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

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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 who entered the labour market immediately after the 1973 oil crisis. Our identification strategy relies on the comparison of individuals who left school at the compulsory age, were born in the same year and had a similar quantity of education – but whose school-leaving behaviour in different years (hence, 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 the 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. For men, the health impact of poor economic conditions at labour-market entry is less obvious and not robust to all specifications.

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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 that 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, 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 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 affect subsequent health is family formation.[9]

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

[2] Kahn (2010) and Oreopoulos et al. (2012) 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 (2015) : her results indicate less persistence for low-skilled or disadvantaged workers.

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

[4] Hershbein (2012) finds that 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 that longer term effects are minimal.

[5] See Schoar and Zuo (2011) for empirical evidence on worse career perspectives (occupational prestige and mobility) and Leombruni et al. (2015) on 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 for instance Caroli and Godard (2016); Fischer and Sousa-Poza (2009); Fletcher et al. (2011); Llena-Nozal (2009) and Robone et al. (2011) on the health effect of job stress, occupational injury risk, perceived job insecurity, long working hours, harmful working conditions, and downward occupational mobility.

[9] See Gustman and Steinmeier (1981); Gutiérrez-Domènech (2008); Hashimoto and Kondo (2012); Kondo (2012); Maclean

In this paper, we examine the lasting impact of leaving full-time education in a bad economy on 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 the compulsory age) – who entered the labour market immediately after the 1973 oil crisis. The proportion of pupils who left full-time education at the compulsory age was remarkably high in the UK during the 1970s – as high 50% (Micklewright et al., 1989). Our identification strategy relies 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 years (hence, different economic conditions) was induced by compulsory schooling laws. Our identification strategy builds on two sources. First, in each birth cohort, "treated" pupils born at the end of the calendar year (September to December) were allowed to leave school almost a year later than "control" pupils born earlier in the year (January to August). Second, unemployment rates sharply increased in the wake of the 1973 oil crisis. Between 1974 and 1976, each school cohort faced worse economic conditions at labour-market entry than its predecessor.[10] Thus, we compare – for instance – individuals born in January-August versus September-December 1958, who were born the same year but allowed to leave school in May/June of 1974 and 1975, respectively, who ended up facing very different economic conditions at labour-market entry. In an extended DiD strategy, we make sure that we can safely attribute the observed health differences between the treated and control groups to the economic conditions at labour-market entry, as opposed to any systematic unobservable differences between September-December- and January-August-born children.

Of course, a potential selection issue concerns the fact that pupils' decisions to leave school at the compulsoryage 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 did not anticipate at that time 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

et al. (2016) 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 the compulsoryage 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.

health behaviour. Our results show that poor economic conditions at labour-market entry are particularly damaging to women's health. In our preferred specification, we find that a one-point increase in school-leaving unemployment rates leads to a 0.042 standard-deviation increase in women's poor health index. In particular, women have a higher probability of consulting a general practitioner as well as of going to the hospital as an inpatient/oupatient over the whole period (1983-2001). For men, the health impact of poor economic conditions at labour-market entry is less obvious and not robust to all specifications.

This paper chiefly relates to the emerging literature investigating the long-term health consequences of graduating in a bad economy.[12] 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 authors consider long periods of economic fluctuations and exploit the variation in country (or state) school-leaving unemployment rates to identify the health effect of economic conditions at career entry. (Maclean, 2013) is the only one to use instrumental-variable (IV) methods to address selection problems related to endogenous sorting. The results obtained thus far have been mixed. For the USA, (Maclean, 2013) shows that men leaving school when the state unemployment rate was high are in worse health at age 40 than otherwise similar men, while leaving school in a bad economy lowers depressive symptoms at age 40 among women.[13] Using European data on the 50+, Hessel and Avendano (2013) find that leaving school when the country unemployment rate is high predicts 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. On European data, Cutler et al. (2015) 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. 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

[12] Our study is also indirectly related to the literature on the short-term health effect of contemporaneous economic fluctuations. Most studies in this literature indicate that at the population level, 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. We argue, however, that these findings can easily be reconciled with our own results. First, we are interested in the long-run (as opposed to the short-run) consequences of contemporaneous economic fluctuations. Second, we focus on a very critical period – the first entry into the labour market. Finally, even the short-term 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.

[13] In a series of recent papers Maclean (2014, 2015, 2016) uses the same data to specifically test whether leaving school in an economic downturn persistently affects drinking behaviour, body weight and the probability of having access to employer-sponsored health insurance.

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. More specifically, we provide evidence that endogeneous timing at the end of compulsory schooling over the 1974-1976 period is not a threat to our identification.[14] 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, 2015). 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.

The remainder of the paper is organised as follows. Section 2 presents the institutional framework 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 (Section 2.1) and provides graphical evidence of the sharp increase in unemployment rates after the 1973 oil crisis (Section 2.2).

2.1 Compulsory schooling in England and Wales

Compulsory schooling laws in England an Wales[15] 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 two different school cohorts.

The current British minimum school-leaving age is 16. The proportion of children leaving education at the first legal opportunity in the UK is high by the standards of other industrialised countries (Micklewright

[14]Thus, there is 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.

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

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)[16] 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 in the Online Appendix B), 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 their 16^{th} birthday, i.e., at age 15.

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).

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 two 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 $t+1$. Importantly, 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.[17] Figure 2 provides an illustration of how the compulsory schooling rules operate by taking the 1958 birth cohort as an example.

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

[16] Two increases were made to reach the current school leaving age of 16, from age 14 to 15 in 1947 and from age 15 to 16 in 1972. 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). 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.

[17] 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. Clark and Royer (2013) indeed show that the additional year of schooling induced by the 1972 ROSLA reform had no effect on health whatsoever.

industrialised countries, including the UK (Bhattarai, 2011).[18]

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).[19] 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 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). Between 1974 and 1977-1978 – when the economy recovered –, male and female youth unemployment rates sharply increased. The sharpest increase occurred between 1974 and 1976 : over this period, 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).[20] 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.[21] 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 from age 16).

3 Empirical approach

Section 3.1 presents the identification strategy and the models we estimate. Section 3.2 discusses whether the endogeneous timing of school-leaving is likely to jeopardize our identification strategy.

3.1 Identification strategy and model

3.1.1 Baseline approach

We consider individuals who left school at the compulsory age (see Section 3.2 below for a detailed discussion on whether pupils' decisions to leave school at compulsory age are endogeneous to economic conditions). Our identification strategy relies on the comparison of very similar individuals – born in the same year and having a similar amount of education (in months) – who, due to the existence of compulsory schooling laws, entered the labour market in different years. Specifically, in each birth cohort, "treated" pupils born at the end of the calendar year (September to December) left school almost a year later than "untreated" pupils born earlier in the year (January to August). Our baseline analysis considers

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

[19] Note that figures taken in July are inevitably higher than at other times of the year because they include school-leavers.

[20] Unemployment rates under 18 excluding school-leavers rose from 3.1% in 1974 to 9.8% in 1976 (for males) and from 2.0% in 1974 to 9.4% in 1976 (for females).

[21] 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.

pupils born in 1958 and 1959 – hence entering the labour market between 1974 and 1976. Each school cohort faced significantly worse economic conditions than did the previous one between 1974 and 1976, so that within each birth cohort, treated pupils ended up facing worse economic conditions at labour-market entry than untreated ones.[22] [23]

We pool observations from the 1958 and 1959 birth cohorts and use a repeated cross-section of individuals over the period 1983-2001 to estimate the following equation by standard probit[24], for men and women separately :

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

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, hence not allowed to leave school until Easter of year *t+1*), and value 0 if non-treated (i.e., born between the 1^{st} of January and the 31^{st} of August, hence allowed to leave school as soon as in Easter or in May/June of year *t*). $BirthYear_i$ is a dummy variable for individual *i*'s year of birth.[25] $InterviewYear_i$ is a dummy variable for individual *i*'s interview year.[26] $f(BirthMonth_i)$ is a linear function of age in months within a birth year. 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).[27] 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).[28] 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.[29] Finally, ϵ_i denotes the error term.

Equation (1) estimates the average effect of the treatment $(\widehat{\gamma})$ over the whole period (1983-2001). Our cross-sectional data, however, also allows us to adopt a lifecourse perspective. To do so, we compute the

[22] 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, treated pupils left school in worse conditions than otherwise similar untreated pupils.

[23] 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.

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

[25] Controlling by birth years is essential in our setup as the identifying variation comes from a within-birth-cohort variation.

[26] 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 than a dummy indicating the survey wave because a survey wave can be conducted over several years – usually two.

[27] Note that when allowing for more flexible specifications of $f(BirthMonth_i)$ – a change in the slope of $f(BirthMonth_i)$ at the August/September discontinuity, or a quadratic specification –, the results go in the same direction.

[28] 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)).

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

marginal effects of the treatment associated with each interview year over the period 1983-2001.[30] 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.

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 within each birth cohort, treated pupils do not face at school-leaving the exact same increase in youth unemployment rates relative to non-treated pupils (a 8.5 (8.2) percentage-point increase for treated men (women) born in 1958 versus a 5.4 (6.7) percentage-point increase for treated men (women) born in 1959).
To address this potential problem, a first solution is to run a separate regression for each birth cohort. A second solution is to consider the treatment as a linear variable. More formally, we estimate the impact of youth unemployment rate at school-leaving on subsequent health. We estimate the following equation by standard probit :

$$H_i^* = \lambda + \pi UR_i + BirthYear_i + f(BirthMonth_i) + InterviewYear_i + \eta_i \tag{3}$$

where UR_i stands for the youth unemployment rate faced at school-leaving by individual i, and the other variables are presented above. This allows us to interpret the impact of a one percentage-point increase in school-leaving unemployment rate on subsequent health.

3.1.2 Extended approach: A difference-in-differences strategy

A key identifying assumption of our baseline approach is that apart from school-exit rules, no other institutional difference generates differences in health between the treated and the control groups within each birth cohort. Another key assumption is that it is only to the extent that individuals born between January and August and individuals born between September and December are comparable in all observable and unobservable characteristics that we can safely attribute observed differences in health to the impact of labour-market conditions at labour-market entry.

There is a number of reasons why these assumptions may not hold. First, school-entry rules introduce a discontinuity between August-born and September-born children. This institutional feature implies that within a given birth cohort, students belong to two different school cohorts. This discontinuity may generate differences in health between treated and untreated pupils by means of age-relative rank, school-cohort size or job-experience effects. Differential incentives to take GCE O-level/CSE examinations at the end of Year 11 may also generate differences between the treated and the control groups in terms of educational achievement. Second, individuals born between January and August and individuals born

[30] As we estimate probit models, we only interpret the sign and the significance of the coefficients associated with these interaction terms.

between September and December may not be comparable in all observable and unobservable characteristics. For instance, a growing body of literature has shown the importance of season-of-birth effects on subsequent labour and health outcomes – see for instance Bound and Jaeger (1996); Kestenbaum (1987) and Doblhammer and Vaupel (2001). A detailed discussion of these matters can be found in Online Appendix C. However, the overall direction of the potential bias stemming from the combined effects of age-relative rank, school-cohort size, job-experience or season-of-birth effects is far from clear.

To make sure 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 any systematic unobservable differences between September-December- and January-August-born children (e.g. age-relative rank, season-of-birth effects, job experience, etc.), we implement a difference-in-differences (DiD) analysis. We use as a "control" group birth cohorts within which September-December- and January-August-born children faced very similar school-leaving unemployment rates at the end of compulsory schooling. Our selection criterion for "control" birth cohorts is as follows : (i) within each birth cohort, the absolute variation in school-leaving unemployment rate faced by September-December-born pupils (the youngest school cohort) compared to January-August-born (the previous school cohort) should be less than ten percent (ii) each birth cohort should fall within a window of ten years centered at 1958-59.[31] This selection criterion yields the 1952-1954 and 1960-1962 cohorts. In the robustness section, we modify this selection criterion to check the sensitivity of our results (see Section 5.2).

To implement our DiD strategy, we estimate the following equation by a linear probability model :

$$H_i = \alpha + \chi T_i + \delta D_i + \beta T_i \times D_i + BirthYear_i + f(BirthMonth_i) + InterviewYear_i + \epsilon_i \qquad (4)$$

where D_i is an indicator variable taking value 1 if individual i is born in 1958-1959 and value 0 if born in 1952-1954 or 1960-1962. $\widehat{\beta}$ is the difference-in-differences estimator. It corresponds to the difference in health between the treated and untreated individuals across the 1958-59 and "control" 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.

3.2 Validity of the identification strategy : endogeneous timing of school-leaving

A key assumption in our identification strategy – be it for the baseline or extended approach – is that pupils in their last year of compulsory schooling do not strategically remain in school when the economy deteriorates.

[31] This restriction ensures that only very similar cohorts (in terms of conditions in utero, during childhood etc.) are compared.

But time of school-leaving may be endogenous to the contemporaneous economic conditions. The sign of the bias arising from endogenous timing is difficult to predict, however. 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 (Maclean, 2013). 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 (see Online Appendix D for a detailed discusion of that matter). 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). 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 very similar, 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. 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.[32] 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.[33] We do check, however, that the gender ratio between treated and control pupils is equal within each birth cohort. It is the case for the 1958-59 cohorts, but not for birth cohorts who reached the compulsory age during the recession of the 1980s.

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, it can be hypothesised that the compulsory age was still binding at that time.

[32]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.

[33]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.

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.[34] 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[35], 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.

In our baseline specification, 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.[36] We also exclude individuals reporting that they never went to school or individuals whose highest qualification was equivalent to Year 12 or more.[37] 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.[38] As the outcomes of interest are not collected consistently over the period (see Data Appendix E.2), we include all possible observations for each outcome to maximise sample size. In our extended DiD specification, our sample includes additional individuals from the "control" birth cohorts and equivalent restriction criteria are then used. Overall, our sample for the baseline specification consists of a maximum of 1096 men over the 1986-2001 period and 1921 women over the period 1983-2001. Our sample for the extended specification consists of a maximum of 3982 men over the 1986-2001 period and 6796 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

[34]See Data Appendix E.1 for more information on the sampling procedure and sample sizes in GHS.

[35]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.

[36]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 the compulsoryage (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 these observations.

[37]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.

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

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).

Summary statistics of demographic and health variables for the baseline sample are shown separately by gender in Tables 1. We also provide a breakdown of our baseline sample by survey wave and birth cohort in Table 2.

5 Results

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

In this section, we successively present the results obtained when implementing our baseline approach (see Section 3.1.1), and our preferred difference-in-differences strategy (see Section 3.1.2).

5.1.1 Baseline results

We first present the estimates of Equation (1). This specification has the disadvantage of restricting the treatment dummy to be the same for the 1958 and 1959 birth cohorts, but we relax this assumption later on. To draw general conclusions regarding the health impact of leaving school in a bad economy, we first present findings for summary indices that aggregate information over multiple treatment effect estimates. 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.[39] We then present the estimates for specific health outcomes. Estimates of Equation (1) are presented in Table 3 for men and women separately. Each line presents the marginal effect (resp. standard error and number of observations used in the model) of the treatment dummy for a different health outcome. The first two lines present our results for the two summary indices,[40] and the remaining lines display the results for specific health outcomes. 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 the study period (1983-2001). The estimated coefficient in Table 3 implies that leaving school in a bad economy increases

[39]The aggregation improves statistical power to detect effects that go in the same direction within a domain (Kling et al., 2007). 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.

[40]Note that in these cases, Equation (1) is estimated by OLS.

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 3 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 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.[41]

In this paragraph, we show the impact of having left school in a bad economy on health outcomes from a lifecourse perspective. While the estimates in Table 3 provide the average impact of the treatment over the entire period (1983-2001), Figures 6 and 7 investigate whether this impact is driven by medium- or long-term effects, for men and women respectively. For a given health outcome, the corresponding figure pictures interview-year-specific treatment effects over the period 1983-2000. 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 3. For instance, Figure 6a (resp. Figure 6b) 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 7a (resp. Figure 7b and Figure 7c) 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 health impact of leaving school in a bad economy does not seem to be particularly driven by medium- or long-term effects – as 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 study period.

A possible disadvantage of the estimates presented so far is that the impact of the treatment is linearised across the 1958 and 1959 birth cohorts. To address this potential problem, we first re-estimate our baseline specification (see Equation 1) for each birth cohort separately. The results go in the same direction. When considering the 1958 birth cohort, our model implies that over the whole period, leaving

[41] Figure 5 provides an overview of the impact of the treatment on the poor health index for each birth cohort between 1952 and 1978 (for men and women). Interestingly, a pattern seems to emerge from Figure 5. Overall, pupils who faced worse economic conditions at labour-market entry seem to be in worse health. Interestingly, this relationship is not clear-cut during the 1980s recession. As it is also a time when endogeneous timing is going on (see Figure 4), this may indicate that the existence of endogeneous timing leads us to underestimate the impact of the treatment. This may imply 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.

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. As a second step, we estimate the impact of school-leaving unemployment rate (introduced as a linear variable) on subsequent health. Estimates of Equation (3) are presented in Table 4 for men and women separately. Our estimates in Table 4 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 (1.9) percentage-point increase in women's probability of reporting poor health (consulting a GP), at the 10% and 1% significance levels respectively. 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).

5.1.2 Extended approach : a difference-in-differences strategy

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 any systematic unobservable differences between September-December- and January-August-born children. We implement a DiD strategy in which the 1952-54 and 1960-62 cohorts are used as a "control" group. Estimates of Equation (4) are presented in Table 5 separately for men and women. Each line presents the coefficient (resp. standard error and number of observations used in the model) associated with the interaction term $T_i \times D_i$ for a different health outcome (where T_i is the treatment dummy). Our results show that poor economic conditions at labour-market entry decrease women's health status by 0.31 standard deviations (at the 10% significance level) relative to women in the control group. In particular, the coefficients in Table 5 imply that women who left school in a bad economy face a 5 percentage-point increase in the probability of consulting a GP (at the 5% significance level) over the whole period (1983-2001). Using this DiD specification, we find that women are more likely to go to the hospital as an outpatient/inpatient during the twelve months preceding the interview (a 5.2 percentage-point increase, significant at the 5% level) over the whole period. Finally, women who left school in a bad economy seem to be less likely to smoke (a 6.7 percentage-point decrease, marginally significant at the 10% level) than otherwise similar women. This effect might be interpreted as a behavioural response to bad health status. Overall, the results obtained for women when implementing a DiD strategy confirm our previous findings. In particular, the DiD estimates are not statistically different in size from the ones obtained with the baseline specification (see Table 3). Our results for men, however, are not robust to the DiD specification. The results presented in Table 5 show that the effect of poor economic conditions at labour-market entry on men's smoking behaviour is no longer significant.

Scaled down by a factor of 7.5 (on average, treated women born in 1958-59 faced a 7.5 percentage-point increase in unemployment rates as compared to untreated women), our DiD estimates imply that a one-point increase in school-leaving unemployment rates leads to a 0.042 standard-deviation increase in women's poor health index. In particular, a one-point increase in school-leaving unemployment rates leads to a 0.7 (0.7) percentage-point increase in women's probability of consulting a GP (going to the hospital as a outpatient/inpatient) off a base of 21 (24) percent.

5.1.3 Mechanisms

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 re-run our DiD models and take labour-market, marriage and fertility charactereristics as outcome variables (see Online Appendix F for a detailed analysis). This analysis deserves an important caveat, however, as we consider potential outcomes at best seven years after the treatment. By then, the catch-up process in earnings or employment prospects may have already taken place. This is especially likely to be the case here, as we consider low-educated individuals.[42] Our results indeed provide little evidence that labour-market, marriage and fertility characteristics are affected from 7 to 26 years after leaving school.

In an additional – descriptive – analysis, we use GHS data over the 1974-1983 period to show short-run labour-market trajectories for different graduation cohorts.[43] Interestingly, we find clear differences in starting unemployment across graduation cohorts leading to differences in average cohort unemployment profiles. The observed initial differences in starting conditions appear to fade over time, and at the time we observe individuals in the mid-1980s, differences in unemployment profiles have already faded (see Online Appendix F for a detailed discussion).

5.2 Robustness Checks

This section performs several robustness checks using our preferred DiD specification (see Equation (4)).

5.2.1 DiD strategy : redefining the "control" group

Until now, our DiD strategy has used the 1952-1954 and 1960-1962 birth cohorts as a "control" group. As a sensitivity check, we loosen our selection criterion to consider birth cohorts ten years before/after 1958-59, for which the absolute variation in school-leaving unemployment rate was less than fifteen percent. This yields the 1949-1950,1952-1954,1960-1962 and 1966-1969 birth cohorts. The corresponding DiD estimates are presented in Table A1. The overall picture is unchanged. The coefficients presented in Table A1 lie in the same range of magnitude as those presented earlier (see Table 5), and confirm the lasting and damaging impact of poor economic conditions at labour-market entry on women's health.

One may worry that pre-1957 birth cohorts − which faced a minimum school-leaving age of 15 − may not be a valid comparison group. To the extent that the impact of poor economic conditions at labour-market entry may interact with the earliest age at which individuals have the opportunity to leave

[42]Empirical work indeed 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).

[43]We do not have information on month of birth for these waves, and we cannot implement our identification strategy. We can, however, show in a descriptive way labour-market trajectories for different graduation cohorts.

school, our DiD estimates will be biased. As a sensitivity check, we re-run our DiD models using only post-1957 cohorts as a control group (e.g. the 1960-1962 and/or 1966-69 birth cohorts). The results are virtually unchanged (results not shown but available upon request).

5.2.2 Gender heterogeneity

Overall, our results show that women's health seems to be particularly affected by poor economic conditions at labour-market entry. In contrast, men's health is virtually non affected. This gender heterogeneity may be explained for a number of reasons. As men in our sample are observed on a shorter period of time (from 1986 onwards), the rather imprecise results obtained for the latter could simply be due to a power problem. To check whether this is the case, we re-run our DiD regressions for women from 1986 onwards (e.g. excluding observations from 1983-1985). The DiD results for women (not presented) show that sample size is not likely to be the first-order explanation of the gender heterogeneity of our results. When restricting our sample to observations from 1986 onwards, leaving school in a bad economy still has a positive and significant impact on women's probability of consulting a GP (coeff : 0.057, p-value : 0.029), of going to the hospital as an inpatient/outpatient (coeff : 0.042, p-value : 0.11), of currently smoking (coeff : -0.077, p-value : 0.062), and even of drinking moderately to heavily (coeff : 0.083, p-value : 0.04). In this context, we can interpret our results as ruling out any clear and lasting impact of poor economic conditions at labour-market entry on men's health.

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 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. In particular, the results consistently show that women who left school in a bad economy have an increased probability of consulting a GP. There is also suggestive evidence that women are more likely to go to the hospital as an inpatient/outpatient and that their overall health (as measured by the health index) is negatively affected. 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. Although we find clear differences in starting unemployment across graduation cohorts, there is no evidence that these differences persist in the long run (from 7 to 26 years after school-leaving).

Overall, our results show that women's health is negatively affected by poor economic conditions at labour-market entry. In contrast, men's health is virtually not affected. This gender heterogeneity may have numerous explanations. For instance, inactivity patterns or disrupted careers may be channels through which women's health is particularly affected. Our data do not allow us to go beyond mere speculation on this matter. A promising avenue for research would consist in estimating a structural model of work, marriage and health, in which multiple mechanisms and their cumulative long-term effects would be studied over the lifecourse.

To situate our study in the literature, we benchmark our findings against the ones obtained by Maclean (2013) on the health effects of leaving school in a bad economy.[44] Using US data, Maclean (2013) considers individuals who left school between 1976 and 1992. She finds that a one percentage-point increase in the school-leaving state unemployment rate leads to a 0.5% to 18% reduction in the measured health outcomes at age 40 among men and a 6% improvement in depressive symptoms at age 40 among women. In contrast, members of our sample left school in the mid-1970s. Our results show that men's health is virtually not affected, while a one percentage point increase in the school-leaving unemployment rate leads to a 0.042 standard-deviation increase in women's poor health index and a 3% reduction in their measured health outcomes (GP and hospital consultations). This contrast may be explained by differences in terms of labour markets and social policies between the USA and Europe, but also by different graduation periods. In particular, women's careers and participation in the labour force – which differ in the two contexts – may explain these conflicting results. Another key feature of our study is that we focus on low-educated individuals, while Maclean (2013) focuses on all individuals. While Maclean (2013) speculates that failure to advance in the labor market may allow women to better balance work and family, or may protect them against work stress, this may not be true for low-educated women. In any case, investigating this gender heterogeneity would be extremely valuable and improve our understanding of the mechanisms through which leaving school in a bad economy deteriorates health.

A potential extrapolation of our findings is that the Great Recession will have lasting and negative health effects among lower-educated women. Of course, the external validity of our findings depends on the similarity between the 1958 and 1959 GLS cohorts and current cohorts of school-leavers. While 50% of pupils left school at the compulsory age in the mid-1970s, less than 20% do so at present. Women's participation in the labour force has steadily increased for the last forty years. 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 an open issue. This caveat aside, we think it is interesting and important that we find lasting health effects for female school-leavers in the 1970s. This variation in economic conditions upon graduation can potentially explain a non-negligible fraction of the health gradients across gender and cohorts.

[44] Other findings in the literature may be explained by both causation and selection mechanisms.

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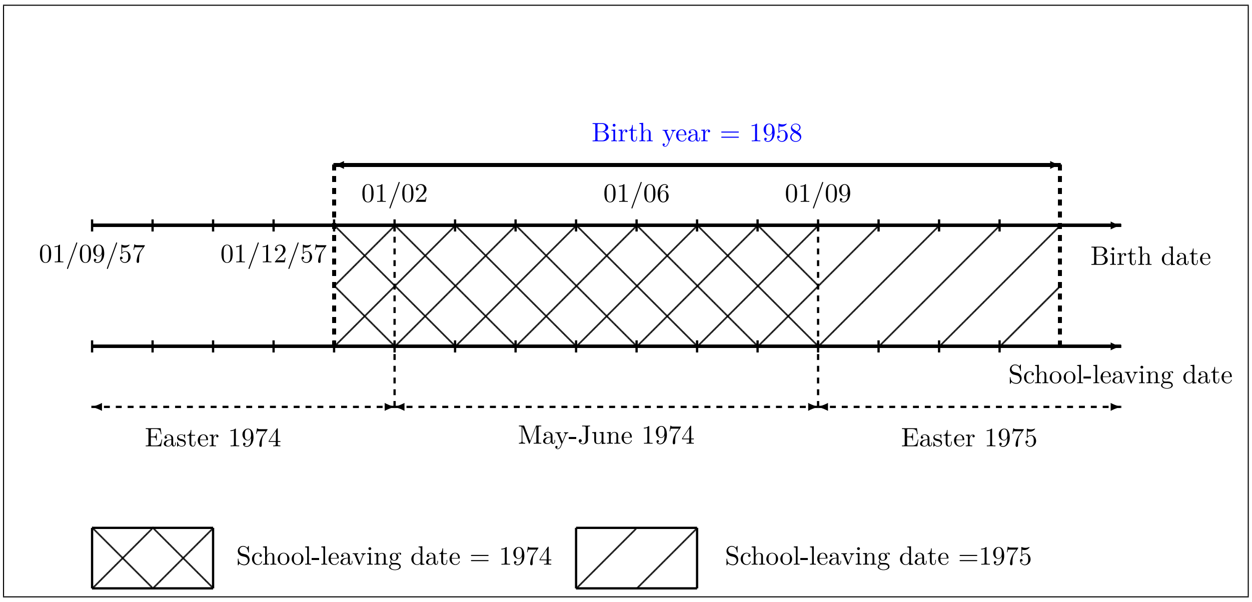
Figure 1: Compulsory schooling rules by month-year of birth

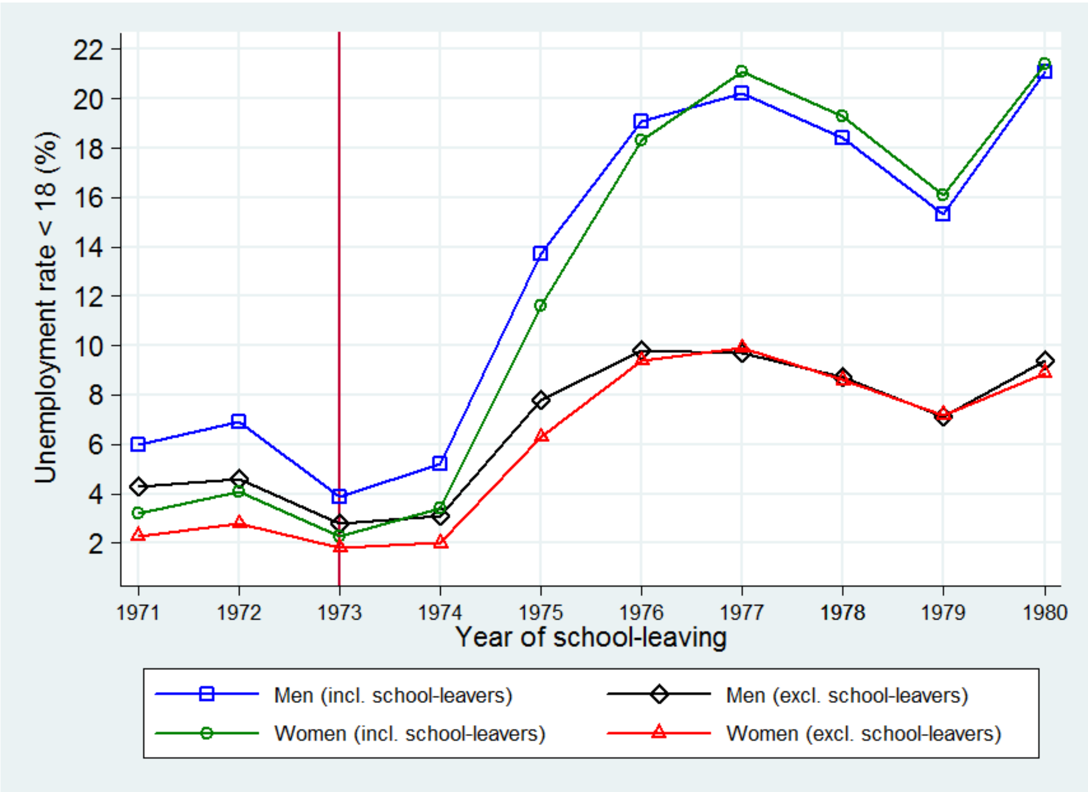
Birth year (1)	Month of birth (2)	School starting date (3)	Earliest opportunity to leave school (4)
1958	January	Sept. 1963	Easter 1974
1958	February	Sept. 1963	May/June 1974
1958	March	Sept. 1963	May/June 1974
1958	April	Sept. 1963	May/June 1974
1958	May	Sept. 1963	May/June 1974
1958	June	Sept. 1963	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



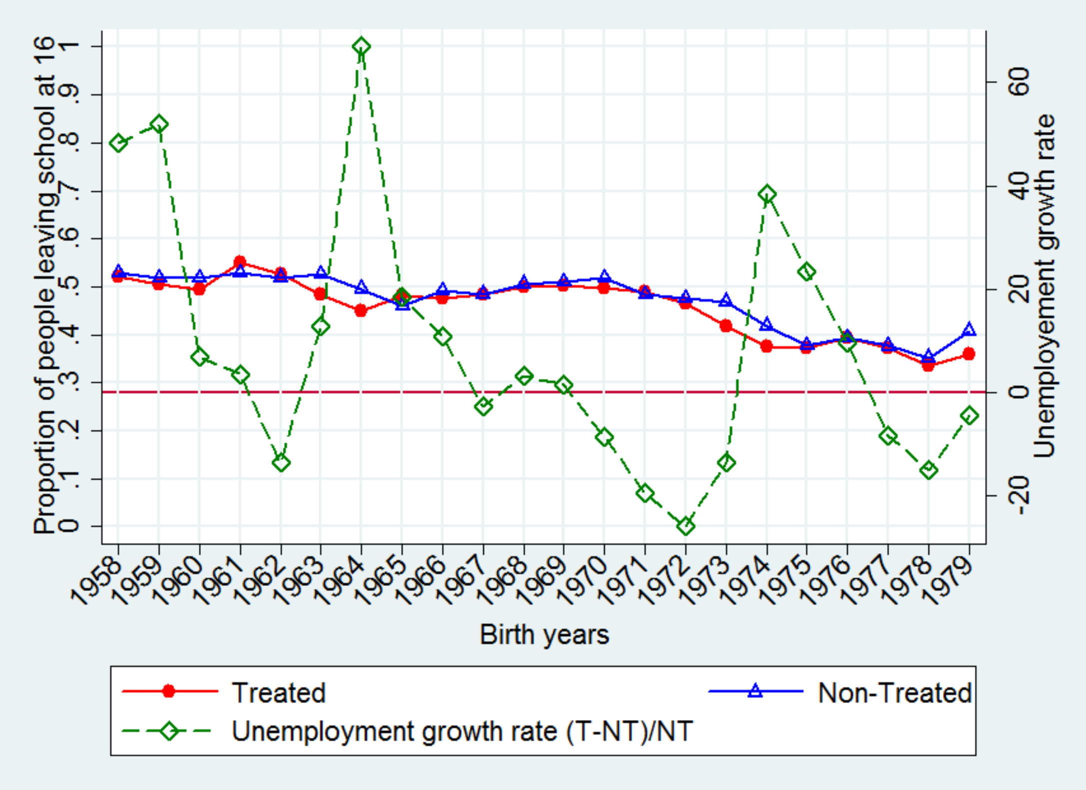
Reading : A pupil born between the 1^{st} of September 1958 and the 31^{st} of December 1958 is allowed to leave school at Easter 1975.

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



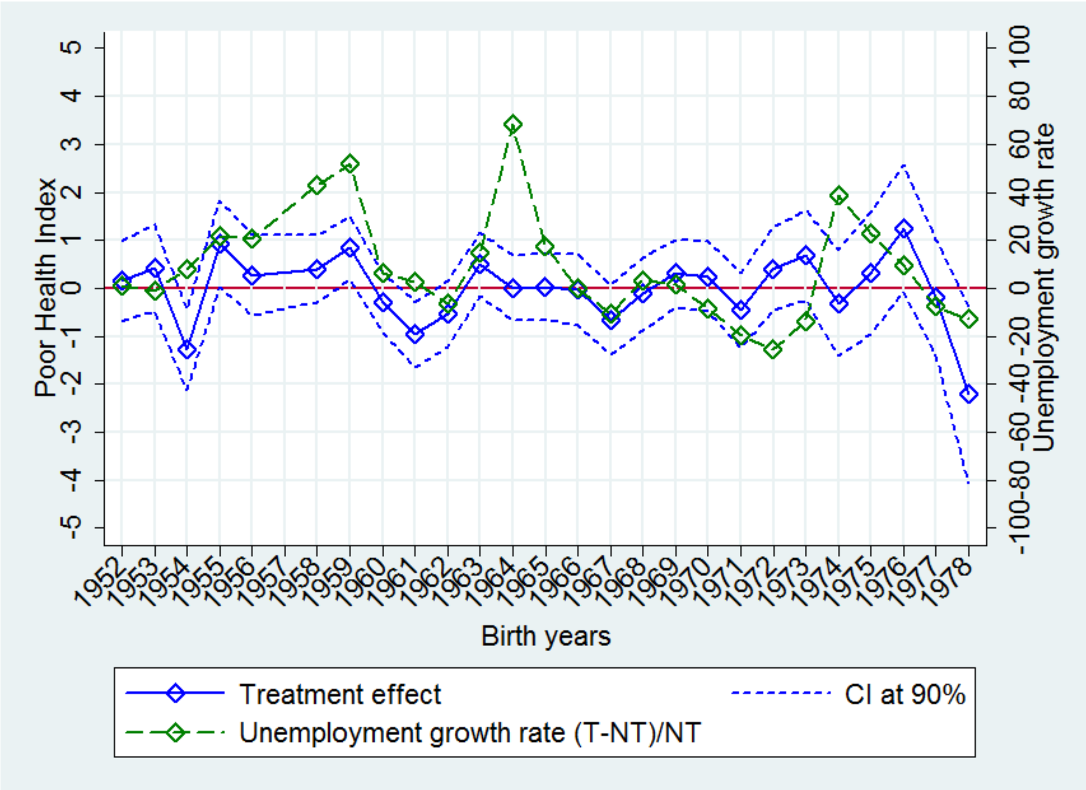
Source : Wells (1983). Note: to compute unemployment rates, numbers of unemployed under 18 were expressed as a percentage of the sum of the number of employees in employment under 18 and the unemployed under 18. The estimated numbers of registered unemployed under 18 exclusive of school-leavers are obtained 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 a more detailed discussion.

Figure 4: Proportion of pupils leaving school at the 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 : Unfortunately, unemployment rates for individuals under 18 are not available from Wells (1983) over a long period of time. We show instead unemployment rates for all individuals from administrative data – namely the monthly "registrant count" (borrowed from Denman and McDonald (1996)).

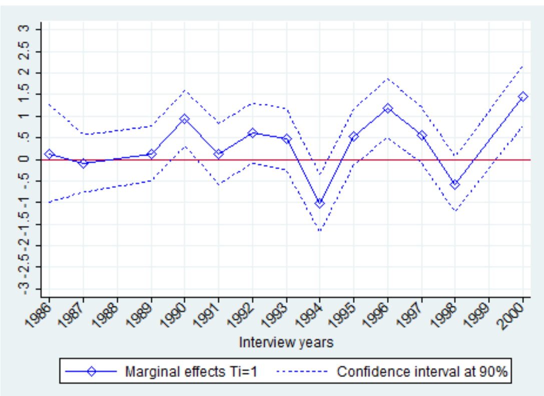
Figure 5: Impact of the treatment dummy (i.e. being born between September and December) on the poor health index for each birth cohort between 1952 and 1978, All individuals ; Growth in school-leaving unemployment rate.



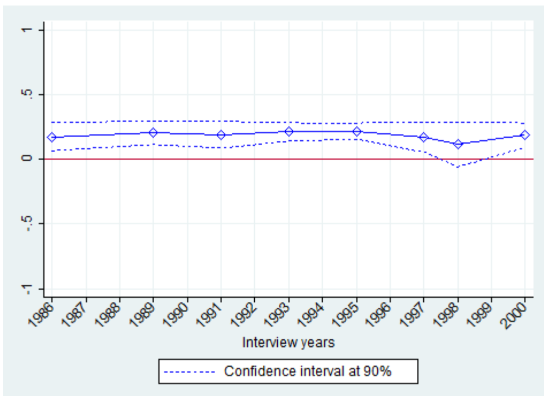
Reading: Figure 5 displays the estimated effect of the treatment dummy (i.e. being born between September and December) on the health index, for all individuals. More specifically, it shows the treatment effect obtained when estimating Equation (1) for each birth cohort separately between 1952 and 1978 (except for 1957); 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 – compared to pupils born the same year but belonging to the previous school cohort. Note : Unfortunately, unemployment rates for individuals under 18 are not available from Wells (1983) over a long period of time. We show instead unemployment rates for all individuals from administrative data – namely the monthly "registrant count" (borrowed from Denman and McDonald (1996)).

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

(a) Poor health index



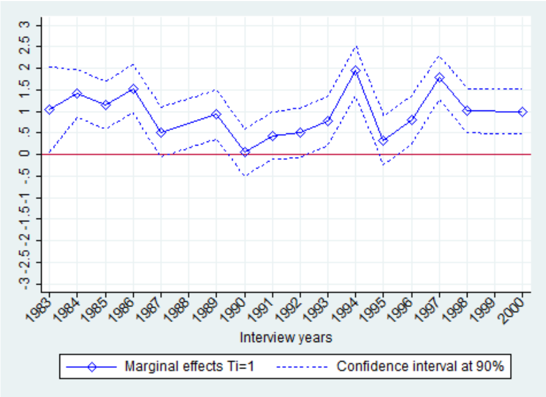
(b) Ever smoked



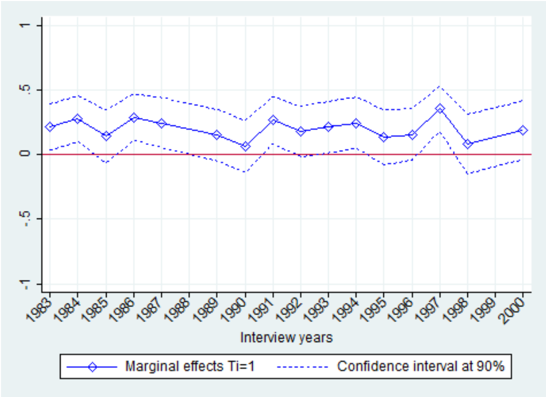
Note : Interview-year-specific treatment effects are computed by estimating Equation (1) and substituting the interaction term $T_i \times InterviewYear_i$ for T_i.

Figure 7: The impact of leaving school in a bad economy over the lifecourse. Women.

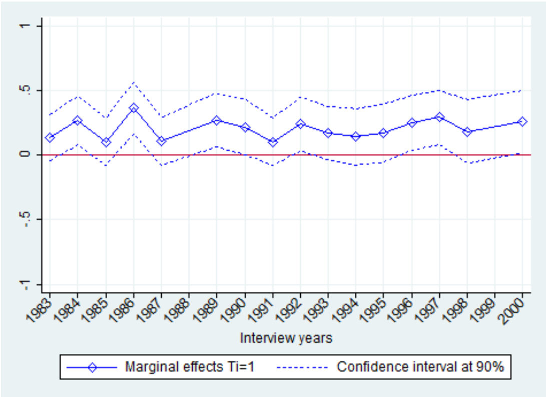
(a) Poor health index



(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 $T_i \times InterviewYear_i$ for T_i.

Table 1: Summary statistics of demographic and health variables for the baseline sample (1958-59 cohorts)

	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 2).

Table 2: Number of observations by survey wave and birth cohort for the baseline sample (1958-59 cohorts)

	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 3: The impact of leaving school in a bad economy on health outcomes – Baseline approach (1958-59 cohorts), the treatment as a dummy variable

	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 4: The impact of leaving school in a bad economy on health outcomes – Baseline approach (1958-59 cohorts), the treatment as a linear variable (school-leaving unemployment rate)

	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. Each line presents the marginal effect of youth unemployment rate at school-leaving 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 (3). 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.). Youth unemployment rates from Wells (1983).

Table 5: 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	-0.075	(0.226)	*3776*	0.314*	(0.170)	*6719*
Behaviour Index	0.223	(0.199)	*2129*	-0.100	(0.168)	*3373*
Linear probability models						
Health status						
Poor self-rated health	-0.015	(0.034)	*3782*	0.015	(0.027)	*6741*
Longstanding illness	0.000	(0.032)	*3978*	0.006	(0.024)	*6786*
Restricts activity	0.002	(0.020)	*3978*	0.004	(0.018)	*6789*
Health care						
GP consultations last 2 weeks	-0.001	(0.023)	*3978*	0.051**	(0.023)	*6787*
Hospital consultation	0.003	(0.027)	*3982*	0.052**	(0.024)	*6796*
Health behaviour						
Currently smokes	0.059	(0.048)	*2242*	-0.067*	(0.038)	*3637*
Ever smoked	0.021	(0.041)	*2242*	-0.016	(0.035)	*3637*
Moderate to heavy drinking	-0.011	(0.050)	*2130*	0.056	(0.037)	*3374*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1. "Control" birth cohorts include birth years 1952-1954 and 1960-1962. Coefficients are obtained by estimating Equation (4) by linear regressions. Each line presents the coefficient (resp. standard error and number of observations used in the model) of the interaction term $T_i \times D_i$ for a different health outcome. Our models include the treatment indicator T_i, a dummy variable D_i indicating whether the individual is born in 1958-1959, dummy variables for interview and birth year as well as a linear function of age in months. Robust standard errors in parentheses (s.e.).

Online Appendix

A Tables

Table A1: Differences-in-differences analysis : the impact of leaving school in a bad economy on health outcomes. Sensitivity check when taking the 1949-1950, 1952-1954, 1960-1962 and 1966-1969 birth cohorts as a "control" group.

	Men			Women		
	coeff	s.e.	*N*	coeff	s.e.	*N*
Linear regressions						
Poor Health Index	-0.145	(0.211)	*6892*	0.245	(0.160)	*10940*
Behaviour Index	0.188	(0.184)	*3761*	-0.094	(0.156)	*5512*
Linear probability models						
Health status						
Poor self-rated health	-0.002	(0.031)	*6907*	0.016	(0.025)	*10975*
Longstanding illness	-0.018	(0.029)	*7288*	0.006	(0.022)	*11058*
Restricts activity	-0.007	(0.019)	*7290*	-0.006	(0.017)	*11066*
Health care						
GP consultations last 2 weeks	-0.005	(0.022)	*7288*	0.041*	(0.022)	*11068*
Hospital consultation	-0.011	(0.025)	*7296*	0.045**	(0.022)	*11080*
Health behaviour						
Currently smokes	0.041	(0.045)	*4066*	-0.042	(0.035)	*6070*
Ever smoked	0.035	(0.037)	*4066*	-0.012	(0.033)	*6070*
Moderate to heavy drinking	-0.016	(0.046)	*3763*	0.044	(0.035)	*5513*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1, $^{\mu}$ p-value<0.15. "Control" birth cohorts include birth years 1949-1950, 1952-1954, 1960-1962 and 1966-1969. Coefficients are obtained by estimating Equation (4) by linear regressions. Each line presents the coefficient (resp. standard error and number of observations used in the model) of the interaction term $T_i \times D_i$ for a different health outcome. Our models include the treatment indicator T_i, a dummy variable D_i indicating whether the individual is born in 1958-1959, dummy variables for interview and birth year as well as a linear function of age in months. Robust standard errors in parentheses (s.e.).

B 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.

C Validity of the baseline strategy

In this section, we discuss the validity of our baseline strategy (see Section 3.1.1). In particular, we discuss to which extent 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 impact of differential incentives to take GCE O-level/CSE examinations as well as the importance of season-of-birth effects.

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.[45] 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, 2015; Welch, 1979). We focus on three school cohorts only, which are not likely to differ greatly in size.[46]

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.

Differential incentives to take GCE O-level/CSE examinations

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

[46] Usually, cohort sizes do not vary substantially from one year to the next. This is why studies have focused on long-term variations (typically 8-25 years) in cohort size (Morin (2015)).

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 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.[47] 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. To check this, we re-estimate our baseline model (see Equation (1)) 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, indicating that differential incentives to take GCE O-level/CSE examinations are not likely to bias our estimates to a large extent.

Season-of-birth effects

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. 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.[48] This effect suggests that even in the presence of seasonaility of births, 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.

[47] 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.

[48] 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 Endogeneous timing of school-leaving

Whether pupils in their last year of compulsory schooling strategically remain in school when the economy deteriorates is an empirical question – see Section 3.2. We provide here a detailed discussion of that matter.

To examine this question, we select individuals born between 1958 and 1979. As before, we restrict our sample to individuals who lived in England and Wales at the time the survey was conducted, and we exclude pupils born in July/August. We estimate the following equation by a linear probability model :

$$CA_i = \lambda + \delta BirthYear_i + \beta T_i \times BirthYear_i + \eta_i \tag{5}$$

where CA_i is a dummy variable taking value 1 if individual i left school at the compulsoryage, 0 else. As before, $BirthYear_i$ is a dummy variable for individual i's year of birth, and 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, hence not allowed to leave school until Easter of year *t+1*), and value 0 if non-treated (i.e., born between the 1^{st} of January and the 31^{st} of August, hence allowed to leave school as soon as in Easter or in May/June of year t). $T_i \times BirthYear_i$ is the interaction term of interest. Finally, ϵ_i denotes the error term.

The estimates corresponding to the interaction terms $T_i \times BirthYear_i$ are presented in Table A2 for men and women separately. Each coefficient yields the difference between the proportion of treated/control pupils who left school at the compulsory age for a given birth cohort. Negative and significant coefficients indicate that treated individuals had a lower probability of leaving school at the compulsory age than control pupils born the same year. A simple look at Table A2 shows that this is the case for birth cohorts 1963, 1964, 1973 and 1974 (see column (1)). Pupils born in those years reach the compulsory age in years 1979/1980, 1980/1981, 1989/1990 and 1990/1991 respectively – i.e. during the recessions of the 1980s and the 1990s. The overall picture is roughly the same when looking separately at men and women (see columns (3) and (5)). Importantly, the coefficients associated with the interaction terms for the 1958 and 1959 birth cohorts are not significant, neither for men, nor for women.

Table A2: Probability of leaving school at the compulsory age

	All		Men		Women	
	coeff	s.e.	coeff	s.e.	coeff	s.e.
	(1)	(2)	(3)	(4)	(5)	(6)
1958×Treatment	-0.006	(0.017)	0.006	(0.026)	-0.015	(0.022)
1959×Treatment	-0.012	(0.017)	0.010	(0.026)	-0.028	(0.023)
1960×Treatment	-0.025	(0.017)	-0.025	(0.026)	-0.024	(0.021)
1961×Treatment	0.020	(0.016)	0.002	(0.026)	0.032	(0.022)
1962×Treatment	0.006	(0.016)	0.014	(0.025)	0.000	(0.022)
1963×Treatment	-0.041**	(0.016)	-0.028	(0.025)	-0.049**	(0.022)
1964×Treatment	-0.045***	(0.016)	-0.060**	(0.025)	-0.034	(0.021)
1965×Treatment	0.022	(0.017)	-0.003	(0.025)	0.043*	(0.022)
1966×Treatment	-0.018	(0.017)	-0.001	(0.026)	-0.034	(0.024)
1967×Treatment	0.001	(0.018)	-0.009	(0.026)	0.009	(0.025)
1968×Treatment	-0.006	(0.018)	0.011	(0.026)	-0.027	(0.026)
1969×Treatment	-0.008	(0.019)	0.008	(0.027)	-0.024	(0.026)
1970×Treatment	-0.021	(0.019)	-0.042	(0.027)	0.000	(0.026)
1971×Treatment	0.005	(0.021)	-0.010	(0.029)	0.018	(0.029)
1972×Treatment	-0.011	(0.022)	-0.057*	(0.031)	0.031	(0.030)
1973×Treatment	-0.051**	(0.023)	-0.086***	(0.033)	-0.017	(0.033)
1974×Treatment	-0.044*	(0.025)	-0.024	(0.036)	-0.063*	(0.035)
1975×Treatment	-0.004	(0.028)	0.003	(0.041)	-0.016	(0.038)
1976×Treatment	-0.001	(0.031)	-0.031	(0.043)	0.032	(0.045)
1977×Treatment	-0.005	(0.032)	-0.013	(0.045)	0.006	(0.045)
1978×Treatment	-0.016	(0.034)	-0.046	(0.048)	0.009	(0.048)
1979×Treatment	-0.048	(0.041)	-0.076	(0.059)	-0.019	(0.056)
Observations	59,788		27,187		32,601	

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1. Coefficients are obtained by estimating Equation 5 by linear probability models. Each line presents the coefficient associated with the interaction term $T_i \times BirthYear_i$. Our models include dummy variables for birth years. Robust standard errors in parentheses (s.e.).

E Data appendix : Sample and variables in GHS

E.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.

E.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.

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

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.[49] To investigate this, we re-run our models and take labour-market, marriage and fertility charactereristics as outcome variables.

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. It also indicates the usual gross weekly earnings from the main job (available and measured in a consistent way over the 1984-2001 period), which is our main measure of earnings.[50] 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 the baseline sample (1958-59) are shown separately by gender in Tables A3 and A4.

To investigate whether labour-market, marriage and fertility characteristics are mechanisms whereby poor economic conditions at labour-market entry affect health in the long run (from 7 to 26 years after school-leaving), we re-run our DiD models and take labour-market, marriage and fertility charactereristics as outcome variables (see Equation 4). The results are presented in Tables A5 and A6. We find no effect on unemployment, inactivity patterns or earnings over the study period, neither for men nor for women (see Table A5). Women who left school in a bad economy have a lower probability of being in their current job for less than one year (coeff : -0.070), although this effect is marginally significant at the 10% level. Given that we consider 18 outcomes and have a 10% chance to reject the null given it is true, this may be due to chance only. 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 does not come as a surprise, since we consider potential outcomes at best seven years after the treatment. By then, the catch-up process in earnings or employment prospects may have already taken place. This is especially likely to be the case here, as we consider low-educated individuals. As such, our results are consistent with empirical work showing 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 A6 presents the effect of leaving school in a bad economy on marriage and fertility outcomes for men and women separately. Overall, there is little evidence that leaving school in a bad economy affects men's and women's

[49]Poor economic conditions at labour-market entry may affect subsequent family formation. On the one hand, leaving school in a bad economy may lead to a substitution effect because worse employment opportunities lower the opportunity costs of marriage and childbearing. On the other hand, negative income effects that derive from deteriorated employment opportunities may act as a barrier for family formation. In any case, family formation is likely to interplay with health outcomes in the long run.

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

marriage and and fertility outcomes in our sample.

These analyses deserve an important caveat, however, as we consider potential outcomes at best seven years after the treatment. In that sense, our models cannot provide any evidence of short-term effects of poor economic conditions at career entry. To show that there are short-term effects of the recession on labour-market outcomes, we use data from the GHS from 1974-1983. We do not have information on month of birth for these waves, and we cannot implement our identification strategy. We can, however, show labour-market trajectories for different graduation cohorts in a descriptive way. In Figures A1a and A1b, we plot the proportion of unemployed individuals across time among those who left school at age 16 for different graduation cohorts (1974, 1975 and 1976) for men and women respectively. Interestingly, one can clearly see differences in starting unemployment across graduation cohorts leading to differences in average cohort unemployment profiles. The Figures also show a clear pattern of convergence. Initial differences in starting conditions appear to fade over time. At the time we observe individuals in the data in the main analysis, differences in unemployment profiles have faded.

Table A3: Summary statistics of labour-market characteristics (1958-59 cohorts)

	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)	

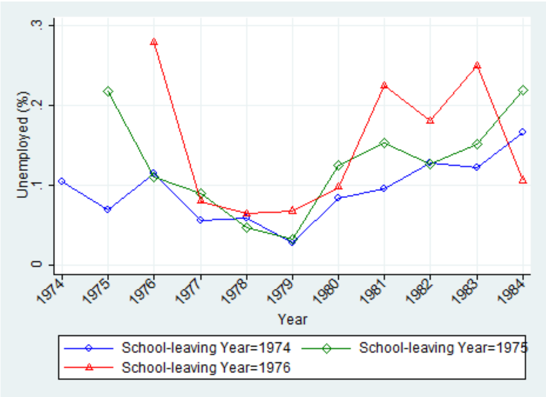
Table A4: Summary statistics of marriage and fertility variables (1958-59 cohorts)

	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*

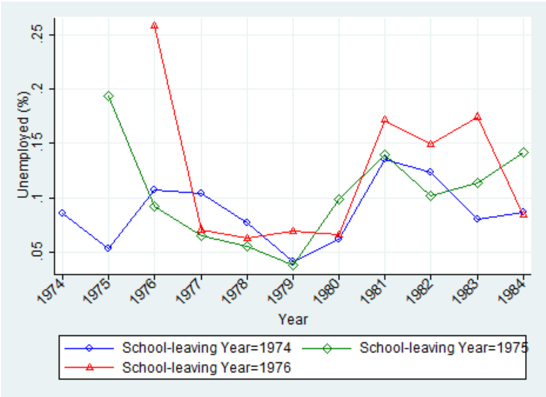
Notes : $^{(a)}$: conditional on having been married. $^{(b)}$: conditional on having at least one child.

Figure A1: Mature and entry level unemployment and experience profiles by school-leaving year.

(a) Men



(b) Women



Note : Each Figure plots average unemployment profiles by school-leaving year for male/female who left school at 16.

Table A5: Differences-in-differences analysis : the impact of leaving school in a bad economy on labour-market outcomes

	Men			Women		
	coeff	s.e.	*N*	coeff	s.e.	*N*
Economic status						
Keeping house	0.009	(0.007)	*3981*	0.003	(0.026)	*6791*
Unemployed	0.017	(0.023)	*3981*	0.005	(0.013)	*6791*
For those currently employed or self-employed						
Less than 1 month	0.009	(0.012)	*2610*	0.010	(0.015)	*3084*
Less than 3 months	0.035	(0.021)	*2610*	0.027	(0.026)	*3084*
Less than 6 months	0.040	(0.029)	*2610*	-0.006	(0.032)	*3084*
Less than 1 year	0.024	(0.036)	*2610*	-0.070*	(0.038)	*3084*
Less than 5 years	-0.003	(0.043)	*2610*	-0.031	(0.040)	*3084*
More than 5 years	0.003	(0.043)	*2610*	0.031	(0.040)	*3084*
Earnings (log)	-0.027	(0.048)	*2832*	0.096	(0.070)	*3398*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1, $^{\mu}$ p-value<0.15. "Control" birth cohorts include birth years 1952-1954 and 1960-1962. Coefficients are obtained by estimating Equation (4) by linear regressions. Each line presents the coefficient (resp. standard error and number of observations used in the model) of the interaction term $T_i \times D_i$ for a different health outcome. Our models include the treatment indicator T_i, a dummy variable D_i indicating whether the individual is born in 1958-1959, dummy variables for interview and birth year as well as a linear function of age in months. Robust standard errors in parentheses (s.e.).

Table A6: Differences-in-differences analysis : the impact of leaving school in a bad economy on marriage and fertility outcomes

	Men			Women		
	coeff	s.e.	*N*	coeff	s.e.	*N*
Marriage						
Married	-0.035	(0.034)	*3982*	0.017	(0.026)	*6795*
Divorced	0.003	(0.014)	*3982*	-0.018	(0.015)	*6795*
Single	0.034	(0.032)	*3982*	0.019	(0.021)	*6795*
Age at first marriage	0.211	(0.320)	*2843*	-0.102	(0.200)	*5612*
Fertility						
Nb of children	-	-		-0.034	(0.067)	*6718*
Age at first birth	-	-		0.129	(0.297)	*4006*

Notes : *** p-value<0.01, ** p-value<0.05, * p-value<0.1, $^{\mu}$ p-value<0.15. "Control" birth cohorts include birth years 1952-1954 and 1960-1962. Coefficients are obtained by estimating Equation (4) by linear regressions. Each line presents the coefficient (resp. standard error and number of observations used in the model) of the interaction term $T_i \times D_i$ for a different health outcome. Our models include the treatment indicator T_i, a dummy variable D_i indicating whether the individual is born in 1958-1959, dummy variables for interview and birth year as well as a linear function of age in months. Robust standard errors in parentheses (s.e.).