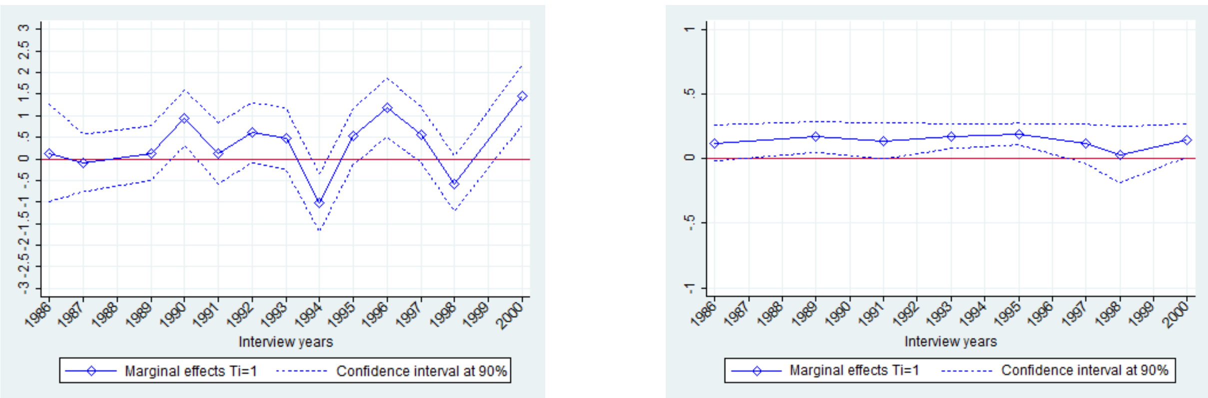
Figure 4: Proportion of pupils leaving school at compulsory age among the treated and the non-treated; Growth in school-leaving unemployment rate.



Reading: Figure 4 displays the proportion of pupils leaving school at compulsory age among the treated (in red) and non-treated group (in blue); The dashed green line shows the growth in school-leaving unemployment rate (calculated for the March-June period) faced by pupils belonging to the youngest school cohort (treated) – compared to pupils born the same year but belonging to the previous school cohort (non-treated).
Note: School-leaving unemployment rates under 18 are not available in a consistent way after 1981 from Wells (1983). The unemployment rates used here to compute the green line hold for all ages and stem from the monthly "registrant count" (borrowed from Denman and McDonald (1996)).

Figure 5: The impact of leaving school in a bad economy over the lifecourse. Men.

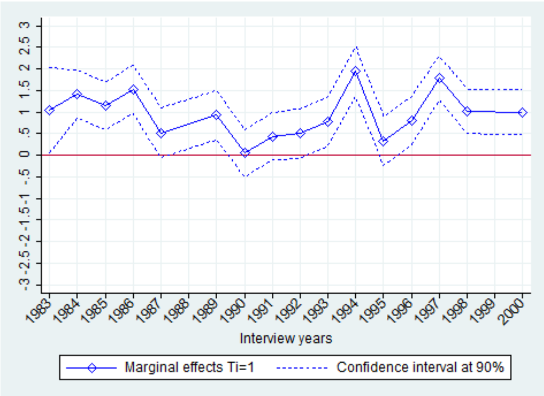
(a) Poor health index (b) Ever smoked



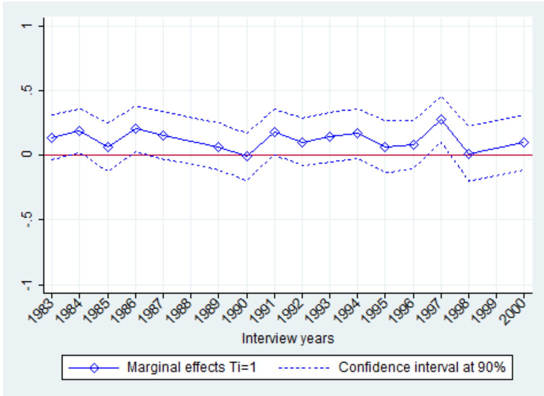
Note : interview-year-specific treatment effects are computed by estimating Equation (1) and substituting the interaction term $T_i * InterviewYear_i$ for T_i.

Figure 6: The impact of leaving school in a bad economy over the lifecourse. Women.

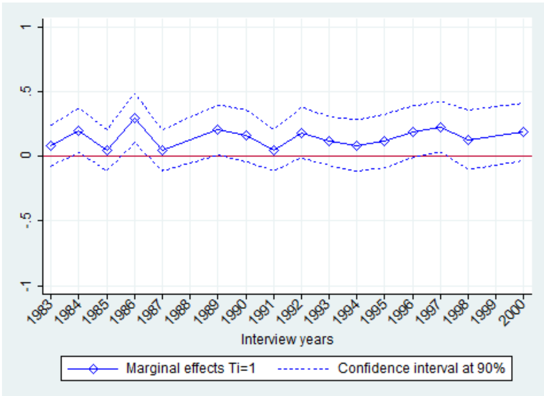
(a) Poor health index



(b) Poor self-rated health



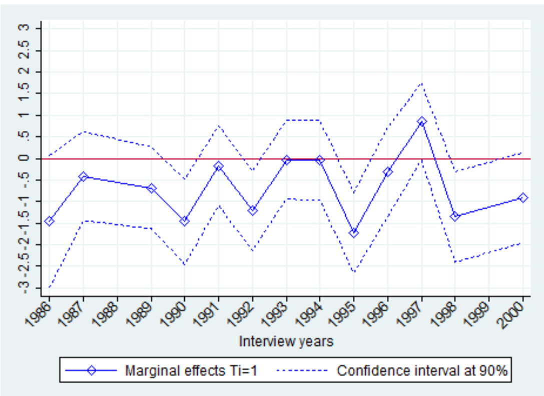
(c) GP consultation



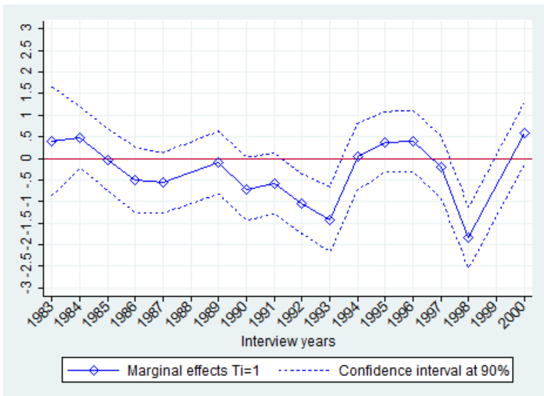
Note : interview-year-specific treatment effects are computed by estimating Equation (1) and substituting the interaction term $T_i * InterviewYear_i$ for T_i.

Figure 7: Placebo test on the health index (1953-1954 birth cohorts), lifecourse perspective.

(a) Men



(b) Women



Note : interview-year-specific treatment effects are computed by estimating Equation (1) and substituting the interaction term $T_i * InterviewYear_i$ for T_i.

Table 1: Summary statistics of demographic and health variables

	Men			Women		
	Mean	s.e	*N*	Mean	s.e	*N*
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	33.20	(4.20)	*1096*	31.19[a]	(5.00)	*1921*
Health status						
Poor self-rated health (yes/no)	0.30	(0.46)	*1044*	0.34	(0.47)	*1909*
Longstanding illness/disability (yes/no)	0.26	(0.44)	*1096*	0.23	(0.42)	*1917*
Restricts activity due to longstanding illness/injury (yes/no)	0.08	(0.27)	*1095*	0.13	(0.33)	*1920*
Health care						
GP consultation last 2 weeks (yes/no)	0.12	(0.32)	*1094*	0.21	(0.41)	*1920*
Outpatient/inpatient spell last 12 months (yes/no)	.16	(.37)	*1094*	.24	(.43)	*1918*
Health behaviour						
Smoking status			*619*			*1029*
Currently smokes (yes/no)	0.43	(0.50)		0.42	(0.49)	
Has smoked but does not anymore (yes/no)	0.33	(0.47)		0.27	(0.44)	
Ever smoked (yes/no)	0.76	(0.43)		0.69	(0.46)	
Self-reported drinking behaviour			*597*			*945*
High to moderate alcohol consumption (yes/no)	0.52	(0.50)		(0.34)	0.47	

Notes : [a] : Women are on average younger than men because they are observed over the whole period (1983-2001) while men are only observed over 1986-2001 (see Table 4).

Table 2: Summary statistics of labour-market characteristics

	Men			Women		
	Mean	s.e	*N*	Mean	s.e	*N*
	(1)	(2)	(3)	(4)	(5)	(6)
Economic status			*1096*			*1920*
Employed or self-employed (yes/no)	0.84	(0.37)		0.58	(0.49)	
Unemployed (yes/no)	0.10	(0.31)		0.06	(0.23)	
Keeping house (yes/no)	0.01	(0.09)		0.34	(0.47)	
Other (yes/no)	0.05	(0.21)		0.02	(0.15)	
For those currently employed or self-employed						
Usual gross weekly earnings from main job (in pounds)	283.72	(880.68)	*819*	109.92	(99.03)	*970*
Time with present employer			*724*			*861*
Less than 1 month (yes/no)	0.02	(0.13)		0.03	(0.17)	
Between 1 and 3 months (yes/no)	0.04	(0.20)		0.06	(0.24)	
Between 4 and 6 months (yes/no)	0.04	(0.20)		0.06	(0.24)	
Between 7 and 12 months (yes/no)	0.08	(0.27)		0.11	(0.31)	
Between 1 and 5 years (yes/no)	0.20	(0.40)		0.34	(0.47)	
Five years or more (yes/no)	0.61	(0.49)		0.38	(0.49)	

Table 3: Summary statistics of marriage and fertility variables

	Men			Women		
	Mean	s.e	*N*	Mean	s.e	*N*
	(1)	(2)	(3)	(4)	(5)	(6)
Marriage						
Marital status			*1096*			*1921*
Married	0.70	(0.46)		0.71	(0.45)	
Single	0.25	(0.43)		0.16	(0.37)	
Widowed	0.00	(0.04)		0.00	(0.05)	
Divorced	0.04	(0.19)		0.08	(0.28)	
Separated	0.02	(0.12)		0.04	(0.20)	
Age at first marriage$^{(a)}$	23.20	(3.75)	*819*	20.86	(3.36)	*1617*
Fertility						
Number of children	-	-		1.68	(1.24)	*1903*
Age at first birth$^{(b)}$	-	-		22.79	(4.11)	*1124*

Notes : $^{(a)}$: conditional on having been married. $^{(b)}$: conditional on having at least one child.

Table 4: Number of observations by survey wave and birth cohort

	Men	Women	All
	(1)	(2)	(3)
Survey wave			
1983	-	159	159
1984	-	153	153
1985	-	127	127
1986	100	130	230
1987	92	140	232
1988-1989	83	140	223
1989-1990	82	102	184
1990-1991	74	126	200
1991-1992	107	124	231
1992-1993	76	97	173
1993-1994	85	109	194
1994-1995	93	98	191
1995-1996	71	121	192
1996-1997	92	118	210
1998-1999	62	85	147
2000-2001	79	92	171
Birth cohort			
1958	544	972	1516
1959	552	949	1501
Total number of observations	1096	1921	3017

Notes: (1) The GHS was conducted annually, except for breaks in 1997-1998 when the survey was reviewed, and 1999-2000 when the survey was redeveloped. (2) Month and year of birth in 1983-1985 are only available for women who completed the Family Information section. They are available for all respondents over 1986-2001.

Table 5: The impact of leaving school in a bad economy on health outcomes (1958-59 cohorts)

	Men			Women		
	m.e.	s.e.	*N*	m.e.	s.e.	*N*
Linear regressions						
Poor Health Index	0.271	(0.476)	*1040*	0.857**	(0.363)	*1902*
Health behaviour Index	0.426	(0.447)	*596*	0.084	(0.337)	*943*
Probit regressions						
Health status						
Poor self-rated health	0.081	(0.078)	*1043*	0.106*	(0.057)	*1907*
Longstanding illness	-0.034	(0.069)	*1095*	0.051	(0.051)	*1915*
Restricts activity	0.056	(0.045)	*1094*	0.040	(0.041)	*1918*
Health care						
GP consultations last 2 weeks	-0.001	(0.049)	*1093*	0.119**	(0.052)	*1918*
Hospital consultation	0.000	(0.058)	*1095*	0.026	(0.051)	*1919*
Health behaviour						
Currently smokes	0.093	(0.105)	*618*	0.042	(0.079)	*1027*
Ever smoked	0.170**	(0.078)	*618*	0.086	(0.071)	*1027*
Moderate to heavy drinking	-0.028	(0.107)	*596*	0.012	(0.080)	*943*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1. Marginal effects (m.e.) are presented (computed as marginal probability effects at the sample mean value of the regressors if probit models are estimated). Robust standard errors in parentheses (s.e.). Our models include dummy variables for interview and birth year as well as a linear function of age in months – see Equation (1).

Table 6: Placebo test on health outcomes (1953-54 cohorts)

	Men			Women		
	m.e.	s.e.	*N*	m.e.	s.e.	*N*
Linear regressions						
Poor Health Index	-0.659	(0.620)	*630*	-0.287	(0.444)	*1200*
Health behaviour Index	-0.569	(0.464)	*372*	0.284	(0.415)	*617*
Probit regressions						
Health status						
Poor self-rated health	-0.059	(0.095)	*631*	-0.073	(0.071)	*1204*
Longstanding illness/disability	-0.007	(0.092)	*664*	0.047	(0.066)	*1210*
Restricts activity	0.012	(0.058)	*663*	-0.002	(0.045)	*1213*
Health care						
GP consultation last 2 weeks	-0.047	(0.051)	*664*	-0.008	(0.056)	*1211*
Hospital consultation	-0.105	(0.061)	*664*	-0.089	(0.054)	*1213*
Health behaviour						
Currently smokes	-0.035	(0.127)	*390*	0.073	(0.098)	*653*
Ever smoked	0.050	(0.089)	*362*	0.052	(0.081)	*653*
Moderate to heavy drinking	-0.177	(0.132)	*372*	-0.001	(0.097)	*617*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1. Marginal effects (m.e.) are presented (computed as marginal probability effects at the sample mean value of the regressors if probit models are estimated). Robust standard errors in parentheses (s.e.). Our models include dummy variables for interview and birth year as well as a linear function of age in months – see Equation (1).

Table 7: Differences-in-differences analysis : the impact of leaving school in a bad economy on health outcomes

	Men			Women		
	coeff	s.e.	*N*	coeff	s.e.	*N*
Linear regressions						
Poor Health Index	-0.105	(0.382)	*1670*	0.416^{μ}	(0.280)	*3102*
Health behaviour Index	0.022	(0.324)	*968*	0.194	(0.262)	*1560*
Linear probability models						
Health status						
Poor self-rated health	0.014	(0.049)	*1674*	0.061*	(0.033)	*3111*
Longstanding illness	0.013	(0.047)	*1759*	0.063**	(0.030)	*3125*
Restricts activity	0.017	(0.030)	*1757*	0.025	(0.023)	*3131*
Health care						
GP consultations last 2 weeks	0.008	(0.033)	*1757*	0.075***	(0.028)	*3129*
Hospital consultation	-0.052	(0.039)	*1759*	0.017	(0.029)	*3132*
Health behaviour						
Currently smokes	0.010	(0.068)	*1008*	-0.027	(0.046)	*1680*
Ever smoked	0.018	(0.052)	*1008*	0.013	(0.042)	*1680*
Moderate to heavy drinking	-0.084	(0.069)	*968*	0.035	(0.046)	*1560*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1, $^{\mu}$ p-value<0.15. Coefficients are obtained by estimating Equation (3) by OLS or linear probability models. Robust standard errors in parentheses (s.e.).

Table 8: The impact of leaving school in a bad economy on labour-market outcomes (1958-59 cohorts)

	Men			Women		
	m.e.	s.e.	*N*	m.e.	s.e.	*N*
Probit regressions						
Economic status						
Keeping house	0.017	(0.033)	*495*	0.053	(0.057)	*1918*
Unemployed	0.017	(0.050)	*1095*	-0.002	(0.026)	*1918*
For those currently employed						
Less than 1 month	0.074**	(0.048)	*512*	0.034	(0.037)	*805*
Less than 3 months	0.022	(0.048)	*613*	0.053	(0.059)	*861*
Less than 6 months	0.001	(0.057)	*723*	0.029	(0.068)	*861*
Less than 1 year	0.053	(0.078)	*723*	-0.035	(0.077)	*861*
Less than 5 years	0.046	(0.098)	*723*	-0.091	(0.089)	*861*
More than 5 years	-0.046	(0.098)	*723*	0.091	(0.089)	*861*
Linear regressions						
Earnings (log)	-0.041	(0.094)	*799*	-0.115	(0.151)	*957*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1. Marginal effects (m.e.) are presented (computed as marginal probability effects at the sample mean value of the regressors if probit models are estimated). Robust standard errors in parentheses (s.e.). Our models include dummy variables for interview and birth year as well as a linear function of age in months – see Equation (1).

Table 9: The impact of leaving school in a bad economy on marriage and fertility outcomes (1958-59 cohorts)

	Men			Women		
	coeff	s.e.	*N*	coeff	s.e.	*N*
Probit regressions						
Married	.107	(.069)	*1095*	-.020	(.053)	*1919*
Divorced	.006	(.032)	*1021*	-.050*	(.026)	*1919*
Single	-.101	(.063)	*1095*	.081*	(.044)	*1919*
Linear regressions						
Age at first marriage	-.390	(.704)	*818*	.176	(.408)	*1615*
Age at first birth				.404	(.610)	*1122*
Nb of children				.180	(.137)	*1901*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1. Marginal effects (m.e.) are presented (computed as marginal probability effects at the sample mean value of the regressors if probit models are estimated). Robust standard errors in parentheses (s.e.). Our models include dummy variables for interview and birth year as well as a linear function of age in months – see Equation (1).

Online Appendix

A Tables

Table A1: The impact of school-leaving unemployment rate on health outcomes.

	Men			Women		
	m.e.	s.e.	*N*	m.e.	s.e.	*N*
Linear regressions						
Poor Health Index	0.000	(0.058)	*1040*	0.121**	(0.049)	*1902*
Health behaviour Index	0.048	(0.055)	*596*	0.002	(0.044)	*943*
Probit regressions						
Health status						
Poor self-rated health	0.001	(0.009)	*1043*	0.014*	(0.007)	*1907*
Longstanding illness/disability	-0.004	(0.005)	*1094*	0.006	(0.005)	*1918*
Restricts activity	0.002	(0.005)	*1094*	0.006	(0.005)	*1918*
Health care						
GP consultation last 2 weeks	-0.005	(0.006)	*1093*	0.019***	(0.006)	*1918*
Hospital consultation	0.005	(0.007)	*1095*	0.004	(0.007)	*1919*
Health behaviour						
Currently smokes	0.013	(0.013)	*618*	0.005	(0.010)	*1027*
Ever smoked	0.027**	(0.011)	*618*	0.007	(0.010)	*1027*
Moderate to heavy drinking	-0.012	(0.013)	*596*	0.001	(0.010)	*943*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1, $^{\mu}$ p-value<0.15. Each line presents the marginal effect of the school-leaving unemployment rate on a different health outcome. Our models include dummy variables for interview and birth year as well as on a linear function of age in months – see Equation (4). Marginal effects (m.e.) are computed as marginal probability effects at the sample mean value of the regressors. Robust standard errors in parentheses (s.e.).

Table A2: The impact of school-leaving unemployment rate on health: instrumental variables (2SLS).

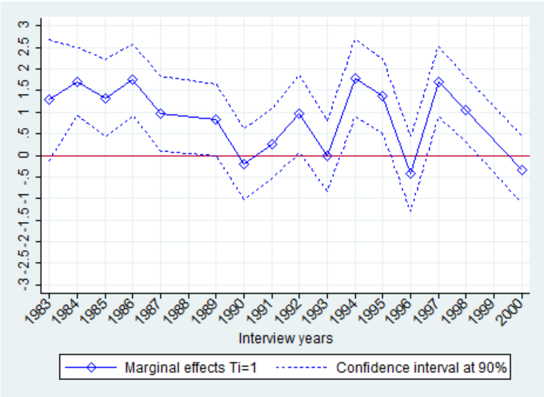
	Men			Women		
	me	se	*N*	me	se	*N*
Second-stage results						
Poor Health Index	0.038	(0.067)	*1040*	0.116**	(0.049)	*1902*
Health Behaviour Index	0.059	(0.062)	*596*	0.011	(0.045)	*943*
Health status						
Poor self-rated health	0.011	(0.010)	*1043*	0.014*	(0.007)	*1907*
Longstanding illness	-0.005	(0.010)	*1095*	0.007	(0.007)	*1915*
Restricts activity	0.008	(0.006)	*1094*	0.005	(0.005)	*1918*
Health care						
GP consultations last 2 weeks	0.000	(0.007)	*1093*	0.016**	(0.006)	*1918*
Hospital consultation	0.000	(0.008)	*1095*	0.004	(0.007)	*1919*
Health behaviour						
Currently smokes	0.012	(0.014)	*618*	0.006	(0.011)	*1027*
Ever smoked	0.025**	(0.013)	*618*	0.012	(0.010)	*1027*
Moderate to heavy drinking	-0.004	(0.015)	*596*	0.002	(0.011)	*943*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1, $^{\mu}$ p-value<0.15. Marginal effects (m.e.) are presented (computed as marginal probability effects at the sample mean value of the regressors). Robust standard errors in parentheses (s.e.). In the first stage, school-leaving unemployment rate is regressed on the treatment variable (i.e. the instrument), dummy variables for interview and birth year as well as a linear function of age in months. In the second stage, Equation (4) is estimated by 2SLS where school-leaving unemployment rate is replaced with its predicted value from the first stage. The covariance matrix of $\hat{\pi}$ is corrected accordingly.

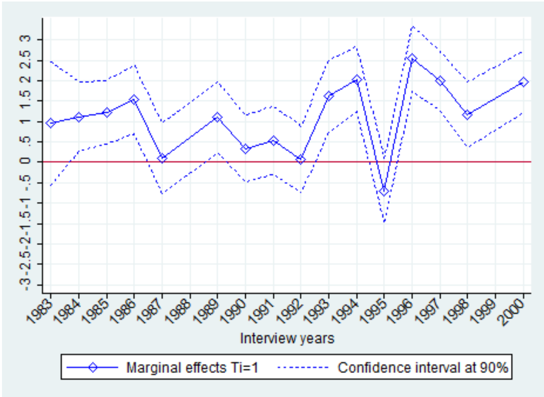
B Figures

Figure A1: The impact of leaving school in a bad economy over the lifecourse. Separate regression for each birth cohort, Women.

(a) 1958 birth cohort



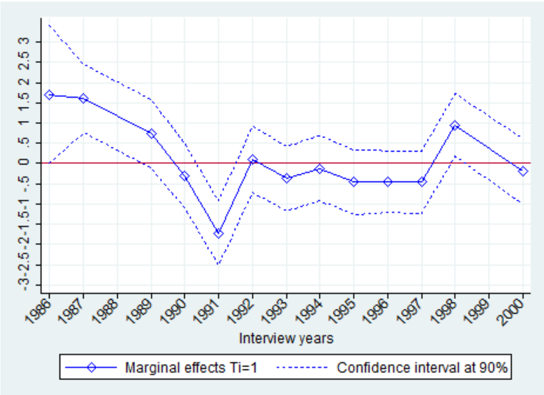
(b) 1959 birth cohort



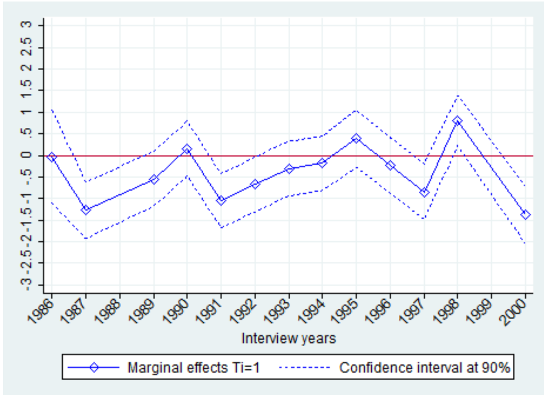
Note : interview-year-specific treatment effects are computed by estimating Equation (1) and substituting the interaction term $T_i * InterviewYear_i$ for T_i.

Figure A2: Placebo tests on the health index (alternative birth cohorts). Men.

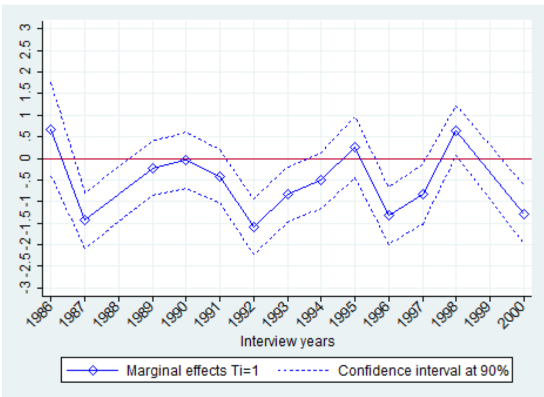
(a) Men born in 1949 and 1950



(b) Men born in 1960 and 1961



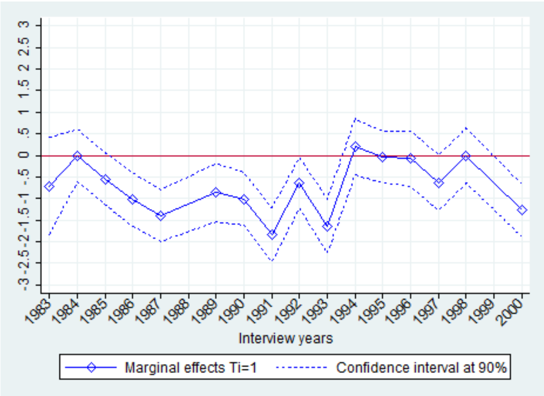
(c) Men born in 1961 and 1962



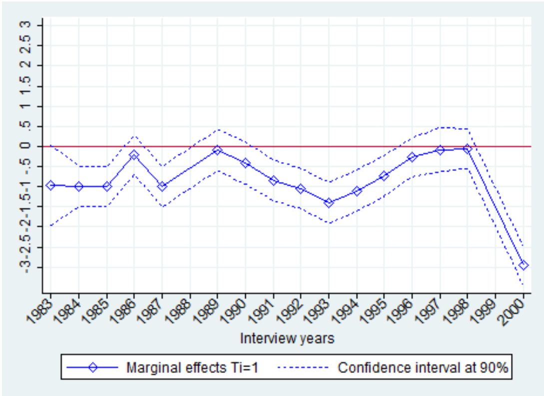
Note : interview-year-specific treatment effects are computed by estimating Equation (1) and substituting the interaction term $T_i * InterviewYear_i$ for T_i.

Figure A3: Placebo tests on the health index (alternative birth cohorts). Women.

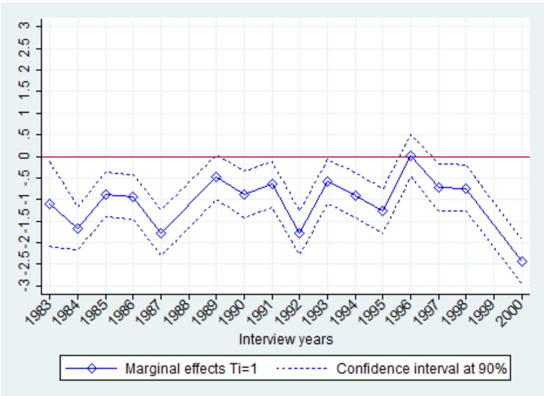
(a) Women born in 1949 and 1950



(b) Women born in 1960 and 1961



(c) Women born in 1961 and 1962



Note : interview-year-specific treatment effects are computed by estimating Equation (1) and substituting the interaction term $T_i * InterviewYear_i$ for T_i.

C Validity of the identification strategy

In this section, we discuss how school-entry rules may generate health differences within a given birth cohort between treated and untreated pupils by means of age-relative rank, school-cohort size or job-experience effects. We also discuss the importance of season-of-birth effects on subsequent labour and health outcomes.

Age-relative rank

School-entry rules imply that treated individuals (born September-December) are the oldest pupils in their *school cohort*, while untreated pupils are the youngest.[41] Yet, there is evidence that relative age effects play a role in school performance. Specifically, older people in a given school cohort tend to have higher wages than younger individuals in the same school cohort – which is interpreted as an indication of the persistence of maturity effects related to age differences between students in the same class (Plug, 2001). As treated pupils are the oldest in their school cohort – and to the extent that relative maturity effects positively affect adult labour-market and health outcomes – we should measure a lower bound, i.e., underestimate the negative impact of poor economic conditions at labour-market entry on long-term labour-market and health outcomes.

School-cohort-size effects

Because treated and non-treated pupils belong to different school cohorts, another concern relates to school-cohort sizes. School-cohort size has been shown to have a negative impact on labour-market outcomes due to an excess of supply on the labour market (Berger, 1985, 1989; Korenman and Neumark, 1997; Macunovich, 1999; Morin, 2011; Welch, 1979). We focus on three school cohorts only, which are not likely to differ in size.[42] To the extent that cohort-size effects exist, however, we should measure a lower bound : the fertility rate peaked in 1957 and declined thereafter, such that for a given birth year, the youngest school cohort (treated) would have higher wages on average than the previous school cohort.

Job-experience effects

School-entry rules imply that within a birth cohort, treated pupils begin school one year later than non-treated pupils. As starting school later entails the opportunity cost of entering the labour market later, treated pupils lack one year of job experience relative to non-treated pupils. However, whether an additional year of job experience has a positive or negative impact on health is not clear. On the one hand, it leads to higher life-time earnings, which is beneficial to health. On the other hand, it implies a longer exposure to adverse working conditions, if any. To the extent that we consider low-skilled individuals, this possibility is not to be discarded. Hence, the direction of the effect of an additional year of job experience on health is not clear.

Season-of-birth effects

[41] Conversely, treated individuals (born September-December) are the youngest pupils in their *birth cohort*, while untreated pupils are the oldest.

[42] Cohort sizes do not vary substantially from one year to the next. This is why studies have focused on long-term (typically 8-25 years) variations in cohort size (Morin (2011)).

A growing body of literature has shown the importance of season-of-birth effects on subsequent labour and health outcomes (Bound and Jaeger, 1996).
First, the seasonality of births varies from one social group to another. Using US data, Kestenbaum (1987) reports that children born to high-income families are more likely to be born in spring.[43] In our framework, this implies that untreated pupils should have more favourable parental sociodemographic characteristics. To the extent that children born to high-income families are in better health on average, this would lead us to overestimate the impact of poor economic conditions at labour-market entry on adult health outcomes. Beyond parental socio-economic characteristics, some health differences have also been demonstrated to show dependence with respect to birth date(Bound and Jaeger, 1996). Doblhammer and Vaupel (2001) report a positive relationship between being born in October to December and longevity at age 50.[44] This month-of-birth effect suggests that even in the presence of parental socio-demographic characteristics, treated pupils should be in better health than untreated pupils, which would lead us to underestimate the impact of economic conditions at entry on adult health outcomes.

[43] Note, however, that we do not find evidence of this in our data. When considering the entire GHS sample and using information on father's occupation (manual or not), we find that the proportion of individuals whose father was in a manual occupation was the same whether individuals were born in September-December or earlier in the year.

[44] They show that those born in the Northern Hemisphere in October to December live as much as approximately 0.6 years longer than those born in April to June. As expected, data for Australia show that, in the Southern Hemisphere, the pattern is shifted by half a year. They conclude that the month-of-birth effect is most likely explained by the seasonal availability of fresh fruit, vegetables and eggs to the pregnant mother in the first and second trimesters.

D Data appendix : Sample and variable construction

D.1 Changes to sampling procedures and sample sizes over time

According to the GHS Time Series Dataset User Guide (2007), "the sampling procedure used on the GHS has changed over time, resulting in different sample sizes between survey years. However, the changes to the GHS sample procedures and sample size were relatively small. As a result it was decided by ONS that these changes were likely to have little impact on the reliability of the estimates. Particularly as a representative sample of the population has been achieved for each survey year."
Non-response weights are only available in the GHS after 2000. As a consequence, all our estimates are unweighted.

D.2 Inconsistencies in variables over time

According to the GHS Time Series Dataset User Guide (2007), "in general variables in the GHS have remained fairly consistent over time. However as the GHS has been revised and research interests have changed, some variables have been modified over the past 30 years to reflect this. For example the marital status variable was revised in the 1986 survey to include a category for cohabitation. Similarly, some questions were only included on a few survey years, or in more recent rounds of the survey series, which limits analysis over time."
Those variables that were only available for a few years, or had substantially changed over time were not used in the analysis.

E School leaving age legislation in England and Wales

Relevant extracts of the 1962 Education Act are borrowed from Del Bono and Galinda-Rueda (2007).

Education Act 1962: relevant extracts from Section 9

Applies to 15 year old individuals in 1963, i.e. people born in 1947 or afterwards.
(2) If he attains that age on any date from the beginning of September to the end of January, he shall be deemed not to have attained that age until the end of the appropriate spring term at this school.
(3) If he attains that age on any date on or after the beginning of February but before the end of the appropriate summer term at his school, he shall be deemed not to have attained that age until the end of that summer term.
(4) If he attains that age on any date between the end of the appropriate summer term at this school and the beginning of September next following the end of that summer term (whether another term has then begun or not) he shall be deemed to have attained that age at the end of that summer term. [...]
(7) In this section, "the appropriate spring term", in relation to a person, means the last term at this school which ends before the month of May next following the date on which he attains the age in question, and "the appropriate summer term" [...] means the last term at this school which ends before the month of September next following that date [...].

Education School leaving Act 1976: relevant extracts from Section 1

Subsections (3) and (4) in Section 9 of the Education Act of 1962 were substituted by the following subsections of Section 1 of the Education School leaving Act 1976 in order to give a more precise meaning to the notion of school leaving dates, particularly for those born after the end of January.
(3) If he attains that age after the end of January but before the next May school leaving date, he shall be deemed not to have attained that age until that date.
(4) If he attains that age after the May school leaving date and before the beginning of September next following that date, he shall be deemed to have attained that age on that date. A new subsection was added at the end of Section 9 of the Education Act of 1962, according to which:
(8) In this section the May school leaving date means the Friday before the last Monday in May.