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ABSTRACT

The Effect of Marriage on Education of Immigrants: Evidence from a Policy Reform Restricting Spouse Import*

We investigate the effect of immigrants' marriage behavior on dropout from education. To identify the causal effect, we exploit a recent Danish policy reform which generated exogenous variation in marriage behavior by a complete abolishment of spouse import for immigrants below 24 years of age. We find that the abrupt change of marriage behavior following the reform is associated with improved educational attainment of young immigrants. The causal impact of marriage on dropout for males is estimated to be around 20 percentage points, whereas the effect for females is small and mostly insignificant. We interpret the results as being consistent with a scenario where family investment motives drive the behavior of males, while the association between marriage and dropout for females is driven by selection effects. The estimated causal effect varies considerably across subgroups.

JEL Classification: I21, J12

Keywords: education, dropout, immigrants, spouse import, marriage migration, family investment model

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1. Introduction

It is well-established for several countries that immigrants who marry other immigrants lack behind in terms of human capital and labor market outcomes compared to immigrants who marry natives (Duncan and Trejo, 2007; Celikaksoy, 2006b; Meng and Gregory, 2005; Kantarevic, 2004; Nielsen et al., 2003; Rosholm, Husted and Nielsen, 2002; Qian, 1999). If this phenomenon represents a causal impact of marriage behavior on educational and labor market outcomes, it provides some justification for policy intervention in the marriage market of immigrants. On the other hand, if it is just a spurious relationship reflecting unobserved abilities or preferences, intervention is clearly not justified by this observation.[1] The much discussed policy intervention in Denmark restricting spouse import ('the 24-year rule'), which was put into force July 2002, supplies us with exogenous variation in the marriage behavior of immigrants which gives us a unique opportunity to investigate the issue of causality.

A prevalent characteristic of immigrants all over the world is their tendency to marry other immigrants living in the country of residence or to marry immigrants who migrate for the purpose of marriage (e.g. European Population Committee, 2002; Duncan and Trejo, 2007; Meng and Gregory, 2005). For Non-Western immigrants in Scandinavian countries, the majority brings their spouse from their country of origin rather than marrying someone who already resides in the country of residence. Furthermore, Non-Western immigrants marry at relatively early ages compared to natives (Pedersen, 2000; Jakobsen and Smith, 2006). In this paper, we concentrate on the effect of this particular marriage behavior of the children of immigrants on educational attainment as measured by their dropout rate from education. To our knowledge, there are no other papers investigating this relationship while focusing on establishing a causal relationship although the literature investigating the relationship between educational attainment and marital behavior is vast. However, the majority of both the empirical and theoretical literature implicitly assumes that these are sequential decisions, where marital decisions follow the completion of education (Fernandez, Guner and Knowles, 2005; Nielsen and Svarer, 2006; Furtado, 2006; Celikaksoy, Nielsen and Verner, 2006). Thus, studies that investigate how the changes in the marriage market or the changes in the marriage behavior of individuals affect individuals' pre-marital investments are very scarce (Iyigun and Walsh, 2007).

This empirical study contributes to the literature by establishing the causal relationship between marriage and dropout behavior for the immigrants in Denmark. We exploit the introduction of 'the 24-year rule' to identify the causal effect of marriage on educational attainment. Our main finding is that the causal effect of marriage on dropout for males is around 20 percentage points, whereas the effect for females is small and not significantly different from zero. The conclusions are very robust since it holds for nearly all subgroups and for different definitions of the treatment variable. The large causal effect of marriage on education for males is consistent with a family investment model where borrowing constrained newly formed households divide the 'borrowing' and 'investment' activities across family members. The male immigrant takes on a 'borrower' role after marriage to a female marriage migrant who is forced into the 'investor' role as she lacks country-specific human capital which is needed in order to live and potentially work in the host country. Apparently, this mechanism is not strong enough for females to actually result in an impact of marriage on education. The results for females indicate that the correlation between marriage and educational attainment is driven by unobservable variables such as norms, religion or traditions.

A simple extrapolation of the effect of the policy reform shows that the magnitude of the estimated effect is sufficiently large to eliminate a substantial part of the gap in the dropout rate between ethnic Danish males and young immigrant males. The estimated causal effect varies by subgroup. It is large and robust for Turks, but not for Pakistanis.

[1] Notice that the issue of policy intervention in the marriage market is a sensitive matter. Even large benefits from intervention may be outweighed by larger non-financial costs due to value systems opposing intervention. This discussion is outside the scope of this paper.

The paper is organized as follows: Section 2 discusses the theoretical background for understanding the mechanism behind the relationship between education and marriage to a marriage migrant. Section 3 describes the Danish migration policy, whereas Section 4 explains the reform-based identification strategy. Section 5 describes the data set. Section 6 presents the results of the empirical analysis and robustness checks. Section 7 concludes the paper.

2. Background

An important obstacle for integration of Non-Western immigrants in the labor market is lack of education. The few studies done on Non-Western immigrants' children show that the lack of education persists although to a relatively smaller extent when compared to their parents.[2] However, on average the members of the group of children of immigrants are quite young, and therefore the economic fate of these individuals is yet to be decided.

Most studies on the educational attainment of immigrants find a large discrepancy in the educational attainment of Non-Western immigrants and natives, but there are different findings regarding the reasons for this discrepancy. One group argues that ethnicity is the main determinant of the immigrant children's educational attainment (Gang and Zimmermann, 1999; Riphahn, 2003; Nielsen et al., 2003), whereas other research shows that ethnicity is not significant after controlling for parental capital (Van Ours and Veenman, 2002). On the other hand, Borjas (1995) argues that to analyze intergenerational transmissions, one should include ethnic capital in addition to parental capital.

We argue that marital behavior should be taken into consideration as well when analyzing the educational attainment and assimilation of young Non-Western immigrants as these groups of immigrants marry at a relatively early age, and they very often marry 'marriage migrants' who immigrate to the destination country because of the marriage (European Population Committee, 2002). Furthermore, empirical studies have found a strong relationship between marital behavior and educational attainment, which indicates that marital behavior interferes with educational investments (Nielsen et al. 2003, Rosholm et al., 2002).

2.1. The family investment model

A theoretical background for incorporating marital behavior into studies of educational attainment of immigrants is found in the studies by Duleep and Sanders (1993) for the US and Baker and Benjamin (1997) for Canada, who argue that a family investment model seems to explain the observed economic activities of immigrant husbands and wives. Following Baker and Benjamin (1997), the idea is that borrowing constrained newly immigrated households obtain household consumption smoothing by use of a strategy where one party (typically the woman) is 'the borrower' and the other party (typically the man) is 'the investor'. One party chooses a low-investment career to help finance the investment of the other in a career with a steeper earnings profile. Hence, the 'borrower' would have a high labor force participation rate in the beginning of the career in the host country and a low enrollment in education and training. As a consequence, the wage-experience profile of the 'borrower' is flat and wage assimilation slow.[3]

Both Duleep and Sanders (1993) and Baker and Benjamin (1997) find that the observed patterns of labor force participation, employment and wages among immigrant husbands and wives lend support to the family investment model. Duleep and Sanders (1993) find that whether a husband invests in skills specific to the US labor market (e.g. language courses, starting own business or taking on jobs with on-the-job training) influences the wife's decision to work. They also find that the immigrant groups where the men have the largest expected earnings growth are the groups with the highest labor force participation of married women, whereas the groups with the smallest expected earnings growth are the groups with the lowest labor force

[2] See, Hummelgaard et al. (2002), Colding (2006), Nielsen et al. (2003), Riphahn (2003), Fertig and Schmidt (2002), Gang and Zimmermann (1999), Van Ours and Veenman (2002), Österberg (2000).

[3] Weiss (1997) present the idea of credit extension and coordination of investment strategies as a purpose for marriage in a more general discussion about economic reasons for marriage.

participation of married women. Furthermore, they see that for immigrant groups with skills that are less transferable to the US, the initial years are characterized by greater investment in US-specific skills for the husband and higher labor force participation rates for the wives.

A major problem when testing the family investment model is the fact that variation in labor force participation and enrollment is driven by preferences and cultural differences. Duleep and Sanders (1993) approach this critique by carefully distinguishing between different ethnic groups, and they still find strong support for the family investment model after accounting for ethnic differences.

The studies by Duleep and Sanders (1993) and Baker and Benjamin (1997) do not claim to establish causal relationships although they do attempt to use all available observable variables to account for endogeneity and confounding effects. A couple of recent papers have tried to exploit the variation in sex ratios to distinguish between selection and causality of marital behavior on wage assimilation.[4] However, evidence is mixed. For Australia, Meng and Gregory (2005) find evidence of a causal interpretation of the difference in wage assimilation by type of marriage. For the US, Kantarevic (2004) finds that difference in assimilation merely represents selection effects. For Denmark, Celikaksoy (2006b) finds that there tend to be marriage penalties for immigrants marrying marriage migrants although the sex ratio instrument is not strong enough to establish a causal relationship in this case. Judging from Angrist (2002), one should not only worry about the strength of the sex ratio as an instrument for marriage, but also about validity. He investigates the direct effect of sex ratio on labor force participation while using migration flows as an instrument. He argues that sex ratios would affect female bargaining power in the marriage market. As a consequence, a sex ratio reflecting an overweight of men means that women who expect to marry need to worry less about developing an independent means of support, while men who expect to marry need to invest more in characteristics attractive to mates, i.e. earnings capability.

Even though there are obvious points of critique, studies using sex ratios as an instrument are the most promising in the line of research trying to establish a causal relationship between marriage of immigrants and labor market outcomes. We have access to a reform-based instrument which sheds more light on this debate while using a completely different type of exogenous variation in data.

2.2. Educational attainment and family investment

Let us use the family investment model in the context of 18-24-year-old immigrant youths who are in the process of building up their host country-specific formal human capital at the same time as making decisions on family formation. As single person households, they would, conditional on their ability and preferences, invest in a career that maximizes the present value of future net benefits subject to potential borrowing constraints. Based on a standard human capital model, the optimal career would most likely imply high investments in education at the beginning of the life cycle and a steep earnings profile after labor market entrance. However, expectation of future family formation means that the optimal investment strategy would also take into account that pre-marital career investments influence marriage outcomes and the sharing of resources in the future household. A more competitive marriage market would thus lead to higher pre-marital investments (Angrist, 2002; Iyigun and Walsh, 2007).

Assume that marriage hits the immigrant youth like an unanticipated shock before his/her planned education has been completed. This may be due to the individual suddenly being encouraged to or - maybe even - forced to marry a marriage migrant. Or, it may simply be that love strikes on a summer holiday in the country of origin, and cultural norms then prescribe marriage rather than cohabitation. After getting married, the couple will most likely make joint career decisions, and if the one spouse has no income, borrowing constraints are now more likely to be binding. Thus, the decision to marry an imported spouse with no income may force the partner to become a 'borrower' (using the language of Baker and Benjamin) because taking on the 'borrower' role may be necessary to allow the newly arrived partner to invest in host country

[4] It is practically always found that immigrants who marry other immigrants assimilate more slowly than immigrants who marry natives. A candidate explanation is the family investment model.

specific capital such as language qualifications and knowledge about culture and institutions. In the case of Denmark, after 1999 it is even *required* by law that newly arrived immigrants participate in an integration course of 36 months' duration which would automatically put the newly arrived partner in the 'investor' role and the young immigrant in the 'borrower' role if they are borrowing constrained. Hence, the young immigrant who has grown up in the host country may actually decide - or rather be forced by law – to give up his/her own education, and thus his/her integration, for the sake of the partner's integration.

The above treatment of family formation regards marriage as an unanticipated shock from the point of view of the young immigrant in the host country. This may or may not be perfectly right. A young immigrant, who is going to marry someone from his/her country of origin, will most likely anticipate this. However, he/she may not know exactly when it is going to take place, and he/she may not know whether he/she will be able to complete his/her education first. Furthermore, in practice the time of the marriage may also depend on things like success in the educational system, availability of partners, maturity, personal development and assimilation. From a theoretical perspective, educational investments would be depressed by the fact that an unanticipated marriage shock may occur, and as a consequence, an interruption of the educational career would occur earlier than it would in a scenario where family formation is part of a forward-looking investment strategy. Therefore, a policy intervention that removes or reduces the amount of unanticipated marriage shocks may have both ex-ante and ex-post effects on educational investments.

Empirical studies have found a strong relationship between marital behavior and educational attainment, indicating that young immigrants who marry other immigrants (i.e. most often a marriage migrant) obtain less education (e.g. Nielsen et al. 2003). Taken at face value, this relationship tends to support the family investment hypothesis as mentioned above. However, the relationship may in fact be spurious if there exist unobservable factors that influence both marriage decisions and educational decisions. Examples of such variables could be language proficiency, religion, or it could be traditional norms or preferences that prescribe women to be housewives and husbands to be providers. Unobserved variables such as language proficiency or academic ability are likely to influence the marriage behavior of males and females similarly, whereas the influence of variables such as religion, norms or preferences is likely to vary by gender.

In the present paper, we investigate whether the observed correlation between educational attainment and marriage reflects causality or selection effects. And to the extent that it reflects a causal relationship, we are going to interpret that as consistent with the family investment strategy. We use a recent policy reform which generated exogenous variation in marriage behavior by complete abolishment of spouse import for spouses below 24 years of age. The reform is described in the next subsection.

3. Danish migration policy and regulation of marriage migration

The Danish migration policy has undergone a large number of changes during the latest decades where most of the reforms have implied a tightening up of the immigration rules for immigrants originating outside the EU and Nordic countries, see Pedersen and Smith (2002). Until 1973, there was in practice free immigration for labor migrants who could support themselves, and a large number of male 'guest workers' mainly from Turkey, Pakistan and Yugoslavia immigrated to Denmark and many other Western European countries. The economic downturn and increasing unemployment rates in the 1970s implied an end to this labor migration. During the next decades, a large number of the former labor migrants were able to bring their families to Denmark. However, the immigration rules relating to family unification have gradually been tightened since the early 1990s, and marriage migration has become an increasingly important source of immigration from Non-Western countries into Denmark. As a response to the increasing number of marriage migrants from Non-Western countries, the Danish regulation of marriage migration has also become more restrictive (see Celikaksoy, 2006a).

3.1. Regulation of marriage migration[5]

During the period of relevance for this study, which is from year 2000 onwards, a number of regulations have been adopted. Below, we sum up some of the most important requirements.[6]

The partnership should be legal according to Danish law, which means, for instance, that the partners should be above 18 years of age. Furthermore, both parties must enter the marriage voluntarily, and pro forma marriages with the purpose of gaining a permit to stay in Denmark are also prohibited. The partners should live together in Denmark at the same address after getting married and after getting the permit to stay. Furthermore, in order to be granted family unification with a spouse from abroad who migrates to Denmark with the purpose of marriage, the couple's 'total affiliation' to Denmark (defined as the number of years the two spouses have stayed in Denmark) should be greater than the 'total affiliation' to the partner's country of origin (defined analogously). From 2003, this requirement was loosened, and people who had 28 years of Danish citizenship and people who had grown up in Denmark and stayed legally for at least 28 years were no longer exposed to this check on affiliation. The spouse living in Denmark should have a Danish or Nordic citizenship, be a refugee under Danish protection or an immigrant with at least three years of permanent residence permit. This person should document a certain wealth, earn a sufficient income to be able to support the partner and document a living arrangement of sufficient size for both of them. However, he/she is exempted from this requirement if he or she is a student.[7] In addition, the person must not have received social assistance within the last year, and there must be no conviction of violence against previous partners.

3.2. The contents of 'the 24-year reform'

We employ a policy reform which has restricted the possibility of spouse import to Denmark extensively. In the public debate, the policy reform has been dubbed 'the 24-year reform'. The purpose of the reform was twofold. One purpose was to reduce the number of marriages that are arranged by parents or families instead of representing the voluntary choice of the partners themselves. Another purpose was to reduce the number of Non-Western immigrants entering the country due to family reunifications. 'The 24-year reform' was brought into effect by an abolishment of the legal claim for family reunification. After the reform in 2002, family reunification due to marriage migration is not possible if one of the partners is below 24 years of age. Immigrants with a 'green card' are not encompassed by this rule.[8]

Table 1. Overview of the timing of the 24-year reform

Date	Political event	Economic event
January 17, 2002	Draft legislation presented	Announcement
June 6, 2002	Final passage	
July 1, 2002	Commencement	Implementation

The timing of the implementation of the reform is described in Table 1. It is seen that there was a relatively long announcement period from January 17 until June 6, where immigrants had the possibility to react on

[5] Immigrants with citizenship from the other Nordic countries have immediate access to Denmark, and immigrants from EU/EEA are covered by other less restrictive regulations.
[6] Details may be found in the 'Danish Aliens Act' and at www.nyidanmark.dk.
[7] This exemption from the law means that the person living in Denmark does not need to drop out of an education in order to document that he or she is able to support an imported spouse. Therefore, a potential causal relationship between marriage and dropout is not caused by overly restrictive requirements for the person living in Denmark.
[8] The 'green card' is tied to education and income. Immigrants who have a documented job offer in Denmark which pays an annual salary above DKK 450,000 (~$80,000) or immigrants who hold an education in short supply (e.g. medical doctors, nurses, researchers) may obtain a 'green card' without further notice.

anticipated enactment of the law. In our empirical analysis, we disregard the whole year of 2002 in order to avoid this announcement period.

4. Reform-based identification strategy

In this paper, we evaluate the effect of a binary treatment, marriage, M_i, on a binary educational outcome, Y_i. Let the educational outcome be determined by an index model:

$$(1) \qquad Y_i = 1[\alpha M_i + \boldsymbol{\gamma}\mathbf{X_i} + u_i > 0],$$

where the treatment indicator, M_i, takes the value one if treatment is received, i.e. the individual gets married, and zero otherwise. The matrix, $\mathbf{X_i}$, consists of a set of control variables including a column of ones, and α and $\boldsymbol{\gamma}$ are parameters to be estimated. If u_i is correlated with M_i, standard univariate models, such as a linear probability model or a probit model, give inconsistent estimates. In this case, we need further assumptions to consistently estimate the treatment effect, α. One identification strategy is to look for an instrument for M_i, which fulfills the IV-assumptions. Assume that M_i is an indicator variable denoting marriage, which - in addition to the matrix, $\mathbf{X_i}$, - is determined by a variable, Z_i, which only influences educational attainment through its influence on M_i :

$$(2) \qquad M_i = 1[\beta Z_i + \varphi\mathbf{X_i} + \varepsilon_i > 0],$$

and that $(u_i, \varepsilon_i) \sim BN(0,0,1,1,\rho)$. The parameters α, β, φ, $\boldsymbol{\gamma}$ and ρ may be estimated by maximum likelihood. And, when the educational outcome variable, Y_i, and the marriage variable, M_i, are defined as indicator variables, the model is a recursive bivariate probit model.[9]

Inference on the causal effect of M_i on Y_i requires that α is successfully disentangled from ρ (i.e. "compensation effects" are ruled out). Therefore, we carefully test for exogeneity (H_0: ρ =0) along the lines suggested by Monfardini and Radice (2007). In particular, we rely on two of their results. Firstly, inference on exogeneity relies heavily on a valid exclusion restriction because that preserves validity of the LR test of exogeneity even when the distributional assumption of normality fails. Secondly, they find that caution should be invoked if a univariate probit estimation of (1) shows a statistically significant estimate of α, while the bivariate probit model leads to insignificant estimates of both α and ρ. This pattern may be seen as a signal of practical identification problems due to compensation effects. In addition to the results by Monfardini and Radice, we use the informal tests suggested by Altonji, Elder and Taber (2005) to investigate to which extent the identification is driven by functional form and to which extent it is driven by the exclusion restriction.

We use policy variation due to 'the 24-year reform' to construct an instrumental variable and thus an exclusion restriction. 'The 24-year reform' restricted marriage migration without affecting the possibilities to obtain education. We disregard the reform year 2002 in order to avoid potential announcement effects after the reform had been announced in early 2002. Thus, we define Z_i =1 for years until 2002, and Z_i =0 after that.

The policy variation in the possibility to obtain family reunification gives identification of the causal effect of marriage on education if the reform indicator is a strong predictor of marriage and if the reform indicator may be validly excluded from equation (1). The latter part implies a number of untestable and potentially problematic assumptions. Firstly, the reform should not have been anticipated before 2002. Therefore, the fact that immigration policy previously has been gradually tightened should not have led people to anticipate this reform and therefore to marry earlier on. If this was the case, it would give a negative bias on the estimate of α. However, the reform was unexpectedly harsh on both immigrants and natives wanting to marry individuals from non-EU/EEA-countries, and it is viewed as being on the borderline of conflict with

[9] The term 'recursive' refers to the fact that one of the dependent variables (here: M_i) enters the equation for the other dependent variable (here: Y_i).

human rights. Therefore, we argue that it is unlikely that it was anticipated. Secondly, there should be no sample attrition which is related to the reform. For instance, if the introduction of the reform led young immigrants to emigrate in order to sustain their marriage plans, it would give a positive bias on α. According to aggregate emigration data for 18-24-year-olds, there was no increase in the relevant emigration after 2002.[10] Finally – and probably most importantly – there should be no calendar year or cohort effects shifting the educational attainment simultaneously with the reform in year 2002. To minimize this problem, we use only data two years apart from the reform on either side. We can do this because we have collected information about the total population of relevance to this study. This leaves us with a sufficient amount of observations close to the reform year 2002. Within this limited time frame, we investigate time effects among ethnic Danes, and compare time effects for the immigrant sample to that of a matched sample of ethnic Danes (cf. next subsection). Furthermore, in the estimations, we allow for some birth cohort and year effects by inclusion of indicator variables which we test the joint significance of. Under the assumption that calendar time and birth cohort effects are either controlled for or unimportant for educational outcome within this restricted time frame, the effect of M_i on Y_i is identified separately from other time effects on Y_i.

5. Data

5.1. Data source

We employ a dataset consisting of the population of immigrants and descendents of immigrants during the period 1984-2004. We select individuals from non-EU/EEA countries[11] who are the immigrants that are subject to 'the 24-year reform' that we use for identification. We focus on middle generation immigrants and descendents of immigrants. Middle generation immigrants are defined as immigrants who arrived before they turned 8 years old; whereas descendents are individuals whose parents were immigrants. In some of the analyses, we compare this dataset to the information in a 10% sample of native Danish youths in the same age groups.

Due to the potential announcement effects during the period January 2002 to July 2002, we disregard data from year 2002; therefore, the final data set contains individuals of age 18-24 for the years 2000, 2001, 2003 and 2004.[12] We disregard people who disappear from the registers (out or return migration) for more than one year and then re-enter the sample (1%), and we delete individuals who cohabit or marry a Dane or another EU- or EEA-citizen (5%). The resulting total number of observations is 44,745. Around 1% of these individuals have missing information on educational outcomes; therefore, the maximum number of observations in the regressions is 44,402 covering 18,043 different individuals. In part of the analysis, we focus on the two largest 'guest worker' immigrant groups, namely Turks and Pakistanis. Individuals with ethnic origin in Turkey constitute 34% of the sample, whereas individuals with ethnic origin in Pakistan constitute 14% of the sample. Education and labor market outcomes vary much with ethnic origin. Turkish immigrants, in particular males, lack behind in terms of education, whereas Pakistani immigrants are more prone to acquire an education. Therefore, detailed analyses of these two groups may point to important

[10] Emigration data are available at www.statistikbanken.dk. Emigration of individuals with a Danish or foreign citizenship to Turkey and Pakistan was stable over the period 2000 to 2004 at around 60 and 30 persons, respectively. In the media, the issue has been raised that immigrants who wish to marry a marriage migrant emigrate to Sweden after the reform year. The number of individuals who emigrated from Denmark to Sweden doubled from 400 to 800 individuals over the period 2000 to 2004. The similar number for individuals with foreign citizenship was stable around 200 during the period. The numbers most likely reflect the opening of the bridge to Sweden in 1999, because migration increased steadily from 1999 to 2003, after which year it was stable.

[11] These are countries outside EU-15 or EEA (Denmark, Finland, Iceland, Liechtenstein, Luxembourg, Monaco, Norway, Sweden, Belgium, France, Greece, the Netherlands, Ireland, Italy, Portugal, Switzerland, Spain, Great Britain, Germany, and Austria). Thus, we include a few individuals who originate from Western countries (about 50 individuals originate from the three largest source countries: US, Japan and Canada), as well as some individuals who originate from Eastern Europe (mainly Poland). More details to follow in Table 4.

[12] For 2005, we only have information on events of enrollment and completion of education and no other variables. We use this information in parts of the analyses.

differences in treatment effects across ethnic origin. Earlier studies argue that analyses should be performed separately by ethnic origin to control for cultural specific unobserved variables such as preferences for work and education (see Duleep and Sanders, 1993; Baker and Benjamin, 1997). Therefore, we view the two sub-samples of Turks and Pakistanis as more homogenous samples with respect to preferences and norms for education and marriage than the full sample.

5.2. Definition of the treatment and the outcome variable

In the empirical analysis, we investigate the effect of a binary treatment, marriage, M_i, on the educational outcome, Y_i. In this subsection, we go through the definition of the treatment and outcome variables in turn.

In Table 2, we describe the marital status of individuals observed in a two-year period before the reform (2000-2001) and a two-year period after the reform (2003-2004). We include observations for individuals cohabiting or marrying a Danish, EU or EEA citizen in order to illustrate the extent of these types of partner selection before we exclude them from the sample.

The proportion who marries an individual with a Danish, EU or EEA citizenship is slightly declining across the reform year, whereas the proportion starting a cohabitant relationship with an individual with a Danish, EU or EEA citizenship is slightly increasing during the period. As a consequence, the sum of the propensity to marry or cohabit with an individual with a Danish, EU or EEA citizenship is stable across the reform year. Hence, we conclude that we validly may condition on *not* forming a couple (either cohabitation or marriage) with a Danish, EU or EEA citizen while analyzing marriage behavior and educational outcome.

Table 2 shows that the proportion of 18-24-year-old males and females who get married drops dramatically after 2002. For males, the proportion getting married is reduced from 6% to 2%, whereas for females, the same figures are 12% to 4%. The decline is almost entirely explained by the drop in the proportion getting married to a marriage migrant, which indicates that the reform mainly influences the probability to marry a marriage migrant.[13]

[13] The proportion who marry a marriage migrant within the period 2003-2004 is .001 (rather than exactly zero) because we count the few 23-24-year-olds who marry marriage migrants who are then granted a stay permit after they both have turned 24 years.

Table 2. Overview of marriage before and after the reform. 18-24-year-old immigrants.

	2000-2001		2003-2004	
	Males	**Females**	**Males**	**Females**
Marry within period	**0.060**	**0.120**	**0.021**	**0.043**
a marriage migrant	0.036	0.064	0.001	0.001
an immigrant living in DK	0.020	0.049	0.017	0.038
a Danish/EU/EEA citizen	0.003	0.006	0.002	0.003
Already married	**0.082**	**0.179**	**0.056**	**0.142**
to a marriage migrant	0.063	0.121	0.038	0.085
to an immigrant living in DK	0.018	0.053	0.016	0.052
to a Danish/EU/EEA citizen	0.001	0.005	0.002	0.005
Start cohabitation within period	**0.038**	**0.056**	**0.050**	**0.071**
with an immigrant	0.009	0.024	0.018	0.037
with a Danish/EU/EEA citizen	0.029	0.031	0.032	0.034
Already cohabiting	**0.011**	**0.018**	**0.013**	**0.026**
with an immigrant	0.003	0.007	0.005	0.013
with a Danish/EU/EEA citizen	0.008	0.012	0.009	0.013
Stay single	**0.809**	**0.628**	**0.859**	**0.718**
All	**1.000**	**1.000**	**1.000**	**1.000**
Total number of individuals	**6422**	**5823**	**8401**	**7721**

From Table 2, it seems that marriage to a marriage migrant has not to a large extent been substituted with marriage to another type of partner, either an immigrant from their country of origin or another Non-EU/EEA country who is already living in Denmark, or a Danish, EU or EEA citizen. The proportion who marry an immigrant living in Denmark has declined slightly, though it is stable if counted in absolute numbers. This marriage pattern is puzzling. We expected some degree of substitution of marriage migrant partners with partners residing in the host country, but this behavior is not seen within the time span of the data.[14]

In the empirical analysis, we look at the binary treatment 'being married':

$$M_i = \begin{cases} 1, & \text{if individual } i \text{ is married} \\ 0, & \text{if individual } i \text{ is not married} \end{cases} \quad (3)$$

As a robustness check, we also look at the treatment 'being married to a marriage migrant', 'getting married' and 'cohabiting'. According to the discussion above, it should not make much difference for the results. We do not use information about children in the family. Therefore, the treatment variable reflects a combined effect of being married and having children. On average 6% and 14% of the males and females, respectively, in the sample have children. Among the 24-year-olds, about two thirds of the married individuals have children.

The educational outcome variable should ideally reflect whether the individual has completed a post-compulsory education. In Denmark, compulsory school starts at the age of 7 and includes 9 years of

[14] As a consequence, we rule out the possibility that a potential causal relationship between marriage and dropout stems from market forces at work at the marriage markets of migrants. One could imagine that the '24-year reform' increased competition on the host country marriage market, and hence induced people to increase their pre-marital investments such as education. This increased investment would increase the possibility to attract an immigrant partner already residing in the host country who is now scarce in supply. However, as of yet, there is no sign of this type of behavior in the data.

schooling. About half of the pupils are enrolled in a '10th grade class', which is a voluntary supplementary grade at compulsory school. After leaving compulsory school at the age of 16-17 years, the individuals may enroll in an upper secondary school, i.e. high school (3 years), a vocational education (3-4 years), or leave the educational system and in some cases start an education later on. High school does not provide formal qualifications which qualify for the labor market, but it qualifies for entry into tertiary education. The tertiary education can be short (1-3 years, e.g. dental hygienist, laboratory technician), medium (3-4 years, e.g. teacher, nurse, bachelor) or long (5-6 years, university).[15]

The majority of the Danish youth does not complete education until they are in their mid or late 20s. Preferably, the outcome variable should be highest completed qualifying education. However, the fact that 'the 24-year reform' took place recently and the fact that the individuals affected are fairly young mean that we do not yet have valid information about final completed education for most of the individuals in the sample. Therefore, as a measure of the educational outcome, Y_i, we use an indicator for dropping out of the educational system after basic school (without enrolling in another education). Hence Y_i is defined as follows:

$$Y_i = \begin{cases} 0, & \text{if individual } i \text{ is undertaking or has completed an education beyond basic school} \\ 1, & \text{if individual } i \text{ has dropped out of education and is not observed to be enrolled in another education} \end{cases} \quad (4)$$

In Table 3, we briefly describe the educational outcomes of the 18,043 individuals who are included in the estimation dataset. We present the educational outcome before and after the reform, respectively.

Table 3. Educational outcome of 18-24-year-old immigrants.

	2000-2001		2003-2004	
	Males	**Females**	**Males**	**Females**
Dropout: No more than primary school or part of high school/vocational education	**0.270**	**0.209**	**0.252**	**0.200**
No more than primary school	0.142	0.134	0.101	0.103
Drop out of high school	0.028	0.029	0.033	0.037
Drop out of vocational education	0.106	0.050	0.125	0.069
Drop out of both	0.007	0.005	0.009	0.009
Completed or enrolled in high school or vocational education	**0.589**	**0.606**	**0.594**	**0.584**
Completed	0.269	0.342	0.254	0.307
Enrolled	0.320	0.264	0.340	0.278
Completed or enrolled in tertiary education	**0.141**	**0.185**	**0.154**	**0.216**
Completed	0.016	0.019	0.018	0.024
Enrolled	0.124	0.166	0.136	0.191
All	**1.000**	**1.000**	**1.000**	**1.000**
Total number of observations	**9765**	**8875**	**13494**	**12268**

[15] There are no tuition fees at Danish educational institutions. Student grants, which are available from the students' 18th birthday, are granted by the state in the form of state education grants and loans. For a further description of the educational system, see e.g. Jakobsen and Smith (2006).

More males compared to females drop out of the educational system with no further qualifications than primary school, and about half of those have never commenced any education beyond primary school. Around 60% of both males and females have completed or enrolled in high school or vocational education, while 15-20% have completed of enrolled in tertiary education. More females (about 20%) than males (about 15 %) have completed or enrolled in tertiary education.

When we compare the educational outcome before and after the reform, we see that the dropout rate declines by 2 and 1 percentage points for males and females, respectively. When we look behind this decline in the dropout rate, it is seen that among the dropouts, fewer dropouts do not enroll at any education after primary school while more individuals drop out from vocational education. There is also a change in the composition of non-dropouts. While there is a slight decline in the proportion that completed secondary education, there is an even bigger increase in the proportion that enrolled in tertiary education. This indicates that the reform resulted in both fewer dropouts and longer education.[16] Our main concern in this paper is the former.

Y_i is defined as the educational state in the same year as we measure marriage.[17] Students receive universal grants while they are under education and they face no tuition fees. This means that they lack incentives to withdraw formally at the same point in time as they decide to withdraw in practice.

5.3. Data description

In Table 4, we present summary statistics for the outcome, the treatment and the explanatory variables by year of observation. As explanatory variables we use age, country of origin, education of the parents and type of immigrant (descendent vs. middle generation). We do not have access to good ability measures such as test scores, and therefore, to the extent that ability is not reflected by the education of the parents, it is among the unobserved factors that may influence the outcome and/or the treatment variables.

As already seen in Table 2, the proportion of married individuals is significantly reduced after year 2002 because the proportion married to a marriage migrant is reduced. The dropout rate also declines over the period - it goes from 24.6% in year 2000 to 22.1% in year 2004. However, it is important to notice that the composition of the immigrant sample changes over time. The average age and the gender distribution are unchanged over the period, but the composition across ethnic origin changes over time. The proportion of 2nd generation immigrants (descendents) goes down from 61% to 54%, and the proportion of individuals from the traditionally dominating ethnic origins, Turkey and Pakistan, goes down from 36% to 32% and from 17% to 12%, respectively, over the observation period, reflecting the fact that new groups of immigrants (mainly refugees) constitute an increasing proportion of the stock of Danish immigrants. There are also small changes in the average education of the parents of the individuals in the sample. The change in the composition of the sample means that the decline in the average rate of marriage and the average dropout are potentially misleading.

[16] The fact that more individuals enroll in a longer education program supports the validity of the assumptions behind our empirical strategy. The fact that being enrolled in an education program allows the immigrants who resides in Denmark to be exempted from the rule stating that he or she should be able to support an imported spouse (cf. Section 3.1.) raises the concern that dropout rates were spuriously high before the reform which would cause an identification problem in our empirical analysis. However, if this was the explanation for the decline in dropout, we would have expected no rise in the enrollment in tertiary education.

[17] We have also tried to use the dropout rate the year after marriage has been registered, but the results are very similar. As discussed in Section 2, the timing of the two events depends on anticipations and expectation formation. We do not necessarily learn more about causality from looking at the timing of the two events.

Table 4. Sample means by year, 18-24-year-old immigrants.

Variable	2000	2001	2003	2004	All four
Married to:	0.225	0.214	0.140	0.109	0.164
A marriage migrant	0.157	0.146	0.080	0.051	0.101
An immigrant living in DK	0.068	0.067	0.059	0.058	0.063
Dropout	0.246	0.235	0.231	0.221	0.232
Age	20.5	20.5	20.5	20.5	20.5
Female	0.476	0.477	0.476	0.477	0.476
2nd generation	0.613	0.596	0.552	0.544	0.572
Turkey	0.363	0.356	0.326	0.319	0.338
Pakistan	0.174	0.154	0.125	0.115	0.138
Ex-Yugoslavia	0.087	0.079	0.068	0.062	0.072
Lebanon	0.048	0.064	0.096	0.105	0.082
Vietnam	0.052	0.051	0.051	0.050	0.051
Residual countries	0.276	0.296	0.334	0.349	0.318
Father no education	0.093	0.100	0.111	0.114	0.106
Father completed primary school	0.445	0.429	0.396	0.384	0.409
Father completed post-compulsory education	0.359	0.368	0.386	0.389	0.378
No information regarding fathers education	0.102	0.104	0.108	0.112	0.107
Mother no education	0.264	0.255	0.236	0.222	0.241
Mother completed primary school	0.414	0.415	0.410	0.412	0.413
Mother completed post-compulsory education	0.290	0.300	0.325	0.335	0.315
No information regarding mothers education	0.032	0.030	0.030	0.031	0.031
Number of observations	8815	9825	12266	13496	44402
Dropout rate for ethnic Danes of similar age	0.162	0.160	0.155	0.156	0.158
Number of observations for ethnic Danes	39837	38387	36605	37087	151795

At the bottom of Table 4, it is seen that the dropout rate for ethnic Danes is stable around 16% over the period, whereas the immigrant dropout rate declines from 24.6 to 22.1%. As a consequence, the raw immigrant-native dropout gap goes down from 8.5 to 6.5 percentage points.[18] This indicates that there are no common trends in educational attainment that influence natives as well as immigrants and potentially disrupt the empirical strategy of the paper. However, the ethnic Danes and the immigrants have very different background characteristics, and therefore the numbers in Table 4 are not readily comparable.

In Table 5, we present the dropout rates by subgroups for matched samples to rule out that the patterns are driven by common trends and composition effects. Samples of subgroups of immigrants are matched with similar ethnic Danes by propensity score matching using nearest neighbor matching. The samples are matched on age, year and parental education, and performance of the matching algorithm is checked by balancing tests; that is t-tests of equality of means of all control variables.

For all male subgroups, the dropout rate is higher for immigrants than for similar ethnic Danes. For the female subgroups, this is not the case after controlling for family background.

For all male subgroups, the dropout rate declines for immigrants while it goes up for similar ethnic Danes. As a consequence, the immigrant-native dropout gap also goes down. For the female subgroups, it is unclear if the dropout rate generally declines for immigrants although it clearly goes down for Turkish immigrants. However, the dropout rate is stable or slightly increasing for the matched ethnic Danes.

[18] Dropout gaps by subgroups (not reported) also decline: For Turkish males, the dropout gap declines from 20.2 to 13.5 (12.6) percentage points in 2004 (2005), while for Turkish females, the gap declines from 11 to 7 (5.5) percentage points in 2004 (2005). For Pakistani males, the gap declines from 9 to 7.8 (5.8) percentage points in 2004 (2005), while for Pakistani females, it only declines from 3.2 to 2.5 (3.1) percentage points in 2004 (2005).

From Table 5, we conclude that the decline in the dropout rate of immigrants is not caused by a common trend affecting also ethnic Danes. Furthermore, the decline is stronger for the subgroups of Turks than it is for Pakistanis, which indicates that decline in dropout in the total sample may be understated due to changing composition by country of origin.

Table 5. Dropout rates for 18-24-year-olds. Matched samples.

		2000	2001	2002	2003	2004	Matches
				All immigrants			
Males	Immigrants	0.251	0.241	0.252	0.240	0.234	20809
	Ethnic Danes	0.176	0.179	0.177	0.178	0.185	44228
	Gap	**0.074**	**0.062**	**0.075**	**0.062**	**0.048**	
Females	Immigrants	0.196	0.182	0.195	0.196	0.183	19215
	Ethnic Danes	0.184	0.181	0.181	0.175	0.179	39969
	Gap	**0.012**	**0.001**	**0.014**	**0.021**	**0.004**	
				Turks			
Males	Immigrants	0.323	0.317	0.323	0.312	0.284	6710
	Ethnic Danes	0.218	0.227	0.233	0.239	0.241	15221
	Gap	**0.105**	**0.090**	**0.090**	**0.073**	**0.043**	
Females	Immigrants	0.241	0.222	0.237	0.231	0.206	6971
	Ethnic Danes	0.238	0.242	0.251	0.238	0.246	14753
	Gap	**0.002**	**-0.021**	**-0.014**	**-0.008**	**-0.040**	
				Pakistani			
Males	Immigrants	0.248	0.247	0.259	0.242	0.232	3424
	Ethnic Danes	0.176	0.185	0.190	0.184	0.200	10695
	Gap	**0.072**	**0.062**	**0.069**	**0.058**	**0.032**	
Females	Immigrants	0.121	0.106	0.114	0.112	0.115	3219
	Ethnic Danes	0.199	0.200	0.197	0.198	0.210	10382
	Gap	**-0.077**	**-0.093**	**-0.083**	**-0.086**	**-0.094**	

Note: We match on: year, age and parental education. Performance is checked by balancing tests on equality of means in the matched samples.

Figure 1 shows the proportion of individuals who are married and the proportion of individuals who are married to a marriage migrant in a given age group in the reform year +/- two years. Time period 0 is the reform year 2002. Before the reform, the proportion of individuals who were married was stable at about 8% for the 18-year-olds, and about 43% for the 24-year-olds. Similarly, the proportion of individuals who were married to a marriage migrant was stable at about 5% for the 18-year-olds, and at about 28% for the 24-year-olds. Hence, marriage migration clearly constituted the dominating form of marriage in the sample.

After 'the 24-year reform' was put into force in year 2002, the proportion married to a marriage migrant within each age group is exactly equal to the proportion observed in the previous year for the age group which is one year younger. Almost the same pattern is observed for the proportion married since marriage migration constituted the dominating part of marriages in the sample.

Figure 1. Marriage patterns around the reform year conditional on age.

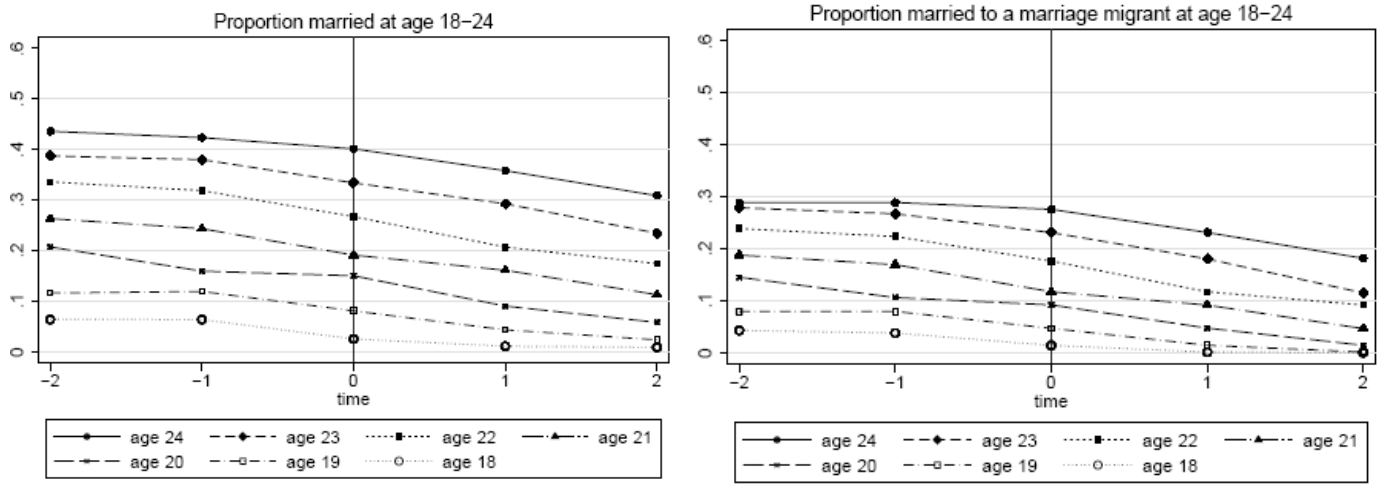
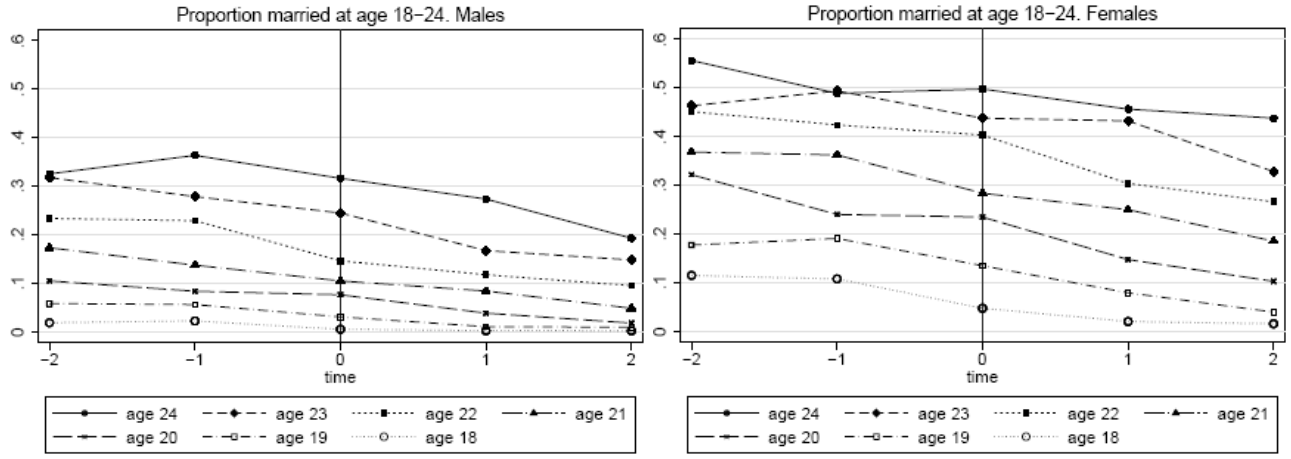


Figure 2. Marriage patterns around the reform year conditional on age, by gender.



In Figure 2, we see that more females than males in the sample get married. This is most likely due to the fact that females marry earlier than males. For instance, 12% of females and only 2% of males were married at age 18 before the reform. At age 24, roughly 50% of females and 35% of males were married before the reform. For marriage migration, the pattern is somewhat similar (not shown).

In Figure 3, we illustrate dropout rates for immigrants by gender. The dropout rate is much higher for males than it is for females at all ages, which is consistent with Lange (2007). After the reform, the dropout rate tends to go down. For males aged 21-24, the dropout rate clearly declines in or after the reform year, whereas the picture is less clear for the younger males. For the 18-19-year-old females, the dropout rate also tends to go down, whereas the picture is less clear for the older females. This gender difference could be explained by the fact that immigrant males tend to marry later than females. If the decline in dropout rates were due to changed marriage patterns, the effects should – exactly as we see in the figure - be strongest for older males and for younger females.

There is a peak in dropout of 19 percentage points in year 0 for the 18-year-old females which could be due to an announcement effect of the reform which led more young females to marry in the first half of year 2002. The peak for the birth cohort who turned 18 in 2002 seems to carry over to the next year when this cohort turns 19 years. The birth cohort who was 17 in the year 2002, 18 years old in year 2003 (year 1 in Figure 3), 19 years old in 2004 (year 2), and 20 years old in 2005 (year 3) seems to have lower dropout rates

than the other birth cohorts. This tendency is also present for the male birth cohort who is 17 in year 2002 although it is not as persistent as it is for females. Judging from these descriptive figures, it seems that the females were more affected by the announcement effect than the males. This makes perfect sense since females get married earlier, and therefore the cost of having to postpone marriage until age 24 would be much larger for females than males, and the families may thus have decided to find an appropriate partner already in the announcement period.

Figure 3. Dropout rates for immigrants around the reform year conditional on age, by gender.

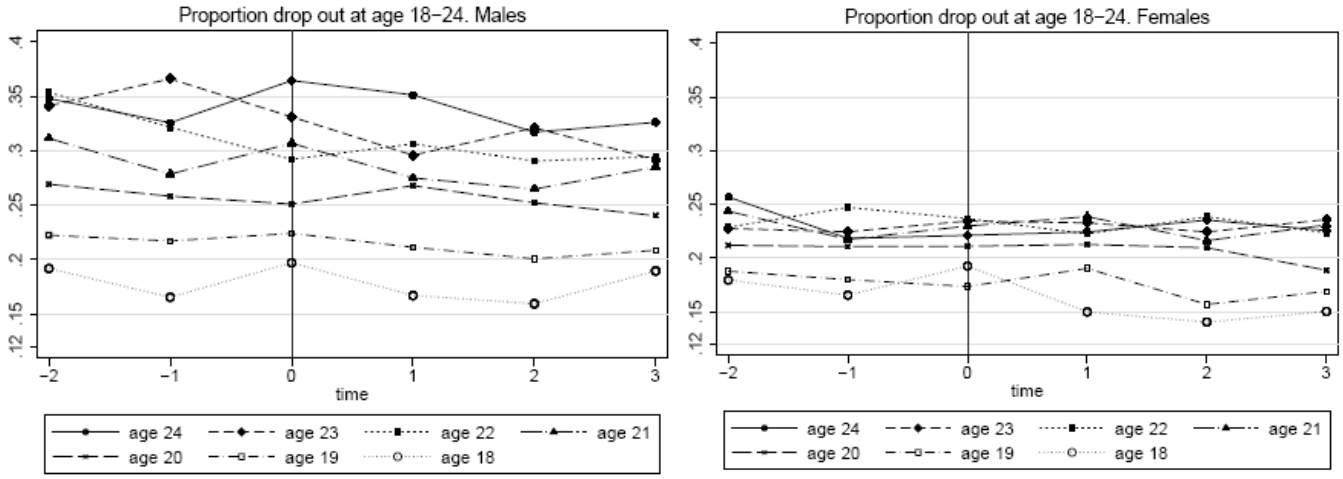
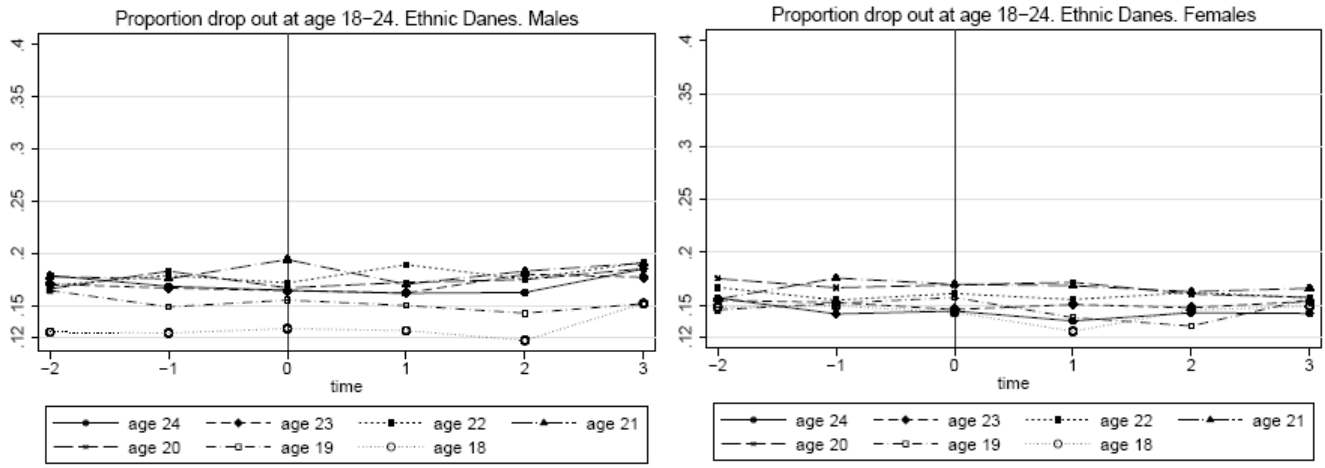


Figure 4. Dropout rates for ethnic Danes around the reform year conditional on age, by gender.



In Figure 4, we depict the dropout rates for ethnic Danes conditional on age, by gender. As seen in Table 4, the level of dropout is much lower for ethnic Danes than for immigrants, and furthermore the difference across ages is much smaller. This emphasizes the fact that the main obstacle explaining low educational attainment of immigrants is that they do not complete the education that they enroll in, whereas for ethnic Danes, lack of educational attainment is to a larger extent a matter of not enrolling. Dropout rates are much more stable over time than for immigrants. And most importantly, there is no decline across the reform year for any of the age groups.

In Appendix, Figures A1-A4, the same graphs are shown for the more homogenous samples of Turkish and Pakistani males and females. The rate of marriage is much higher for Turks than it is for Pakistanis, and it goes down after the reform for both groups although the effect is not as prevalent for some of the age groups

among the Pakistanis as it generally is for Turks. Dropout rates decline for Turks, but the decline for Pakistanis is much smaller.

6. Empirical Analyses

Let Z_i be a policy variable indicating the possibility to obtain family reunification with a partner from abroad. The possibility to obtain family reunification shifts in July 2002. We use annual data, and therefore we exclude the reform year 2002. Let $Z_i = 1$ until 2002, and $Z_i = 0$ otherwise. We assume that the identifying assumptions hold as discussed in Section 4, such that the policy variation in the possibility to obtain family reunification gives identification of the causal effect of marriage on education under the stated assumptions. We choose to estimate bivariate probit models because both the outcome and the treatment variables are binary variables. However, as a benchmark we estimate univariate probit models.

In Table 6, we show the marginal effects from univariate probit regressions of dropout rates from education on an indicator for being married. In specification (1), we see that the dropout rate is 16.6 percentage points higher for individuals who are married than it is for singles, and this number is similar for males and females. In specification (2), we distinguish between whether the individual is married to a marriage migrant or to another immigrant living in Denmark. We see that the coefficients for the two spouse types are similar for females, but they are significantly different from each other for males. Being married to a marriage migrant is associated with a 19.1 percentage points higher dropout rate, whereas being married to another immigrant is associated with only a 9.3 percentage points higher dropout rate. These effects are consistent with the family investment hypothesis presented in Section 2. Under an assumption of no correlation between the unobservables affecting dropout and the unobservable affecting marriage (u_i, ε_i), that is $\rho = 0$, the numbers in Table 6 identify the average treatment effect (ATE).

Table 6. Marginal effects of dropout on being married. Univariate probit models. Pre-reform data only.

Specification	All	Males	Females
(1) Married	0.166 **	0.165 **	0.160 **
	0.009	*0.015*	*0.011*
(2) Married			
to marriage migrant	0.168 **	0.191 **	0.151 **
	0.011	*0.018*	*0.013*
immigrant	0.161 **	0.093 **	0.175 **
	0.015	*0.026*	*0.017*
Number of observations	18640	9765	8875

Note: We report coefficient estimates and standard errors in italics.
As explanatory variables are included indicator variables for age categories, being a 2nd generation immigrant, countries of origin and for parents' education.

By use of a recursive bivariate probit model with a reform-based exclusion restriction, we now investigate to what extent these effects are causal or not by allowing for $\rho \neq 0$. Following Monfardini and Radice (2007), we are cautious in case the significant estimate of α from the univariate probit does not transform into either a statistically significant estimate of α or a statistically significant estimate of ρ in the bivariate probit model.

6.1. Main results

In Table 7, we present the results from estimation of the bivariate probit model separately for males and females. For both males and females, the effect of the reform indicator on the marriage indicator is significant and strong (t-statistics around 8). Hence, the drop in marriage that we find after controlling for a

set of explanatory variables is consistent with what we saw in Figures 1 and 2. Looking at the estimates from the dropout equation, we see that the causal effect of marriage on dropout rates is positive and significant for males, whereas it is not statistically significant for females. The average treatment effect on the treated (ATET) is 21.0 and -5.1 percentage points for males and females, respectively, and it is only significant for males; the average treatment effect (ATE) is estimated to be slightly lower. For males, the estimated correlation coefficient, ρ, is insignificant, whereas it is estimated to be .392 and significantly positive for females.[19] Hence, the bivariate probit model indicates that the correlation between marriage and dropout that we saw in the univariate probit model in Table 6 reflects a causal effect for males, while it reflects correlation between unobservables for females.

For males, the estimated correlation coefficient, rho, is insignificant, which means that the unobservables influencing marriage and dropout are not correlated, and therefore a univariate probit model would be sufficient. In Table 6, the estimated marginal effect (ATE) for males is .165 percentage points, which is not significantly different from the ATE in Table 7 of .197 percentage points. The estimates for females are significantly different: .160 percentage points in the univariate model compared to .an ATE of -.051 in the bivariate model. For females, the unobservables are positively correlated, which indicates that some unobservable characteristics increase the probability of marriage as well as the probability of dropout. Those variables could be family variables such as traditional values or norms. The candidate unobserved variables should be variables that influence the marriage behavior of males and females differently, and therefore it seems more likely that they are variables such as religion, norms or preferences rather than for instance language proficiency.

[19] The first row of Table 8 shows that the same conclusion about the correlation coefficient is reached no matter whether we rely on the t-test or the LR-test, the latter of which is more robust to violation of the functional form assumption.

Table 7. Results from estimation of a recursive probit model, by gender.

	Males				Females			
	Dep.var: Drop out		Dep.var: Married		Dep.var: Drop out		Dep.var: Married	
Variable	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.
Married	0.567 **	*0.121*			-0.149	*0.158*		
Before 2002			0.407 **	*0.064*			0.449 **	*0.052*
Age 19	0.155 **	*0.032*	0.544 **	*0.082*	0.107 **	*0.036*	0.454 **	*0.048*
Age 20	0.311 **	*0.036*	0.835 **	*0.092*	0.255 **	*0.043*	0.835 **	*0.058*
Age 21	0.338 **	*0.043*	1.159 **	*0.105*	0.320 **	*0.054*	1.164 **	*0.072*
Age 22	0.414 **	*0.051*	1.445 **	*0.126*	0.357 **	*0.064*	1.448 **	*0.090*
Age 23	0.457 **	*0.060*	1.726 **	*0.147*	0.372 **	*0.076*	1.749 **	*0.108*
Age 24	0.458 **	*0.069*	1.989 **	*0.171*	0.419 **	*0.088*	2.026 **	*0.127*
2nd generation	-0.109 **	*0.022*	-0.265 **	*0.031*	-0.139 **	*0.024*	-0.161 **	*0.025*
Pakistan	-0.126 **	*0.033*	-0.516 **	*0.040*	-0.431 **	*0.043*	-0.634 **	*0.034*
Ex-Yugoslavia	-0.013	*0.042*	-0.788 **	*0.057*	0.077	*0.053*	-0.631 **	*0.044*
Lebanon	0.013	*0.041*	-0.654 **	*0.059*	0.419 **	*0.043*	-0.063	*0.044*
Vietnam	-0.707 **	*0.057*	-1.503 **	*0.093*	-0.698 **	*0.084*	-1.469 **	*0.079*
Residual countries	-0.202 **	*0.034*	-1.354 **	*0.047*	-0.348 **	*0.049*	-0.886 **	*0.031*
Father completed primary school	0.080 **	*0.032*	-0.020	*0.046*	-0.002	*0.034*	-0.009	*0.037*
Father completed post-compulsory education	-0.038	*0.033*	-0.041	*0.049*	-0.088 **	*0.035*	-0.079 **	*0.039*
Father education missing	0.179 **	*0.040*	-0.152 **	*0.064*	0.167 **	*0.044*	-0.163 **	*0.050*
Mother completed primary school	-0.116 **	*0.023*	-0.012	*0.032*	-0.174 **	*0.025*	-0.079 **	*0.026*
Mother completed post-compulsory education	-0.186 **	*0.026*	-0.058	*0.040*	-0.167 **	*0.031*	-0.253 **	*0.032*
Mother education missing	0.049	*0.051*	0.015	*0.075*	0.107 *	*0.060*	-0.166 **	*0.069*
Constant	-0.732 **	*0.049*	-2.129 **	*0.099*	-0.694 **	*0.063*	-1.333 **	*0.061*
Year and cohort dummies included	yes		yes		yes		yes	
rho	-0.100	*0.068*			0.392 **	*0.104*		
ATE	0.197 **	*0.045*			-0.039	*0.040*		
ATET	0.210 **	*0.043*			-0.051	*0.057*		
	LR test	p-value			LR test	p-value		
LR-test for cohort and year dummies	57.34	*<0.000*			135.3	*<0.000*		
Number of observations	23259				21143			

Note: ** indicates significance at a 5%-level, whereas * indicates significance at a 10%-level.
Standard errors on ATE and ATET are calculated using the delta method.

Looking at the estimated effect of the other variables in the estimation, we see that the rate of marriage is largest for Turks (the reference group), whereas the dropout rate is largest for Turks and individuals from Lebanon and Ex-Yugoslavia.

When we look at the effect of parents' education on marriage and dropout rates, it is seen that mainly the effect of mothers' education is important, and it is most important for the behavior of the females. Education of the parents does not influence marriage of males, but it does indeed influence marriage of females significantly. The higher the level of education of the parents, the lower is the probability that a female is married at a given age. The effect of mothers' education on marriage is stronger than that of fathers' education, which may be because mothers' education to some extent proxy norms and traditions in the family regarding gender role patterns. Education of the parents does also influence dropout rates of females significantly. The higher the level of education, the lower is the dropout rate.

The fact that the apparent relationship between marriage and dropout for females disappears in the bivariate probit model seems to be explained by the fact that the families where daughters both marry and dropout are a certain type of families where the parents have low or no education, and/or where certain unobservable

characteristics are present. Hence, the findings regarding the effects of parents' education and the estimated correlation coefficients are very consistent.

For males, the picture is different, and the correlation between marriage and dropout seems to be a causal impact. Our interpretation of this effect is that it is due to a family investment strategy where males who marry marriage migrants choose to suffer their own education in order to allow the imported spouse to invest in country specific capital. Notice that the effect cannot be caused by the very restrictive requirements in the law stating that the person living in Denmark should be able to support his/her future spouse because students are exempted from this requirement.

In Table 7, we have included indicator variables for cohort and year which are jointly significant. However, in Appendix A, Table A3, we present the results excluding these indicator variables, and the main conclusions are unchanged.

6.2. Heterogeneous treatment effects

In order to allow for heterogeneous treatment effects, we look at different age groups separately and at Turks and Pakistanis separately. Furthermore, we look at individuals with self-employed fathers and without self-employed fathers separately to potentially gain further insight as to whether the family investment hypothesis is the right interpretation of the results.

In Table 8, we present the results for the parameters of main interest, while the detailed results are given in Appendix A. Tables A1 and A2 show the marginal effects for subgroups as estimated from univariate probit models. It is seen that marriage is associated with a significantly higher dropout rate for all subgroups, and that the coefficient for being married to a marriage migrant is systematically larger for all subgroups of males. Below, we investigate which of these associations represent a causal impact, and which of the associations are spurious.

For age-specific sub-samples, individuals are grouped in two-year intervals. The samples become smaller, and thus the coefficients are less well-determined. However, the effect is large and relatively well-determined for the 18-19-year-olds of both genders; for males, the ATET is almost 40 percentage points, whereas the effect for females is 9 percentages points (s.e. .061). For 18-19-year-old males the correlation coefficient is significantly negative, while it is positive for 18-19-year-old females (p-value .089), which indicates a different behavior of males versus females.

For males, the ATET is significantly positive for all age groups except the oldest one. The correlation coefficient is estimated to be negative, but it is only significantly different from zero for two of the sub-samples. For females up until about 21 years, the association between marriage and dropout is driven by correlation of unobservables, while for females aged 22-24 years, we are not able to disentangle the correlation between the unobservables from the causal impact since both are estimated to be insignificant.

The fact that the results vary by gender and age-group is reassuring since it indicates that the main results are not just driven by general trends in enrollment or completion over time or cohorts. Figures 1 and 2 revealed that female immigrants marry at an earlier age than male immigrants, and the age-specific coefficients in Table 8 show that early marriage of females is not likely to induce dropout since the apparent association between marriage and dropout is driven by correlation of unobservables. Only for the subgroup of 18-19-year-olds, there may be a rather small causal effect (which is only significant at a 12% significance level).

Table 8. Selected results from recursive bivariate probit regressions. By subgroups.

	Males		Females	
	ATET	**Rho**	**ATET**	**Rho**
		All		
All immigrants	0.210 **	-0.100	-0.051	0.392
	0.043	*p=0.147*	*0.057*	*p<0.001*
Number of observations	23259		21143	
		By age group		
18-19-year-olds	0.387 **	-0.414	0.094	0.182
	0.085	*p=0.059*	*0.061*	*p=0.089*
Number of observations	8659		7996	
19-20-year-olds	0.362 **	-0.266	0.011	0.325
	0.105	*p=0.152*	*0.059*	*p=0.002*
Number of observations	7784		7122	
20-21-year-olds	0.259 **	-0.110	0.013	0.331
	0.093	*p=0.454*	*0.066*	*p=0.006*
Number of observations	6914		6232	
21-22-year-olds	0.306 **	-0.278	0.089	0.179
	0.080	*p=0.038*	*0.076*	*p=0.221*
Number of observations	6145		5459	
22-23-year-olds	0.251 **	-0.201	0.054	0.164
	0.080	*p=0.127*	*0.094*	*p=0.373*
Number of observations	5459		4917	
23-24-year-olds	0.131	-0.005	-0.027	0.253
	0.105	*p=0.976*	*0.186*	*p=0.481*
Number of observations	4790		4332	
		By country of origin		
Turkey	0.156 **	0.011	0.157 **	0.002
	0.064	*p=0.909*	*0.063*	*p=0.989*
Number of observations	7573		7439	
Pakistan	0.309 **	-0.349	-0.062	0.433
	0.128	*p=0.156*	*0.076*	*p=0.019*
Number of observations	3151		2978	
		By self-employment status of father		
Self-employed father	0.426 **	-0.428	-0.120	0.504
	0.096	*p=0.019*	*0.106*	*p=0.008*
Number of observations	2811		2584	
Not self-employed father	0.193 **	-0.067	0.054	0.196
	0.043	*p=0.334*	*0.040*	*p=0.008*
Number of observations	17778		16268	

Note: ** indicates significance at a 5%-level, whereas * indicates significance at a 10%-level.
As explanatory variables are included those from table 7, except for age groups and country of origin, where cohorts and year indicators are excluded. p-value is for LR-test for H0: Rho=0.
Standard errors on ATE and ATET are calculated using the delta method.

For the Turkish and Pakistani sub-samples, results differ a lot. We find that the causal effect of marriage on dropout is significantly positive for both Turkish females and males, while the correlation coefficient cannot be rejected to be zero. For Pakistani males, the causal effect is significantly positive, while the correlation coefficient is estimated to be negative (p-value .16). For Pakistani females, the causal effect cannot be rejected to be zero, while the correlation coefficient is significant and large in magnitude (.43).

The fact that the results vary by ethnic origin is reassuring since it indicates that the main results are not driven by composition effects or trends over time or cohort. The sub-samples divided by ethnic origin are more homogenous with respect to preferences, norms and traditions for marriage and education, and therefore they should give a clearer picture of the relevant relationships. We indeed find that behavior varies by ethnic origin since marriage tends to induce dropout for individuals of Turkish origin no matter their gender, while this is only the case for males of Pakistani origin and not for females of Pakistani origin.

When we find a positive effect of being married, we interpret that as being consistent with a family investment strategy of a borrowing constrained household; the spouse living in Denmark gives up his or her education to allow the newly arrived partner to invest in host-country specific capital. If this interpretation is correct, we would expect particular patterns for youths with a self-employed father, who would usually be the owner of a small pizza restaurant or a kiosk. Females with a self-employed father may have the opportunity to offer the newly arrived partner a job with flexible hours and limited language requirements. This would facilitate her staying in education and staying in the investor role, and thus depress the effect of her marriage on the dropout probability. Males with a self-employed father may have the opportunity to get a job more easily than other immigrants. This would facilitate him undertaking the borrower role in the newly formed partnership and increase the effect of marriage on dropout.

In Table A2, we see that the association between dropping out and being married as well as the association between dropping out and being married to a marriage migrant in the univariate probit models are larger for males with a self-employed father than for males without a self-employed father. The opposite is the case for females with a self-employed father. At the bottom of Table 8, we report the results from the bivariate probit model. The same pattern shows up in the estimated causal impact of marriage; however, the difference is clearly not significant for females. We find that the results from this exercise are in accordance with our interpretation of the impact of marriage.

6.3. Results: Other treatment and outcome variables

Table 9 presents selected results from estimations using different treatment indicators. In specification (1), we replicate the main results (cf. section 6.1). In specification (2), we use as a treatment indicator ‘being married to a marriage migrant’ instead of just ‘being married’. We find a slightly higher causal effect for males (.26), and we now find a positive though insignificant treatment effect for females. In specification (3), we change the treatment indicator to indicate whether the individual is cohabiting or married with an immigrant. The results are similar to the results for specification (1). In specification (4), we use as a treatment indicator whether the individual is getting married or not, i.e. the indicator assumes the value of 1 only in the year of the marriage event. In this case, the estimated impact of marriage is estimated to be zero. We have also tried (not reported) versions of the dropout variable that only take the value one in the year of the dropout or in the year before the dropout, but none of these changes affected the results.

The robustness checks in Table 9 confirm the finding that the causal impact is significantly positive for males, but not for females, even when we use a set of alternative treatment indicators. However, specification (4) shows that it is unclear when the effect of marriage on dropout kicks in for males; it is not necessarily an immediate or delayed effect, dropout may as well happen before the wedding as after the wedding.

Table 9. Selected results from recursive bivariate probit regressions. Different treatment indicators.

	Males		Females	
	ATET	**Rho**	**ATET**	**Rho**
(1) Being married	0.210 **	-0.100	-0.051	0.392
	0.043	*p=0.147*	*0.057*	*p<0.001*
(2) Being married to a marriage migrant	0.260 **	-0.133	0.020	0.200
	0.047	*p=0.074*	*0.049*	*p=0.016*
(3) Cohabiting with or being married to an immigrant	0.246 **	-0.191	-0.061	0.428
	0.043	*p=0.0094*	*0.077*	*p<0.001*
(4) Getting married	0.106	-0.139	0.075	-0.074
	0.132	*p=0.473*	*0.093*	*p=0.645*
Number of observations	23259		21143	

Note: ** indicates significance at a 5%-level, whereas * indicates significance at a 10%-level.
As explanatory variables are included those from table 7. p-value is for LR-test for H0: Rho=0.
Standard errors on ATE and ATET are calculated using the delta method.

Above we discussed the consequences of slight changes in the treatment indicator. We have also experimented with slight changes in the outcome variable. In Table 3, we saw that the proportion of males and females that completed or enrolled in a tertiary education program increased by more than 1 and more than 3 percentage points after the reform. Therefore, we have tried an alternative definition of the outcome variable, dropout, which takes the value one if the individual drops out of education after high school or vocational education, and zero otherwise. Using this outcome variable in the bivariate probit model, we find a large causal impact of marriage on dropout after high school or vocational education for females, ATET=.215 (.049), and a smaller insignificant impact for males, ATET=.030 (.038). The estimated correlation coefficient, ρ, is estimated to be significantly positive for males, ρ=.198 (.099), but not for females. To conclude, we see that marriage interferes with educational attainment at the bottom of the educational distribution for males, while it only potentially interferes with educational attainment at the top of the distribution for females.

6.4. Exclusion restriction vs. functional form: A comparison of bivariate probit models and 2SLS

As indicated by Monfardini and Radice (2007), a signal of practical identification problems due to compensation effects could be that a univariate probit estimation of (1) shows a statistically significant estimate of α, while the bivariate probit model leads to insignificant estimates of both α and ρ. As is clear from Table 8 (and Tables A1 and A2), this is only the case for the three oldest sub-samples of females and for the 23-24-year-old males.

To further investigate whether our estimates of the effect of marriage on dropout are identified by the functional form assumption or the exclusion restriction, we estimate 2SLS as well as different versions of the bivariate probit model as recommended by Altonji, Elder and Taber (2005) as informal checks of the source of identification.

Firstly, we estimate 2SLS which only imposes the linearity assumption while ignoring the fact that both treatment (that is, being married) and outcome (that is, dropout) are binary variables. However, differences between the estimates from the bivariate probit model and the 2SLS may also arise because the linear probability model provides a poor approximation for binary outcome variables relative to the bivariate probit.

Secondly, we estimate bivariate probit models with and without imposing the exclusion restriction. The exclusion restriction is useful for non-parametric identification, but the linearity and normality assumptions of the model are sufficient for identification. We check whether the estimated coefficients and standard errors are affected by the exclusion restriction as this necessarily indicates whether the exclusion restriction contributes to identification.

Finally, we take a completely different approach to examining the extent to which the identification goes through the functional form assumptions or the exclusion restriction. As suggested by Altonji, Elder and Taber (2005), we estimate two stage probit models. In the first step, we estimate the marriage equation $M_i = 1[\beta Z_i + \varphi \mathbf{X_i} + \varepsilon_i > 0]$ by a univariate probit model. In the second step, we estimate the dropout equation by a univariate probit model, but rather than including $\Phi(\hat{\varphi}\mathbf{X_i} + \hat{\beta}Z_i)$, we include separate predicted probabilities holding $\mathbf{X_i}$ and Z_i constant at their sample means. The second stage models for dropout are then:

$$(5) \qquad Y_i = 1[\gamma \mathbf{X_i} + \alpha_1 \Phi(\hat{\varphi}\overline{\mathbf{X_i}} + \hat{\beta}Z_i) + \alpha_2 \Phi(\hat{\varphi}\mathbf{X_i} + \hat{\beta}\overline{Z_i}) + u_i > 0]$$

The coefficient of main interest is α_1, which captures the effect of marriage on dropout which is driven by the variation in Z_i. As will be clear when the results are presented, the estimate of α_1 is often similar to the α as estimated by 2SLS, which is known to identify the local average treatment effect (LATE), which is the effect of marriage on dropout for the individuals who change their marriage behavior due to a change in Z_i.[20]

Table 10. Selected results from a reduced form OLS and from 2SLS. By subgroups.

	Males								**Females**							
	(1)		**(2)**		**(3)**		**(4)**		**(1)**		**(2)**		**(3)**		**(4)**	
	2SLS		**ATE w. excl.**		**ATE w/o excl.**		**M.E. *Xi* constant**		**2SLS**		**ATE w. excl.**		**ATE w/o excl.**		**M.E. *Xi* constant**	
	Married	std. err.	Married	std. err.	Married	std. err.	Married	std. err.	Married	std. err.	Married	std. err.	Married	std. err.	Married	std. err.
All immigrants	0.229 **	*0.082*	0.197 **	*0.045*	0.196 **	*0.046*	0.388	*0.410*	0.126 **	*0.046*	-0.039	*0.040*	-0.066	*0.0441*	0.121	*0.1125*
Number of obs.	23259								21143							
	By age groups								**By age groups**							
18-19-year-olds	0.428	*0.283*	0.475 **	*0.151*	0.468 **	*0.161*	0.716	*0.5065*	0.204 **	*0.076*	0.074	*0.055*	-0.062	*0.0608*	0.222 **	*0.0866*
Number of obs.	8659								7996							
19-20-year-olds	0.122	*0.177*	0.383 **	*0.131*	0.478 **	*0.111*	0.150	*0.2522*	0.066	*0.068*	0.008	*0.045*	-0.095	*0.0727*	0.062	*0.0714*
Number of obs.	7784								7122							
20-21-year-olds	0.136	*0.144*	0.255 **	*0.103*	0.334 **	*0.113*	0.160	*0.1927*	0.048	*0.072*	0.010	*0.054*	-0.141	*0.145*	0.042	*0.070*
Number of obs.	6914								6232							
21-22-year-olds	0.246 **	*0.120*	0.307 **	*0.082*	0.345 **	*0.094*	0.274 *	*0.143*	0.124	*0.080*	0.081	*0.074*	-0.213 **	*0.0947*	0.111	*0.0756*
Number of obs.	6145								5459							
22-23-year-olds	0.303 **	*0.114*	0.248 **	*0.081*	0.200 *	*0.123*	0.307 **	*0.120*	0.136	*0.098*	0.050	*0.091*	-0.259 **	*0.067*	0.115	*0.0905*
Number of obs.	5459								4917							
23-24-year-olds	0.216	*0.142*	0.125	*0.106*	0.026	*0.170*	0.214	*0.1437*	0.140	*0.142*	-0.025	*0.170*	-0.356 **	*0.0347*	0.119	*0.1285*
Number of obs.	4790								4332							
	By country of origin								**By country of origin**							
Turks	0.271 **	*0.081*	0.149 **	*0.065*	-0.054	*0.105*	0.267 **	*0.0812*	0.166 **	*0.066*	0.154 **	*0.064*	0.130	*0.1718*	0.136 **	*0.0558*
Number of obs.	7573								7439							
Pakistanis	0.088	*0.241*	0.327 **	*0.149*	0.386 **	*0.138*	0.118	*0.2727*	-0.018	*0.096*	-0.044	*0.049*	-0.098	*0.0746*	-0.024	*0.0934*
Number of obs.	3151								2978							
	By self-employment status of the father								**By self-employment status of the father**							
Self-employed father	0.470 **	*0.240*	0.421 **	*0.093*	0.407 **	*0.104*	0.723 **	*0.3361*	0.051	*0.122*	-0.081	*0.056*	-0.208 **	*0.046*	0.045	*0.106*
Number of obs.	2811								2584							
Not self-employed father	0.174 *	*0.090*	0.179 **	*0.045*	0.186 **	*0.050*	0.225 *	*0.1354*	0.092 *	*0.050*	0.046	*0.036*	0.003	*0.059*	0.080 *	*0.0454*
Number of obs.	17778								16268							

Note: As explanatory variables are included indicator variables for age categories, being a 2nd generation immigrant, countries of origin and for parents' education. For All immigrants, cohort and year indicators are included in M.E. and ATE. ** indicates significance at a 5%-level, whereas * indicates significance at a 10%-level. Std.errors are corrected for the use of generated regressors when needed.

[20] When we refer to α_1 in the text, we actually mean the *marginal effect* of the term $\Phi(\hat{\varphi}\overline{\mathbf{X_i}} + \hat{\beta}Z_i)$ on the dropout probability.

We summarize the results in Table 10. For the bivariate probit models we report the estimated ATEs rather than the ATETs because they are directly comparable with the coefficients from 2SLS and α_1.

For the sample of all immigrant males, the effect of marriage on dropout is consistently around 20 percentage points with the exception of the estimate of α_1 in equation (5), which is somewhat higher though imprecisely estimated. As also indicated by the exogeneity tests (c.f. Section 6.2.), marriage is not endogenous with respect to the dropout rate, and this is also why 2SLS, univariate probit and OLS (not reported) give very similar results.[21] When we look at separate age-groups of males, the estimated effects are also similar, although the 2SLS and α_1 are much noisier with higher standard errors, and only for 21-22-year-olds and 22-23-year-old males, the point estimates are similar to those estimated in the bivariate probit models. The main conclusion is that for the males, we could also rely on the conclusions from the univariate models.

For the sample of all immigrant females, we find that the effect of marriage on dropout is not estimated to be significantly different from zero, and marriage is *not* exogenous with respect to dropout. However, the insignificant effect seems to be partly a consequence of the functional form assumption as the 2SLS shows that the effect of marriage is 12.6 percentage points and significantly different from zero, and the point estimate of α_1 is similar. Both the magnitude of the effect and the fact that the effect is significant are inconsistent with the finding from the bivariate probit model. When we divide by age-group, it is seen that relying on the variation in the instrument rather than non-linearity tends to show a positive and significant effect for 18-19-year-olds, which is exactly the one group where we found a borderline significant causal effect using the bivariate probit model. For the 21-22 and 22-23-year-olds, the bivariate probit model was inconclusive about the causal effect, but when we rely more on the variation in the instrument than the functional form assumptions, we find borderline significant effects (t-values about 1.4-1.5). When we estimate the bivariate probit model without the exclusion restriction, the effect of marriage is estimated to be significantly negative, which clearly underlines that the results depend on the exclusion restriction.

When we separate by ethnic origin, we see that the exclusion restriction is an important source of identification for Turkish immigrants in the bivariate probit model; for males the point estimate and the standard error differ when identification relies only on functional form, whereas for females, the point estimate is similar when identification relies only on function form, but it is not well-determined. In both cases, the point estimates from 2SLS and α_1 are higher (but not significantly so). For Pakistani immigrants, our main results show a significant effect for males, while neither 2SLS nor α_1 can estimate precise effects.

When we separate by self-employment status, the effect of being married is not dependent on the exclusion restriction in the case of males. For females with a self-employed father, the conclusion of a zero effect clearly relies on the exclusion restriction since the effect becomes significantly negative without the exclusion restriction. For females without a self-employed father, the positive effect of marriage approaches zero when we disregard the exclusion restriction, whereas both the 2SLS and α_1 are significant at a 10% level. These robustness checks tend to support the idea of the family investment model. We argued earlier that this hypothesis implies that sons of self-employed fathers are more likely to drop out of education when they marry, whereas the daughters of self-employed fathers are less likely to drop out of education when they marry.

6.5. Prediction of the long-run effect of 'the 24-year reform'

Turning back to the main results in Section 6.1, we now predict the long-run effect of the reform on dropout. The empirical evidence suggests that the effect of marriage on dropout is significant for males, and that the

[21] When we compare 2SLS to univariate probit (or OLS), we should look at the coefficient to 'Being married to a marriage migrant' because the 2SLS estimates the local average treatment effect (LATE), which is the effect of marriage on dropout for the compliers. In our case, the compliers are the people who would marry before the reform, but refrain from marrying after the introduction of 'the 24-year reform'; that is exactly people who prefer to marry marriage migrants.

effect of being married is a .2 increase in the dropout rate. Evidence is mixed regarding the females though it tends to point towards a conclusion of no causal effect. We disregard them from this exercise.

The reform was implemented recently, and a number of young people got married due to anticipation of the reform, and therefore we have not seen the full consequence of the reform yet. Only during the next years when data for the years after 2005 become available, and when the birth cohorts who were aged 17-18 years in 2002 get older, we can estimate the full consequence of the reform.

Based on the results presented in this paper, we can make predictions of the long-term effect of 'the 24-year reform' on dropout for males. The results are of course tentative and should be interpreted cautiously. We apply two different assumptions about the expected effect of 'the 24-year reform' on marriage behavior: a conservative and an extreme assumption.

For the conservative prediction, we assume that the proportion who get married between age 18 and 24 is reduced by only 12 percentage points, as is the case in Table 4. A back-of-the-envelope calculation shows that a reduction like that would decrease the dropout rate for males by about .12*.21 = 2.5 percentage points. A more extreme, but potentially realistic, prediction would forecast an even larger effect of the reform on dropout. Assume that only few of the 18-year-olds who were not married in 2002 get married before they turn 24 years.[22] Then, according to Figure 2, the rate of marriage would be reduced from 35% to about 10% for males in the long run. This 'extreme' assumption would predict that the gap in dropout between ethnic Danes and immigrants would be reduced even further due to the reform. For males, the effect would then be: .25*.21=.053.

The magnitude of the dropout gap between male immigrants in our sample of study and a matched sample of ethnic Danish males was around 7 percent before the reform (cf. Table 5). Thus, both under the 'conservative' scenario and under the 'extreme' scenario, this gap is predicted to be reduced significantly due to the reform. For Turkish immigrants, the gap was about 10 percent, and the reduction would also be large both under the conservative and the extreme scenario.

7. Conclusion

It is well-established for several countries that immigrants who marry other immigrants lack behind in terms of educational attainment and other outcomes compared to immigrants who marry natives. If this phenomenon represents a causal impact of marriage behavior on educational outcomes, it provides economic justification for policy intervention in the marriage market of immigrants. On the other hand, if it is just a spurious relationship reflecting unobserved abilities or preferences, intervention is clearly not economically justified.

The issue of policy intervention in the marriage market is a sensitive matter. Even large economic benefits from intervention may be outweighed by even larger non-economic costs due to value systems opposing intervention in this market. In addition, in order to comply with human rights, any policy intervention would need to have implications for natives' marriage possibilities as well, which induces a powerful opposition against the interventions. On the other hand, if forced or arranged marriages are widespread phenomena, human right welfare effects for the young immigrants who are subject to these marriages may work in the same direction as the economic benefits. Though these issues are highly important, we only analyze potential economic factors and benefits in this paper.

The much discussed policy intervention in 2002 in Denmark, 'the 24-year reform', which restricted spouse import, supplies us with exogenous variation in the marriage behavior of immigrants and offers a unique

[22] This assumption is consistent with the fact that there seems to be a low substitutability between spouse types (cf. Table 2 and Figures 1-2). Comparing the left and right panel of Figure 1, it is seen that only 10-12% of the 24-year-olds are married to an immigrant living in Denmark in each of the years 2000-2004. This number is the same for each year before and after the reform, and it indicates that the abolishment of marriage to marriage migrants among 18-24-year-olds will lead to a rate of marriage before age 24 around 10-12%.

opportunity to investigate the issue of causality. We find that the reform induced a large drop in marriage rates among the 18-24-year-olds, and that this drop in marriage rates spills over to improved educational outcomes for some immigrant groups.

We find that the causal impact of marriage on dropout from education after primary school is positive for males, no matter which subgroup we look at. Changing the treatment indicator or the estimation method also preserves the conclusion. However, for females, the association between marriage and dropout does not generally reflect a causal impact. Only for the youngest females aged 18-19 years, for Turkish females and for education beyond the secondary level, we find a positive causal impact. For the main part of the females, the association between marriage and dropout is due to correlation between unobservables influencing both marriage and dropout. Examples of relevant unobservable variables could be norms or traditions for marriage and education within the family.

Based on the results presented in this paper, we forecast the long-term effect of 'the 24-year reform' on dropout behavior. The results are of course tentative and should be interpreted cautiously. Assume that the post-reform marriage behavior in 2003-2004 is indicative of the long-term effect of the reform, then back-of-the-envelope calculations would tell that a reduction like that would decrease the dropout rate of males by 2-5 percentage points, depending on the exact approach. These figures should be compared to the magnitude of the gap in dropout rates between our sample of study and a matched sample of ethnic Danes which was about 7 percentage points before the reform. Thus, a substantial part of the dropout gap between immigrants and native Danes is expected to disappear as a consequence of changed marriage behavior due to the reform.

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Appendix A. Additional results.

Table A1. Marginal effects from univariate probit models of dropout on being married.Pre-reform data only. By age-groups.

	18-19 year olds		19-20 year olds		20-21 year olds		21-22 year olds		22-23 year olds		23-24 year olds	
Specification	**Females**	**Males**	**Females**	**Males**	**Females**	**Males**	**Females**	**Males**	**Females**	**Males**	**Females**	**Males**
(1) Married	0.181 **	0.142 **	0.167 **	0.189 **	0.178 **	0.196 **	0.180 **	0.152 **	0.146 **	0.151 **	0.122 **	0.180 **
	0.023	*0.041*	*0.020*	*0.033*	*0.020*	*0.029*	*0.020*	*0.026*	*0.021*	*0.026*	*0.022*	*0.027*
(2) Married												
to a marriage migrant	0.134 **	0.173 **	0.151 **	0.215 **	0.165 **	0.206 **	0.159 **	0.152 **	0.142 **	0.169 **	0.149 **	0.233 **
	0.027	*0.049*	*0.024*	*0.038*	*0.023*	*0.032*	*0.023*	*0.029*	*0.024*	*0.029*	*0.026*	*0.030*
to an immigrant	0.259 **	0.058	0.194 **	0.112 *	0.201 **	0.164 **	0.219 **	0.152 **	0.153 **	0.102 **	0.074 **	0.059
	0.038	*0.071*	*0.032*	*0.060*	*0.030*	*0.055*	*0.030*	*0.048*	*0.030*	*0.043*	*0.029*	*0.040*
Number of observations	3334	3607	3104	3350	2755	3045	2325	2657	1981	2240	1721	1889

Notes: As explanatory variables are included indicator variables for age categories, being a 2nd generation immigrant, countries of origin and for parents' education.
** indicates significance at a 5%-level, whereas * indicates significance at a 10%-level.

Table A2. Marginal effects from univariate probit models of dropout on being married. Pre-reform data only. By ethnic origin.

	Turkey		Pakistan		Self-employed father		Not self-employed father	
Specification	**Males**	**Females**	**Males**	**Females**	**Males**	**Females**	**Males**	**Females**
(1) Married	0.184 **	0.178 **	0.181 **	0.111 **	0.174 **	0.102 **	0.140 **	0.146 **
	0.021	*0.017*	*0.035*	*0.024*	*0.032*	*0.021*	*0.013*	*0.009*
(2) Married								
to a marriage migrant	0.204 **	0.176 **	0.209 **	0.093 **	0.251 **	0.104 **	0.174 **	0.160 **
	0.022	*0.018*	*0.039*	*0.026*	*0.040*	*0.031*	*0.016*	*0.012*
to an immigrant	0.109 **	0.187 **	0.057	0.168 **	0.053	0.107 **	0.081 **	0.143 **
	0.037	*0.027*	*0.070*	*0.048*	*0.046*	*0.027*	*0.019*	*0.013*
Number of observations	3378	3326	1596	1446	2759	2565	17830	16287

Notes: As explanatory variables are included indicator variables for age categories, being a 2nd generation immigrant and parents' education.
** indicates significance at a 5%-level and * indicates significance at at 10%-level.

Table A3. Results from estimation of a recursive probit model.

	Males				Females			
	Dep.var: Drop out		Dep.var: Married		Dep.var: Drop out		Dep.var: Married	
Variable	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.
Married	0.528 **	*0.107*			0.078	*0.108*		
Before 2002			0.504 **	*0.027*			0.501 **	*0.022*
Age 19	0.146 **	*0.032*	0.521 **	*0.077*	0.085 **	*0.035*	0.412 **	*0.045*
Age 20	0.290 **	*0.032*	0.847 **	*0.074*	0.216 **	*0.039*	0.789 **	*0.043*
Age 21	0.320 **	*0.034*	1.209 **	*0.072*	0.274 **	*0.045*	1.118 **	*0.043*
Age 22	0.387 **	*0.037*	1.488 **	*0.071*	0.291 **	*0.051*	1.342 **	*0.044*
Age 23	0.392 **	*0.041*	1.727 **	*0.071*	0.271 **	*0.058*	1.555 **	*0.045*
Age 24	0.372 **	*0.046*	1.953 **	*0.072*	0.301 **	*0.064*	1.726 **	*0.046*
2nd generation	-0.110 **	*0.022*	-0.261 **	*0.031*	-0.131 **	*0.024*	-0.160 **	*0.025*
Pakistan	-0.130 **	*0.032*	-0.515 **	*0.040*	-0.390 **	*0.039*	-0.635 **	*0.034*
Ex-Yugoslavia	-0.018	*0.041*	-0.784 **	*0.057*	0.129 **	*0.045*	-0.635 **	*0.044*
Lebanon	0.006	*0.041*	-0.652 **	*0.059*	0.432 **	*0.042*	-0.070	*0.044*
Vietnam	-0.714 **	*0.056*	-1.502 **	*0.093*	-0.618 **	*0.073*	-1.484 **	*0.079*
Residual countries	-0.208 **	*0.033*	-1.347 **	*0.046*	-0.295 **	*0.040*	-0.887 **	*0.031*
Father completed primary school	0.080 **	*0.032*	-0.017	*0.046*	0.000	*0.035*	-0.005	*0.037*
Father completed post-compulsory education	-0.037	*0.033*	-0.039	*0.049*	-0.084 **	*0.036*	-0.076 **	*0.038*
Father education missing	0.178 **	*0.040*	-0.147 **	*0.064*	0.179 **	*0.044*	-0.158 **	*0.050*
Mother completed primary school	-0.117 **	*0.023*	-0.013	*0.032*	-0.171 **	*0.026*	-0.078 **	*0.026*
Mother completed post-compulsory education	-0.188 **	*0.026*	-0.060	*0.040*	-0.155 **	*0.030*	-0.251 **	*0.032*
Mother education missing	0.050	*0.051*	0.014	*0.075*	0.117 *	*0.060*	-0.177 **	*0.070*
Constant	-0.720 **	*0.044*	-2.017 **	*0.081*	-0.740 **	*0.053*	-1.273 **	*0.053*
Year and cohort dummies included	no		no		no		no	
rho	-0.077	*0.061*			0.252 **	*0.066*		
ATE	0.182 **	*0.040*			0.021	*0.030*		
ATET	0.196 **	*0.038*			0.025	*0.035*		
Number of observations	23259		23259		21143		21143	

Note: ** indicates significance at a 5%-level, whereas * indicates significance at a 10%-level.
Standard errors on ATE and ATET are calculated using the delta method.

Table A4. Results from estimation of a recursive probit model. By gender for Turkey.

	Males				Females			
	Dep.var: Dropout		Dep.var: Married		Dep.var: Dropout		Dep.var: Married	
Variable	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.
Married	0.408 **	*0.171*			0.492 **	*0.197*		
Before 2002			0.570 **	*0.037*			0.484 **	*0.033*
Age 19	0.208 **	*0.056*	0.526 **	*0.098*	0.023	*0.062*	0.490 **	*0.066*
Age 20	0.367 **	*0.057*	0.847 **	*0.094*	0.100	*0.075*	0.924 **	*0.064*
Age 21	0.366 **	*0.065*	1.261 **	*0.091*	0.109	*0.098*	1.343 **	*0.064*
Age 22	0.474 **	*0.075*	1.576 **	*0.090*	0.064	*0.110*	1.555 **	*0.065*
Age 23	0.474 **	*0.087*	1.851 **	*0.091*	0.031	*0.121*	1.736 **	*0.066*
Age 24	0.419 **	*0.100*	2.112 **	*0.092*	0.142	*0.131*	1.886 **	*0.070*
2nd generation	-0.153 **	*0.036*	-0.164 **	*0.041*	-0.106 **	*0.039*	-0.146 **	*0.037*
Father completed primary school	0.056	*0.050*	-0.104 *	*0.059*	-0.169 **	*0.053*	-0.045	*0.054*
Father completed post-compulsory education	-0.097	*0.059*	-0.163 **	*0.071*	-0.178 **	*0.062*	-0.163 **	*0.063*
Father education missing	0.280 **	*0.077*	-0.175 *	*0.092*	-0.067	*0.085*	-0.206 **	*0.086*
Mother completed primary school	-0.117 **	*0.034*	-0.035	*0.040*	-0.162 **	*0.036*	-0.077 **	*0.035*
Mother completed post-compulsory education	-0.030	*0.048*	-0.055	*0.058*	-0.156 **	*0.059*	-0.303 **	*0.055*
Mother education missing	0.171 *	*0.094*	-0.016	*0.108*	-0.062	*0.111*	-0.255 **	*0.111*
Constant	-0.726 **	*0.068*	-2.107 **	*0.104*	-0.661 **	*0.077*	-1.372 **	*0.078*
rho	0.011	*0.100*			0.002	*0.120*		
ATE	0.149 **	*0.065*			0.154 **	*0.064*		
ATET	0.156 **	*0.064*			0.157 **	*0.063*		
Number of observations	7573		7573		7439		7439	

Note: ** indicates significance at a 5%-level, whereas * indicates significance at a 10%-level.
Standard errors on ATE and ATET are calculated using the delta method.

Table A5. Results from estimation of a recursive probit model. By gender for Pakistan.

	Males				Females			
	Dep.var: Dropout		Dep.var: Married		Dep.var: Dropout		Dep.var: Married	
Variable	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.	Coeff.	Std. err.
Married	0.907 **	*0.388*			-0.234	*0.271*		
Before 2002			0.411 **	*0.065*			0.549 **	*0.059*
Age 19	0.108	*0.096*	0.612 **	*0.241*	0.015	*0.115*	0.507 **	*0.170*
Age 20	0.275 **	*0.095*	1.030 **	*0.229*	0.224 *	*0.121*	0.914 **	*0.161*
Age 21	0.329 **	*0.099*	1.330 **	*0.226*	0.283 **	*0.135*	1.284 **	*0.156*
Age 22	0.345 **	*0.102*	1.479 **	*0.224*	0.273 *	*0.148*	1.448 **	*0.156*
Age 23	0.329 **	*0.110*	1.680 **	*0.221*	0.344 **	*0.172*	1.802 **	*0.155*
Age 24	0.230 *	*0.132*	1.990 **	*0.221*	0.397 **	*0.193*	2.070 **	*0.154*
2nd generation	-0.159 **	*0.072*	-0.531 **	*0.072*	-0.138 *	*0.072*	0.096	*0.073*
Father completed primary school	-0.088	*0.111*	0.403 **	*0.165*	0.265 *	*0.136*	-0.059	*0.123*
Father completed post-compulsory education	-0.152	*0.108*	0.370 **	*0.161*	0.114	*0.136*	-0.095	*0.121*
Father education missing	0.118	*0.127*	0.261	*0.190*	0.371 **	*0.162*	0.035	*0.154*
Mother completed primary school	-0.178 **	*0.068*	0.036	*0.089*	-0.184 **	*0.084*	-0.049	*0.084*
Mother completed post-compulsory education	-0.225 **	*0.077*	-0.067	*0.100*	-0.026	*0.092*	0.012	*0.094*
Mother education missing	-0.292 *	*0.152*	0.106	*0.181*	0.494 **	*0.151*	0.352 **	*0.164*
Constant	-0.633 **	*0.134*	-2.742 **	*0.276*	-1.303 **	*0.158*	-2.418 **	*0.193*
rho	-0.349	*0.243*			0.433 **	*0.187*		
ATE	0.327 **	*0.149*			-0.044	*0.049*		
ATET	0.309 **	*0.128*			-0.062	*0.076*		
Number of observations	3151		3151		2978		2978	

Note: ** indicates significance at a 5%-level, whereas * indicates significance at a 10%-level.
Standard errors on ATE and ATET are calculated using the delta method.

Figure A1. Proportion of Turks married around the reform year conditional on age, by gender.

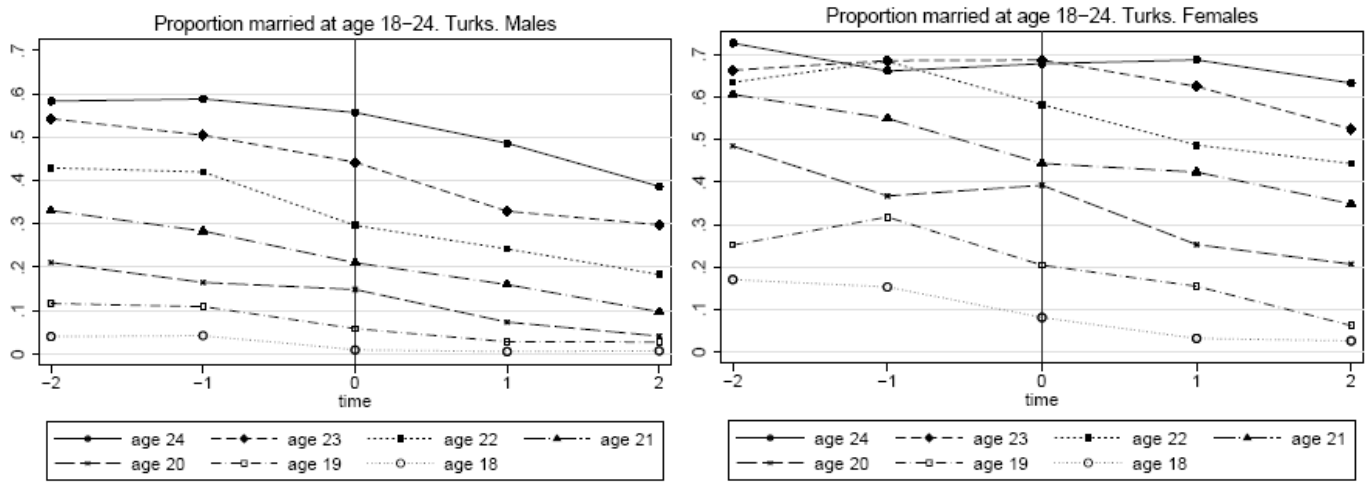

Figure A2. Proportion of Pakistanis married around the reform year conditional on age, by gender.

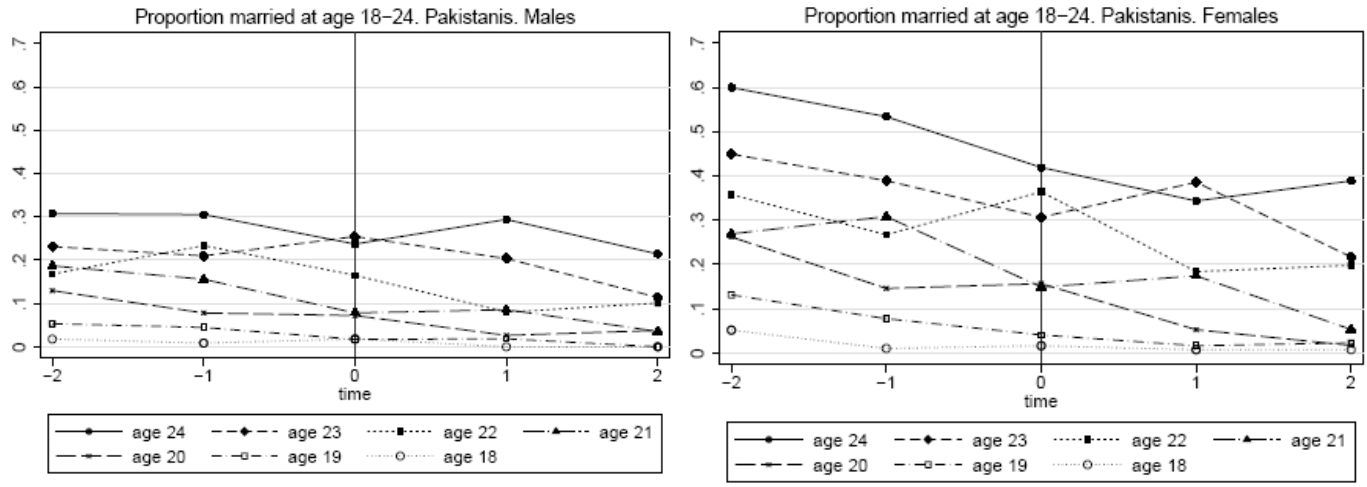

Figure A3. Dropout rates for Turks around the reform year conditional on age, by gender.

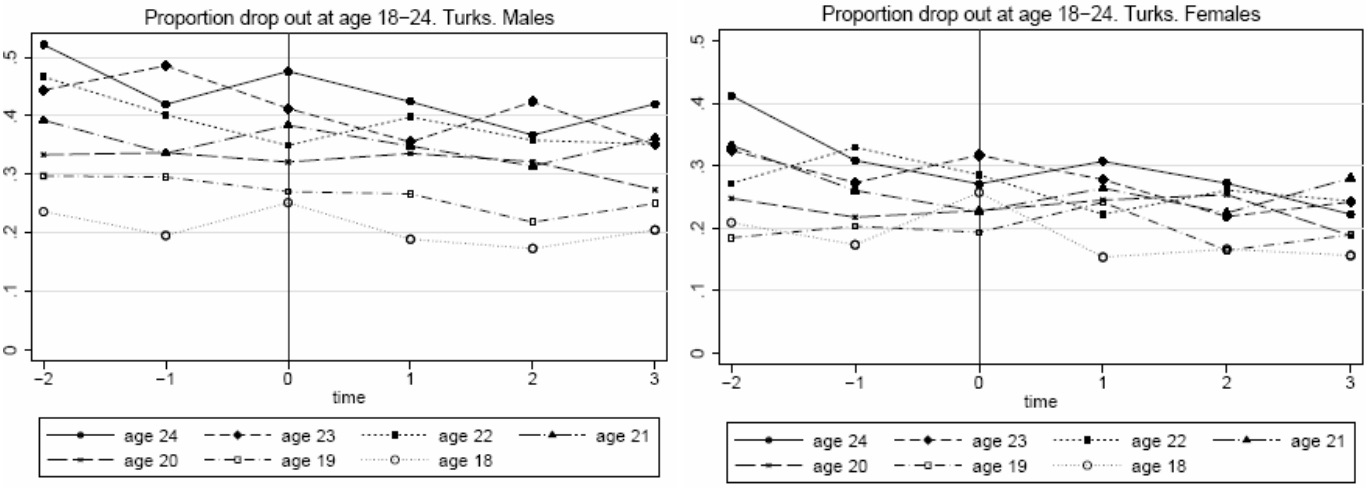

Figure A4. Dropout rates for Pakistanis around the reform year conditional on age, by gender.