RESEARCH REPORT
STUDY PAPER 187
MARCH 2023

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THE ROCKWOOL FOUNDATION
RESEARCH

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Published by:

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THE DEMOGRAPHIC AND SOCIOECONOMIC CONSEQUENCES OF RESTRICTING ACCESS TO MARRIAGE FOR YOUNG MIGRANT WOMEN IN DENMARK[*]

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[*] Corresponding author: Peter Fallesen. This work was funded by the ROCKWOOL Foundation (#1207). Peter Fallesen acknowledges additional funding from the Swedish Research Council for Health, Working Life and Welfare (#2016-07099). All work carried out in independence from the funders. Andreas Eklundh Sørensen provided competent research assistance. We thank Tim Bruckner, Alicia Adsera, Thomas Crosley, Fabrizia Mealli, Deadric Williams, Youngmin Yi, Belén Rodríguez Moro, and seminar participants at the ROCKWOOL Foundation and European University Institute for helpful comments. Previous versions presented at meetings of the Population Association of America and the RC28. Authorship is shared equally across authors and author order randomized using the AEA author randomization tool (confirmation code X1ub2DRZuv6c).

Abstract

In this paper, we study how delaying and foregoing marriage among a population group generally prone to marry young affect subsequent fertility behavior and human capital accumulation. In July 2002, reforms limited the marriage opportunities for all Danish and non-EU citizens under 24 living in Denmark, who wished (or whose parents wished for them) to marry a person from outside the European Union (EU). Prior to the reform, more than 80 % of first- and second-generation migrants from outside the EU married spouses from their parents' countries of origin. Using full population administrative data, we show that the policy delayed marriages, increased premarital cohabitation, and changed the composition of spouses. Further, in-wedlock fertility delayed and decreased, while educational attainment increased. Our results emphasize that reforms constraining access to external marriage markets have lasting effect especially on marriage demographics among migrants.

European populations are increasingly being shaped by migration. As of 2014, 1 in 6 people residing in Europe were either a first- or second-generation immigrant, up from 1 in 7 in 2008 (Eurostat 2016). The speed at which migrant populations adapt to the marriage and fertility schedule of the host country depends on a host of differences between sending and receiving country, which may extend over generations (Fernández and Fogli 2009). Further, the degree of intermarriage between migrants and natives also reflects integration into the host society. Such intermarriage may also reflect immigrant group size and access to culturally similar marriage partners (e.g., Qian and Lichter 2018). Alignment to the fertility and marriage schedule of the host country can ultimately be seen as markers of assimilation (Adserà and Ferrer 2015). At the same time, marriage and fertility decisions are intrinsically linked to educational attainment and labor market activity, such that changes to one of these dimensions likely spill over into the other (e.g., Alderotti et al. 2021; Arendt et al. 2021; Kleven et al. 2019; Nitsche et al. 2018).

In this paper, we study how delaying and foregoing marriage among women of non-EU background in Denmark—a population group generally prone to marrying early—affect subsequent fertility behavior and human capital accumulation. In July 2002, a series of reforms limited the marriage opportunities for all Danish and non-EU citizens living in Denmark, who wished (or whose parents wished for them) to marry a person from outside the European Union. A declared aim of the reforms was to reduce arranged marriages between a migrant living in Denmark and a spouse from the country of origin (Jørgensen 2014). First, one reform prohibited family reunification between a person residing in Denmark and a spouse from abroad if any of the two partners was younger than 24 years of age. Because most people who were under 24 and found a spouse abroad were first- or second-generation immigrants to Denmark, this reform predominantly affected this group. Second, another reform required that couples had to prove a stronger (early just: as strong) affiliation to Denmark than to other countries for a spouse to gain residency in Denmark. From start of 2004, the

requirement was waived for people who had been Danish citizens or lived legally in Denmark for more than 28 years. These reforms substantially limited younger Danish residents' ability to enter marriage with non-residents (Schultz-Nielsen and Tranæs, 2010).

The decision to marry may have direct implications for subsequent (absence of) educational attainment (Blossfeld and Huinink 1991; Lawrence and Breen 2016; Oppenheimer 1988; Raymo 2003; Sabbah-Karkaby and Stier 2017)—especially in population with a higher probability of marrying young (such as people with Middle Eastern, North African, and Pakistani [MENAP] country background). Marrying young has been seen as a consequence partly of culture (e.g. Sabbah-Karkaby and Stier 2017) and partly of the absence of good alternatives (e.g. Waite and Spine 1981). This study provides evidence for an immigrant population of what happens when the standard temporal relationship between marriage and education is disrupted when the marriage decision becomes subjected to exogenous shocks. From a policy perspective, we consider the consequences for fertility and educational attainment of restricting and delaying the marriage decision among young immigrants in Denmark. From a theoretical perspective, our study considers how relaxing the assumption that human capital decision is made prior to the marriage decision changes our understanding of the relationship between human capital, marriage and fertility.

We find that the reform indeed delayed marriage and increased non-martial cohabitation. After accounting for already ongoing trends, those affected by the reform also decreased transition to motherhood, with some indications of an increase in births out of wedlock. We also find plausible evidence of an increase in educational attainment. The paper proceeds as follows: first we discuss the theoretical background and contribution of our study. We then provide an overview of recent Danish migration history and policy. We then introduce the data and analytical strategy. Following the results section, we provide a discussion of the implications of our empirical findings.

Family Formation, Education, and Migrant Background

Migrants bring with them a different life course script and different traditions, which both reflect upon the marriage markets they are active in and the partners they choose. Whereas such practices represent identity and history, they may also pose barriers to integration and assimilation into host society, especially for women if migrating from a more patriarchal context to one less so. Maintenance of distinct practices of intra-marriage have often been tied to the size of migrant groups (Blau 1977) and the strength of group identification and social sanctions for going outside the group borders (Kalmijn 1998). Practices and barriers to intermarriage tend to erode as time in host country increases (Chiswick and Houseworth 2011; Fernández and Fogli 2009). Further, the incorporation of family formation practices of the host country has been taken as a clear sign of integration and assimilation (Adserà and Ferrer 2015).

Especially for women, educational attainment delays marriage, increase intermarriage, and affects fertility (Blossfeld and Huinink 1991; Chiswick and Houseworth 2011; Lawrence and Breen 2016; Oppenheimer 1988; Raymo 2003; Sabbah-Karkaby and Stier 2017). The marriage (and fertility) delay comes from the 'incapacitation' effect of being enrolled in an educational institution, whereas increased intermarriage likely both occurs through exposure to a new marriage market (Kirkebøen et al. 2021) as well as the acquisition of host-country specific capital, which makes migrants more likely to intermarry with out-group partners (Chiswick and Houseworth 2011). Thus, a clear causal sequence described in the literature connects educational decisions to subsequent family formation.

Yet, scant literature examines the consequences of intra-marriage markets being constrained by an out-group partner, such as a state actor. Increased educational attainment delays marriage, but does constraining migrants' traditional marriage markets increase intermarriage, educational activity, and subsequent returns to human capital? This question has received much less consideration because the educational decision often is made prior to the marital decision in people's life course. Yet, for

population groups who tend to marry young, forgoing a (early) marriage (and thereby also early childbearing) may pave the way for increased educational attainment. In one study considering the same policy reform we consider, Nielsen et al. (2009) found that foregoing (early) marriage to a marriage migrant decreased educational dropout of young men with a migrant background, but had no detectable effect for women. While the results in Schultz-Nielsen and Tranæs (2010) suggest a positive effect on educational enrolment of the reform for both young immigrant men and women, but results are sensitive to the inclusion of time-trends. However, beyond that, studies of how shocks to (intra-)marriage markets affect family formation and education has mainly considered shocks in more traditional settings, such as changes to dowry and mehr payments (e.g., Chowdhury et al. 2020; Corno et al. 2020), historical cases (e.g. Eriksson et al. 2022), or constraints imposed by events such as mass-incarceration (Charles and Luoh 2010) and deindustrialization (Autor et al. 2019). In this study, we instead consider the consequences when entire population groups prone to intramarry lose access to their main channel of acquiring spouses, marriage migration, in contemporary Denmark. In the following section we present this context in detail.

Recent History of Migration to Denmark and Danish Migration Policy

During the first 60 years of the 20th century, Denmark on average saw more emigration than immigration most years, with immigrants predominantly arriving from the rest of Scandinavia, Germany, the UK, and North America (Matthiessen 2009). However, from 1960 and onwards, most years saw net-positive inflow of migrants, who increasingly originated from outside the Nordic and EU member states (from here on we refer to these as EU countries), and North America. In 1974, a total of 89,855 foreign citizens resided in Denmark, with 56 % originating from the Nordic countries, EU member states, and North America. In 2000, the number of foreign citizens residing in Denmark had increased to 290,490 with 30 % originating from the EU countries and North America. From 1974 to 2000, the Danish population grew with 6 %, but the migrant population

grew by 323 %, and Denmark received more immigrants per capita than, for example, Sweden and Germany. In 2002, the largest migrant and descendant groups outside the EU countries originated from Turkey, Iraq, Lebanon, (former) Yugoslavia, Pakistan, Somalia, Iran, and Vietnam. Further, prior to 2002, 80% of male and 70% of female non-EU migrants and descendants aged 18-25 with at least 10 years in Denmark who married found spouses in their parents' country of origin.

Thus, substantial amount of immigration to Denmark occurred through marriage- or chain-migration, and it has (together with asylum seekers and refugees) been the most important driver of permanent migration to Denmark from 1973 and until the early 2000s (Bauer et al. 2004). The right to family reunification is based on the principle of family unity and the right to family life, which stems from conventions on human rights (Christensen et al. 2006, p. 127). The rules for family reunification in Denmark are regulated through the Aliens Act, section 9. For family reunification with spouses, the person living in Denmark must be a citizen of Denmark or a Nordic country, a refugee, or a foreigner who has held a permanent residence permit for Denmark for a specified number of years, which have varied over time. Prior to 2000, Danish family reunification policy saw a gradual tightening from being considered having one of the most humanitarian refugee policies in the world in the beginning of the 1980s to gradually introducing more requirements for family reunification (Bauer et al. 2004). However, the early 2000s represented a watershed moment, whereas series of reforms curtailed general possibilities for migration, and especially the possibilities for marriage migration. The main changes in the rules regarding family reunification for spouses in the period 2000-2010 are listed in Table 1.[1] For an overview of other changes in the Aliens Act, see Hvidtfeldt and Schultz-Nielsen (2018).

[1] Under this regulation cohabiting partners during a period of at least 1½ year are treated as spouses.

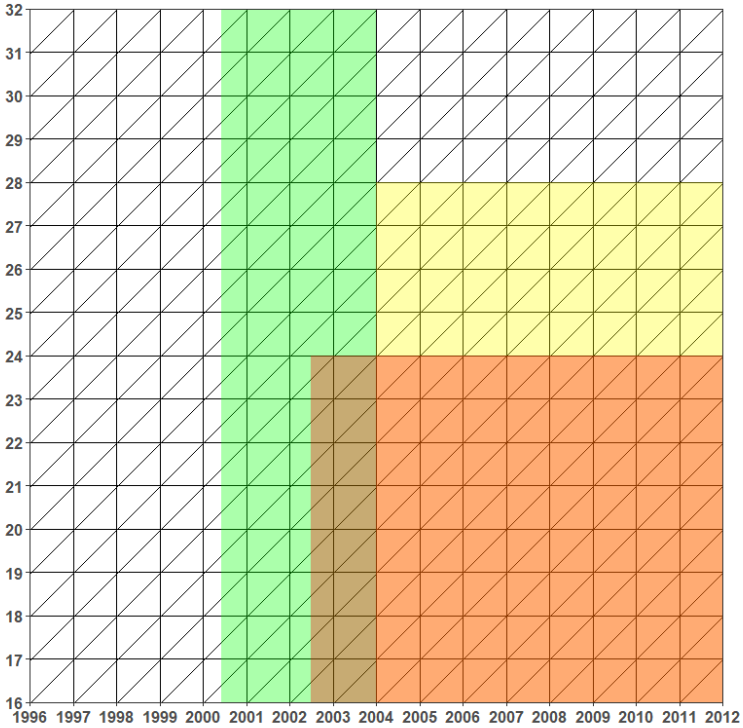
Table 1: Key Changes to Danish Family Reunification Policies from 2000-2012

Law number	**Name**	**Date passed**	**Date in force**	**Content**
L424	*The attachment rule*	31/5/2000	2/6/2000	Total degree of attachment to Denmark for two spouses should be at least as strong as their attachment to another country. The rule only applies for non-Danish residents.
L365	*Strengthened attachment rule*	06/6/2002	7/6/2002[a]	The total degree of attachment to Denmark for the two spouses should be *stronger* than their attachment to another country. The rule also applies to Danish citizens.
L365	*The 24-year rule*	06/6/2002	7/6/2002[a]	Both spouses have reached 24 years of age before family reunification can take place with a partner from outside EU and the Nordic countries. Similar rules apply for cohabiting partners.
L1204	*The 28-year rule (exception from attachment rule)*	27/12/2003	1/1/2004	Exception from the attachment rule if the spouse resident in Denmark have been a Danish citizen or have lived in Denmark for 28 years

[a]De facto not enforced for applications for family reunifications handed in before 1/7/2002.

These four key legal changes affected different cohorts at different ages, which we illustrate using a lexis surface in Figure 1. Calendar time is presented at the horizontal axis and age at the vertical axis, while the diagonal shows the events over time for a given cohort. The white area illustrates the marriage regime before July 2000, where neither "attachment rule" nor "24-year rule" restrict the family reunification for anybody. From July 2000 the attachment rule is implemented and affect all regardless of age in the following years (marked with green). The 24-year rule are implemented from July 2002 and affect all under the age of 24 (marked with orange). At the same time the attachment rule is strengthened. From January 2004 the exception from the attachment rule (the 28-year rule) is in force and only those under the age of 28 (marked with yellow) are subject to the attachment rule.

Figure 1: Lexus surface showing reforms affecting migrants and descendants' marriage market in Denmark



Note: Green shows the impact of the attachment rule. Orange shows the 24-year rule. Yellow shows the 28-year rule.

Thus, different birth cohorts were subject to different amount of time under the various policy regimes. Further, whereas the policy changes surround the spouses' attachment relied on an evaluation by civil servant about whether a potential couple is more attached to Denmark than to somewhere else, the 24-year rule sets a clearly defined cutoff for when people was affected by the reform and when they were not. Qualitative evidence further suggest that the 24-year rule is well-known among young immigrants in Denmark, while the attachment rule is less known (Schmidt et al. 2009). Previous work utilizing the same reform has generally shown that the 24-year rule likely provided the key shock to the marriage market for immigrants and descendants in Denmark (Andersen et al. 2021; Nielsen et al. 2009), and that the surrounding policies had much smaller impact (see also Figure 2 below). We extend the evidence below by demonstrating how the

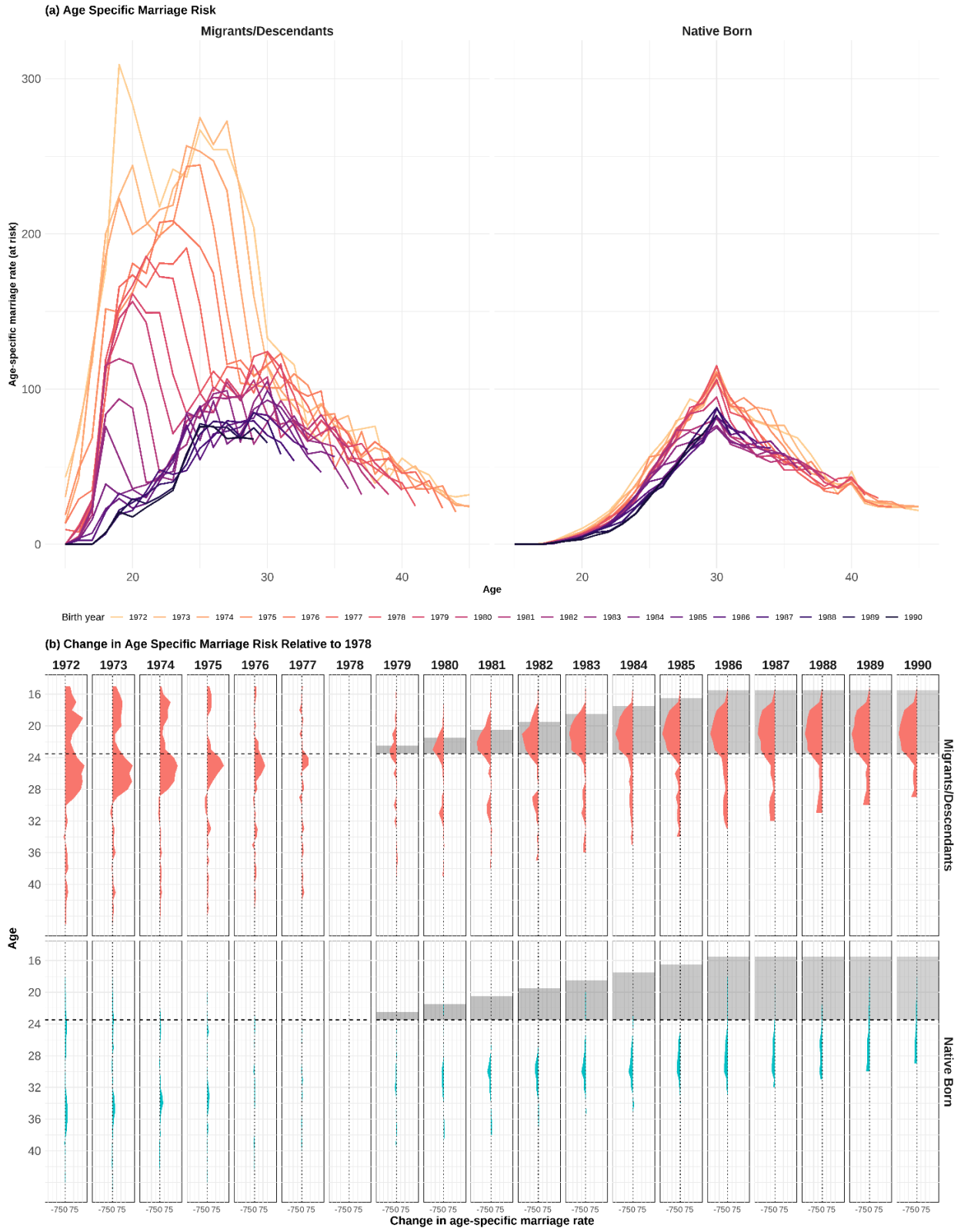
marriage rate changed for different age groups among non-EU migrants and descendants in Denmark and that the substantial change coincided with the reform.

The reform impact on marriage rates

Figure 2 shows the age-specific marriage rates for first- and second-generation non-EU Immigrant women compared to ethnic Danish women, as well as the relative change in marriage rate compared to women born in 1978, the last cohort not affected by the 24-year rule. The left side of the first panel of the figure shows that across birth cohorts, age at marriage increased and marriage rates generally declined for non-EU first- and second-generation migrant women (see also Figure A1 in appendix). Comparatively for the right side of the first panel, while native born women also saw a decline in marriage rates, this occurred at older ages/later and to a lesser degree. The second panel shows the relative change in age-specific marriage rates compared to the 1978 cohorts, which was the last cohort to not be affected by the 24-year rule. For cohorts born prior to 1978, there was a general decline (see also Figure A1 in appendix). For the 1978, the grey square shows the part of the life course until 24 that a given cohort was affected by the reform. As can be seen, the marriage rate declines for first- and second-generation non-EU migrant women coincided with the part of their life course, where they were covered by the reform. Contrastingly, native born women did not see a decline in their marriage rate before much later—a decline that coincided with the onset and aftermath of the great recession in 2008-09, where marriage rates in Denmark declined with 30 % within two years[2]. Further, there is no indications that the earlier reform in 2000 or the later reform in 2004 had any substantial impact on marriage rates for first- and second-generation non-EU female immigrants.

[2] Own calculation on public data from Statistics Denmark: www.statistikbanken.dk/VIE307.

Figure 2. Age-Specific Marriage Rates and Relative Change Measured Against 1978 Cohort for Female Migrants/Descendants and Native Born for Cohorts Born 1972-1990



Note: Grey bar indicates the part of the life course lived under the 24-year rule. Marriage rate measured as # new married women during the year per 1,000 unmarried women at start of year.

Further, figure A1 in appendix reports the annual marriage rate for first- and second-generation immigrants in Denmark affected differentially by the reforms across age as well as the share married by age 24 for both men and women. The marriage rate for both men and women declined drastically at the 2002 reform year, with the marriage rate for both men and women younger than 24 years of age declining with 60-65% from 2001 to 2003, and the marriage rate for the 24-27 years old declining with 50%. For women 28-42 years old, no discrete change emerged, whereas men older than 28 did see a small discrete change as well, likely due to the tendency for men to marry women some years younger than them. Hence the focus on women in this study.

Data and Methods

Data

We use full population deidentified administrative data supplied by Statistics Denmark. All residents in Denmark are assigned a unique personal identification number at birth or day of immigration that allow us to follow people over time, and link information on demographics, education, and labor market affiliation. Data include information on migrant/descendant status, year of migration, and own/parents' country of origin. We include cohorts born 1972-1990. Those born before July 1, 1978 were not subject to the 24-year rule, while those born from July 1, 1984 and onwards were subject to the rule from their 18^{th} birthday and until their 24^{th} birthday.

Children born in Denmark to parents where neither are born in Denmark and Danish citizens are viewed as descendants until at least one parent born in Denmark becomes Danish citizen. Children born outside Denmark to parents where neither were both Danish citizen and born in Denmark are viewed as immigrants. Children born to at least one parent born in Denmark who holds Danish citizenship is viewed as native born. We exclude immigrants and descendants originating from inside the EU and Nordic countries from the main sample, because the 24-year rule did not affect

spousal migration from these countries. We exclude first-generation immigrants arriving after their 15th birthday, to not include potential marriage migrants in the sample. From the data we obtain age at migration (for first-generation migrants) and country of origin for first- and second-generation migrants. If parents have different non-EU countries of origin, we assign mothers country of origin for second-generation migrants. Table 2 shows the distribution of origin in the sample across birth cohorts. Not only is the share of the population made up of women with migration background increasing across cohorts, but descendants are also increasingly accounting for more of the share with migration background. Table A1 in appendix shows the distribution across country of origin.

Table 2: Number of women in sample across non-EU migration background and birthyear

Birthyear	Native Danes	1st gen migrant	2nd gen migrant	Share of cohort made up by 1st/2nd generation
1972	36257	311	149	0.013
1973	34238	366	240	0.017
1974	34177	428	317	0.021
1975	34677	479	369	0.024
1976	31109	532	378	0.028
1977	29676	605	401	0.033
1978	29596	663	510	0.038
1979	28435	597	522	0.038
1980	27397	742	595	0.047
1981	25486	873	603	0.055
1982	25233	910	577	0.056
1983	24461	1050	526	0.061
1984	24791	1143	594	0.065
1985	25887	1168	682	0.067
1986	26258	1232	829	0.073
1987	26373	1214	1029	0.078
1988	27630	1261	1064	0.078
1989	28920	1228	1225	0.078
1990	29748	1315	1304	0.081

Source: Own calculation on data from Statistics Denmark

Outcome variables

The first outcome we consider is the probability of being married at each age between 25 and 32, with information obtained from the Danish Population Register, which holds daily level data on marriage date. Given the reform mechanically limited the marriage market for those under 24 years of age, we wait until age 25 to see if this only caused short term postponement, or whether the consequences of the reform were more long felt. To examine whether the composition of spouses also changed, or people just forewent marriage, we also include three joint outcomes: being married and a) having a spouse who is a Danish native; b) having a spouse born in Denmark or who migrated to Denmark before turning 15; c) having a spouse born in EU. Further, we consider alternatives to being married: cohabiting in a non-married union and living with parents. To capture cohabiting unions, we rely on dwelling level data from Statistics Denmark and use their definition of cohabitation: two people of opposite sex who are living together, unmarried, and either i) have a child together; or ii) are not related by blood, the only two adults in the dwelling, and within 15 years age of each other.

Second, we consider transition to parenthood. Using data from the Danish population database, we consider the outcome at each age between 25 and 32 years with a binary indicator for whether the women had had at least one child at the given age. Besides general transition to parenthood, we further consider the probability of having a child while not married as an additional outcome.

Third, we consider permanent investment in human capital captured as months of completed education. Using data from the Education Registry, we transform the highest obtained clearing houses approved educational degree a woman has into the number of months of fulltime study it would take to attain that degree. For example, a Danish high school degree would be 144 (12*12) months of education, and an undergraduate degree would be 180 (12*15) months of education. Table A2 in appendix reports outcome statistics across age, birth cohorts and immigrant status.

Analytical Strategy

Different birth cohorts spent different share of the early adult life under the 24-year rule (as seen from Figure 1). People born before July 1, 1978 spent none of their first 24 years of life affected by the 24-year rule, whereas each day born later than July 1, 1978 resulted in one more day under the policy until reaching those born June 30, 1984 or later who spent the full first six years of adulthood under the policy. We approach this as a dosage treatment (later treated receives a larger dosage of the policy), but we relax linearity assumptions about the effect of dosage. As a comparison group, we use native born Danes who likely were least affected by the policy change, given they generally do not find spouses from non-EU countries. We employ a difference-in-differences design where we compare difference between birth cohorts and between women with a migration background and native Danish born. To do so, we run the following regression:

$$y_i^A = \alpha_0 + \boldsymbol{BY_i\beta} + Migrant_i \times \boldsymbol{BY_i\gamma} + \boldsymbol{Generation_i\delta} + \boldsymbol{Age\ at\ entry_i\varphi} + \boldsymbol{Country\ of\ origin_i\rho} + \epsilon_i \quad (1)$$

where y_i^A is the outcomes discussed above for each woman *i* at age *A*, ***BY*** is a vector capturing birthyear, *Migrant* is indicator of being either first- or second-generation migrant, ***Generation*** is a vector distinguishing between first- and second-generation migrants, ***Age at entry*** is a vector that is 0 for everyone born in Denmark and between 0 and 14 for all first-generation migrants, and ***Country of origin*** is a vector of non-EU countries. If $Migrant_i \times \boldsymbol{BY_i}$ is conditionally independent of the error term, then the parameter vector $\boldsymbol{\gamma}$ describes the impact of the reform of the outcome and should only be zero for cohorts born before July 1, 1978. We do not distinguish between reform effects for first- and second-generation migrants. We do not include a migrant main effect, because

this is captured by the ***Generation*** vector. The results expressed by $\boldsymbol{\gamma}$ are all captured relative to the baseline behavior of the non-migrant control group.

Threats to identification

Our analytical strategy faces some threats to identification. First, the share of migrants as part of the full sample increases across birth cohorts and the composition between first and second generation also changes (see Table 2). Further, the composition of country of origin in the migrant group also changes (Schultz-Nielsen and Tranæs 2010). To account for the changing composition of migrants, we control for both first generation immigrant/second generation (descendant) status, age at arrival for first-generation migrant, and country of origin.

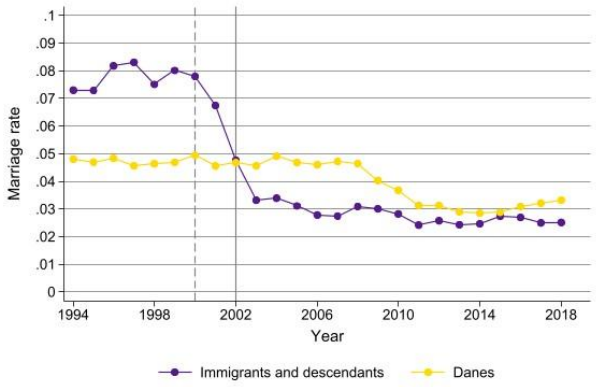
Second, people may choose to leave Denmark to marry a spouse from outside the EU. This will induce collider bias because people prone to marry may also be more prone to leaving the sample. In a study of outmigration following the 24-year rule policy, Bratu et al. (2020) document an increasing rate of outmigration from Denmark to the neighboring country of Sweden. Annual outmigration increased from around 3 to around 4 % among Danish citizens with migrant background, with the migration increase completely driven by increased moves to Sweden. Within 8 years more than half of all that out-migrated had returned to Denmark. The increase in outmigration was concentrated among migrants living near Copenhagen, which is connected by bridge to Sweden as of year 2000. To ensure our results are not driven by selection out-of-sample, we re-estimate main regressions excluding people living on the island where Copenhagen is located. Results are robust to exclusion of this part of the country.

Third, diverging trends in outcomes between women with migrant background and women born to native Danish parents prior to the reform will challenge identification. First, given Denmark from the 1970s has seen large-scale immigration from outside the EU, processes of integration, social

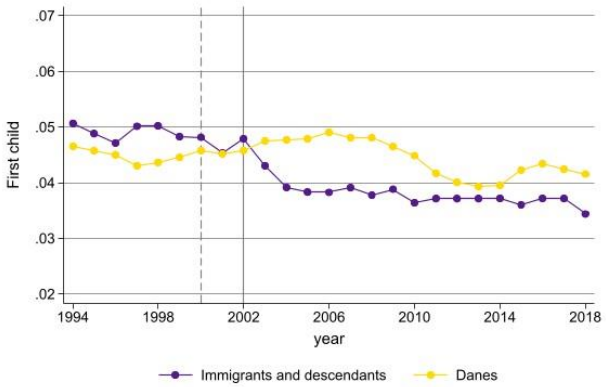
acclimatization, and assimilation are likely still ongoing, and may affect intermarriage (Schwartz 2013), fertility patterns (Adserà and Ferrer 2015), and human capital accumulation (Adserà and Ferrer 2015). Thus, secular trends could be ongoing and present a challenge to identification because it would violate the parallel trends assumption underlying the identification strategy. Top panel of figure 3 in shows the marriage and first birth rates for 18-32-year-old women in Denmark across migration background.

Figure 3. Marriage and First Birth Rates for 18-32 Year Old and Educational attainment at 25 Years of Age across Migration Background

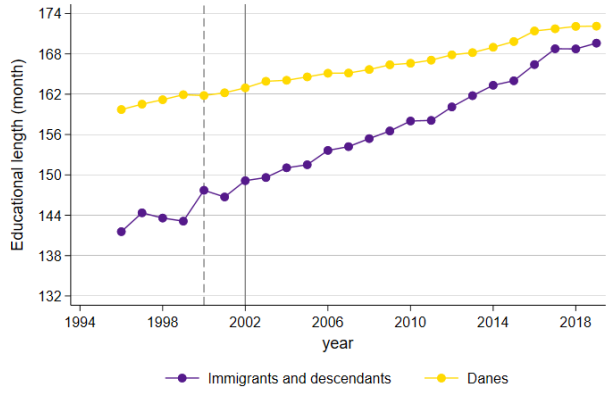
(a) Transition to marriage



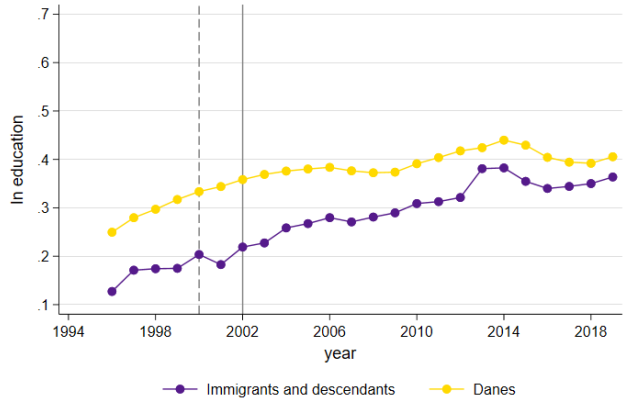
(b) Transition to first birth



(c) Educational attainment at 25



(d) In education at 25



Source: Own calculations on data from Statistics Denmark.
Note: Full line in designates implementation of 24-year rule. Dashed line designates implementation of the attachment rule.

The figure shows that prior to the 24-year reform, there is no indication of different trends including following the attachment rule introduction in 2000. The year of the introduction of the reform

(which occurred July 1, 2002), marriage rate declined drastically and then settled at a new, lower level, in 2003. Further, there is no indication of a dynamic effect in the years following the reform. However, the figure highlights a different issue—the great recession lowered marriage and birth rates (see Comolli et al. 2021 for a discussion of the latter), but only appear to do so for native born Danes. Thus, estimates of the effect of the reform are likely to be biased towards zero for marriage and fertility outcomes measured at older ages.

Bottom panel of Figure 3 shows educational length and share in education, both captured at age 25. For educational length, there is some evidence of differences in trends across groups prior to the reform. Whereas this may mainly reflect compositional changes over time, it could also reflect ongoing processes of integration. To take this into account, we present results both from the regression shown in Eq. 1, as well as an analysis where we detrend the education outcome (Bilinski and Hatfield 2019):

$$y_i^{\mathrm{A}} = \alpha_0 + \boldsymbol{BY_i\beta} + \gamma_y Migrant_i \times \sum_{y=1978}^{1987} I(BY = y) + \tau BY_i \times Migrant_i + \boldsymbol{Generation_i\delta} + \boldsymbol{Age\ at\ entry_i\varphi} + \boldsymbol{Country\ of\ origin_i\rho} + \epsilon_i \quad (2)$$

where τ is a linear time trend for migrants and γ_y is birth year dummies for all cohorts who spent at least some of their years before turning 24 under the 24-year rule.

Alternative identification of reform effect

The reform occurred both at a specific point in time and differentially affected specific cohorts. As such, it can be viewed as both a cohort and period level treatment for immigrants and descendants' probability of marrying, and thus can be seen as cohort and period effects within an age-period-cohort (APC) framework. Fosse and Winship (Fosse and Winship 2019a, 2019b) have shown that the APC problem is a linear-in-means problem, and that nonlinear effects, such as those caused by policy shocks to specific cohorts at specific points in time, are readily identifiable in observational

data. For the sample of migrants/descendant, we residualize the probability of being married by regressing it on migrant/descendant indicator, country of origin, and age at migration to account for changing composition over time. For the sake of brevity, we present the APC model results in the appendix.

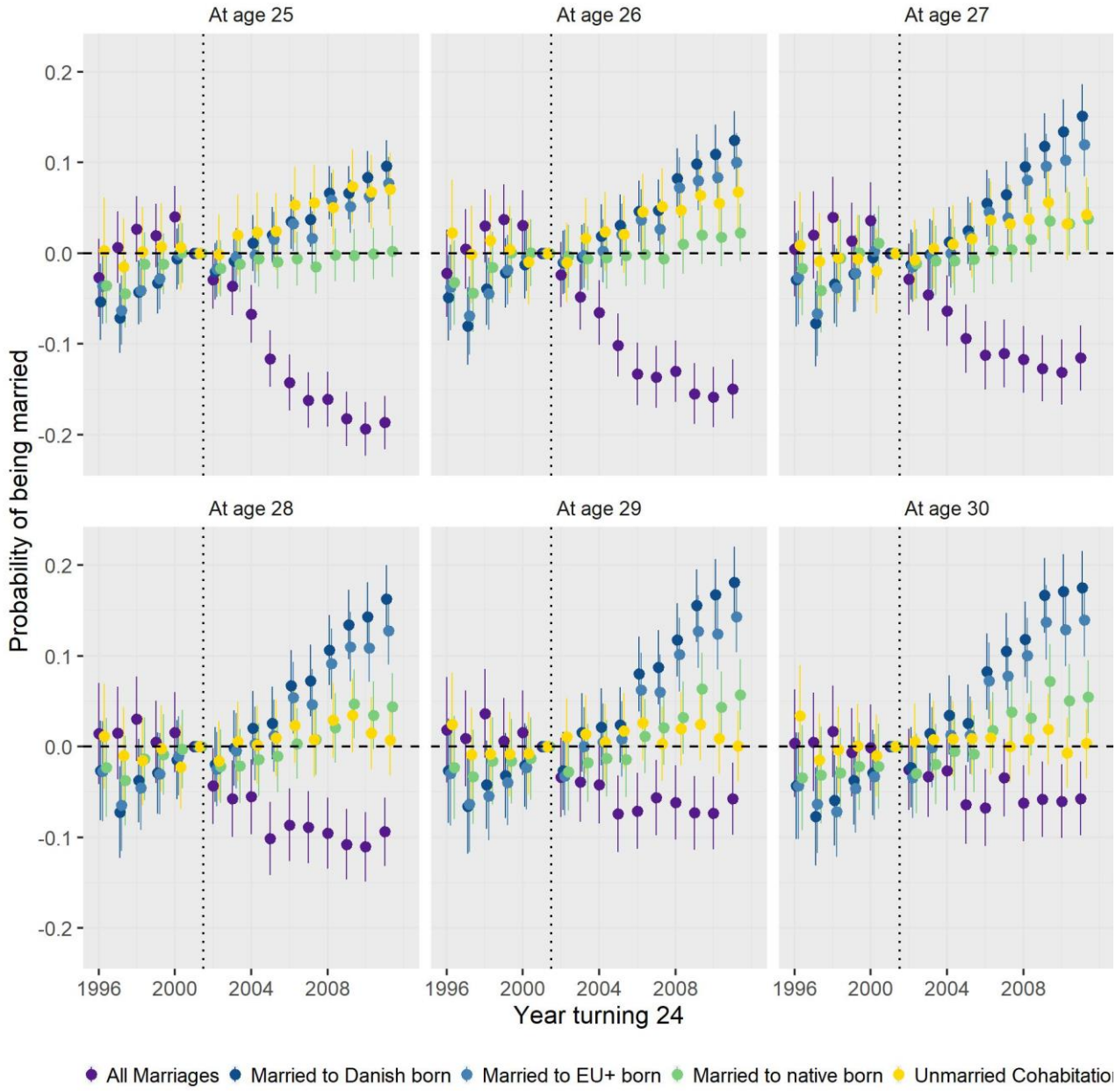
Results

Marriage behavior

Figure 4 provides results on the marriage outcomes at each age between 25 and 30 captured relatively to the year that the women turned 24 (x-axis). The dashed vertical line indicates the introduction of the 24-year rule. All estimates should be interpreted as changes relative to the baseline cohort (those turning 24 in 2001) and the development in the ethnic Danish population from the same cohorts. The top panel examines the probability of being married, as well as the probability of being married to a spouse who is a) Danish native ('native born'); b) born in Denmark or who migrated to Denmark before turning 15 ('Danish born'); c) born in EU ('EU born'). First, the probability of being married declines following the introduction of the reform. At age 25, women who spent the entire first six years of the adulthood (turning 24 in 2010) under the 24-year rule saw close to twenty percentage-points decline in the probability of being married relative to women who turned 24 before the introduction of the reform. At age 30, they still had a six percentage-points lower probability of being married. From the pre-treatment cohorts, there is no evidence of any trends prior to the introduction of the 24-year rule. The estimates for nonlinear effects for age, period, and cohort are presented in Appendix Figure A2. Figure A2 in appendix provides evidence on the nonlinear impact of the reform on age-, period-, and cohorts-effects among immigrant and descendant women. The figure shows that the reform both provided an immediate period shock in 2002 (and that no indications of a shock occurred in 2000) and a shock

to the probability of being married beginning with the cohort born in 1980. Thus, the APC-results align with the general finding of women both postponing and foregoing marriage (at least in their 20s).

Figure 4: Results for probability of being married, spouse composition, and alternate living arrangements behavior across birth cohorts comparing immigrants/descendants relative to native born

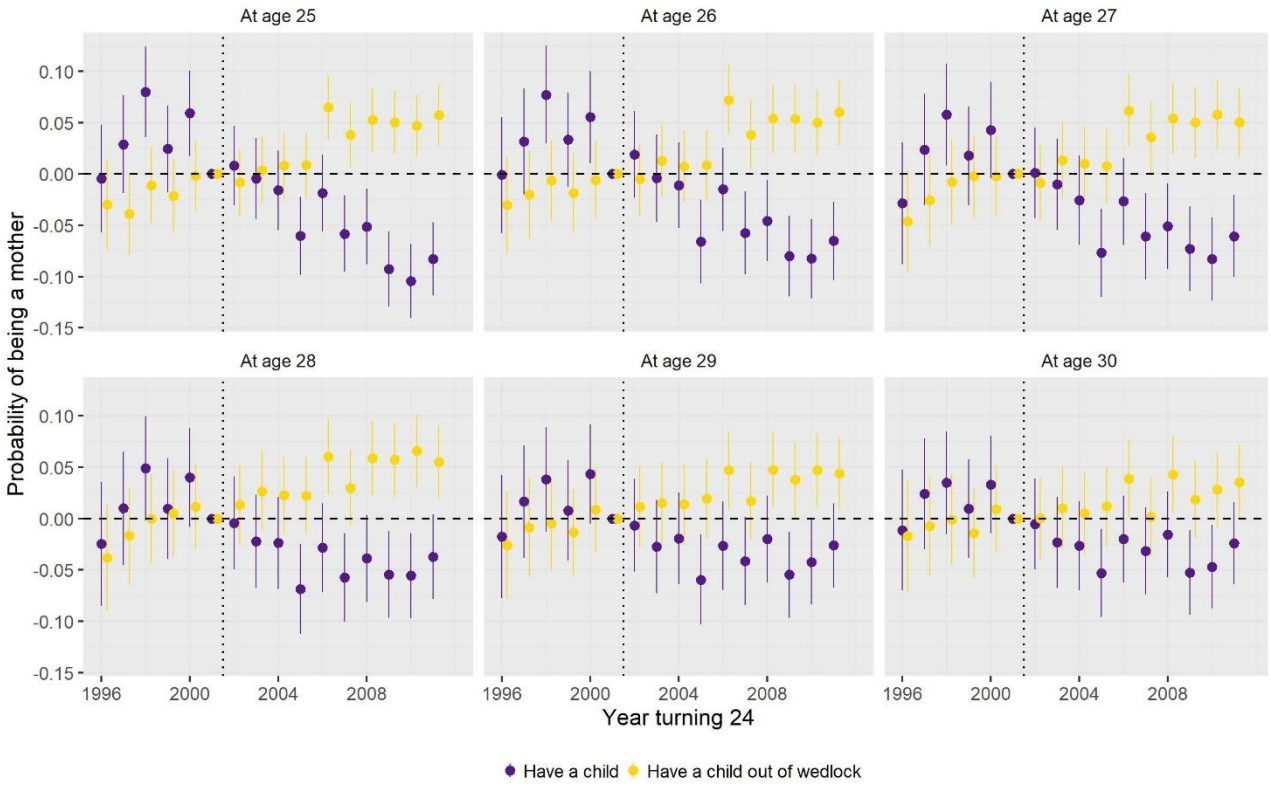


Note: Own calculations on data from Statistics Denmark. Results adjusted for age at migration and country of origin. 95% confidence intervals.

The composition of spouses among those who still married also appears to change following the reform. The probability of marrying a spouse born in Denmark increased, with marriage to spouses born to native born Danish parents ultimately accounting for up to a third of the increase, with the remaining two-thirds being accounted for by marriage to a spouse born in Denmark to immigrant parents. Effects on the probability of bringing in a spouse from any EU country including Denmark were practically identical to results for finding a spouse born in Denmark, meaning people shifted to a national and not a European marriage market.

Yet, for both national marriage outcomes, there are indications of pre-reform trends. Figures A3 and A4 in appendix examine the outcomes trend-adjusted, and Table A3 reports the parameters for the trend-adjustment. The trend-adjustment is significant for the probability of marrying a partner born in Denmark at all ages, but only at ages 25 and 26 for marrying a native born. The trend-adjustment explodes standard errors and some parameter estimates become negative. In total, it cannot be concluded that the 24-year rule let to a change in spousal composition. For alternative living arrangements, there is no indication of any pre-reform trend in cohabitation or still living at home. Here is an increase in the probability of cohabitation at younger ages that decreases as the women grow older and postponed marriages are caught up. APC-results (Appendix Figure A2) also indicate shocks to both period and cohort nonlinear effects, further corroborating the conclusion that cohabitation increased with the introduction of the 24-year rule. Further, results are also robust to excluding people residing on the island of Zealand which house the Capital region and which saw the largest outmigration to Sweden following the 24-year rule's implementation (Figure A4 in appendix).

Figure 5: Results for fertility outcomes across birth cohorts for immigrants/descendants estimated relative to native-born women



Note: Own calculations on data from Statistics Denmark. Results adjusted for age at migration and country of origin. 95% confidence intervals.

Fertility

Figure 5 report the results for the probability of having transitioned to motherhood, as well as the probability of having a child out of wedlock without adjustment for pre-reform trend. The probability of having become a mother decreased substantially, with women fully covered by the reform being on average around 10 percentage point less likely to be mother at age 25 than women not affected by the reform were. There is no significant pre-trend for entrance into motherhood (see Table A2 in appendix), and point estimates remain of similar size after trend-adjustment (Figure A5 in appendix). The impact on the probability of being a mother declines across age, but some decrease remains at age 30, with parameters likely being biased towards zero due to the impact of the great recession on native-born women's fertility (cf. Figure 3b). For being a mother while

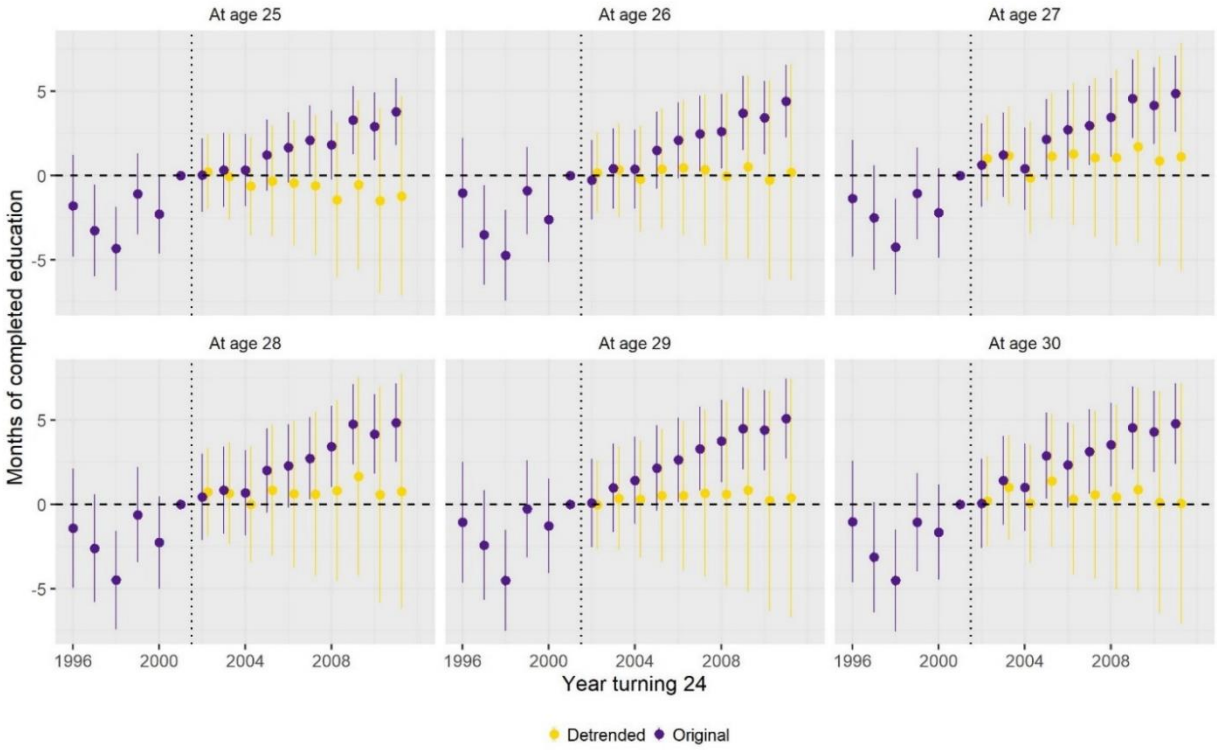
unmarried, the results show at face-value an increase in the probability, but for younger ages (25 and 27) there is significant pre-trends (cf. Table A2), and after adjusting for pre-trends point estimates move downwards and are not different from zero (cf. Figure A5). Thus, entrance into motherhood declined following the 24-year rule, and there are some indications of an increase in out of wedlock fertility.

Educational Attainment

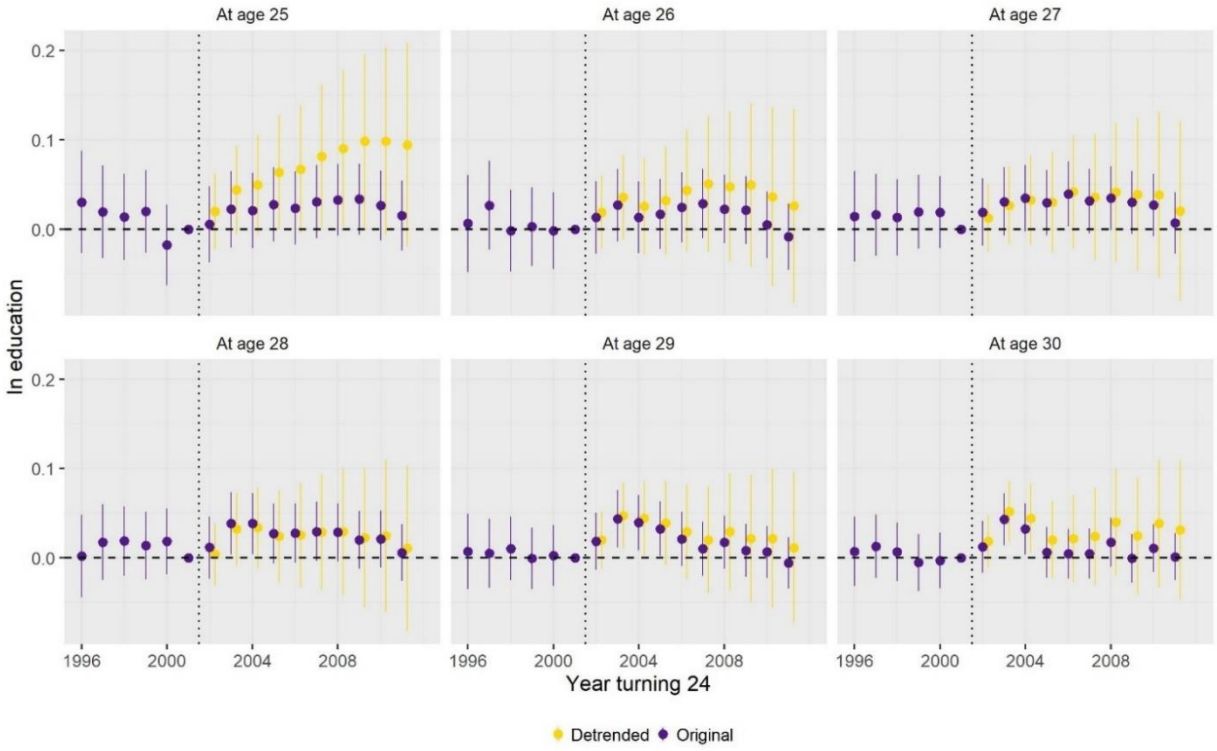
Figure 6 reports the results for length of accredited education in months (top panel) and the probability of being enrolled in education (bottom panel). We show results with and without detrending. For educational length, assuming similar trends, we consistently see higher levels of completed education among the birth cohorts who spent more of their early adulthood under the reform. Once detrended, standard errors explode, and estimates move close to zero. However, we only find evidence of significant difference in trends at age 25 and 26, but not at higher ages (Table A3 in appendix). Thus, we do see indications that constraining access to spouses from outside the EU increased educational attainment. Bottom panel provides further evidence, using an indicator of being in education. Although the women in our study are measured at age 25 and above, we still see a clear trend—the likelihood of still being in education is higher at ages 25-27 (although not always significantly so).

Figure 6: Results for educational length and still being in education across birth cohorts comparing immigrants/descending relative to native-born women with and without detrending

(a) Educational length



(b) In education



Note: Own calculations on data from Statistics Denmark. Results adjusted for age at migration and country of origin. 95% confidence intervals.

Discussion and Conclusion

The literature on women's marriage postponement have predominantly focused on how increased educational enrollment and shocks to the value of those on the local marriage market affect age at and probability of marriage. In this article, we have taken a different approach—we studied what happened to marriage probability, fertility behavior, and human capital when the size of a group's marriage market became constrained by outside forces among women who often marry young. Not surprisingly, when the marriage market became constrained, women's probability of marriage declined and age at marriage increased. They also changed relationship behavior—non-marital cohabitation increased, there were some indications that women shifted their focus to the national marriage market, and fertility transitions became delayed. Lastly, educational length also increased. Thus, postponing marriage likely increased education in the group of non-EU migrant women considered in this study as well as possibly allowed women access to new marriage market through educational attainment.

However, although likely increasing education, postponing people's marriage and fertility transitions through marriage-market limiting policies may have drawbacks beyond the immediate discriminatory and paternalistic aspects. Ultimately, people may see their marriage market position weaken as they grow older, which may mean they either completely forego marriage or have to settle for a more poorly matched spouse (Oppenheimer 1988). As a counterpoint, removing the option of bringing in a marriage migrant partner may hasten integration and assimilation into the host society, because it provides an exogenous excuse for finding a partner outside the group wherein search behavior traditionally is enforced.

Our study does come with limitations. First, our study period covers the period during and after the Great Recession. The recession had substantial impact on both marriage behavior (Figure 3a) and fertility (Figure 3b and Comolli et al. 2021) behavior concentrated among ethnic Danes, which

means that the negative effect of the 24-year rule on marriage and fertility likely are underestimated at higher ages/later periods. Second, for several outcomes, the parallel trends assumption underlying the identification strategy was rejected. Although we addressed this by providing evidence from detrended regressions, the detrending relies on correctly specifying the functional form of the trend line. Given the limited number of cohorts studied, we believe relying on linear trends suffices, yet we cannot rule out the presence of nonlinear trends completely. Last, our sample may be compromised by cohort-dependent outmigration because of the policy. If those most likely to marry migrated at high rates, it could produce the findings we observe. Bratu et al. (2020) demonstrated the although outmigration occurred, it was geographically confined to the Greater Copenhagen area in Denmark, and most people returned within the period covered in this study. Further, results hold when only considering parts of Denmark not affected by outmigration. Yet, future work should consider also aiming to incorporate changing migration patterns when shocks to marriage markets occur.

In conclusion, we have demonstrated that migration policies not only affect the inflow of migrants to a society, but also the family formation behavior of migrants already in country. Migrants often arrive with different life scripts than those of the host society. Limiting access to out-of-country marriage markets by tightening rules around marriage migrants delay union formation and force marriage age individuals to search within the local market. Doing so fosters increased assimilation in terms of union formation patterns but comes at the cost of constraining choices of parts of society based of their ethnic/national origin.

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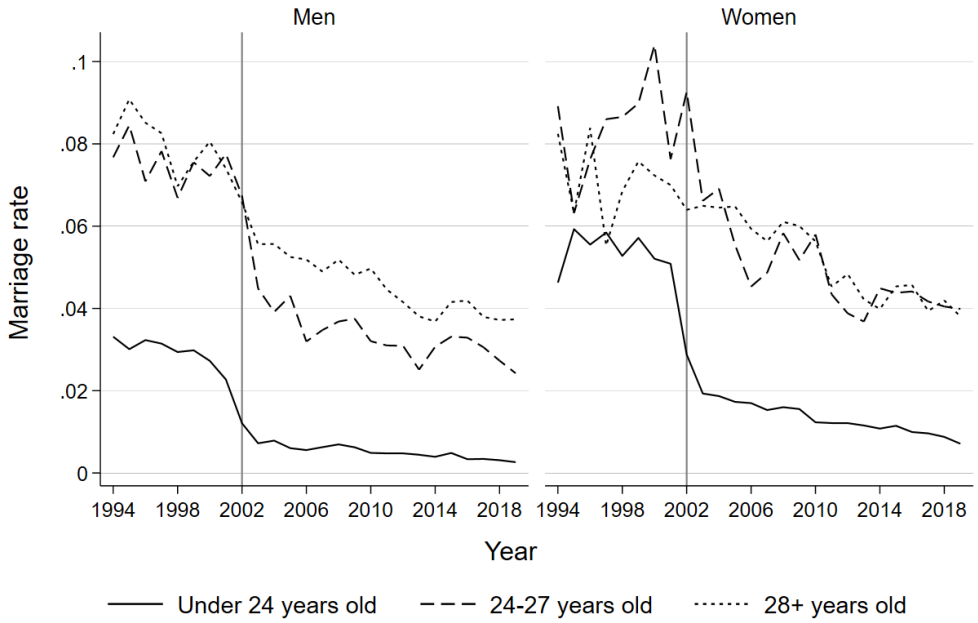
Appendix

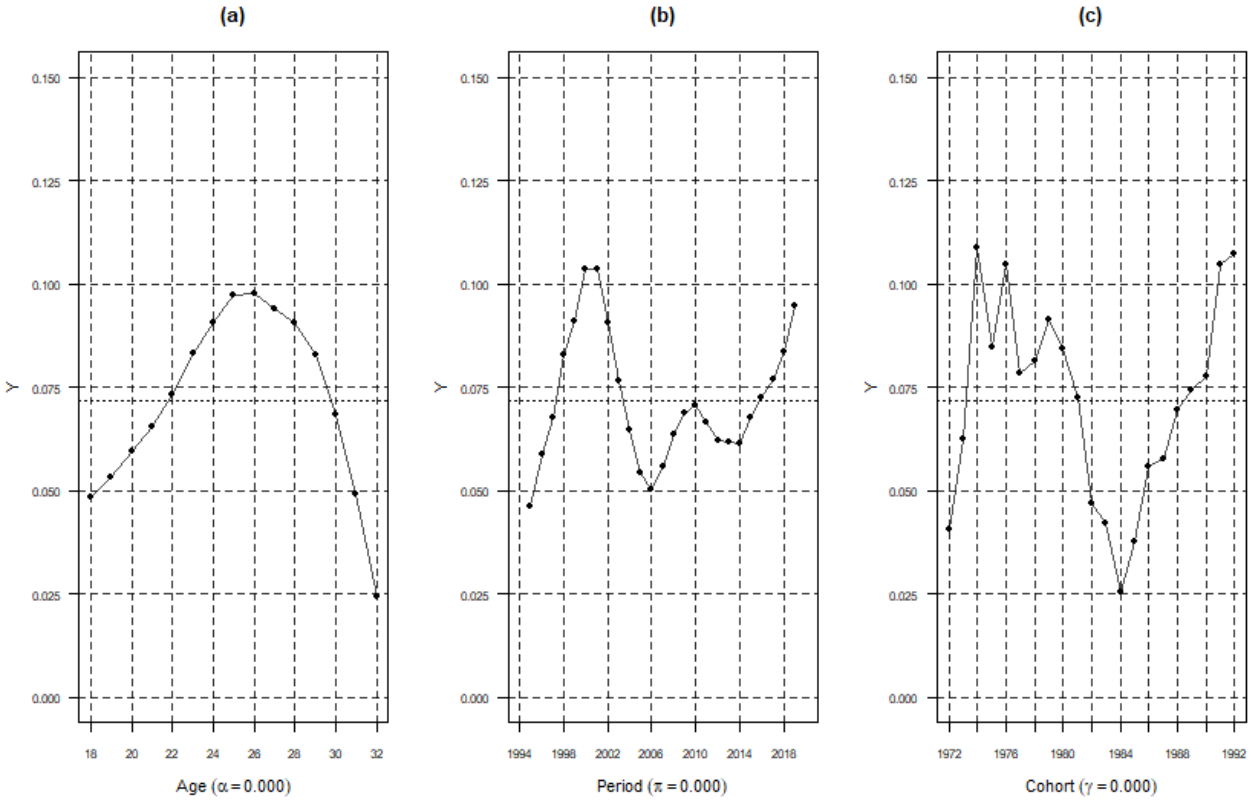
Figure A1. Changes in marriage behavior across the policy periods for Male and Female First and Second-Generation non-EU Immigrants

(a) Annual marriage rate across age groups, 1994-2019



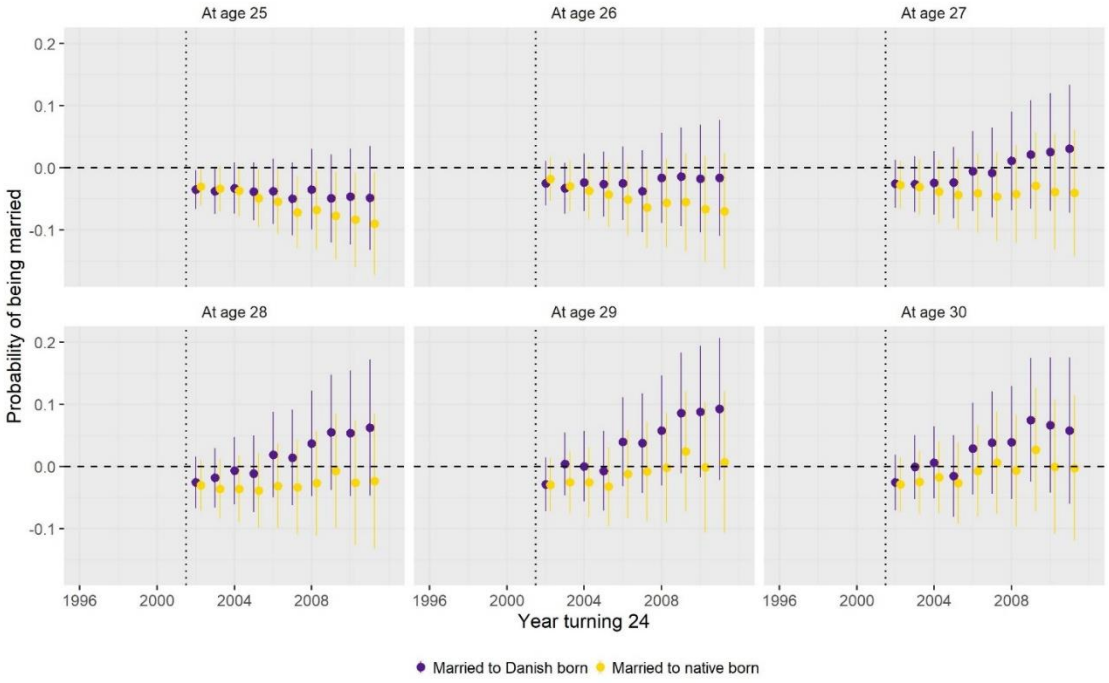
Source: Own calculations on data from Statistics Denmark.
Note: Full line shows implementation of 24-year rule.

Figure A2. Nonlinear age- (a), period- (b) and cohort-effects (c) for residualized probability of being married.



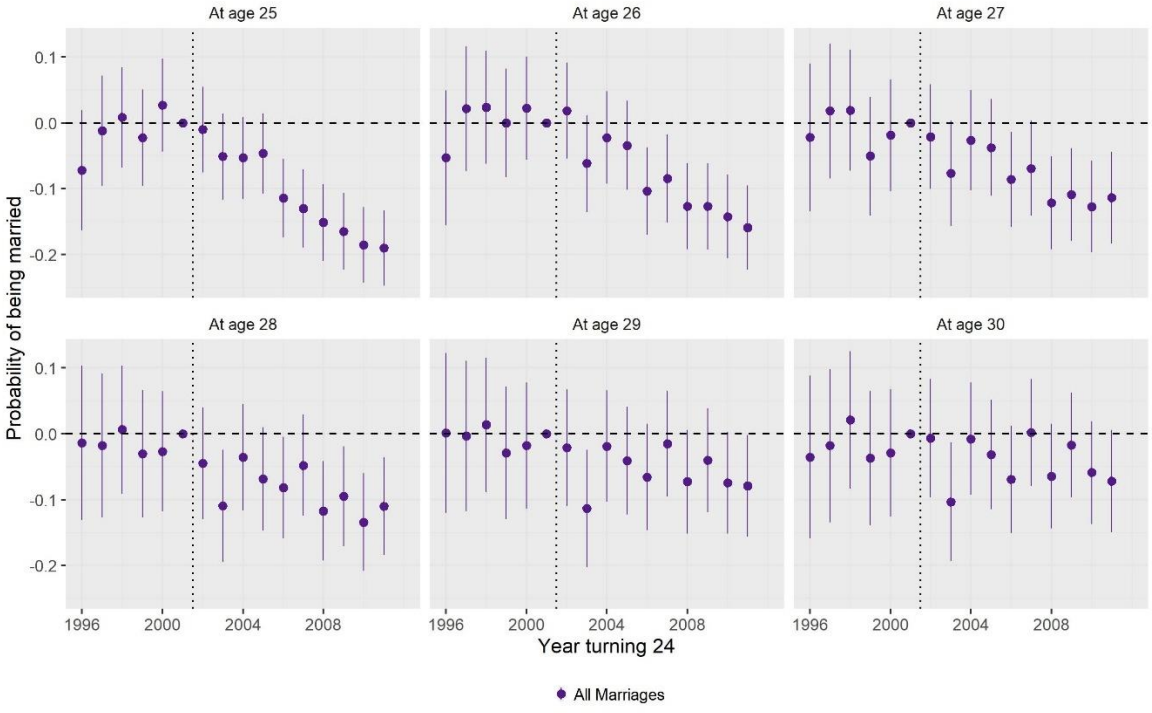
Source: Own calculations on data from Statistics Denmark.

Figure A3. Detrended results for marriage market outcome across birth cohorts comparing immigrants/descending relative to native-born women.



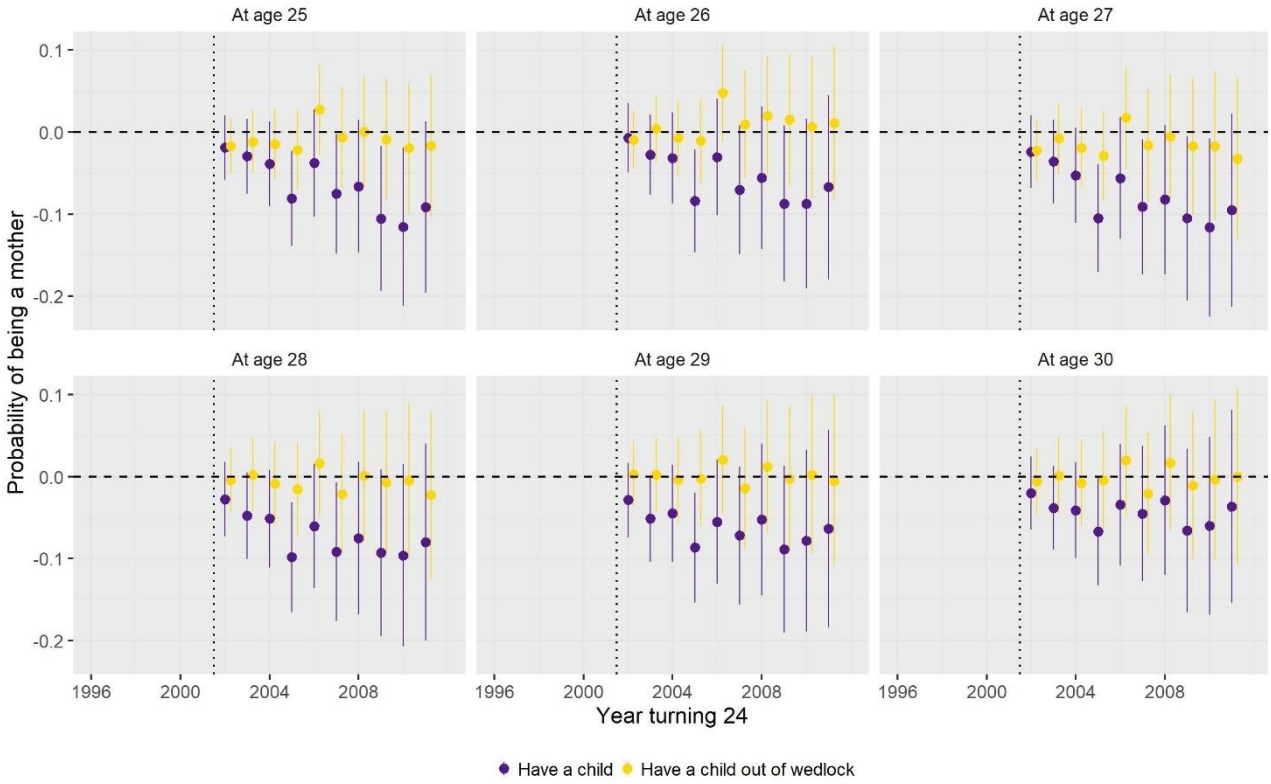
Source: Own calculations on data from Statistics Denmark.

Figure A4. Main marriage results for birth cohorts comparing immigrants/descending relative to native-born women excluding the island of Zealand.



Note: Own calculations on data from Statistics Denmark. Results adjusted for age at migration and country of origin. 95% confidence intervals.

Figure A5. Detrended results for fertility outcome across birth cohorts comparing immigrants/descending relative to native-born women.



Note: Own calculations on data from Statistics Denmark. Results adjusted for age at migration and country of origin. 95% confidence intervals.

Table A1: Distribution of Sample by Country of Origin

Country	Amount	Country	Amount	Country	Amount	Country	Amount	Country	Amount
Afghanistan	2699	Costa Rica	5	Jordan	551	North Korea	8	Taiwan	31
Albania	25	Cuba	36	Kazakhstan	23	Oman	< 5	Tajikistan	5
Algeria	202	Djibouti	12	Kenya	153	Pakistan	5557	Tanzania	98
Angola	28	Dominica	< 5	Kosovo	221	Panama	< 5	Thailand	1256
Argentina	108	Dominican Republic	13	Kuwait	417	Papua New Guinea	< 5	Togo	24
Armenia	122	Ecuador	36	Kyrgyzstan	< 5	Paraguay	5	Tonga	< 5
Azerbaijan	61	Egypt	352	Laos	16	Peru	67	Trinidad and Tobago	8
Bahrain	27	El Salvador	5	Lebanon	6700	Philippines	896	Tunisia	200
Bangladesh	62	Equatorial Guinea	< 5	Lesotho	6	Qatar	20	Turkey	15492
Belarus	61	Eritrea	107	Liberia	20	Russia	562	Turkmenistan	< 5
Belize	< 5	Ethiopia	248	Libya	51	Rwanda	69	Uganda	305
Benin	8	Fiji	< 5	Macedonia	711	Saint Lucia	< 5	Ukraine	269
Bhutan	69	Gabon	< 5	Madagascar	< 5	Samoa	< 5	United Arab Emirates	28
Bolivia	20	Gambia, The	147	Malawi	< 5	Saudi Arabia	38	Uruguay	49
Bosnia and Herzegovina	3635	Georgia	31	Malaysia	26	Senegal	14	USA	482
Botswana	< 5	Ghana	287	Mali	< 5	Serbia	91	Uzbekistan	19
Brazil	217	Guatemala	7	Mauritania	9	Serbia and Montenegro	186	Venezuela	47
Burundi	120	Guinea	27	Mauritius	9	Seychelles	< 5	Vietnam	3048
Cambodia	44	Guinea-Bissau	7	Mexico	31	Sierra Leone	79	West Indies	< 5
Cameroon	32	Guyana	13	Moldova	26	Singapore	26	Yemen	81
Canada	137	Haiti	< 5	Mongolia	5	Somalia	4117	Yugoslavia	3911
Cape Verde	5	Honduras	9	Montenegro	27	South Africa	72	Yugoslavia, Federal Republic	359
Central African Republic	< 5	India	675	Morocco	2376	South Korea	119	Zambia	56
Chad	< 5	Indonesia	63	Mozambique	20	South Sudan	< 5	Zimbabwe	32
Chile	263	Iran	2534	Myanmar	158	Soviet Union	190		
China	837	Iraq	5659	Namibia	6	Sri Lanka	2458	Stateless	149
Colombia	65	Israel	119	Nepal	46	Sudan	131		
Comoros	8	Ivory Coast	78	New Zealand	17	Suriname	< 5	Not stated	196
Congo, Democratic Republic	245	Jamaica	11	Nicaragua	21	Swaziland	< 5		
Congo, Republic	120	Japan	135	Nigeria	81	Syria	1721		

Table A2: Outcome measure by age, year, and immigrant status

Ethnic Danish women											
Age	**Year**	**Married**	**Danish born spouse**	**Native born spouse**	**EU-born spouse**	**Cohabitation**	**Motherhood**	**Unmarried motherhood**	**In education**	**Months of education (mean)**	**Months of education (sd)**
24	1996	0.141	0.136	0.136	0.138	0.421	0.235	0.152	0.315	157.573	20.628
25	1996	0.199	0.192	0.191	0.194	0.415	0.313	0.184	0.250	159.723	22.940
26	1996	0.267	0.260	0.259	0.263	0.394	0.401	0.209	0.195	161.710	24.538
27	1996	0.331	0.322	0.322	0.326	0.364	0.491	0.236	0.150	163.276	25.058
28	1996	0.395	0.386	0.385	0.390	0.334	0.576	0.253	0.116	164.116	25.637
29	1996	0.454	0.444	0.443	0.448	0.297	0.646	0.261	0.093	163.693	26.036
30	1996	0.500	0.488	0.487	0.495	0.267	0.701	0.264	0.078	163.420	26.403
24	1997	0.133	0.129	0.128	0.130	0.427	0.224	0.146	0.341	157.828	20.544
25	1997	0.189	0.183	0.183	0.186	0.419	0.298	0.177	0.280	160.524	22.843
26	1997	0.250	0.243	0.242	0.246	0.403	0.385	0.208	0.213	162.453	24.761
27	1997	0.318	0.310	0.310	0.314	0.377	0.476	0.231	0.161	163.821	25.825
28	1997	0.382	0.372	0.371	0.377	0.343	0.560	0.250	0.123	165.062	25.976
29	1997	0.453	0.443	0.442	0.448	0.299	0.634	0.249	0.101	165.363	26.227
30	1997	0.493	0.482	0.481	0.488	0.270	0.692	0.261	0.083	164.662	26.399
24	1998	0.121	0.116	0.116	0.118	0.429	0.206	0.137	0.364	158.310	20.252
25	1998	0.180	0.174	0.174	0.177	0.430	0.286	0.170	0.297	161.183	22.876
26	1998	0.242	0.235	0.235	0.238	0.406	0.368	0.199	0.234	163.588	24.662
27	1998	0.304	0.296	0.295	0.299	0.382	0.457	0.226	0.175	164.805	26.025
28	1998	0.372	0.363	0.362	0.367	0.353	0.545	0.244	0.131	165.654	26.627
29	1998	0.441	0.431	0.430	0.435	0.308	0.621	0.249	0.104	166.500	26.526
30	1998	0.493	0.483	0.482	0.488	0.275	0.684	0.252	0.088	166.392	26.558
24	1999	0.115	0.110	0.110	0.112	0.434	0.207	0.140	0.387	157.988	20.265
25	1999	0.168	0.161	0.161	0.164	0.434	0.265	0.160	0.317	161.928	22.758

26	1999	0.232	0.225	0.225	0.228	0.418	0.358	0.195	0.242	164.492	24.795
27	1999	0.298	0.290	0.289	0.294	0.386	0.440	0.218	0.188	166.246	25.979
28	1999	0.356	0.347	0.346	0.351	0.359	0.525	0.240	0.142	166.775	26.978
29	1999	0.431	0.421	0.420	0.426	0.316	0.609	0.248	0.112	167.013	27.163
30	1999	0.486	0.474	0.474	0.480	0.279	0.673	0.250	0.091	167.475	26.823
24	2000	0.114	0.110	0.109	0.111	0.439	0.203	0.138	0.398	158.213	20.173
25	2000	0.163	0.157	0.157	0.160	0.432	0.267	0.166	0.334	161.810	22.843
26	2000	0.220	0.213	0.213	0.216	0.419	0.334	0.185	0.260	165.337	24.703
27	2000	0.294	0.287	0.286	0.290	0.390	0.434	0.215	0.195	167.080	26.043
28	2000	0.354	0.345	0.345	0.349	0.360	0.511	0.234	0.151	168.289	26.834
29	2000	0.415	0.405	0.404	0.410	0.322	0.591	0.245	0.118	168.269	27.465
30	2000	0.475	0.465	0.464	0.470	0.288	0.664	0.250	0.095	168.070	27.531
24	2001	0.109	0.104	0.104	0.106	0.438	0.197	0.133	0.420	158.632	20.285
25	2001	0.157	0.151	0.151	0.153	0.440	0.261	0.163	0.344	162.213	22.855
26	2001	0.211	0.204	0.203	0.207	0.422	0.336	0.190	0.269	165.547	24.880
27	2001	0.274	0.266	0.265	0.269	0.398	0.408	0.209	0.208	168.283	26.009
28	2001	0.348	0.340	0.339	0.344	0.363	0.508	0.234	0.156	169.299	26.947
29	2001	0.411	0.402	0.401	0.406	0.322	0.582	0.241	0.124	169.979	27.353
30	2001	0.461	0.450	0.449	0.456	0.291	0.647	0.249	0.100	169.419	27.766
24	2002	0.103	0.098	0.098	0.099	0.442	0.188	0.130	0.434	159.347	20.344
25	2002	0.150	0.144	0.144	0.147	0.441	0.254	0.161	0.358	162.949	22.939
26	2002	0.207	0.201	0.200	0.203	0.432	0.329	0.188	0.279	165.917	24.770
27	2002	0.270	0.262	0.261	0.265	0.397	0.407	0.209	0.215	168.634	26.193
28	2002	0.329	0.321	0.319	0.325	0.368	0.481	0.225	0.163	170.599	26.877
29	2002	0.407	0.398	0.396	0.402	0.324	0.575	0.239	0.123	171.092	27.524
30	2002	0.460	0.450	0.449	0.455	0.291	0.641	0.244	0.102	171.242	27.653
24	2003	0.097	0.094	0.093	0.095	0.435	0.180	0.126	0.446	159.505	20.799
25	2003	0.143	0.138	0.137	0.139	0.446	0.245	0.157	0.369	163.925	23.126
26	2003	0.198	0.192	0.191	0.195	0.428	0.324	0.189	0.291	166.877	24.852
27	2003	0.261	0.253	0.252	0.256	0.408	0.407	0.214	0.219	169.246	26.201
28	2003	0.322	0.313	0.312	0.317	0.371	0.483	0.229	0.171	170.985	27.071

29	2003	0.391	0.382	0.381	0.387	0.331	0.553	0.235	0.130	172.296	27.416
30	2003	0.455	0.445	0.444	0.451	0.292	0.636	0.245	0.101	172.271	27.803
24	2004	0.097	0.094	0.093	0.095	0.436	0.178	0.124	0.453	159.611	21.147
25	2004	0.140	0.136	0.135	0.137	0.438	0.238	0.152	0.376	164.074	23.409
26	2004	0.196	0.190	0.189	0.192	0.434	0.317	0.184	0.297	167.993	24.978
27	2004	0.255	0.248	0.248	0.251	0.408	0.402	0.214	0.230	170.123	26.229
28	2004	0.322	0.314	0.313	0.317	0.375	0.484	0.232	0.174	171.678	27.144
29	2004	0.387	0.377	0.376	0.382	0.335	0.557	0.240	0.136	172.749	27.587
30	2004	0.443	0.434	0.432	0.439	0.298	0.621	0.243	0.107	173.612	27.725
24	2005	0.093	0.091	0.090	0.091	0.447	0.174	0.123	0.456	160.313	21.777
25	2005	0.138	0.134	0.133	0.136	0.438	0.236	0.151	0.380	164.580	23.822
26	2005	0.187	0.183	0.182	0.184	0.436	0.309	0.183	0.302	168.429	25.154
27	2005	0.255	0.249	0.248	0.251	0.409	0.393	0.209	0.233	171.425	26.173
28	2005	0.316	0.308	0.307	0.312	0.374	0.479	0.232	0.180	172.682	26.939
29	2005	0.388	0.380	0.379	0.384	0.335	0.561	0.243	0.138	173.577	27.530
30	2005	0.438	0.428	0.427	0.433	0.304	0.623	0.247	0.111	174.171	27.787
24	2006	0.095	0.092	0.091	0.093	0.430	0.176	0.123	0.457	159.924	22.030
25	2006	0.136	0.132	0.131	0.132	0.446	0.236	0.153	0.384	165.123	24.300
26	2006	0.187	0.183	0.182	0.185	0.431	0.309	0.183	0.306	168.785	25.751
27	2006	0.247	0.241	0.239	0.243	0.413	0.390	0.211	0.235	171.848	26.434
28	2006	0.313	0.305	0.304	0.309	0.380	0.472	0.228	0.180	174.013	26.998
29	2006	0.382	0.374	0.372	0.378	0.335	0.555	0.241	0.144	174.598	27.464
30	2006	0.441	0.432	0.430	0.436	0.303	0.629	0.250	0.115	174.863	27.852
24	2007	0.094	0.091	0.090	0.091	0.431	0.179	0.127	0.460	160.458	22.348
25	2007	0.138	0.134	0.133	0.135	0.436	0.237	0.150	0.377	165.151	24.788
26	2007	0.188	0.183	0.182	0.185	0.434	0.308	0.182	0.300	169.437	26.107
27	2007	0.251	0.246	0.244	0.248	0.401	0.389	0.205	0.231	172.334	27.139
28	2007	0.308	0.302	0.300	0.304	0.382	0.472	0.231	0.177	174.467	27.291
29	2007	0.382	0.373	0.371	0.377	0.340	0.549	0.236	0.136	175.841	27.480
30	2007	0.437	0.428	0.427	0.433	0.300	0.622	0.245	0.116	175.974	27.756
24	2008	0.093	0.090	0.089	0.091	0.422	0.179	0.127	0.449	161.016	22.860

25	2008	0.139	0.134	0.133	0.135	0.431	0.241	0.154	0.373	165.672	24.940
26	2008	0.190	0.186	0.185	0.187	0.421	0.309	0.179	0.294	169.496	26.625
27	2008	0.249	0.243	0.242	0.245	0.402	0.393	0.208	0.228	172.844	27.337
28	2008	0.314	0.307	0.305	0.310	0.370	0.469	0.224	0.173	174.838	27.906
29	2008	0.376	0.369	0.366	0.372	0.339	0.550	0.239	0.137	176.279	27.704
30	2008	0.438	0.427	0.426	0.432	0.303	0.619	0.242	0.111	177.004	27.682
24	2009	0.091	0.088	0.087	0.089	0.410	0.180	0.128	0.452	161.206	23.278
25	2009	0.132	0.128	0.127	0.129	0.424	0.240	0.156	0.374	166.381	25.518
26	2009	0.187	0.183	0.181	0.183	0.421	0.314	0.184	0.294	170.076	26.695
27	2009	0.242	0.237	0.235	0.239	0.407	0.388	0.209	0.225	172.965	27.848
28	2009	0.306	0.299	0.297	0.301	0.375	0.472	0.231	0.173	175.507	28.112
29	2009	0.374	0.366	0.364	0.370	0.334	0.546	0.238	0.134	176.754	28.353
30	2009	0.427	0.420	0.417	0.423	0.304	0.621	0.252	0.114	177.537	27.953
24	2010	0.090	0.087	0.086	0.087	0.410	0.183	0.131	0.466	161.892	23.693
25	2010	0.128	0.125	0.124	0.125	0.419	0.239	0.159	0.391	166.602	25.799
26	2010	0.179	0.174	0.173	0.175	0.417	0.309	0.186	0.309	170.612	27.247
27	2010	0.238	0.232	0.230	0.233	0.402	0.392	0.215	0.237	173.454	27.936
28	2010	0.296	0.289	0.287	0.292	0.384	0.475	0.239	0.180	175.498	28.605
29	2010	0.362	0.354	0.351	0.356	0.344	0.550	0.246	0.143	177.258	28.491
30	2010	0.419	0.410	0.407	0.414	0.310	0.616	0.254	0.112	178.090	28.530
24	2011	0.082	0.079	0.078	0.080	0.412	0.186	0.137	0.489	162.536	24.135
25	2011	0.121	0.118	0.116	0.118	0.421	0.238	0.160	0.404	167.072	26.116
26	2011	0.165	0.161	0.159	0.161	0.421	0.305	0.192	0.329	170.682	27.402
27	2011	0.224	0.219	0.217	0.221	0.408	0.386	0.217	0.248	173.858	28.405
28	2011	0.283	0.277	0.274	0.278	0.385	0.465	0.243	0.191	175.836	28.711
29	2011	0.345	0.338	0.335	0.341	0.358	0.549	0.259	0.148	177.213	28.941
30	2011	0.402	0.394	0.392	0.397	0.322	0.614	0.263	0.119	178.575	28.636
24	2012	0.077	0.075	0.073	0.075	0.409	0.182	0.137	0.509	162.460	24.318
25	2012	0.112	0.109	0.107	0.109	0.422	0.238	0.166	0.418	167.860	26.417
26	2012	0.157	0.153	0.151	0.153	0.419	0.303	0.194	0.338	171.161	27.679
27	2012	0.206	0.201	0.199	0.202	0.409	0.375	0.224	0.264	173.899	28.637

28	2012	0.271	0.265	0.263	0.267	0.385	0.459	0.245	0.202	176.275	29.096
29	2012	0.331	0.324	0.321	0.326	0.362	0.539	0.263	0.158	177.542	29.067
30	2012	0.387	0.379	0.376	0.382	0.332	0.613	0.276	0.127	178.358	29.196
24	2013	0.067	0.064	0.063	0.064	0.407	0.172	0.133	0.526	163.092	24.708
25	2013	0.108	0.104	0.103	0.104	0.424	0.234	0.165	0.424	168.186	26.764
26	2013	0.144	0.140	0.137	0.140	0.430	0.300	0.200	0.337	172.248	28.021
27	2013	0.198	0.193	0.190	0.193	0.415	0.373	0.224	0.269	174.416	28.802
28	2013	0.248	0.243	0.240	0.244	0.396	0.446	0.253	0.208	176.416	29.392
29	2013	0.317	0.310	0.308	0.313	0.363	0.529	0.265	0.162	178.025	29.528
30	2013	0.367	0.359	0.355	0.361	0.340	0.601	0.285	0.131	178.829	29.211
24	2014	0.062	0.061	0.059	0.060	0.404	0.166	0.129	0.529	163.983	24.705
25	2014	0.096	0.093	0.091	0.093	0.428	0.225	0.163	0.440	168.982	26.965
26	2014	0.141	0.136	0.134	0.136	0.429	0.297	0.198	0.345	172.537	28.250
27	2014	0.185	0.180	0.177	0.181	0.423	0.369	0.233	0.264	175.624	29.148
28	2014	0.239	0.234	0.231	0.235	0.405	0.444	0.256	0.209	176.984	29.536
29	2014	0.292	0.286	0.283	0.287	0.373	0.519	0.278	0.165	178.358	29.775
30	2014	0.356	0.349	0.345	0.351	0.345	0.593	0.286	0.130	179.398	29.685
24	2015	0.059	0.056	0.055	0.056	0.409	0.154	0.122	0.516	165.085	24.776
25	2015	0.091	0.089	0.087	0.088	0.425	0.220	0.161	0.429	169.837	26.855
26	2015	0.129	0.125	0.123	0.126	0.436	0.288	0.198	0.341	173.612	28.457
27	2015	0.183	0.178	0.175	0.178	0.427	0.369	0.232	0.256	176.020	29.287
28	2015	0.229	0.223	0.219	0.224	0.415	0.445	0.265	0.200	178.154	29.749
29	2015	0.289	0.283	0.279	0.284	0.385	0.517	0.281	0.161	178.839	29.854
30	2015	0.335	0.328	0.324	0.330	0.354	0.586	0.299	0.129	179.670	29.951
24	2016	0.057	0.056	0.054	0.056	0.421	0.152	0.119	0.504	166.021	24.996
25	2016	0.090	0.086	0.084	0.087	0.434	0.212	0.156	0.404	171.408	27.163
26	2016	0.130	0.127	0.124	0.127	0.437	0.285	0.195	0.316	174.945	28.713
27	2016	0.174	0.168	0.165	0.169	0.437	0.366	0.237	0.245	177.441	29.647
28	2016	0.231	0.225	0.222	0.226	0.417	0.449	0.266	0.182	178.861	29.999
29	2016	0.282	0.275	0.271	0.276	0.395	0.525	0.294	0.151	180.108	30.059
30	2016	0.333	0.326	0.322	0.328	0.364	0.591	0.306	0.120	180.385	29.929

24	2017	0.057	0.055	0.053	0.055	0.419	0.151	0.118	0.504	166.142	24.882
25	2017	0.090	0.087	0.085	0.087	0.438	0.206	0.150	0.394	171.734	27.286
26	2017	0.126	0.122	0.120	0.122	0.445	0.280	0.193	0.300	175.962	28.641
27	2017	0.173	0.169	0.165	0.169	0.435	0.357	0.228	0.226	178.401	29.528
28	2017	0.226	0.219	0.215	0.220	0.420	0.444	0.265	0.180	180.044	29.996
29	2017	0.289	0.282	0.277	0.283	0.389	0.528	0.287	0.138	180.621	30.053
30	2017	0.332	0.323	0.319	0.325	0.367	0.596	0.311	0.115	181.440	29.955
24	2018	0.059	0.056	0.054	0.057	0.416	0.145	0.112	0.516	165.979	24.632
25	2018	0.087	0.084	0.081	0.084	0.442	0.204	0.150	0.392	172.092	27.152
26	2018	0.132	0.128	0.125	0.128	0.438	0.270	0.182	0.296	176.199	28.549
27	2018	0.174	0.168	0.165	0.169	0.444	0.353	0.225	0.227	179.071	29.351
28	2018	0.222	0.215	0.211	0.216	0.422	0.436	0.262	0.171	180.698	29.820
29	2018	0.283	0.275	0.271	0.277	0.392	0.522	0.287	0.139	181.756	30.032
30	2018	0.339	0.331	0.327	0.333	0.363	0.598	0.303	0.112	181.868	29.977
24	2019	0.056	0.053	0.052	0.053	0.419	0.135	0.103	0.529	166.353	24.583
25	2019	0.088	0.085	0.082	0.085	0.435	0.196	0.140	0.406	172.128	27.128
26	2019	0.124	0.120	0.117	0.120	0.451	0.269	0.184	0.295	176.560	28.552
27	2019	0.180	0.175	0.172	0.176	0.430	0.346	0.214	0.219	179.467	29.249
28	2019	0.225	0.218	0.215	0.220	0.426	0.432	0.255	0.172	181.385	29.623
29	2019	0.279	0.272	0.267	0.274	0.397	0.514	0.283	0.136	182.317	29.860
30	2019	0.331	0.322	0.317	0.324	0.368	0.591	0.303	0.113	182.995	29.989

Non-EU migrants/descendants

Age	Year	Married	Danish born spouse	Native born spouse	EU-born spouse	Cohabitation	Motherhood	Unmarried motherhood	In education	Months of education (mean)	Months of education (sd)
24	1996	0.543	0.095	0.055	0.080	0.113	0.477	0.080	0.178	142.585	23.406
25	1996	0.639	0.080	0.042	0.050	0.095	0.599	0.072	0.127	141.569	25.977
26	1996	0.704	0.090	0.046	0.053	0.078	0.701	0.085	0.100	137.377	26.423
27	1996	0.757	0.126	0.079	0.088	0.056	0.762	0.097	0.085	141.137	27.879
28	1996	0.771	0.129	0.082	0.091	0.085	0.812	0.125	0.085	137.912	26.510
29	1996	0.719	0.134	0.103	0.103	0.091	0.834	0.162	0.055	140.319	27.096
30	1996	0.763	0.128	0.082	0.096	0.068	0.890	0.164	0.050	138.869	27.776
24	1997	0.594	0.080	0.042	0.061	0.072	0.516	0.061	0.181	142.394	24.223
25	1997	0.552	0.098	0.063	0.086	0.106	0.529	0.098	0.171	144.367	25.075
26	1997	0.661	0.089	0.038	0.048	0.078	0.656	0.086	0.121	143.324	27.780
27	1997	0.713	0.099	0.058	0.065	0.085	0.734	0.097	0.085	138.799	27.553
28	1997	0.779	0.128	0.081	0.087	0.048	0.794	0.090	0.087	140.882	28.044
29	1997	0.764	0.123	0.082	0.088	0.085	0.833	0.138	0.072	138.871	27.467
30	1997	0.714	0.119	0.083	0.083	0.115	0.849	0.175	0.052	140.713	27.576
24	1998	0.608	0.087	0.049	0.057	0.087	0.533	0.071	0.194	141.059	22.591
25	1998	0.621	0.080	0.040	0.056	0.073	0.569	0.071	0.174	143.588	25.623
26	1998	0.573	0.111	0.071	0.093	0.124	0.583	0.109	0.146	146.048	26.790
27	1998	0.705	0.100	0.049	0.059	0.081	0.697	0.089	0.114	144.852	28.999
28	1998	0.729	0.117	0.071	0.078	0.073	0.749	0.102	0.090	140.096	29.158
29	1998	0.801	0.137	0.092	0.098	0.045	0.824	0.086	0.083	142.651	29.826
30	1998	0.757	0.123	0.079	0.085	0.085	0.861	0.155	0.076	140.141	28.555
24	1999	0.580	0.097	0.054	0.070	0.084	0.475	0.062	0.212	145.964	22.970
25	1999	0.635	0.101	0.059	0.068	0.088	0.601	0.088	0.175	143.125	24.691
26	1999	0.636	0.079	0.043	0.058	0.085	0.624	0.101	0.169	144.689	26.567
27	1999	0.611	0.140	0.094	0.115	0.120	0.613	0.104	0.130	146.845	27.900
28	1999	0.712	0.114	0.060	0.071	0.082	0.734	0.101	0.095	146.219	30.053

29	1999	0.737	0.109	0.072	0.079	0.082	0.769	0.102	0.097	140.740	29.627
30	1999	0.787	0.138	0.090	0.096	0.045	0.841	0.117	0.093	143.323	30.446
24	2000	0.575	0.103	0.056	0.078	0.095	0.486	0.082	0.212	144.932	22.558
25	2000	0.608	0.104	0.057	0.077	0.092	0.524	0.078	0.204	147.720	24.466
26	2000	0.654	0.112	0.058	0.073	0.097	0.648	0.104	0.149	144.050	25.646
27	2000	0.669	0.096	0.057	0.074	0.084	0.676	0.105	0.135	146.694	28.672
28	2000	0.640	0.158	0.105	0.130	0.130	0.656	0.112	0.110	148.074	29.137
29	2000	0.733	0.131	0.074	0.087	0.087	0.785	0.117	0.087	147.264	30.537
30	2000	0.729	0.110	0.065	0.075	0.090	0.802	0.118	0.095	141.266	29.666
24	2001	0.520	0.098	0.045	0.069	0.094	0.423	0.093	0.245	147.677	22.641
25	2001	0.612	0.121	0.062	0.092	0.102	0.548	0.098	0.183	146.723	24.076
26	2001	0.639	0.118	0.067	0.090	0.090	0.585	0.092	0.164	149.591	26.019
27	2001	0.672	0.122	0.070	0.085	0.090	0.688	0.118	0.134	145.881	27.253
28	2001	0.683	0.112	0.070	0.088	0.086	0.706	0.115	0.127	147.910	29.778
29	2001	0.642	0.162	0.109	0.135	0.135	0.698	0.142	0.112	149.487	29.907
30	2001	0.732	0.131	0.077	0.093	0.087	0.801	0.123	0.079	148.562	31.115
24	2002	0.528	0.092	0.034	0.060	0.081	0.439	0.076	0.269	147.582	23.049
25	2002	0.569	0.120	0.057	0.086	0.094	0.490	0.100	0.219	149.147	23.905
26	2002	0.621	0.124	0.065	0.099	0.090	0.594	0.103	0.173	148.162	25.768
27	2002	0.633	0.130	0.076	0.099	0.089	0.630	0.118	0.146	150.846	27.100
28	2002	0.683	0.130	0.073	0.090	0.084	0.722	0.126	0.124	146.879	28.294
29	2002	0.675	0.125	0.080	0.097	0.076	0.743	0.136	0.107	149.493	31.045
30	2002	0.641	0.159	0.106	0.132	0.142	0.732	0.165	0.104	150.823	30.458
24	2003	0.508	0.088	0.029	0.064	0.117	0.414	0.079	0.273	149.009	22.544
25	2003	0.533	0.092	0.033	0.060	0.102	0.506	0.099	0.228	149.613	25.109
26	2003	0.584	0.127	0.057	0.091	0.095	0.542	0.114	0.189	151.078	25.665
27	2003	0.637	0.139	0.078	0.111	0.090	0.650	0.127	0.152	150.245	27.869
28	2003	0.643	0.132	0.075	0.102	0.099	0.667	0.130	0.127	152.644	28.291
29	2003	0.694	0.137	0.081	0.095	0.082	0.750	0.139	0.113	147.709	28.822
30	2003	0.680	0.122	0.086	0.104	0.071	0.778	0.153	0.106	149.908	31.356
24	2004	0.458	0.113	0.039	0.070	0.101	0.394	0.092	0.296	148.397	23.073

25	2004	0.527	0.108	0.035	0.076	0.111	0.473	0.094	0.259	151.061	24.470
26	2004	0.558	0.114	0.050	0.079	0.094	0.567	0.113	0.205	151.529	27.012
27	2004	0.597	0.137	0.063	0.101	0.110	0.607	0.132	0.150	152.666	26.840
28	2004	0.644	0.148	0.082	0.117	0.088	0.692	0.142	0.137	151.792	28.981
29	2004	0.651	0.143	0.080	0.107	0.085	0.707	0.128	0.104	154.072	29.658
30	2004	0.688	0.134	0.077	0.089	0.084	0.780	0.158	0.101	148.877	29.615
24	2005	0.394	0.107	0.029	0.071	0.122	0.355	0.090	0.319	150.659	23.552
25	2005	0.475	0.120	0.040	0.073	0.124	0.458	0.109	0.268	151.517	25.140
26	2005	0.528	0.118	0.040	0.083	0.118	0.523	0.119	0.228	153.761	26.210
27	2005	0.570	0.123	0.050	0.084	0.106	0.611	0.125	0.169	154.082	28.688
28	2005	0.622	0.154	0.079	0.118	0.109	0.653	0.134	0.127	154.492	27.833
29	2005	0.654	0.159	0.086	0.123	0.090	0.740	0.158	0.111	153.945	30.156
30	2005	0.654	0.151	0.083	0.112	0.093	0.741	0.142	0.090	154.612	29.569
24	2006	0.357	0.127	0.035	0.091	0.157	0.386	0.139	0.305	150.998	24.652
25	2006	0.417	0.121	0.035	0.081	0.138	0.407	0.108	0.280	153.650	25.986
26	2006	0.490	0.134	0.042	0.083	0.132	0.516	0.127	0.223	153.818	26.930
27	2006	0.545	0.132	0.046	0.093	0.118	0.585	0.140	0.187	156.189	28.160
28	2006	0.578	0.131	0.054	0.090	0.102	0.651	0.149	0.138	155.740	29.742
29	2006	0.630	0.168	0.092	0.133	0.095	0.694	0.150	0.118	155.530	28.777
30	2006	0.654	0.166	0.089	0.126	0.089	0.766	0.173	0.099	154.757	30.360
24	2007	0.305	0.111	0.026	0.061	0.172	0.333	0.122	0.327	152.030	24.447
25	2007	0.381	0.135	0.041	0.097	0.165	0.448	0.169	0.271	154.211	27.247
26	2007	0.450	0.142	0.045	0.095	0.139	0.452	0.123	0.226	156.359	27.868
27	2007	0.512	0.144	0.051	0.097	0.119	0.564	0.142	0.191	155.573	28.286
28	2007	0.559	0.150	0.051	0.110	0.120	0.624	0.157	0.164	157.560	29.423
29	2007	0.598	0.140	0.062	0.099	0.114	0.692	0.163	0.127	156.337	30.166
30	2007	0.649	0.187	0.106	0.150	0.092	0.730	0.163	0.105	156.790	29.361
24	2008	0.295	0.126	0.030	0.089	0.156	0.342	0.145	0.333	153.148	25.107
25	2008	0.350	0.132	0.032	0.076	0.169	0.403	0.148	0.281	155.405	26.687
26	2008	0.411	0.156	0.049	0.110	0.157	0.505	0.191	0.232	157.010	29.422
27	2008	0.474	0.150	0.051	0.105	0.132	0.512	0.142	0.184	158.713	29.620

28	2008	0.546	0.170	0.065	0.120	0.113	0.613	0.158	0.164	157.732	29.793
29	2008	0.586	0.183	0.068	0.132	0.111	0.659	0.161	0.155	158.762	30.220
30	2008	0.627	0.163	0.074	0.114	0.104	0.732	0.166	0.111	157.274	30.644
24	2009	0.260	0.125	0.027	0.081	0.158	0.304	0.152	0.363	154.088	25.830
25	2009	0.339	0.153	0.041	0.111	0.156	0.403	0.163	0.290	156.537	27.587
26	2009	0.388	0.148	0.041	0.090	0.169	0.455	0.166	0.242	158.329	28.384
27	2009	0.439	0.168	0.055	0.121	0.163	0.558	0.206	0.195	159.521	30.870
28	2009	0.489	0.164	0.060	0.112	0.133	0.568	0.163	0.154	160.507	30.169
29	2009	0.563	0.180	0.073	0.132	0.107	0.659	0.171	0.152	159.750	30.912
30	2009	0.607	0.197	0.078	0.144	0.102	0.702	0.176	0.146	160.480	30.937
24	2010	0.262	0.141	0.023	0.090	0.153	0.288	0.145	0.370	154.047	26.205
25	2010	0.314	0.149	0.039	0.101	0.169	0.362	0.165	0.309	158.017	28.464
26	2010	0.380	0.174	0.051	0.127	0.161	0.455	0.180	0.254	159.823	29.735
27	2010	0.420	0.169	0.053	0.106	0.161	0.515	0.188	0.204	160.751	29.803
28	2010	0.482	0.193	0.067	0.142	0.162	0.608	0.216	0.164	161.069	31.573
29	2010	0.516	0.168	0.062	0.115	0.134	0.621	0.185	0.154	161.803	30.658
30	2010	0.590	0.204	0.086	0.147	0.119	0.692	0.183	0.135	160.623	31.258
24	2011	0.255	0.143	0.023	0.102	0.160	0.303	0.154	0.384	156.511	26.713
25	2011	0.297	0.161	0.031	0.105	0.164	0.350	0.164	0.313	158.103	28.820
26	2011	0.343	0.175	0.049	0.121	0.177	0.419	0.187	0.270	160.788	30.116
27	2011	0.395	0.185	0.052	0.133	0.171	0.512	0.206	0.220	162.400	31.066
28	2011	0.450	0.180	0.059	0.115	0.152	0.560	0.196	0.180	162.258	30.779
29	2011	0.490	0.206	0.074	0.151	0.165	0.650	0.231	0.151	162.449	32.015
30	2011	0.532	0.177	0.068	0.123	0.135	0.660	0.199	0.117	164.017	31.050
24	2012	0.215	0.128	0.018	0.088	0.172	0.270	0.153	0.431	157.779	26.588
25	2012	0.294	0.164	0.026	0.113	0.165	0.366	0.177	0.321	160.112	28.922
26	2012	0.331	0.178	0.036	0.116	0.164	0.416	0.187	0.263	160.850	30.368
27	2012	0.367	0.189	0.056	0.130	0.187	0.481	0.209	0.229	163.396	31.153
28	2012	0.428	0.201	0.061	0.149	0.174	0.567	0.224	0.191	164.250	31.850
29	2012	0.479	0.194	0.070	0.129	0.149	0.618	0.211	0.150	163.768	31.235
30	2012	0.500	0.214	0.081	0.165	0.154	0.689	0.247	0.128	163.364	32.372

24	2013	0.203	0.127	0.015	0.089	0.169	0.262	0.149	0.461	158.741	27.287
25	2013	0.256	0.152	0.024	0.103	0.178	0.332	0.171	0.381	161.789	28.825
26	2013	0.327	0.181	0.029	0.123	0.187	0.427	0.202	0.256	163.325	30.562
27	2013	0.353	0.197	0.043	0.128	0.168	0.471	0.220	0.232	163.308	31.378
28	2013	0.391	0.205	0.067	0.143	0.187	0.537	0.233	0.187	165.698	32.167
29	2013	0.454	0.210	0.069	0.157	0.168	0.623	0.242	0.162	165.670	32.406
30	2013	0.501	0.211	0.082	0.143	0.156	0.662	0.226	0.132	164.801	31.483
24	2014	0.189	0.121	0.018	0.087	0.180	0.249	0.156	0.451	159.599	27.483
25	2014	0.243	0.148	0.019	0.106	0.184	0.325	0.178	0.383	163.331	29.519
26	2014	0.294	0.170	0.030	0.114	0.177	0.391	0.193	0.304	165.326	30.849
27	2014	0.356	0.200	0.035	0.134	0.184	0.486	0.221	0.213	165.657	31.548
28	2014	0.380	0.207	0.045	0.134	0.175	0.534	0.245	0.190	165.486	31.904
29	2014	0.416	0.219	0.076	0.155	0.180	0.580	0.248	0.157	166.696	32.423
30	2014	0.460	0.216	0.068	0.158	0.169	0.663	0.265	0.145	166.942	32.888
24	2015	0.184	0.112	0.013	0.082	0.178	0.234	0.143	0.443	161.236	27.429
25	2015	0.234	0.149	0.024	0.107	0.188	0.305	0.178	0.355	164.004	30.055
26	2015	0.283	0.172	0.027	0.124	0.188	0.383	0.197	0.300	167.092	31.126
27	2015	0.337	0.196	0.040	0.132	0.181	0.452	0.205	0.239	168.023	32.005
28	2015	0.387	0.215	0.042	0.144	0.176	0.550	0.241	0.170	167.825	32.048
29	2015	0.409	0.231	0.051	0.149	0.176	0.587	0.261	0.151	166.938	32.288
30	2015	0.441	0.240	0.088	0.171	0.187	0.622	0.259	0.127	168.122	32.696
24	2016	0.176	0.119	0.016	0.087	0.176	0.220	0.139	0.439	164.062	27.887
25	2016	0.233	0.144	0.020	0.103	0.191	0.302	0.170	0.340	166.416	29.816
26	2016	0.274	0.176	0.034	0.126	0.189	0.373	0.208	0.261	167.928	31.795
27	2016	0.308	0.190	0.032	0.137	0.203	0.447	0.226	0.213	170.260	32.112
28	2016	0.366	0.215	0.051	0.148	0.193	0.510	0.230	0.173	170.450	32.898
29	2016	0.418	0.234	0.056	0.162	0.176	0.609	0.268	0.135	169.260	32.347
30	2016	0.432	0.244	0.063	0.161	0.173	0.628	0.276	0.128	168.393	32.379
24	2017	0.175	0.121	0.017	0.099	0.166	0.211	0.134	0.454	163.890	26.957
25	2017	0.218	0.147	0.022	0.108	0.189	0.277	0.160	0.344	168.762	30.023
26	2017	0.263	0.168	0.026	0.120	0.190	0.357	0.187	0.255	170.119	31.300

27	2017	0.304	0.196	0.039	0.140	0.192	0.440	0.230	0.205	170.573	32.601
28	2017	0.340	0.215	0.041	0.158	0.199	0.506	0.248	0.160	172.434	32.631
29	2017	0.389	0.235	0.062	0.164	0.197	0.569	0.253	0.139	172.243	33.151
30	2017	0.433	0.244	0.064	0.171	0.183	0.651	0.283	0.120	170.503	32.336
24	2018	0.157	0.107	0.018	0.086	0.163	0.204	0.131	0.458	164.362	27.226
25	2018	0.213	0.147	0.024	0.122	0.175	0.268	0.159	0.350	168.742	29.339
26	2018	0.253	0.170	0.029	0.125	0.192	0.340	0.185	0.275	172.301	31.355
27	2018	0.306	0.201	0.039	0.144	0.197	0.418	0.211	0.201	172.661	32.103
28	2018	0.340	0.217	0.048	0.157	0.184	0.500	0.248	0.169	172.494	32.786
29	2018	0.376	0.240	0.053	0.177	0.191	0.569	0.266	0.140	173.854	32.822
30	2018	0.412	0.249	0.068	0.176	0.193	0.617	0.269	0.118	173.306	33.114
24	2019	0.127	0.089	0.013	0.075	0.169	0.183	0.125	0.473	165.824	28.058
25	2019	0.190	0.129	0.024	0.103	0.177	0.256	0.157	0.364	169.599	29.715
26	2019	0.247	0.170	0.030	0.139	0.193	0.328	0.180	0.282	172.630	30.785
27	2019	0.294	0.204	0.042	0.152	0.197	0.403	0.208	0.211	175.035	32.179
28	2019	0.336	0.227	0.052	0.167	0.201	0.472	0.232	0.171	174.518	32.147
29	2019	0.373	0.244	0.058	0.176	0.181	0.556	0.263	0.143	173.940	33.107
30	2019	0.398	0.263	0.065	0.195	0.194	0.616	0.281	0.122	174.806	32.825

Table A3: Estimates for trend adjustments

	Married	Married to Danish born	Married to native born	Married to EU-born	Cohabiting	Motherhood	Child out of wedlock	In education	Educational length
Age 25	0.004	0.014***	0.009*	0.012***	0.002	-0.002	0.007*	-0.007	0.577*
	(0.004)	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.005)	(0.245)
Age 26	0.003	0.014***	0.009*	0.013***	-0.004	-0.003	0.005	-0.003	0.518
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.265)
Age 27	-0.002	0.012**	0.007	0.011**	-0.001	0.001	0.008	-0.002	0.46
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.279)
Age 28	-0.003	0.010*	0.007	0.011*	-0.001	0.002	0.007	-0.001	0.486
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.288)
Age 29	-0.004	0.010*	0.005	0.010*	-0.002	0.002	0.005	-0.002	0.51
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)	(0.292)
Age 30	-0.002	0.013**	0.006	0.012*	-0.003	0	0.003	-0.003	0.539
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)	(0.294)