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Anderberg, Dan; Zhu, Yu

Published in:
Journal of Population Economics

DOI:
10.1007/s00148-013-0493-5

Publication date:
2014

Document Version
Peer reviewed version

Noname manuscript No.
(will be inserted by the editor)

What a Difference a Term Makes: The Effect of Educational Attainment on Marital Outcomes in the UK

Dan Anderberg · Yu Zhu

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Abstract In the past, students in England and Wales born within the first five months of the academic year could leave school one term earlier than those born later in the year. Focusing on women, those who were required to stay on an extra term more frequently hold some academic qualification. Using having been required to stay on as an exogenous factor affecting academic attainment, we find that holding a low level academic qualification has no effect on the probability of being currently married for women aged 25 or above, but increases the probability of the husband holding some academic qualification and being economically active.

Keywords Education, Marriage, Assortative Mating
JEL Classification: J12, J24

Dan Anderberg
Royal Holloway, University of London, Egham, Surrey, TW20 0EX, United Kingdom; IFS; CEPR; CESifo
E-mail: Dan.Anderberg@rhul.ac.uk

Yu Zhu
University of Kent, Canterbury, Kent, CT2 7NZ, United Kingdom
E-mail: Y.Zhu-5@kent.ac.uk

1 Introduction

Two stylized facts regarding the relationship between education and marriage are very well known. First, individuals who invest more in education tend to marry more educated partners than those who invest less, i.e. there is a positive assortative mating on education. Second, while individuals who invest more in education tend to marry later in life, at higher ages they are nevertheless more likely to be married.

The positive assortative mating in the marriage market has led to a popular argument that one part of an individual's economic return to acquiring education obtains through an increased probability of marrying a more qualified and higher-earning spouse (Goldin 1992). The hypothesis that, by acquiring education, an individual can affect the identity of his/her future spouse however assumes that education has a *causal* effect on the individual's marriage outcome. This is not implied by the observed positive assortative mating: whom an individual marries may well be determined by social background, geographic location, etc., factors that are also correlated with education, and could lead the observed correlation in spouses' education to be partly or wholly spurious. The degree of assortative mating also has wider implications of broad general interest. For instance Fernandez and Rogerson (2001) have, using a dynamic model of intergenerational education acquisition, fertility, and marital decisions, shown that a large increase in marital sorting will significantly increase income inequality in the long run.[1]

In this paper we present new evidence on the effect of education on marital outcomes for women using UK data.[2] To do so we exploit a within-cohort discon-

[1] Moreover, using household surveys from 34 countries, Fernandez et al (2005) find strong empirical evidence of a positive and significant relationship between several measures of the skill premium and of the degree of correlation of spouses' education (marital sorting).

[2] There are two key reasons why we focus our analysis specifically on women's marital outcomes. The first reason is a statistical one. Below we argue that there is no evidence of any impact of holding an academic qualification on women's probability of being married, thus allowing us to argue that we can identify the effect of women's education on the economic properties of their husbands. A corresponding analysis of the impact of holding a degree on men's probability of being married leads to less conclusive results, allowing us neither to rule out a positive effect nor verify it. Given that possible effect of academic qualification on selection into marriage, we then cannot identify the effect of men's education on the economic properties of their wives. The second reason relates to the outcome variable studied. For

tinuity in educational attainment induced by a unique historical school exit rule in England and Wales which forced children born after a specific cut-off date to stay on until the end of the school year during which high-stakes exit exams took place. In particular, we use that, in the past, individuals who were born in the first five months of the academic year (September through January) were allowed to leave school at the end of the spring term in the year in which they reached the compulsory schooling age of 16, whereas those born in the remaining seven months (February through August) had to stay on for one more term. For the academic cohorts that we consider, this feature, due to its interaction with the timing of examinations, implied a substantial effect of date of birth on academic attainment: those born after the January-February threshold date are significantly more likely to hold some academic qualification - typically a Certificate of Secondary Education (CSE) or, to a smaller extent, a General Certificate of Education Ordinary Levels ("GCE O-level) degree, both of which would be obtained at the age of 16 – than those born before the threshold date.[3] We thus explore how marital outcomes vary with the individual's month of birth *within* the academic cohort to which she belongs, and relate those differences to the observed differences in academic attainment. The particular identification strategy that we use enables us to overcome problems with absolute and relative age effects that would obtain from using discontinuities between academic cohorts and with general equilibrium effects that arise with alternative identification strategies, for instance based on the raising of school-leaving age.

The main findings from the paper can be summarized as follows. Using data on individuals belonging to 14 academic cohorts born between September 1957 and August 1971 from the UK Labour Force Survey we present three main findings. First,women born after the January-February threshold date (who were required to stay on for one more term) are more likely to hold some academic qualification

women, we consider whether holding an academic qualification increases the probability of the husband being economically active, and we perform this analysis under the interpretation that the husband being economically active is a favorable outcome for the wife. While a corresponding analysis could be done for men, it is less clear that the wife working would indicate a favorable outcome as it more likely would reflect specialization.

[3] Del Bono and Galindo-Rueda (2007), focusing on the wage returns to education, present similar finding using, in part, the same data.

than those born before the threshold. Second, holding an academic qualification does not affect the probability of a woman being currently married: women born before the threshold are as likely to be currently married as women born after the threshold. Third, holding an academic qualification does affect the properties of a woman's spouse: IV estimates are consistent with a strong positive causal effect of women's educational attainment on husbands' education and labor market attachment.

Since the social structures of marriage were first brought to light, much effort has been devoted to measuring marital patterns across time, countries and subgroups of the population.[4] Despite this substantial literature, surprisingly little is known about to what extent an individual's education choice affects her marital outcomes. Indeed, only a very small literature has applied statistical techniques that have allowed causal interpretations for the findings regarding the impact of education. Here we briefly review this modest literature.

Looking first at the effect of education on marital status, there appears to be a short-run effect of staying in school longer, consistent with individuals delaying marriage; however, turning to marital status later in life, education appears to have little or no effect on the probability of an individual being married. For instance Duflo et al (2010), using data from Kenya, investigate the effect of an educational program which reduced the cost of education by providing free school uniforms. The program was implemented among students enrolled in grade 6 in 2003, and was found to have reduced the probability of girls being married two years later. Similarly, Kirdar et al (2010) exploit the extension of compulsory schooling in Turkey from five to eight year in 1997. They find that the schooling reform brought about a reduction in the frequency of young (by age 17) marriages. In contrast, the analysis of Fort (2007) suggests that any effect of increased schooling on the timing of marriage must have been short-lived: exploiting the 1963 reform act in Italy which increased the minimum school leaving age from 11 to 14, Fort finds no causal effect of education on age at first marriage between ages 18-26. Turning to even longer horizons, Breierova and Duflo (2004) make use of a large school construction program in Indonesia between 1973 and 1978, the timing of which

[4] For early studies of marital patterns, see Hunt (1940), Burgess and Wallin (1943) and Rockwell (1976)

varied across regions. Using data from the 1995 Indonesian Intercensal Survey and focusing on women, the authors find that increased education leads to a higher age at first marriage, but has no impact on the probability of a woman being currently married. Further evidence suggesting no significant long run effect of education on marital status is provided by Lefgren and McIntyre (2006) who, following the approach of Angrist and Krueger (1991), use quarter of birth as instrument for educational attainment, applied on U.S. Census data. While their point estimate for the causal effect of an additional year of education on the probability of a woman being married on census day is negative, the effect is statistically insignificant.

Even less is known about to what extent positive assortative mating on education can be given a causal interpretation, i.e. to what extent an increase in an individual's education leads her to marry a more qualified spouse. However, the evidence that does exist suggests a positive causal effect. Behrman and Rosenzweig (2002) use data on 600 married female monozygotic twins from the Minnesota Twins Registry. They show that the correlation between spouses' education is significantly lower when using variation in education within twins pairs than when using cross-sectional variation. Nevertheless the authors still find that a woman's education has a causal effect on the schooling of her spouse: a one-year increase in schooling for a woman increases the schooling of her spouse by little less than 0.4 of a year. Using the same technique on Norwegian administrative data on married siblings and twins-pairs, Oreopoulos and Salvanes (2009) find that a one year increase in an individual's education increases the spouse's length of schooling by about 0.23 of a year. Lefgren and McIntyre (2006) using quarter of birth as instrument (see above) find that an extra year of education increases husband's earnings by about $4,000. Relatedly, McCrary and Royer (2011) use natality data from California and Texas, which includes information on the mother's exact date of birth. They show that women born just after the state school entry cut-off date have less education and also have less educated partners.

The rest of the paper is outlined as follows. The next section discusses the conceptual and identification issues using a simple theoretical model. Section 3 details the institutional context. After describing the data in Section 4, we present our main results in Section 5. Section 6 concludes.

2 Conceptual Issues: Equilibrium Education and Marriage

What are the channels through which an individual's educational attainment can have a causal effect on her marital outcome? There are three distinct possibilities: (i) an individual's education may impact on how *many* potential partners she meets; (ii) it may impact on *which* potential partners she meets; (iii) it may affect the *likelihood* of any given match leading to marriage.

There are relatively few available theoretical models of marriage markets with pre-marital investments in education. Chiappori et al (2009) model a frictionless marriage market in the style of Becker (1973) and Becker (1991).[5] Hence their model focuses on the last of the three channels. Here we sketch a model with frictions which allows for channel (ii). The purpose of the model is to illustrate to aid the discussion of identification. The main results to take away from the analysis are the following. First, the effect of education on the probability of marriage is generally heterogenous in the population. Second, a key requirement for the instrumental variable is that it must not directly affect with whom an individual matches, a requirement discussed in some detail below. Third, if the instrumental variable does satisfy this requirement, then it is possible to identify an average effect of education on the probability of marriage. Fourth, if furthermore, if education has no impact on the marriage probability (for those whose education decisions depend on the instrument), it is also possible to identify an average effect of education on partner's skill level.

2.1 A Simple Model of Education and Marriage

Consider an economy with an equal number of women and men.[6] Each individual i is associated with an k-dimensional vector of characteristics α_i which has a discrete distribution, represented by a p.d.f. $f(\boldsymbol{\alpha})$ defined over a support A. There is binary variable $z_i \in \{0, 1\}$ which is independent of the individual's other characteristics.

Assumption 1 *The instrument is independent of the individual's other characteristics,* $z_i \perp \alpha_i$.

[5] See also Peters and Siow (2002).

[6] The current model draws in part on the model by Konrad and Lommerud (2010).

Individual i decides whether or not to invest in education, $x_i \in \{0,1\}$, at a utility cost $c(\boldsymbol{\alpha}_i, z_i)$.

ASSUMPTION 2 *The instrument reduces the cost of education,* $c(\boldsymbol{\alpha}_i, 1) < c(\boldsymbol{\alpha}_i, 0)$ *for all* $\alpha_i \in A$.

After deciding on education, individual i meets with one potential partner, denoted $-i$. The potential partner, $-i$, may be either skilled or unskilled, $x_{-i} \in \{0,1\}$.

ASSUMPTION 3 *The probability that individual* i *matches with a skilled potential partner depends on her characteristics and on her skill level and is denoted* $p(\boldsymbol{\alpha}_i, x_i)$.

Note that $p(\boldsymbol{\alpha}_i, x_i)$ is an *equilibrium* probability which crucially does not depend directly on z_i. The match between i and $-i$ is associated with match quality $\theta_i \in R$ which enters the utility of both partners additively if they marry. θ_i is a continuous random variable with c.d.f. $G(\theta_i)$. The skill level of individual i determines her earnings $y_i \in \{y_0, y_1\}$ where $y_1 > y_0$. If individual i does not marry, her consumption is her own earnings $c_i = y_i$. If she does marry, then her consumption is $c_i = (1-\gamma) y_i + \gamma y_{-i}$ where $\gamma \in [0, 1/2]$ indicates the degree of consumption sharing which is assumed to be positive and fixed.

2.2 The Marriage Decision

Individual i will agree to marry $-i$ if and only if $(1-\gamma) y_i + \gamma y_{-i} + \theta_i \geq y_i$. However, for the match to lead to marriage, $-i$ must also agree to marry i. If they have the same skill level, they marry if and only if $\theta_i \geq 0$. If one is skilled the other is unskilled, they marry if and only if $\theta_i \geq \gamma \Delta y \geq 0$ where $\Delta y \equiv y_1 - y_0$. Define $\pi_s \equiv 1 - G(0)$ and $\pi_m \equiv 1 - G(\gamma \Delta y)$ as the probability of marriage conditional on a "skill-symmetric" (s) match and a "mixed-skill" (m) match, respectively.

2.3 The Investment Decision

The benefit to individual i from investing in education is given by the change in expected utility from consumption and match quality, $B(\boldsymbol{\alpha}_i) \equiv E[c_i + \theta m_i | \boldsymbol{\alpha}_i, x_i = 1] -$

$E[c_i + \theta m_i | \boldsymbol{\alpha}_i, x_i = 0]$.[7] Individual i invests if and only if the benefit exceeds the cost, $x(\boldsymbol{\alpha}_i, z_i) = I_{B(\boldsymbol{\alpha}_i) \geq c(\boldsymbol{\alpha}_i, z_i)}$. Since z_i reduces the cost of education, there will generally be a set of types who invest if and only if $z_i = 1$. Hence define the (equilibrium) set

$$A^* \equiv \{\boldsymbol{\alpha}_i \in A | x(\boldsymbol{\alpha}_i, 1) = 1 \text{ and } x(\boldsymbol{\alpha}_i, 0) = 0\}, \tag{1}$$

and assume that this is a non-empty subset of A. Following Angrist et al (1996) we refer to A^* as the set of "compliers".[8]

2.4 The Effect of Education on Marital Outcomes

Let $\mu(\boldsymbol{\alpha}_i, x_i)$ denote the marriage probability of individual i given her characteristics and skill level. The *effect of education* on the marriage probability of individual i is then

$$\Delta\mu(\boldsymbol{\alpha}_i) \equiv \mu(\boldsymbol{\alpha}_i, 1) - \mu(\boldsymbol{\alpha}_i, 0) = \{p(\boldsymbol{\alpha}_i, 1) - [1 - p(\boldsymbol{\alpha}_i, 0)]\}(\pi_s - \pi_m), \tag{2}$$

which may be either positive or negative and is generally heterogenous in the population. There is one interesting case in which educational attainment has no impact on the individual's marriage probability. This is when the probability of a "skill-symmetric match" is the same for either education choice: $p(\boldsymbol{\alpha}_i, 1) = 1 - p(\boldsymbol{\alpha}_i, 0)$. This may hold for some types but not for others.

Consider next the effect of education on the partner's skill level, defined as the difference in the probability of i's partner being skilled *conditional* on her being

[7] It can be shown that the benefit takes the form

$$B(\boldsymbol{\alpha}_i) = \Delta y + (\theta_s - \theta_m)\{p(\boldsymbol{\alpha}_i, 1) - [1 - p(\boldsymbol{\alpha}_i, 0)]\} - \gamma\pi_m \Delta y \{[1 - p(\boldsymbol{\alpha}_i, 1)] + p(\boldsymbol{\alpha}_i, 0)\},$$

where $\theta_s \equiv E[\theta I_{\theta \geq 0}]$ and $\theta_m \equiv E[\theta I_{\theta \geq \gamma \Delta y}]$ (where I_S is the indicator function which is unity if S is true and zero otherwise), with $\theta_s > \theta_m$.

[8] For the current purposes we do not need to consider the equilibrium distribution in education choice in the population. Suffices to say that a complete description of the equilibrium would need to specify the matching technology and then a characterization of two mutually consistent functions $x(\boldsymbol{\alpha}, z)$ and $p(\boldsymbol{\alpha}, x)$ as either depend on the other.

married.[9] Note that this is given by

$$\Delta x_{-i}(\boldsymbol{\alpha}_i) \equiv \frac{p(\boldsymbol{\alpha}_i, 1)\,\pi_s}{\mu(\boldsymbol{\alpha}_i, 1)} - \frac{p(\boldsymbol{\alpha}_i, 0)\,\pi_m}{\mu(\boldsymbol{\alpha}_i, 0)}. \tag{3}$$

A sufficient condition for $\Delta x_{-i}(\boldsymbol{\alpha}_i)$ to be strictly positive is that $p(\boldsymbol{\alpha}_i, 1) \geq p(\boldsymbol{\alpha}_i, 0)$ and $\pi_s \geq \pi_m$, with at least one inequality being strict. Hence as long as (i) investing in education leads to a better chance of matching with a skilled potential partner, and/or (ii) skill-symmetric matches have a higher acceptance rate than mixed-skill matches, education will have a positive effect on partner skill level. Note that $\Delta x_{-i}(\boldsymbol{\alpha}_i)$ can be non-zero even if education has no effect on the individual's marriage probability.

2.5 Identification

Assume now that we have a random sample of women. For each woman we observe her marital status m_i, educational attainment x_i, instrument z_i, and, if married, the educational attainment of her partner, x_{-i}. Given that z_i only influences m_i via x_i an IV approach can be expected to identify the effect of educational attainment on marriage. To this end, note that, using the law of iterated expectations, $E[m_i|z_i] = E[\mu(\boldsymbol{\alpha}_i, x(\boldsymbol{\alpha}_i, z_i))\,|z_i]$. Taking the difference between $z_i = 1$ and $z_i = 0$, exploiting the independence of $\boldsymbol{\alpha}_i$ and z_i and noting that only compliers contribute to this difference, yields that

$$\frac{E[m_i|z_i = 1] - E[m_i|z_i = 0]}{E[x_i|z_i = 1] - E[x_i|z_i = 0]} = E\left[\Delta\mu(\boldsymbol{\alpha}_i)\,|\boldsymbol{\alpha}_i \in A^*\right]. \tag{4}$$

The IV/Wald estimator, which replaces the expectations on the left hand side with the corresponding sample means, is hence a consistent estimator for the average effect of education on marriage probability among compliers (Imbens and Angrist 1994).

Identification of the effect of education on partner's skill level is complicated not only by endogeneity of x_i but also by selection into marriage. In the absence of a second instrumental variable (affecting marriage probability), we can identify the

[9] Formally, we define the effect of education on the partner's skill level as

$$\Delta x_{-i}(\boldsymbol{\alpha}_i) \equiv \Pr(x_{-i} = 1|m_i = 1, \boldsymbol{\alpha}_i, x_i = 1) - \Pr(x_{-i} = 1|m_i = 1, \boldsymbol{\alpha}_i, x_i = 0).$$

Expression (3) follows from applying Bayes' rule.

effect of education on partner's skill level only under the assumption that education does not affect the probability of an individual being married. Specifically, we need the marriage probability to be skill-independent for those types whose education decisions are affected by the instrument.

ASSUMPTION 4 *The marriage probability is skill-independent for all "compliers":* $\mu(\boldsymbol{\alpha}_i, 1) = \mu(\boldsymbol{\alpha}_i, 0) = \widehat{\mu}(\boldsymbol{\alpha}_i)$ *for all* $\boldsymbol{\alpha}_i \in A^*$.

Assumption 4 implies that $E[m_i|z_i]$ is independent of z_i, which is of course what is tested by the IV/Wald estimator in (4). Under Assumption 4 it can be shown that

$$\frac{E[x_{-i}|m_i = 1, z_i = 1] - E[x_{-i}|m_i = 1, z_i = 0]}{E[x_i|m_i = 1, z_i = 1] - E[x_i|m_i = 1, z_i = 0]} = E_{\widehat{f}}\left[\Delta x_{-i}(\boldsymbol{\alpha}_i) | \boldsymbol{\alpha}_i \in A^*\right], \quad (5)$$

where the notation $\widehat{f}$ indicates that the expectation over $\boldsymbol{\alpha} \in A^*$ is taken using the marriage-probability weighted density $\widehat{f}(\boldsymbol{\alpha}_i) \equiv \widehat{\mu}(\boldsymbol{\alpha}_i) f(\boldsymbol{\alpha}_i) / \sum_{\boldsymbol{\alpha} \in A^*} \widehat{\mu}(\boldsymbol{\alpha}) f(\boldsymbol{\alpha})$ rather than the standard conditional density. The IV/Wald estimator in (5), which replaces the expectations on the left hand side with the corresponding sample means, is hence, under the additional Assumption 4, a consistent estimator for the average effect of educational attainment on partner educational attainment among compliers.

2.6 Further Issues

The above stylized model captures the following features. Each individual is associated with a set of unobservable background characteristics – for instance ability and parental background – which play two roles: (i) they affect the individual's cost of education, and (ii) they may directly affect the individual's probability of matching with a skilled potential partner. The individual's education choice may also impact on her probability of matching with a skilled potential partner. This can be through very direct routes whereby prospective partners meeting in education or for instance by affecting the individuals subsequent labour market choices and hence her social contacts. The model generally features assortative mating on educational attainment which may or may not be casual. Identification of any causal effect can be obtained if there is an suitable instrument for educational attainment.

The crucial assumption is that the instrument z_i affects the probability of matching with a skilled potential partner only via the individual's chosen education. This is non-trivial requirement that will fail for a number of commonly used instruments for educational attainment. Consider for instance using the raising of a school leave age (RoSLA) as instrument for education. The instrument z_i would be "switched on" for birth cohorts born after a certain date. However, since an individual's academic cohort naturally defines her social contacts, the instrument will directly affect the probability of matching with a skilled potential partner. Intuitively, comparing the marriage outcomes of individuals born before and after the RoSLA cutoff birth date will confound individual effects of educational attainment with general equilibrium effects of a generally increased level of educational attainment in the marriage market. The same comments apply to for instance distance to college since individuals naturally interact socially with others in their local environment.

The current setting where there is an educational-attainment relevant threshold date within the academic year hence provide a unique opportunity to study the effect of academic qualifications on marital outcomes.[10] The potential threat to identification in the current setting would include a (i) correlation between month of birth and unobservable characteristics, and (ii) selective interaction with peers based on month of birth. Both issues are explored in the paper. In particular we show that individuals born before and after the January-February threshold date have similar family background characteristics and that there is no evidence of any correlation in partners' month of birth.[11]

[10] Note that even using date of birth relative to the cutoff date *between* academic cohorts (in our case, the August-September threshold) could fail the identifying assumption in so far as an individual's academic cohort constitute a marriage-market relevant social grouping. This is suggested by the finding of McCrary and Royer (2011) who use date of birth relative to school entry cutoff as instrument for educational attainment for mothers in Texas and California, and find that mothers born after the school entry cutoff date have younger partners.

[11] Note also that the analysis of identification assumes that education precedes marriage. In the analysis below, we will use as measure of educational attainment an indicator for whether an individual holds any academic qualification. Since this is typically determined by exams taken at the age of 16 in the UK, we perceive this to be a negligible issue.

3 Institutional Context

The school education system in the UK is divided into three stages: primary education, compulsory secondary education and post-compulsory secondary education. While the education and training systems of England, Wales and Northern Ireland are broadly similar, the education system in Scotland has always been completely independent with its own laws and practices. In the following, we will focus on the education system in England and Wales.[12]

The academic year runs from September 1 to August 31 and children start school in the academic year in which they turn 5. More critical for our purposes however is the school exit policy that used to exist. The British government has raised the minimum school leaving age several times since the introduction of compulsory education in 1870. The current school leaving age of 16 has been in force since September 1973, as a result of the Raising of School Leaving Age Order of 1972. Unlike in the US, children in the UK are generally not deemed to have attained the age of compulsory schooling on the exact date in which they themselves attain the age of 16. Instead, since the Education Act of 1962 and up until 1997, the minimum school leaving age arrangements were as follows. A child whose sixteenth birthday fell in the period September 1 to January 31 inclusive, was allowed to leave compulsory schooling at the end of the Spring term (which ends just before Easter). A child whose sixteenth birthday fell in the period February 1 to August 31, was allowed to leave on the Friday before the last Monday in May.[13]

From 1998 onward, a new single school leaving date was set as the last Friday in June in the school year in which the child reaches the age of 16. However, since our empirical analysis will focus on individuals who attained the minimum school leaving age of 16 during the 1970s and the 1980s, the dual school leaving arrangements outlined above will be the relevant ones for our purposes.

[12] The education system in Northern Ireland differs slightly from that in England and Wales. For instance, the cutoff date between academic cohorts is July 1 in Northern Ireland as opposed to September 1 in England and Wales. For this reason we will not include Northern Ireland in the analysis below.

[13] The justification for dual exit dates seems to have been the belief that a common exit date, given the share of students leaving school at the minimum age, would negatively affect the functioning of the labor market.

Hence the academic cohorts that are the focus of our study were each split in two groups: the September-January born individuals who were allowed to leave school at Easter, and the February-August born individual who were required to stay on until the end of the school year. The particular school leaving rule thus generated a threshold date *within* each each academic cohort. There would of course also be a threshold date (September 1) *between* academic cohorts. However, focusing on the within-cohort threshold has many advantages compared to between-cohort threshold.

Those born after the between-cohort threshold would start school later than those born before it and would belong to a one-year later academic cohort. Hence those born after the between-cohort threshold would have a higher *absolute* age at school start and also a higher age *relative* to their academic cohort peers. Since both absolute and relative age effects on academic attainment have been observed (Crawford et al 2007) using this threshold would be potentially problematic. In contrast, for the within-cohort discontinuity, individuals born on either side of the threshold would start school at the same age and have effectively the same relative age within their cohort, thus avoiding absolute and relative age effects confounding the analysis. Hence our analysis will focus exclusively on the January-February within-cohort discontinuity.

The significance of the within-cohort discontinuity is, however, not only that it implies a nominal difference of up to two months (one term) of required schooling. More importantly, for the cohorts that is the focus of our analysis the school leaving policy interacted strongly with the qualification system in England and Wales under which students aged 16 sit high-stakes exit examinations at the end of the school year.

3.1 Exams Sat at 16

At the end of five years of compulsory secondary education, students in England and Wales take exams in a range of subjects. Students who were academically inclined and attended "grammar schools" would take General Certificate of Education Ordinary Levels ("GCE O-level) examinations. In contrast, less academically oriented students attending "secondary modern schools" could take the Certificate of Secondary Education (CSE) examinations at 16 before leaving school. Less

demanding than GCE O-level, results in the CSE exams were nevertheless graded on the same scale, with the top CSE grade, grade 1, being equivalent to a simple pass at GCE O-level.

The introduction in 1988 of the General Certificate of Secondary Education (GCSE), which superseded the O-level and CSE exams, marked a turning point in UK educational system. The GCSE is a single subject exam and students usually take up to ten GCSE exams in different subjects. Students are given a letter score of A-G where A is the top grade. Although grades A-G are all officially pass grades, only grades A to C are generally regarded as equivalent to the "pass" grades in the previous O-level system.

Our empirical analysis will focus on the academic cohorts that faced the previously existing O-level/CSE system for which we observe a significant difference in academic attainment by date of birth relative to the January-February threshold. With the introduction of the more inclusive GSCE system, the fraction of individuals holding some academic qualification increased and the date of birth effect is no longer apparent. Moreover, we will focus on those cohorts that faced the minimum school leaving age of 16. Under the previous age of 15, whether or not a student could leave at Easter was less consequential since leaving at the earliest possible date meant leaving school a year prior to the qualifications-generating examinations sat at age 16.[14] Hence in our analysis below, the main focus will be on individuals born after September 1957 (and hence born late enough to face the current age 16 minimum school leaving age) but born before August 1971 (and hence born early enough to face the previous O-level/CSE examination system).

As a measure of academic attainment we will also focus mostly on whether an individual holds *any academic qualification* (as opposed to holding no academic qualification). There are several reasons for doing so, generally having to do with timing and exams. Generally individuals tend to obtain academic qualifications in a certain sequence, implying that higher levels of qualifications are obtained at higher ages. Beyond the age of 16, students in England and Wales sit A-level exams (an "Advanced Level" examinations relevant for entry into higher education) at the age of 18 before going on to obtain first and higher degrees in higher

[14] Even when the minimum school leaving age was 16, students leaving at Easter had the option of returning for exams and evidence suggests that a substantial fraction of students did so (Del Bono and Galindo-Rueda 2007).

education. Whether or not an individual will ever obtain *any* academic qualification is therefore typically determined by the examinations taken at age 16. Hence when we consider any given academic cohort, as we observe them across time (i.e. as they age), the cohort members will tend to gradually improve their academic qualifications. However, the fraction of cohort members who hold *any* academic qualification is effectively constant across time from age 16 onwards. Also, as shown below, the instrument that we use for our analysis – the individual's date of birth relative to the within-cohort January-February threshold date – particularly affected individuals on the no academic qualification versus a first academic qualification margin by affecting the individual at the age of 16. A further benefit of focusing on the education outcome at age 16 is that it would have preceded any marriage decision for the vast majority of individuals.

4 Data and Sample

The data we will use comes from the UK Labour Force Survey (LFS) which is the largest regular household survey in the United Kingdom and is intended to be representative of the whole UK population. The sample design currently consists of about 60,000 responding households every quarter, representing about 0.2% of the British population. Prior to 1992 LFS data is available on an annual basis, based on interviews taking place in the Spring (March-May). However, since 1992 LFS data is available on a quarterly basis.[15] The LFS is suitable for our purposes due to its size and since it contains the basic information needed for our application: year and month of birth, educational attainment, and marital status. We also use information on ethnicity and employment status. We pool all data from the survey years 1984 to 2006. The LFS surveys prior to 1983 are not comparable with later surveys because of inconsistencies in measurement, definitions and coverage, while 2006 is the last year for which month of birth has been made publicly available.

[15] Indeed, with the restructuring of the LFS in 1992, the survey was transformed into a "rotating panel". Each quarter's LFS sample is made up of five "waves". Each wave is interviewed in five successive quarters, such that in any one quarter, one wave will be receiving their first interview, one wave their second, and so on, with one wave receiving their fifth and final interview. However, since we are not interested in time varying characteristics or outcomes, we will not be making use of the panel structure of the LFS. Instead we will only be using information provided by individuals in their first interview.

The sample criteria we use are as follows. We select women (i) born and currently living in England or Wales,[16] (ii) who are aged 18 or above at the time of interview,[17] and (iii) born between September 1957 and August 1971 and hence belonging to the 14 academic cohorts who faced the current minimum school leaving age of 16 but the pre-GCSE academic qualification system. This sample criterion gives us a main sample of 226,965 women. Figure 1 provides basic information about the distribution of the age, survey year, academic cohort (where academic cohort = 57 for respondents born between September 1957 - August 1958 etc), and also the rate at which the respondents are currently married by age.

Insert figure 1 here.

For each individual we have information on year and month of birth. For marital status we will focus exclusively on whether or not the respondent is currently married: the survey does not allow us to determine any details of the individuals' marital histories.[18] We observe the current employment status of each individual and label an individual as being "economically active" if currently employed or self-employed.

With regards to educational attainment we have several pieces of information. We have information on the individual's highest academic qualification. For descriptive purposes we classify academic qualifications into five "levels" where (i) "Level 1" denotes a CSE qualification, (ii) "Level 2" denotes an O-level qualification, (iii) "Level 3" denotes an A-level qualification, (iv) "Level 4" denotes a first

[16] Prior to 2001 there is no information about in which country of the UK individuals are born. We then keep those born in the UK and currently living in England and Wales. Hence for earlier survey years there is some unavoidable degree of noise due to migration from Scotland and Northern Ireland.

[17] We do not impose any explicit upper limit on age. However, per construction, the oldest individual that will be included in the data will be someone born in the Autumn of 1957 and observed in the Autumn of 2006. Hence no one in the main sample will be aged above 50.

[18] We focus on whether the respondent is currently married. Hence we do not measure "partnership status" which would include cohabitation. Unfortunately cohabitation can only be consistently identified in the data from 1991 at which point the cohabitation rate was 6 percent while the married rate was 76 percent. More generally, about 40 percent of women in the main cohorts of interest had ever cohabited *at some point in their lives* by the age of 40 (Beaujouan and Bhrolchin 2011). However, in most cases this constituted pre-marital cohabitation.

degree (or equivalent), and (v) "Level 5" denotes a higher degree at postgraduate level. However, as noted above, for the main part of the analysis we will simply focus on whether the respondent holds any academic qualification. Note that the Level 1 and the Level 2 qualifications would have been obtained through examinations taken at the minimum school leaving age of 16 whereas the higher level qualifications would have been obtained through post-compulsory education. We also have information about the age at which the respondents left continuous full time education which we will also use at some stage below.

Table 1 provides summary statistics broken down by current marital status. As expected, married women are, on average, older than unmarried women. Married and unmarried women have very similar economic activity rates in the current sample. Married women more frequently hold some academic qualification. The table also shows that there are relatively few ethnic minority women in our sample, largely due to our focus on individuals born in the UK. Hence we will not be able to separately consider ethnic minorities in the analysis below. Among the husbands to the married women in the sample, 68 percent hold some academic qualification and 90 percent are economically active. As a short-hand we refer to individuals born in the months February-August as "required to stay on". This group constitute 59 percent of the sample.

Insert table 1 here.

In the empirical analysis below the instrument used is based on month of birth (MoB), with z_i "switched on" for those born in months that would make them required to stay on,

$$z_i = \begin{cases} 1 \text{ if } MoB_i \in \{2, ..., 8\} \\ 0 \text{ else} \end{cases}. \tag{6}$$

4.1 Preliminary Analysis of the Validity of the Instrument

Having introduced the data, we can, as a preliminary step, explore the validity of the instrument. As discussed above, two key features are required of the instrument in order for the analysis to be valid. First, z_i should be unrelated to the individual's (unobserved) personal characteristics at least for individuals close to

the threshold.[19] A key concern here is if month of birth is correlated with key family background factors that may affect both the individual's educational attainment and also her marital outcome.[20] In order to explore this, we construct a youth sample from the LFS. In particular we construct a sample of individuals who are born in the last four of the academic cohorts that are the main focus of our analysis and who live in the same household as their parents. Using this youth sample we can whether the instrument – the dummy variable for having been born in February or later in the academic year – is correlated with parental economic characteristics. The economic characteristics that we focus on are the same as used in the main analysis of spouse characteristics, namely whether the father/mother hold any academic qualification and whether the father/mother is economically active. Further details on the youth sample and the analysis is presented in the Appendix. The analysis find no evidence of any difference in parental characteristics of individuals born before versus after the January-February threshold.

Second, the instrument z_i should not be correlated across individuals who interact socially in the marriage market. In principle it could be that individuals who are born in the first half of the academic year tend to interact disproportionately among themselves as they are the relatively older members of each academic cohort. If that was the case, then there could be a positive correlation between the instrument for the individual z_i and that of her potential partners, z_{-i}. This can be checked by considering the correlation in the requirement to stay on among married couples. Estimating this in the married sample yields a correlation of 0.002 which is not significant at any level. Hence there is no evidence of any correlation in the partner's month of birth relative to the January-February threshold.

[19] Formally, unbiasedness obtains in the limit as the window size is reduced to zero. See for instance Hahn et al (2001) for a theoretical discussion and McCrary and Royer (2011) for an application.

[20] The use of quarter of birth as an instrument for educational attainment in the US context has recently been criticized by Buckles and Hungerman (2008) They highlight, for instance, that women giving birth in the winter months are more often teenagers, less frequently married, less frequently white, less educated and younger.

5 Results

We present our results in three subsections. In the first subsection we consider how academic attainment varies with month of birth. We show that those who were required to stay on are significantly more likely to hold *some* academic qualification. In particular, the gap in attainment obtains on the margin between holding no academic qualification and holding some low level (level 1 or 2) qualification. We show that the gap in academic attainment diminishes in later cohorts. We show that academic attainment changes monotonically at the January-February threshold: those born after the threshold date (and hence would have been required to stay on) have uniformly higher academic attainment than those individuals born before the threshold date.[21]

In the second subsection we look at marital status. After verifying that individuals with academic qualifications are, at higher ages, more likely to be married, we consider in detail how the probability of being married varies with month of birth. We find no consistent evidence of any such relation. In particular, we cannot find any evidence that those who were required to stay on for the extra term are either more or less likely to be married. Hence we conclude that there was no causal effect of holding an academic qualification on the probability of being currently married.

In the final subsection we restrict the sample to married women and look at the characteristics of their spouses. After verifying that holding some academic qualification is strongly positively correlated with the spouse holding some academic qualification and being economically active, we consider whether the husband's characteristics vary with the woman's month of birth. Our findings suggest that women who were required to stay on more frequently are married to husbands who hold some academic qualification and who are economically active. Hence our estimates suggest a causal effect of the woman's academic qualification on the economic properties of her spouse.

[21] In contrast, academic attainment does *not* change monotonically at the threshold between academic cohorts: while those born before this (August-September) threshold are more likely to hold some low level qualification, those born after the threshold are more likely to hold some higher level qualification.

5.1 Month of Birth and Academic Attainment

We begin with an analysis of the relationship between month of birth and academic attainment.[22] Figure 2 plots the distribution of highest academic qualification by month of birth. There is a marked increase in the fraction holding a level 1 academic qualification at the January-February threshold, along with a corresponding drop in the fraction holding no academic qualification. The figure also suggests that having been required to stay on is potentially associated with a smaller increase in the probability of holding a level 2 academic qualification. For higher qualifications there is no clear indication of any discontinuity at the January-February threshold date. Hence a first pass suggests that the Easter Leaving Rule that applies at the age of 16 only seem to have affected the two levels of qualifications that are obtained at the age of 16 – that is the CSE and the O-level – and not any qualifications obtained at higher ages.

Insert figure 2 here.

A key requirement for the instrumental variable approach to generate interpretable results is that the instrumental variable should have a monotonic impact on the endogenous variable. From Figure 2 it is clear that having been required to stay on increased the probability of the individual holding some low level of qualification. However, we also want to compare the cumulative distribution functions of academic attainment for those born before and after the threshold in order to verify that there is no academic attainment level *at or above* which those required to stay on are relatively infrequent.

In order to do this we report the results from a set of estimated models where, for each academic qualification level j, we test whether individuals born after the January-February threshold are more likely to hold that level of qualification or above than are individuals born before the threshold. The results of this analysis are given in Table 2.

Insert table 2 here.

Specifications 1 - 4 report the estimated coefficient on z_i, defined as in (6), in a set of linear probability regression models. Each regression also includes a full

[22] A detailed analysis of the impact of the school-leaving rule for actual school-leaving behavior is presented in Del Bono and Galindo-Rueda (2007). Some of their main findings on this are summarized below.

set of academic cohort dummies, survey year dummies and ethnicity dummies, as well as age measured in months in linear, square and cubic form. Specification 1 uses all individuals born between September and June in each academic cohort. Moreover in this specification, all individuals included in the regression are given the same weight and no trend in the outcome variable with respect to the individual's month of birth within the academic year is considered. In order to place more weight on individuals close to the threshold, Specification 2 uses the same sample, but each observation is given a weight equal to $1/d_i$ where d_i is the distance of the individual's month of birth from the January-February cutoff point. Specification 3 uses only individuals born within two months of the January-February threshold. Specification 4 extends specification 2 by including a linear trend (on either side of the threshold) with respect to the individual's month of birth within the academic year. Specification 5 introduces even more flexibility by estimating the frequency of the particular outcome as a non-linear function of the individual's month of birth within the academic year, with separate functions fitted for individuals born September-January and individuals born February-June. Column 5 then reports the estimated gap (or "discontinuity") between the two functions at the January-February threshold. The non-linear functions are estimated as local linear regressions with a bandwidth of 4.

The results in columns 1-5 confirm that, in the main sample cohorts, the key effect of having been required to stay on was on the "no qualification" versus "some qualification" margin: the effect of being born after the threshold on the probability of holding at least a level 1 academic qualification is economically significant, around three and a half percentage points and relatively stable across alternative specifications. The regressions suggest that those who were required to stay on are also slightly more likely to hold level 2 qualifications (O-level or CSE grade 1) which would also typically have been obtained at age 16.[23] Of key importance for our purposes, the complete absence of any statistically significant *negative* coefficients in Table 2 suggests an unambiguously positive impact of being born after the threshold date on academic attainment.

[23] The finding that the main effect of having been required to stay on was an increase in the probability of holding a low level academic qualification is in line with Del Bono and Galindo-Rueda (2007).

The final two columns perform a robustness check. As noted above, the Easter Leaving Rule was in effect both before the 1973 RoSLA and also after the introduction of the GCSE system. However, for the reasons outlined in Section 3, we would expect the Easter Leaving Rule to have had a smaller impact on the qualifications held be individuals outside the key 1957-1970 cohorts that form our main sample.[24] Hence in column 6 we report the results for equivalent regressions for a pre-RoSLA sample and post-GSCE-introduction sample. For both the pre-RoSLA cohorts and the post-GCSE cohorts there is evidence that being born after the January-February threshold was associated with a higher frequency of holding a level 1 qualification but the estimated effects among these cohorts are indeed noticeably smaller than the corresponding estimated effect for the main sample cohorts. For qualifications above level 1 there is no clear evidence of an impact of having been born after the January-February threshold.[25]

Above we found that the main impact of the requirement to stay on on academic attainment was to move individuals from the no-qualifications group to the level 1 qualifications group. Here we illustrate this in a different way by looking at the age at which the individuals left full-time education. Consider the hypothesis that the *only* effect of the Easter Leaving Rule was to induce some people born after the January-February threshold date to stay on for exactly the extra weeks required. Consider in particular those individuals who wanted to leave school at the earliest possible opportunity. Among these individuals, those born in January would leave at Easter, while those born in February would leave towards the end of May. Since both groups leave education in the *same* calendar year and *after their birthdays*, both groups would have the same age stated in years when leaving education. Hence, under the hypothesis, there should be *no differences* in the distributions of age at leaving full time education between those required to stay on and those not. Table 3 explores this prediction, focusing particularly at the

[24] It is also possible that, in the pre-RoSLA period in particular, the estimated effect may due to students completing the academic year at age 15 in some cases receiving a "school leaving certificate" which in some cases are likely to have been recorded as a CSE academic qualification (Dickson and Smith 2011)

[25] The presented specifications for the pre-RoSLA cohorts and the post-GCSE cohorts use the narrow December to March window and no weighting. Other specifications are available on request.

distinction between leaving school aged 16 or below versus staying on until age 17 or higher. The dummy for leaving at age 16 or below thus serves as an indicator for having left school at the earliest possible opportunity. The results in Table 3 show that for any estimation that place particular weight on individuals born close to the January-February threshold, there is no systematic difference in the likelihood of having left school at age 16 or below and hence no indication that those who were required by the Easter Leaving Rule to stay on responded by staying on even longer than required.

Insert table 3 here.

Insert figure 3 here.

So far we have not considered whether the effect of having been required to stay on was the same in all academic cohorts. To consider this, Figure 3 plots the fraction of individuals in each academic cohort, separated into those born before and those born after the threshold, who hold some academic qualification. For the purpose of this particular figure we have also extended the sample to include the five academic cohorts before our main sample and seven cohorts following. The five academic cohorts before the current main sample were not affected by the 1973 RoSLA and hence faced a minimum school leaving age of 15. This meant that everyone had the option of leaving school before the exams at age 16. As a result, the fraction holding some academic qualification is markedly lower and, specifically, it is less clear whether there are any differences between those born before and after the January-February threshold. For the main sample cohorts, we observe that the rate of holding some academic qualification trends upwards. Moreover, Figure 3 illustrates how the gap in attainment between those required to stay on and those not was particularly large in the early years following the ROSLA. Gradually the gap then reduced as the overall level of attainment increased. This increase in the overall level of attainment occured throughout the 1970s and early 1980s as the demand for low skilled labour decreased. Our main sample stops with the replacement of the CSE and O-level qualifications with the current GCSE (General Certificate of Secondary Education) system: the final students to sit the former O-Level/CSE examinations were those of May-June 1987.

To sum up, the requirement to stay in school for one extra term at the compulsory age of 16 imposed on those born after the January-February threshold had an unambiguously positive impact on their academic attainment, with the main

effect being an increase in the rate of holding a level 1 academic qualification and a corresponding decrease in the rate of holding no academic qualification.

It is worth stressing that those who were induced to drop out of school by the Easter Leaving Rule did so shortly before the high stake exams that would provide them with their first academic qualification. Although up to two-thirds of the students who did leave at Easter came back to take exams, dropping out at Easter nevertheless had a detrimental effect on the probability of attaining any academic qualifications. Using the Youth Cohort Study which contains information on actual school leaving, Del Bono and Galindo-Rueda (2007) show an effect of the Easter Leaving Rule on the overall school leaving behaviour of cohorts born between 1968 and 1972 (men and women pooled) of about 12 percentage points, while the effect on leaving and not coming back to sit exams was 5 percentage points. Moreover, the authors estimate the effect on the probability of obtaining a qualification to have been 2.8 percentage point overall and larger for women than for men.

5.2 Marital Status

We now consider marital status. We start by noting that individuals who invest in education have lower frequencies of being married at lower ages but higher frequencies of being married at higher ages. This is highlighted in Figure 4 which shows the fraction of individuals who are currently married by level of academic attainment relative to individuals who hold no academic qualification.[26] The figure shows how, up until the age of around 28, those who obtain a level 4-5 academic qualification (corresponding to university studies) are markedly less frequently married than those with no qualifications. A similar, but smaller, effect is evident for those who obtain a level 3 academic qualification. After the age of 30, however, those with no academic qualification are the least likely to be married out of all attainment groups, with the gap in marriage frequency being around 10 percentage points relative to every other level of attainment. Hence there is a strong association between academic attainment and the probability of being currently

[26] Specifically, the figure illustrates the coefficients from a set of regressions, one for each age, of the outcome variable "currently married" on the various levels of academic attainment where the regressions also include controls for academic cohort, survey year, and ethnicity.

married. However, it is less clear whether that association reflects a causal effect rather than pure selection. To consider this we examine how the fraction currently married varies with month of birth.

Insert figure 4 here.

Figure 5 consider the difference in the probability of being currently married, at each age, between women born in the months February - April (who were thus required to stay on) and women born in the months November - January (who were thus not required to stay on). The difference centres on zero and the overall difference across all ages (indicated by the hatched line) is actually negative but very small and not statistically significant. Hence there is no clear indication that having been required to stay on is associated with either a higher or a lower probability of being currently married.

Insert figure 5 here.

To consider this in further detail, we use a set of models, similar to those used above for qualifications, to estimate any potential discontinuity in marriage frequency at the January-February threshold. The results from this analysis is given in the upper panel of Table 4. Specification 1 uses all individuals born in the months September through to June and regresses a dummy for being currently married on a dummy for being born February through June, along with a set of controls. Specification 2 uses the same sample, but weights each observation by its inverse distance from the January-February threshold. Specification 3 reports the result from using a narrower "window". Specification 4 adds a linear trend (on either side of the threshold) in month of birth within the academic year. Specification 5 estimates the marriage frequency as a separate non-linear function of month of birth on either side of the threshold and reports the estimated gap between the two functions at the threshold. For women aged 25 or above, the regressions show no systematic effect of having been born after the January-February threshold on the probability of being currently married. The estimates are numerically small, not statistically significant, and centered on zero. For younger women the estimates are consistently negative. However, due to the smaller sample size the estimates are less precise. These reduced form models thus suggest that having been required to stay on due to having been born after the January-February threshold may have led some women to marry later. However, beyond the age of 25 there is no clear

suggestion that women who were required to stay on were either more or less likely to be currently married.

Insert table 4 here.

The lower panel of the same table reports the corresponding IV estimates of the effect of holding an academic qualification (induced by having been required to stay on) on the probability of being currently married.[27] These estimates of the causal effect of holding a qualification thus suggest a potential delay in marriage of the form suggested by Fort (2007) and Breierova and Duflo (2004). However, there is no clear evidence of any effect on current marital status beyond the age of 25.[28]

5.3 Spousal Characteristics

So far we have found that those who, due to being born later in the academic year, were required to stay on for an extra term more frequently obtained some academic qualification. In contrast, we can not find any difference in the probability of being currently married between those required to stay on and those not. From this latter observation, we conclude that holding an academic qualification had no impact on the marriage probability for the group of individuals whose educational attainment strictly depended on whether they were required to stay on or not.

[27] Specifications 1 - 4 are basic 2SLS IV models where the outcome variable is a dummy for the individual being currently married and the endogenous variable – the dummy variable indicating whether the individual holds an academic qualification – is instrumented for using z_i, the dummy indicator for whether the individual was, due to her month of birth, required to stay on. Specification 5 is the Wald IV estimator formed by taking the ratio of the estimated gap in the outcome variables at the threshold point to the estimated gap in the endogenous variable. See Imbens and Lemieux (2008) and Lee and Lemieux (2010).

[28] The effect of holding an academic qualification on "ever being married" could be different from the effect of "being currently married" if there was an effect on divorce probability. However, we find no evidence to suggest an impact of holding an academic qualification on the probability of being divorced. In the age-group 25+ the reduced form estimates of the effect of being born after the January-February threshold on the probability of being currently divorced range from -0.008 to 0.002 and are never statistically significant. The corresponding IV estimates are centered on zero and never statistically significant. (Details are available on request.) Nevertheless, it should be noted that our results apply to "current" marriages.

We now proceed to study the characteristics of the spouses of the married women in the sample. We consider two partner characteristics: (i) whether the husband holds any academic qualification, and (ii) whether or not the husband is economically active. In doing so we rely on the fact that our finding of no difference in marriage frequency between those required to stay on and those not is consistent with the identifying assumption that the marriage probabilities of all "compliers" do not depend on whether they hold any academic qualifications or not.

Insert table 5 here.

As expected there is a strong positive association between a woman's academic qualification level and that of her spouse. Table 5 shows the OLS estimated effect of holding an academic qualification at various levels on the probability of the husband holding *some* academic qualification. Women with academic qualifications are much more likely to be married to husbands who also have some academic qualification.[29] Indeed, while the probability of being married to a partner with some academic qualification increases with the individual's own qualification level, the largest difference obtains between women with no qualification and a level 1 qualification. Table 5 also shows the OLS estimated effect of a woman holding various levels of academic qualifications on the probability of her husband being economically active. Here the main difference is precisely between women with no academic qualification and *some* academic qualification: conditional on holding some academic qualification, the husband's economic activity rate varies little with the particular qualification level held by the woman.

Consider then how the husband's characteristics vary with the woman's month of birth. To consider this, we regress the dummy for the husband holding a qualification and the dummy for the husband being economically active, respectively, on a set of month-of-birth dummies for the woman (leaving out February as reference group) and a full set of academic cohort dummies, survey year dummies, ethnicity dummies and age in months in linear, squared and cubic form. The left panel of Figure 6 shows how the probability of a woman being married to a husband who holds some academic qualification differs by her own month of birth relative to

[29] More generally it is also true that there is marital sorting by qualification level. For instance, for any academic qualification level j (including no qualification) a woman with qualification level j is more likely to be married to a qualification level j male than any other women, and vice versa.

the omitted February reference group. While somewhat noisy, the figure suggests that women born in the first five months of the academic year are less likely to be married to husbands with some academic qualification. The right panel of Figure 6 shows the corresponding results for the husbands' economic activity rates. This figure shows a clear tendency for women born in the first five months of the academic year to be more frequently married to economically inactive husbands.

Insert figure 6 here.

In order to explore in more detail any potential discontinuities in the husband's economic properties at the January-February threshold point for the wife's month of birth, we adopt the same approach used for both academic qualifications and marriage rates above. The results from this analysis are provided in Table 6 and the five specifications follow the same pattern as in the previous sections. The top row of Table 6 suggests that women in the sample born February or later in the academic year are little over a percentage point more likely to be married to husbands who hold some academic qualification compared to women born earlier in the academic year. The first three specifications are more precise but neglect the possibility of any trends with respect to the woman's month of birth within the academic year. The last two specifications, while obviously less precise, suggest that the conclusion is robust to the inclusion of such a trend. Similarly, the first row of the second panel in Table 6 suggests that women in the sample born February or later in the academic year are little over half a percentage point more likely to be married to husbands who are economically active.

Insert table 6 here.

The second row of each panel provides corresponding instrumental variable estimates of the causal effects of a married woman's holding of an academic qualification (induced by having been required to stay on) on the husband's economic characteristics. For both husband's characteristics, the estimates are always positive and statistically significant. Moreover, the estimates are highly robust to model specification. Note also that the estimated causal effects are very similar in magnitude to the OLS-estimated effects of holding a level 1 academic qualification reported in Table 5.[30]

[30] The effect of the wife holding an academic qualification on the husband's economic activity rate persists, with nearly identical point estimates, also if we control for the husband himself holding some academic qualification. Moreover, this is true whether or not we instrument for

The evidence thus suggests that the requirement for some women to stay on for an extra term at the compulsory school leaving age not only significantly increased their rate of holding some academic qualification, but also increased the rate at which they are (years later) married to husbands who hold some academic qualification and who are economically active. Indeed, the estimates of the causal effect of a woman holding an academic qualification on the properties of her husband are very similar to the OLS estimates. This is in itself somewhat surprising in that it suggests that most of the positive association we observe between women's holding of academic qualifications and the academic qualifications and economic activity rates of their husbands operate through causal channels.

5.4 Robustness Analysis

The main findings so far have been that women born in February or later in the academic year (i) more frequently hold some academic qualification, and (ii) are more frequently married to husbands who hold some academic qualification and who are economically active. From this it was argued that the holding of an academic qualification affected the properties of the women's subsequent husbands.

Table 2 and Figure 3 showed that the gap in the qualification rate between those born after the January-February threshold was the strongest among the key cohorts that form our main sample. Among the pre-RoSLA and the post-GCSE cohorts the Easter Leaving Rule was also in place, but its effect on qualification holding was less pronounced (see columns 6-7 in Table 2). A natural robustness test is then to check whether the impact of month of birth relative to the January-February threshold was particularly strong also in terms of marital outcomes for the main sample cohorts relative to the pre-RoSLA and the post-GCSE cohorts. To explore this we use an extended sample of married women which includes all academic cohorts from 1952 through to 1975, fitting models of the same type as in previous sections. The results are reported in Table 7.

Insert table 7 here.

For the husband's holding of an academic qualification there is no evidence of any systematic impact of having been born after the threshold in either the

the husband's holding of an academic qualification using whether or not he, due to his month of birth, would have been required to stay on.

pre-RoSLA and the post-GCSE cohorts. For the husband's economic activity rate, the point estimates for the pre-RoSLA and the post-GCSE cohorts of the effect of having been born after the threshold are mostly positive, but never larger than the corresponding point estimates for the main sample period.

A potential further concern could be that partners meet in school and that those born later in academic the year are more likely to match with partners also born later in the academic year. In that case there could be a "mechanical" effect creating a positive correlation between partners' academic qualifications. However it was noted in Section 4 that, in the sample of married couples, there is no observed correlation between the wife being born February or later in the academic year and the husband being born February or later. Moreover, only 13 percent of married couples are from the same academic cohort, and re-estimating the regressions in Table 6 using only partners who are from *different* academic cohorts has a negligible effect on the estimates.[31]

6 Discussion

In this paper we have exploited a particular historical feature of the schooling laws in England and Wales which allowed those individuals born in the first five months of the academic year to leave education at Easter of the year in which they reached the minimum school leaving age, one term ahead of their class mates born in the remaining seven months of the academic year. For the 14 academic cohorts that we focus on, the interaction of this feature with the exam system implied a discontinuity in the rate of holding some academic qualification with respect to month of birth, with a woman born in February or later being more than 3.5 percentage points more likely to hold some academic qualification than a woman born earlier in the academic year.

We have briefly discussed the channels through which one's educational attainment can have a causal effect on her marital outcome in Section 2. In particular we have outlined a model with frictions in which education has two main effects. First, it increases the individual's future earnings and hence attractiveness as partner. Second, education increases the individual's chance of matching with a skilled po-

[31] Results are available on request from the authors.

tential partner. Relating to the first channel, Del Bono and Galindo-Rueda (2007) exploit the same Easter Leaving Rule with the Labour Force Survey from 1993 to 2003 and find a significant causal effect of qualifications on participation, employment and earnings, with the effect being at least as large for women as for men.[32]

While there is a strong positive association between holding an academic qualification (at any level) and being currently married for women beyond their mid-20s, there is no corresponding clear indication of any difference in the rate of being married between those women who were required to stay on for the extra term and those who were not. Hence our findings suggest that holding an academic qualification had no long-run effect on the probability of being married for the population that we study. The absence of an effect on the probability of being married, however, does not imply that holding an academic qualification was necessarily marriage-irrelevant. Indeed, those who, due to their month of birth, were required to stay on for the extra term were found to be married to husbands who more frequently hold some academic qualification and who more frequently are economically active.

While we have emphasized qualifications, acknowledging that the Easter Leaving Rule implies a difference in the length of schooling, our results could be given a broader interpretation: the requirement to stay in school had an effect on marriage outcomes either through qualification or length of schooling. Using the Labour Force Survey from 1993 to 2010, Dickson and Smith (2011) explicitly address the question whether returns to schooling reflect the extra length of schooling or the increase in qualifications, by exploiting the Easter Leaving Rule and the raising of school leave age (RoSLA) reform. They argue the Easter Leaving Rule, while affecting the probability of obtaining qualifications had a negligible impact of length of schooling (22 to 44 days) relative to the the RoSLA which imposed an extra year of schooling. By comparing the IV estimates of the effect of qualifications on employment using the Easter Leaving Rule as an instrument to the corresponding estimates using the RoSLA as an instrument, they conclude that much of the returns to RoSLA is driven by qualifications. Moreover, if the effect of the Easter

[32] For example, their preferred IV specification suggests that having academic qualification increases the probability of labour force participation by 32 percentage points ($p<0.01$)

Leaving Rule was mainly through the extra time spent in schooling, then the effect should have been present also in the periods before and after our main sample period when it was also in place. However, our results suggest that the main period is the only period for which having been required to stay on is robustly associated with better marital outcomes.

Acknowledgements The authors would like to thank Stacey Chen, Arnaud Chevalier, John Knowles, Costas Meghir, Jonathan Wadsworth, and seminar participants at Kent, Sheffield, CESifo, the Max-Planck Institute, WPEG, ESPE, EEA, CUHK, RES and the GEARY Institute for helpful comments, and Vikesh Amin for excellent research assistance. Financial support from the Nuffield Foundation (grant nr. CPF/37724) is gratefully acknowledged.

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Table 1: Descriptive Statistics for the Labour Force Survey Sample

Variable	**All**	**Married**	**Not Married**	**Difference**
Age in Months	368.5	391.2	343.6	47.5
				(0.344)**
Ethnicity: White	0.968	0.980	0.955	0.025
				(0.001)**
Ethnicity: Asian	0.009	0.010	0.008	0.002
				(0.000)**
Ethnicity: Black	0.016	0.006	0.027	-0.020
				(0.001)**
Ethnicity: Other	0.007	0.004	0.010	-0.006
				(0.000)**
Ec. Active	0.687	0.682	0.692	-0.009
				(0.002)**
No Ac. Qual	0.209	0.195	0.225	-0.030
				(0.002)**
Level 1 Ac. Qual.	0.160	0.166	0.153	0.012
				(0.002)**
Level 2 Ac. Qual.	0.399	0.413	0.383	0.030
				(0.002)**
Level 3 Ac. Qual.	0.113	0.104	0.123	-0.019
				(0.001)**
Level 4 Ac. Qual.	0.099	0.100	0.097	0.003
				(0.001)**
Level 5 Ac. Qual.	0.021	0.023	0.019	0.004
				(0.001)**
Nr. Obs.	226,965	118,894	108,071	

Notes: The sample includes women observed in the UK Labour Force Survey 1984-2006, living in England or Wales, born in the UK between September 1957 and August 1971, and aged 18 or above at the time of the survey. The final column reports the difference in mean between married and not married with standard error on the estimated difference in parenthesis. Significance levels: $**$: 1% $*$: 5%

Table 2: Effect of Having Been "Required to Stay on" on the Probability of Holding Academic Qualification Level j or Above.

Qual. Lev.	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Level 1+	0.037	0.037	0.036	0.041	0.036	0.019	0.013
	(0.002)**	(0.003)**	(0.003)**	(0.010)**	(0.005)**	(0.006)**	(0.006)*
Level 2+	0.005	0.009	0.011	0.020	0.015	0.005	0.008
	(0.003)	(0.003)**	(0.004)**	(0.012)	(0.004)**	(0.006)	(0.006)
Level 3+	-0.002	-0.001	0.000	-0.007	-0.002	0.005	0.005
	(0.002)	(0.003)	(0.003)	(0.010)	(0.003)	(0.005)	(0.006)
Level 4+	0.001	0.004	0.004	0.006	0.006	0.004	0.007
	(0.002)	(0.002)	(0.002)	(0.008)	(0.004)	(0.004)	(0.005)
Level 5	-0.001	-0.001	-0.001	-0.004	-0.002	0.002	0.003
	(0.001)	(0.001)	(0.001)	(0.004)	(0.002)	(0.002)	(0.002)
Nr Obs.	189,637	189,637	76,016	189,637	189,637	25,715	24,121
Ac. Coh.	1957-70	1957-70	1957-70	1957-70	1957-70	1952-56	1971-77
Window	Sep-Jun	Sep-Jun	Dec-Mar	Sep-Jun	Sep-Jun	Dec-Mar	Dec-Mar
Weighting	No	Inv. Dist.	No	Inv. Dist.	No	No	No.
Trend	No	No	No	Linear	Loc. linear	No	No
Covariates	Yes	Yes	Yes	Yes	No	Yes	Yes

Notes: The table reports the estimated discontinuity in the frequency of holding an academic qualification of level j or above with respect to the individual's month of birth within the academic year at the January-February threshold point. Specifications 1 - 4 report the coefficient on a dummy for being born in February or later in the academic year in a set of linear regressions. These regressions include a full set of academic cohort dummies, survey year dummies and ethnicity dummies, as well as age measured in months in linear, square and cubic form. Specifications 2 and 4 use "inverse distance weighting" whereby each observation is given a weight equal to $1/d$ where d is the distance in months from the threshold point. Specifications 1 - 3 do not model any trend in the outcome variable with respect to the individual's month of birth within the academic year. Specification 4 includes a linear trend on either side of the threshold point. Specification 5 estimates the frequency of holding an academic qualification of level j or above as a separate non-parametric function of month of birth on either side of the January-February threshold. The non-parametric functions are local linear regressions estimated with a triangle kernel function and bandwidth equal to 4. The reported discontinuity is the gap between the two non-parametric functions at the January-February threshold and the standard error is bootstrapped (1,000 replications). Specifications 6 and 7 are the same as specification 3, but uses a sample of pre-RoSLA individuals and post-GCSE individual respectively as indicated by "Academic Cohort". Significance levels: $**$: 1% $*$: 5%

Table 3: Effect of Having Been "Required to Stay on" on the Probability of Leaving Full-Time Education at Age 16 or below.

Age Left FTE	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Age 16 or below	-0.0059	0.0006	0.0053	-0.0014	-0.0042
	(0.0029)*	(0.0037)	(0.0038)	(0.0113)	(0.0076)
Nr Obs	185,411	185,411	74,229	185,411	185,411
Window	Sep-Jun	Sep-Jun	Dec-Mar	Sep-Jun	Sep-Jun
Weighting	No	Inv. Dist.	No	Inv. Dist.	No
Trend	No	No	No	Linear	Loc. Linear
Covariates	Yes	Yes	Yes	Yes	No

Notes: The table reports the estimated discontinuity in the frequency of leaving full time education at various ages with respect to the individual's month of birth within the academic year at the January-February threshold point. See notes to Table 2 for a description of the different specifications. Significance levels: ∗∗ : 1% ∗ : 5%.

Table 4: Estimates of the Effect of Holding an Academic Qualification on the Probability of Being Currently Married among Women.

	(1)	(2)	(3)	(4)	(5)
Effect of "Required to Stay on" on the Prob. of Being Married					
Women Aged 25+	-0.003	0.000	-0.000	0.001	0.001
	(0.003)	(0.004)	(0.004)	(0.012)	(0.008)
Women Aged 18-24	-0.003	-0.004	-0.004	-0.003	-0.011
	(0.006)	(0.007)	(0.007)	(0.019)	(0.007)
Effect of Holding an Ac. Qual. on the Prob. of Being Married					
Women Aged 25+	-0.066	0.007	-0.005	0.012	0.009
	(0.080)	(0.101)	(0.106)	(0.101)	(0.180)
Women Aged 18-24	-0.114	-0.147	-0.177	-0.152	-0.364
	(0.238)	(0.277)	(0.333)	(0.274)	(0.278)
Nr Obs Aged 25+	143,749	143,749	57,916	143,749	143,749
Nr Obs Aged 18-24	45,888	45,888	18,100	45,888	45,888
Window:	Sep-Jun	Sep-Jun	Dec-Mar	Sep-Jun	Sep-Jun
Weighting:	No	Inv. Dist.	No	Inv. Dist.	No
Trend:	No	No	No	Linear	Loc. Linear
Covariates:	Yes	Yes	Yes	Yes	No

Notes: The top panel of the table reports the estimated discontinuity in the probability of being currently married with respect to the individual's month of birth within the academic year at the January-February threshold point. See notes to Table 2 for a description of the different specifications. The lower panel reports corresponding instrumental variable estimates of the effect of holding an academic qualification on the probability of being currently married. Specifications 1-4 report the coefficient on holding an academic qualification from 2SLS regressions where having been required to stay on is used as instrument for holding an academic qualification. Specification 5 in the bottom panel reports the IV Wald estimator formed by taking the ratio of the estimated effect of having been required to stay on on the probability of being currently married and the estimated effect of having been required to stay on on the probability of holding an academic qualification. Significance levels: $**$: 1% $*$: 5%

Table 5: Effect of Holding Academic Qualifications on the Probability of Husband Holding Some Academic Qualification and on the Probability of the Husband being Economically Active, Estimated by OLS

	Dependent Variable	
Qual. Lev.	**Ac. Qual.**	**Ec. Activity**
Level 1	0.273	0.116
	(0.004)**	(0.003)**
Level 2	0.343	0.147
	(0.004)**	(0.002)**
Level 3	0.463	0.162
	(0.005)**	(0.003)**
Level 4	0.545	0.160
	(0.005)**	(0.003)**
Level 5	0.545	0.153
	(0.009)**	(0.006)**
Nr Obs	114,519	117,801

Notes: The table reports the estimated coefficients on a set of dummies for the woman holding academic qualification level 1-5 in two regressions where the outcome variables are a dummy for the husband holding some academic qualification and a dummy for the husband being economically active, respectively. The omitted reference group is "no academic qualification". All regressions also include a full set of academic cohort dummies, survey year dummies and ethnicity dummies, as well as age measured in months in linear, square and cubic form. Significance levels: ** : 1% * : 5%.

Table 6: Estimates of the Effect of Holding an Academic Qualification on Husband's Economic Characteristics.

	(1)	(2)	(3)	(4)	(5)
Effect of "Required to Stay on" on Husband's Prob. of holding Ac. Qual.					
	0.010	0.012	0.012	0.016	0.013
	(0.004)**	(0.005)*	(0.005)*	(0.015)	(0.007)*
Effect of Woman's Ac. Qual. on Husband's Prob. of holding Ac. Qual.					
	0.218	0.259	0.264	0.263	0.297
	(0.079)**	(0.098)**	(0.105)*	(0.095)**	(0.150)*
Nr Obs	96,267	96,267	38,360	96,267	96,267
Effect of "Required to Stay on" on Husband's Prob. of Being Ec. Active					
	0.006	0.007	0.006	0.008	0.008
	(0.002)*	(0.003)*	(0.003)*	(0.010)	(0.004)*
Effect of Woman's Ac. Qual. on Husband's Prob. of Being Ec. Active					
	0.136	0.152	0.142	0.150	0.186
	(0.052)**	(0.066)*	(0.071)*	(0.065)*	(0.092)*
Nr Obs	99,015	99,015	39,478	99,015	99,015
Window:	Sep-Jun	Sep-Jun	Dec-Mar	Sep-Jun	Sep-Jun
Weighting:	No	Inv. Dist.	No	Inv. Dist.	No
Trend:	No	No	No	Linear	Loc. Linear
Covariates:	Yes	Yes	Yes	Yes	No

Notes: The table reports the estimated discontinuity in the frequency of the husband holding an academic qualification and being economically active with respect to the wife's month of birth within the academic year at the January-February threshold point. It also reports the estimated effect of the wife holding an academic qualification on the husband's economic characteristics using the wife's month of birth relative to the January-February threshold as instrument. See Tables 2 and 4 for a description of the different specifications. Significance levels: ∗∗ : 1% ∗ : 5%

Table 7: Effect of Having Been "Required to Stay on" on Husband's Characteristics by Period

	(1)	(2)	(3)	(4)	(5)
Husband holds Academic Qualification					
Period: 1952-56	0.001	0.001	0.007	-0.013	-0.005
	(0.005)	(0.007)	(0.007)	(0.023)	(0.014)
Period: 1957-70	0.012	0.012	0.012	0.014	0.013
	(0.004)**	(0.005)**	(0.005)**	(0.015)	(0.007)*
Period: 1971-75	0.004	-0.006	-0.005	-0.054	-0.014
	(0.011)	(0.013)	(0.017)	(0.047)	(0.029)
Husband is Economically Active					
Period: 1952-56	0.005	0.004	0.006	0.003	0.007
	(0.003)	(0.004)	(0.005)	(0.014)	(0.007)
Period: 1957-70	0.006	0.006	0.006	0.009	0.008
	(0.002)**	(0.003)**	(0.003)*	(0.010)	(0.004)*
Period: 1971-75	0.005	0.002	0.005	-0.027	-0.000
	(0.007)	(0.009)	(0.011)	(0.033)	(0.011)
Nr Obs 1952-57	57,265	57,265	19,182	57,265	57,265
Nr Obs 1970-70	118,894	118,894	59,912	118,894	118,894
Nr Obs 1971-1975	9,619	9,619	59,912	3,160	9,619
Window:	Sep-Jun	Sep-Jun	Dec-Mar	Sep-Jun	Sep-Jun
Weighting:	No	Inv. Dist.	No	Inv. Dist.	No
Trend:	No	No	No	Linear	Loc. Linear
Covariates:	Yes	Yes	Yes	Yes	No

Notes: The table reports the estimated discontinuities in husband's characteristic at the January-February threshold by subperiod in the wife's month of birth with respect to the January-February threshold. See Table 2 for a description of the different specifications. Significance levels: ∗∗ : 1% ∗ : 5%.

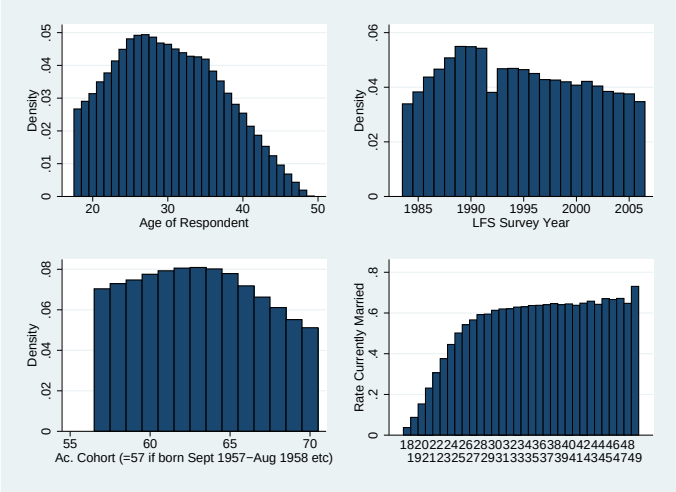

Fig. 1: The distribution age, survey year and academic cohort, and frequency of being currently married by age.

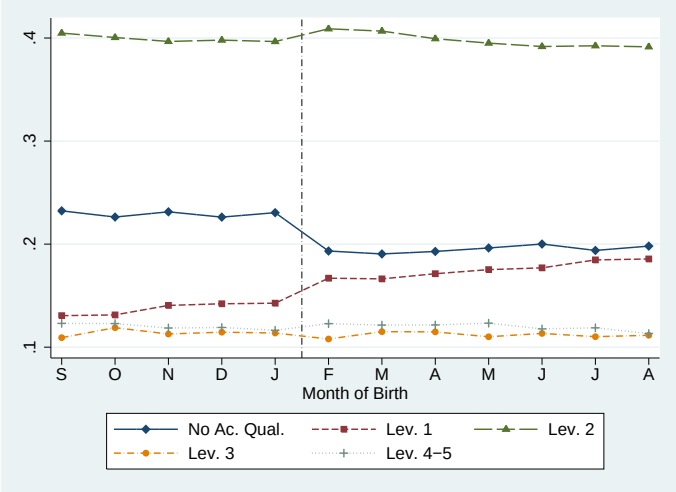

Fig. 2: The distribution of highest academic qualification by month of birth.

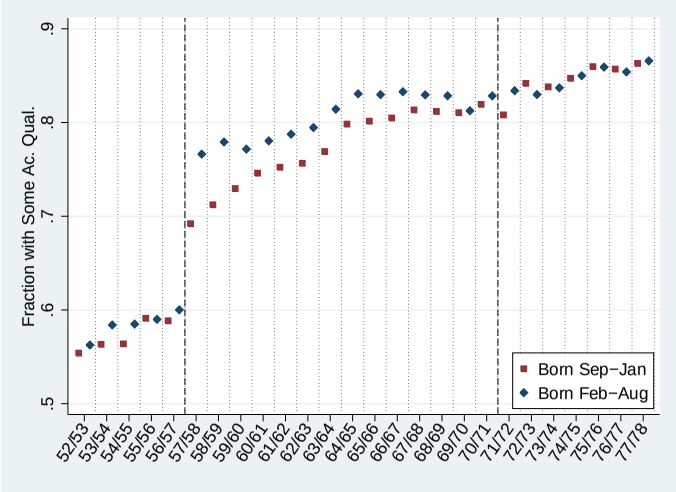

Fig. 3: Fraction holding some academic qualification by academic cohort.

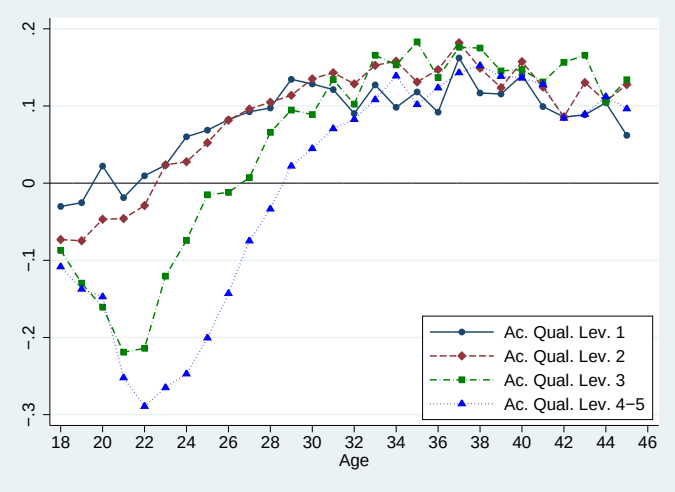

Fig. 4: Fraction currently married relative to individuals with no academic qualifications, by age.

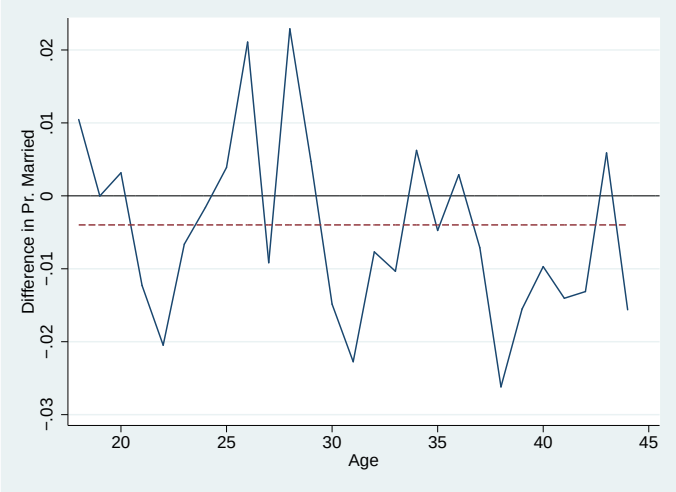

Fig. 5: Fraction currently married by requirement to stay on.

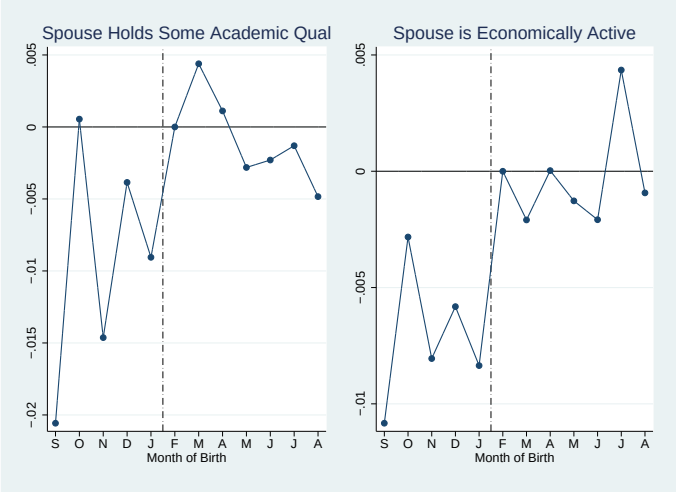

Fig. 6: Husbands' economic characteristics by woman's month of birth relative to February.

Appendix

In this appendix we explore whether individuals who, due to their month of birth, were required to stay on had parents with different economic characteristics than those individual who were not required to stay. In order to do this, we assemble a sample of youth from the LFS for whom we can observe also their parents as they are in the same household. The sample consists of individuals born towards the later stages of our main sample period, specifically between September 1967 and August 1971, who are observed in 1985 through to 1987, and for whom we have information about parents' characteristics. For this sample we estimate regression models of the same type used in the main body of the paper in order to explore whether the parents of those individuals born after the January-February threshold had different characteristics to the parents of those individuals born before the January-February threshold. The outcome variables used directly correspond to those used in the analysis of partner characteristics in the Table 6 of the paper, that is, whether the parent holds an academic qualification and whether the parent is economically active. The results are provided in Table 8 and reveal no systematic association between the individual's requirement to stay on and parental characteristics.

Insert table 8 here.

Table 8: Estimates of Discontinuity in Parental Characteristics at January-February Threshold

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Father holds Ac. Qual.	0.018	0.006	0.003	0.001	-0.010
	(0.013)	(0.015)	(0.014)	(0.018)	(0.020)
Mother holds Ac. Qual.	0.032	0.016	0.022	0.004	-0.014
	(0.012)**	(0.014)	(0.014)	(0.018)	(0.024)
Father is Ec. Active	-0.005	-0.018	-0.009	-0.030	-0.020
	(0.010)	(0.012)	(0.011)	(0.014)	(0.021)
Mother is Ec. Active	0.015	0.013	0.012	0.011	0.010
	(0.012)	(0.014)	(0.014)	(0.018)	(0.025)
Nr Obs	15,327	15,327	5,741	15,327	15,327
Window	Sep-Jun	Sep-Jun	Dec-Mar	Sep-Jun	Sep-Jun
Weighting	No	Inv. Dist.	No	Inv. Dist.	No
Trend	No	No	No	Linear	Loc. linear
Covariates	Yes	Yes	Yes	Yes	No

Notes: The table reports the estimated discontinuity in each respective parental characteristic with respect to the individual's own month of birth within the academic year at the January-February threshold point. See Table 2 for a description of the different specifications. Significance levels: ∗∗ : 1% ∗ : 5%