Marriage Market Equilibrium, Qualifications, and Ability*

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Abstract

Using data on marital outcomes for individuals born close to the threshold for the 1973 UK Raising of the School Leaving Age (RoSLA) reform, we estimate a equilibrium marriage market model where individuals differ not only in academic qualifications but also in unobserved ability. Following Choo and Siow (2006) the estimated model uses a transferable utility matching framework with random preferences over partner types. We show that accounting for unobserved ability is central for fitting the stylized marriage market responses to the reform. The findings indicate positive marital "ability premia" for both men and women, but no premia for holding a basic academic qualification. We further find that, through its general equilibrium effects, the reform systematically affected the marriage probabilities also of many individuals who were not directly affected in terms of their own educational choices and outcomes.

Keywords: Marriage, Qualifications, Assortative mating, Unobserved ability
JEL Classification: J12, J13

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1. Introduction

Marriage formation and marital sorting informs about the gains from marriage, and how these gains are split within a household (Becker, 1973; Choo and Siow, 2006). Social scientists are concerned with these issues because they are confounded with fertility decisions and human capital investments (Greenwood, Guner, and Knowles, 2003; Chiappori, Iyigun, and Weiss, 2009; Chiappori, Salanié, and Weiss, 2017), and have implications for, among other things, segregation (Fernández, 2002), growth and inequality (Fernández, Guner, and Knowles, 2005; Fernández and Rogerson, 2001; Eika, Mogstad, and Zafar, 2014; Greenwood, Guner, Kocharkov, and Santos, 2016), and intergenerational transmissions (Ermisch, Francesconi, and Siedler, 2006; Guell, Mora, and Telmer, 2015). A full appreciation of these implications necessitates a detailed understanding of equilibrium formation in the marriage market and how the marriage market adjusts to shocks, trends, and policy interventions.

This paper uses the marriage market response to a major UK educational reform to identify and estimate a transferrable utility model of the marriage market in the mold of Choo and Siow (2006), but with observed and unobserved marriage market traits, specifically (academic) qualifications and ability, that are correlated because of selection into qualifications based on ability. We use the estimated model to shed light on the marriage market trade-off between qualifications and ability, as well as the marital return to qualifications (Chiappori, Iyigun, and Weiss, 2009; Bruze, 2015; Chiappori, Salanié, and Weiss, 2017). The paper makes several contributions to the literature on marriage formation and marital sorting.

Our first contribution is to document the marriage market response to the UK Raising of the School-Leaving Age (RoSLA) legislation, a major 1973 UK education reform that raised the school leaving age from 15 to 16.[1] RoSLA induced a distinct shift in the qualification distribution in the treated cohorts, sharply reducing the likelihood of leaving school with no qualifications, while sharply increasing the likelihood of leaving with a basic qualification.[2] We uncover three novel empirical facts regarding the marriage market response to RoSLA. First, the never-married rate of unqualified men and women in the RoSLA-treated cohorts—a group made relatively scarce by RoSLA—increased dramatically, whereas never-married rates for those with basic or advanced qualifications stayed on trend. Second, we confirm overall strong assortative mating on qualifications, and show that assortative mating among those holding no academic qualification increased permanently after RoSLA, for both men and for women, with no discernible RoSLA-

[1] RoSLA, along with an earlier similar 1947 reform, have been heavily utilized for identifying the causal effect of education across an array of different outcomes, typically within instrumental variable or regression discontinuity designs. Our estimates of RoSLA's impact on qualification rates are in line with previous studies, examples of which include studies of the labor market return to education (Harmon and Walker, 1995; Oreopoulos, 2006; Devereux and Hart, 2010), health behaviours and outcomes (Clark and Royer, 2013), criminal activity (Machin, Marie, and Vujić, 2011), and civic engagement (Milligan, Moretti, and Oreopoulos, 2004).

[2] Throughout the paper, a "basic qualification" refers to the formal academic qualification obtained at the minimum school leaving age, and an "advanced qualification" refers to a formal academic qualification obtained in post-compulsory education.

effect on assortative mating among those holding basic and advanced qualifications. Third, we document a temporary and modest shift in the husband-wife age gap distribution in the immediate aftermath of RoSLA: The very first RoSLA treated academic cohorts, i.e. 1957 and 1958, were more likely to marry among themselves than earlier and later cohorts.

That individuals marry assortatively on qualifications with a positive husband-wife age gap are well-established empirical facts, see e.g. Mare (1991) and Mansour and McKinnish (2014). Here, we use RoSLA to provide the first direct empirical evidence on how age and qualifications are traded off in the marriage market in the wake of a large shift in the qualification distribution. Such trade-offs are exactly what one would expect based on standard equilibrium marriage market theory (Choo and Siow, 2006). However, the increase in the never-married rate among individuals with no qualifications is perplexing in light of that same body of theory, which dictates that groups in short supply have lower never-married rates.[3] The failure of this prediction to materialize in the aftermath of RoSLA is suggestive of a confounding influence of an unobserved marriage market trait, namely ability. We note here that the observed marriage market response to RoSLA suggests that both ability and qualifications are intrinsically valued on the marriage market. If, on the one hand, only ability mattered on the marriage market, there would be no need for individuals to trade off partner's age and qualifications around RoSLA. If, on the other hand, only qualifications mattered, or if ability was uncorrelated with qualification, we are left with the counterfactual prediction regarding never-married rates of individuals with no qualifications following RoSLA.

To explore these issues further, in our second contribution, we enrich the preference structure of the Choo-Siow equilibrium marriage market model to include preferences over husband-wife age gaps, unobserved ability, formal academic qualifications, as well as a simple selection model describing the relationship between ability and qualification attainment. Ability is unobservable to the econometrician, but not to the marriage market participants, and is unaffected by RoSLA. Instead, we interpret RoSLA as reducing the opportunity cost of obtaining a basic qualification, with selection on ability implying that individuals responding to RoSLA by obtaining a (basic) qualification having higher ability than those not responding.[4] As a result, the post-RoSLA ability distribution among individuals with no qualifiactions shifts to the left. Insofar as ability is a valued marriage market trait, this compositional change leaves open the possibility that never-married rates among unqualified individuals from the post-RoSLA cohorts increases, as observed in the data. Our identification strategy treats pre- and post-RoSLA cohorts as belonging to a single marriage market, where marriages can occur across policy regimes, and where the tradeoffs involved in doing so is modelled in terms of systematic preferences over age gaps.

[3]Decker, Lieb, McCann, and Stephens (2012) develop formal comparative statistics results to this effect for the Choo and Siow (2006)-model.

[4]We present empirical evidence that supports selective RoSLA responses, whereby those who—even after the reform—did not obtain any qualification were particularly negatively selected in terms of socio-economic backgrounds.

Our model thus features an unobserved characteristic, ability, and evolves around selection on ability into education, a theme with a long history in labor and education economics, see e.g. Willis and Rosen (1979). As is well known, selection on unobservables poses a identification problem, the solution to which requires variation in qualifications that is orthogonal to ability. RoSLA delivers this variation. In the labor and education economics literature, estimation of a "treatment effect" typically proceeds along the lines of a instrumental variable (IV) regression or within a regression discontinuity design (RDD, Lee and Lemieux, 2010). These approaches to estimation, however, invoke the Stable Unit Treatment Value Assumption (SUTVA)—the assumption that the treatment applied to one unit does not affect the outcomes of other units (Cox, 1958; Rubin, 1980). SUTVA is clearly violated in our context. Indeed, preferences over both academic qualifications and age-gaps implies that the marriage market prospects of the non-treated, pre-1957 cohorts are affected by the RoSLA-treatment applied to the later cohorts.

Evaluation of treatment effects with equilibrium adjustments can be handled by explicitly incorporating equilibrium formation in the empirical analysis (Heckman, LaLonde, and Smith, 1999). In the context of the marriage market, following the seminal work of Choo and Siow (2006), a number of recent papers has estimated marriage market equilibria, see e.g. Galichon and Salanié (2015), Mourifié and Siow (2015), Brandt, Siow, and Vogel (2016), Dupuy and Galichon (2014), Choo (2015), Chiappori, Costa Dias, and Meghir (2017), and Chiappori, Salanié, and Weiss (2017).[5] None of these studies, however, account for unobservable marriage market traits correlated with educational choices, such as ability. In this way, our paper bridges the IV and RDD approach that tackles the issue of selection on unobservables, but fails to take account of equilibrium effects, and the empirical marriage market literature that employs equilibrium models where academic qualification is a key marriage market trait, but fails to account for unobservables correlated with educational attainment. We structurally estimate the proposed marriage market model, allowing for selection into qualifications levels based on ability, using RoSLA-induced variation in the selection process, and find that the estimated model is largely consistent with observed marriage market behavior around RoSLA, as described above.

Of particular relevance to our study, Chiappori, Salanié, and Weiss (2017) provide a comprehensive empirical analysis of marriage market returns to education, extending the Choo and Siow approach to a multi-market framework, assigning successive US cohorts born between 1943 and 1972 to different marriage markets. This allows the authors to use changing proportions of men

[5] Galichon and Salanié (2015) allow for unobservable characteristics (but assume that these do not mutually interact in generating marital surplus) and for general distributions of idiosyncratic preferences. Brandt, Siow, and Vogel (2016) use an equilibrium marriage market model to analyze the marriage response to a large demographic shock—the Great Chinese Famine—and argue that the observed marital responses were consistent famine-born cohorts being not only of smaller size, but also of lower "quality". Mourifié and Siow (2015) allow for peer effects in marital choices. Dupuy and Galichon (2014) allow for continuous types when studying matching on personality traits. Choo (2015) extend the Choo-Siow framework to a dynamic programming setting in order to study the gains to marriage by age. Chiappori, Costa Dias, and Meghir (2017) who consider a three-stage lifecycle model of education, marriage and labor supply.

and women across education levels as exogenous variation in the supply of different education types in order to identify changes in assortative mating and in the marriage education premium by gender and across time. While their model is very rich, and includes parents' investment in the human capital of their offspring, Chiappori, Salanié, and Weiss (2017) do not account for unobservable personal characteristics correlated with education such as ability, non-cognitive skills, social background affecting choices and outcomes.

Indeed, following on from Chiappori, Iyigun, and Weiss (2009), Bruze (2015), and Chiappori, Salanié, and Weiss, 2017, our paper's third contribution is to decompose the model-implied marital returns to education into marital returns to ability and marital returns to qualifications. In our data, the raw marital return—not accounting for selection on ability—to obtaining a basic qualification over no qualification is positive and similar for men and women. The raw marital returns to obtaining a advanced qualification over a basic qualification is essentially zero for men, and negative for women. These results are broadly consistent with results reported in the aforementioned papers on the marital return to education. Moreover, in line with the findings in Chiappori, Salanié, and Weiss (2017), we find that the raw qualification returns are increasing over time for women, and stable for men. However, the raw returns to basic qualifications exhibit a discrete jump at the RoSLA reform threshold. When we decompose the raw returns into a return to ability and a return to qualification, quite a different picture emerges. For men, we find substantial positive returns to ability in the marriage market, but negative returns to qualifications. For women, there is a positive ability premium, while the basic qualification premium is essentially zero. Moreover, even though the total marital return to education—the return to ability plus the return to qualifications—is strongly increasing for women (less so for men), they do not exhibit discrete jumps at the RoSLA threshold.

Our fourth contribution is to conduct counterfactual simulations where RoSLA was not implemented, and show that the equilibrium effecta of the reform was by no means confined to cohorts and ability-levels directly affected by the reform. Indeed, the reform lowered the proportion ever-married among low ability individuals and increased it among high ability individuals, with these effects affecting men born even before the reform threshold.

Finally, our analysis is also related to an emerging literature dealing with identification of preference structures in matching games with transferrable utility in the presence of unobservables. Fox, Yang, and Hsu (2018), which to the best of our knowledge represents the current state of the art in this literature, derives identification results pertaining to the distribution of unobserved characteristics in two-sided matching games with transferrable utility. They consider one-to-one, many-to-one and many-to-many matching, and matching with trading networks. They do not, however, deal with selection on unobservables, and their results requires the researcher to observe many markets, a identification arrangement that does not apply to our case. We prove identification of our extended model, essentially employing the RDD logic to the RoSLA-reform, but within the context of an equilibrium marriage market model.

The rest of the paper proceeds as follows. In Section 2 we describe the data sources used. In Section 3 we describe the 1973 RoSLA reform, outline it's impact on academic qualifications and present evidence supporting the notion of a selective response. In Section 4 we describe the marriage market outcomes for a set of cohorts born around the RoSLA threshold. In Section 5 we outline the empirical model and the estimation approach. In Section 6 we present the model estimates and fit with the data. In Section 7 we consider ability and qualification marital premia while in Section 8 we use a counterfactual simulation to highlight how various cohorts and ability types were affected by the reform in terms of their marital outcomes. Section 9 concludes.

2. Data Sources

Our main analysis will combine data from the UK Labour Force Survey (LFS), Population Statistics from the Office for National Statistics, and the UK Censuses. Here we describe each source in turn and how we use it.

Our focus will be on academic cohorts. The UK academic year run from the 1st of September to the 31st of August in the following calendar year. We will thus refer to the 1957 academic cohort – which was the first to be affected by the RoSLA – as those individuals born between September 1957 through August 1958. Our focus will be on a set of academic cohorts born around the RoSLA threshold. Specifically, we will focus on 1953 to 1960 academic cohorts. For notational ease we will denote this set of academic cohorts by $C = \{1953, ..., 1960\}$ and we will refer interchangeably to an individual as "being from cohort c" and "being born in year c".

2.1. Labour Force Survey

The UK Labour Force Survey (LFS) is the largest regular household survey in the United Kingdom and is intended to be representative of the UK population. From 1983 to 1991, the LFS was annual and thereafter it has been quarterly.[6] The LFS contains information on year and month of birth for each household member and on relationships between household members. Detailed information on qualifications held by each person has been included since 1984. Hence we pool all individuals observed in the 1984 - 2014 LFS, born in the UK and resident in England and Wales and who are from some academic cohort $c \in C$.[7]

We use the LFS for two purposes. First, we use the full sample to highlight the impact of the RoSLA on the academic qualification rates. Second, we use all individuals observed as married to characterize the marriage patterns in terms of cohort and qualification profiles.[8]

[6] At this stage the LFS also became a "rotating panel" whereby each household remains in the survey for five quarters before being replaced. We use information provided by individuals in their first interview.

[7] We are not conditioning on age. This means that our sample will have an age range of 22 (for someone born in 1961 and observed in 1984) to 61 (for someone born in 1953 and observed in 2014).

[8] The LFS only provides information about the respondent's current marriage. Hence our analysis will be based

Table 1 provides descriptive statistics for full LFS sample by gender and marital status. 68 percent of the men and 70 percent of the observed women and are married at the time of the interview. The average age is 39 for both men and women.[9] The average cohort is close to 56.5 as the observed individuals are nearly uniformly drawn from the cohorts in C.

The delination of academic qualification levels will be described in further detail below, but, as noted in the introduction, twe will work with three ordered levels, $z \in Z = \{z_0, z_1, z_2\}$, representing no academic qualifications, a basic academic qualification (formally, O-level or CSE level qualification), and an advanced academic qualification, (formally, A-level or higher), respectively. For both males and females, the basic qualification is the most common academic attainment, followed by no academic qualification, and then by advanced qualifications.

Table 1: Descriptive Statistics for the Pooled LFS Sample of Individuals from Academic Cohorts 1953-1960.

	Males			**Females**		
Variable	**All**	**Single**	**Married**	**All**	**Single**	**Married**
Age in Years	38.86	38.05	39.25	38.99	39.44	38.80
	(9.11)	(9.56)	(8.86)	(9.12)	(9.41)	(8.98)
Ac. Cohort	56.60	56.93	56.44	56.60	56.86	56.49
	(2.29)	(2.28)	(2.28)	(2.30)	(2.30)	(2.29)
No Qual.	0.35	0.38	0.34	0.32	0.35	0.31
	(0.48)	(0.48)	(0.47)	(0.47)	(0.48)	(0.46)
CSE/O-lev.	0.38	0.36	0.39	0.43	0.40	0.45
	(0.49)	(0.48)	(0.49)	(0.49)	(0.49)	(0.50)
A-level+	0.27	0.26	0.27	0.24	0.25	0.24
	(0.44)	(0.44)	(0.44)	(0.43)	(0.43)	(0.43)
Obs.	147,878	47,832	100,046	156,549	47,059	109,490

Notes: The sample pools all individuals observed in the 1984-2014 UK Labour Force Surveys from academic cohorts 1953-1960 with non-missing information on age, qualification and marital status.

2.2. ONS Population Statistics and Census Data

We use birth statistics from the Office for National Statistics (ONS) to calculate academic cohort size by gender for England and Wales.[10] The UK experienced a baby boom that started in the mid-1950s and peaked in 1964. Hence, as a general characterization, the cohorts that we are

on the assumption that the marriage pattern – in terms of spousal characteristics – among currently observed marriages is representative of first marriages.

[9]We make no further use of the age variable as we instead use direct estimates at population-level of the fraction never-married by age 45 by gender, cohort and qualification as described below.

[10]We further apply gender-cohort mortality rates to calculate academic cohort size at age 25. The gender-mortality rates by age were obtained from the Office of National Statistics' principal projection of historic and projected mortality rates from the 2010-based UK Life Tables. Our focus on UK birth cohorts also means that are ignoring migration when calculating the relative populations supplies.

studying were increasing in size as illustrated in the left panel of Figure 1. As usual, there were more men than women born in any given cohort. However, due to the population growth, the gender ratio at the conventional 1-2 year husband-wife age gap (see below) is fluctuates quite close to unity as illustrated by the right panel of Figure 1.

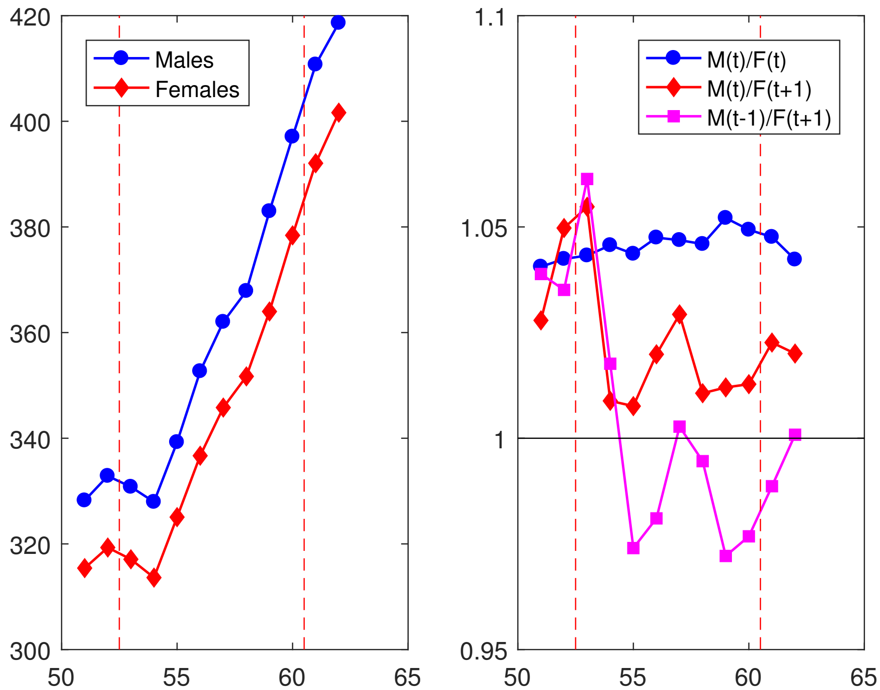

Figure 1: Cohort Sizes (thousands) by Gender and Gender Ratio at 0, 1, and 2 Year Husband-Wife Age Gap

We further commissioned tabulated data from the ONS based on the 2011 Census in order to characterize never-married rates, by gender, academic cohort and qualification level. As the census is fixed in time, we adjusted the tabulated data to account for first marriages occuring past the age of 45, leaving us with a measure of the proportion never-married by age 45 by gender, academic cohort and qualification level.[11] This measure will be illustrated and discussed in Section 4.

3. The 1973 Raising of the School Leaving Age

3.1. The Reform

In March 1972 the UK Government introduced Statutory Act 444, known as the Raising of School Leaving Age (RoSLA) Order, which came into operation on September 1st 1972. This

[11] First marriages past the age of 45 are rare, whereby the adjustments are very small. We use using information from ONS cohort tables on the proportions of never-married individuals by cohort over single years of age. As these ONS cohort tables do not contain qualification information the calculation assumes that the rate of entry into first marriage beyond age 45 is homogenous across qualification groups.

legislation mandated an increase to the compulsory schooling requirement, raising the minimum school leaving age by one year to age 16 and thus affecting those individuals born from 1st September 1957 onwards. A substantial fraction of the population were impacted by the RoSLA reform, with the proportion of individuals leaving education after 16 years of age increasing by around 25 percentage points in response to the new leaving age requirement (see e.g., Chevalier, Harmon, Walker, and Zhu, 2004; Silles, 2011; or Clark and Royer, 2013). The bite of the RoSLA reform was limited to those individuals at the lower end of the education distribution, with researchers routinely finding that the reform had no effect on the probability of leaving at ages 17 or above.

An key feature of the RoSLA education reform is that it impacted not only the duration of schooling, but also the likelihood of leaving school with an academic qualification. In England and Wales there are two levels of examinations sat during school. The first tier, leading to the Ordinary Level (O-Level) or Certificate of Secondary Education (CSE) qualifications, are not available until the end of the academic year in which an individual turns 16. After a further two years of study a second set of academic examinations, leading to the Advanced Level (A-Level) qualification, a pre-requisite of entry to Higher Education, can be taken. Therefore the increase in education mandated by RoSLA obliged students to remain in school including the year in which the first level of academic qualifications are conferred. At the RoSLA threshold there is a decrease in the proportion of individuals without academic credentials of around 10 percentage points, see Dickson, Gregg, and Robinson (2016) and Chevalier, Harmon, Walker, and Zhu (2004), with a slightly higher decrease for women as compared to men, see Grenet (2013). This decrease is accompanied by a corresponding increase in the proportion of individuals obtaining the first tier of academic qualifications (i.e. basic qualifications in the terminology of this paper), but with no ripple-upward effect on either the second tier of qualifications, see Chevalier, Harmon, Walker, and Zhu (2004) and Grenet (2013), nor on the proportion of individuals with a university degree, see Dickson and Smith (2011).

3.2. The Impact of the RoSLA on Qualifications

Figure 2 shows the qualifications distribution by academic cohort and gender as observed in our full LFS sample. In line with previous studies, we see a sharp drop in the rate of holding no qualification and a corresponding discontinuous increase in the proportion holding a basic qualification (i.e. O-level/CSE-level qualification). As has been frequently observed in the literature, the reform does not appear to have had any upward spillover on the rate of holding a advanced qualifications obtained through post-compulsory schooling (A-level or higher).

In Table 2 we provide the most basic regression discontinuity estimates of the impact of the RoSLA reform on the qualification distribution by gender. Specifically, for each qualification

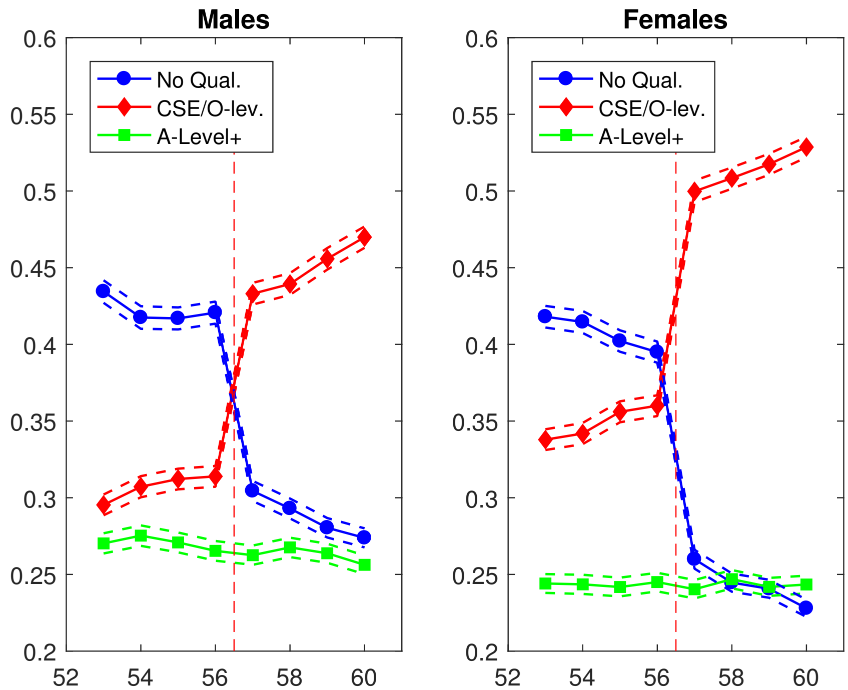

Figure 2: Distribution of Academic Qualifications by Cohort and Gender

level, we estimate a linear probability model of the form

$$y_i = \alpha + \beta_0 \delta_i + \beta_1 \delta_i R_i + \varphi R_i + \psi X_i + \varepsilon_i, \tag{1}$$

where the "running variable" δ_i is defined as the time of birth in months of individual i relative to the first affected month of birth cohort (September 1957), R_i is a dummy for the respondent being affected by the reform ($\delta_i \geq 0$), and X_i contains further demographic controls.

Using our full LFS sample Equation (1) is estimated, by gender, for each qualification level $z \in Z$, with y_i being a dummy for holding that specific qualification level, and both with and without demographic contols. The estimated parameter of interest is φ and Table 2 reports the estimated values of φ across the twelve regressions. Well in line with the literature, the result indicate that the RoSLA reduced the fraction holding no qualification by about 10 percent for men and by about 12 percent for women.[12]

3.3. Some Evidence of a Selective Response

The estimated responses to the RoSLA suggests that the reform reduced the proportion of women and men holding no academic qualification by about a quarter. This raises the question whether there was a systematic difference between those who responded to the reform by gaining

[12]Further analysis shows that the results are robust to the choice of bandwidth and the selection of control variables. Results are available on request from the authors.

Table 2: Regression Discontinuity Estimates of the Impact of the RoSLA on Qualification Rates by Gender

	Males		**Females**	
No Qual.	-0.099***	-0.106***	-0.120***	-0.128***
	(0.008)	(0.006)	(0.009)	(0.007)
CSE/O-lev.	0.094***	0.106***	0.119***	0.131***
	(0.008)	(0.005)	(0.010)	(0.007)
A-Level+	0.006	-0.000	0.001	-0.002
	(0.005)	(0.005)	(0.005)	(0.004)
Obs	147,878	147,878	156,549	156,549
Controls	No	Yes	No	Yes

Notes: The sample used in each regression is the same in Table 1. Each reported coefficient comes from a separate regression with dependent variable being a dummy for having that level of academic attainment. The table reports the estimated coefficient on a RoSLA dummy for being born September 1957 or later. Distance of date of birth from the September 1957 threshold measured in months is used as "running variable" and is included in linear form and interacted with the RoSLA dummy. The demographic controls include a third degree polynomial in age, month of birth dummies, and year of interview dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

a qualification and those after the reform still did not obtain any qualification. Here we present some evidence to suggest that this was indeed the case.

As the LFS does not contain much information about the respondents' longer-term social backgrounds, we turn to a different data set for this purpose. We will use the Health Survey for England (HSE) which is a representative survey of individuals living in England, with approximately 10,000 respondents each wave. The HSE has been running annually since 1991, however due to the availability of detailed qualifications information, we use data from 1998 to 2014. Respondents complete a core questionaire containing demographic, lifestyle and health-related questions which is further supplemented with physical measurements, including height, taken by a health-care professional. Each wave contains a supplemental module, focussing on a particular condition or disease. As part of the Cardio-Vascular module respondents are asked questions regarding their parental history, including whether each (natural) parent is alive. As there is a strong social gradient in mortility we will use the respondent's natural father being dead as a marker of adverse social background (Blane, Smith, and Bartley, 1990). Similarly, individual height is also widely regarded as a marker of childhood conditions since inadequate nutrition and childhood illness contribute to lower achieved height (Wadsworth, Hardy, Paul, Marshall, and Cole, 2002).

Individual height is available in every HSE survey. In contrast, the Cardio-Vascular module module is available in the 1998, 2003, and 2006 surveys. In order to obtain larger sample sizes, we expand our focus and include in our analysis all individuals born in the academic cohorts 1947 to 1966.

The HSE was recently used by Clark and Royer (2013) to estimate, using an RD design, the effect of education on health outcomes using the same educational reform that we focus on here, along with an earlier one which raised the school-leaving age from 14 to 15. Their main finding was that there was little or no effect of qualifications gain through these reforms on health outcomes. The outcomes that we are focusing on – own height and father's mortality – are highly unlikely to have been affected by the RoSLA, and we confirm this below.

Our interest here, however, is different as our aim is to highlight a selective response to the reform. In particular, we show that, prior to the reform, unqualified individuals had worse outcomes along both dimensions, being both shorter than qualified individuals and being more likely to report their fathers being dead. We further show that, at the reform threshold, the outcome gaps between qualified and unqualified individuals widened. The evidence is consistent with a selective response to the RoSLA whereby those who responded to the reform – the "compliers" – were positively selected on social background among those who would fail to obain any qualification in the pre-reform regime. Conversely, the evidence suggests that those who even after the reform did not obtain any qualification – the "never-takers" – were increasingly negatively selected based on social background.

The results are provided in Table 3.[13] In the first column we estimate a regression in the form of equation (1) with y_i being a dummy for holding some academic qualification and where we pool male and female respondents and include a gender dummy as demographic control X_i. The coeffient on being RoSLA affected indicates an 8.3 percentage point increase the rate of holding some qualification. This is slightly below the response estimated in the LFS data above, but broadly speaking consistent.

In columns 2 and 4 we then replace the outcome variable with own height (measured in centimeters) and an indicator for the respondent's father being dead respectively. The estimates in column 2 shows that the average height was 172 among men and 162 among women, and that the RoSLA predictably did not affect average height. Similarly, as shown in column 4, about half of the respondents reported their fathers to be dead at the time of the interview, with only a minor difference between male and female respondents, and again with no suggested effect of the RoSLA.

In columns 3 and 5 we then extend equation (1) to include an indicator for holding no academic qualification and the same indicator interacted with the RoSLA dummy,

$$y_i = \alpha + \beta_0 \delta_i + \beta_1 \delta_i R_i + \varphi R_i + \rho_1 I_i^{z=0} + \rho_2 I_i^{z=0} R_i + \psi X_i + \varepsilon_i, \tag{2}$$

The coefficients in column 3 then suggest that, among individuals born prior to the RoSLA,

[13] A more detailed RD analysis demonstrating the robustness of the results presented here are available on request from the authors.

Table 3: Regression Discontinuity Estimate of the Impact of the RoSLA on the Academic Qualification Rate and of the Relationship Between Holding an Academic Qualification and Own Height and Father's Mortality Based on Data from the Health Survey for England

	Ac. Qual.	**Own Height**		**Father Dead**	
Constant	0.658***	175.32***	176.01***	0.494***	0.468***
	(0.010)	(0.08)	(0.09)	(0.013)	(0.013)
Gender (female)	0.023***	-13.27***	-13.32***	-0.019*	-0.017*
	(0.004)	(0.06)	(0.06)	(0.008)	(0.008)
RoSLA ($\geq$ 57)	0.083***	0.05	-0.02	-0.007	-0.011
	(0.012)	(0.11)	(0.11)	(0.015)	(0.0157)
No Qual.			-2.12***		0.082***
			(0.08)		(0.010)
No Qual * RoSLA			-0.40**		0.037*
			(0.13)		(0.015)
Obs.	58,456	53,947	53,947	15,661	15,661

Notes: The sample pools all individuals from academic cohorts 1947-1966 with information about academic qualifications observed in the Health Survey for England, 1998-2014. The running variable splits each academic year into three periods (Sept-Dec, Jan-April, May-Aug) and is centred on the first RoSLA treated group (born Sept-Dec, 1957). Each regressions includes the running variable and its interaction with the RoSLA indicator. Information on father's mortality is available only in the 1998, 2003 and 2006 surveys. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the difference in height between qualified and unqualified was little over 2 centimeters on average. At the RoSLA, this gap increased by 0.4 centimeters, an 20 percent increase over the pre-reform gap. Similarly, column 5 tells us that, among individuals born prior to the RoSLA, the proportion reporting that their fathers are dead was 8 percentage points higher among unqualified than among qualified individuals. Moreover, among individuals born after the reform threshold, this gap unqualified and qualified increased by 3.7 percentage points, an increase of more than 40 percent over the pre-reform gap.

In short, the evidence presented here strongly suggests a selective response to the RoSLA whereby those who even after the reform did not obtain any qualification were particularly negatively selected in terms of social background.

4. Marriage Market Outcomes

Before we turn to the empirical marriage market modelling we will begin by highlighting how assortative mating on qualifications and positive age husband-wife age gaps hold in the current data. Moreover, we will highlight any observable impact – temporary or otherwise – of the

RoSLA on the the marriage outcomes by cohort and gender.

4.1. Assortative Mating on Qualifications

We start by highlighting the usual feature of positive assortative mating on qualifications. Figure 3 uses the LFS sample of married individuals. The left panel thus uses all married males born between 1953 and 1960 and shows the distribution of their wives' qualification level by the male's own qualification level. The right panel provides the corresponding distribution of husbands' qualifications by the wife's own qualification for the sample of married women born 1953 to 1960. The high degree of assortative mating is highlighted by the fact that, for each gender and qualification level, the spouse having the same qualification level is the most frequent category.

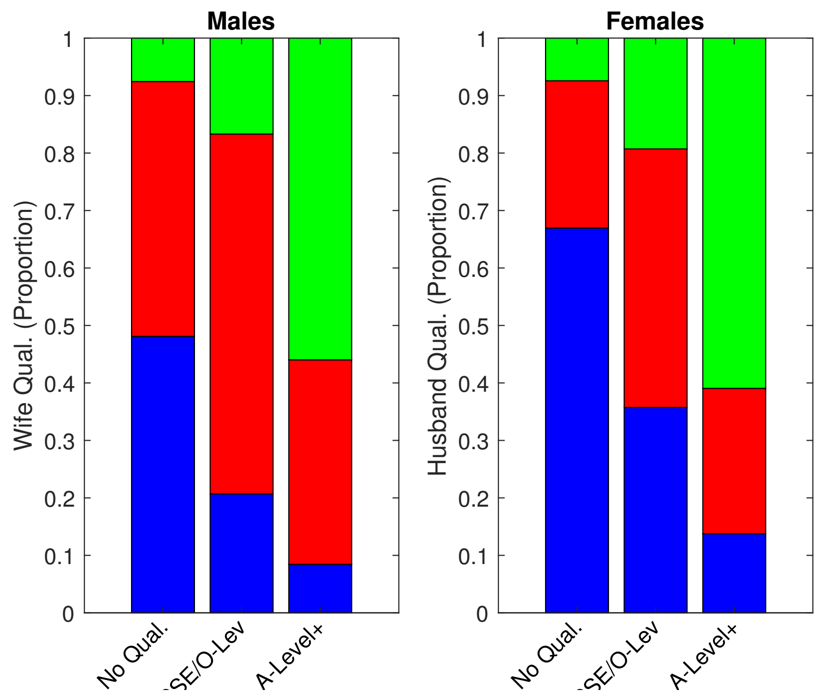

Figure 3: Assortative Matching on Qualifications by Gender

A perhaps more interesting question is whether the RoSLA affected the degree of assortative mating on qualification. As a first attempt at answering this question, we can split the sample into those born before and after the RoSLA 1957 threshold. The first column of Table 4 uses the subsample of married men born between 1953 and 1956 while the second column uses the married men born between 1957 and 1960. For each subgroup, the table reports the Goodman-Kruskal gamma measure of the rank correlation.[14] The third and the fourth columns do the same for the subsamples of married women. While the table confirms the high rank correlation

[14]The Goodman-Kruskal gamma measure of the rank correlation is preferred to the Spearman rho and the Kendall tau when the variables in question are ordered categorical and there are many ties as a consequence.

in spouses' qualifications, it does not provide any conclusive evidence that the reform affected the degree of assortative mating on qualifications.

Table 4: Goodman-Kruskall Rank Correlation in Spouses' Academic Qualification Levels

	Males		**Females**	
	Pre-Reform (1953-56)	**Post-Reform (1957-60)**	**Pre-Reform (1953-56)**	**Post-Reform (1957-60)**
	0.628***	0.633***	0.638***	0.637***
	(0.004)	(0.005)	(0.004)	(0.005)
Obs.	47,419	45,448	50,766	50,057

Notes: The overall sample all includes married couples observed in the UK Labour Force Survey 1984-2014 with available information on the academic qualification for both spouses. Each column conditions on the husband or the wife being born in the stated set of academic cohorts. Spouses can be drawn from any cohort. The rank correlation measure provided is the Goodman-Kruskal gamma. Asymptotic standard errors are in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Closer inspection however shows that the aggregate rank correlation masks heterogeneous impacts at the various qualification levels. In order to highlight this, consider the following simple measure of sorting at qualification level z that can be applied in any population of married couples,

$$S(z) = \frac{\Pr(z_m = z, z_f = z)}{\Pr(z_m = z)\Pr(z_f = z)}. \tag{3}$$

The numerator is the probability that, for a randomly drawn couple, both spouses have qualification level z. The denominator is the product of the probabilities of the husband and the wife having qualification level z respectively. If matches were randomly generated, the joint probability would be equal the product of the marginal probabilities, whereby $S(z)$ would be equal to unity. A value of $S(z)$ above unity thus indicates positive sorting at qualification level z. The advantage of the measure $S(z)$ is that it can be applied for each qualification level separately.[15] Hence we use again the LFS sample of married individuals and compute $S(z)$ for each qualification level. Moreover, we do this by gender and cohort. Specifically, in left panel of Figure 4, we plot $S(z)$ for each qualification level $z \in Z$ by the academic cohort $c \in C$ of the husband.[16] The right panel does the same using the sample of married women.

The figure shows that the strongest assortative mating occurs among those holding an advanced qualification, a pattern that is stable over the cohorts of interest. What is more interesting for our purposes is what happened to the assortative mating among individuals with no qualifications or holding a basic qualification. The figure highlights a clear increase in the degree

[15]Indeed, the measure could be applied for any given husband-wife qualification profile. However, our interest here is to explore whether the tendency for married couples to have the same qualification level was strengthened by the RoSLA and, if so, for what qualification level this happened.

[16]For the subsample of married couples where the husband is from cohort $c \in C$ the wife can be from any cohort, including cohorts not in C.

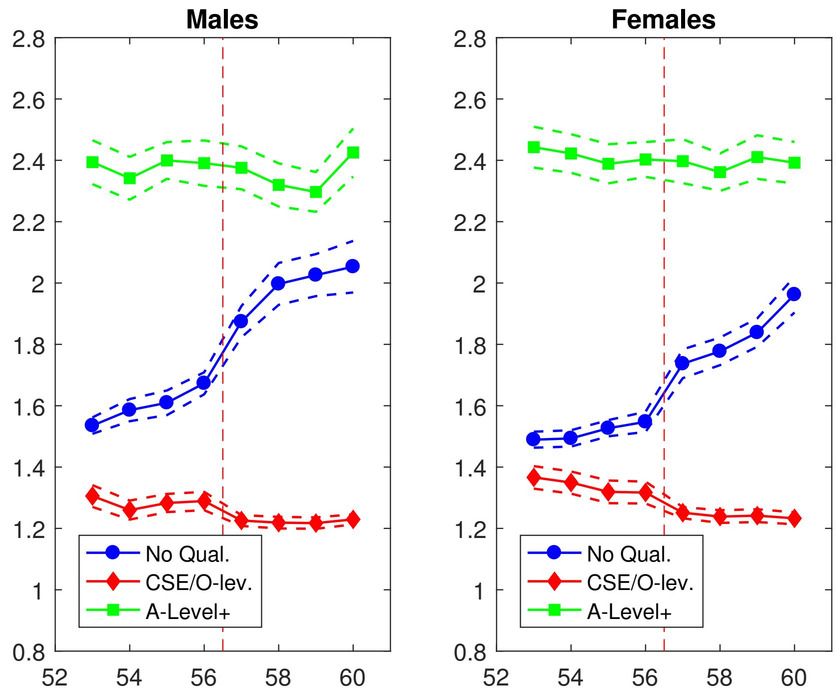

Figure 4: Assortative Matching by Cohort, Gender, and Qualification Level

of assortative mating among those holding no academic qualification after the RoSLA, both for men and for women. Moreover, this change was not just temporary but appears to have been a permanent increase. Hence, after the RoSLA, unqualified men and women became increasingly prone to marry each other. In contrast, the degree of assortative mating among those with a basic academic qualification reduced slightly after the reform.

4.2. Never-Married Rates

Figure 5 shows the never-married rate (by age 45) by gender and level of qualification. The most striking feature is how the never-married rate for unqualified individuals increased at RoSLA threshold. Indeed, for women the first affected academic cohort marks a key turning point. Whereas traditionally, the most qualified women would have been the least likely to marry in their lives, the first RoSLA affected cohort is also the first for which unqualified women were the least likely to ever marry. Among men, the unqualified were already the group least likely to marry in their lives, but at the reform threshold, the gap in the never-married rates for unqualified and qualified rose distinctly.

4.3. Age Gaps

For our purposes, we define the husband-wife age gap, denoted here by d, as the difference in their academic cohorts. Figure 6 shows the aggregate husband-wife age gap distribution in

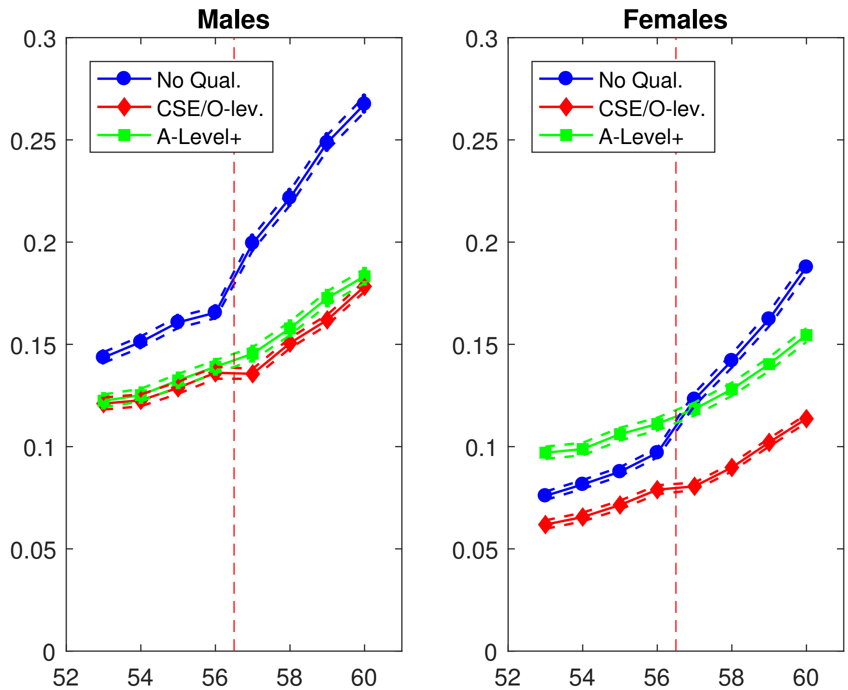

Figure 5: Never-Married (by age 45) Rates by Cohort, Gender, and Academic Qualification Level

the academic cohorts of interest.[17] The figure shows that age gaps of 0, 1 and 2 are the most common. There is a sharp drop in frequency when moving to negative age gaps, but fat right tail for positive age gaps.

The left panel of Figure 7 shows the distribution of husband-wife age gaps for married men born in some $c \in C$, while the right panel does the same for the married women.[18] The figure suggests that qualified individuals are slightly more likely to be married with a low ($d \leq 0$) husband-wife age gap while unqualified individuals are slightly more likely to be married with a large ($d \geq 4$) age gap. These patterns are reminiscent of findings presented by Mansour and McKinnish (2014).

Is there any evidence that the age gap distribution was affected by the RoSLA? To explore this, we consider how the age gap distribution differered in the key cohorts around the reform threshold from the aggregate one. Let y_i^d be a dummy indicator for (married) individual i being married with husband-wife age gap d. For every age gap $d \in \{-3, +3\}$ and for each cohort $c \in C$ we regress y_i^d a cohort-dummy for being from cohort c. This way we determine, for each d, whether individuals from cohort c had a different likelihood of being married with this

[17] The sample used to construct Figure 5 thus includes all married couples observed in the LFS 1984-2014 where at least one spouse is from an academic cohort $t \in T$. The same underlying sample is used also in Figures 7 and 8 but with indicated conditioning.

[18] There are two reasons why the two panels are not identical. First, while marriages are assortative on qualifications they are not perfectly so. Second, the figure does not restrict the spouse to be born in the cohorts of interest as that would have biased the shape of the empirical age gap distribution.

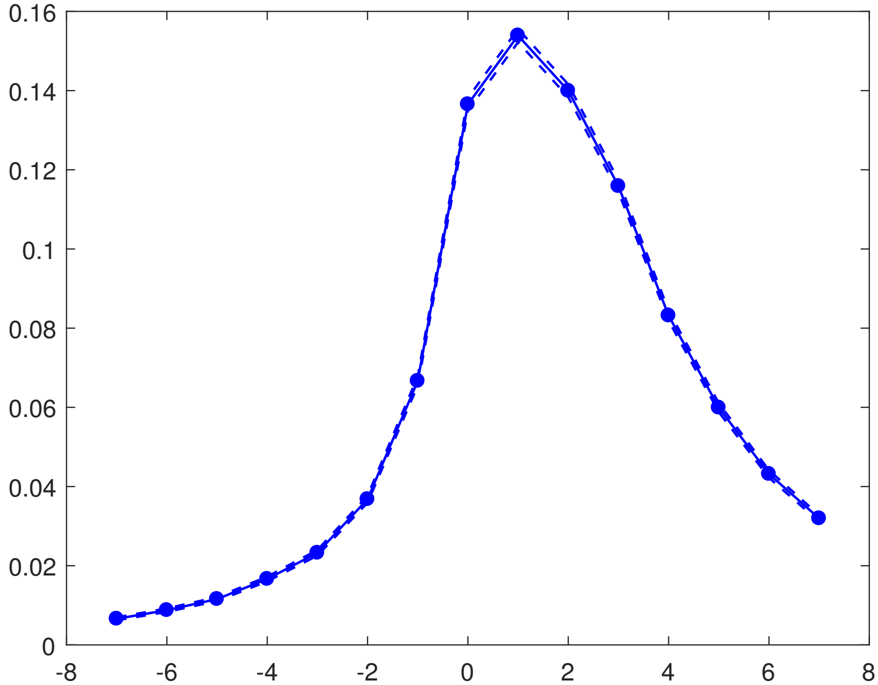

Figure 6: The Aggregate Husband-Wife Age Gap Distribution

particular age gap compared to individuals from all other cohorts in C.

The top row of Figure 8 shows the results for men in cohorts 1956 to 1958. In each figure a vertical line has been added; for the age gaps to the right (left) of this line, the wife is from a post-reform (pre-reform) cohort.[19] The only male cohort with statistically significant different age gap frequencies is the 1957 cohort – the first RoSLA affected cohort – who were about one percentage point more likely to be married with an age gap of either 0 or +1. Conversely, they were less likely to be married with a negative age gap.

The bottom row of Figure 8 shows the corresponding results for women. Consistent with the findings for men, the most notable deviations are for early post-reform women – born in the 1957 and 1958 academic cohorts – who were about one percentage point more likely to marry with an age gap of 0 and +1 respectively (thus both marrying 1957 cohort men). The evidence here thus suggest that the RoSLA temporarily – but rather modestly – shifted the age gap distribution, with the early RoSLA-affected men and women more frequently choosing to marry each other.

5. Model

The basic workhorse model that we will draw upon is the standard transferable utility marriage market model of Choo and Siow (2006). Here, men and women fall into discrete types and the systematic marital surplus varies with a couple's type-profile. In addition, following CS,

[19]Note that the vertical line shifts as the location of the own cohort relative to the reform threshold shifts.

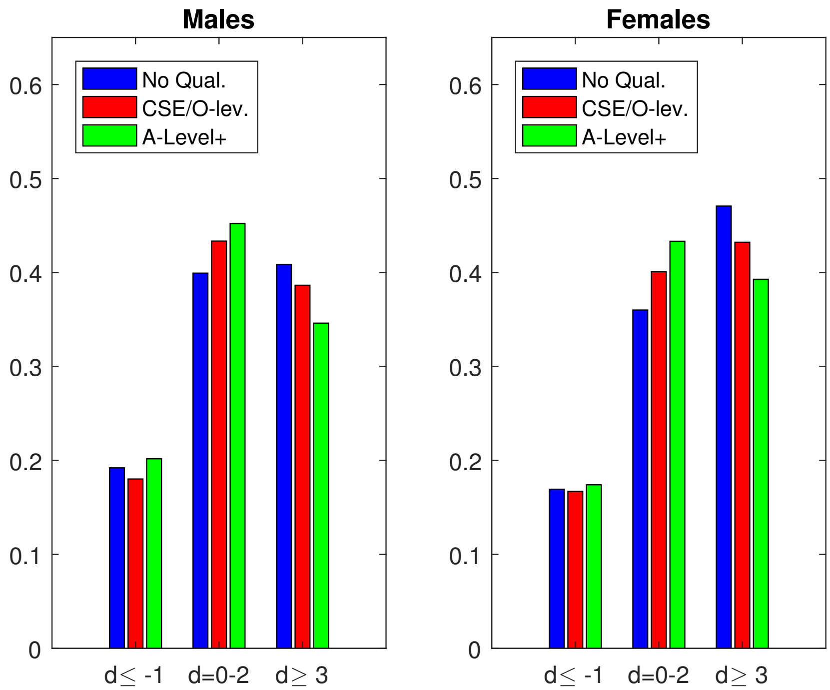

Figure 7: Age Gap Distribution by Gender and Qualification

individuals have idiosyncratic preferences over partner types of the logistic type. The Choo-Siow framework has been further developed by *inter alia* Chiappori, Salanié, and Weiss (2017) and by Galichon and Salanié (2015). Galichon and Salanié in particular advocate imposing restrictions in the marital surplus matrix that can be used for hypothesis testing and for identifying the stochastic structure.

We depart from this literature in one central aspect: we assume that one personal characteristic – which we will refer to as "ability" – while observable to all individuals in the marriage market is unobserved by the researcher. Hence while the marriage market attains an equilibrium based on all three personal characteristics – cohort, ability, qualification – as researchers we only observe an aggregration of this equilibrium over ability. Identification of the model is achieved by a combination of two key assumptions. First, we assume that the impact of the RoSLA on qualifications was monotonic with a particularly simple, but arguably natural, relationship between ability and qualifications in the pre- and post-reform regimes. Second, we assume that the marital surplus function is additively separable between a couple's age gap on the one hand and their qualification-ability profile on the other hand. Before turning to a discussion of identification, we will start by outlining the matching model that will estimate.

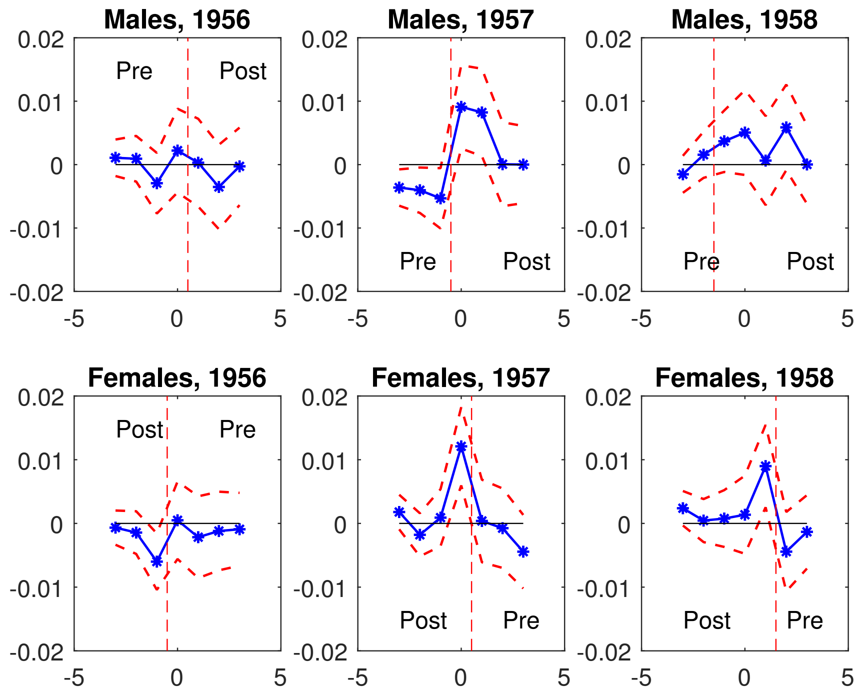

Figure 8: Deviations from the Aggregate Age Gap Distribution by Cohort and Gender

5.1. Stable Matching with Discrete Types

Consider a population of men $i \in I$ and women $j \in J$. A *matching* consists of (i) a matrix $\mu = (\mu_{ij})$ such that $\mu_{ij} = 1$ if i and j are married and zero otherwise, and indicators μ_{i0} and μ_{0j} which are unity if i and/or j are unmarried respectively and zero otherwise, and (ii) a set of utilities (or "payoffs") $\{u_i\}_{i\in I}$ and $\{v_j\}_{j\in J}$. A match between i and j allows them to share total utility, denoted σ_{ij}, and the division of this utility is achieved through transfers determined in equilibrium. Unmarried individuals get utilities σ_{i0} and σ_{0j} respectively. A matching is *stable* if there exists a division of the utility in each realized match such that no man i and woman j can both achieve strictly higher utility by pairing up together, and no married individual can achieve a higher utility by instead choosing to unmarried. Formally stability requires that (i) $u_i + v_j \geq \sigma_{ij}$ for all $(i,j) \in I \times J$, and (ii) $u_i \geq \sigma_{i0}$ and $v_j \geq \sigma_{0j}$ for every $i \in I$ and $j \in J$.

Individuals fall into different "types", with a finite types-space, denoted X. The type of man i is denoted x_i and the type of woman j is denoted x_j. There is an infinite number of men and women of any given type $x \in X$ and we let $h^m(x)$ and $h^f(x)$ denote the mass of men and women of type x respectively. As the equilibrium will be invariant to scale, we can assume that the total mass is unity, $\sum_{x\in X} \left[h^m(x) + h^f(x)\right] = 1$.

Individuals have unobserved heterogeneity in tastes. Man $i \in I$ has a random utility component $\varepsilon_i(x_j)$ associated with marrying a woman of type $x_j \in X$ and a random utility component $\varepsilon_i(0)$ associated with remaining unmarried. Similarly, woman j has a random utility component

$\varepsilon_j(x_i)$ associated with marrying a male of type $x_i \in X$ and $\varepsilon_j(0)$ associated with remaining unmarried. Following Choo and Siow (2006) we assume that all random utility components are i.i.d. across individuals and types as standard type I extreme value.

The non-random total utility available to a couple depends on their type-profile. In particular, we assume there exists a mapping $\Sigma : X \times X \to \mathbb{R}$ such that

$$\sigma_{ij} = \Sigma(x_i, x_j) + \varepsilon_i(x_j) + \varepsilon_j(x_i), \tag{4}$$

and we normalize the systematic utility from remaining unmarried to zero, $\sigma_{i0} = 0$ and $\sigma_{0j} = 0$ for all $i \in I$ and $j \in J$. It can then be shown (see Chiappori, Salanié, and Weiss, 2017) that, in any stable matching equilbrium, there exists two mappings $U : X \times X \cup \{0\} \to \mathbb{R}$ and $V : X \cup \{0\} \times X \to \mathbb{R}$ such that $U(x_i, x_j) + V(x_i, x_j) = \Sigma(x_i, x_j)$ for any $(x_i, x_j) \in X \times X$, and $U(x_i, 0) = 0$ and $V(0, x_j) = 0$, and (i) man i of type x_i achieves utility $u_i = \max_{x \in X \cup \{0\}} \{U(x_i, x) + \varepsilon_i(x)\}$ and makes the choice that attains the maximum, and (ii) woman j of type x_j achieves utility $v_j = \max_{x \in X \cup \{0\}} \{V(x, x_j) + \varepsilon_j(x)\}$ and correspondingly makes the choice that attains the maximum.

Let $\mu^m_{x_j|x_i}$ denote the probability that a man of type x_i marries a woman of type x_j, and, correspondingly let $\mu^f_{x_i|x_j}$ denote the probability that a woman of type x_j marries a man of type x_i. Then under the assumed extreme value distribution on the random utility terms, it follows that

$$\ln\left(\frac{\mu^m_{x_j|x_i}}{\mu^m_{0|x_i}}\right) = U(x_i, x_j), \text{ and } \ln\left(\frac{\mu^f_{x_i|x_j}}{\mu^f_{0|x_j}}\right) = V(x_i, x_j), \tag{5}$$

where $\mu^m_{0|x_i}$ is the probability that a man of type x_i remains unmarried and $\mu^f_{0|x_j}$ is the same for a woman of type x_j. In equilibrium, $h^m(x_i)\mu^m_{x_j|x_i} = h^f(x_j)\mu^f_{x_i|x_j}$. Using this to substitute for $\mu^f_{x_i|x_j}$ and adding the equations in (5) gives that

$$\mu^m_{x_j|x_i} = \sqrt{\mu^m_{0|x_i}\mu^f_{0|x_j}}\sqrt{\frac{h^f(x_j)}{h^m(x_i)}} \exp\left[\frac{\Sigma(x_i, x_j)}{2}\right]. \tag{6}$$

This is the familiar matching equation developed by Choo and Siow (2006).[20] In addition, the equilibrium satisfies the adding-up conditions,

$$\sum_{x_j \in X \cup \{0\}} \mu^m_{x_j|x_i} = 1 \text{ and } \sum_{x_i \in X \cup \{0\}} \mu^f_{x_i|x_j} = 1. \tag{7}$$

It was recently demonstrated by Decker, Lieb, McCann, and Stephens (2012) that, for a given population distribution and given surplus function $\Sigma(x_i, x_j)$, the marriage market equilibrium

[20]The only difference between equation (6) and equation (11) in Choo and Siow (2006) is that we have chosen to express the equilibrium marital choices in terms of frequencies rather than counts.

is known to exist and be unique. Moreover, the authors demonstrate that the equilibrium exhibits some natural comparative statics properties, most notably with respect to the population distribution. Consider e.g. an decrease in the supply of males of type x_i. As $h^m(x_i)$ decreases men of this type will obtain a larger share of the marital surplus from a marriage to any type of woman; as a result men of type x_i should become more likely to marry relative to remaining unmarried, that is $\mu^m_{0|x_i}$ should decrease.

5.2. Empirical Implementation

In our empirical setting, an individual's type x has three dimensions. First, by her date of birth, an individual belongs to an academic cohort $c \in C$. Second, she is of some ability level $a \in A = \{a_0, a_1, a_2\}$. Finally, she holds some academic qualification level, $z \in Z$. Hence an individual's type is a triple

$$x = (c, a, z) \in X = C \times A \times Z, \tag{8}$$

though not all combinations will exist under our framework (see below).

For notational simplicity, we will use R_0 and R_1 to denote the pre- and post-RoSLA cohorts or "regimes" respectively. The ability space A is assumed to have three ordered elements which we will refer to as "low", "medium" and "high" ability respectively. The mapping from ability to qualification is assumed to be (weakly) monotonic, but discontinuous at the reform threshold. In particular, since in the post-RoSLA regime R_1 all individuals were required to remain in school until the age of qualifying exams, we assume that there was perfect sorting into qualifications by ability.

In the pre-RoSLA regime R_0, we also assume that low and high ability individuals remained unqualified and obtained a high qualification respectively. However, as individuals could leave school a year before the qualifying exams, there was in under this regime a foregone earnings cost associated with obtaining a basic (O-level/CSE) qualification. Hence we assume that only a positive fraction of the medium ability individuals acquired this basic academic qualification. For reference we write this down as a formal assumption and illustrate it in Figure 9.

Assumption 1. If $c \in R_1$ and $a = a_k$ then $z = z_k$ for $k \in \{0, 1, 2\}$. If $c \in R_0$ and $a = a_k$, then $z = z_k$ for $k \in \{0, 2\}$, but $\Pr(z = z_1 | a = a_1, c \in R_0) = \gamma^g$ and $\Pr(z = z_0 | a = a_1, c \in R_0) = 1 - \gamma^g$, where γ^g is gender-specific, $g = m, f$.

Crucially—and building on the logic of the RD literature—we assume that the distribution of the unobserved ability a did not change discontinuously at the RoSLA threshold. This assumption is justified here on the grounds that ability—as relevant to probability of success in obtaining an academic qualification—would likely reflect family background and/or individual academic skills developed through lower levels of schooling. Whilst the distribution of ability may change over time – for instance through improvement in quality of primary- and lower level

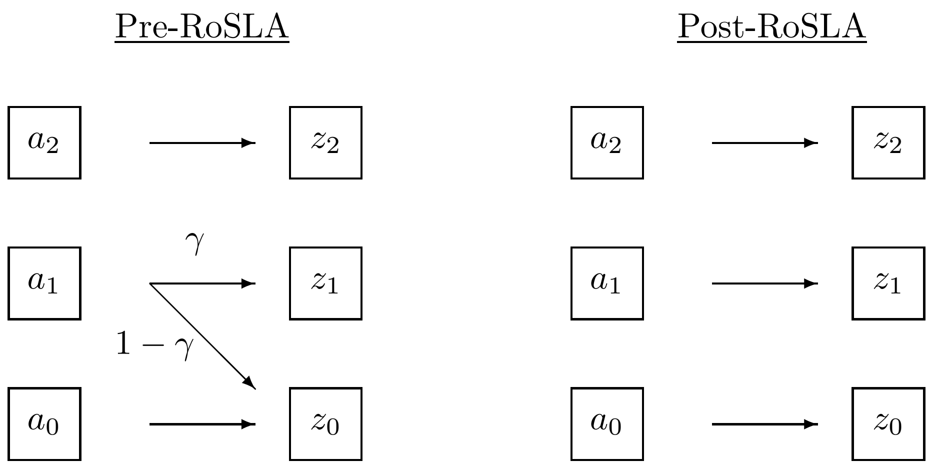

Figure 9: Assumed Relation between Ability and Qualification by Education Regime

secondary schooling at the national level – there is no reason to expect a discontinuity in this process at the 1957 cohort. Assumption 1 is then also naturally consistent with a widening of the gap in ability/social background between qualified and unqualified individuals at the RoSLA threshold as suggested by the evidence put foward in Section 3.3.

We obtain estimates of γ^g by gender from the points estimates of the increase in the rate of holding a basic qualification (i.e. CSE/O-level qualification) at the RoSLA threshold. The point estimate for men is 0.755 while for women it is 0.735. This would suggest that, about a quarter of those who had the ability to obtain a basic academic qualification did not do so in the pre-RoSLA regime. We will in the following take these values as given and base our estimation on it.[21]

Given Assumption 1 and given γ^g, the distribution of full types, $h^g(x)$, can be backed out for each gender from the population counts by cohort and the qualification distribution and cohort. By Assumption 1, only four individual ability-qualification profiles exist

$$H \equiv \{(a_0, z_0), (a_1, z_0), (a_1, z_1), (a_2, z_2)\}. \tag{9}$$

Within this set, there is variation in ability among the unqualified, and there is variation in qualification among medium ability individuals. The distributions of full types among men and women, $h^m(x)$ and $h^f(x)$, is the first component determining the marriage market equilibrium.

[21]Specifically, γ^g is estimated by (one minus) the increase in the proportion holding a z_1 qualification at the RoSLA over the proportion holding this qualification just after the RoSLA. We thus use the estimate of the proportion holding a basic qualification (CSE/O-level qualification) at the threshold from Table 2 along with the rate of this level of qualification in the 1957 cohort to form the estimator. We use bootstrapping to for 95 percent confidence intervals; these are about ± 0.02 for each gender. Hence, as robustness we have re-estimated our model for values of each γ^g between 0.715 and 0.775. Doing so affects the point estimates of the elements of the marital surplus matrix, but does not qualitatively affect the conclusions. Details are available on request. It should be noted that the discontinuity in the qualification distribution at the RoSLA threshold identifies γ^g at that point. We are thus implicitly assuming that γ^g can be taken as fixed in the cohorts 1953-1956 cohorts.

The second component of the marriage market equilibrium is the marital surplus function $\Sigma(x_i, x_j)$ relating couples' type-profiles to their systematic marital surplus. For this, we adopt – as noted above – an additively separable specification, with one component that depends on the couple's ability-qualification profile, $\zeta : H \times H \to \mathbb{R}$ and a second component that depends on their cohorts and abilities, $\Lambda : C \times C \times A \times A \to \mathbb{R}$, whereby

$$\Sigma(x_i, x_j) = \zeta(a_i, z_i, a_j, z_j) + \Lambda(c_i, c_j; a_i, a_j). \tag{10}$$

Since $|H| = 4$, $\zeta(\cdot)$ can be represented as a 4×4 matrix.

For identification purposes, we will restrict $\Lambda(\cdot)$ to depend additively on (i) a flexible function of the husband-wife age gap $d_{ij} \equiv c_j - c_i$, and (ii) and piece-wise linear trends by gender and ability. Hence we specify

$$\Lambda(c_i, c_j; a_i, a_j) = \lambda(c_j - c_i) + \tau^m(c_i; a_i) + \tau^f(c_j; a_j), \tag{11}$$

where, specifically,

$$\lambda(d_{ij}) = \sum_{d \in \{-3,-2,-1,1,2,3\}} \beta^d I_{d_{ij}=d} + \left(\beta_0^- + \beta_1^- d_{ij}\right) I_{d_{ij} \leq -4} + \left(\beta_0^+ + \beta_1^+ d_{ij}\right) I_{d_{ij} \geq 4}, \tag{12}$$

and

$$\tau^g(c; a) = \sum_{a \in A} \left[\beta_a^g (c - 53) I_a + \beta_{a,R_1}^g (c - 56) I_a I_{c \geq 57}\right], \quad g = m, f. \tag{13}$$

A few things are worth noting about the specification of $\Lambda(\cdot)$. First, for the age gap function, note that a zero age gap is used as base category with $\lambda(0) = 0$. $\lambda(\cdot)$ is then fully non-parametric on the central age gaps -3 to $+3$, but for parsimony uses linear representations for larger age gaps. Overall, the specified $\lambda(\cdot)$ function has ten estimated parameters (compared to the 15 possible age gaps observed in the data). Second, β_a^g is the overall trend for gender g and ability level a, whereas β_{a,R_1}^g is the additional trend that applies to this group after the reform. Allowing for slope changes is standard in the regression discontinuity literature in order to avoid biasing the characterization of the relevant discontinuity.

Critically, the marriage surplus function which represents the fundamental preferences over marriage are assumed to be stable with the possible exception of the piecewise but continuous linear trends. This is tightly tied to our methodological approach and associated identification strategy. We want to use the discontinuous change in the supply of types generated by the reform, and the associated observable changes to marital outcomes, to identify preferences that are assumed to be stable.

5.3. Identification and Estimation

Since the RoSLA reform induced a marked change in the qualification distribution a natural approach would be to consider individuals belonging to the pre- and post-RoSLA regimes to also belong to separate marriage markets and to estimate the marriage surplus function—assumed to be stable across regimes/markets—using a multi-market approach in the spirit of Chiappori, Salanié, and Weiss (2017). However, it is easy to see that such an approach, even under assumption 1, could not identify the full $\zeta(\cdot)$ function, including the role of unobserved ability.[22]

The multi-market approach can, however, be used for a diagnostic test of whether unobserved ability actually matters for marital surplus. To see this, note that if only qualifications matter—not ability—$\zeta(\cdot)$ reduces to a 3×3 matrix, $\zeta(z_i, z_j)$ (slightly abusing the notation). Moreover, as qualifications are readily observable, a basic Choo-Siow approach is available. Estimating the reduced $\zeta(\cdot)$ matrix on pre- and post-reform marriage data respectively should then generate estimates that are not statistically different from each other. Stated differently, marital outcomes – in terms of the relative frequencies of husband-wife qualification profiles and never-married rates – should change from the pre- to the post-regime in response to the change in the supply of qualification types, but they should do so in a way that is compatible with a stable marital surplus function.

Panels A and B of Table 5 presents the results from estimating, using maximum likelihood, the reduced marital surplus matrix using pre- and post-RoSLA data respectively.

Table 5: Estimates of Marital Surplus by Qualification Profile in the Pre- and Post-RoSLA Regime Based on a "Before-After" Model without Unobserved Ability

Panel A: Pre-RoSLA Cohorts (1953-1956)				**Panel B: Post-RoSLA Cohorts (1957-1960)**			
Females / Males	No Qualification	CSE/ O-Level	A-Level or higher	Females / Males	No Qualification	CSE/ O-Level	A-Level or higher
No Qual.	2.828	2.104	-1.106	No Qual.	1.577	1.617	-2.141
	(0.035)	*(0.046)*	*(0.063)*		*(0.038)*	*(0.039)*	*(0.071)*
CSE/O-Level	1.553	3.054	0.758	CSE/O-Level	0.802	3.514	0.928
	(0.043)	*(0.048)*	*(0.055)*		*(0.042)*	*(0.036)*	*(0.047)*
A-Level+	-0.885	1.807	3.260	A-Level+	-2.623	1.206	2.775
	(0.059)	*(0.053)*	*(0.048)*		*(0.074)*	*(0.043)*	*(0.044)*

Notes: The pre-RoSLA (alt. post-RoSLA) estimation uses all married couples observed in the LFS 1984-2014 and both spouses born between 1953-1956 (alt. 1957-1960) to characterize marriage frequencies by husband-wife qualification profile. Qualifications rates by gender and regime are computed using the full LFS sample of individuals born 1953-56 and 1957-60 respectively. The never-married rates by gender and qualification level uses the census data for the 1955-56 and 1957-58 cohorts respectively.

The estimates from the two regimes share some basic features: consistent with assortative mating on qualifications, both matrices exhibit "increasing differences" (or "supermodularity"),

[22] Data on marriages by qualification-profile in the post-reform regime would identify the nine out of the sixteen components of ζ involving the types that are perfectly sorted into qualifications based on their abilities. Data from the pre-reform regime would provide a further nine moments, but four of these would over-identify the surpluses associated with marriages between medium- and high-ability/basic- and advanced-qualification types, leaving only five further independent moments to identify the remaining seven surplus terms.

compatible with assortative mating on qualifications. Indeed, with only one exception, a movement away from the lead diagonal where the couple have the same level of qualification is associated with a lowering of the systematic marital surplus.

The data can then be used to formally test whether the $\zeta(\cdot)$ matrix is stable across regimes. To do so, we re-estimate, again by maximum likelihood, using data from both regimes simultaneously in order to fit a common $\zeta(\cdot)$ matrix. Once this restricted model has been estimated, a likelihood ratio test can be applied to test whether the restriction to a common surplus matrix fails to be rejected. The null hypothesis of a stable surplus matrix defined on qualifications is however rejected at any standard level of significance (p-value< 0.0001).[23]

But perhaps more important than the formal statistical rejection is the *nature* of the failure of the model with a single surplus matrix to accurately fit the marriage market response to the RoSLA reform.

Table 6: Observed and Predicted Changes in Never-Married Rates by Qualification Level and Gender Based on a "Before-After" Model without Unobserved Ability

	Males		**Females**	
	Observed change	Predicted change	Observed change	Predicted change
No Qual.	0.047	-0.023	0.040	-0.036
CSE/O-lev	0.011	0.019	0.010	0.018
A-level+	0.016	-0.019	0.015	0.001

Notes: See Table 5.

To highlight this, Table 6 gives the stylized observed changes in the never-married rates between the pre- and post-RoSLA cohorts and contrast these to the corresponding predicted changes based on the multi-market model with a common $\zeta(\cdot)$ matrix. For both men and women, the model predics that the never-married rate of unqualified individuals would go up by down by 3-4 percentage points, which the *direct opposite* to the observed changes. These mis-predictions are driven by the sharp decrease in the supply of unqualified individuals after the reform (Decker, Lieb, McCann, and Stephens, 2012) In contrast, the model's mis-predictions are numerically smaller and less systematic for the other two qualification groups.

Hence the multi-market model where only qualifications matter is not only statistically rejected: it also qualitatively fails to fit the data. Moreover, treating the pre- and post-reform cohorts as separate marriage markets does not allow the identification of a more general model with unobserved ability. Our approach to identification does not involve treating the pre- and

[23]The estimated common $\zeta(\cdot)$ matrix has each surplus value being inbetween the corresponding estimated values shown in Table 5 and are hence not shown here. The rejection of the model with a common matrix defined over qualifications is robust to the inclusion of a post-reform dummy (trend term). Details of the estimation of the restricted model is available upon request.

post-cohorts as belonging to separate marriage markets, but rather treating them as belonging to a single marriage market, where marriages can occur across regimes and where the tradeoffs involved in doing so is modelled in terms of systematic preferences over age gaps.

The following outlines the logic of the identification of our full model. First, as noted above under our assumptions γ is identified from the discontinuity in the qualification rates among men and women, and once γ is identified, so is the joint distribution of ability and qualifications is identified. Second, the age-gap preferences $\lambda(\cdot)$ are identified from the observed age gap distribution, with $\lambda(d)$ reflecting the frequency of age-gap d marriages relative to the zero-gap base category.[24]

Third, with perfect sorting into qualifications by ability in the post-RoSLA regime, the observed distribution of qualification profiles among married couples from the post-RoSLA regime identifies the portion of the full (4×4) $\zeta(\cdot)$ matrix involving types with aligned abilities and qualifications. Finally, the marital surplus from marriages involving unqualified medium ability individuals is identified from the marriage patterns of unqualified individuals in the pre-reform regime (who have a known ability distribution) using that marriages across regimes involve age gaps with a known surplus gains/costs. Closely related our model with unobserved ability naturally rationalizes the observed increase in never-married of unqualified individuals at the RoSLA threshold by the fact that, prior to the reform, the unqualified population included a sizeable portion of medium ability individuals. Similarly, the increased assortative mating among unqualified individuals after the reform (highlighted in Figure 4) is explained by that group becoming homogenous.

We estimate the model by maximum likelihood, solving for the equilibrium at each trial value of the parameters using a basic Newton algorithm.

6. Results and Model Fit

The top panel of Table 7 gives the estimates of $\zeta(\cdot)$ matrix in equation (10), defined over full ability-qualification type. Most terms, including those involving types that are not directly observable, are fairly precisely estimated.

The upper left 2×2 sub-matrix gives the estimates of the surplus from marriages where both spouses are unqualified, but with low or medium ability. We see that, among the unqualified there is strong complementarity in the surplus function with respect to ability.

If we then disregard the second column and second row, the remaining 3×3 matrix gives the estimated surpluses associated with marriage of types whose (individual) abilities and qualifications are perfectly aligned. This reduced model thus corresponds (up to a constant) to the surplus matrix estimated on the post-regime data in Table 5 and shares key properties. Most

[24]Trend terms are identified from trends in never-married rates as, for instance, a decrease in marital surplus for later born cohorts will imply predicted increases in never-married rates.

notably it too exhibits increasing differences.[25]

Table 7: Estimates of Contribution of Ability-Qualification Profile to Marital Surplus and the Marginal Contribution of Ability and Qualification to Marital Surplus

Panel A: Marital Surplus by Ability-Qualification Profile

Females / Males	Low Ab., No Qual.	Medium Ab., No Qual.	Medium Ab., CSE/O-Lev.	High Ab., A-Level+
Low Ab., No Qual.	-0.580	-3.061	-2.030	-5.042
	(0.222)	*(0.544)*	*(0.139)*	*(0.159)*
Medium Ab., No Qual.	-2.697	-1.472	-2.234	-4.921
	(0.559)	*(0.669)*	*(0.407)*	*(0.443)*
Medium Ab., CSE/O-Lev.	-2.500	-2.501	-1.133	-3.064
	(0.197)	*(0.338)*	*(0.083)*	*(0.090)*
High Ab., A-Level+	-4.656	-4.271	-2.310	-0.498
	(0.208)	*(0.355)*	*(0.089)*	*(0.090)*

Panel B: Value of Ability and Qualification

	Male:		Female:	
Spouse	Ability	Qual.	Ability	Qual.
Low Ab., No Qual.	-2.117	0.197	-2.481	1.031
	(0.633)	*(0.522)*	*(0.681)*	*(0.529)*
Medium Ab., No Qual.	1.589	-1.029	1.225	-0.762
	(0.943)	*(0.597)*	*(0.821)*	*(0.545)*
Medium Ab., CSE/O-Lev.	-0.204	1.101	-0.001	1.368
	(0.514)	*(0.397)*	*(0.511)*	*(0.327)*
High Ab., A-Level+	0.122	1.857	0.385	1.961
	(0.565)	*(0.435)*	*(0.532)*	*(0.344)*

Notes: The estimation sample pools all individuals observed in the 1984-2014 UK LFS from academic cohorts 1953-1960 with non-missing information on age, qualification and marital status to characterize the qualification distribution by gender and cohort, and to characterize the marriage frequencies by husband-wife cohort- and qualification profile. Cohort sizes are based on ONS birth statistics and the never-married rates by gender and qualification level are based on UK census data as outlined in Section 2.

From the estimates in the top panel of Table 7 we can obtain estimates of the contribution of ability of men to marital surplus—given no qualification—by taking the difference between the second and the first row. The contribution of ability of women—given having no qualification—is correspondingly given by the difference between the second and first column. Similarly, estimates of the contribution of a basic qualification to marital surplus—given medium ability—are obtained by taking the difference between the third and the second row for men and between the third and second column for women. In doing so, we obtain estimates of the contribution of ability and a basic qualification not only by gender, but also by the full type of the spouse.

[25]If we instead disregard the first column and row to explore complementarity in qualifications by including medium ability individuals with and without any qualification, we again obtain a 3×3 matrix exhibiting increasing differences.

These estimated values are highlighted in the lower panel of Table 7.

Some distinct patterns emerge among these estimates. First, the results are fairly similar for men and women. A qualification always increases marital surplus when the spouse has at least a basic qualification, but does not necessarily do so when the spouse is unqualified. Ability by itself does not signficantly increase marital surplus when the spouse is academically qualified; in contrast, when the spouse is unqualified, the complementarity in ability means that it increases surplus when the spouse also has medium ability but decreases it when the spouse only has low ability.

The estimated contributions of age gaps and trends to the marital surplus are presented in Table 8. The top panel reports the estimated parameters of the age gap function $\lambda(\cdot)$ while the lower panel reports the estimated parameters of the trend functions $\tau^g(c;a)$, $g = m, f$. As we will see below, the estimated $\lambda(\cdot)$ is closely related to the empirical aggregate age gap distribution. Most estimated trend parameters in Panel B are negative, which is consistent with never-married rates increasing across cohorts.

Table 8: Estimates of Marital Surplus: Age Gap and Trend Terms.

Part A: Age Gap Function, $\lambda(c_j - c_i)$					
β_{-3}	β_{-2}	β_{-1}	β_{+1}	β_{+2}	β_{+3}
-3.579	-2.628	-1.435	0.249	0.055	-0.300
(0.042)	*(0.032)*	*(0.024)*	*(0.019)*	*(0.020)*	*(0.022)*
β_0^-	β_1^-	β_0^+	β_1^+		
-3.147	0.286	0.088	-0.284		
(0.163)	*(0.030)*	*(0.074)*	*(0.013)*		
Part B: Trend Functions, $\tau^k(c;a), k = m, f$					
$\beta_{a_0}^m$	β_{a_0,R_1}^m	$\beta_{a_0}^f$	β_{a_0,R_1}^f	$\beta_{a_1}^m$	β_{a_1,R_1}^m
-0.076	-0.109	-0.187	-0.054	0.066	-0.136
(0.023)	*(0.032)*	*(0.036)*	*(0.044)*	*(0.015)*	*(0.028)*
$\beta_{a_1}^f$	β_{a_1,R_1}^f	$\beta_{a_2}^m$	β_{a_2,R_1}^m	$\beta_{a_2}^f$	β_{a_2,R_1}^f
0.014	-0.136	-0.039	-0.065	-0.085	-0.087
(0.017)	*(0.031)*	*(0.017)*	*(0.036)*	*(0.019)*	*(0.039)*

Notes: See notes to Table 7 for sample used and text for the specifications of estimated functions.

Consider next model fit. First, not surprisingly, the model replicates the overall assortative mating on qualifications well: Figure 10 shows the model-predicted version of the empirical distributions in Figure 3. But more interestingly, the model also replicates quite well the increase in assortative mating among unqualified individuals after the reform: Figure 11 shows the predicted versions of the measure $S(z)$ by gender and cohort as hatched lines (with solid lines

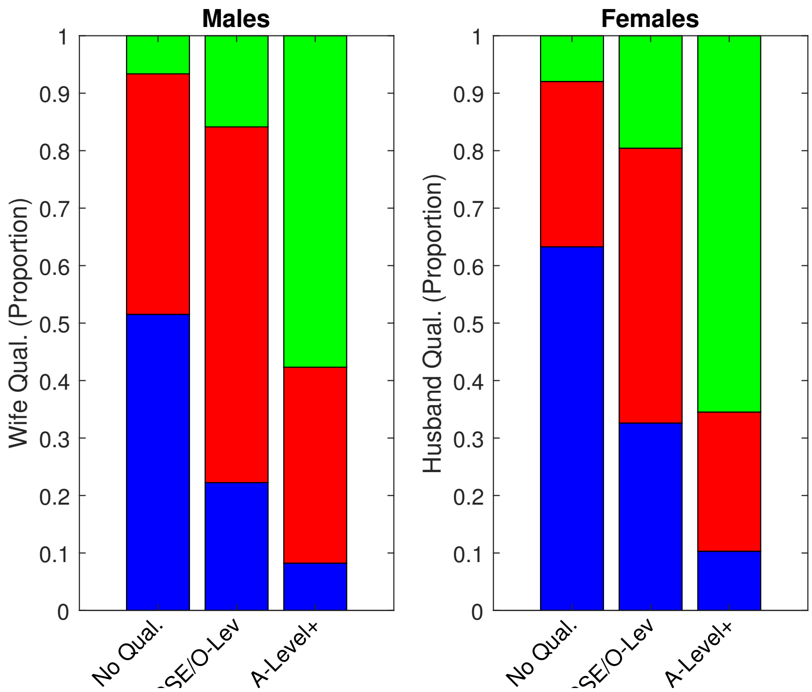

Figure 10: Model Predicted Assortative Matching on Qualifications

still representing the empirical data). Indeed, the model predicts an increases in $S(z_0)$ for both men and women at the reform threshold and of empirically reasonable values.

Turning to never-married rates, we see in Figure 12 that the estimated model replicates the overall pattern in never-married rates across cohorts and qualifications quite well. Specifically, the model predicts increasing never-married rates for both unqualified men and women at the reform threshold. As noted above, allowing for an unobserved ability in the model is crucial in this respect as a model where only qualifications matter would mispredict that the never-married rates of unqualified males and females should be going down as their supply reduces.[26]

The model also fits well the never-married rates for individuals with basic and advanced qualifications, possibly with the exception of men with a basic qualification before the reform. The model generally tends to predict an increase in the never married rates at the RoSLA of individuals holding this level of qualification due to their increased supply. However, there is little evidence of such an effect in the data.

Consider then the age gap distribution. Figure 13 plots the predicted age gap distribution

[26]This was illustrated below using the simple "before- and after" model. We have also estimated a version of our main model where ability does not matter. This is thus a constrained version of our model where the second row and column of the surplus matrix presented in Panel A of Table 7 are constrained to be equal to the first row and column respectively. This thus imposes seven constraints on the parameters which can be formally tested using a likelihood ratio test. Such a test strongly rejects the set of constaints. Looking specifically at the model fit from such a constrained model, it predicts a sharp reduction in the never-married rates of unqualified individuals at the reform threshold. The estimated parameters and predicted never-married rates from this constrained model are presented in an Appendix that is available on request from the authors.

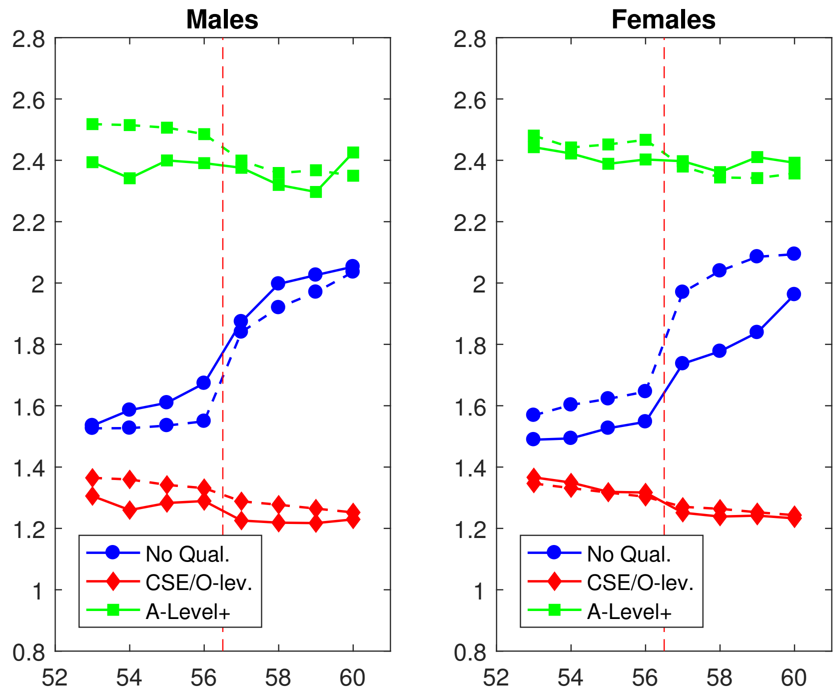

Figure 11: Model Predicted and Empirical Assortative Matching by Cohort, Gender, and Qualification Level

alongside the empirical one for the central values of -4 to $+4$, showing a very close fit. In addition, and using the right scale, the figure plots the estimated $\lambda(\cdot)$ in exponential form, thus highlighting the tight connection between $\lambda(\cdot)$ and the observable age gap distribution.[27]

7. The Marriage Market Qualification Premia

In a recent key contribution, Chiappori, Salanié, and Weiss (2017) highlighted the central role played by the marriage market return to a college degree for understanding the how investments in education have changed over time and differentially so for men and women. The authors develop a household model where couples form, produce household public goods, and invest in their children's education. As a child's human capital is produced using parents' time and own human capital, the authors argue theoretically that, as the returns to education increases and as technological innovations reduce the time needed for other domestic production, we should observe an increase in marital sorting, particularly among highly educated. Morever, the model predicts that "marital college premium" should increase particularly for women. Using US data on the marriage patterns of individuals born over a 30-year period, 1942-1973, the authors find empirical support for their model. Specifically, in their main empirical analysis, they estimate a multi-market version of the Choo-Siow model in which different cohorts are treated as separate

[27]Note that the base category of zero age gap has $\exp(\lambda(0)) = 1$ since $\lambda(0)$ is normalized to zero.

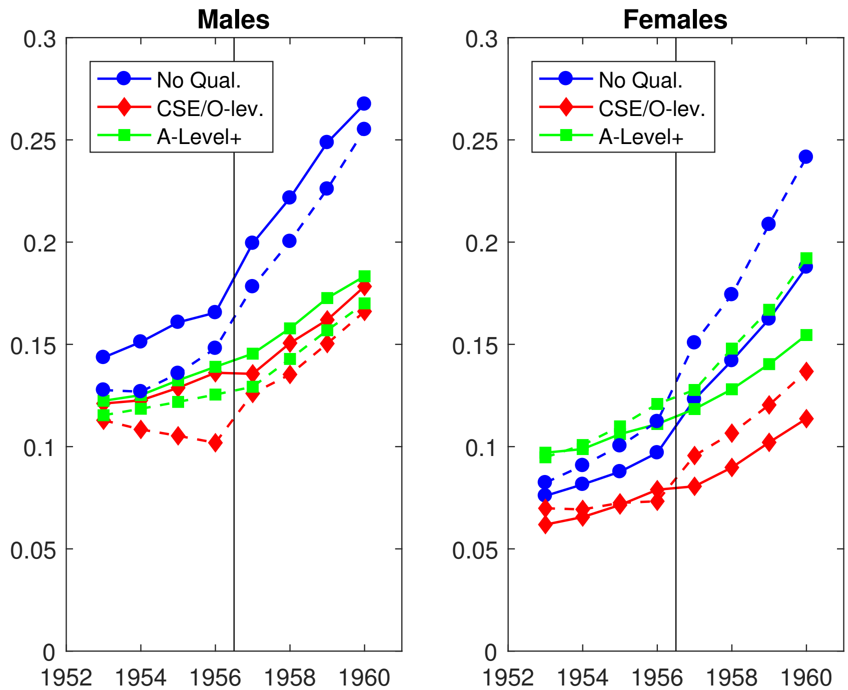

Figure 12: Model Predicted and Empirical Never-Married Rates by Cohort, Gender, and Qualification Level

marriage markets and where variation in the qualification distribution across cohorts provides identification.

Our model offers a different perspective on the marital premium associated with gaining academic qualifications. Specifically, the RoSLA reform that we focus on created a very sudden cohort variation in the distribution of qualifications. This sharp reform-induced variation makes the assumption that the ability distribution was unchanged plausible which allowed us to separately identify the contribution of a (basic) academic qualification and of ability to marital surplus. Our approach further allows us to separate out a "marital qualification premium" and a "marital ability premium".

In order to proceed we will start by defining these marital premia. Define the expected marital utility for a male of full type $x_i \in X$ as

$$\overline{u}\left(x_i\right) \equiv E\left[\max_{x_j \in X \cup \{0\}} \left\{U\left(x_i, x_j\right) + \varepsilon_i\left(x_j\right)\right\}\right] = -\log\left(\mu^m_{0|x_i}\right), \tag{14}$$

where the expectation is taken over the random utility components, and where the second equality follows from the properties of the extreme value distribution. Similarly, for a female of type $x_j \in X$, the expected marital utility can be written as

$$\overline{v}\left(x_j\right) \equiv E\left[\max_{x_i \in X \cup \{0\}} \left\{V\left(x_i, x_j\right) + \varepsilon_j\left(x_i\right)\right\}\right] = -\log\left(\mu^f_{0|x_j}\right). \tag{15}$$

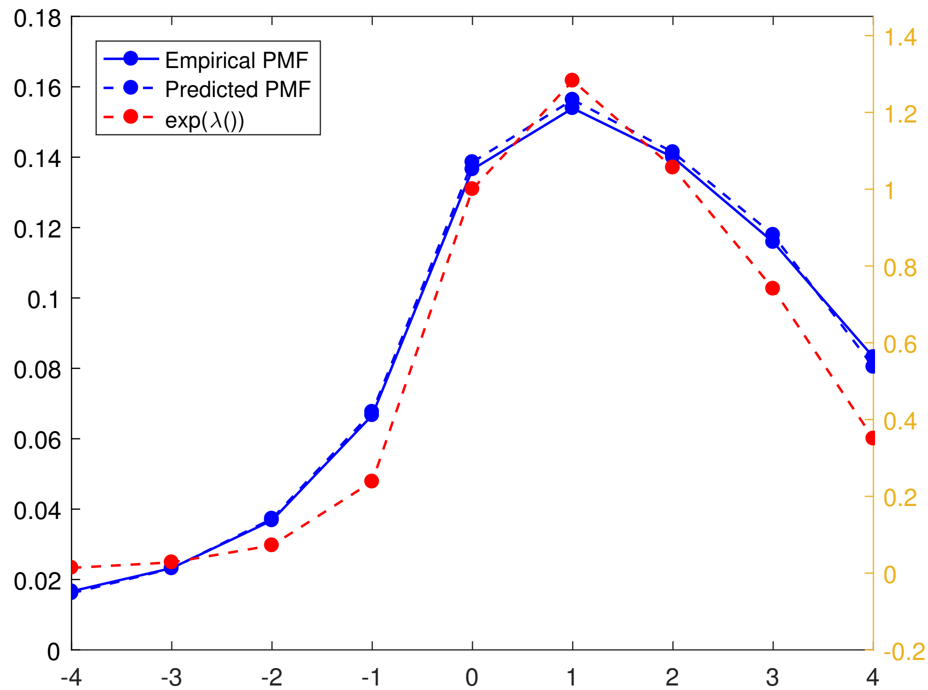

Figure 13: Model Predicted and Empirical Age Gap Distribution and Estimated Contribution of Age Gaps to Marital Surplus

Following Chiappori, Salanié, and Weiss (2017) we can define a marital qualification premium for males of cohort $c \in C$, denoted $QP^m(c)$, as the increase in expected utility associated having qualification level z_1 rather than z_0, holding the ability level fixed at a_1. A qualification premium for women, $QP^f(c)$, is analogously defined,

$$QP^m(c) \equiv \overline{u}(c, z_1, a_1) - \overline{u}(c, z_0, a_1) \quad \text{and} \quad QP^f(c) \equiv \overline{v}(c, z_1, a_1) - \overline{v}(c, z_0, a_1). \tag{16}$$

Correspondingly we define a marital ability premium for males of cohort c, denoted $AP^m(c)$, as the increase in expected utility associated with having ability level a_1 rather than a_1, holding the qualification level fixed at z_0. A female ability premium $AP^f(c)$ can be defined correspondingly,

$$AP^m(c) \equiv \overline{u}(c, z_0, a_1) - \overline{u}(c, z_0, a_0) \quad \text{and} \quad AP^f(c) \equiv \overline{v}(c, z_0, a_1) - \overline{v}(c, z_0, a_0). \tag{17}$$

We will further refer the "total marital premium" for gender g and cohort c as

$$TP^g(c) \equiv QP^g(c) + AP^g(c). \tag{18}$$

The $TP^g(c)$ thus refers to the difference in expected marital utility for an individual with medium ability and holding a basic qualification and an individual from the same cohort c who is has low ability and no qualification. The model estimated above separately identifies $QP^g(c)$

and $AP^g(c)$ for gender g in the pre-reform cohorts $c \in R_0$. After the reform, per assumption, there are no longer any unqualified individuals with medium ability. Nevertheless, the $TP^g(c)$ is remains defined also for $c \in R_1$.

Before presenting our estimated decomposed premia, we will start by inspecting the empirical log-difference in never-married rates for adjacent qualification types by gender. As suggested by (14) and (15) the negative of the log-never-married rate can be interpreted as a measure of the expected marriage utility. In particular, the log difference in never-married rates for adjacent qualification types can be viewed as a "raw" empirical marital qualification premium for holding the higher qualification rather than the lower one when unobserved ability is unaccounted for.[28] Figure 14 thus plots $-\ln\left(\widetilde{\mu}^g_{0|c,z_j}/\widetilde{\mu}^g_{0|c,z_{j-1}}\right)$ for $j = 2, 1$ and for $g = m, f$, where $\widetilde{\mu}^g_{0|c,z}$ is the empirical never-married rate for individuals of gender g and cohort c and holding qualification z.

The red lines in Figure 14 are thus the raw empirical marriage premia associated with an advanced qualification. It is distinctly negative for women as the high qualified women married less frequently in their lives than did the basic qualified women in every cohort. In contrast, it is close to zero for men as high- and basic-qualified men had about the same ever-married rates in all cohorts. Even though we are using only eight cohorts, the raw high-qualification marriage premium echo the finding of Chiappori, Salanié, and Weiss (2017) of an increasing qualification premium for women and a stable one for men. The blue lines illustrate the corresponding raw marriage premia for men and women holding a basic qualification. The two notable features of this basic-qualification premium are (i) it is positive for both men and women and for all cohorts, and (ii) there is a discontinuity at the reform threshold.

We now turn to the qualifications and ability marital premia implied by our estimated model, highlighted in Figure 15. These decomposed premia offer a very different perspective. For men the model indicates a substantial positive ability marriage premium, but a negative qualification premium. For women, while there is a positive ability premium, the basic qualification premium is essentially zero.

The total marriage premium – the premium associated with both an increase in ability and gaining a basic qualification – is positive and increasing for both men and women. While the total premium can be decomposed for cohorts born prior to the reform threshold, after the reform this is no longer possible. Nevertheless, unlike for the raw empirical basic qualification premium, there are no upward jumps at the reform threshold.

A positive qualification premium among the pre-RoSLA cohorts would have indicated that a basic qualification had a positive "causal" effect on the probability of ever marrying as it compares the likelihood of married with and without such a qualification whilst holding ability

[28] Indeed, if we interpreted each cohort-qualification combination in $C \times Z$ as a separate type and implemented a Choo-Siow model with a fully unconstrained 24×24 marital surplus matrix, the model would perfectly fit every moment, including all empirical never-married rates by cohort, qualification and gender.

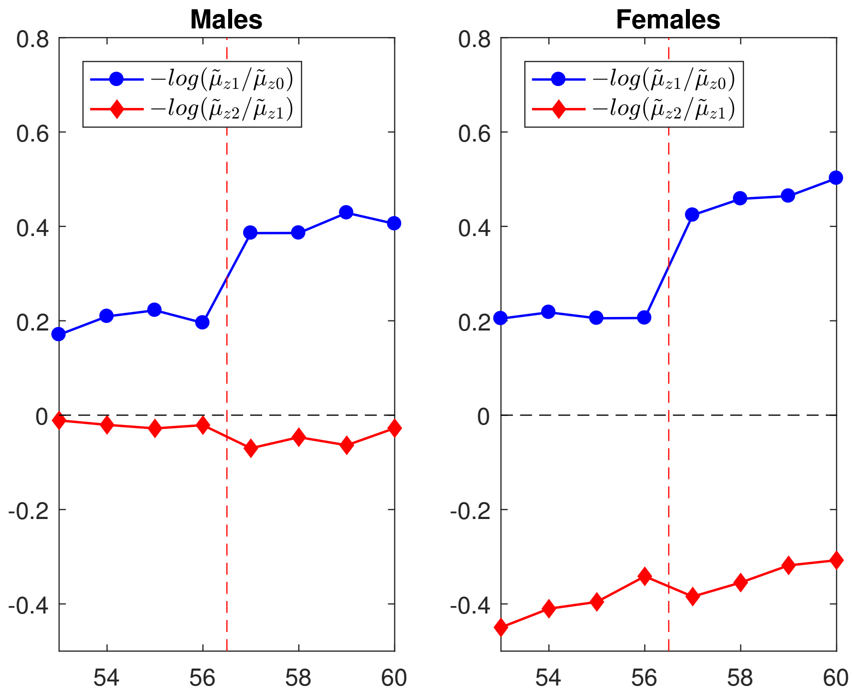

Figure 14: "Raw" Empirical Marital Qualification Premia

constant. Only a small number of contributions provide estimates of the causal effect of education on the probability of getting married without relying on educational reforms that affect entire cohorts. Lefgren and McIntyre (2006) studying marital outcomes of women in the US use quarter of birth as instrument for educational attainment. However, their IV estimate of the causal effect of educational attainment on the probability of being married is negative but small; unfortunately it is also imprecisely estimated, leaving them to conclude that it is "difficult to rule out a moderate-sized effect of either sign" (Lefgren and McIntyre, 2006, p. 812). Similarly, Anderberg and Zhu (2014) consider the causal effect specifically of holding a basic CSE/O-level qualification on marital outcomes of women in the UK, relying on the previously existing Easter-school leaving rule which split each academic in two parts. Similar to Lefgren and McIntyre (2006), their point estimates of the causal effect on the probability of being married are negative but not statistically significant. Hence we conclude that the finding here of a zero basic qualification premium for women is consistent with available evidence. Neither study provides any corresponding estimates for males.

Both the cited studies, however, provide evidence of a causal impact of a woman's educational attainment on the "quality" of the husband as measured by his income or educational attainment. Figure 3 shows that a man or a women who holds a basic qualification is about 30 percentage points more likely to be married to a spouse holding some academic qualification than in an unqualified individual. In Figure 16 we use our estimated model to predict how the probability of being married to an academically qualified spouse increases with own ability and holding

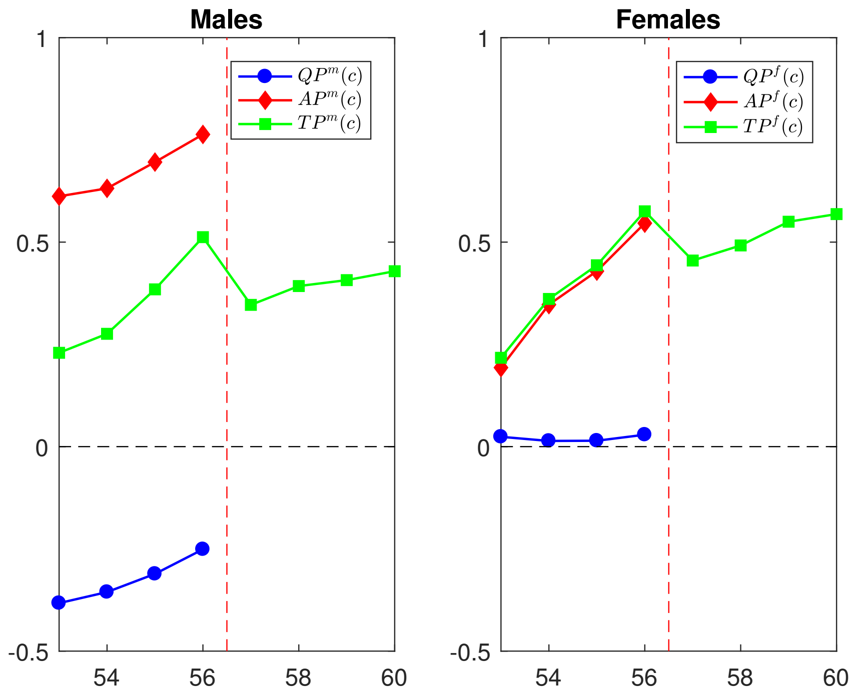

Figure 15: Marital Qualifications and Ability Premia

a basic qualification separately (conditional on being married). As in the case of the marital premia, this decomposition is only available before the reform whereas the total effect of both increasing ability and gaining a basic qualification is defined for all the cohorts. The figure suggests that, for both men and women, the gap in the spousal qualification rate is caused in roughly equal parts by own ability and qualifications.

This section has thus extracted two insights from the estimated equilibrium model of the marriage market. First, the findings in this section caution against interpreting estimates of marital premia that do not account for unobserved ability and relying on data with cohort variation in qualifications with an unknown relationship to unobserved ability. Relying on a sharp cohort variation in qualifications – where the ability distribution can be assumed to be stable – our estimates suggest that, for both men and women, ability was a more important personal characteristic than a basic academic qualification in terms of the likelihood of ever-marrying. Second, in terms of the probability of marrying a qualified spouse, both ability and holding a qualification matters, as also suggested by the scantily available IV estimates. The benefit to the approach used here is that it can be used to uncover not only the causal effects on marriage probability and partner type, but also the underlying preference structure generating these effects and in a context with potentially significant general equilibrium effects. In the next section we will consider whether the general equilibrium effects induced by the reform were indeed substantial.

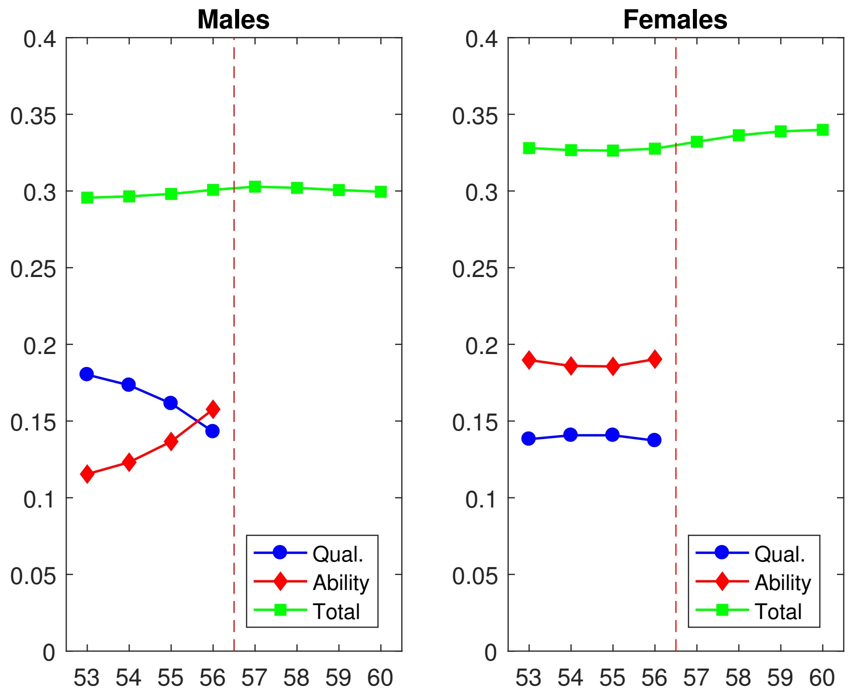

Figure 16: The Effect of Own Ability and Academic Qualification on the Probability of the Spouse Holding an Academic Qualification.

8. The Effects of the RoSLA on Marital Outcomes

The RoSLA reform raised the level of academic attainment for a portion of men and women born after September 1957. However, the reform may well have affected the marital outcomes of a substantially larger set of people, including individuals whose educational choices and outcomes were not directly affected by the reform. In this section we use our estimated model to explore the effects of the RoSLA on the probability of ever-marrying and on marital sorting. We focus on ability types as individual ability was not affected by the reform, and in this sense is a more fundamental individual characteristic than the correlated academic qualification.

To characterize the effect of the reform we will contrast the predicted marriage outcomes from our main model to those obtained from a counterfactual simulation where the reform was never implemented. In our counterfactual scenario we thus assume that the pre-reform mapping from ability to qualifications illustrated in Figure 9 continued to apply also in the post-reform cohorts, providing us with a counterfactual distribution of qualifications. We then use the counterfactual distribution of full types along with the estimated marriage surplus parameters to compute the predicted counterfactual marriage market equilibrium. Using this approach allows us to ask, for instance, whether the marital prospects of low ability individuals where adversely affected by the reform by inducing medium ability individuals to more frequently gain academic qualifications.

Consider first how the reform affected the proportion never-married by ability type, gender and cohort. This is illustrated in Figure 17.

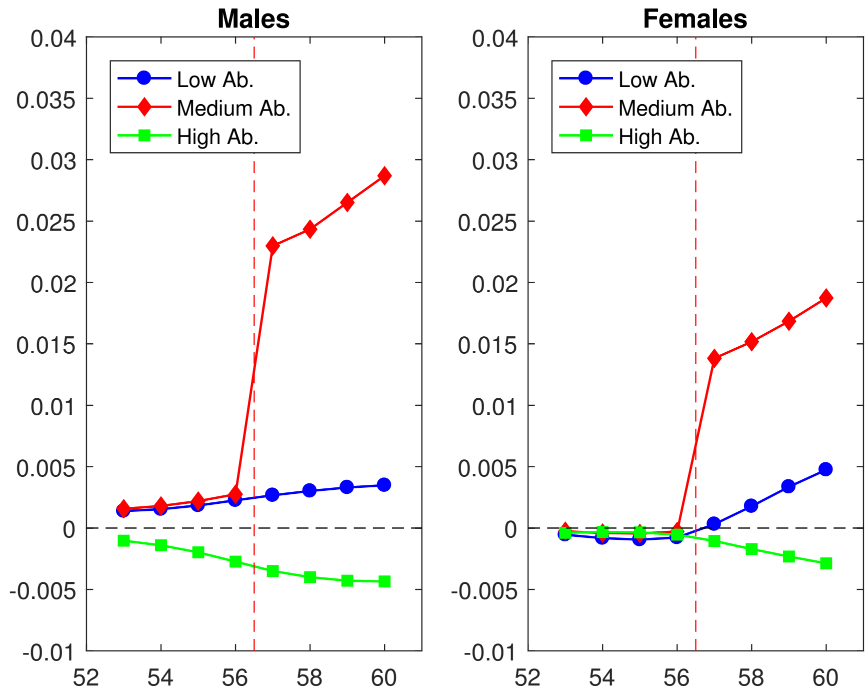

Figure 17: The Effect of the RoSLA on the Proportion Never-Married by Ability Type, Cohort and Gender

In our empirical model we included four pre- and four post-reform cohorts. This modelling choice reflected in part the immediate empirical observation that most husband-wife age gaps fall in within the range of ± 4 years, but also a more general conjecture that the marital outcomes of individuals born pre-1953 would probably not have been significantly affected by the reform. Indeed, the simulations suggest that the impact of the reform on the never-married rate was negligible for nearly all ability types and for both men and women in the 1953 cohort.

Consider first the medium ability individuals. As this was the only ability type directly affected by the reform in terms of their academic qualifications, this is also the only type for which the impact of the reform was discontinuous at the threshold. The model suggests that the reform significantly increased the never-married rates for medium ability individuals of both genders. This reflects that the reform created a positive supply shock for basic qualified individuals. However, it also reflects the marital qualification premium; as shown in Figure 15 this was found to be negative for men and zero for women, explaining why the increase in the never-married rate for medium ability men induced by the reform was larger than that for women.

The other two ability types were not directly affected by the reform in terms of their academic attainment, but they may have been affected in terms of their marital outcomes. Before the reform, low ability men and women frequently married unqualified but medium-ability partners.

When this type disappeared after the reform, the low ability individuals lost a natural choice of marital partner and, as a result, their never-married rate increased as a consequence of the reform. Note that the typically positive husband-wife age gap meant that the reform increased the never-married rate for low ability males born even before the threshold whereas for women the effect was concentrated among the post-reform cohorts.

In contrast, for high ability individuals (always holding an advanced qualification) the unqualified but medium-ability type was never an attractive marriage partner. Instead, when more individuals of the opposite gender gained basic qualifications as a result of the reform, the high ability individuals benefited from the increased supply of academically qualified potential partners, leading them to marry more frequently. While the effect of the reform, due to the marital age gaps, affected earlier cohorts of men than of women, within a couple of years, both low and high ability types of both genders where permanently affected in terms of their probability of ever-marrying at a rate of close to half a percentage point, negatively for the low ability type and positively for the high ability type. While economically significant, it is worth noting that these effects are still relatively small compared to the trend in never-married rates over the sample cohorts.

Figure 18 illustrates the effect of the reform on marital sorting. In particular, it displays the impact of the reform on the distribution of spouse ability type by own ability type, cohort and gender conditional on marriage. For instance, the top left panel shows that low ability males, as a consequence of the reform, more frequently married low ability women and less frequently medium ability women. A corresponding effect is highlighted for the low ability women in the bottom left panel. The result so far thus indicate that the reform unambiguously worsened the marital prospects for the low ability types, reducing their chances of ever marrying and making them more prone to marry among themselves rather than to "marry up" in terms of ability.

The two right panels in contrast show that the high ability individuals, as a consequence of the reform, less frequently married among themselves and more frequently married medium ability, reflecting that the reform increased the academic qualification rate of the medium ability individuals.

This is also reflected in the two middle panels which highlight the effect of the reform on the medium ability individuals. Here discontinuities naturally occur as the reform directly affected the academic qualifications held by the medium ability type.

For instance, the reform increased the probability of medium ability type males marrying low ability women in pre-reform cohorts (whose own qualifications were not increased by the reform) but lowered the same probability in the post-reform cohorts (whose own qualifications were increased by the reform). Nevertheless, the most salient feature is that the reform, in the post-reform cohorts, increased the frequency of marriages to high ability spouses and lowered the frequency of marriages to medium ability spouses.

Hence while the results suggest that the reform left the low ability individuals more isolated in the marriage market, it naturally reduced the marital sorting on ability among medium and high ability types as they became more homogenous in terms of their academic qualifications.

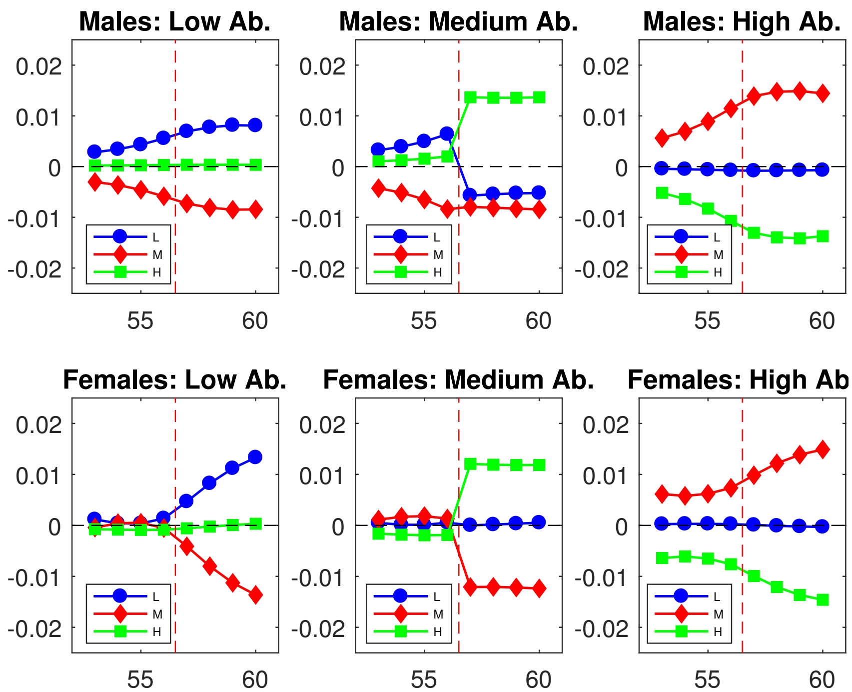

Figure 18: The Effect of the Reform on the Distribution of Spouse Ability

The results in this section have highlighted the reform's general equilibrium effects, showing how individuals – including from pre-reform cohorts – whose educational choices and outcomes were not directly affected by the RoSLA, were affected in terms of their marital outcomes. Such general equilibrium effects have implications for how one can use reforms such as the RoSLA to study the impact of educational attainment on outcomes shaped at the household level, including most obviously marital and fertility outcomes but possibly also outcomes such as health. Contributions to this literature routinely rely adopt IV/RD designs that assume the absence of general equilibrium effects. These issues have been noted before in the literature, but to the best of our knowledge never quantified.[29]

9. Conclusions

How large are the marriage market returns to educational qualifications? Does the answer to this question depend on whether one accounts for unobservable characteristics correlated with

[29]Contributions that use compulsory school leaving are reforms to investigate the impact of educational attainment on marital and fertility outcomes include, for instance, Black, Devereux, and Salvanes (2008), Silles (2011), Cygan-Rehm and Maeder (2013), and Güneş (2016). Lefgren and McIntyre (2006) note that compulsory schooling laws do not offer identification "because any change in schooling laws will change the schooling of all potential husbands and competing women" (Lefgren and McIntyre, 2006, p. 807). Hence, as noted above, they rely instead on quarter of birth as instrument for educational attainment.

investments in education? How large are the general equilibrium effects in marriage markets when there is a shock to the supply of types interacting in the marriage market? To answer these questions, we have relied on the 1973 UK raising of the school-leaving age (RoSLA), a reform well-known to have had a large impact on the academic qualification rate for the cohorts directly affected.

In term of methodology, we have set out to build on and combine two highly influential literatures. The first is a well-known IV/RD literature that relies on reforms such as the RoSLA for identifying the effects of educational attainment on a variety of outcomes. The problem that this literature faces in the context of marriage-related outcomes—potentially including also outcomes such as fertility—is that it, typically implicitly, assumes away general equilibrium effects. But general equilibrium effects can be expected to be pervasive in the context of reforms as large as the RoSLA, with cohorts and ability types not directly affected by the reform experiencing externalities in the marriage market.

The second literature that we relate and contribute to is the growing literature that, following Choo and Siow (2006), empirically estimate marriage market equilibria. Estimating structural models, this literature aims to recover the sources of marriage surplus and the equilibrium utilities that various types of individuals experience via the marriage market. Most recently, a key contribution by Chiappori, Salanié, and Weiss (2017) highlighted how the marriage market return to a college degree has evolved over time, and the importance of this for understanding investments in education especially for women. The shortcoming to date of this literature, particularly when used to explore variation in educational attainment, is that it has not been able to account for unobservable personal characteristics correlated with education. Tackling the issue of potential bias due to unobservables is of course a main aim of the IV/RD literature. We have shown that, by exploiting RD logic, one can identify and estimate an equilibrium marriage market model with unobservable ability that is correlated with educational attainment. In doing so, we were able to provide concrete answers to the above questions.

We first verified that accounting for unobservables is central to fitting the data. In particular, we provided empirical evidence that the never-married rate of unqualified individuals—both men and women—increased at the RoSLA threshold which we showed to be contrary to the predictions from an estimated model without unobserved ability. Our extended model offers a simple explanation for the observed increase: the educational response to the RoSLA was "selective" with those responding to the reform by gaining an academic qualification having higher ability than those not responding. The average ability among unqualified thus decreased after the reform, and as ability is a highly valued characteristic in the marriage market, this compositional shift raised the never-married rate among unqualified men and women.

Second, we used our model to decompose the marital premium into an ability component and a qualification component. We find positive ability premia for both men and women, but no positive (basic) qualification premia for either gender. We argued that this finding is consistent with available evidence that find no positive causal effect of holding a qualification on the probability of marrying.

Finally, we explored the magnitude of the general equilibrium effects induced by the reform using a counterfactual simulation where we assumed that the reform was never implemented. Focusing specifically on the proportion ever-married, we showed that the effect of the reform was by no means confined to cohorts and ability types directly affected by the reform. Indeed, the reform lowered the proportion ever-married among low ability individuals and increased it among high ability individuals, with these effects affecting men born even before the reform threshold.

Hence, by exploiting the well-known UK 1973 RoSLA we have highlighted the importance of accounting for unobserved ability in marriage market analysis and for general equilibrium effects when using large-scale reforms to identify the effects of qualifications on family-related outcomes. To what extent these findings call into question conclusions from the related literatures is a question for future research.

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Appendix: The Model without Unobserved Ability

In this Appendix we present estimates from our full model when constrained so that ability does not matter for marital surplus. In this case a type is simply defined as $x \in C \times Z$ with all combinations possible. Hence there are 24 types of men and women. We impose the same separability assumption on marital surplus as in our main model. We also impose the same age-gap preference structure $\lambda(\cdot)$ and the same trend structure except that trends in marital surplus are now defined directly over qualification types, $\tau^g(c;z)$ (as ability no longer features in the model). The model presented here is thus restricted relative to the main model in that the main $\zeta(\cdot)$ function is now defined only over qualification profiles, $\zeta : Z \times Z \to \mathbb{R}$. It is thus represented by a 3×3 matrix, imposing seven restrictions relative to our main model. Specifically, in terms of the matrix in Panel A of Table 7, the restrictions imposed is that the first column is the same as the second coloumn and the second row is the same as the first row.

The estimated contributions of all possible qualification profiles to marital surplus are presented in Table A1 and the age-gap and trend terms are presented in Table A2. As both the restricted and the main model are estimated by maximum likelihood, a likelihood ratio test can be used to test the restriction that ability does not matter for marital surplus. The test statistic is 323.3 which is rejected by a χ^2-test (with seven degrees of freedom) at any conventional level of significance, $p - value < 0.001$.

As important as the statistical rejection is the nature of the failure of the restricted model to fit the data. Above we used the simple "before-after" model to argue that when neglecting to acccount for ability, the model predicts that the never-married rates of unqualified should decrease at the reform threshold. This mis-prediction carries over to this larger model and is illustrated in Figure A.1. When ability is removed form the main model it predicts, contrary to the data, a sharp decrease in the proportion of unqualified individuals who never marry.

Table A1: Estimates of Marital Surplus by Qualification Profile in Constrained Model

Females / Males	No Qualification	CSE/ O-Level	A-Level or higher
No Qual.	0.244	-1.413	-4.350
	(0.076)	*(0.079)*	*(0.091)*
CSE/O-Level	-1.701	-1.088	-3.029
	(0.083)	*(0.083)*	*(0.090)*
A-Level+	-3.762	-2.285	-0.482
	(0.095)	*(0.089)*	*(0.090)*

Notes: See notes to Table 7.

Table A2: Estimates of Marital Surplus: Age Gap and Trend Terms in Constrained Model

Part A: Age Gap Function, $\lambda(c_j - c_i)$					
β_{-3}	β_{-2}	β_{-1}	β_{+1}	β_{+2}	β_{+3}
-3.580	-2.630	-1.435	0.248	0.053	-0.302
(0.042)	*(0.032)*	*(0.024)*	*(0.019)*	*(0.020)*	*(0.022)*
β_0^-	β_1^-	β_0^+	β_1^+		
-3.147	0.286	0.081	-0.283		
(0.162)	*(0.030)*	*(0.073)*	*(0.013)*		
Part B: Trend Functions, $\tau^k(c; z), k = m, f$					
$\beta_{z_0}^m$	β_{z_0,R_1}^m	$\beta_{z_0}^f$	β_{z_0,R_1}^f	$\beta_{z_1}^m$	β_{z_1,R_1}^m
-0.135	-0.131	-0.255	-0.088	0.056	-0.126
(0.014)	*(0.030)*	*(0.018)*	*(0.037)*	*(0.014)*	*(0.028)*
$\beta_{z_1}^f$	β_{z_1,R_1}^f	$\beta_{z_2}^m$	β_{z_2,R_1}^m	$\beta_{z_2}^f$	β_{z_2,R_1}^f
0.013	-0.141	-0.044	-0.059	-0.084	-0.090
(0.017)	*(0.031)*	*(0.017)*	*(0.036)*	*(0.019)*	*(0.039)*

Notes: See notes to Table 7.

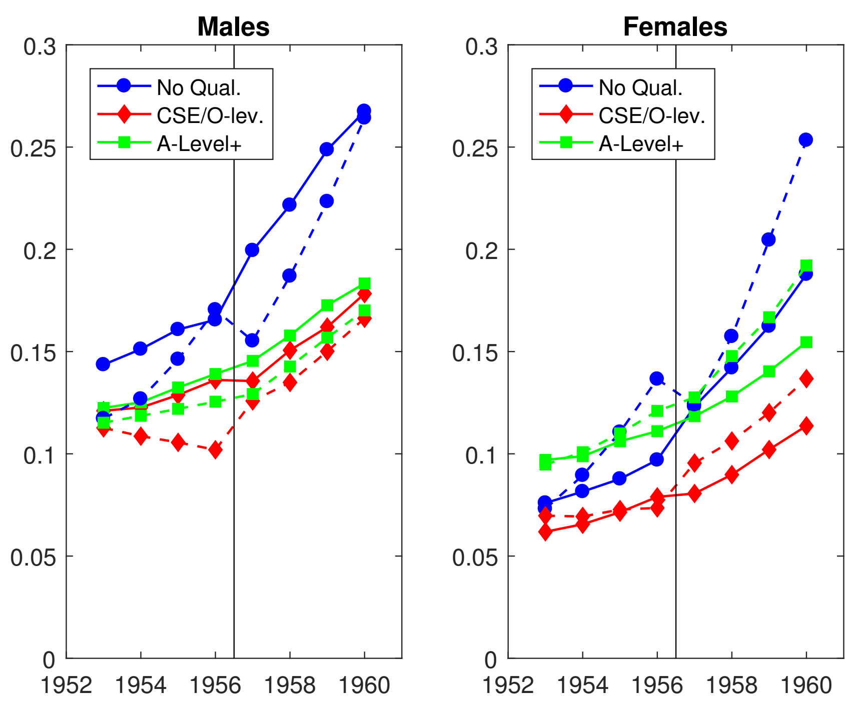

Figure A.1: Model Predicted (Constrained Model) and Empirical Never-Married Rates by Cohort, Gender, and Qualification Level