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The Effect of Education on Marital
Outcomes in the UK**

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What a Difference a Term Makes: The Effect of Education on Marital Outcomes in the UK *

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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 a women's probability of being married, but increases the probability of her husband holding some academic qualification and being economically active.

Keywords: Education, Marriage, Assortative Mating

JEL Classification: J12, J24

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I 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 are 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 factors such as 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. E.g. Fernández 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 particular historical feature of the educational

[1]Moreover, using household surveys from 34 countries, Fernández 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

system in England and Wales. 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 than those born before the threshold date.[3] Our identification strategy will hence involve exploring how marital outcomes vary with month of birth, and to relate those differences to the observed differences in academic attainment. 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 find that:

- Women born after the January-February threshold date (who were required to stay on for one more term) are close to four percentage points more likely to hold some academic qualification than those born before the threshold.

- 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.

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 wifes. The second reason relates to the outcome variable studied. For 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 favourable outcome for the wife. While a corresponding analysis could be done for men, it is less clear that the wife working would indicate a favourable 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.

- Holding an academic qualification does affect the properties of a woman's spouse: women born after the threshold date are more likely to be married to men who hold some academic qualification and are economically active.

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. E.g. 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 timing of marriage must have been short: 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 probability of 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 varied across regions. Using data from the 1995 Indonesian Intercensal Survey and focusing on women, the authors found that increased education leads to a higher age at first marriage, but has no impact on the probability

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

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, Oreopoulous 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) finds 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 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 III details the institutional context. After describing the data in Section IV, we present our main results in Section V. Section VI concludes.

II Conceptual Issues: Equilibrium Education and Marriage

What are the channels through which an individual's choice of education 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. A recent exception is Chiappori et al. (2009a).[5] They model a frictionless marriage market in the style of Becker (1973, 1991). Hence, in terms of the three channels outlined above, their model focuses on the last channel. Here we sketch a model with frictions which allows for channel (ii). The essentials of the model are as follows. Each individual is associated with a set of background characteristics (ability, parental income, etc) which play two roles: (i) they affect the individual's cost of education, and (ii) they may directly affect the individual's (equilibrium) probability of matching with a skilled potential partner. The individual's education choice may also impact on her chance of matching with a skilled potential partner. A match may be turned down on economic grounds due to asymmetry in earnings. The model thus generally features assortative mating, which may or may not have a causal interpretation. The purpose of the model is primarily to aid the discussion of empirical identification. To that end, the model includes an instrumental variable, uncorrelated with the individual's other characteristics, which only affects the cost of education.

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 strictly depend on the instrument), it

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

is also possible to identify an average effect of education on partner's skill level.

A Simple Model of Education and Marriage

Consider an economy with continuums (of equal size) of women and men.[6] For notational simplicity ignore gender differences. 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 also a binary instrumental variable $z_i \in \{0,1\}$. A key assumption is that z_i is independent of the individual's other characteristics.

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

Individual i decides whether or not to invest in education, $x_i \in \{0,1\}$. The individual's investment cost, denoted $c(\boldsymbol{\alpha}_i, z_i)$, is, for simplicity, modeled as a direct utility cost.

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: the crucial aspect of Assumption 3 is that z_i *does not directly* affect the equilibrium probability of matching with a skilled partner.

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 which is i.i.d. across potential couples, and 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.

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

The Marriage Decision

Individual i will agree to marry $-i$ if and only if $(1-\gamma)\, y_i + \gamma y_{-i} + \theta_i \geq y_i$.[7] However, for the match to lead to marriage, $-i$ must also agree to marry i. Trivially, if they have the same skill level, they marry if and only if $\theta_i \geq 0$. If one is skilled and one 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, with $\pi_s \geq \pi_m$.

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.[8] This can be written as

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

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 $\theta_s > \theta_m$. The first term in (1) is the earnings increase. The second term is non-negative if investing in education increases the probability of a skill-symmetric match. The third component is non-positive since the individual may suffer a consumption sharing loss when skilled, whereas she may gain from consumption sharing when unskilled.

Individual i invests if and only if the benefit exceeds the cost; formally, $x(\boldsymbol{\alpha}_i, z_i) \equiv I_{B(\boldsymbol{\alpha}_i) \geq c(\boldsymbol{\alpha}_i, z_i)}$. 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

$$A^* \equiv \{\boldsymbol{\alpha}_i \in A | x(\boldsymbol{\alpha}_i, 1) = 1 \text{ and } x(\boldsymbol{\alpha}_i, 0) = 0\}, \quad (2)$$

[7] The current model's focus on consumption and income sharing as the driver behind spousal preferences is merely for illustrative purposes. The key components of the analysis is that individuals are making education investment decisions which are affected by the instrument, and that the marriage probability depends on the skill composition of the match. A corresponding analysis could e.g. be constructed for cases where individuals have direct preferences for the skill level of the partner, either in absolute terms or relative to the own skill level.

[8] Formally the benefit $B(\boldsymbol{\alpha}_i)$ is defined as the difference $E[c_i + \theta m_i | \boldsymbol{\alpha}_i, x_i = 1] - E[c_i + \theta m_i | \boldsymbol{\alpha}_i, x_i = 0]$.

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".

Equilibrium

While it is not necessary for the current purposes to characterize the details of the equilibrium, we will outline what doing so would entail. To complete the model, the matching technology would need to be specified. As an example, suppose that $\boldsymbol{\alpha}_i$ and x_i are summarized in an unidimension index $\mathcal{I}_i = \boldsymbol{\beta}\alpha_i + \delta x_i + \varepsilon_i$ for some vector $\boldsymbol{\beta}$, scalar δ, and "error" ε_i (unknown to the individual when deciding on education) and assume that individuals match by rank of $\mathcal{I}_i$.[9] If $\delta = 0$, this simply leads to a correlation between α_i and α_{-i}. When $\delta > 0$, individual i can increase $\mathcal{I}_i$ by investing in skills and hence affect the probability distribution over α_{-i} and x_{-i}. Turning to the description of an equilibrium, the probability function $p(\boldsymbol{\alpha}, x)$ depends on the investment function $x(\boldsymbol{\alpha}, z)$ via the matching technology; conversely, individual behaviour $x(\boldsymbol{\alpha}, z)$ depends on $p(\boldsymbol{\alpha}, x)$. An equilibrium (corresponding to a fixed point) consists of two mutually consistent functions.[10]

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

$$\Delta\mu(\boldsymbol{\alpha}_i) \equiv \mu(\boldsymbol{\alpha}_i, 1) - \mu(\boldsymbol{\alpha}_i, 0) = \{p(\boldsymbol{\alpha}_i, 1) - [1 - p(\boldsymbol{\alpha}_i, 0)]\}(\pi_s - \pi_m), \tag{3}$$

which may be either positive or negative and is generally heterogenous in the population. There are two cases in which education has *no impact* on the marriage probability of individual i: (i) when there is no income sharing ($\gamma \to 0$) and all marriages are entirely "love-based", and (ii) when the probability of a "skill-symmetric match" is the same for both skill levels: $p(\boldsymbol{\alpha}_i, 1) =$

[9] Two recent contributions by Chiappori et al. (2009b) and Galichon and Salanie (2010) consider models of the marriage market where individuals are heterogenous in multiple dimensions.

[10] In principle, an equilibrium may be in mixed strategies. However, we assume the existence of a pure strategy equilibrium.

$1-p(\boldsymbol{\alpha}_i, 0)$. Note that while the first case implies that the effect of education on the marriage probability is zero for everyone, the latter condition may hold for some types but not for others.

Consider next the effect of education on the partner's skill level. Since we will only observe the partners of married individuals, we interpret the effect of education on partner's skill level for individual i as the difference in the probability of her partner being skilled, *conditional* on her being married.[11] This is given by

$$\Delta x_{-i}(\boldsymbol{\alpha}_i) \equiv \frac{p(\boldsymbol{\alpha}_i, 1)\,\pi_s}{\mu(\boldsymbol{\alpha}_i, 1)} - \frac{p(\boldsymbol{\alpha}_i, 0)\,\pi_m}{\mu(\boldsymbol{\alpha}_i, 0)}. \tag{4}$$

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.

Identification

Assume now that we have a random sample of women. For each woman we observe her marital status m_i, skill level x_i, instrument z_i, and, if married, the skill level of her partner, x_{-i}. We assume here that $\boldsymbol{\alpha}_i$ is completely unobserved. Given that z_i only influences m_i via x_i an IV approach can be expected to identify the effect of education 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 $E[\Delta\mu(\boldsymbol{\alpha}_i)|\boldsymbol{\alpha}_i \in A^*] \cdot \sum_{\boldsymbol{\alpha}_i \in A^*} f(\boldsymbol{\alpha}_i)$. By a similar logic, the difference of $E[x_i|z_i]$ for $z_i = 1$ and $z_i = 0$ measures the fraction of compliers in the

[11] 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 (4) follows from applying Bayes' rule.

population. Hence, following the logic of Imbens and Angrist (1994), we have that

$$\frac{E\left[m_i|z_i=1\right]-E\left[m_i|z_i=0\right]}{E\left[x_i|z_i=1\right]-E\left[x_i|z_i=0\right]}=E\left[\Delta\mu\left(\boldsymbol{\alpha}_i\right)|\boldsymbol{\alpha}_i\in A^*\right]. \tag{5}$$

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.

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

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

Assumption 4 implies that $E\left[m_i|z_i\right]$ is independent of z_i, which is of course what is tested by the IV/Wald estimator of the effect of education on marriage probability. Under Assumption 4 it can be shown that

$$\frac{E\left[x_{-i}|m_i=1,z_i=1\right]-E\left[x_{-i}|m_i=1,z_i=0\right]}{E\left[x_i|m_i=1,z_i=1\right]-E\left[x_i|m_i=1,z_i=0\right]}=E_{\widehat{f}}\left[\Delta x_{-i}\left(\boldsymbol{\alpha}_i\right)|\boldsymbol{\alpha}_i\in A^*\right], \tag{6}$$

where the notation $\widehat{f}$ indicates that the expectation over $\boldsymbol{\alpha}\in A^*$ is taken using the marriage-probability weighted density (which is defined only for compliers and under Assumption 4) $\widehat{f}\left(\boldsymbol{\alpha}_i\right)\equiv\widehat{\mu}\left(\boldsymbol{\alpha}_i\right)f\left(\boldsymbol{\alpha}_i\right)/\sum_{\boldsymbol{\alpha}\in A^*}\widehat{\mu}\left(\boldsymbol{\alpha}\right)f\left(\boldsymbol{\alpha}\right)$ rather than the standard conditional density. The IV/Wald estimator, 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 (weighted) average effect of education on partner skill level among compliers.

Further Issues

The above model, while fairly general, does contain a few restrictive features. First, it assumes that education precedes marriage. In the analysis below, we will use as education measure 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. Second, the model assumes that each individual meets precisely one potential partner. This reduces "competition" in the marriage market relative to cases where individuals meet multiple potential partners. Nevertheless, we believe that the identification strategy would continue to be valid in more general settings.

A crucial, but somewhat subtle, assumption in the model 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 some commonly used instruments for educational attainment. Consider e.g. using the raising of a school leave age 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 constitutes a marriage-market relevant social grouping, 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 cutoff birth date for being affected by the raising of the school leaving age will confound individual effects of education with general equilibrium effects of an increased level of educational attainment in the marriage market. The same comments apply to e.g. distance to college when marriage markets have a geographical dimension. 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.[12]

[12]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.

III 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, Wales.[13].

The Academic Year and School Entry Policy

The academic year runs from September 1 to August 31 with three terms starting in September, January, and April, respectively. By UK law, all children of compulsory school age (between 5 and 16) must receive a full-time education. In England and Wales, children must start school at the beginning of the term after they turn 5. There is, however, significant variation in admissions policies across local education authorities (LEAs). While the statutory policy is adopted by only around 1% of LEAs, most LEAs operate a triple-entry-point system that admits children at the beginning of the term in which they turn 5. The system that is becoming increasingly popular over time is based on a single-entry-point, which implies that all children start school in September of the academic year in which they turn 5, regardless of age.[14]

School Exit Policy

The British government has raised the minimum school leaving age several times since the introduction of compulsory education in 1870. The main motivations given have been focused on generating more skilled labour by providing additional time for students to gain skills and qualifications. The current school leaving age of 16 has been in force since September 1973, as a

[13]The education system in Northern Ireland differs slightly from that in England and Wales. E.g. 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

[14]Nearly half of the children born between 1997 and 1999 started school in an LEA where this single-entry-point system was in operation (see Crawford et al. 2007).

result of the Raising of School Leaving Age (RoSLA) Order of 1972. This built on the previous RoSLA from 14 to 15 which occurred in April 1947, following the 1944 "Butler" Education Act.

Unlike in the US, children in the UK are generally not deemed to have attained the age of compulsory schooling, and therefore allowed to leave school, on the exact date in which they themselves attain the age of 16. 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.[15]

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, as a result of the 1996 Education Act. 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 earlier school leaving arrangements will be the relevant ones for our purposes.

The combination of the school entry policy and the school leaving policy implied two distinct discontinuities in required length of schooling with respect to date of birth. First, a discontinuity obtained at the cutoff dates between academic cohorts, i.e. at the August-September threshold. While August-born children were forced to stay until the end of the school year, September-born children were allowed to leave at Easter. Since those born after the threshold date have shorter required schooling, this discontinuity has strong similarities to the discontinuities generated by school entry policy in the US as used by Angrist and Krueger (1991) and many after them. Second, a discontinuity obtains also at the winter cutoff date for being allowed to leave at Easter, i.e. at the January-February threshold. While January-born children were allowed to leave at

[15]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 labour market.

Easter, February-born children were forced to stay until the end of the school year. Although the two discontinuities at a first glance appear to be “flipsides” of each other there is nevertheless an important difference: while the first discontinuity obtained *between* academic cohorts, the second discontinuity obtained *within* academic cohorts. The fact the first discontinuity obtains between academic cohorts implies that the required length of schooling is not the only difference between individuals born on opposite sides of the threshold. Those born after the 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 threshold would have a higher *absolute* age at school start and also a higher age *relative* to their academic cohort peers. In contrast, for the second discontinuity, by virtue of January and February born children belonging to the same academic cohort, and generally starting school at the same time, neither age effect would have obtained. For this reason the second discontinuity has stronger appeal as a pure required education effect and will be the one focused on in this paper.

The significance of the discontinuity is, however, not only that it implies a nominal difference of up to two months (one term) of required schooling. More importantly, it interacts with the qualification system in England and Wales under which students aged 16 sit crucial intermediate-level examinations at the end of the summer term.

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. Historically, different types of schools entered their pupils for different examinations at age 16. 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 (there is no upper limit) 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 present. 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 effectively inconsequential since leaving at the earliest possible date meant leaving school a year prior to the qualifications-generating examinations sat at age 16.[16] 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).

IV 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,

[16] 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).

since 1992 LFS data is available on a quarterly basis.[17] We pool 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.

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.

The basic sample criteria we use are as follows. We select women born and currently living in England or Wales who are aged 18 or above at the time of interview.[18] As noted above, in our main analysis we further restrict our attention to individuals belonging to a certain set of academic cohort, in particular, we include individuals born between September 1957 and August 1971.

For each individual we have information on year and month of birth. Since we also know the year and month of interview we know the individual's age in months when surveyed. For marital status we will consider exclusively whether or not the individuals is currently married – the survey does not allow us to determine any details of the individuals' marital histories.[19]

[17]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.

[18]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. 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.

[19]We focus on whether an individual is currently legally married. Those that are not married hence include both never married and divorced (and legally separated). Moreover, those currently not married also include

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. Fundamentally the individuals report the age at which they left continuous full time education and what qualifications they hold. The standard measure of age when leaving full time education is not the most useful for the current purposes as will be made clear below. Instead we focus on formal qualifications held by the individuals. In particular, we will focus on *academic* qualifications. Indeed, for most of the analysis we will simply consider whether an individual holds *any* academic qualification. There are several reasons for doing so, generally having to do with timing and exams.

First and foremost, as shown below, the natural experiment that we consider, generated by the school leaving rule which treated differentially individuals born before versus after the January-February threshold, affected precisely whether the individual holds no academic qualification versus a low level academic qualification. Generally individuals tend to obtain academic qualifications in a certain sequence, implying that higher levels of qualifications are obtained at higher ages. Whether or not an individual will ever obtain *any* academic qualification is typically determined by the O-level and CSE 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.

Second, the instrument that we use for our analysis (date of birth relative to the January-February threshold) would have had its impact on the particular education measure (no academic qualification versus a low level academic qualification) at the age of 16 and would thus have preceded any marriage decision for the vast majority of individuals in the data.

Third, while we also have information on vocational qualifications, such qualifications are

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cohabitants. While the data allows us to consistently measure who is currently married, unfortunately, due to changes in the underlying survey question, it does not allow us to identify divorced individuals and cohabitants consistently across time.

more frequently obtained at various stages in the individuals' lives. As a result, when we consider the fraction of any given academic cohort that holds some qualification (academic or vocational) we observe this fraction to be increasing over time as the cohort ages. Such apparent "skill upgrading" would cause a host of problems for the analysis, including problems relating to interpretation; e.g. we would be less certain that skill acquisition comes before marriage.

Mostly for descriptive purposes we classify the individuals by their highest academic qualification 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 (an "Advanced Level" examinations taken at age 18 relevant for entry into higher education), (iv) "Level 4" denotes a first degree (or equivalent), and (v) "Level 5" denotes a higher degree (at postgraduate level).

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.

Month of Birth and Family Background

In theoretical analysis above it was assumed that the instrument z_i was uncorrelated with the individual's characteristics α_i. 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,

$$z_i = \begin{cases} 1 & \text{if } MoB_i \in \{2, ..., 8\} \\ 0 & \text{else} \end{cases} . \tag{7}$$

This short section explores whether individuals' characteristics are unrelated to their month of birth.[20] It is important to note that even if there is a systematic relationship between month of birth and some personal characteristic, identification can still be secured in two cases. The first case is when the characteristic is observed and hence can be directly controlled for. The second case is when the distribution of the individual characteristic is continuous with respect to date of birth at the relevant threshold date. In this latter case, individuals on either side of, but close to, the threshold are effectively identical in all other respect other than with respect to the instrument and the analysis above holds for individuals close to the threshold.[21] This motivates why, in the analysis below, we carefully examine the sensitivity of our estimates with respect to window size.

It is clear that there is a seasonal pattern in births in the UK, with a disproportionate number of births occurring in the spring months. E.g. in our main LFS sample described in Table 1, 26.4 percent of women are born in the months March through May, while only 23.9 percent of women are born in the months October through December. The key question is whether the degree of seasonality varies with family type. This question was investigated for the UK in an early study by James (1971) who concluded that the degree of seasonality increases with father's "social class".[22] This would suggest that children born in the spring-months may more often have professional, non-manual fathers.

In order to consider this potential threat to identification in closer detail we use data from the Youth Cohort Study (YCS) which surveys selected academic cohorts of potential school leavers in England and Wales. The first three cohorts to be surveyed were those born between Sept. 1967 - Aug. 1968, Sept. 1968 - Aug. 1969, and Sept. 1969 - Aug. 1970 respectively. The YCS did not survey the following cohort, so the fourth YCS cohort comprised individuals born

[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 e.g. that women giving birth in the winter months are more often teenagers, less frequently married, less frequently white, less educated and younger.

[21]Formally, unbiasedness obtains in the limit as the window size if reduced to zero. See e.g. Hahn, Todd and van der Klaauw (2001) for a theoretical discussion and McCrary and Royer (2011) for an application.

[22]James' analysis was based on the traditional Registrar General's Social Class I-V classification system where the father's social class is derived from his occupation as recorded on the child's birth certificate.

between Sept. 1971 - Aug. 1972. There was then another gap, so that the fifth YCS cohort comprised individuals born between Sept. 1973 - Aug. 1974. Ideally we would like to focus on individuals born in our main period of interest, i.e. up to Sept. 1971. However, in the interest of sample size, we include the fourth YCS cohort as a substitute for the non-surveyed 1970-71 cohort.[23] The YCS cohort members were surveyed in three consecutive years, starting in the spring of the year following that in which they had reached the minimum school leaving age. The advantage of using the YCS is that it offers a good-sized sample of individuals of school-leaving age, born alongside our main LFS sample, and with information about the education and employment status of the respondent's parents.

We use information on the individual's year and month of birth, ethnicity, whether the father and/or mother hold a degree, and the parents' employment status. We include all individuals with complete information on year and month of birth (96 percent) and who report living with at least one parent (97 percent). Descriptive statistics of the sample is provided in Table 2.[24] Since the key issue for our purposes is whether there is any difference in parental characteristics for children born on either side of the January-February threshold, the table provides the means for individuals born in the months November-January and in the months February-April, and the implied difference. The only statistically significant raw differences are with respect to ethnicity (which will be directly controlled for in the analysis below). To consider parental education and economic activity in closer detail, Figure 1 plots father's and mother's degree rates and employment rates against the respondent's month of birth (with 95 percent confidence intervals). The figure reveals no clear seasonal pattern with respect to parents' education. Individuals born late in the spring appear to be more likely to have economically active fathers, whereas individuals born in the early autumn appear to be more likely to have economically active mothers. However, there is no obvious discontinuities at the January-February threshold.

[23] The results when not using cohort 4 (or, indeed, including cohort 5) are qualitatively similar.

[24] The number of observations refers the maximum number satisfying the selection criterion. Data limitations imply that the sample size is lower for a number of variables. E.g. parents' education was collected in the third sweep for each cohort, leading to a lower response rate.

In order to focus more particularly on differences around the threshold date, we perform a regression analysis where each parental variable is regressed on z_i (defined as in (7)) and on a full set of academic cohort- and ethnicity dummies. Moreover, each regression is carried out for five "window-sizes" around the January-February threshold, starting from a wide window of five months on either side of the threshold and going down to a single month on either side. The use of window sizes amounts to a particular form of weighting scheme where an observation is given a weight of unity if within the window and a zero weight if outside it. As an alternative, the final column uses the largest window size, but applies an inverse distance weighting scheme where 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 threshold date.[25] This weighting scheme has the advantage of using more observations, while ensuring that identification predominantly comes from variation close to the discontinuity. The results are provided in Table 3.

The regressions suggest no differences in neither father's education nor mother's employment. For mother's education the regression coefficients are negative, but no statistically significant relation obtains when using a window size that exceeds one month on each side (and Figure 1 does not suggest any difference between spring and autumn born). Turning to father's economic activity, we see that the coefficients are generally positive but small and not statistically significant for narrow window sizes or using inverse distance weighting.

To conclude, using a sample of individuals born towards the end of our main period of interest with detailed information about mother's and father's education and employment, we find no consistent evidence to suggest that those born after the threshold date had more educated and economically active parents than those born before the threshold.

Spousal Correlation in Month of Birth

In the theoretical analysis above it was further assumed that z_i does not directly affect the equilibrium probability of matching with a skilled partner. This assumption could be violated

[25] Formally we define $d_i = 0.5$ for individuals born in January and February, $d_i = 1.5$ for individuals born in December and March, etc.

if partners meet during the extra period of education induced by the school-leaving rule or, indeed, more generally if there is positive correlation in partners' month of birth/requirement to stay on. If there were a positive correlation in partners' month of birth, then women born after the January-February threshold would tend to be more frequently married to men holding some qualification than women born before the threshold as their partners would have more frequently been required to stay on the extra term.[26] Hence we consider in this very brief subsection the spousal correlation in month of birth.

We start however by noting that few marriages involve spouses from the same academic cohort. Only 13 percent of married partners come from the same academic cohort. In contrast, 14 percent of married women have husbands who are from the academic cohort preceding their own. Looking more closely within those couples where the partners are from the same academic cohort the correlation in the dummy for having been required to stay on is 0.013 and not statistically significant.

More generally, looking across all couples, there is no evidence that women born after the January-February threshold tend to be matched with men born later in the academic year. This is illustrated in Figure 2. The figure plots the coefficients from eleven regressions (along with 95 percent confidence intervals). For each month j equal to October to August, we regress a dummy for the husband being born in month j or later within the academic year on a dummy for the wife having been born in the months February to August (i.e. z_i). Hence the regressions explore whether women who, due to their month of birth, were required to stay on tend to be married to husband born "later" within the academic year.[27] As no coefficient is even close to being statistically significant, we conclude that there is not evidence to suggest a positive correlation in partners' month of birth/requirement to stay on.[28]

[26]Formally a positive correlation in partners month of birth would imply a positive correlation between z_i and z_{-i} which would violate Assumption 3.

[27]Specifically, for $j = February$, the regression tests for a correlation between z_i and z_{-i} among married couples.

[28]The regressions reported in Figure 2 use all married women from our main LFS sample. We have performed corresponding analysis focusing on women born closer to the January-February threshold date but have not found

V Results

We present our result 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.

There are two threshold points in the academic year. The first is the August-September threshold. Those born after this threshold (i.e. in September onwards) would belong to the following academic cohort and would generally have to wait to start school relative to those born before the threshold. Moreover, those born after this threshold would not have been required to stay on. The second is the January-February threshold. Individuals on either side of this threshold would belong to the same academic cohort, but would differ in the requirement to stay on.

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. In contrast, academic attainment does *not* change monotonically at the August-September threshold: while those born before this threshold are more likely to hold some low level qualification, those born after the threshold are more likely to hold some higher level qualification. Due to this lack of monotonicity at the August-September threshold, we henceforth focus on the January-February threshold.

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 little evidence of any such

any evidence to suggest that women born after the threshold are more frequently married to men born later within the academic year.

variation. In particular, we cannot find any evidence that those who were required to stay on in school 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 spouse'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 IV estimates suggest a causal effect of the woman's academic qualification on the properties of her spouse.

Month of Birth and Academic Attainment

We begin with an analysis of the relationship between month of birth and academic attainment.[29] Figure 3 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 decrease in the fraction holding no academic qualification. The figure also suggests that having been required to stay on is potentially associated with a slight increase in the probability of holding a level 2 academic qualification. For higher qualifications there is no evidence of any discontinuity at the January-February threshold date.

A key requirement for the instrumental variable approach to generate interpretable results is that the impact of the instrumental variable should have a monotonic impact on the endogenous variable. From Figure 3 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

[29]A detailed analysis of the impact of the school-leaving rule for actual school-leaving behaviour is presented in Del Bono and Galindo Rueda (2007).

required to stay on are relatively infrequent. In order to do this we report the results from a set of estimated linear probability models where, for each academic qualification level j, we regress a dummy for the individual holding that level of qualification or above on a dummy for having been required to stay on. Only if *all* estimated coefficients are non-negative (and some strictly positive) can we argue that those born after the January-February threshold are unambiguously more academically qualified. Moreover, in order to explore the sensitivity of the estimates, we vary the "window-size" from five months on either side of the threshold (i.e. including everyone born October through May) down to one month on either side (i.e. only including those born in January and February). We also use the same inverse distance weighting scheme as used in Section IV.

Table 4 gives the coefficient on having been required to stay on in each regression. The table confirms that the main effect of having been required to stay on is 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 point and relatively stable with respect to the window size. 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.[30] Of key importance for our purposes, the complete absence of any statistically significant *negative* coefficients in Table 4 suggests an unambiguously positive impact of being born after the threshold date on academic attainment.

This contrasts the August-September threshold which partitions academic cohorts. A corresponding analysis of the effect of this threshold point shows that individuals born before the August-September threshold (who were required to stay on) are indeed *more* likely to hold some academic qualification. However, they are *less* likely to hold qualifications at levels 2 through to 4. This feature likely reflects the type of relative-age-at-school-start effect highlighted by Crawford et al. (2007) whereby those who are oldest within their academic cohort perform

[30]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).

better.[31]

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 school-leaving policy was to induce some people born after the January-February threshold date to stay on for exactly one extra term. Then some January-born children would leave at Easter while the corresponding individuals 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. We partition age when leaving full time education into three categories: 16 or below, 17 or 18 (thus indicating having stayed on past the minimum school leaving age), and 19 or above. Table 5 presents estimates of the effect of having been required to stay on on the probability of leaving full-time education at these various ages, based on regressions that contain the standard set of controls. The noticeable feature of Table 5 is the near complete absence of any effect. The only statistically significant effects obtains for leaving at age 16 or below and 19 and above and in each case only for the widest window size. The effect at age 16 is likely to be driven by the inclusion of June-born individuals whose birthday fall after the exam season. The effect on leaving at age 19 or above may is likely to reflect the relative-age-at-school-start effect mentioned above. For more narrow window sizes and for the distance weighted specifications, there is no evidence of any impact of having been born after the January-February threshold on the distribution of school leaving ages.

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 4 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

[31] Details of this analysis are available on request.

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 raising of the school leaving age (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, there are no noticeable differences between those born before and after the January-February thresholds. For the main sample cohorts, we observe that the rate of holding some academic qualification trends upwards. Moreover, Figure 4 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. 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. The figure shows that only in the first year of the new system was there any noticeable gap in the rate of holding some academic qualification.

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.

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 5 which shows the fraction of individuals who are currently married by level of academic attainment relative to individuals who hold no academic qualification.[32] The figure shows how, up until the age of around 28-30, those who obtain a level

[32]Specifically, the figure illustrates the coefficients on the various levels of academic attainment from regressions, one for each age, which also includes controls for academic cohort, survey year, and ethnicity.

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 point relative to every other level of attainment. Hence there is a strong association between academic attainment and the probability of being 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.

The upper part of Figure 6 shows the fraction currently married at each age, for individuals born in the months November-January (and hence not required to stay on) and February-April (and hence required to stay on) respectively, controlling for academic cohort, survey year, ethnicity, and for differences in age in months. Due to the scale it is difficult to visually detect any differences. For that reason, the lower part of Figure 6 shows the estimated *difference* by age. The overall difference (indicated by the hatched line) is actually negative, but very small and not statistically significant. Hence, we cannot find any evidence to suggest that those who were required to stay on are more likely to be married.

We can also consider in some more detail how the fraction currently married varies with month of birth. Since we only observe a positive association between (a low level) academic qualifications and marriage for individuals above their early 20s we focus here on individuals aged 23 or above.[33] To this end we regress a dummy for being married on a set of month of birth dummies (leaving out February as reference group), along with a full set of academic cohort dummies, survey year dummies, ethnicity dummies, and age in months (in linear, square and cubic form). The left panel of Figure 7 plots the frequency of being currently married by month of birth relative to February (i.e. the estimated coefficient on each month of birth in the regression). The figure shows that the probability of being married declines slightly with

[33]A second benefit to leaving out younger individuals from this part of the analysis is that vast majority of individuals – 98 percent in the main sample – would have left full time education by age 23.

month of birth within the academic year. This finding is somewhat surprising if one believes that individuals' demographic life events depend not only on their absolute age, but also on their "social age" defined by their academic cohort (Skirbekk, Kohler and Prskawetz, 2004). Nevertheless, the most important aspect is that there is no suggestion of any "discontinuity" at the January-February threshold date.

In order to explore in further detail whether there is any difference in marriage frequency we estimate a set of linear probability models where the dummy for being married is regressed on a dummy for having been required to stay on along with the same controls as above, but with varying window sizes around the threshold date. Focusing again on those aged 23 or above, the right panel of Figure 7 shows the coefficient on having been required to stay on (and the 95 percent confidence interval) as the window size is gradually reduced from five months on either side of the threshold down to only one month on either side. The figure shows that the difference in the fraction currently married is effectively zero for all window sizes except the very smallest. Hence, from this analysis, we conclude that there is no indication that the probability of being currently married changes discontinuously with month of birth at the threshold point.

Consider then using the dummy for having been required to stay on as an instrumental variable for estimating the effect of holding an academic qualification on the probability of being married. Focusing again on individuals aged 23 or above, Table 6 presents the estimated effect of holding some academic qualification on the probability of currently being married as the window size is gradually reduced. The OLS estimates consistently show a nine percentage point increase in the probability of being currently married. The IV estimates are effectively zero (or negative) for all window sizes except the smallest one (which is also highly imprecise). Hence we conclude that for women there is no evidence of any causal effect of holding an academic qualification on the probability of being currently married.[34]

[34] We have also performed a similar analysis using only individuals aged 18 to 22. However, unsurprisingly, for this age group the estimates are too imprecise to allow any meaningful conclusions to be drawn.

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 could 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 concluded 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.

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 partner holds any academic qualification, and (ii) whether or not the partner 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 qualification or not (see Section II).

As expected there is a strong positive association between a woman's academic qualification and that of her spouse. Table 7 shows the OLS estimated effect of holding an academic qualification at various levels on the probability of the spouse holding *some* academic qualification. Women with academic qualifications are much more likely to be married to husbands who also have some academic qualification.[35] 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 7 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:

[35]More generally it is also true that there is marital sorting by qualification level. E.g. 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.

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 spouse characteristics vary with the individual's month of birth. Adopting the same regression approach as above (and the same controls), the left panel of Figure 8 shows how the probability of a woman being married to a husband who holds some academic qualification differs by her month of birth relative to 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 8 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 married to economically inactive husbands.

Figure 9 looks more closely at the difference in the properties of the husbands of those women who were required to stay on and those that were not. Following the approach from above, the figure illustrates the estimated coefficients on a dummy for having been required to stay on (using regressions with the same controls as above), and with varying window sizes. The left panel shows that women born after the January-February threshold are more likely to have husbands who hold some academic qualification than are married women born before the threshold. Moreover, the difference is fairly stable with respect to the window size and is statistically significant for all window sizes except the smallest one. The right panel shows that the same is true also for the economic activity rate of the husbands: those women who were required to stay on are more frequently married to working husbands. Moreover, the gap is stable with respect to the window size and is statistically significant at every window size except the smallest one.

These findings map into corresponding IV estimates. Table 8 presents the estimated effects using OLS and using IV (and for varying window sizes). For the partner holding some academic qualification, the IV estimates are always positive, reasonably stable with respect to window size, and statistically significant at all window sizes except the smallest one. The IV estimates are smaller than the OLS estimates, but are more similar to the OLS estimate of specifically holding a level 1 academic qualification (See Table 7). Similarly, for the partner being economically active, the IV estimates are always positive, stable with respect to window size, and statistically significant at all window sizes except the smallest one. Moreover, the IV estimates are very similar to the OLS estimates.[36]

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 married husbands holding some academic qualification and who (years later) are more frequently economically active. Indeed, the IV estimates which purport to measure 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 an academic qualification and the academic qualification and economic activity rate of their husbands operate through causal channels. As the theoretical exposition above made clear, an otherwise very plausible *non-causal* channel would be that individuals who meet naturally tend to have correlated characteristics.

Robustness Analysis

The main finding so far has 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.

In Figure 4 it was shown that the gap in the qualification rate only existed in academic cohorts from 1957 to 1970, i.e. the academic cohorts born late enough to face the minimum

[36]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 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.

school leaving age of 16 but early enough to sit exams before the introduction of the GCSEs. A natural robustness test is then to check whether month of birth relative to the January-February threshold only matters for the 1957 to 1970 academic cohorts also in terms of marital outcomes.[37] If e.g. the results were driven by a seasonal pattern in parental characteristics, then the same effect of month of birth should be observed outside the main period of interest as such a mechanism would not be confined to the 14 academic cohorts of interest. To this end we use an extended sample of married women which includes all academic cohorts from 1952 through to 1975. We regress each spousal characteristic on the dummy for having been required to stay on (along with survey year dummies, academic cohort dummies, ethnicity dummies, and age in month in linear, square and cubic form), but we allow the effect of having been required to stay on to be different in the "pre-period" (the five academic cohorts 1952-1956), the "main period" (the 1957-1970 academic cohorts), and the "post-period" (the five academic cohorts 1971-1975). As above, we do this for varying window sizes.

The results are provided in Table 9. Looking first at the spouse holding some qualification, the top part of Table 9 shows that, in the main period, having been required to stay on is associated with about a one percentage point higher probability of being married to a partner who holds some academic qualification. Moreover, this estimate is not sensitive to the window size and is statistically significant for all window sizes except the smallest. In contrast, in the pre-period, the estimates are close to zero and never statistically significant, while in the post period the estimates are generally negative (but not very precise) and not statistically significant. Hence the positive relation between having been required to stay on and spouse qualification rate only appears to exist in the main period, i.e. the only period in which having been required to stay on is associated with a higher own qualification rate.

Turning to spouse economic activity rate, for the main period, having been required to stay on is associated with 0.6-0.8 percentage point higher probability of being married to an economically active husband, with the estimates being stable with respect to window size and

[37] It should be noted however that this is not an ideal test in the sense that the 1957 to 1970 academic cohorts were not the only ones to face the Easter-exit rule – the same rule also affected earlier and later cohorts.

statistically significant for all window sizes except the smallest. In this case the estimates tend also to be positive in the pre- and post-periods. Nevertheless, the estimates in these periods are numerically smaller, not statistically significant, and are effectively zero for the small window size. Hence, while slightly less conclusive, the results suggest that the main period is the only period for which having been required to stay on is robustly associated with a higher probability of being married to an economically active husband.

VI Conclusions

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.

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 suggestion 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 strongly 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. In fact, our results suggest that most of the observed positive association between a woman's holding of an academic qualification and her husband's characteristics can be given a causal interpretation.

A causal effect of a woman's academic qualification on the characteristics of her spouse can obtain fundamentally through two distinct channels. First, a qualification can make her more "attractive" in the marriage market, leading to a different marriage propensity at any given match with a potential partner: with a qualification she may not be turned down by someone who would have done so had she been unqualified, and she herself may become more inclined to reject less qualified men. Second, it may be that investing in education leads a woman to meet a different selection of potential partners. In this context it is interesting to note that those who were required to stay on would typically not have had to change school or even class in order to comply. Moreover, very few of those required to stay on for an extra term appear to have responded by staying on even longer and obtain some higher level of qualification. This suggest that the staying on requirement is unlikely to have affected the selection of potential partners met directly through school.

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Table 1: Descriptive Statistics for 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: Descriptive Statistics for the Youth Cohort Study Sample

Variable	**All**	**Born Feb-Apr**	**Born Nov-Jan**	**Difference**
Father holds Degree	0.152	0.148	0.148	0.000
				(0.006)
Mother holds Degree	0.081	0.075	0.081	-0.006
				(0.005)
Father is Employed	0.903	0.908	0.903	0.006
				(0.004)
Mother is Employed	0.427	0.422	0.434	-0.011
				(0.007)
Ethnicity: White	0.929	0.938	0.925	0.013
				(0.003)**
Ethnicity: Asian	0.031	0.026	0.033	-0.007
				(0.002)**
Ethnicity: Black	0.013	0.011	0.015	-0.004
				(0.001)*
Ethnicity: Other	0.026	0.024	0.026	-0.002
				(0.002)
Nr. Obs.	49,950	11,715	12,813	24,528

Notes: The sample includes young people from YCS cohorts one to four, who reached minimum school leaving age in the academic years 1983-84, 1984-85, 1985-86, 1987-88 (and hence born between September 1967 and August 1972) with complete year and month of birth information (96 percent) and who report living with at least one parent (97 percent). The final column reports the difference in mean between February-April born and November-January born with standard error on the estimated difference in parenthesis. Significance levels: $**$: 1% $*$: 5%

Table 3: Estimates of Discontinuity in Parental Characteristics at January-February Threshold

	Window					
Variable	**Sep-Jun**	**Oct-May**	**Nov-Apr**	**Dec-Mar**	**Jan-Feb**	**Sep-Jun**
Father holds Degree	0.004	0.002	0.000	0.001	-0.015	-0.004
	(0.005)	(0.005)	(0.006)	(0.008)	(0.011)	(0.006)
Mother holds Degree	-0.002	-0.003	-0.004	-0.006	-0.017	-0.008
	(0.004)	(0.004)	(0.005)	(0.006)	(0.008)*	(0.005)
Father is Employed	0.005	0.007	0.003	0.004	0.003	0.004
	(0.003)	(0.003)*	(0.004)	(0.005)	(0.007)	(0.004)
Mother is Employed	-0.007	-0.007	-0.004	0.000	0.004	-0.002
	(0.005)	(0.005)	(0.006)	(0.008)	(0.011)	(0.006)
Weighting	No	No	No	No	No	Inv. Dist.
Nr. Obs.	41,687	33,082	24,528	16,190	7,851	41,687

Notes: The table reports the estimated coefficients on a dummy for being born in February or later in the academic year in a set of linear regressions where each parental characteristic is an outcome variable. Each regression also includes a full set of academic cohort- and ethnicity dummies. Columns indicate "window size" around the January-February threshold. The final column uses inverse distance weighting where each observation is given a weight equal to $1/d$ where d is the distance months from the threshold. Significance levels: $**$: 1% $*$: 5%

Table 4: Effect of Having Been Required to Stay on on the Probability of Holding Academic Qualification Level j or Above by Window Size.

	Window					
Qual. Lev.	**Sep-Jun**	**Oct-May**	**Nov-Apr**	**Dec-Mar**	**Jan-Feb**	**Sep-Jun**
Level 1	0.037	0.037	0.038	0.036	0.037	0.037
	(0.002)**	(0.002)**	(0.003)**	(0.003)**	(0.004)**	(0.003)**
Level 2	0.005	0.007	0.011	0.011	0.012	0.009
	(0.003)	(0.003)*	(0.003)**	(0.004)**	(0.005)*	(0.003)**
Level 3	-0.002	-0.001	0.001	0.000	0.000	-0.001
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)
Level 4	0.001	0.002	0.003	0.004	0.006	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Level 5	-0.001	0.000	0.000	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Weighting	No	No	No	No	No	Inv. Dist.
Nr. Obs.	189,637	151,629	112,726	76,016	37,652	189,637

Notes: The table reports the estimated coefficients on a dummy for being born in February or later in the academic year in a set of linear regressions where the outcome variable in each case is a dummy for holding an academic qualification at level j or above. 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. Columns indicate "window size" around the January-February threshold. The final column uses inverse distance weighting where each observation is given a weight equal to $1/d$ where d is the distance months from the threshold. Significance levels: $**$: 1% $*$: 5%

Table 5: Effect of Having Been Required to Stay on on the Probability of Leaving Full-Time Education at Various Ages

			Window			
Age Left FTE	**Sep-Jun**	**Oct-May**	**Nov-Apr**	**Dec-Mar**	**Jan-Feb**	**Sep-Jun**
Age 16 or below	0.007 (0.003)*	0.002 (0.003)	-0.003 (0.003)	-0.005 (0.004)	-0.003 (0.005)	0.000 (0.004)
Age 17-18	-0.001 (0.003)	0.001 (0.003)	0.002 (0.003)	0.004 (0.003)	0.000 (0.005)	0.000 (0.003)
Age 19 or above	-0.006 (0.002)**	-0.003 (0.002)	0.000 (0.002)	0.001 (0.003)	0.003 (0.004)	0.000 (0.003)
Weighting	No	No	No	No	No	Inv. Dist.
Nr. Obs.	189,637	151,629	112,726	76,016	37,652	189,637

Notes: The table reports the estimated coefficients on a dummy for being born in February or later in the academic year in a set of linear regressions where the outcome variable in each case is a dummy for having left full time education at age j. Each regression also includes 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. Columns indicate "window size" around the January-February threshold. The final column uses inverse distance weighting where each observation is given a weight equal to $1/d$ where d is the distance months from the threshold. Significance levels: $**: 1\%$ $*: 5\%$.

Table 6: Estimates of the Effect of Holding an Academic Qualification on the Probability of Being Currently Married among Women Aged 23 or Above.

	Window					
Outcome Variable	**Sep-Jun**	**Oct-May**	**Nov-Apr**	**Dec-Mar**	**Jan-Feb**	**Sep-Jun**
OLS						
Currently Married	0.092	0.093	0.089	0.090	0.093	0.092
	(0.003)**	(0.003)**	(0.004)**	(0.005)**	(0.006)**	(0.004)**
IV						
Currently Married	-0.051	-0.011	-0.049	-0.011	0.068	-0.018
	(0.080)	(0.083)	(0.087)	(0.106)	(0.145)	(0.101)
Weighting	No	No	No	No	No	Inv. Dist.
Nr. Obs.	159,944	128,157	95,475	64,377	31,869	159,944

Notes: The table reports the estimated coefficients on a dummy for holding some academic qualification in a set of regressions where in each case the outcome variable is a dummy for being currently married. In the IV regressions, a dummy for being born in February or later in the academic year is used as instrument for holding some academic qualification. Each regression also includes 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. Columns indicate "window size" around the January-February threshold. The final column uses inverse distance weighting where each observation is given a weight equal to $1/d$ where d is the distance months from the threshold. Significance levels: $**$: 1% $*$: 5%

Table 7: Effect of Holding Academic Qualifications on the Probability of Spouse Holding Some Academic Qualification and on the Probability of the Spouse 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. 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 8: Estimates of the Effect of Holding an Academic Qualification on Husband's Economic Characteristics.

	Window					
Outcome Variable	**Sep-Jun**	**Oct-May**	**Nov-Apr**	**Dec-Mar**	**Jan-Feb**	**Sep-Jun**
OLS						
Spouse has Ac. Qual.	0.371	0.370	0.369	0.369	0.366	0.366
	(0.004)**	(0.004)**	(0.005)**	(0.006)**	(0.008)**	(0.005)**
Spouse is Ec. Active	0.146	0.143	0.141	0.135	0.145	0.145
	(0.002)**	(0.003)**	(0.003)**	(0.004)**	(0.005)**	(0.004)**
IV						
Spouse has Ac. Qual.	0.218	0.183	0.273	0.264	0.250	0.259
	(0.079)**	(0.082)*	(0.084)**	(0.105)*	(0.146)	(0.098)**
Spouse is Ec. Active	0.136	0.124	0.142	0.142	0.178	0.152
	(0.052)**	(0.054)*	(0.057)*	(0.071)*	(0.097)	(0.066)*
Weighting	No	No	No	No	No	Inv. Dist.
Nr. Obs.	92,267	77,048	57,000	38,360	18,977	92,267

Notes: The table reports the estimated coefficients on a dummy for holding some academic qualification in a set of regressions with outcome variables as indicated in the table. In the IV regressions, a dummy for being born in February or later in the academic year is used as instrument for holding some academic qualification. Each regression also includes 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. Columns indicate "window size" around the January-February threshold. The final column uses inverse distance weighting where each observation is given a weight equal to $1/d$ where d is the distance months from the threshold. Significance levels: $**$: 1% $*$: 5%

Table 9: Effect of Having Been Required to Stay on Spouse Characteristics by Period

	Window			
Variable	**Sep-Jun**	**Nov-Apr**	**Jan-Feb**	**Sep-Jun**
Spouse holds Academic Qualification				
RTSO*(Ac.Coh. 1952-56)	0.001	0.001	-0.003	0.001
	(0.005)	(0.006)	(0.010) (0.007)	
RTSO*(Ac.Coh. 1957-70)	0.012	0.014	0.011	0.011
	(0.004)**	(0.004)**	(0.007)	(0.005)**
RTSO*(Ac.Coh. 1971-75)	0.004	-0.001	-0.019	-0.006
	(0.011)	(0.014)	(0.024)	(0.013)
Weighting	No	No	No	Inv. Dist.
Nr. Obs.	150,099	88,890	29,490	150,099
Spouse is Economically Active				
RTSO*(Ac.Coh. 1952-56)	0.005	0.006	0.001	0.004
	(0.003)	(0.004)	(0.006)	(0.004)
RTSO*(Ac.Coh. 1957-70)	0.006	0.007	0.007	0.006
	(0.002)**	(0.003)**	(0.004)	(0.003)*
RTSO*(Ac.Coh. 1971-75)	0.006	0.006	-0.004	0.002
	(0.007)	(0.009)	(0.015)	(0.009)
Weighting	No	No	No	Inv. Dist.
Nr. Obs.	154,314	91,386	30,350	154,314

Notes: The table reports the estimated coefficients on a dummy for being born in February or later in the academic year ("RTSO"), interacted with three dummies indicating subperiod, in a set of linear regressions where the outcome variable, in each case, is a dummy for the husband holding some academic qualification or being economically active. 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. Columns indicate "window size" around the January-February threshold. The final column uses inverse distance weighting where each observation is given a weight equal to $1/d$ where d is the distance months from the threshold. Significance levels: $**$: 1% $*$: 5%.

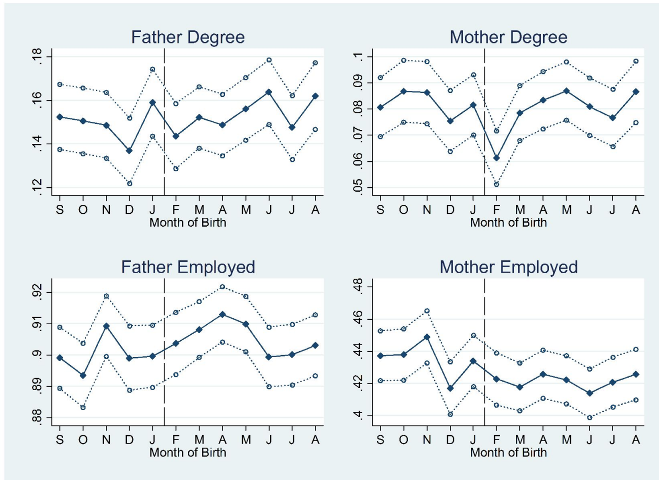

Figure 1: Parental Characteristics by Youth's Month of Birth

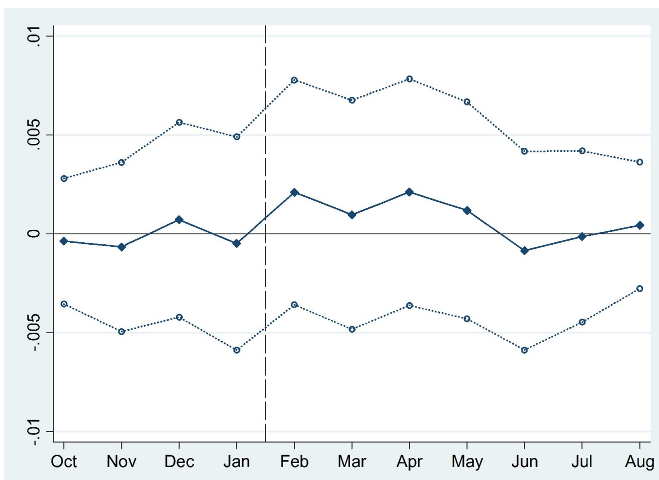

Figure 2: Difference in probability of being married to a husband born in month j or later within the academic year for women born in the months February to August compared to women born September to January.

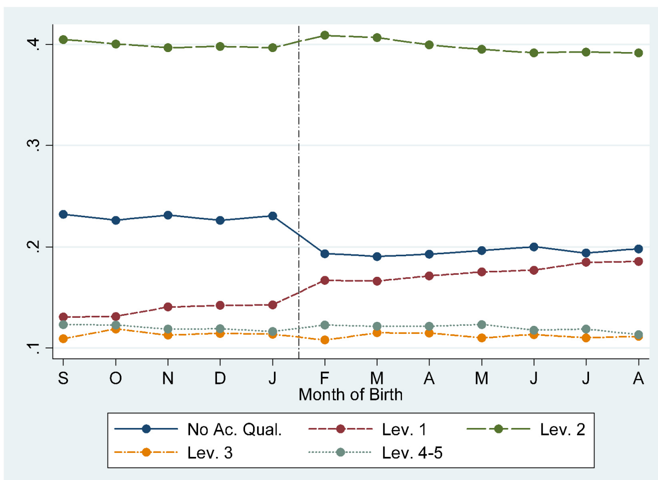

Figure 3: The Distribution of Highest Academic Qualification by Month of Birth

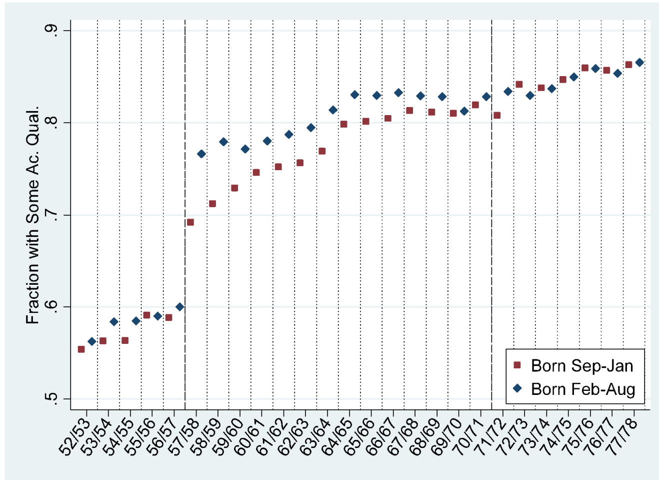

Figure 4: Fraction Holding Some Academic Qualification by Academic Cohort

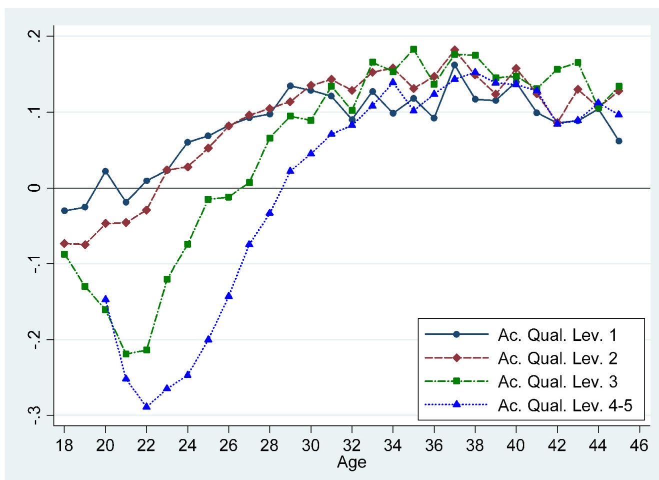

Figure 5: Fraction Currently Married Relative to Individuals with No Academic Qualifications by Age

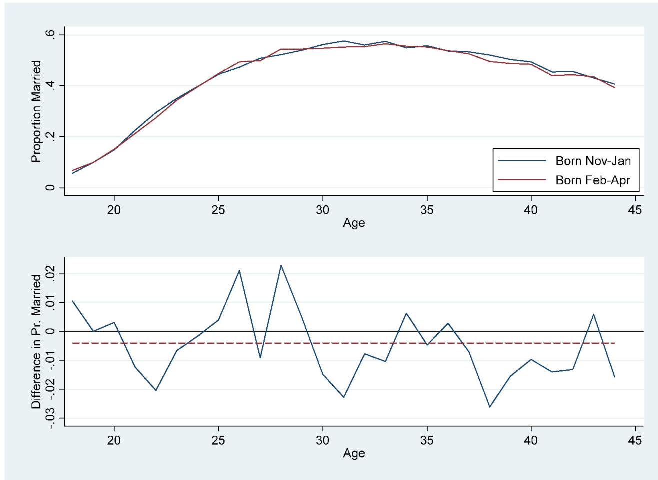

Figure 6: Fraction Currently Married by Requirement to Stay On

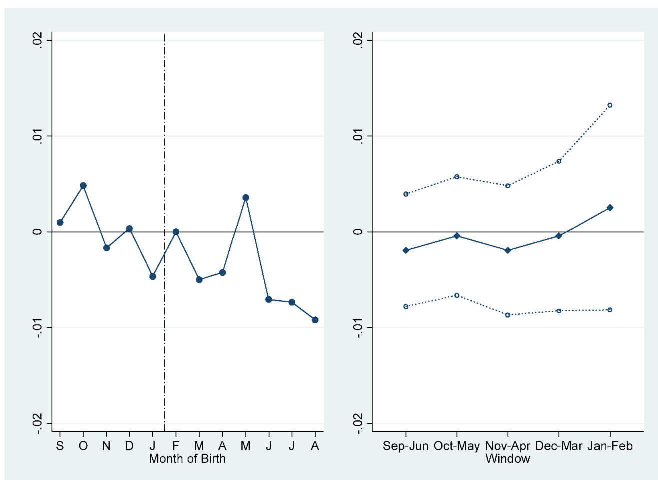

Figure 7: Fraction Currently Married by Month Birth Relative to February, and OLS Estimates of the Difference in Rate of Being Married by Requirement to Stay On, Women Aged 23 and Above

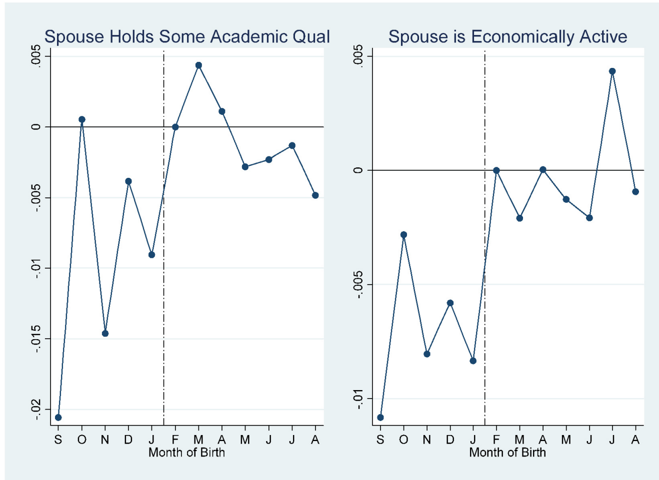

Figure 8: Husbands' Economic Characteristics by Month of Birth of Female Relative to February

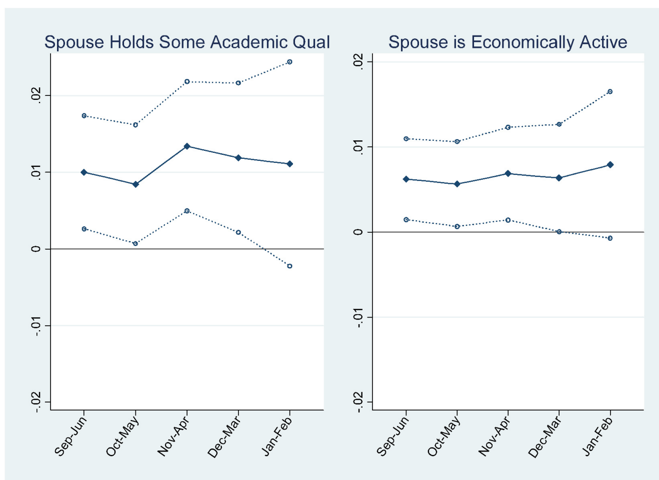

Figure 9: OLS Estimates of the Difference in Husbands' Economic Characteristics by Wife's Requirement to Stay On