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IZA DP No. 14013

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ABSTRACT

The Effect of Education on Geographic Mobility: Incidence, Timing, and Type of Migration[1]

We take advantage of a major compulsory school reform in Turkey to provide novel evidence on the causal effect of education on both the incidence and timing of internal migration. In addition, for the first time in literature, we provide causal effects of education on migration by reason for migration. We find that while education substantially increases the incidence of migration among men, there is no evidence of an effect among women. Women, however, become more likely to migrate at earlier ages and their migration reasons change. Revealing the empowering role of education, women become more likely to move for human capital investments and for employment purposes and less likely to be tied-movers.

JEL Classification: J61, I2

Keywords: education, internal migration, incidence and timing of migration, reason for migration, 2SLS, regression discontinuity design

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1. Introduction

A well-established finding in population research is that more educated individuals have a higher propensity to migrate within a country (Greenwood, 1997, chp 12). This positive correlation, however, may not be pointing to a causal relationship due to unobservable individual characteristics that shape both education and migration decisions (Sjaastad, 1962). Only a few studies exist in the literature that aim to identify the causal effect of education on migration propensity. In addition to providing new evidence on the causal effect of education on migration propensity, we extend this sparse literature by examining the effect of education on the timing of migration and distinguishing across different reasons for migration. For this purpose, we use the 1997 compulsory schooling (CS) reform in Turkey to estimate the causal effect of education on geographic mobility separately for men and women. This reform increased compulsory schooling from 5 to 8 years and made a large impact on schooling outcomes, increasing middle school completion rate by around 17 percentage points among men and 21 percentage points among women.

Hicks (1932) proposed that differences in net economic advantages, mainly differences in wages, across geographic regions are the main causes of migration (see also Sjaastad, 1962). This hypothesis interprets migration as a form of human capital investment and forms the basis of most analysis of migration. In this framework, education may change the prospects of migration by changing the return portfolio across locations or by affecting the costs of moving. The current literature studying the effect of education on migration interprets its findings under the above hypothesis that internal migration arises due to geographic differences in labor market opportunities.

Given the prevailing view of migration as a form of investment, it is surprising that the causal impact of education on the timing of this investment—whether education changes the timing of migration—has received little attention in the literature. This study differs from existing studies by estimating the effect of education on both the propensity (or level) of migration and the timing of migration. Using a sample of women for whom we have information on the complete history of migration, we estimate the effect of education on migration at different ages. This analysis focuses on women between the ages of 17-24, which is the part of the life-cycle during which migration incidence is the highest.

The current literature does not distinguish between different reasons for migration and interprets migration patterns observed in the data as mainly driven by geographic differences in labor market prospects. Individuals, however, may migrate due to various reasons. In fact, when we consider inter-county migration in the US over 2010-2011 using the CPS data, we observe that only 38% of men and 34% of women state employment-related reasons for moving.[2] Other reasons for mobility include a change in marital status, the desire to own a home, moving to a better neighborhood or a better house, attending or leaving college, climate, and health reasons. Migration for employment related reasons is similarly low in Europe (38%) and Turkey (31.7%).[3]

These figures indicate that interpreting the impact of education on migration only through the lens of employment can be misleading.[4] Moreover, the effect of increased education on migration propensity may differ across moves motivated by different reasons, increasing the prospects of some while decreasing others. For example, an education-induced increase in the earnings potential of females may reduce migration among females as tied-movers, as their labor market prospects become a more decisive factor in family migration decisions. [5] While reducing the prospects of migration as tied-movers, education may increase geographic mobility among women for another purpose, such as migration for human capital acquisition. Compared to a high school dropout, a female high school graduate may be more likely to relocate in order to attend university in another region. Due to such potential differences, studying the causal effect of education on different migration types can provide new insights about the role that education plays on mobility decisions. This study extends existing studies by distinguishing among different reasons for migration and estimates the causal effect of education by reason for migration for the first time in the literature. We do this using a sample of women who report various reasons for migration: education, employment, marriage, or as a tied-mover (either with parents or the spouse).

[2] For high school dropouts this rate is 31.4% while it is 41.8% for university degree holders. Among workers aged 30 to 44, 41.3% give employment related reasons for migration.

[3] In the Eurobarometer survey carried out in 2005 (EB 64.1), which focuses on intra-European migration spanning the EU25, 38% of movers report employment-related reasons for moving (Vandenbrande, 2006). The 2011 Turkish Census shows that among those that move within or across provinces over the 2010-2011 period, 31.7% of high school dropouts and 51.9% of university graduates report moving for employment reasons.

[4] Machin et al. (2012) consider the possibility of education affecting mobility through the timing of first child and household formation but find no effects.

[5] See e.g. Acemoğlu and Angrist, 2000, Spohr, 2003, Aydemir and Kirdar, 2017, Torun, 2018

The context of this study also differs from existing studies that focus on developed countries. This paper's focus on a developing country context is interesting because both the level and timing of internal migration may differ between developed and developing countries. For example, compared to developed countries, the composition of employment—which may affect the incentives for migration—is different in developing countries. More people work in agriculture and fewer people work in paid employment. In addition, structural transformations such as a sectoral shift from agriculture to industry to services occurring in developing countries induce migration patterns that may differ from those in developed countries. Important differences also exist in demographic characteristics that interact with mobility, such as marriage rates, assortative mating patterns, age at marriage, and fertility patterns (Anukriti et al., 2017). However, no evidence exists so far about the causal effect of education on migration in developing country contexts and our paper provides the first evidence on this issue. Where data are available, our analysis also explores gender differences in the education and migration relationship, reporting results separately for men and women. [6]

The compulsory schooling (CS) reform in the Turkish context that we exploit for identification is particularly attractive from a methodological perspective for a number of reasons. First, as we discuss below, the major motivation for the reform was political and the timing was independent of the macroeconomic context that may affect migration incentives. Second, this reform differs from the compulsory school reforms in other studies because it binds a cross-section of the population, unlike the studies in developed country settings where the affected population comes from the lower end of the ability distribution. The enrollment rate in secondary school in the 1996-97 school year, the year before the CS law changed, indicates that around 47% of the school aged children did not continue school beyond grade 5. Hence, the reform affected a very large fraction of the population in our context, and the resulting local average treatment estimate (LATE) is closer to the average treatment effect

[6] Most of the previous studies are conducted either for men or all individuals irrespective of gender. Increased education may, however, have different effects on the mobility of men and women. This may arise due to differences in the spatial distribution of the markets in which they search for jobs, which may be due to the high degree of occupational and industry sorting by gender, or differences in the prevalence of various types of migration. This calls for an analysis that distinguishes between the effects of education on migration by gender. For these reasons, in this study, we provide estimates of the effects of education on migration propensity for men and women separately.

(ATE) compared to those in previous studies.[7] Third, the extension in schooling was high, increasing CS by three years. The facts that a large fraction of the school-age population is bound by the policy and that the policy increased schooling by three years provide a very strong first-stage in our estimation. Fourth, since most children start school at age 6 in Turkey, individuals who are primarily affected by the reform are between ages 12 and 14. This age range aids our identification because, given that little migration takes place over this age range, the changes in migration behavior in our context are unlikely to be due to an incapacitation effect that prevents mobility of individuals by keeping them at school. Finally, reforms of this type have been carried out in European and North American contexts in the past—including those affecting similar grade levels, and many lower income countries where compulsory schooling is around 5 years may still carry out such reforms.[8]

In our analysis, we use the yearly Turkish Household Labor Force Surveys (THLFS) from 2009 to 2017 and the 2013 Demographic and Health Survey of Turkey (TDHS), both of which are nationally representative. The TDHS is an especially rich dataset for the purpose of analyzing migration outcomes because it includes the complete migration history after age 12—including the reason for migration for the female sample. In terms of estimation, we use both instrumental-variables difference-in-difference estimation and fuzzy regression discontinuity design, where we instrument middle school completion by the reform dummy.

Our results show that there is a strong effect of education on ever-migrating by mid 20s for men but not for women. The completion of middle school (an additional three years of education) increases the probability of migration by close to 50 percent for 23- to 28-year-old male individuals. This level analysis, however, masks the timing effects for women. Women become more likely to migrate at earlier ages and for different purposes. In particular, migration of women for education purposes increases remarkably until the early 20s. There is some suggestive evidence that migration for employment purposes also rises. At the same

[7] Machin et al. (2012), who also examine the effect of education on migration, report that as a result of the reform that they use an instrumental variable, the bottom of the educational distribution shifted upwards by two years in Norway, and this affected roughly 10–15% of people.

[8] In addition to the CS reforms discussed in the text for Norway and the US, similar reforms were implemented, for instance, by Germany (1949-1969) and Finland (1972-1977) among others (Murtin and Viarengo, 2011). Similar extensions could be adapted by other developing countries. The duration of compulsory schooling in 2014 was 5 years in Bangladesh, Laos, Madagascar, and Myanmar; 6 years in Angola, UAE, Cameroon, Guinea, Haiti, Iraq, Jamaica, Malaysia, and Congo.

time, women become less likely to migrate as tied-movers (i.e. with parents or with spouse). These results indicate that an increase in educational attainment changes both the timing of migration and the distribution of migration reasons among women. Hence, education empowers women not only through an increased earnings potential but also by enhancing their migration for further human capital acquisition and for employment purposes. Women become less likely to move as secondary migrants and start accumulating migration capital at an earlier period in their life-cycle.

In the next section, we discuss the relevant literature, followed by a discussion of the conceptual framework in section 3. Section 4 discusses the education system and the CS reform in Turkey. Section 5 discusses the data used in the analysis while Section 6 presents the identification method and estimation. Results are given in Section 7. Section 8 concludes.

2. Relevant Literature

Our paper is related to a small number of studies that use the institutional characteristics of education systems that create exogenous variation in education levels to estimate the effects of education on internal mobility. Machin et al. (2012) and McHenry (2013) study changes in education induced by compulsory schooling reforms within the Norwegian and the US contexts, respectively. The samples in these studies refer to individuals who are in their mid- or late-careers: 28- to 55-year-oldsin the Norwegian context, 30- to 64-year-oldsin the US Census data, and 32-year-olds in the US PSID data.[9], [10] While Machin et al. (2012) find that more schooling has a large and positive effect on migration, McHenry (2013) finds that additional schooling reduces geographic mobility. Machin et al. (2012) also show that the probability to migrate is affected by education more strongly in the youngest (28-36 year olds) and oldest (46-54 year olds) age groups. Weiss (2015), on the other hand, uses compulsory schooling reforms in eight European countries and finds a positive impact of

[9] The compulsory schooling increased from 7 to 9 years in Norway and this change occurred at different times across municipalities over the period of 1961 to 1972. Using administrative data for 1986-2002, Machin et al. (2012) study the effect of education on the annual propensity to move to another county, the total number of moves over the 17-year period between 1986 and 2002, and the probability of moving to an urban area.

[10] McHenry (2013) uses changes in state compulsory schooling laws in the US over much of the 20th century that affected grades 6 to 9. The measures of migration include an indicator for living in a state other than one's birth state, and an indicator for living in a state other than one's state of residence five years ago, indicators for living at a different state (commuting zone) at age 32 that is different from that where the respondent grew up, and indicators for living in different states (commuting zones) at ages 27 and 32.

education on within country migration between the ages of 15 and 50.[11] Some papers estimate the effect of increases in schooling beyond secondary school (e.g., Malamud and Wozniak (2012) in the US context, Bockerman and Haapanen (2013) and Haapanen and Bockerman (2017) in the context of Finland) and report positive effects of education on migration. [12]

Existing literature mainly focuses on the effect of education on migration propensity. The effect of education on the timing of migration and how this effect varies over the life-cycle have received little attention.[13] With the exception of Bockerman and Haapanen (2013), the above studies report estimates of education on migration when individuals are at least in their late twenties. This precludes analysis at earlier ages when migration propensity is much higher. Given the investment nature of migration and that timing plays a crucial role in potential returns in an investment framework, it is of significant interest to estimate the effect of education on the timing of migration. Using the information available in our data on the migration history of females, we trace how the effect of education on migration changes over the part of the life-cycle when migration is most intense, i.e. between ages 15 and 24. This analysis shows that while education does not change the propensity to migrate by age 24, it changes the age that migration takes place, highlighting that an analysis focusing only on migration propensity may mask important timing effects of education.

Existing studies also do not distinguish between migration types motivated by different reasons (employment, education, marriage, or as a tied-mover either with parents or with the

[11] Weiss (2015) uses a sample of 50+ year olds who report retrospective information on residences they lived for more than six months. Migration is defined as mobility across NUTS regions. Similar to Weiss (2015), Fenoll and Kuehn (2017) use compulsory schooling laws in Europe to estimate the impact of schooling on migration across European countries. They find that increases in length of compulsory schooling reduce the propensity to migrate.

[12] Bockerman and Haapanen (2013) study the effects of a polytechnic reform that took place in Finland in the 1990s.This reform, which transformed former vocational colleges into polytechnics offering a bachelor's degree, provides exogenous variation in the regional supply of higher education. The migration measures include migration across NUTS-3 regions as well as residence outside the matriculation region (i.e. the region in which an individual graduated from high school). Malamud and Wozniak (2012) uses variation in college attainment caused by draft-avoidance behavior during the Vietnam War. Focusing on individuals in their late twenties or early thirties, they find that additional years of college significantly increase the likelihood that men reside outside their birth states.

[13] There is some evidence of age-specific effects of education (Bockerman and Haapanen (2013) and Machin et al. (2012)). In particular, Bockerman and Haapanen (2013) find that an effect of education on migration exists over the short run, although this effect dissipates several years after the completion of education.

spouse). Given the prevalence of various reasons for migration other than employment related reasons, we estimate the effects by reason of migration. Our study is the first in the literature to report the causal estimates of education by reason of migration and shows that the effects vary significantly across moves motivated by different reasons.

Few studies in the literature report estimates by gender. Machin et al. (2012) and Haapanen and Bockerman (2017) report separate estimates for males and females while Malamud and Wozniak (2012) focus only on men. We study the effects on propensity of migration separately for males and females. Due to data availability, we study the timing effects and effects by reason of migration only for females. Our analysis focusing on women is interesting because the relationship between education and migration among females is not much studied and reasons for migration other than employment are more common among women. The results in this paper, especially those by reason of migration, also contribute to the growing literature on the non-pecuniary benefits of education (see e.g. Oreopoulos & Salvanes, 2011).

3. Conceptual Framework

Human capital models consider internal migration as an investment where individuals compare the present value of earnings in alternative locations and decide to migrate if the expected gain from migration net of migration costs, including the psychic costs of leaving the home location, is positive.[14] In this framework, the motive for migration is employment related and geographic mobility of labor works as a mechanism that improves the allocation of workers across jobs resulting in efficiency gains.

Despite the general view in the empirical analysis that treats internal migration as job-related, migration may be motivated by a variety of reasons. Other reasons for moving include a change in marital status, a desire to move to a better neighborhood, a desire to own a home or a better house, attending or leaving college, climate, and health. Several studies highlight the prevalence of these non-job-related reasons of migration (e.g. McHenry (2013), Korpi and Clark (2017), Niedomysl (2011)). For example, Niedomysl (2011) discusses that about one

[14] Extending this structure to a dynamic model with multiple destinations and a sequence of migration decisions, Kennan and Walker (2011) examine the migration decision of individuals across states in the US. In a similar dynamic model, Gemici (2016) examines the joint migration decisions of a couple. In this case, the couple maximizes the expected present value of the sum of their consumption levels in a setting where divorce is possible.

third of respondents in Sweden list employment-related reasons as their main motive for moving. The significance of these other reasons for migration may be especially high among females. Within the Turkish context, by age 24, 27% of ever-migrated women did so for marriage reason. The corresponding fractions for migration due to educational purposes, employment reasons, and as tied-movers were 6.6%, 3.7%, and 10.6%, respectively. Given that a large fraction of migrants in both developed and developing countries move for non-job-related reasons, it is of interest to understand how education affects migration types motivated by different reasons.

Increasing education levels may induce individuals to search for employment opportunities outside their local labor markets, as increased education opens up new opportunities in the national labor market (McCormick 1997). Increasing education may also decrease migration costs, especially the psychic costs of migration through improved knowledge of the destinations and more familiarity with diversity. More educated may also find it easier to finance the costs of migration.[15] These channels suggest that higher education may cause higher employment related migration. McHenry (2013), however, argues that additional education at low levels may increase the strength of local job network ties and thereby provide employment stability in the local area. This may result in an increase in the opportunity cost of migration and a reduction in geographic mobility. Thus, at low levels of education, the effect of increased education on mobility is theoretically ambiguous.

Education may also increase the incentives to migrate for non-pecuniary aspects of locations. For example, Glaeser et al. (2001) argue that big cities are increasingly valued as high amenity locales from housing to cultural life. More educated individuals may have higher demand for these types of amenities resulting in higher incidence of migration.

In addition to the consumption opportunities, geographic locations also offer different educational opportunities, either in terms of access to education or its quality. Investments in education increases the stock of future skills which, as a result of dynamic complementarities, increases the return to future investments. This leads to further investments in education.[16] If

[15] In fact, Kennan and Walker (2015) estimate that migration costs for college-educated individuals are much lower in the US setting.

[16] There may be other reasons why increased education may lead to further investments in education. When education increases through an exogenous change, such as an increase in compulsory schooling duration, this changes the menu of options for individuals and further investments in schooling may become optimal. Another

increased education triggers further investments in education, this may in turn increase migration propensity for educational purposes to access educational opportunities offered in other locations.

Increased education may also affect migration propensity and the reason for migration through its influence on family decision making. For example, women's earnings prospects become a more important factor in family migration decisions as their education increases. This, in turn may affect the propensity of women to migrate as a tied-mover. Increased education may postpone marriage age as individuals either enter employment or pursue further human capital investments. In fact, previous studies show that the CS in Turkey increased employment (Aydemir and Kırdar [2017]) and reduced teenage marriage for women (Kırdar et al., 2018). Therefore, individuals may become less likely to move with their parents due to their ties to employers or educational institutions.

While increased education tends to open up new opportunities in the labor market, the extent of such nationwide opportunities may be different across occupations and skill groups. If males and females become more likely to specialize in different occupations and skills as a result of increased education, the impact of increased education on migration propensity for employment related reasons may differ by gender. The multiplicity of channels through which education may affect migration also implies that the effects of increased education on migration propensity may be different for males and females. The effect of education on propensity of migration for different purposes may be different, increasing the prospects of some types of migration but decreasing others. Given that prevalence of various reasons for migration is different between males and females, increased education may affect their migration propensity differently.

4. Education System in Turkey and the 1997 Education Reform

Prior to the implementation of the 1997 education reform in Turkey, only primary school (grades 1-5) was compulsory. The new policy combined primary and secondary schools (grades 6-8) and made the attendance of grades 1-8 mandatory. The reform was implemented

reason is that as the compulsory schooling duration changes the age at which individuals may drop out of school, there may be an associated change in individuals' preferences and their valuation of human capital investments. Moreover, further education may reveal new information about individual's abilities and result in an update in the expected returns from human capital investments. As a result, some individuals who would otherwise drop out of school may decide to continue schooling.

nationwide in the 1997-98 school year, and it covered students who were in grade 4 or a lower grade in the 1996-97 school year. This implies that students who started school in September 1993 or afterwards were bound by the policy. Since most children in Turkey start school at the age of six, children born in or after January 1987 are affected by the policy.[17]

A nontrivial number of students, however, start school earlier or later than the usual school start-age. This means that students who were born one year later or earlier than this date may also be affected by the new policy. Put differently, students born in 1986 but start school later than normal age would be bound by the policy while students born in 1987 but start school before the normal age would be exempt from it. Therefore, fuzziness in the treatment status exists for these two birth cohorts immediately around the cutoff date.

Before the legislation of the reform, extension of compulsory schooling beyond grade five had been sporadically discussed since the early 1960s. The actual timing of the reform was closely related to the political developments of the time.[18] Importantly, the timing was independent of the macroeconomic context and, therefore, did not coincide with other factors that would have a bearing on migration decisions.

The CS reform bound a large fraction of students due to high dropout rates prior to its implementation. National education statistics show that in the 1996–1997 school year, the year before the policy change took effect, enrollment at the middle school level (grades 6 to 8) was 52.8%. Following the reform, the government implemented a number of policy tools to meet the needs of an increased student population: hiring of new teachers, expanding the number of classes in existing schools, bussing an additional half a million (mostly rural)

[17] The cutoff month is January because according to the Turkish education policy, students start school in September of the calendar year that they complete age six. Hence, a child who completes age 6 in December starts school in September before she/he completes age six.

[18] Prior to the reform, students could choose between different education streams at the middle school level starting in grade 6: general schools and two types of vocational schools, including *Imam Hatip* schools and technical schools. Around 10 percent of middle school graduates in 1996-97 were enrolled in *Imam-Hatip* schools and 1.4 percent were in technical schools. Vocational and technical schools offered some specialized courses depending on school type on the top of the curriculum offered in the general stream. Students from both vocational and non-vocational schools could compete for university seats on equal footing, and many of the vocational and technical school graduates went on to occupations similar to those of graduates from the general stream. The secular coalition government that took power in 1997 saw this reform as a tool to curtail the demand for *Imam Hatip* Schools. Vocational streams at the middle school level were closed down and all students were offered a unified general education in grades 6 through 8.

children to nearby schools, and constructing almost 600 boarding schools in more remote areas. The net enrollment rate at the compulsory schooling stage (grades 1 to 8) reached 95.3% by the 2000–01 school year, four years after the law was enacted. One may be concerned that the large extension in the duration of compulsory schooling may be accompanied by a decline in the quality of education. A previous study shows that despite the large increase in the student population, no evidence of a deterioration exists in the quality of schooling in terms of student-to-class ratio, student-to-teacher ratio, and student test scores (Aydemir and Kırdar, 2017).

Appendix Figure A1 presents the number of students in basic education (grades 1 to 8) between the 1991-92 and 2002-03 school years. There was an upward trend in the number of students in urban areas but a downward trend in rural areas prior to the implementation of the reform in 1997-98 school year. The downward trend in rural areas is a reflection of migration from rural to urban areas. The upward trend in urban regions reflects the joint effects of increasing enrollment rates and increasing student population due to rural to urban migration. Although the trends in student numbers prior to the policy implementation were different in urban and rural areas, both figures show sharp increases with the implementation of the reform.

5. Data

We use two different datasets in our analysis: the Turkish Household Labor Force Survey (THLFS), conducted by the Turkish Statistical Institute, and the Turkish Demographic and Health Survey (TDHS), conducted by the Institute of Population Studies of Hacettepe University. The THLFS is a nationally representative sample of individuals in Turkey, which is conducted annually and includes information on educational attainment and migration outcomes, in addition to labor market outcomes. We use the 2009 to 2017 cross-sections because these surveys elicit the age at last interprovincial migration from respondents.[19] The THLFS offers a large sample size, however it does not provide information on the reason for migration or on previous migration experience if an individual migrated more than once.

In the THLFS data, the question on the timing of last migration allows us to generate an ever-migrated status variable at the survey year. The ever-migrated status variable takes the value

[19] The survey asks the following questions that allow us to derive migrant status questions: "Have you been living in this province since birth?" and "Since which year have you been living in this province?"

of one if an individual has migrated since age 15 by the survey year and the value of zero otherwise. We focus on migration after age 15 because it comes after the completion of middle school, mandated by the new CS reform. We can, thus, exclude from our analysis child migrants who might have moved with their families at earlier ages.[20]

The second dataset used in our analysis is the 2013 Turkish Demographic and Health Survey (TDHS). The target population in this survey is women at the reproductive age (15-49), including both ever-married and never-married women. Although providing a smaller sample size than the THLFS, the TDHS has a number of important features that allow a richer analysis. First, it includes the complete migration history after age 12, along with information on the reason for each migration. In other words, we know whether or not an individual migrates at each age and if so for what reason. We group the reasons for migration as follows: (i) marriage, (ii) education, (iii) employment, (iv) with parents, (v) with spouse (after marriage). Note that the reasons are not mutually exclusive; an individual can migrate at age 18 for education, then at age 22 for employment. While the TDHS collects some information on spouses, the information on migration history is available only for women. Therefore, the analysis of the level and timing of migration by reason can be carried out only for women.

The second advantage of the TDHS is that it provides geographic identifiers of the various regions an individual has ever resided since age 12, which allows us to distinguish between within-province and between-provinces migration, as long as the move within provinces includes a change in the type of location of residence (village, district center, and province center). In other words, we identify a within province migration if it is from a village center to a district center, but fail to identify if it is from one district center to another district center (that is not a province center). Furthermore, since we know about the type of location of residence before age 15 in terms of its size and region, we can examine heterogeneity in migration patterns by these initial conditions. Third, the 2013 TDHS survey includes information on month as well as year of birth for all respondents,[21] which is important in our identification strategy.

[20] Note that some of the existing work uses the definition of migrant as someone whose place of birth is different from the current place of residence. This definition, dictated by data availability, necessarily pools child migrants with those migrating at later ages.

[21] The previous 2008 TDHS survey includes information on month and year of birth only for ever-married women.

Using the information on complete migration history, we put the THDS data into person-age format.[22] As in the THLFS data, we define an individual as a migrant if this individual has ever migrated since age 15 (after completing middle school). Given the full history of migration, we can determine ever-migrated status for every age after age 15. This is different from the THLFS data, where we know the ever-migrated status only at the age of the individual in the survey year.

5.1 Descriptive Statistics

Panel (A) of Table 1 presents the mean values of the variables used in the econometric analysis of the THLFS sample. The average age of both males and females is around 26. However, females are less educated, less likely to be employed, and more likely to be married. Age at marriage is lower among females than males, which leads to higher marriage rates for females in our sample. Interestingly, females are slightly more likely to have ever migrated by the survey year than males (20% of males and 23% females). This may be a result of migration due to marriage, which is common among females but much less likely among males.

Panel (B) of Table 2 provides the mean values of the variables in the TDHS sample restricted to those born between 1977 and 1996, allowing a 20-year interval around the policy cutoff. Around 45.7% of individuals in the sample have ever-migrated status after age 15 and the average age of these women is 26.6.[23] Around 63% are middle school graduates and around 66% live in large or small cities. In the rest of this subsection, we provide a detailed description of education and migration outcomes.

5.1.1 Education Outcomes

Figures 1 to 3 display the policy effect on educational outcomes by plotting the mean value of educational outcomes over year-of-birth cohorts, using the THLFS sample. The 95% confidence intervals for the mean values are also displayed. The outcome is middle school completion in Figure 1, high school completion in Figure 2, and college completion in Figure

[22] In this person-age data, it is important to note that the birth cohort composition changes with age.

[23] Higher migration rates for women in the TDHS sample compared to the THLFS sample arise mainly due to within-province migration, which can be identified in the TDHS data but not in the THLFs data. Note also that the data come from different surveys years in the two data sets.

3.[24] Each figure contains two panels; while panel (B)s include a donut-hole—the 1986 and 1987 birth cohorts are excluded due to the fuzziness in the treatment status of these birth cohorts,[25] panel (A)s include all birth-year cohorts in the given range. Within each panel, the figure on the left refers to females while the one on the right refers to males. In each figure, the vertical line represents the introduction of the new CS reform.

In Figure 1, we observe a big jump at the cutoff for middle school completion for both men and women. These jumps are much more pronounced in panel (B), which includes the donut-hole. For example, in panel (B), the increase in middle school completion rate for women is more than 20 percentage points.[26] In panel (A), for both men and women, the middle school completion rate for the 1986 birth cohort is markedly above the fitted line on the left-hand side of the cutoff whereas the completion rate for the 1987 birth cohort is markedly below the fitted line on the right-hand side of the cutoff—indicating the importance of fuzziness in the treatment status of these two birth cohorts right around the cutoff.

While the policy did not mandate schooling beyond grade 8, both panels (A) and (B) in Figure 2 indicate a spillover effect of the policy on high school completion for both men and women. The jump at the cutoff in panel (B) is more than 5 percentage points for both men and women.[27] This spillover effect is also reported in Kırdar et al. (2016) and Torun (2018).[28]

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[24] In Figure 1, where the outcome is middle school completion, we restrict the sample to individuals aged 17 or older. Hence, the latest birth cohort in our sample is the 2000 birth cohort. Therefore, we have 14 birth cohorts on the right-hand side of the cutoff (1987-2000). Accordingly, on the left-hand side of the cutoff, we take 14 birth cohorts as well (1973-1986). A similar approach is taken in determining the birth cohorts in Figures 2 and 3, given the age restriction made for the educational outcome in each figure.

[25] The use of a donut-hole approach and the reason for it are discussed in Section 7.1, where we estimate the policy effect on educational attainment. Note that a donut-hole RDD is not needed in our core findings on the effect of education on migration outcomes, where we use a fuzzy RDD.

[26] However, for neither group grade 8 completion is universal after the cutoff. This may reflect imperfect compliance with the law, as well as late implementation of the policy in certain areas (e.g., rural areas in which bussing schemes had to be established and locations where school construction spanned over a few years following the reform date).

[27] In Figure 2, which reports the fraction completing at least high school among individuals aged 20 and higher, high school completion rates seem to fall for the 1992-1995 birth cohorts. For these birth cohorts, individuals who are university students (hence are high school graduates) and live in dormitories will not be sampled by the HLFS survey. Also, male high school graduates who are serving in the military on the survey date will not be in the sample. The seemingly declining high school graduation rate is an artifact of this sampling issue. Note that in this paper we focus on the effect of middle school completion and there is no indication of a similar dip in Figure

Figure 3 suggests a positive policy effect on college completion (including 2-year colleges) as well. While the jumps at the cutoff for both men and women are marginal in panel (A), they become much larger and more visible with the donut-hole in panel (B)—especially for women.[29] Earlier studies examining the effects of this policy did not include an analysis of the policy effect on college completion due to data limitations.[30]

Next, we examine schooling outcomes using the TDHS. An important advantage of this dataset is the availability of month and year of birth of individuals. With this information in hand, we can define cohorts that are affected by the policy much more precisely. Since month of birth information is available only for women, the graphs are given only for them. Figures A3 to A5 in the Appendix display, respectively, the fraction of women completing at least middle school, the fraction of women completing at least high school, and the fraction of women with some college education.[31] The horizontal axis denotes how far each birth cohort (as defined by month-year of birth) is from the policy implementation date.

Figures A3 and A4 indicate clear jumps for middle school and high school completion. The magnitude of the jump at the cutoff for high school completion (with the donut-hole), at around 10 percentage points, is higher than that suggested by the THLFS—although the confidence intervals are much wider due to the smaller sample size. We also observe a significant jump at the cutoff in the fraction of women with some college in Figure A5. While the 95% confidence intervals somewhat overlap in the plot without a donut-hole, they barely

1. Moreover, we present our results for various age groups that effectively restricts the analysis to birth cohorts for which sampling issues matter much less, and we show that our results are robust.

[28] Similar spillover effects of schooling reforms are reported in other contexts. For example, Meghir and Palme (2005) in Swedish context, Brunello, Fort, and Weber (2009) in a host of European countries, and Machin et al. (2012) in the Norwegian context show compulsory schooling effects that go beyond the post reform minimum school leaving age.

[29] Figure A2 in the Appendix, which replicates the analysis in Figures 1-3 when the outcome variable is completed years of schooling, shows a significant jump at the cutoff in completed years of schooling that is close to one full year for both females and males.

[30] Since one has to impose a higher age threshold to examine the policy effect on college graduation, the number of birth cohorts that are affected by the policy was very small at the time of these studies. For instance, in Kırdar et al. (2018), which uses the 2013 TDHS, only 1987 to 1989 birth cohorts are affected by the policy when the sample is restricted to individuals aged 24 or above to examine college completion.

[31] We cannot examine college completion with the 2013 THDS because as we set a minimum age limit of 24 for the analysis of college completion, very few birth cohorts that are affected by the policy remain. This is not the case with the THLFS as the surveys cover years until 2017.

overlap when a donut-hole is taken. In addition, the magnitude of the jump is more than 5 percentage points with the donut-hole.

5.1.2 Migration Outcomes

Based on the THLFS data, Figure 4 displays the fraction ever-migrated at each age, after age 15, for men and women separately.[32] Both gender groups display a similar pattern of ever migrating by age, and about one third of each group has ever migrated by age 38. At the same time, women are more likely to be migrants than men at each age. The gender gap in migration propensity widens until the mid20s and then narrows. A key feature of Figure 4 is that the increase in fraction ever-migrated is the fastest at ages 18-23 for both men and women.[33] In other words, the migration propensity peaks at early ages in Turkey. Our econometric analysis focuses on the migration behavior during these young ages when migration activity is intense.

Figure 5 presents the correlation between educational attainment and the fraction ever-migrated after age 15 for males and females, separately, using the THLFS data. Here, the sample is restricted to birth cohorts that are not affected by the CS reform, and the minimum age is taken as 24 because we compare the migration profile of college graduates with those of other education groups. For both men and women, we observe a strong positive correlation between education and migration. The migration propensity is especially high for college graduates. A significant difference also exists in the migration propensity of high school and middle school graduates relative to primary school graduates, which is more pronounced among females. In fact, our study investigates to what extent this observed positive correlation between education and migration in Figure 5 is causal.

Next, using the TDHS data, we examine the migration outcomes for women. In the TDHS data, it is also possible to distinguish between interprovincial migration (the migration type captured by the THLFS data) and intraprovincial migration. Figure 6 shows the fraction ever-

[32] For the male sample, Figure 4 omits the points for ages 20 and 21. Many males in this age range complete their military service (which lasts 6 to 18 months). Since HLFS data do not survey individuals who are in institutions (including those under military service), this results in a change in the composition of the male sample. Therefore, we do not report ever-migrated fractions for this group.

[33] Figure A6 in the Appendix shows the migration hazard rates at each age calculated from the fractions of individuals migrated in Figure 4. Note that these are not technically hazard rates, as we do not follow individuals over time here.

migrated by age (after age 15) among women; panel (A) does it for both intra- and interprovincial migration whereas panel (B) considers only interprovincial migration. In both panels, we show the fraction migrated for any reason, as well as for specific reasons (migration for marriage, education, employment, with parents, and with spouse after marriage). The level of migration is considerably higher when intraprovincial migration is included. While about 37% of women migrate interprovincially between the ages of 15 and 36, about 58% of women migrate intra- or interprovincially. However, a closer examination of Figure 6 reveals that most of the intraprovincial migration is for marriage purposes. For other purposes, the fraction migrated changes little between panels (A) and (B).[34]

The pattern and the level of interprovincial migration for any reason in panel (B) of Figure 6, are similar to those in Figure 4 based on the THLFS data.[35] Figure 6 also shows that migration for reasons other than employment is highly important. Marriage is the most frequent reason for migration at all ages from 15 to 36. Education is the second most common reason for migration between ages 17 and 24.

Given that other reasons for migration are quite significant, it is important to understand the association between education and migration by reason for migration. Figure 7 takes a first look at this issue. The top-left and top-right panels present this relationship for migration for marriage purposes and migration with spouse, respectively. These two figures show that migration for marriage or migration with spouse becomes much less likely as the education level of women increases. The bottom-left panel presents the education and migration relationship for migration for educational purposes while the bottom-right panel presents this relationship for migration for employment purposes. These two figures show that propensity of having ever migrated for educational purposes and for employment purposes increases with the education level. Thus, Figure 7 shows negative correlations between education and

[34] Figure A7 in the Appendix presents migration hazard rates by reason for migration; panel (A) does it for all migration (inter- and intraprovincial) whereas panel (B) does it only for interprovincial migration. The propensities to migrate by age and gender implied by these figures are also similar to those in Figure A6 based on the THLFS data.

[35] The only notable difference is observed at early ages, 15 to 18. In the THLFS data, the information for these ages comes when respondents are actually at these ages; on the other hand, in the TDHS data, the information for these ages mostly comes from the migration history of older individuals. Therefore, individuals who live in institutions like dormitories at these ages would not show up in the THLFS data, which does not sample these institutions; however, these individuals would show up in the TDHS data at later ages and provide information for their migration status at ages 15 to 18.

migration for marriage and migration with spouse whereas it shows a positive correlation between education and migration for education and employment purposes. Whether these observed correlations capture causal relationship running from education to migration is another question that we tackle in our econometric analysis.

Finally, using the month-year of birth cohort information in the TDHS, we examine how fraction migrated changes over birth cohorts and particularly focus on whether we observe a jump at the January 1987 cutoff. Figure 8 does this for any reason for migration, whereas Figures 9 to 11 consider migration for marriage, migration for education, and migration for employment, respectively. In all figures, the analysis is conducted conditional on age—at each age from age 17 in the top-left panel to age 22 in the bottom-right panel. (At higher ages, the number of data points on the right-hand side of the cutoff is small.) The horizontal axis denotes how far each birth cohort (as defined by month-year of birth) is from the time of policy implementation. The bandwidths are taken as 10 years on each side of the cutoff in Figures 8 to 11. Corresponding to Figures 8 to 11 are Figures A8 to A11 in the Appendix where 10-year intervals with donut-holes are taken, Figures A12 to A15 where 5-year intervals on each side of the cutoff are taken, and Figures A16 to A19 where 5-year intervals with donut-holes are taken.[36]

Figure 8 indicates a jump at the cutoff in the fraction ever-migrated for any reason by age 17. While this jump persists at higher ages, the 95% confidence intervals start overlapping and the degree of this overlap increases monotonically in age. With the donut-hole in Figure A8 in the Appendix, the jump is more pronounced at all ages; in fact, Figure A8 suggests an increase in the fraction of ever-migrated by age 20 but not later. With the 5-year bandwidths in Figure A12, the patterns are similar to those in Figure 8; however, the jumps are somewhat less pronounced. When a donut-hole is added to the 5-year bandwidths, the jumps become more visible until age 19 (Figure A16 in the Appendix).

In the fraction ever-migrated for marriage purposes, Figure 9 shows a clear jump at the cutoff at ages 17 and 18, however, after age 18, we do not observe a notable jump at the cutoff as the confidence intervals overlap substantially. Figure 10 indicates a jump with the policy in the fraction ever-migrated for education at all ages from 17 to 22. Moreover, the magnitude of

[36] Here, donut-holes are taken—as a sensitivity check—as we examine the reduced-form policy effect on migration outcomes. This will not be needed in our core analysis where we estimate the effect of schooling on migration outcomes using fuzzy RDD.

this hike increases monotonically in age. By age 22, the level of jump at the cutoff is about 5 percentage points—which holds true with the 5-year bandwidths in Figure A14 in the Appendix as well. The fraction ever-migrated for employment, given in Figure 11, exhibits an upward jump at all ages and the magnitude of the jump increases monotonically in age. However, the confidence intervals substantially overlap at all ages except for 21 and 22. When 5-year instead of 10-year intervals are taken around the cutoff, the patterns for migration for education and migration for employment are similar, whereas the jumps for marriage at ages 17 and 18 are less pronounced. Taking a donut-hole strengthens the patterns for migration for marriage, but not for migration for education or employment.

6. Identification Method and Estimation

In the estimation of the effect of education on migration, the identification challenge arises due to omitted factors like ability, motivation, willingness to invest, and parental connections that may jointly determine education and migration outcomes. To solve this endogeneity problem, we use the compulsory schooling reform in Turkey as a source of exogenous variation in schooling. The identification of the causal effect of education on migration decisions comes from the variation across birth cohorts in the exposure to the policy.

In our context, the timing of the policy is independent of schooling and migration outcomes because its implementation was triggered by political developments. Identification requires that the policy not coincide with other interventions that also influence the outcome of interest. Appendix Figure A20 checks this possibility by plotting various aggregate indicators across calendar years. The upper-left panel shows a sharp discontinuity in education's share in public expenditures around the policy cutoff date as the government increased educational spending needed for school construction, bussing, teacher hiring and such.[37] The other aggregate series that reflect aggregate economic conditions in Figure A20, such as GDP per capita, employment rates or expenditures in other areas such as health, display no discontinuity at the cutoff date. This shows that the policy did not coincide with other major interventions in the economy.

We use both regression discontinuity design (RDD) and difference-in-differences methodologies. The empirical methodology we choose depends on the dataset because of the

[37] Two other notable but substantially smaller scale educational interventions were implemented much later: the public conditional cash transfer (CCT) policy in 2003 and an NGO-driven CCT policy in 2005.

differences in the structures of the TDHS and THLFS. We prefer RDD whenever it is applicable due to its superior properties; in fact, Lee and Lemieux (2010) claim that it is potentially more credible than other quasi-experimental approaches.[38] In examining the effect of education on migration outcomes, we use RDD with the TDHS; however, this is not possible with the THLFS, so we use difference-in-differences.

In the THLFS, we know the ever-migrated status only for the age at the time of the survey. As a result, when we examine the ever-migrated status over birth cohorts for a specific age, all birth cohorts in our sample are treated by the policy for all ages below 23. For instance, when we take 20-year-olds with our 2009–2017 THLFS data, we have the 1989–1997 birth cohorts—who are all bound by the policy. Hence, we cannot use a RDD; instead, we pool all age groups in the THLFS data and estimate a treatment effect, averaged over ages, using a difference-in-difference analysis. On the other hand, we do not have such a limitation in the TDHS dataset. Since we observe the whole migration history, we can put the data in person-age format after age 15. In this format, the sample of 20-year-olds include a 20-year-old in the 2013 TDHS, who is born in 1993 and treated by the policy, as well as a 30-year-old in the 2013 THDS, who is born in 1983 and not treated by the policy. For the 30-year-old in 2013, we know her migration status when she was 20 years old from her migration history.

6.1 Difference-in-differences

To find the causal effect of education on migration using the THLFS data, we carry out the following two-stage least squares estimation:

$$s_i = \alpha_0 + \alpha_1 D_i + X^{'}\theta + u_i \qquad (1)$$

$$m_i = \beta_0 + \beta_1 s_i + X^{'}\delta + v_i \qquad (2)$$

where *m* denotes migration status, *s* denotes middle-school completion status, *D* is a dummy variable for the policy, and *X* denotes the set of covariates including dummy variables for each age and calendar year. The migration-status variable, *m,* takes the value of one if the individual has ever migrated (interprovincially) since age 15 and zero otherwise.

[38] The reason for this, Lee (2008) argues, is that there is no need to assume that the treatment variable is “as good as” randomly assigned in RDD because of invidivuals’ imprecise control over the assigment variable. Hence, RDD is much closer to randomized experiments (the “gold standard” of program evaluation methods) than other quasi-experimental methods.

The policy dummy variable, D, which we use as an instrument for middle school completion status, takes the value of one if an individual is born in or after 1987 and zero otherwise.

Table 2 illustrates our identification methodology in a matrix form where calendar years form the rows, ages form the columns, and birth years are given in the cells. Treated birth cohorts, which give the policy dummy, are highlighted. As can be seen from the table, the policy dummy varies across ages and across calendar years; hence we can identify the policy effect separately from calendar year and age effects. It is important to note that the policy dummy varies only across ages 23 to 30; therefore, the estimated policy effect (and hence the education effect) is only for these age groups. Because of this, we conduct our analysis for various age groups; we start with the 23–30 age group and expand it gradually on both sides by taking the 21–32, 18–35, and 15–38 age groups. We take these alternative samples with wider age ranges not only as a robustness check but also to increase the number of clusters in the estimation—which we address in more detail shortly.

In the difference-in-differences regression specification above, the calendar year effects do not vary by age. In other words, if there is a time trend in the outcome variable, this is assumed to be the same across all age groups. While this might be a parsimonious assumption with the sample including the 23–30 age group, it becomes a stronger assumption as we widen the age range in the sample. Therefore, we use alternative specifications in which we relax this assumption gradually. First, we interact age group dummies with a time trend; second, we interact individual age dummies with a time trend.

In the estimation of equation (2), since our instrument varies at the birth-year level, we cluster the standard errors at this level. However, due to the age-range restrictions we impose on our samples, we have relatively few clusters. For instance, with the sample including the 23–30 age group, we have 16 clusters. The number of clusters increases to 32 with the sample including the 15–38 age group. As shown in Cameron et al. (2008), when the number of clusters is small, clustering the standard errors might not be sufficient. Moreover, MacKinnon and Webb (2013) report that when the clusters are unbalanced in terms of the sample size, the effective number of clusters could be even fewer. In order to address this few clusters issue, we conduct wild-cluster bootstrap to calculate p-values (Cameron and Miller, 2015).[39]

[39] Cameron et al. (2008) and Cameron and Miller (2015) implement wild-cluster bootstrap for the few-clusters problem in the OLS setting and find that it performs much better than the standard cluster-robust inference (as well as inference based on alternative techniques) in Monte-Carlo rejection rates of the null hypothesis.

6.2 Regression Discontinuity Design

The availability of month-year of birth information in the TDHS data provides us variation in the exposure to the CS reform across different month-year of birth cohorts. Exploiting this information, we adopt an RDD to estimate the causal effect of education on migration. As discussed in the preceding sections, the possibility of early and late school-starts introduces fuzziness in exposure to the policy. Due this fuzziness, we adopt a fuzzy RDD and estimate the following 2SLS model:

$$s_i = \alpha_0 + \alpha_1 D_i + g(x_i) + X^{'}\theta + u_i \qquad (3)$$

$$m_i = \beta_0 + \beta_1 s_i + f(x_i) + X^{'}\delta + v_i \qquad (4)$$

where *m* denotes ever-migrated status since age 15 (which includes both interprovincial and intraprovincial moves), *s* denotes middle-school completion status, *x* stands for month-year of birth as the running variable (which is normalized using the time of discontinuity), and *X* denotes the set of covariates including location of residence at age 12—in the form of NUTS-2 level region and the type of the location of residence (large city, small city, village), mother tongue (Turkish, Kurdish, and other), father's and mother's literacy status, and dummies for month of birth. The policy dummy variable, *D*, which serves as the instrument for middle school completion status, takes the value of one if an individual is born in or after January 1987 and zero otherwise.

Using the data put in person-age format based on the migration history of each woman, we estimate equation (4) at each age separately. Note that the sample size changes by age because a person at a certain age in 2013 enters the sample for ages 15 to that age. Finally, in the estimation of equation (4), standard errors are clustered at the month-year of birth level. Here, we do not have the few clusters problem, unlike the case with the THLFS data, because the instrument varies at the month as well as year of birth level.

In the identification of equation (4), the critical aspect is distinguishing the jump from the trends on either side of the cutoff. We take two different approaches for this. In the first

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Davidson and McKinnon (2012) develop wild cluster bootstrap methods for IV; however, their main concern is the weak-instrument problem and they do not allow for clustering. Gelbach et al. (2009) apply a variant of the wild-cluster bootstrap in Cameron et al. (2008) to an IV setting.

approach, we restrict the bandwidth to 10-year intervals on each side of the cutoff but experiment with different degrees of polynomials for the trends. This is basically the global approach in RDD, where typically high-degree polynomials for trends are used with the full data. For instance, Cattaneo et al. (2017) suggest using 4^{th}-order or 5^{th}-order polynomials in global polynomial approaches, but lower order polynomials for restricted bandwidths. Since we restrict our bandwidth to 10-year intervals rather than taking the full data, we use at most cubic polynomials. Moreover, the highest degree of the polynomial we take depends on whether we allow for split polynomials on either side of the cutoff or use a single polynomial in the full bandwidth. In the former case, we take up to quadratic polynomials; whereas in the latter case, we take up to cubic polynomials. While we show the results with split and with single polynomials separately (for checking robustness), we prefer split polynomials as they are more flexible.

In the first approach explained above—which we call the global approach, we fix the bandwidth but experiment with different functional forms for the trends. In the second approach, we do the opposite; we fix the functional form for time trends but experiment with different bandwidths. In this case, the functional form for time trends is split linear polynomials on each side. Regarding the bandwidths, we start with 10-year intervals and gradually zoom in around the cutoff by narrowing the bandwidth. We call this approach global-to-local approach because, as in a local polynomial approach, we take small bandwidths around the cutoff. However, unlike a local polynomial approach, we do not choose an optimal bandwidth but instead show the results for several bandwidths—that yield a first stage.

We do not use a local polynomial approach with an optimal bandwidth because this approach typically chooses narrow bandwidths for which a first-stage policy effect might not exist in our setting due to the fuzziness in the treatment status of the two birth-year cohorts right around the discontinuity. In measuring the policy effect on schooling outcomes, this fuzziness causes a downward bias—because as shown in the Data Section, the average schooling outcomes for the 1986 birth cohort is way above the fitted line on the left side of the cutoff and those for the 1987 birth cohort is way below the fitted line on the right side of the cutoff. With the THLFS data, this downward bias results in smaller policy effects in magnitude but statistical significance is preserved; whereas with the smaller sample size of the TDHS data, even the statistical significance of the policy effect is lost with many small bandwidths—where the fuzziness is more likely to dominate. Therefore, in the global-to-local approach we use with the TDHS data, we narrow the bandwidth only to the degree that a first stage is preserved.

The reason for our use of two different approaches within RDD is the trade-off between potential bias and precision. The global approach uses more data and, therefore, offers more precision. However, using a wider bandwidth comes at the potential cost of misspecifying the functional form of the time trends. When we zoom in gradually around the cutoff with the global-to-local approach, the chances of functional-form misspecification fall. However, this comes at the cost of reduced statistical power. We use low-order polynomials when we zoom in around the cutoff, as suggested by Gelman and Imbens (2014).

The fundamental identifying assumption in RDD is that potential outcome distributions are smooth around the cutoff. While this continuity assumption is not directly testable, some common diagnostics are used to check its plausibility: (i) continuity of the score density around the cutoff, (ii) null treatment effects on pre-treatment covariates, (iii) null treatment effects at artificial cutoff values.

The continuity assumption may be violated if individuals manipulate the running variable to be on one side the cutoff. We use the formal test developed by Cattaneo, Jansson and Ma (2017), which is based on comparison of the density of observations near the cutoff, to check this possibility. The value of t statistic used to test the hypothesis is -0.172 and the associated p-value is 0.863. Thus, we fail to reject the null that no difference exists in the density of treatment and control groups at the cutoff. Figure A21 presents a graphical illustration of this test: panel (A) gives the histogram of the running variable and panel (B) the estimated densities on both sides of the cutoff.

A second diagnostic check is for covariate balance around the cutoff. In the absence of sorting around the cutoff, we would expect the individuals on either side of the cutoff to be similar. To test this, using 10-year intervals around the cutoff, we regress the covariates on the policy dummy and split linear time trends on each side of the cutoff. The results given in Table A1 in the Appendix indicate no evidence of a jump at the cutoff for the covariates.

Finally, we test the continuity assumption at each age level from 16 to 24 by checking the existence of treatment effects at artificial cutoff values, given in Table A2 in the Appendix. In column (5), we give the treatment effect at the actual cutoff by regressing the ever-migrated status on the policy dummy as well as split linear time trends on each side of the cutoff within the 20-year interval around the cutoff. In columns (1)-(4), we restrict the sample to the pre-treatment period, i.e. 1977-1986 birth cohorts, and incrementally shift the cutoff by 2 years to the left from column (4) to column (1). In a parallel manner, we restrict the sample to the post-treatment period of 1987-1996 in columns (6)-(9) and incrementally shift the cutoff to the right by 2 years from column (6) to (9). For most age values, we find no evidence for alternative cutoffs. For a few age levels, evidence for an alternative cutoff appears; however, these are for columns (1) and (9) with extreme cutoff locations, where the number of observations on one side of the cutoff remains very low.

6.3 Additional Caveats on Identification

The validity of our instrument requires that the instrument satisfy the exclusion restriction assumption—the instrument has no effect on migration outcomes other than through its effect on individuals' education. The instrument potentially changes marriage market and labor market opportunities of individuals. For instance, it could improve employment prospects of a woman thereby increasing her migration propensity for a new job. Similarly, it could change marriage prospects of a woman with a man who lives further away. However, these are exactly some of the channels through which increased education changes migration decisions, and we examine these channels in this paper.

A problematic issue arises when the instrument affects the migration decision of an individual through its effect on the educational attainment of his/her spouse. This would be important only in the migration decisions of women—in the paternalistic structure of Turkish families. For women, this issue is potentially important only in the analyses of migration for marriage and migration with spouse after marriage. It is unlikely to bite in the analyses of migration for education and migration for employment because these type of migrations for women are almost universally for single women in Turkey. Even in the case of migration for marriage and migration with spouse, the policy would affect the husband, as well as the wife, only if both of them are born after 1987. Given the age difference between the husband and the wife in Turkey, the husband would not be affected by the policy for a woman born at the cutoff—for whom our estimates are given. In fact, the mean age difference between the husband and the wife is especially high for young couples in Turkey—who form the married individuals in our sample. According to the 2013 TDHS, the mean interspousal age difference was 7.0 years for women aged 15–19 and 5.3 for women aged 20–24.

7. Results

Here, in the first subsection, we examine the policy effect on educational attainment of men and women. Then, in the second subsection, we move on to analyze the effect of education on migration. The second subsection has four parts. In the first part, we examine the effect of education on the incidence of migration for men and women using the THLFS; in the second part, we examine the effect on both the incidence and timing of migration for women using the TDHS; in the third part, we add the reason for migration to the analysis in the second part. In the last part, also using the TDHS, we examine the heterogeneity in the effect of education on migration by the type of location of residence before age 15.

7.1 Policy Effect on Educational Attainment

We can use RDD in examining the policy effect on education outcomes with both datasets. The reason for this difference with the THLFS dataset is that while we know migration outcomes only for the age at the time of survey, we can infer education outcomes for earlier ages as well—under the assumption that educational outcomes do not change beyond a certain age. For instance, assuming that middle school completion does not change after age 17, we know the middle school completion history of a person from age 17 to the age in the survey year—as in the migration history in the TDHS. We assume that high school completion status does not change after age 20 and college graduation status does not change after age 24.

The RDD that we use to estimate the policy effect on education outcomes is different from the RDD we use to estimate the effect of education on migration outcomes in a number of ways. First, we use sharp RDD here because we essentially estimate a reduced-form policy effect rather than a 2SLS estimate. Second, the fuzziness in the treatment status of the 1986 and 1987 birth cohorts matters in the estimation of the policy effect because it causes a downward bias, as explained before. However, when we estimate the effect of education on migration using a 2SLS regression, this fuzziness is already accounted for because—within a Wald-estimator interpretation—both the numerator on the policy effect on migration and the denominator on the policy effect on education are adjusted by the fuzzy treatment.[40]

[40] In fact, as shown by Hahn et al. (2001), the fuzzy regression discontinuity design—which is equivalent to a two-stage least squares estimation—addresses this problem using the random assignment of the instrumental variable.

Therefore, in this subsection, in addition to the standard RDD, we also use a donut-hole RDD where we remove the fuzziness from our data by excluding the 1986 and 1987 birth cohorts (the donut-hole). Donut-hole RDD, which excludes observations in the immediate vicinity of the cut-off, have recently been used in a number of studies (see, for instance, Almond et al. [2011], Bajari et al. [2011], Barreca et al. [2011], Card and Guiliano [2014]).[41] As discussed in Cattaneo et al. (2017), a donut-hole RDD regression can also be viewed as a falsification approach, where sensitivity of the findings to the observations just around the cutoff is checked. It is important to note that the observations in the donut-hole in our design are not more influential as we use parametric regressions. While the donut-hole RDD removes the bias due to fuzziness in measuring the policy effect, it comes at a cost. While all RDD require extrapolation outside the support of the data by definition, the level of extrapolation is higher in donut-hole RDD, as it requires extrapolation within the donut-hole until the cut-off point. Our robustness checks with various polynomials and bandwidths help us in assessing the sensitivity of our findings to this extrapolation.

Table 3 shows the estimation results for the policy effect on educational outcomes for men and women using the 2004–2017 THLFS data. Panel (A) gives the results for middle school completion, panel (B) for high school completion, and panel (C) for college completion. In each panel, the estimates are given with and without a donut-hole. Here, we use the global-to-local approach, explained in Section 6. We start with the widest possible balanced bandwidth given the minimum age restriction in the sample for that specific educational outcome. For instance, since we restrict the sample to individuals aged 20 or higher in examining high school outcomes, our widest bandwidth includes the 1976–1997 birth cohorts. Then, we gradually narrow the bandwidths until four or five years remain on each side of the cutoff. For all bandwidths, we take split linear polynomials on each side. Due to the few clusters problem discussed in Section 6, we also provide wild-cluster bootstrap p-values.

As can be seen in panels (A) and (B), the estimates—with or without donut-hole—indicate a clear positive policy effect on middle school completion and high school completion. At the same time, as expected, the coefficient estimates are higher when a donut-hole is included. According to the estimates with the donut-hole samples, the policy increases middle school

[41] For instance, Barreca et al. (2011) find that the previous estimates by Almond et al. (2010) on the effect of crossing the very-low-birth-weight threshold on infant mortality falls by more than 50% when the observations in the immediate vicinity of the threshold are excluded. In most of these studies, the reason for the exclusion of observations near the threshold is manipulation of the running variable around the threshold by the agents.

completion by about 21 percentage points for women and by about 15–17 percentage points for men. The spillover effects on high school completion are also strong; the policy increases high school completion by about 7–8 percentage points for women and by about 5–8 percentage points for men. Panel (C) shows that when a donut-hole is not included, no evidence exists for a policy impact on college graduation either for men or women—although the estimates are consistently positive across various samples for women. However, once a donut-hole is included, a positive policy effect on college graduation of women but not of men emerges—which is robust across all samples. In fact, the policy increases college graduation of women by 2–3 percentage points. All of these findings are robust to calculation of standard errors by wild-cluster bootstrap. Moreover, not surprisingly, these findings are consistent with what Figures 1–3 suggest as discussed in the Data Section.

Using the TDHS data, Table 4 presents the policy effect on certain schooling outcomes of women: (i) middle school completion, (ii) high school completion, (iii) some college. The results are again given with and without the donut-hole, as well as for various bandwidths around the cutoff. Split linear polynomials are used for trends. The results in panel (A) show a strong policy effect on middle school completion both with and without the donut-hole; however, the effect is much bigger with the donut-hole. The policy increases middle school completion of women by about 21 to 23 percentage points, which is similar to the estimated effect with the THLFS.

The results in panel (B) of Table 4 indicate a positive policy effect on high school completion. Compared to the estimates with the THLFS data in Table 3, the estimated effect is smaller when the donut-hole is not used but larger when it is used. In addition, unlike the case with the THLFS data, the results are statistically significant only when the donut-hole is used. This is expected as the sample size of the TDHS data is much smaller. As can be seen in panel (C), the patterns of the policy effect on some college experience are very similar to the patterns of the policy effect on high school completion in panel (B). Evidence of a policy effect on some college experience of women emerges only when a donut-hole is used, as in the THLFS data. However, the estimated effect is much larger than the estimated effect on college completion with the THLFS data.

In essence, both datasets provide strong evidence of a policy effect on high school completion of both men and women, as well as suggestive evidence of a policy effect on college attendance of women. Here, it is also important to note that entrance to universities is based on a nationally administered university entrance examination; and, hence, many high school graduates do not go on to college. However, this does not seem to be the case for the set of *compliers* in this sample. Prior to the implementation of the CS reform, almost half of the females dropped out before completing middle school. The compliers to the reform within this large pool may include females with high levels of ability who continue schooling beyond 8 years once pushed by the reform to complete the middle school. These spillover effects will be critical in understanding the findings on the effect of education on migration for education in later sections.

7.2 The Effect of Education on Migration

7.2.1 Incidence of Migration

In this section, using the THLFS data and the difference-in-differences methodology outlined in Section 6, we study the impact of education on the incidence of migration by gender. As explained in Section 6, the key estimated coefficient is the average effect of middle school completion on the ever-migrated status since age 15 for individuals aged 23 to 30. Table 5 reports the estimation results for different age groups starting with individuals aged 15–38 in panel (A), and gradually narrowing down the age range to 18–35 in panel (B), to 21–32 in panel (C), and to 23–30 in panel (D). Within each panel, for a given gender, the first column reports the OLS estimate followed by three different 2SLS estimates. The baseline 2SLS specification includes controls for age and year dummies; the second 2SLS specification adds age-group dummies interacted with a year trend; and the third 2SLS specification replaces this interaction with age dummies interacted with a year trend—for the reasons discussed in Section 6. The corresponding first-stage results are given in Table A3 in the Appendix. The first-stage results indicate a large policy effect on middle school completion, which is consistent with the findings in the previous section. Moreover, F-statistics are well above the suggestive threshold in the literature.

The OLS estimates in Table 5 indicate a large positive association between middle school completion and migration for both men and women. The baseline 2SLS specifications in columns (2) and (6) also show a positive effect of middle school completion on ever-migrated status of both men and women—except for the female sample in panel (D), with the narrowest age range. However, as we gradually make the specification more flexible by adding the interactions of age-group dummies and a year trend in columns (3) and (7) and the interactions of age dummies and year trend in columns (4) and (8), the magnitude of the positive effect also gradually diminishes for both men and women—but especially for women.

In fact, the 2SLS estimates with the interactions of age dummies and year trend in column (8) indicate a virtually zero effect of middle school completion on migration of women. On the other hand, with the same specification, middle school completion (equivalent to a three year increase in the years of schooling) increases the probability of ever migrating after age 15 by 7.9 percentage points for a sample of men between the ages 23 and 30 from a baseline value of 23%. Thus, middle school completion increases the migration propensity by 34% for this group. Note that this estimated effect for men in column (4) is remarkably consistent across various samples.

It is also important to note that in panel (D), with the narrowest age interval, allowing for more flexible age and trend interactions makes little difference. For instance, in panel (D), the estimated effects—regardless of the specification—are all virtually zero for the female sample. However, as we widen the age-range, allowing for more flexible specifications becomes critical.

Compared to the 2SLS estimates, the OLS estimates in panel (D) overestimate the impact of education on migration for both men and women. This indicates that those who decide to migrate may have unobservable characteristics that are positively correlated with both education and migration decisions, leading to an upward bias in the OLS estimates. Another interpretation of the difference in results may be due to potential heterogeneity in the effect of education on migration and that our estimates capture LATEs that may differ from the ATE.

Machin et al. (2012) finds in the Norwegian context that an additional year of education increases the annual migration rate by 15%. They also report a lower point estimate for women than men. Within the context of several European countries that adopted CS reforms, Weiss (2015) finds that an additional year of education increases the number of regional migrations by 16% and the probability of moving to another region by 6% between the ages of 15 and 50. McHenry (2013), on the other hand, reports a negative effect of education on migration of men in the US context. While contextual differences and different definitions of migration limit comparisons of estimates across studies, our results for men are similar to the first two studies in that they also report large effects of education on migration. Our finding of no effects for women are in line with the findings of Machin et al. (2012) and Haapanen and

Bockerman (2017), who find a smaller—albeit positive—effect of education on migration of women than of men. The differences in reported estimates by gender underline the importance of exploring heterogenous impacts of education on migration prospects.

7.2.2 Incidence and Timing of Migration

Here, using information available in the TDHS data on the migration history of women, we examine how the effect of education on migration changes over the life-cycle. Table 6 reports RDD estimates of the effect of education on migration by age where the outcome variable is the ever-migrated status for any reason, with the global approach where 10-year intervals are taken on each side of the cutoff. Panel (A) reports the results when we use a single time trend up to polynomial order of three while panel (B) reports the results when we allow for split time trends up to order of two. The estimations are carried out by age and the results are presented in different rows of Table 6.[42] The corresponding first-stage results are given in Table A4 in the Appendix.

Before we discuss our main findings, we highlight a few issues about the first-stage results. Consistent with our findings in the previous subsection, a sizeable policy effect on middle school completion exists, and the estimated effects are quite robust across specifications. The coefficients are precisely estimated with F values well above 10, with few exceptions when we allow split time trends of order two (last column). We adopt the specification that allows split time trends of order one, presented in column 4 of Table 6, as our preferred specification.

In Table 6, the OLS results presented in the first column indicate a large and positive association between middle school completion and ever-migrated status for all ages beyond age 17. Moreover, the magnitude of the association between schooling and ever-migrated status increases in age. However, the causal relationship between these two variables given by the 2SLS estimates in columns (2) to (6) is quite different. We observe a positive effect of middle school completion on ever-migrated status at ages 17 through 21. Moreover, the magnitude of this effect is substantial; for instance, middle school completion (three years of extra schooling) increases the probability of ever-migrated status at age 20 by 55 percentage points, from a baseline level of 32%. However, after age 21, not only the statistical

[42] Note that the number of observations declines as age increases because while we can observe (retrospectively) outcomes for everyone at age 16, it is not possible to observe outcomes of individuals beyond their age as of the survey date.

significance but also the magnitude of the relationship diminishes. In fact, at age 24, the estimated coefficient with the preferred specification is about zero.[43]

Next, we present the results on the effect of middle school completion on migration decisions over the life-cycle using the global-to-local approach. Table 7 presents the first-stage results where we gradually restrict the time interval on each side of cutoff from 10 years to 3 years, while using split linear polynomials for time trends.[44] The important issue here is the change in the estimates by bandwidth. As can be seen from the table, the magnitude of the estimated policy effect on middle school completion decreases as the bandwidth gets narrower. This is expected because the downward bias resulting from the fuzziness in the treatment status of the 1986 and 1987 birth cohorts becomes more important as the bandwidth gets narrower. Nonetheless, a positive policy effect that is statistically significant at the 1-percent level is preserved until the bandwidth is narrowed to 5-year intervals on each side of the cutoff. When 4-year intervals are taken, statistical significance falls to the 10-percent level; whereas no evidence of a policy effect remains with 3-year intervals around the cutoff.

This highlights the danger of using very narrow bandwidths in a fuzzy RDD—a first-stage might not exist. In fact, this is the very reason we shy away from using the local polynomial approach in this setting, which typically chooses very narrow bandwidths around the cutoff. In accordance with the finding in Table 7, in carrying out the global-to-local approach in our fuzzy RDD analysis, we limit the range of bandwidths to be between 10-years and 5-years on each side of the cutoff. The results given in Table 8 show that our key findings in Table 6 are robust to the choice of different bandwidths.

In essence, Tables 6 and 8 show that while middle school completion does not change migration incidence for women by age 24, it changes timing of the migration by that age. It is important to note the lack of evidence of a middle school completion effect on the ever-migrated status at ages 23 and 24 in Table 6 is consistent with our finding with the THLFS data that middle school completion has virtually a zero-effect on the ever-migrated status of women at ages 23–30. Similar to our findings, Bockerman and Haapanen (2013) find that the

[43] Given that the policy increases the completed years of schooling of an average woman by about one year, the probability of ever-migrated status of an average 20-year-old woman increases from 32% to roughly 50%. What the estimates in Table 6 imply for the ex-post ever-migrated status for the average woman at ages 21 to 24 are consistent with this 50% level. For instance, at age 21, the policy increases the ever-migrated status of the average women by roughly 14 percentage-points (0.409/3) to also about 50%.

[44] The estimated coefficients change by age due to the variation in the sample size.

initial positive effect of education on migration in the Finnish context dissipates several years after the completion of education.

7.2.3 Incidence and Timing of Migration by Reason for Migration

We next turn to the question of how education affects the incidence and timing of migration by reason for migration. Table 10 presents the results for five main reasons for migration (marriage, education, employment, with spouse, with parents) using the global approach with 10-year intervals on each side of the cutoff. In the global approach of Table 10 and of the other tables in the rest of the main text, we present the results only for our preferred specification (split time trends of order one).[45] The results based on the global-to-local approach are given in Table 11 for each reason for migration (in the same format as in Table 9).

These results reveal some striking differences by reason for migration. Evidence for a positive effect of middle school completion on migration for marriage exists at ages 17 and 18. This is consistent with the findings in Kırdar et al. (2018), who report that the CS reform lowered the marriage hazard rates before age 17—when girls are mandated to stay in school and a couple of more years beyond that—while increasing the marriage hazard rates at ages 17 and 18. In fact, they find no change in the probability of ever-married after age 18.

The most striking finding in Tables 10 and 11 is the effect of middle school completion on ever-migrated status for education. As a result of middle school completion, ever-migrated probability increases substantially for all ages. Moreover, the magnitude of this effect increases rapidly until age 20. In other words, the effect of increased education on migration for education is more pronounced during the period individuals start college. This finding is consistent with the suggestive evidence presented in Subsection 7.1 that the policy has a positive effect on college enrollment of women. In addition, the higher supply of high school and college graduates resulting from the policy might have pushed some students into higher-quality tertiary institutions (to distinguish themselves from the rest of the pool in a higher-average-education environment), which are likely to be in major urban areas—away from their home provinces. In addition, Table 9 also indicates evidence of an effect of middle school completion on migration for education at ages 16 and 17—even though migration

[45] Tables A5 to A9 in the Appendix present the results, respectively, for migration for marriage, for education, for employment, with parents, and with spouse—for all specifications in the global approach. The results show that the findings in Table 9 are robust to alternative specifications.

levels are low at these ages. This implies that some children become more likely to migrate for high school education. In rural areas, where schools providing education beyond the compulsory schooling grades do not exist, students are likely to go to boarding schools or live with a close relative in a bigger city.

Tables 9 and 10 also indicate suggestive but not conclusive evidence for a positive effect of middle school graduation on migration for employment reasons. This effect, albeit imprecisely estimated, grows in magnitude as age increases. At the same time, both tables provide evidence that education reduces migration with parents or with spouse; in other words, women become less likely to be tied-movers.

In essence, the evidence shows that while education increases migration for some reasons, it leads to a decline for others. Although no evidence for an effect of education on ever-migrated status by age 24 exists, it leads to a change in both the timing and composition of migration. Results indicate an empowerment effect of education where women acquire more (and may be of higher quality) education by moving to other places and become less likely to be tied-movers.

Earlier descriptive analysis showed that some migration types are more likely to be within province moves than others. For example, Figure 7 reveals that while about half of the migrations for marriage is within-province, most of the migration for education purposes is interprovincial. Table 11 explores the effect of middle school completion by reason for migration, distinguishing between interprovincial and intraprovincial migration. This allows us to explore whether education increases migration for different reasons by inducing moves that cross provincial boundaries and are likely to be of longer distance. The estimates show that while education primarily leads to increases in interprovincial migration for education and employment purposes, it leads to increases in both interprovincial and intra provincial migration for marriage purposes.

7.2.4 Heterogeneous Effects

Here, we examine how the extent to which education enables higher mobility among individuals differs by their initial conditions. We consider two aspects of initial conditions: the type of place of residence before age 15 and the geographic region of residence before age 15.

Table 12 presents estimates by the type of place of residence before age 15: (i) province and district centers, (ii) villages. The results indicate that positive effects of education on migration for marriage and on migration for education are driven by individuals who reside in a province or district center before age 15. In particular, no evidence exists for an effect of education on migration for education purposes for women residing in villages. A potential reason for this result is that the CS reform may have been unsuccessful in raising education levels in rural regions due to poor compliance with the policy. We estimate the policy effect on middle school completion separately for province and district centers and for villages. The results, given in Appendix Table A10, indicate that in fact the effect of the CS reform on middle school completion is actually stronger in villages.[46] Despite the sharp increase in the middle school completion rate in rural areas, the absence of an increase in migration for educational purposes points to other reasons restricting the mobility of women for this purpose, such as the costs involved in such moves (including the psychic costs) being prohibitively high for rural families.

In order to gain a better understanding of the mechanisms underlying the positive effect of education on migration for education purposes, Table 13 presents estimates by geographic region of residence before age 15. The results show that positive effects are concentrated among women living in the western and northern regions of the country. Weak evidence for an effect on those living in the central region exists as well. The estimates of policy effects by geographic regions provide some hints as to why these effects are concentrated in these regions.

Appendix Table A11 presents the policy effects of the CS reform on middle school and high school completion of women aged 15-38 by geographic region, using the THLFS.[47] The results show that a significant policy effect on middle school completion exists in all regions but eastern Turkey. However, the spillover effects on high school completion are visible only in western, central, and northern Turkey; and these are much stronger in western and northern

[46] In fact, Kırdar et al. (2016) find that the reform reduced the urban-rural schooling gap tremendously for both boys and girls.

[47] These estimates are based on THLFS data where we have much larger samples sizes that allow us to estimate first stage regression by regions. The regions in THLFS and TDHS are defined somewhat differently, therefore we cannot present these first stage results based on geographic classifications in Table 13. Note also that we estimate policy effects with the THLFS based on the place of residence as of the survey date. Some individuals may have relocated over time, which biases our estimates of policy effects.

Turkey. That is, the effect of education on migration for education exists in exactly the regions where we observe significant spillover effects of the CS reform. In addition, it is also important to note that migration costs from provinces in western and central Turkey are lower because the prime destinations for migrants (Istanbul, Ankara, Izmir, and Bursa) also lie within these regions.

8. Conclusion

This study uses a major compulsory school reform in Turkey to provide novel evidence on the causal effect of education. Only a few studies in the literature aim to identify the causal effect of education on migration propensity. We extend this literature by examining the effect of education on both the incidence and timing of migration. In addition, for the first time in literature, the paper provides causal effects of education on migration by reason of migration.

Using an instrumental-variables difference-in-differences estimation—where we instrument middle school completion by the reform dummy—and the Turkish Household Labor Force Surveys (THLFS) from 2009 to 2017, we find that education substantially increases the incidence of migration for men whereas no such evidence exists for women.

This finding, however, masks effects of education on the timing and composition of migration as indicated by our analysis of the 2013 Demographic and Health Survey of Turkey (TDHS). Using information on the complete migration history after age 12—including the reason for migration—for the female sample, the TDHS data allow a much richer analysis of migration outcomes. Adopting a fuzzy regression discontinuity design, we find that while education does not change the migration propensity by the mid-20s, women become more likely to migrate at earlier ages. Hence, estimating the effect of education on the propensity of migration by a certain age—as in previous literature—it is possible to miss the effects on the timing of migration. Given that migration is a type of investment, this result underlines the importance of estimating the effect of education on migration over the life-cycle.

This paper also provides estimates of the causal impact of education by reason of migration. The results show that education affects migration prospects differently depending on the reason for migration. For women, migration for education purposes increases remarkably until their early 20s while migration with parents or spouse, i.e. migration as a tied-mover, declines. There is also suggestive evidence that education increases migration for employment purposes. Hence, increasing education not only changes the timing but also the distribution of different types of moves among women. Education fosters further education among women thus induce them to move for education purposes, possibly as they seek a higher level or higher quality of education. These results provide evidence for an empowerment effect of education among women, making them more likely to move for human capital investments and for employment purposes and less likely as tied-movers.

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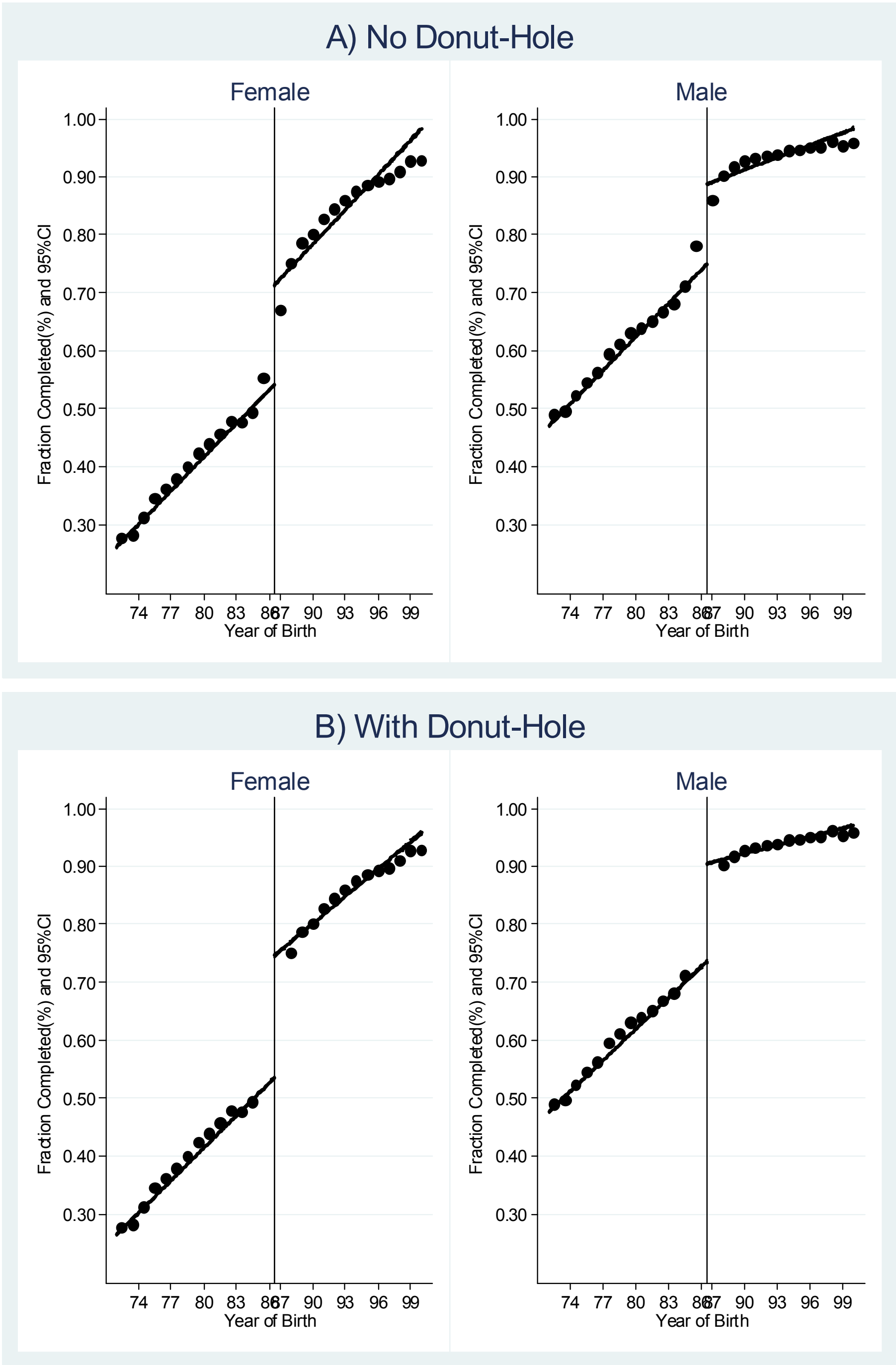
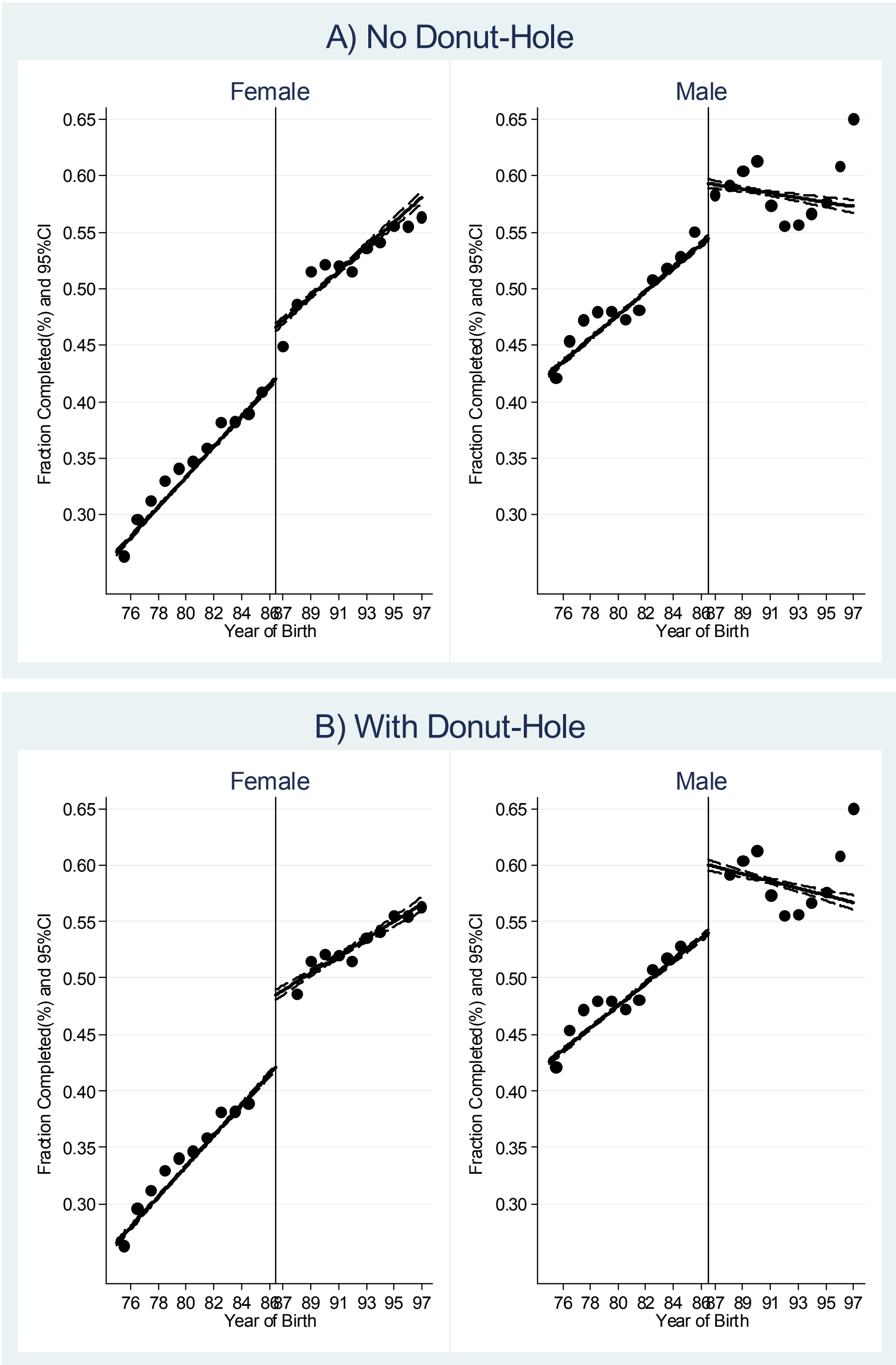
Figures

Figure 1 - Fraction Completing at Least Grade 8, THLFS



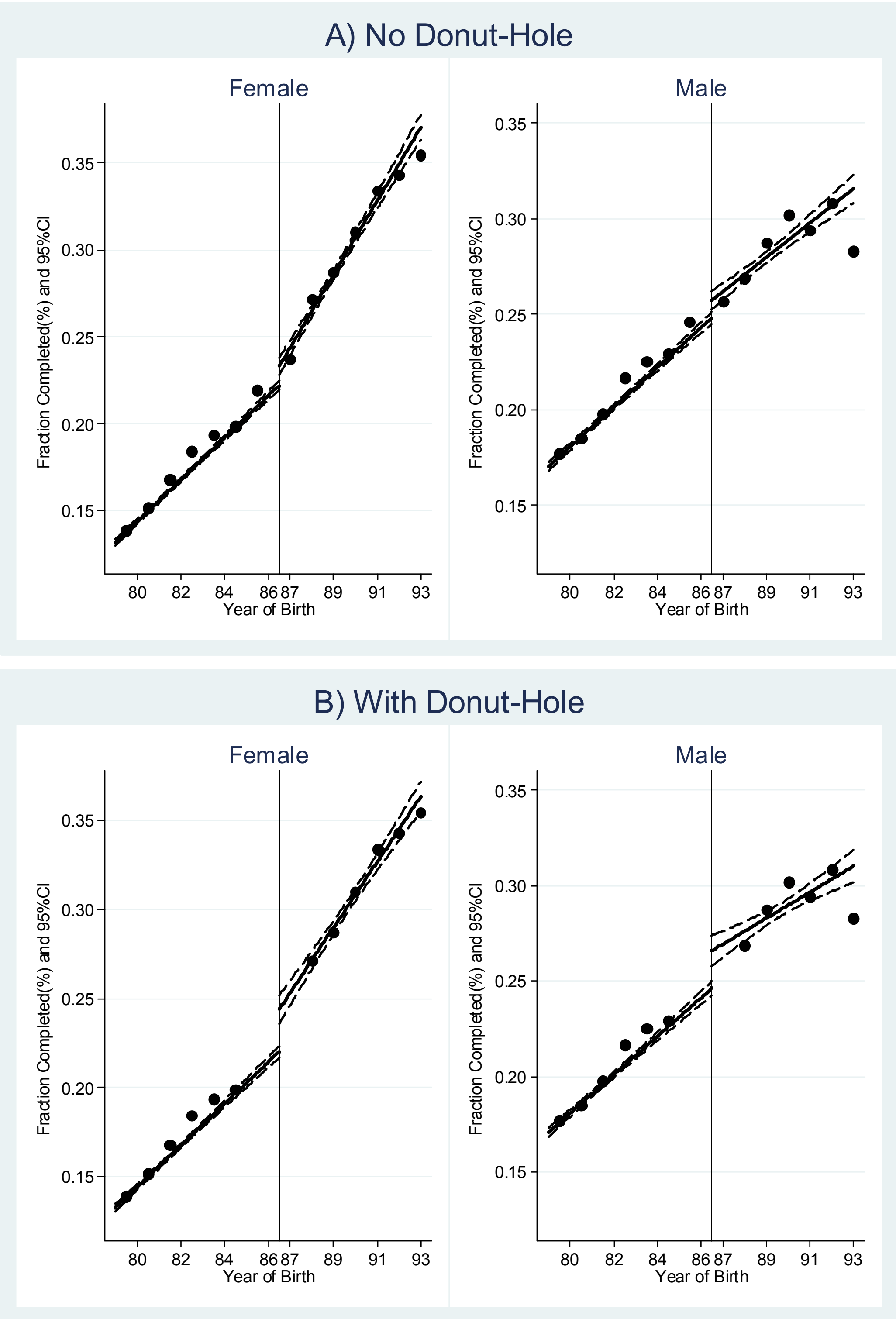
Note: The sample, drawn from the 2004-17 THLFS, is restricted to individuals aged 17 or higher.

Figure 2 - Fraction Completing at Least High School, THLFS



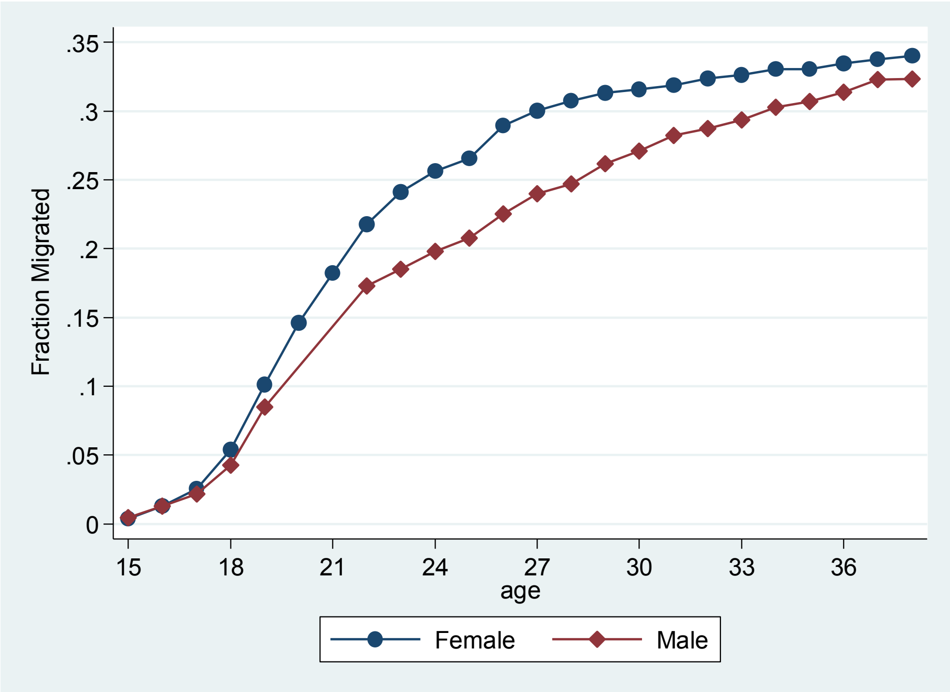
Note: The sample, drawn from the 2004-17 THLFS, is restricted to individuals aged 20 or higher.

Figure 3 - Fraction Completing at Least College, THLFS



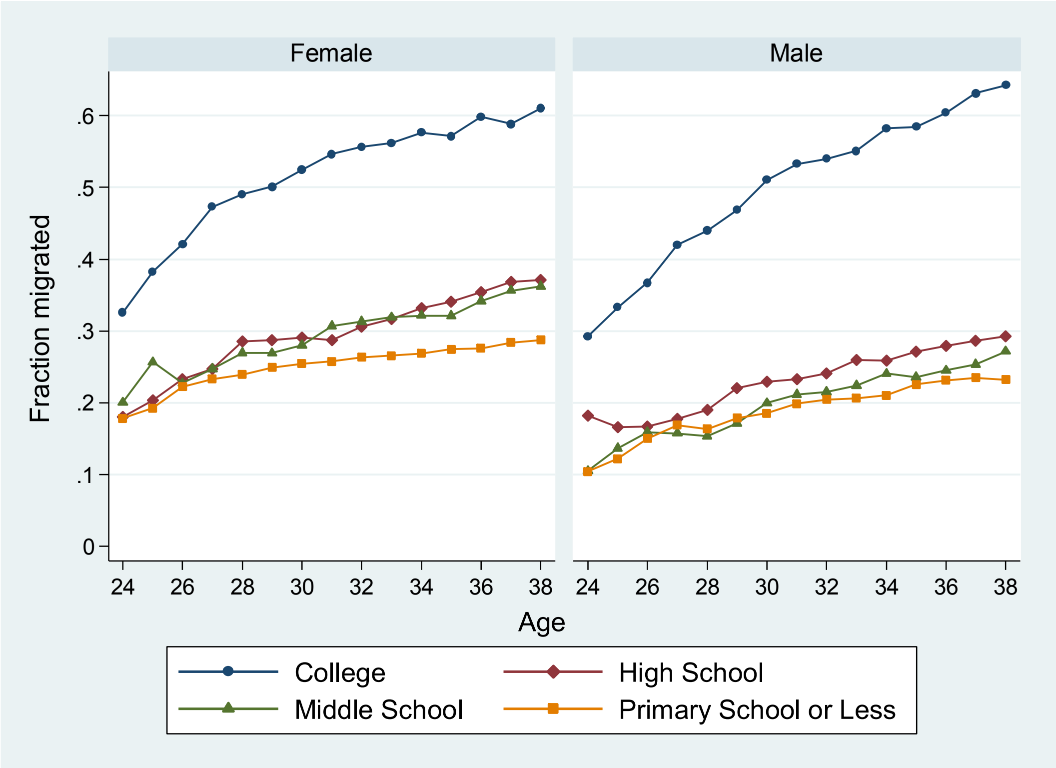
Note: The sample, drawn from the 2004-17 THLFS, is restricted to individuals aged 24 or higher.

Figure 4 – Fraction Migrated by Gender and Age, THLFS



Notes: The sample, drawn from the 2009-17 THLFS, is restricted to individuals aged 15 or higher. The dependent variable is ever-migrated status after age 15. For the male sample, ages 20 and 21 are excluded because an important fraction of men do their mandatory military service at these ages.

Figure 5 – Fraction Migrated by Education, Gender and Age, THLFS



Notes: The sample, drawn from the 2009-17 THLFS, is restricted to individuals born before 1986. College education refers to 2-year or 4-year post-secondary education.

Figure 6 – Fraction Migrated by Age among Women, TDHS

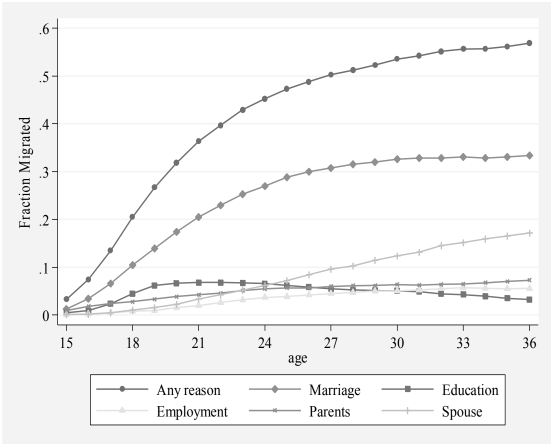
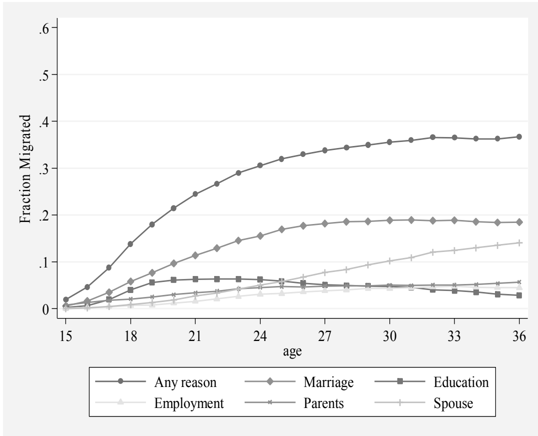
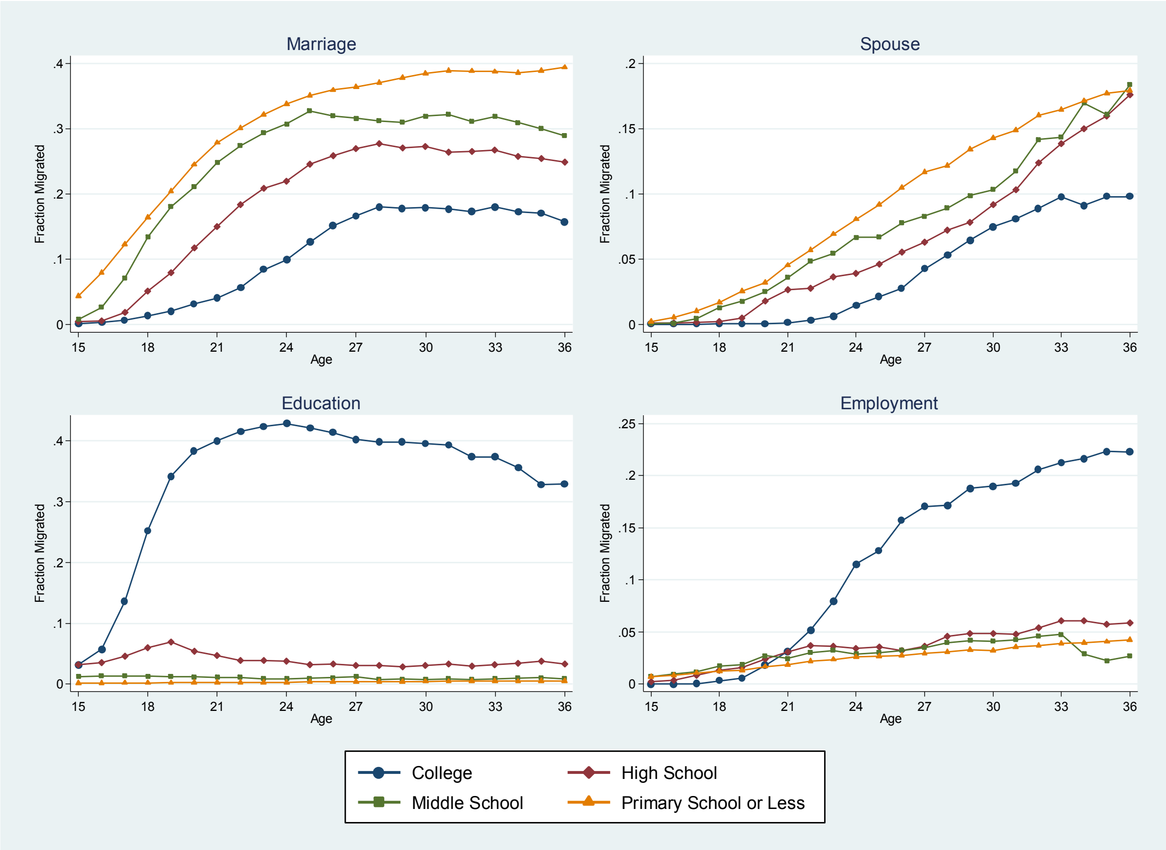
A) Intra- and Inter-provincial Migration	B) Only Interprovincial Migration
	

Figure 7 - Fraction Ever Migrated by Education Level and Reason of Migration, Inter- or intraprovincial



Note: The sample, drawn from the 2013 TDHS, is restricted to individuals aged 15 or higher.

Figure 8 - Fraction Migrated for any Reason by Age

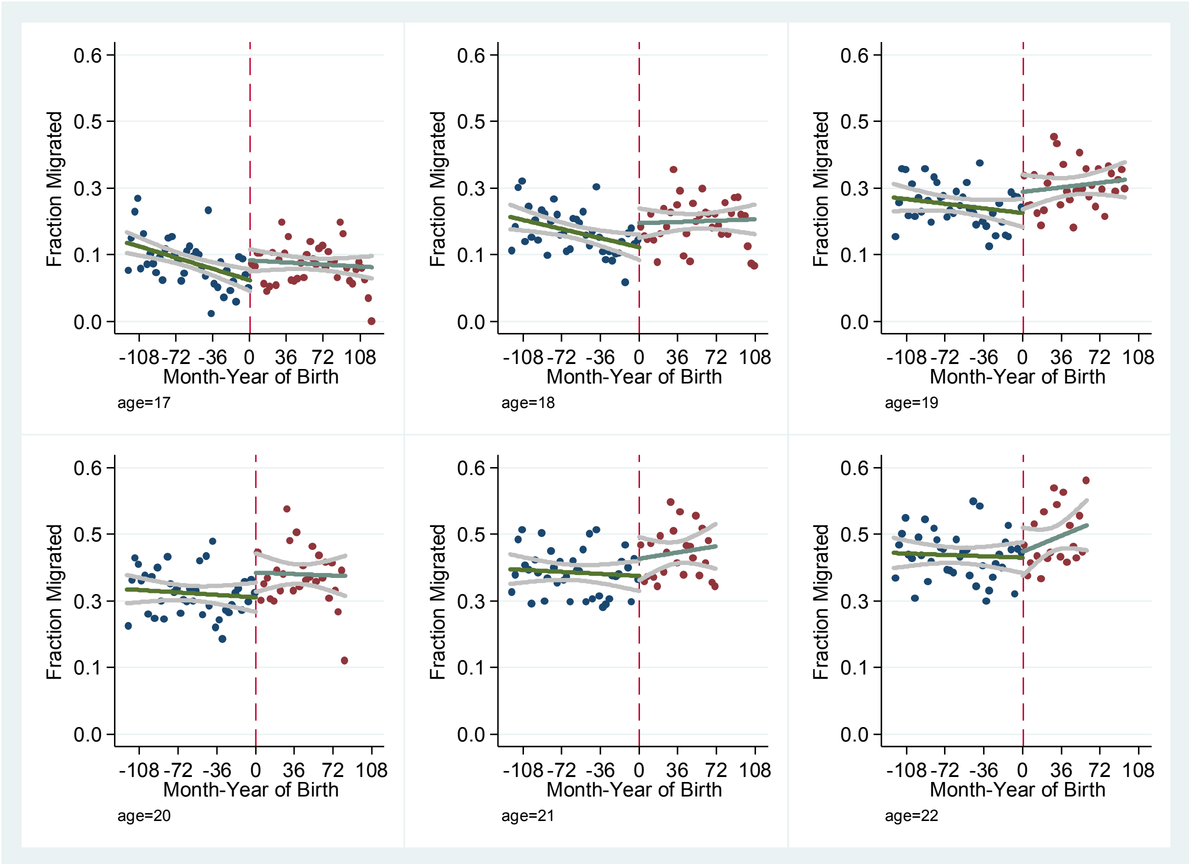


Figure 9 - Fraction Migrated for Marriage Purposes by Age

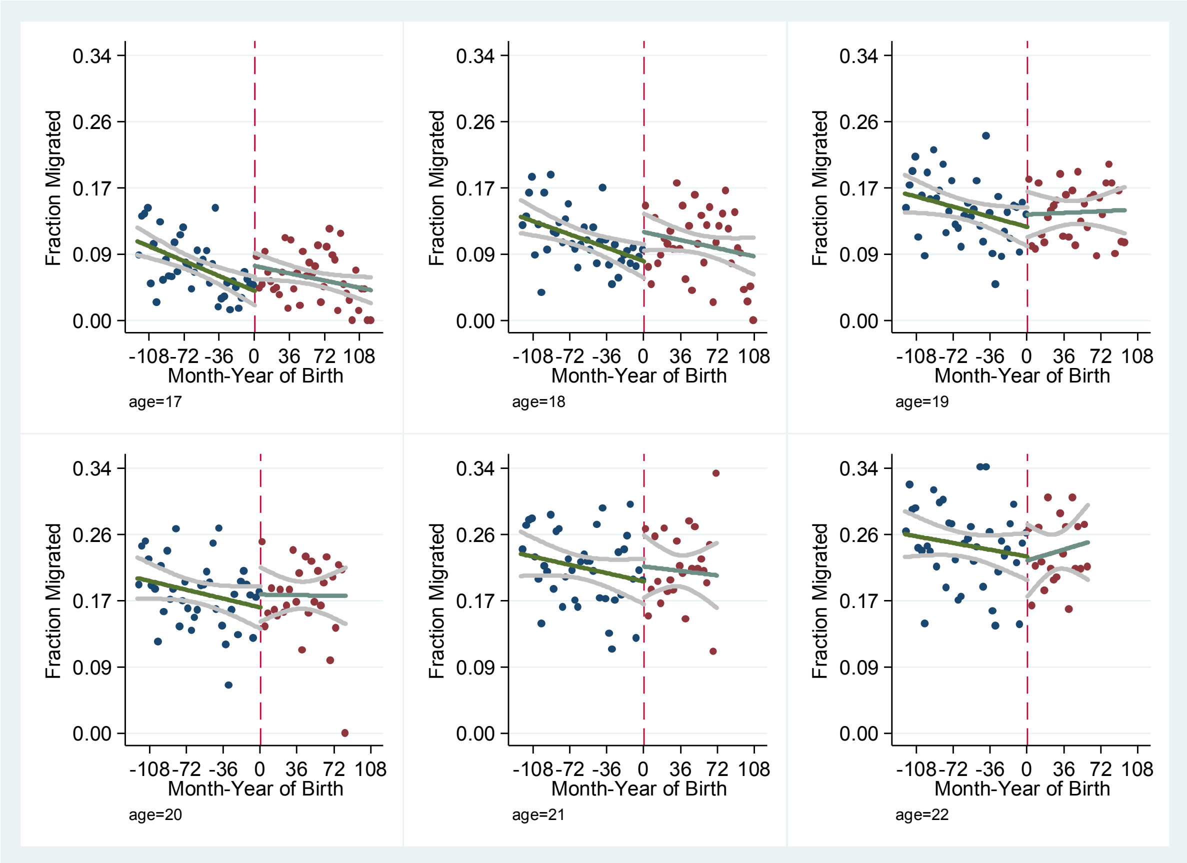


Figure 10 - Fraction Migrated for Employment Purposes by Age

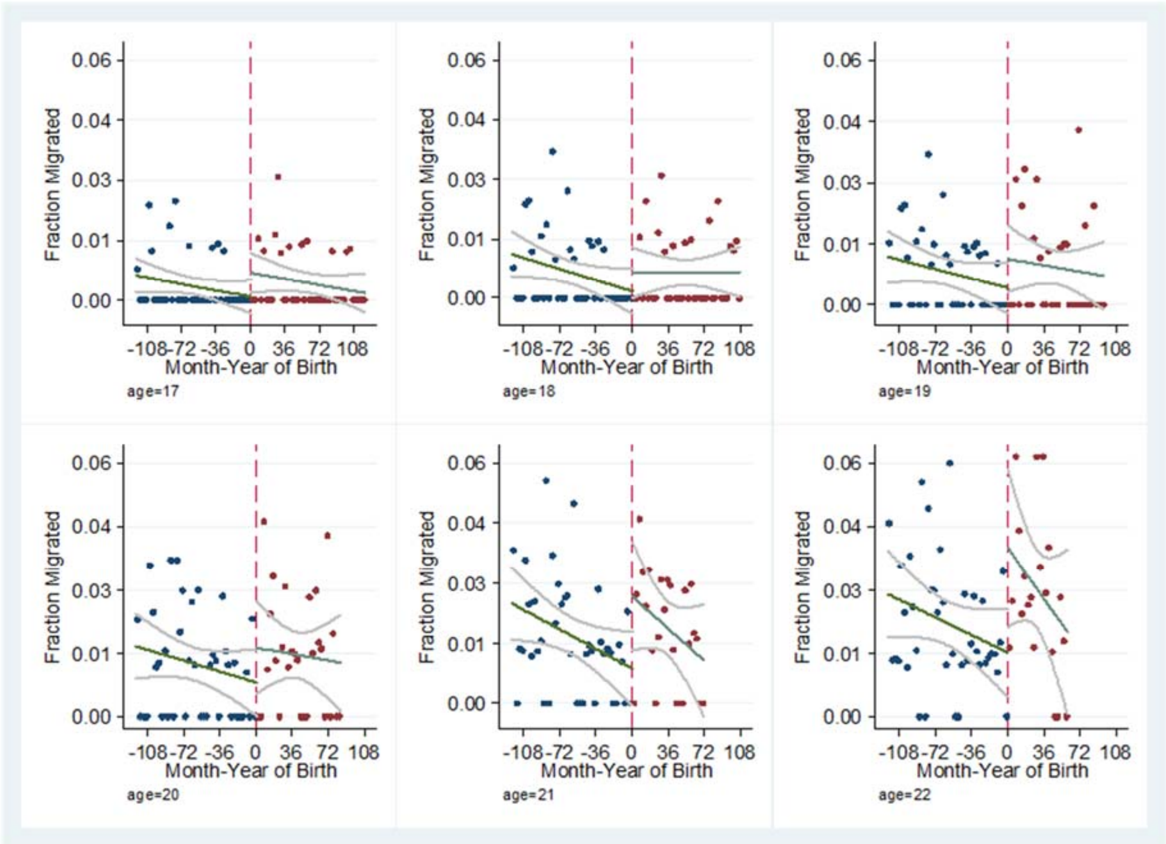
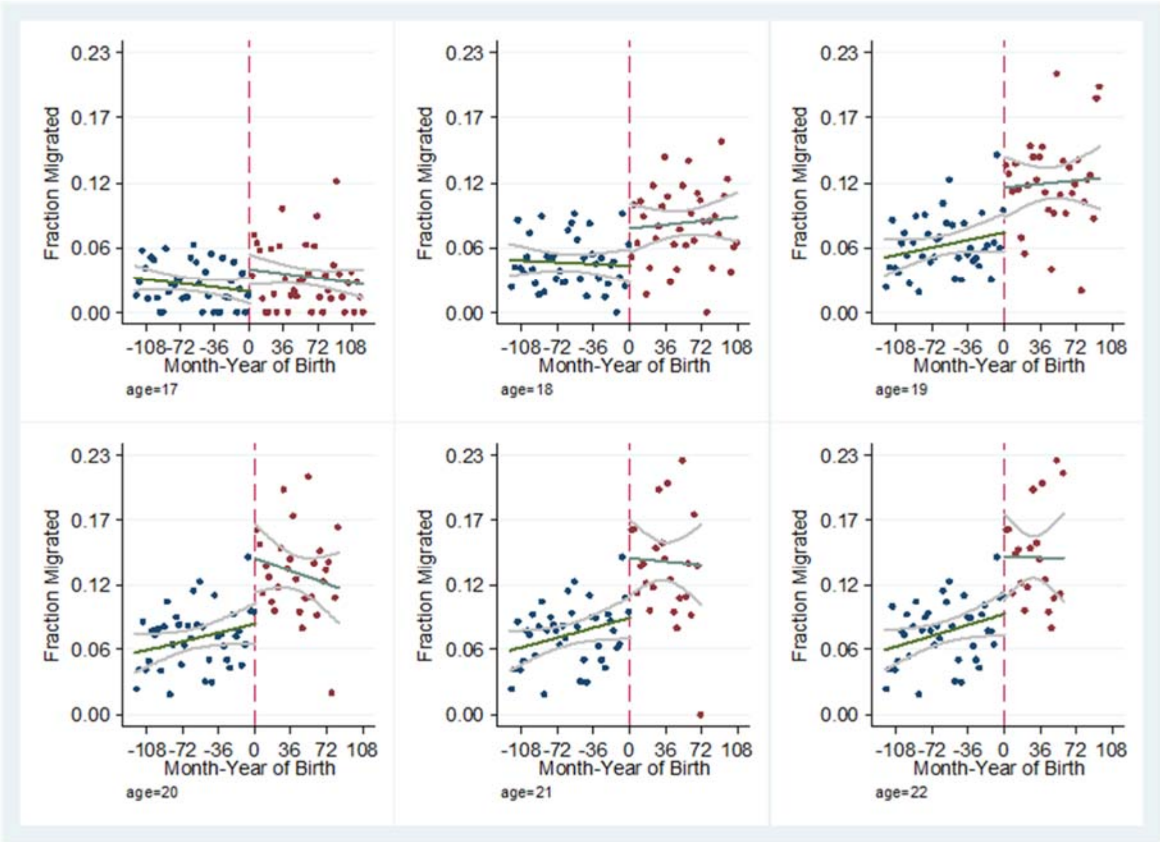


Figure 11 - Fraction Migrated for Educational Purposes by Age



Tables

Table 1 – Summary Statistics for the Two Surveys Used in the Study

A) Means of Variables in the 2009-17 Turkish Household Labor Force Survey Sample (15- to 38-year-old individuals)

A1) Male Sample		A2) Female Sample	
Migrated after age 15	0.196	Migrated after age 15	0.230
Less than middle school	0.222	Less than middle school	0.375
Middle school	0.361	Middle school	0.290
High school	0.269	High school	0.203
Two year college or university	0.148	Two year college or university	0.132
Age	26.078 (7.168)	Age	26.321 (7.117)
Employed	0.662	Employed	0.285
Ever married	0.439	Ever married	0.583

B) Means of Variables in the 2013 Turkish Demographic and Health Survey Sample (Women born between 1977 and 1996)

Migrated after age 15	0.457	Region of Childhood Location of Residence	
Completed the 8th Grade	0.627	NUTS-2 Region 1	0.125
Year of Birth	1986 (5.856)	NUTS-2 Region 2	0.013
Age	26.601 (5.859)	NUTS-2 Region 3	0.018
Mother Literate	0.630	NUTS-2 Region 4	0.042
Father Literate	0.906	NUTS-2 Region 5	0.031
Mother Tongue = Turkish	0.793	NUTS-2 Region 6	0.042
Mother Tongue = Kurdish	0.187	NUTS-2 Region 7	0.037
Mother Tongue = Arabic	0.016	NUTS-2 Region 8	0.036
Mother Tongue = Other	0.004	NUTS-2 Region 9	0.059
Month of Birth		NUTS-2 Region 10	0.030
January	0.109	NUTS-2 Region 11	0.021
February	0.078	NUTS-2 Region 12	0.055
March	0.092	NUTS-2 Region 13	0.050
April	0.097	NUTS-2 Region 14	0.027
May	0.078	NUTS-2 Region 15	0.037
June	0.081	NUTS-2 Region 16	0.019
July	0.076	NUTS-2 Region 17	0.016
August	0.089	NUTS-2 Region 18	0.049
September	0.089	NUTS-2 Region 19	0.042
October	0.088	NUTS-2 Region 20	0.021
November	0.062	NUTS-2 Region 21	0.027
December	0.063	NUTS-2 Region 22	0.031
Type of Childhood Location of Residence		NUTS-2 Region 23	0.039
Large City	0.412	NUTS-2 Region 24	0.036
Small City	0.255	NUTS-2 Region 25	0.059
Village	0.334	NUTS-2 Region 26	0.037

Notes: In panel (A), the number of observations in the THLFS sample is 774,693 for the male sample and 818,352 for the female sample. In panel (B), the number of observations is 5,834 for all variables.

Table 2 - The Cohorts Affected by the Reform

Year \ Age	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38
2009	1994	1993	1992	1991	1990	1989	1988	1987	1986	1985	1984	1983	1982	1981	1980	1979	1978	1977	1976	1975	1974	1973	1972	1971
2010	1995	1994	1993	1992	1991	1990	1989	1988	1987	1986	1985	1984	1983	1982	1981	1980	1979	1978	1977	1976	1975	1974	1973	1972
2011	1996	1995	1994	1993	1992	1991	1990	1989	1988	1987	1986	1985	1984	1983	1982	1981	1980	1979	1978	1977	1976	1975	1974	1973
2012	1997	1996	1995	1994	1993	1992	1991	1990	1989	1988	1987	1986	1985	1984	1983	1982	1981	1980	1979	1978	1977	1976	1975	1974
2013	1998	1997	1996	1995	1994	1993	1992	1991	1990	1989	1988	1987	1986	1985	1984	1983	1982	1981	1980	1979	1978	1977	1976	1975
2014	1999	1998	1997	1996	1995	1994	1993	1992	1991	1990	1989	1988	1987	1986	1985	1984	1983	1982	1981	1980	1979	1978	1977	1976
2015	2000	1999	1998	1997	1996	1995	1994	1993	1992	1991	1990	1989	1988	1987	1986	1985	1984	1983	1982	1981	1980	1979	1978	1977
2016	2001	2000	1999	1998	1997	1996	1995	1994	1993	1992	1991	1990	1989	1988	1987	1986	1985	1984	1983	1982	1981	1980	1979	1978
2017	2002	2001	2000	1999	1998	1997	1996	1995	1994	1993	1992	1991	1990	1989	1988	1987	1986	1985	1984	1983	1982	1981	1980	1979

Note: Individuals born in or after 1987 are affected by the Compulsory Schooling Reform.

Table 3: Policy Effect on Educational Outcomes of Men and Women, HLFS 2004-17

With Donut-Hole					Without Donut-Hole				
	Female		Male			Female		Male	
Birth Cohorts in Sample	Estimates	No. Obs.	Estimates	No. Obs.	Birth Cohorts in Sample	Estimates	No. Obs.	Estimates	No. Obs.
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A) Policy Effect on Completing Middle School									
1973-1985, 1988-2000	0.209***	1,059,525	0.168***	974,988	1973-1986, 1987-2000	0.169***	1,168,448	0.135***	1,070,882
	[0.011]		[0.008]			[0.024]		[0.018]	
Wild Bootstrap p-value	0.000		0.000		Wild Bootstrap p-value	0.000		0.000	
1976-1985, 1988-1997	0.213***	870,289	0.170***	799,045	1976-1986, 1987-1997	0.167***	979,212	0.131***	894,939
	[0.010]		[0.008]			[0.025]		[0.020]	
Wild Bootstrap p-value	0.000		0.000		Wild Bootstrap p-value	0.000		0.000	
1979-1985, 1988-1994	0.210***	655,853	0.173***	596,778	1979-1986, 1987-1994	0.154***	764,776	0.121***	692,672
	[0.009]		[0.009]			[0.026]		[0.024]	
Wild Bootstrap p-value	0.000		0.001		Wild Bootstrap p-value	0.000		0.000	
1982-1985, 1988-1991	0.210***	391,108	0.152***	352,096	1982-1986, 1987-1991	0.130***	500,031	0.088***	447,990
	[0.006]		[0.008]			[0.027]		[0.022]	
Wild Bootstrap p-value	0.000		0.000		Wild Bootstrap p-value	0.026		0.027	
B) Policy Effect on Completing High School									
1976-1985, 1988-1997	0.070***	739,984	0.066***	668,986	1976-1986, 1987-1997	0.050***	825,705	0.054***	743,617
	[0.009]		[0.011]			[0.013]		[0.009]	
Wild Bootstrap p-value	0.000		0.012		Wild Bootstrap p-value	0.001		0.000	
1978-1985, 1988-1995	0.074***	626,918	0.080***	564,653	1978-1986, 1987-1995	0.051***	712,639	0.061***	639,284
	[0.010]		[0.012]			[0.013]		[0.010]	
Wild Bootstrap p-value	0.002		0.003		Wild Bootstrap p-value	0.000		0.001	
1980-1985, 1988-1993	0.078***	491,554	0.076***	441,481	1980-1986, 1987-1993	0.050***	577,275	0.053***	516,112
	[0.012]		[0.015]			[0.014]		[0.011]	
Wild Bootstrap p-value	0.011		0.015		Wild Bootstrap p-value	0.015		0.008	
1982-1985, 1988-1991	0.073***	335,412	0.048**	299,380	1982-1986, 1987-1991	0.041***	421,133	0.034***	374,011
	[0.013]		[0.017]			[0.012]		[0.009]	
Wild Bootstrap p-value	0.031		0.203		Wild Bootstrap p-value	0.044		0.057	
C) Policy Effect on Completing College									
1980-1985, 1988-1993	0.022***	348,029	0.013	321,940	1980-1986, 1987-1993	0.008	401,932	0.003	372,809
	[0.004]		[0.011]			[0.006]		[0.005]	
Wild Bootstrap p-value	0.035		0.294		Wild Bootstrap p-value	0.258		0.666	
1981-1985, 1988-1992	0.021***	285,100	0.005	264,256	1981-1986, 1987-1992	0.007	339,003	-0.001	315,125
	[0.005]		[0.009]			[0.006]		[0.004]	
Wild Bootstrap p-value	0.093		0.623		Wild Bootstrap p-value	0.353		0.845	
1982-1985, 1988-1991	0.023***	223,038	0.007	208,074	1982-1986, 1987-1991	0.006	276,941	0.000	258,943
	[0.004]		[0.009]			[0.006]		[0.004]	
Wild Bootstrap p-value	0.078		0.609		Wild Bootstrap p-value	0.398		0.918	
1983-1985, 1988-1990	0.032***	164,588	0.003	153,641	1983-1986, 1987-1990	0.006	218,491	-0.002	204,510
	[0.003]		[0.003]			[0.007]		[0.005]	
Wild Bootstrap p-value	0.094		0.375		Wild Bootstrap p-value	0.328		0.844	

Notes: The sample includes observations from 2004-2017 Turkish Household Labor Force Surveys. The sample is restricted to ages 17 and above in panel (A), to ages 20 and above in panel (B), and to ages 24 and above in panel (C) in order to prevent censoring in each schooling outcome. As a result, while the youngest birth cohort is the 2000 birth cohort in panel (A), it is the 1997 birth-cohort in panel (B) and the 1993 birth-cohort in panel (C). In each panel, we use alternative bandwidths gradually zooming in around the cutoff. The policy dummy is one when year of birth is greater 1987. Each cell comes from a separate regression of the specified schooling outcome on the policy dummy as well as split linear time trends on either side of the cutoff and survey year dummies. The number of observations is given in columns (3), (5), (8), and (10). Standard errors are clustered at the year-of-birth level. However, as the number of clusters is relatively few, we also calculate p-values using the wild-cluster bootstrap estimation of Cameron et al. (2008). Statistical significance is *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 4: Policy Effect on Educational Outcomes of Women, TDHS

With Donut-Hole			Without Donut-Hole		
Birth Cohorts in Sample	Estimates	No. Obs.	Birth Cohorts in Sample	Estimates	No. Obs.
(1)	(2)	(3)	(4)	(5)	(6)
A) Middle School Completion					
1977-1985, 1988-1996	0.214***	5,222	1977-1986, 1987-1996	0.159***	5,801
	[0.027]			[0.025]	
1979-1985, 1988-1994	0.230***	3,988	1979-1986, 1987-1994	0.154***	4,567
	[0.032]			[0.029]	
1981-1985, 1988-1992	0.222***	2,863	1981-1986, 1987-1992	0.125***	3,442
	[0.043]			[0.033]	
B) High School Completion					
1980-1985, 1988-1993	0.082**	3,403	1980-1986, 1987-1993	0.029	3,982
	[0.041]			[0.031]	
1981-1985, 1988-1992	0.102**	2,863	1981-1986, 1987-1992	0.032	3,442
	[0.047]			[0.034]	
1982-1985, 1988-1991	0.153***	2,279	1982-1986, 1987-1991	0.042	2,858
	[0.054]			[0.037]	
C) Some College					
1980-1985, 1988-1993	0.074*	2,870	1980-1986, 1987-1993	0.030	3,449
	[0.041]			[0.032]	
1981-1985, 1988-1992	0.089**	2,556	1981-1986, 1987-1992	0.031	3,135
	[0.043]			[0.033]	
1982-1985, 1988-1991	0.082*	2,247	1982-1986, 1987-1991	0.019	2,826
	[0.049]			[0.035]	

Notes: The data come from the 2013 TDHS. The sample is restricted to individuals aged 17 and above in panel (A), to individuals aged 20 and above in panel (B), and to individuals aged 22 and above in panel (C). The dependent variable is given in individual panel headings. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate OLS regression of the schooling outcome on the policy dummy and split linear time trends in the running variable on each side of the cutoff as well as other control varibles. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 5 - The Effect of Middle School Degree on Ever-Migrated Status after Age 15 (THLFS data)

	Men				Women			
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
A) Age Interval = 15-38								
Coefficient	0.117***	0.187***	0.120***	0.079**	0.105***	0.137***	0.044**	-0.003
Standard Error	[0.006]	[0.023]	[0.030]	[0.031]	[0.008]	[0.021]	[0.022]	[0.028]
Bootstrap p-value	0.000	0.000	0.000	0.059	0.000	0.000	0.380	0.934
Sample Mean	0.196	0.196	0.196	0.196	0.230	0.230	0.230	0.230
Number of Clusters	32	32	32	32	32	32	32	32
Number of Observations	774,693	774,693	774,693	774,693	818,352	818,352	818,352	818,352
B) Age Interval = 18-35								
Coefficient	0.116***	0.164***	0.099***	0.079**	0.104***	0.110***	0.027	-0.002
Standard Error	[0.003]	[0.025]	[0.032]	[0.031]	[0.006]	[0.024]	[0.024]	[0.028]
Bootstrap p-value	0.000	0.000	0.011	0.046	0.000	0.016	0.527	0.914
Sample Mean	0.218	0.218	0.218	0.218	0.257	0.257	0.257	0.257
Number of Clusters	26	26	26	26	26	26	26	26
Number of Observations	552,267	552,267	552,267	552,267	591,529	591,529	591,529	591,529
C) Age Interval = 21-32								
Coefficient	0.111***	0.132***	0.092**	0.080**	0.103***	0.064***	0.024	-0.003
Standard Error	[0.003]	[0.022]	[0.038]	[0.031]	[0.004]	[0.024]	[0.028]	[0.029]
Bootstrap p-value	0.000	0.000	0.072	0.072	0.000	0.229	0.607	0.928
Sample Mean	0.231	0.231	0.231	0.231	0.279	0.279	0.279	0.279
Number of Clusters	20	20	20	20	20	20	20	20
Number of Observations	362,378	362,378	362,378	362,378	387,609	387,609	387,609	387,609
D) Age Interval = 23-30								
Coefficient	0.104***	0.092***	0.081***	0.079**	0.100***	0.004	-0.007	-0.003
Standard Error	[0.003]	[0.019]	[0.030]	[0.031]	[0.003]	[0.034]	[0.030]	[0.028]
Bootstrap p-value	0.000	0.006	0.015	0.063	0.000	0.919	0.828	0.920
Sample Mean	0.230	0.230	0.230	0.230	0.287	0.287	0.287	0.287
Number of Clusters	16	16	16	16	16	16	16	16
Number of Observations	244,301	244,301	244,301	244,301	258,241	258,241	258,241	258,241
Age Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Group Dummies * Year Trend			Yes				Yes	
Age Dummies * Year Trend				Yes				Yes

Notes: The samples are drawn from 2009-17 THLFS. The sample in each panel is restricted to the age group given in panel headings. The dependent variable is ever-migrated status after age 15. In addition to the key variable of interest, middle school degree, the control variables include age and year dummies. The specifications in columns (3) and (7) also include interactions of year trend with dummies for age groups (15-17, 18-20, 21-23, 24-27, 28+). The specifications in columns (4) and (8) also include the interactions of year trend with age dummies. The standard errors are clustered at the year of birth level. Due to the relatively small number of clusters, wild-cluster bootstrap p-values are also provided. Statistical significance is *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 6 - Effect of Middle School Completion on Ever-Migrated Status by Age (TDHS data), Global Approach

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	2SLS						
Type of Poly.		A) Single			B) Split			
Degree of Poly.		First	Second	Third	First	Second	Baseline	No. Obs
Age=16	0.002	0.073	0.064	-0.040	0.063	-0.102	0.076	5,834
	[0.010]	[0.095]	[0.089]	[0.130]	[0.088]	[0.163]		
Age=17	0.016	0.444***	0.436***	0.344*	0.435***	0.338	0.137	5,798
	[0.013]	[0.142]	[0.140]	[0.196]	[0.140]	[0.250]		
Age=18	0.055***	0.524***	0.507***	0.650***	0.501***	0.689*	0.208	5,483
	[0.015]	[0.163]	[0.171]	[0.251]	[0.170]	[0.356]		
Age=19	0.076***	0.473***	0.446**	0.813***	0.440**	1.073**	0.270	5,186
	[0.017]	[0.162]	[0.187]	[0.273]	[0.183]	[0.491]		
Age=20	0.087***	0.469***	0.610***	0.654**	0.554***	0.600	0.321	4,902
	[0.019]	[0.173]	[0.234]	[0.265]	[0.215]	[0.391]		
Age=21	0.087***	0.419**	0.445	0.520*	0.409*	0.535	0.365	4,632
	[0.020]	[0.173]	[0.276]	[0.274]	[0.236]	[0.442]		
Age=22	0.098***	0.335*	0.529	0.578*	0.357	0.378	0.401	4,370
	[0.021]	[0.177]	[0.337]	[0.314]	[0.266]	[0.330]		
Age=23	0.112***	0.170	0.342	0.333	0.119	0.148	0.433	4,112
	[0.021]	[0.167]	[0.289]	[0.291]	[0.240]	[0.318]		
Age=24	0.109***	0.029	0.332	0.133	-0.057	0.276	0.456	3,845
	[0.022]	[0.183]	[0.297]	[0.340]	[0.244]	[0.290]		

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to 10-year intervals on each side of the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.

(b) The dependent variable is ever-migrated status (for any reason) since age 15. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell in column (1) comes from a separate OLS regression of ever-migrated status on middle-school completion status and a third-degree polynomial in the running variable in addition to other control variables, whereas each cell in columns (2) to (6) comes from a separate 2SLS regression of ever-migrated status on middle-school completion status -- which is instrumented by the policy dummy -- and the specified time trends in month-year of birth as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 7: First-Stage Results -- Policy Effect on Middle School Completion (TDHS data), Global to Local Approach

		(1)	(2)	(3)	(4)	(5)	(6)
		Time Interval on Each Side of the Cutoff					
		10-year	8-year	6-year	5-year	4-year	3-year
Age=16	Coef.	0.157***	0.153***	0.127***	0.112***	0.074*	0.058
	S.E.	[0.025]	[0.028]	[0.033]	[0.036]	[0.040]	[0.042]
	No obs.	5,834	4,564	3,441	2,857	2,277	1,728
Age=17	Coef.	0.157***	0.153***	0.127***	0.112***	0.074*	0.058
	S.E.	[0.025]	[0.028]	[0.033]	[0.036]	[0.040]	[0.042]
	No obs.	5,798	4,564	3,441	2,857	2,277	1,728
Age=18	Coef.	0.152***	0.153***	0.127***	0.112***	0.074*	0.058
	S.E.	[0.025]	[0.028]	[0.033]	[0.036]	[0.040]	[0.042]
	No obs.	5,483	4,564	3,441	2,857	2,277	1,728
Age=19	Coef.	0.147***	0.151***	0.127***	0.112***	0.074*	0.058
	S.E.	[0.026]	[0.028]	[0.033]	[0.036]	[0.040]	[0.042]
	No obs.	5,186	4,539	3,441	2,857	2,277	1,728
Age=20	Coef.	0.135***	0.138***	0.127***	0.112***	0.074*	0.058
	S.E.	[0.027]	[0.029]	[0.033]	[0.036]	[0.040]	[0.042]
	No obs.	4,902	4,255	3,441	2,857	2,277	1,728
Age=21	Coef.	0.132***	0.135***	0.125***	0.112***	0.074*	0.058
	S.E.	[0.029]	[0.031]	[0.033]	[0.036]	[0.040]	[0.042]
	No obs.	4,632	3,985	3,396	2,857	2,277	1,728
Age=22	Coef.	0.116***	0.118***	0.107***	0.112***	0.074*	0.058
	S.E.	[0.030]	[0.032]	[0.034]	[0.036]	[0.040]	[0.042]
	No obs.	4,370	3,723	3,134	2,825	2,277	1,728
Age=23	Coef.	0.124***	0.125***	0.114***	0.120***	0.072*	0.058
	S.E.	[0.033]	[0.035]	[0.037]	[0.039]	[0.040]	[0.042]
	No obs.	4,112	3,465	2,876	2,567	2,252	1,728
Age=24	Coef.	0.138***	0.140***	0.129***	0.135***	0.086*	0.060
	S.E.	[0.038]	[0.039]	[0.041]	[0.044]	[0.044]	[0.041]
	No obs.	3,845	3,198	2,609	2,300	1,985	1,700

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the time intervals specified in column headings. Note that these are maximum time intervals on the right-hand-side of the cutoff; the maximum time interval on the right-hand-side of the cutoff is three years at age 24, four years at age 23, five years at age 22, and so forth.

(b) The dependent variable is middle-school completion status. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate OLS regression of middle-school completion status at the specified age on the policy dummy and split linear time trends in the running variable on each side of the cutoff as well as other control varibles. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 8 - Effect of Middle School Completion Status on Ever-Migrated Status by Age, Global to Local Bandwidths (TDHS data)

		(1)	(2)	(3)	(4)
		Time Interval on Each Side of the Cutoff			
		10-year	8-year	6-year	5-year
Age=16	Coef.	0.063	0.014	-0.039	-0.038
	S.E.	[0.088]	[0.096]	[0.129]	[0.169]
	No obs.	5,834	4,564	3,441	2,857
Age=17	Coef.	0.435***	0.303**	0.415*	0.586**
	S.E.	[0.140]	[0.147]	[0.212]	[0.290]
	No obs.	5,798	4,564	3,441	2,857
Age=18	Coef.	0.501***	0.443**	0.671**	0.882**
	S.E.	[0.170]	[0.179]	[0.261]	[0.347]
	No obs.	5,483	4,564	3,441	2,857
Age=19	Coef.	0.440**	0.525***	0.733***	0.788**
	S.E.	[0.183]	[0.194]	[0.284]	[0.338]
	No obs.	5,186	4,539	3,441	2,857
Age=20	Coef.	0.554***	0.617***	0.572**	0.648**
	S.E.	[0.215]	[0.228]	[0.274]	[0.318]
	No obs.	4,902	4,255	3,441	2,857
Age=21	Coef.	0.409*	0.508**	0.487*	0.579*
	S.E.	[0.236]	[0.254]	[0.284]	[0.325]
	No obs.	4,632	3,985	3,396	2,857
Age=22	Coef.	0.357	0.540*	0.507	0.607*
	S.E.	[0.266]	[0.292]	[0.327]	[0.333]
	No obs.	4,370	3,723	3,134	2,825
Age=23	Coef.	0.119	0.297	0.237	0.295
	S.E.	[0.240]	[0.256]	[0.277]	[0.275]
	No obs.	4,112	3,465	2,876	2,567
Age=24	Coef.	-0.057	0.139	0.065	0.125
	S.E.	[0.244]	[0.256]	[0.276]	[0.276]
	No obs.	3,845	3,198	2,609	2,300

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the time intervals specified in column headings. Note that these are maximum time intervals on the right-hand-side of the cutoff; the maximum time interval on the right-hand-side of the cutoff is three years at age 24, four years at age 23, five years at age 22, and so forth.
(b) The dependent variable is ever-migrated status (for any reason) since age 15. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate 2SLS regression of ever-migrated status on middle-school completion status -- which is instrumented by the policy dummy -- and split linear time trends in month-year of birth on each side of the cutoff as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 9 - Effect of Middle School Completion on Ever-Migrated Status by Reason of Migration and Age (TDHS data), Global Approach

Cause:	Marriage		Education		Employment		with Parents		with Spouse		
	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Age=16	0.040	0.029	0.054*	0.009	-0.003	0.003	-0.069	0.019	-0.013	0.002	5,834
	[0.052]		[0.030]		[0.014]		[0.050]		[0.017]		
Age=17	0.276***	0.058	0.164**	0.028	0.042*	0.005	-0.036	0.025	-0.036	0.005	5,798
	[0.091]		[0.070]		[0.024]		[0.054]		[0.025]		
Age=18	0.316**	0.095	0.278***	0.059	0.039	0.008	-0.090	0.029	-0.046	0.009	5,483
	[0.124]		[0.106]		[0.035]		[0.062]		[0.042]		
Age=19	0.178	0.130	0.306**	0.084	0.074*	0.010	-0.145**	0.034	-0.009	0.014	5,186
	[0.137]		[0.126]		[0.043]		[0.072]		[0.052]		
Age=20	0.210	0.165	0.424***	0.092	0.091	0.017	-0.216**	0.039	-0.030	0.021	4,902
	[0.160]		[0.149]		[0.068]		[0.088]		[0.064]		
Age=21	0.242	0.195	0.406**	0.096	0.136	0.022	-0.276***	0.044	-0.152	0.033	4,632
	[0.214]		[0.164]		[0.085]		[0.105]		[0.093]		
Age=22	0.075	0.222	0.490**	0.095	0.254*	0.029	-0.183	0.046	-0.190	0.041	4,370
	[0.246]		[0.205]		[0.138]		[0.129]		[0.128]		
Age=23	-0.134	0.247	0.572***	0.096	0.177	0.037	0.045	0.054	-0.140	0.049	4,112
	[0.272]		[0.217]		[0.128]		[0.115]		[0.145]		
Age=24	-0.057	0.265	0.432**	0.094	0.219	0.044	-0.018	0.059	0.033	0.058	3,845
	[0.286]		[0.196]		[0.172]		[0.152]		[0.143]		

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the 20-year interval around the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.

(b) The dependent variable is ever-migrated status since age 15 for marriage in columns (1) and (2), for education in columns (3) and (4), for employment in columns (5) and (6), with parents in columns (7) and (8), with spouse in columns (9) and (10), and with parents or spouse in columns (11) and (12). The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate 2SLS regression of ever-migrated status on years of schooling -- which is instrumented by the policy dummy -- and split linear time trends in month-year of birth on either side of the cutoff as well as other control variables. Other control variables include dummy controls for month of birth, mother's mother-tongue (Turkish, Kurdish, Arabic, and other), mother's and father's literacy status, type of location of residence in childhoold in terms of size (large city, small city, village) and region (12 NUTS-1 level regions). The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 10 - Effect of Middle School Completion on Ever-Migrated Status by Reason of Migration and Age (TDHS data), Global to Local Bandwidths

	A) Migration for Marriage				B) Migration for Education			
Time interval (each side):	*10-year*	*8-year*	*6-year*	*5-year*	*10-year*	*8-year*	*6-year*	*5-year*
Age=16	0.040	-0.009	0.071	0.120	0.054*	0.029	0.063	0.021
	[0.052]	[0.057]	[0.087]	[0.115]	[0.030]	[0.033]	[0.046]	[0.063]
Age=17	0.276***	0.224**	0.323**	0.431**	0.164**	0.095	0.254**	0.326*
	[0.091]	[0.102]	[0.151]	[0.211]	[0.070]	[0.083]	[0.125]	[0.178]
Age=18	0.316**	0.289**	0.482**	0.564**	0.278***	0.212*	0.329**	0.450**
	[0.124]	[0.132]	[0.196]	[0.250]	[0.106]	[0.113]	[0.167]	[0.227]
Age=19	0.178	0.242*	0.385*	0.460*	0.306**	0.305**	0.423**	0.450*
	[0.137]	[0.140]	[0.198]	[0.250]	[0.126]	[0.129]	[0.189]	[0.233]
Age=20	0.210	0.260	0.321	0.391	0.424***	0.448***	0.485**	0.530**
	[0.160]	[0.168]	[0.203]	[0.241]	[0.149]	[0.155]	[0.196]	[0.241]
Age=21	0.242	0.319	0.374	0.388	0.406**	0.428**	0.457**	0.521**
	[0.214]	[0.225]	[0.261]	[0.296]	[0.164]	[0.169]	[0.200]	[0.245]
Age=22	0.075	0.216	0.221	0.377	0.490**	0.496**	0.523**	0.492**
	[0.246]	[0.248]	[0.280]	[0.295]	[0.205]	[0.209]	[0.252]	[0.251]
Age=23	-0.134	0.011	-0.024	0.077	0.572***	0.582***	0.642**	0.631**
	[0.272]	[0.266]	[0.299]	[0.289]	[0.217]	[0.222]	[0.270]	[0.267]
Age=24	-0.057	0.105	0.083	0.165	0.432**	0.444**	0.477**	0.477**
	[0.286]	[0.287]	[0.321]	[0.322]	[0.196]	[0.197]	[0.234]	[0.233]

	C) Migration for Employment				D) Migration with Parents or Spouse			
Time interval (each side):	*10-year*	*8-year*	*6-year*	*5-year*	*10-year*	*8-year*	*6-year*	*5-year*
Age=16	-0.003	0.015	0.004	-0.002	-0.078	-0.087	-0.189**	-0.157
	[0.014]	[0.017]	[0.014]	[0.014]	[0.050]	[0.054]	[0.081]	[0.098]
Age=17	0.042*	0.052*	0.046	0.044	-0.070	-0.084	-0.198**	-0.156
	[0.024]	[0.028]	[0.030]	[0.034]	[0.057]	[0.063]	[0.088]	[0.102]
Age=18	0.039	0.073*	0.081*	0.091	-0.117*	-0.133*	-0.269**	-0.202
	[0.035]	[0.037]	[0.046]	[0.057]	[0.070]	[0.077]	[0.111]	[0.129]
Age=19	0.074*	0.078*	0.087	0.093	-0.119	-0.115	-0.250**	-0.181
	[0.043]	[0.044]	[0.056]	[0.071]	[0.082]	[0.083]	[0.123]	[0.145]
Age=20	0.091	0.116	0.057	0.077	-0.214**	-0.215**	-0.343**	-0.257
	[0.068]	[0.071]	[0.079]	[0.099]	[0.106]	[0.107]	[0.146]	[0.166]
Age=21	0.136	0.152*	0.118	0.118	-0.394***	-0.375***	-0.400**	-0.302*
	[0.085]	[0.085]	[0.092]	[0.111]	[0.140]	[0.141]	[0.164]	[0.183]
Age=22	0.254*	0.265**	0.227	0.188	-0.338*	-0.315*	-0.301	-0.261
	[0.138]	[0.134]	[0.145]	[0.135]	[0.184]	[0.188]	[0.217]	[0.211]
Age=23	0.177	0.177	0.134	0.080	-0.088	-0.080	-0.032	-0.035
	[0.128]	[0.124]	[0.137]	[0.132]	[0.189]	[0.190]	[0.214]	[0.205]
Age=24	0.219	0.217	0.179	0.153	0.016	0.012	0.055	0.037
	[0.172]	[0.166]	[0.186]	[0.183]	[0.228]	[0.230]	[0.248]	[0.232]

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. In each column, the sample is restricted to the interval around the cutoff specifed in the column heading. The right hand side of the interval around the cutoff becomes incrementally shorter with age; for instance, at age 24, the only affected birth cohorts are the 1987-1989 birth cohorts.

(b) The dependent variable is ever-migrated status since age 15 for marriage in panel (A), for education in panel (B), for employment in panel (C), or with parents or with spouse in panel (D). The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate 2SLS regression of ever-migrated status on years of schooling -- which is instrumented by the policy dummy -- and a linear polynomial in the running variable that is split around the cutoff, as well as other control variables. Other control variables include dummy controls for month of birth, mother's mother-tongue (Turkish, Kurdish, Arabic, and other), mother's and father's literacy status, type of location of residence in childhoold in terms of size (large city, small city, village) and region (12 NUTS-1 level regions). The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 11 - Effect of Middle School Completion on *Interprovincial and Intraprovincial* ever-Migrated Status by Reason of Migration (TDHS data), Global Approach

	A) Migration for Marriage				B) Migration for Education				C) Migration for Employment			
	Inter-provincial		Intra-provincial		Inter-provincial		Intra-provincial		Inter-provincial		Intra-provincial	
	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Age=16	0.019	0.016	0.022	0.013	0.049**	0.006	0.004	0.004	-0.003	0.003	---	---
	[0.041]		[0.039]		[0.024]		[0.020]		[0.014]		---	
Age=17	0.158**	0.033	0.118**	0.024	0.158**	0.024	0.017	0.005	0.038	0.005	0.004	0.000
	[0.072]		[0.059]		[0.068]		[0.023]		[0.024]		[0.005]	
Age=18	0.226**	0.057	0.084	0.038	0.288***	0.054	0.006	0.006	0.042	0.007	-0.007	0.001
	[0.104]		[0.071]		[0.105]		[0.025]		[0.034]		[0.008]	
Age=19	0.099	0.076	0.075	0.055	0.346***	0.078	-0.011	0.008	0.087**	0.008	-0.021	0.002
	[0.122]		[0.095]		[0.129]		[0.028]		[0.043]		[0.014]	
Age=20	0.060	0.099	0.139	0.068	0.485***	0.087	-0.029	0.008	0.093*	0.013	-0.015	0.005
	[0.143]		[0.113]		[0.153]		[0.035]		[0.055]		[0.033]	
Age=21	0.092	0.119	0.133	0.079	0.458***	0.091	-0.020	0.008	0.149**	0.017	-0.022	0.005
	[0.182]		[0.135]		[0.166]		[0.034]		[0.071]		[0.039]	
Age=22	0.015	0.137	0.082	0.089	0.572***	0.091	-0.054	0.008	0.251**	0.023	-0.011	0.007
	[0.218]		[0.160]		[0.210]		[0.049]		[0.122]		[0.045]	
Age=23	-0.222	0.154	0.065	0.099	0.640***	0.092	-0.061	0.007	0.188	0.030	-0.038	0.007
	[0.267]		[0.168]		[0.224]		[0.045]		[0.115]		[0.049]	
Age=24	-0.019	0.166	-0.026	0.104	0.469**	0.091	-0.087*	0.007	0.202	0.037	0.001	0.007
	[0.259]		[0.171]		[0.199]		[0.049]		[0.144]		[0.047]	

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the 20-year interval around the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts. The number of observations is 5,834 at age 16; 5,798 at age 17; 5,483 at age 18; 5,186 at age 19; 4,902 at age 20; 4,632 at age 21; 4,370 at age 22; 4,112 at age 23; and 3,845 at age 24.

(b) The dependent variable is ever-migrated status at the intra-provincial or inter-provincial level, as given in column headings, since age 15 for marriage in columns (1) and (2), for education in columns (3) and (4), for employment in columns (5) and (6), with parents in columns (7) and (8), with spouse in columns (9) and (10), and with parents or spouse in columns (11) and (12). The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate 2SLS regression of ever-migrated_inter status on years of schooling -- which is instrumented by the policy dummy -- and split linear time trends in month-year of birth on either side of the cutoff as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 12 - Effect of Middle School Completion on Ever-Migrated Status by Reason of Migration and the *Type of Place of Residence before Age 15* (TDHS data), Global Approach

		A) Migration for Marriage				B) Migration for Education				C) Migration for Employment			
		Province or District Center		Village		Province or District Center		Village		Province or District Center		Village	
		2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Age=16	Coef.	0.073	0.007	-0.031	0.065	0.073	0.005	-0.024	0.013	-0.009	0.001	0.008	0.002
	S.E.	[0.052]		[0.094]		[0.045]		[0.049]		[0.017]		[0.021]	
	No obs.	3,666		2,168		3,666		2,168		3,666		2,168	
Age=17	Coef.	0.242**	0.021	0.133	0.112	0.208**	0.027	-0.026	0.018	0.034	0.001	0.047	0.005
	S.E.	[0.102]		[0.129]		[0.090]		[0.059]		[0.027]		[0.034]	
	No obs.	3,641		2,157		3,641		2,157		3,666		2,157	
Age=18	Coef.	0.336**	0.040	0.102	0.169	0.307**	0.068	0.087	0.027	0.012	0.002	0.055	0.012
	S.E.	[0.153]		[0.182]		[0.152]		[0.084]		[0.032]		[0.048]	
	No obs.	3,432		2,051		3,432		2,051		3,666		2,051	
Age=19	Coef.	0.071	0.056	0.174	0.225	0.411**	0.106	0.052	0.037	0.034	0.002	0.037	0.013
	S.E.	[0.150]		[0.216]		[0.173]		[0.104]		[0.050]		[0.051]	
	No obs.	3,211		1,975		3,211		1,975		3,666		1,975	
Age=20	Coef.	0.036	0.074	0.246	0.276	0.645***	0.117	0.039	0.035	0.014	0.004	0.067	0.019
	S.E.	[0.195]		[0.271]		[0.232]		[0.115]		[0.064]		[0.076]	
	No obs.	3,020		1,882		3,020		1,882		3,666		1,882	
Age=21	Coef.	0.195	0.089	0.113	0.325	0.596**	0.122	0.032	0.036	0.101	0.007	0.092	0.021
	S.E.	[0.243]		[0.266]		[0.237]		[0.115]		[0.089]		[0.083]	
	No obs.	2,833		1,799		2,833		1,799		3,666		1,799	
Age=22	Coef.	0.197	0.108	-0.204	0.363	0.599**	0.123	-0.018	0.036	0.072	0.012	0.239*	0.025
	S.E.	[0.296]		[0.345]		[0.273]		[0.145]		[0.114]		[0.141]	
	No obs.	2,668		1,702		2,668		1,702		3,666		1,702	
Age=23	Coef.	-0.128	0.128	-0.300	0.395	0.692***	0.123	-0.102	0.034	0.059	0.021	0.178	0.031
	S.E.	[0.259]		[0.400]		[0.260]		[0.178]		[0.104]		[0.158]	
	No obs.	2,507		1,605		2,507		1,605		3,666		1,605	
Age=24	Coef.	-0.194	0.134	-0.367	0.423	0.621**	0.123	-0.181	0.032	0.219	0.033	0.154	0.031
	S.E.	[0.300]		[0.443]		[0.259]		[0.212]		[0.156]		[0.172]	
	No obs.	2,330		1,515		2,330		1,515		3,666		1,515	

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the 20-year interval around the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.

(b) The dependent variable is ever-migrated status since age 15 for individuals living in a province or district center before age 15 in columns (1), (2), (5), (6), (9), (10) and in a village in columns (3), (4), (7), (8), (11), (12). The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate 2SLS regression of ever-migrated_inter status on years of schooling -- which is instrumented by the policy dummy -- and split linear time trends in month-year of birth on either side of the cutoff as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (12 NUTS-1 level regions). The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 13 - Effect of Middle School Completion on ever-Migrated Status for Education Purposes by *Region of Residence before Age 15* (TDHS data), Global Approach

		Dependent Variable: Migration for Education											
		Istanbul		West		Central		South		North		East	
		2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline	2SLS	Baseline
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Age=16	Coef.	0.000	0.004	0.135**	0.007	0.021	0.016	-0.166	0.012	-0.001	0.017	0.132	0.005
	S.E.	0.000		[0.062]		[0.050]		[0.173]		[0.059]		[0.147]	
	No obs.	378		937		858		757		1,003		1,901	
Age=17	Coef.	-0.080	0.023	0.206*	0.029	0.125	0.035	-0.279	0.034	0.112	0.047	0.408	0.013
	S.E.	[0.182]		[0.112]		[0.102]		[0.243]		[0.103]		[0.294]	
	No obs.	377		933		853		751		997		1,887	
Age=18	Coef.	0.131	0.054	0.312*	0.080	0.072	0.055	-0.007	0.066	0.225	0.087	0.506	0.028
	S.E.	[0.318]		[0.177]		[0.139]		[0.331]		[0.163]		[0.561]	
	No obs.	355		883		811		715		943		1,776	
Age=19	Coef.	-0.092	0.085	0.460*	0.127	0.002	0.071	-0.137	0.101	0.360*	0.103	1.036	0.035
	S.E.	[0.391]		[0.240]		[0.161]		[0.410]		[0.208]		[0.799]	
	No obs.	335		830		763		683		901		1,674	
Age=20	Coef.	-0.207	0.095	0.635**	0.142	0.174	0.074	-0.058	0.112	0.516**	0.108	1.027	0.039
	S.E.	[0.437]		[0.271]		[0.185]		[0.518]		[0.261]		[0.843]	
	No obs.	312		786		716		646		866		1,576	
Age=21	Coef.	-0.345	0.094	0.597*	0.144	0.052	0.080	-0.043	0.117	0.605*	0.110	1.363	0.043
	S.E.	[0.519]		[0.311]		[0.197]		[0.596]		[0.315]		[1.404]	
	No obs.	290		747		685		616		826		1,468	
Age=22	Coef.	-0.277	0.089	0.830**	0.140	0.208	0.082	-0.357	0.119	0.557	0.111	1.161	0.045
	S.E.	[0.581]		[0.393]		[0.269]		[0.986]		[0.405]		[1.660]	
	No obs.	276		704		643		582		796		1,369	
Age=23	Coef.	0.039	0.091	0.719*	0.147	0.501	0.079	-0.215	0.119	0.765	0.109	0.664	0.042
	S.E.	[0.517]		[0.367]		[0.339]		[0.604]		[0.705]		[0.728]	
	No obs.	253		665		610		549		763		1,272	
Age=24	Coef.	0.209	0.086	0.661*	0.144	0.431	0.082	-0.198	0.116	0.318	0.110	0.443	0.037
	S.E.	[0.673]		[0.374]		[0.321]		[0.721]		[0.622]		[0.404]	
	No obs.	235		632		572		511		724		1,171	

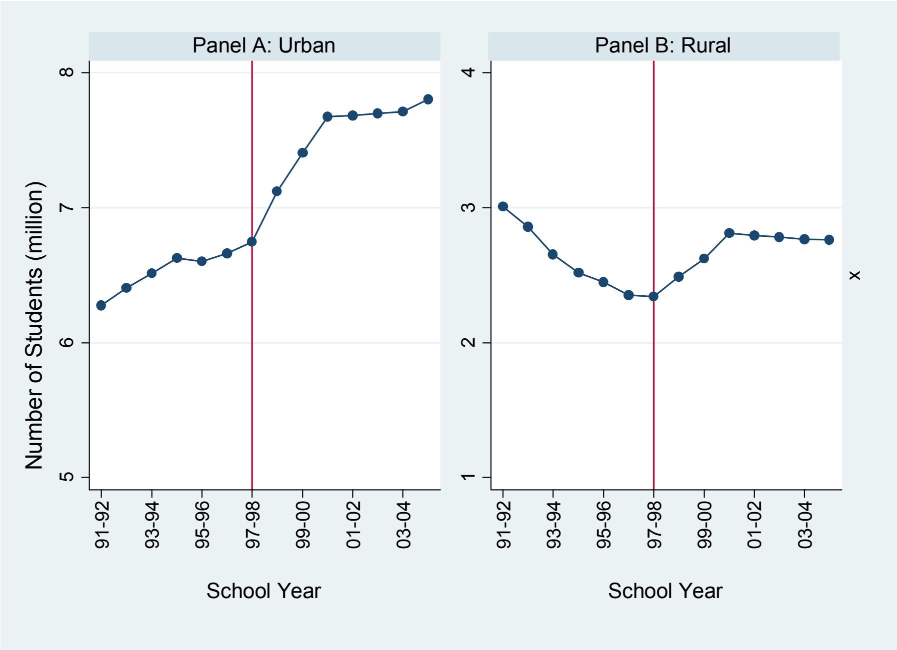
Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the 20-year interval around the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.

(b) The dependent variable is ever-migrated for education status since age 15 for Istanbul in columns (1) and (2), for West in columns (3) and (4), for Central in columns (5) and (6), for South in columns (7) and (8), for North in columns (9) and (10), and for East in columns (11) and (12). The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate 2SLS regression of ever-migrated status on middle school completion -- which is instrumented by the policy dummy -- and split linear time trends in month-year of birth on either side of the cutoff as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

APPENDIX

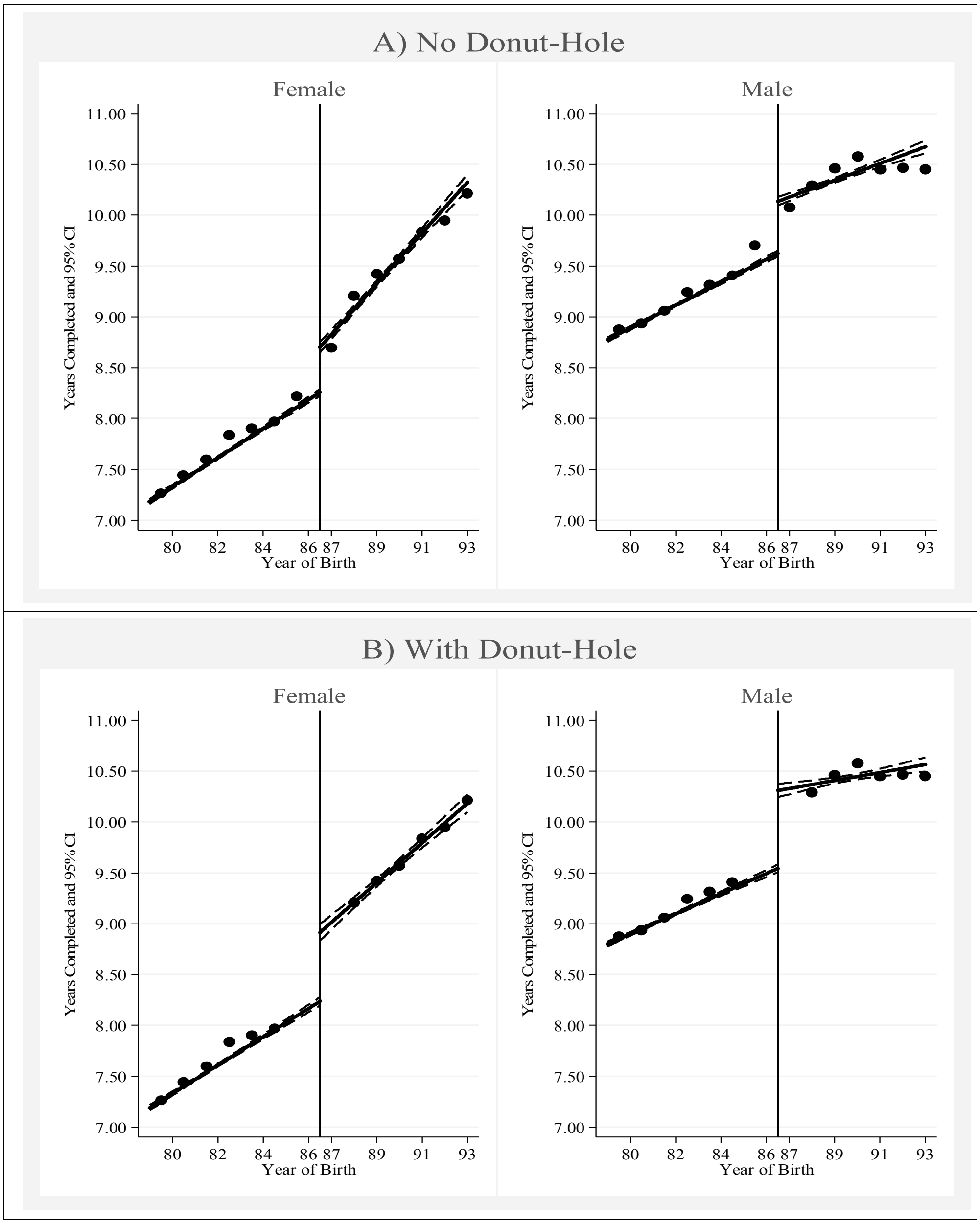
Supplementary Figures

Figure A1 - Number of Students in Basic Education (Grades 1 to 8)



Source: Turkish Statistical Institute (1992-2005)

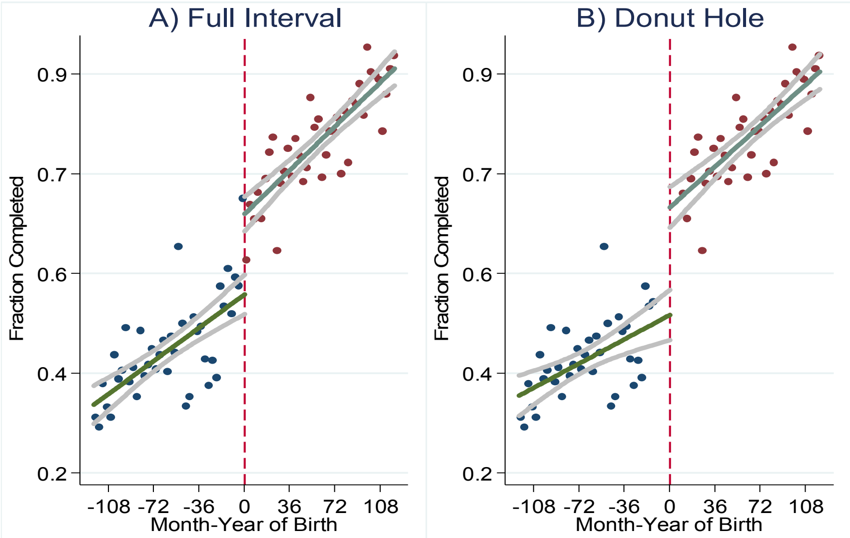
Figure A2 - Completed Years of Schooling, THLFS[48]



Note: The sample, drawn from the 2004-17 THLFS, is restricted to individuals aged 24 or higher.

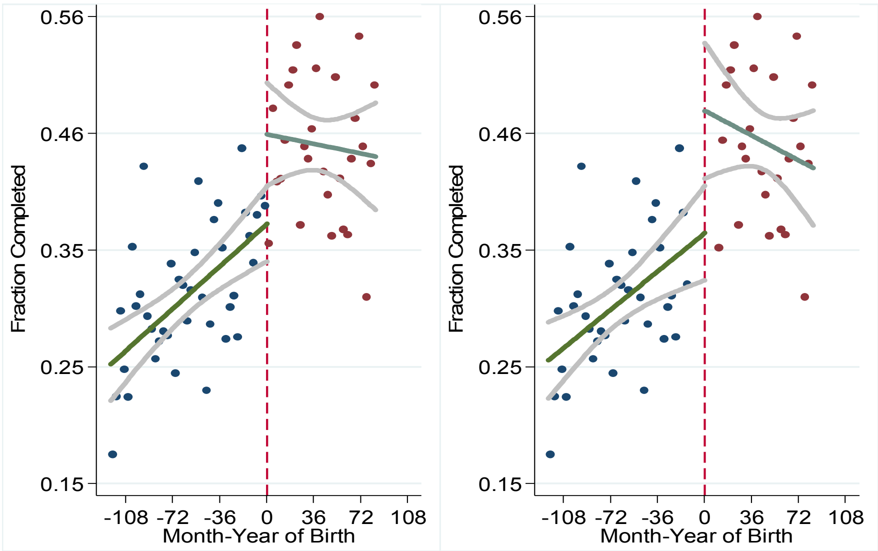
[48] The THLFS data provides information on the level of schooling attainment but not on completed years of schooling. In order to generate the years of schooling variable based on the on the highest completed level of schooling, we use information from the TDHS data—which includes information on both completed degrees and completed years of schooling. Using this information, we generate the mean years of schooling for each of the highest completed schooling level in THLFS sample. Using information on the distribution of years of schooling in the TDHS data for each degree attainment, we find that the average years of schooling is 0.15 years for illiterates, 2.05 years for literates with no degrees, 5.11 years for primary school graduates, and 8.44 years for middle school graduates. We thus assign zero years for illiterates, two years for literates with no degrees, five years for primary school graduates, 8 years for middle school graduates, and 11 years for high school graduates in generating the years of schooling variable.

Figure A3: Fraction of Women completing at Least Middle School



Note: The sample is restricted to individuals aged 17 or higher.

Figure A4: Fraction of Women completing at Least High School



Note: The sample is restricted to individuals aged 20 or higher.

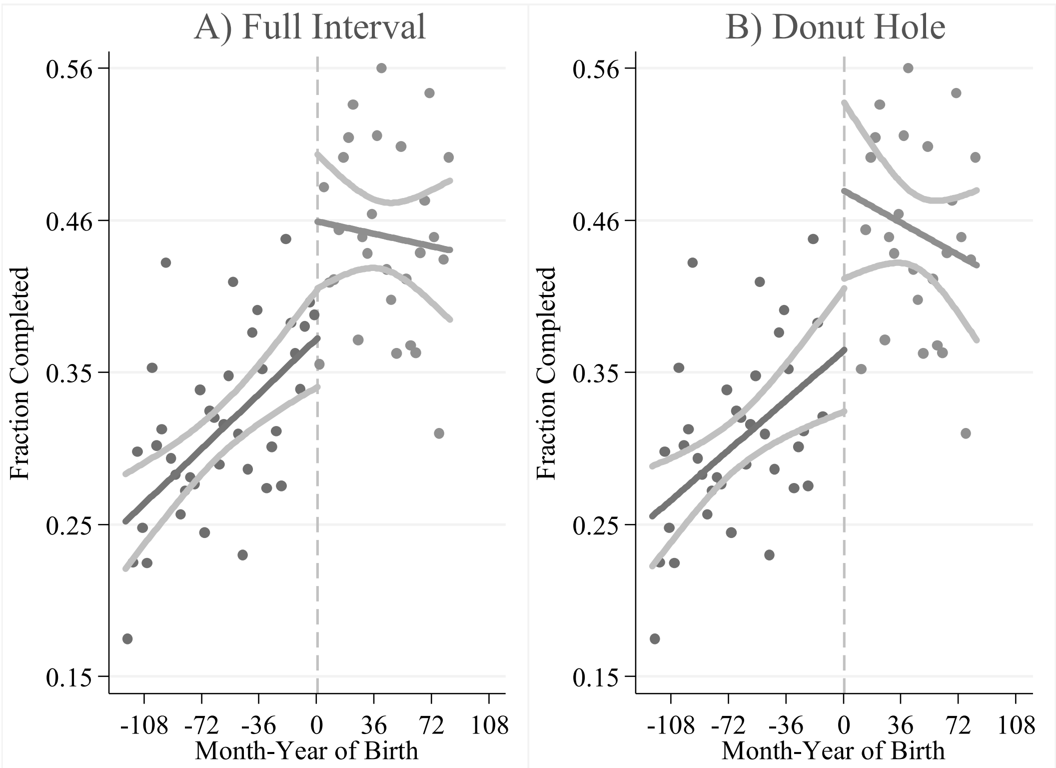
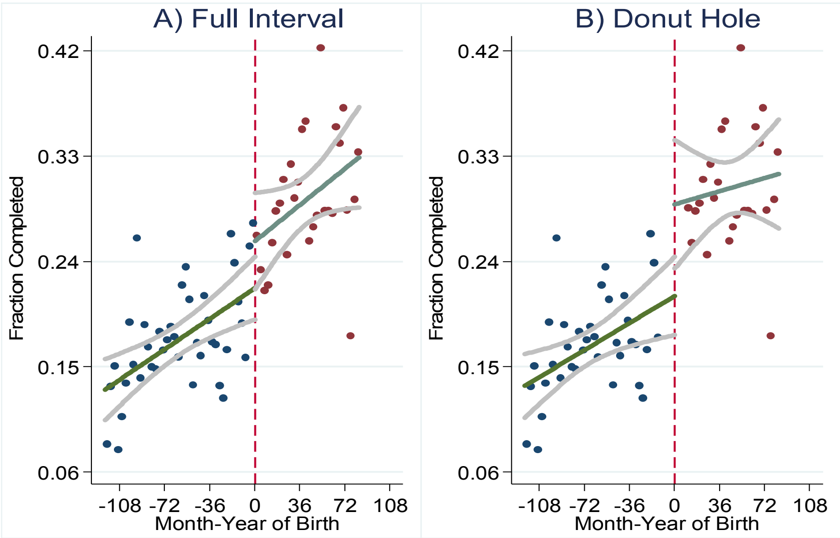
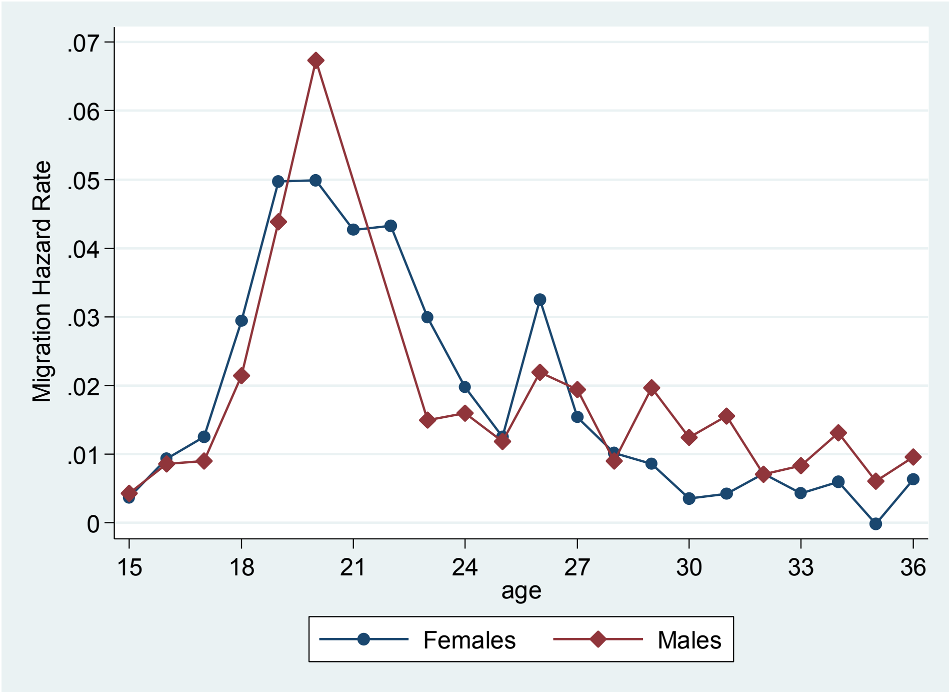


Figure A5: Fraction of Women with Some College



Note: The sample is restricted to individuals aged 22 or higher.

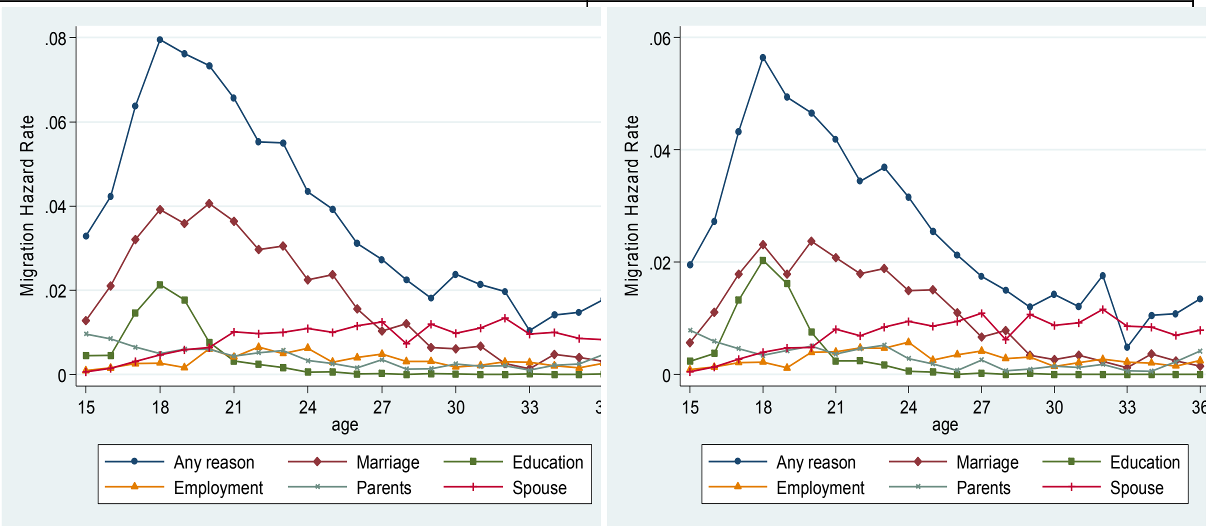
Figure A6 - Migration Hazard Rate by Gender and Age



Note: The sample, drawn from the 2009-17 THLFS, is restricted to individuals aged 15 or higher.

Figure A7 - Migration Hazard Rate by Reason of Migration

A) Migration Hazard Rates	B) Inter-provincial Migration Hazard Rates



Note: The sample, drawn from the 2013 TDHS, is restricted to individuals aged 15 or higher.

Figure A8: Fraction Migrated for any Reason by Age with Donut-Hole, 10-year intervals

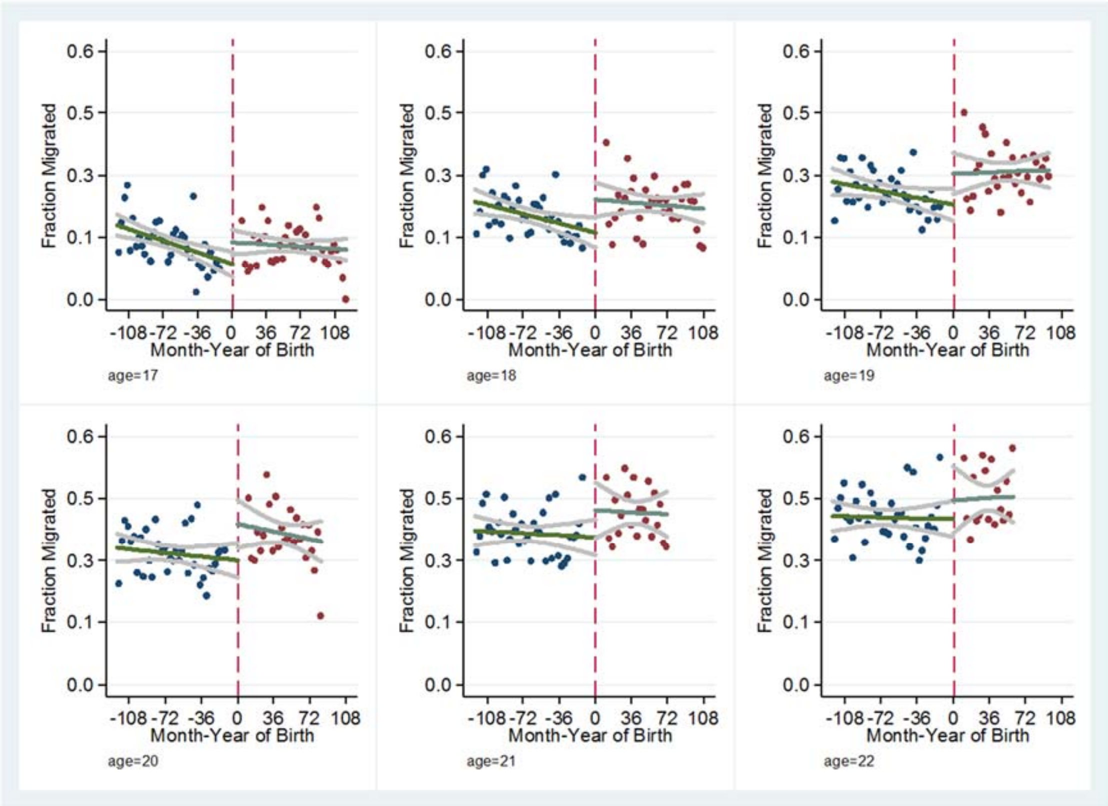


Figure A9: Fraction Migrated for Marriage, 10-year intervals with Donut-Hole

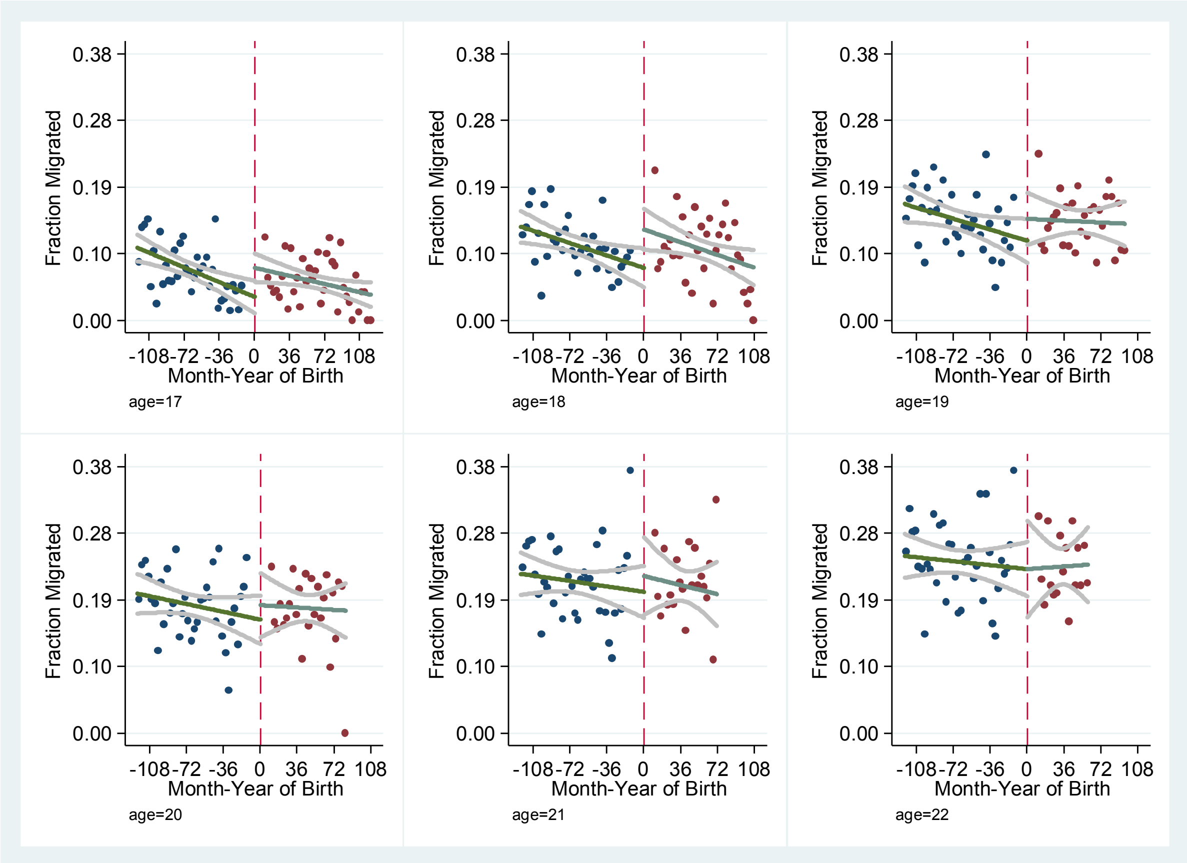


Figure A10: Fraction Migrated for Education, 10-year intervals with Donut-Hole

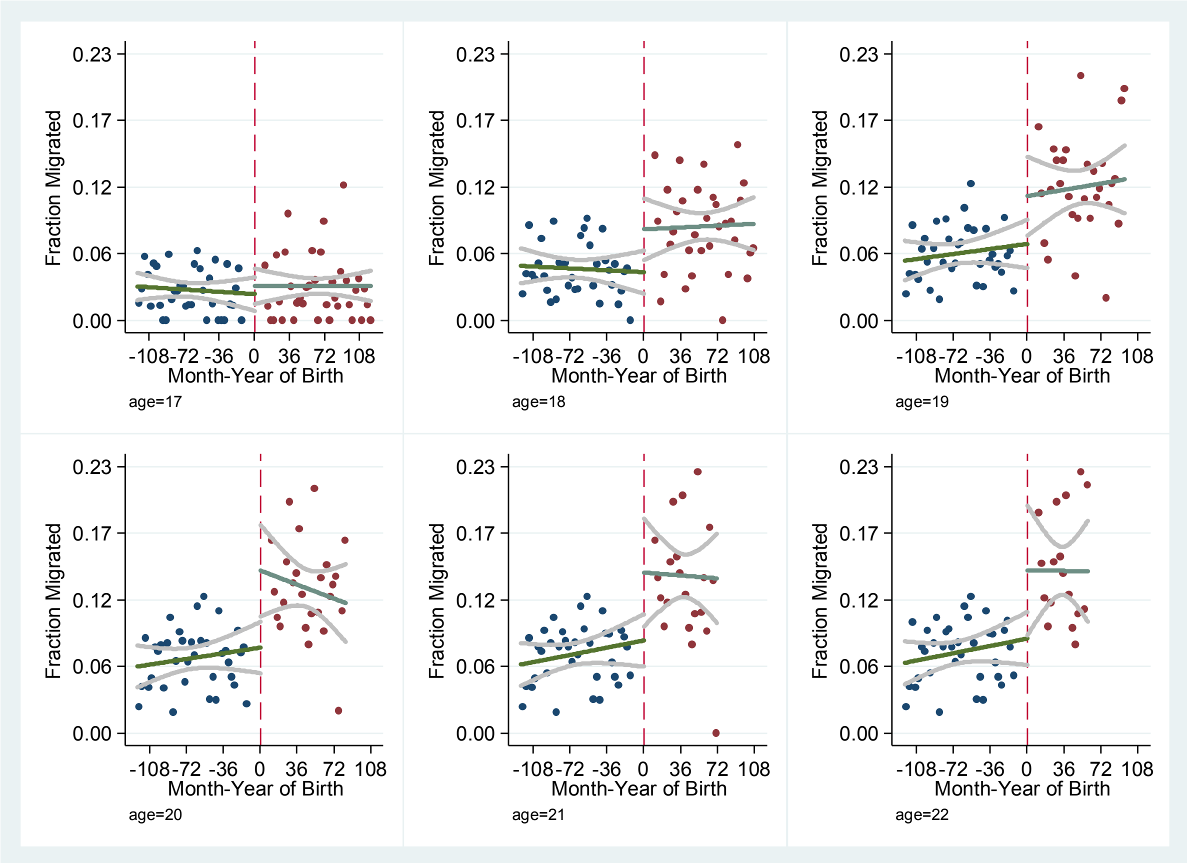


Figure A11: Fraction Migrated for Employment, 10-year intervals with Donut-Hole

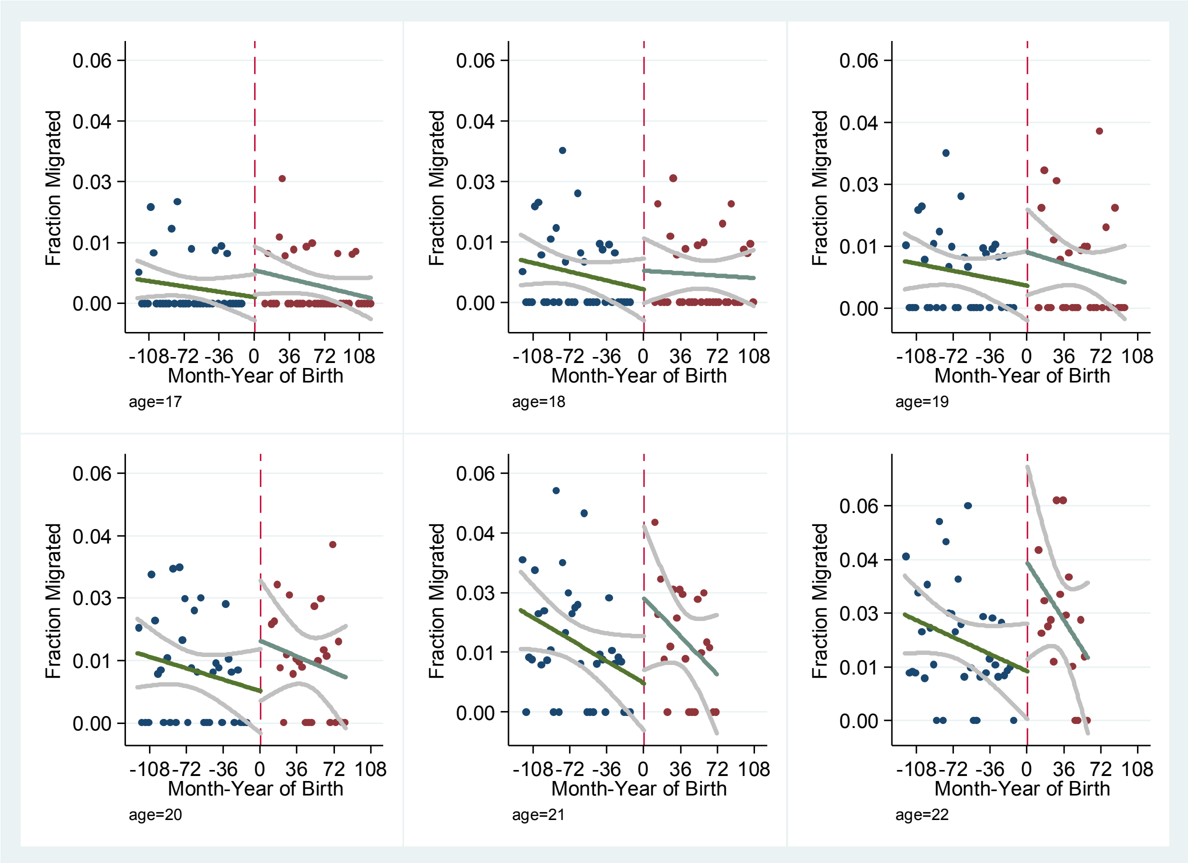


Figure A12: Fraction Migrated for any Reason by Age, 5-year intervals

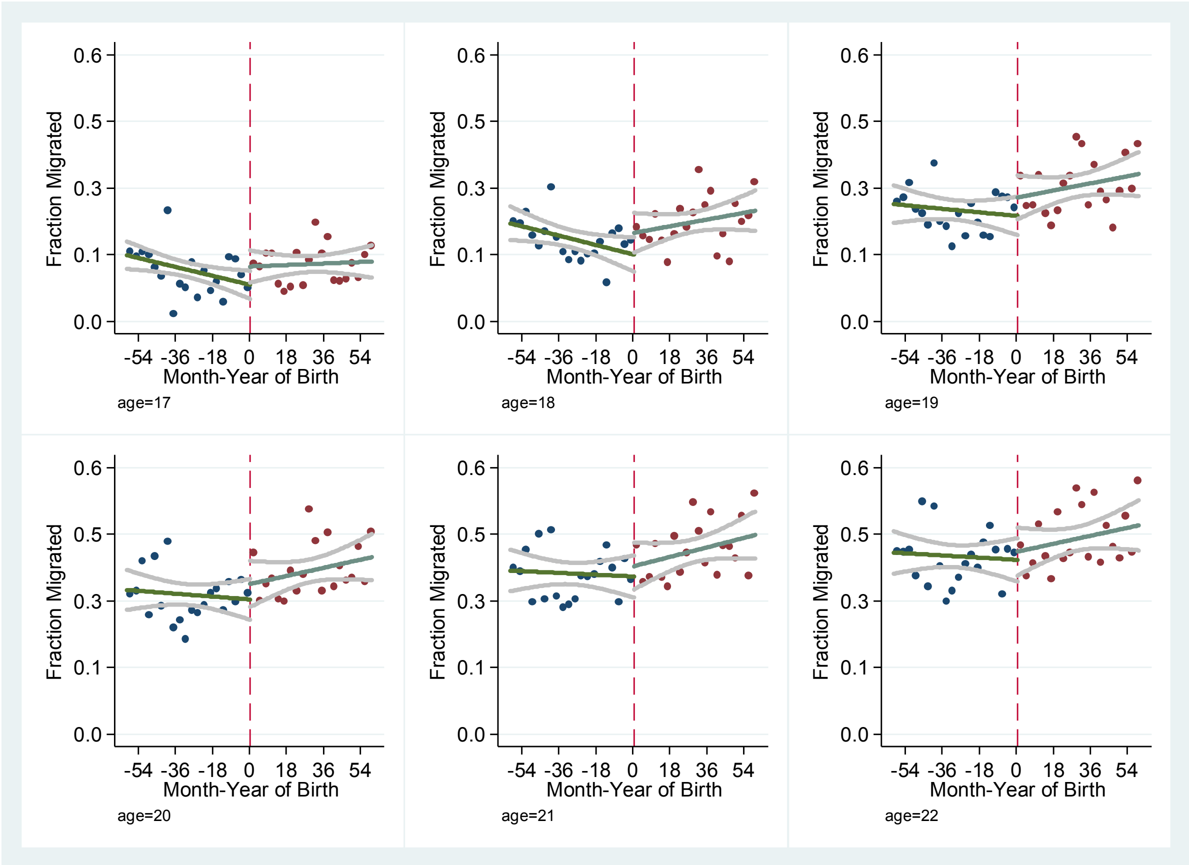


Figure A13: Fraction Migrated for Marriage by Age, 5-year intervals

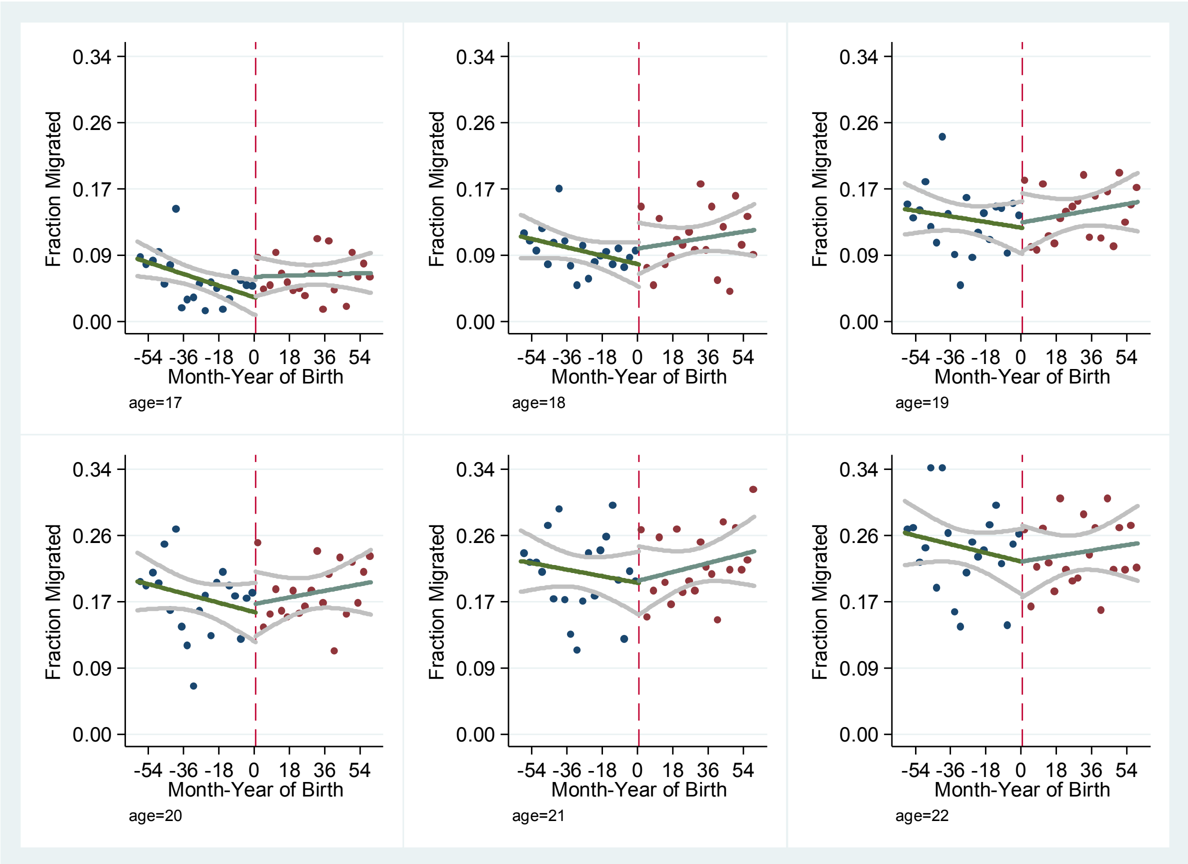


Figure A14: Fraction Migrated for Education by Age, 5-year intervals

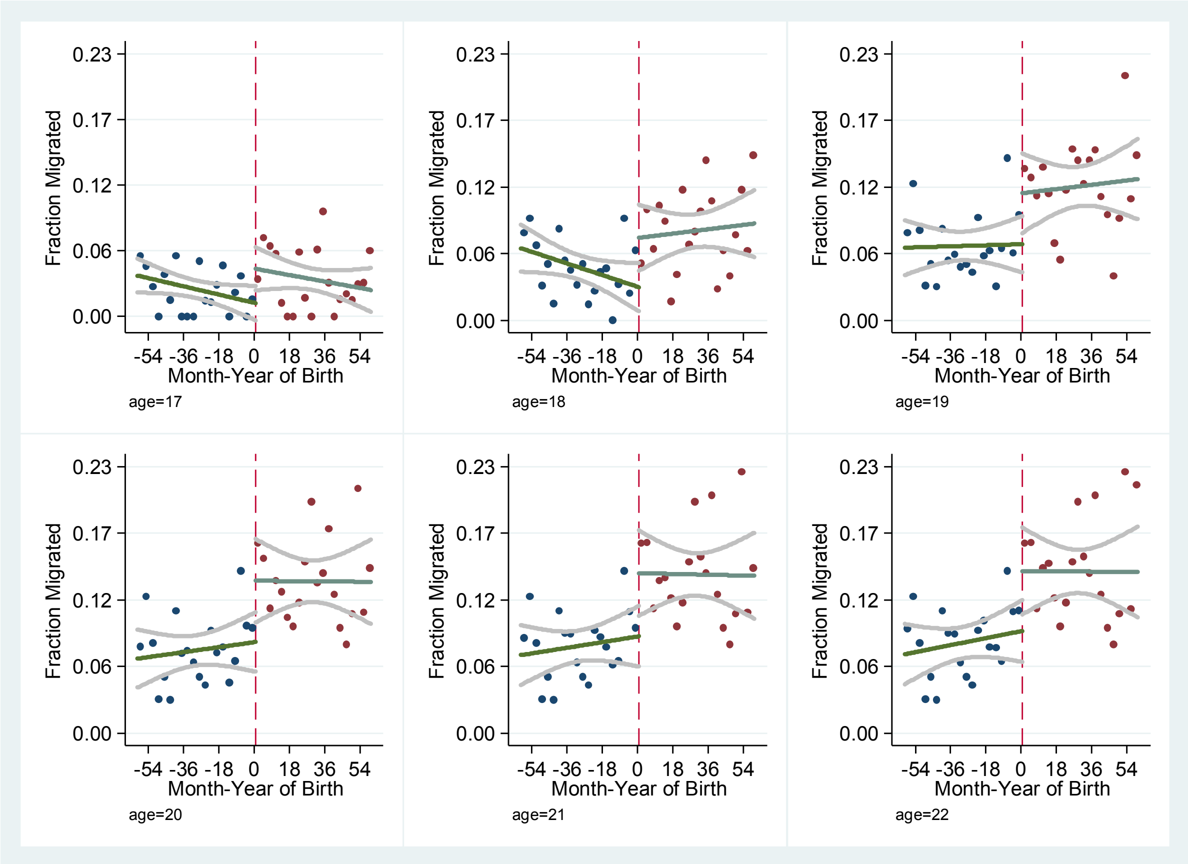


Figure A15: Fraction Migrated for Employment by Age, 5-year intervals

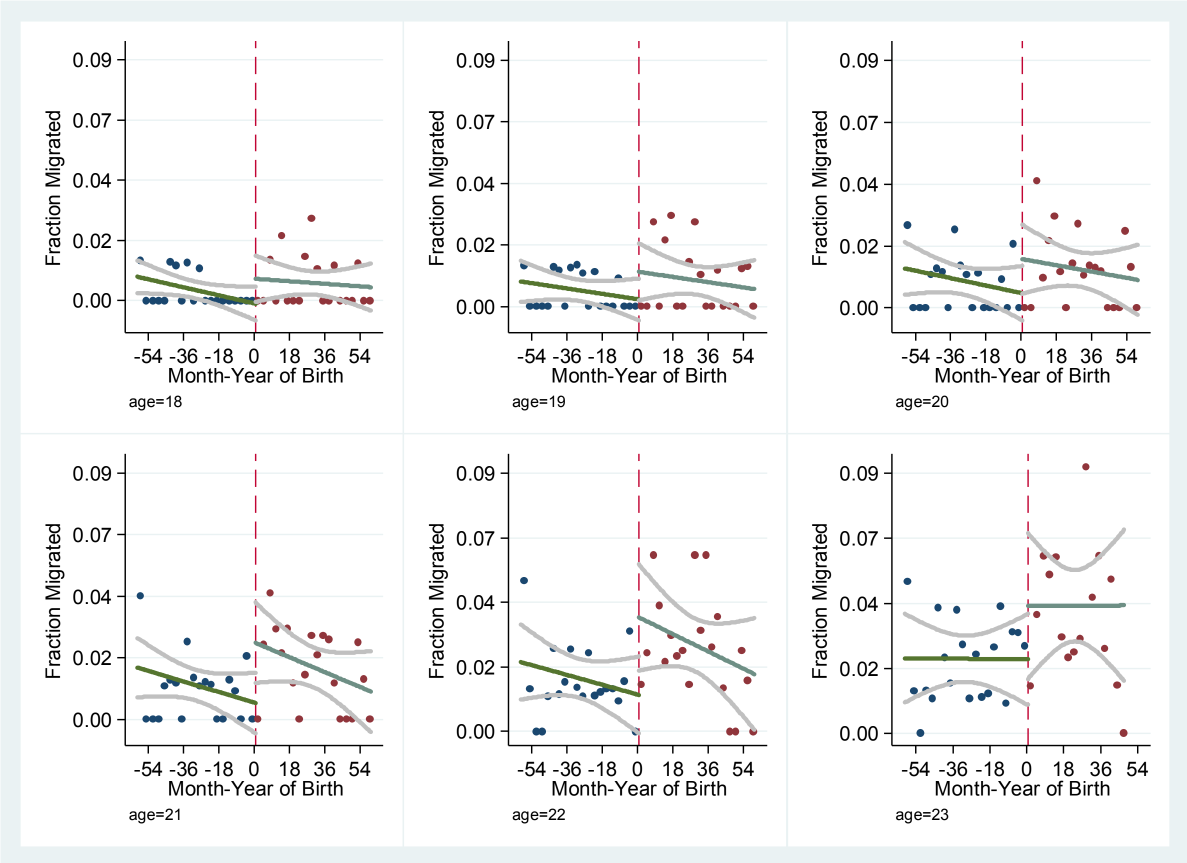


Figure A16: Fraction Migrated for any Reason by Age with Donut-Hole, 5-year intervals

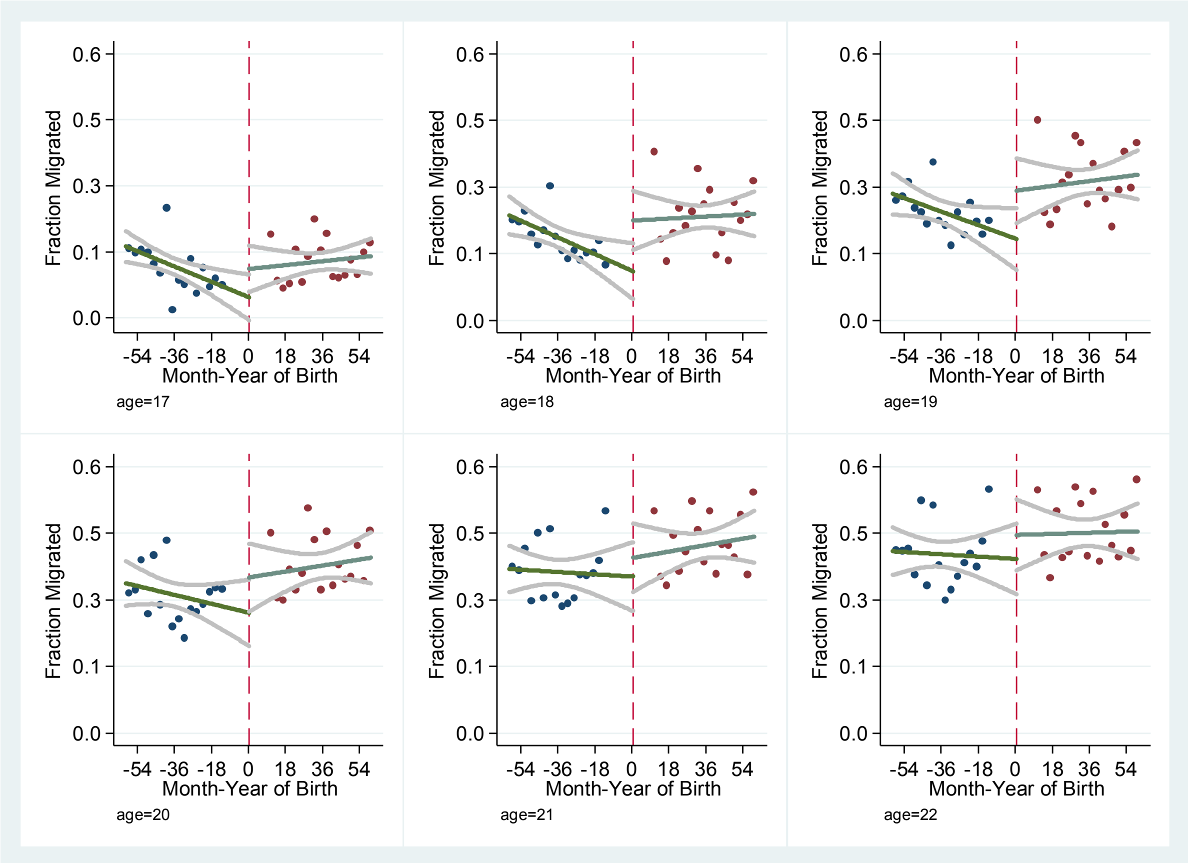


Figure A17: Fraction Migrated for Marriage by Age, 5-year intervals with Donut-Hole

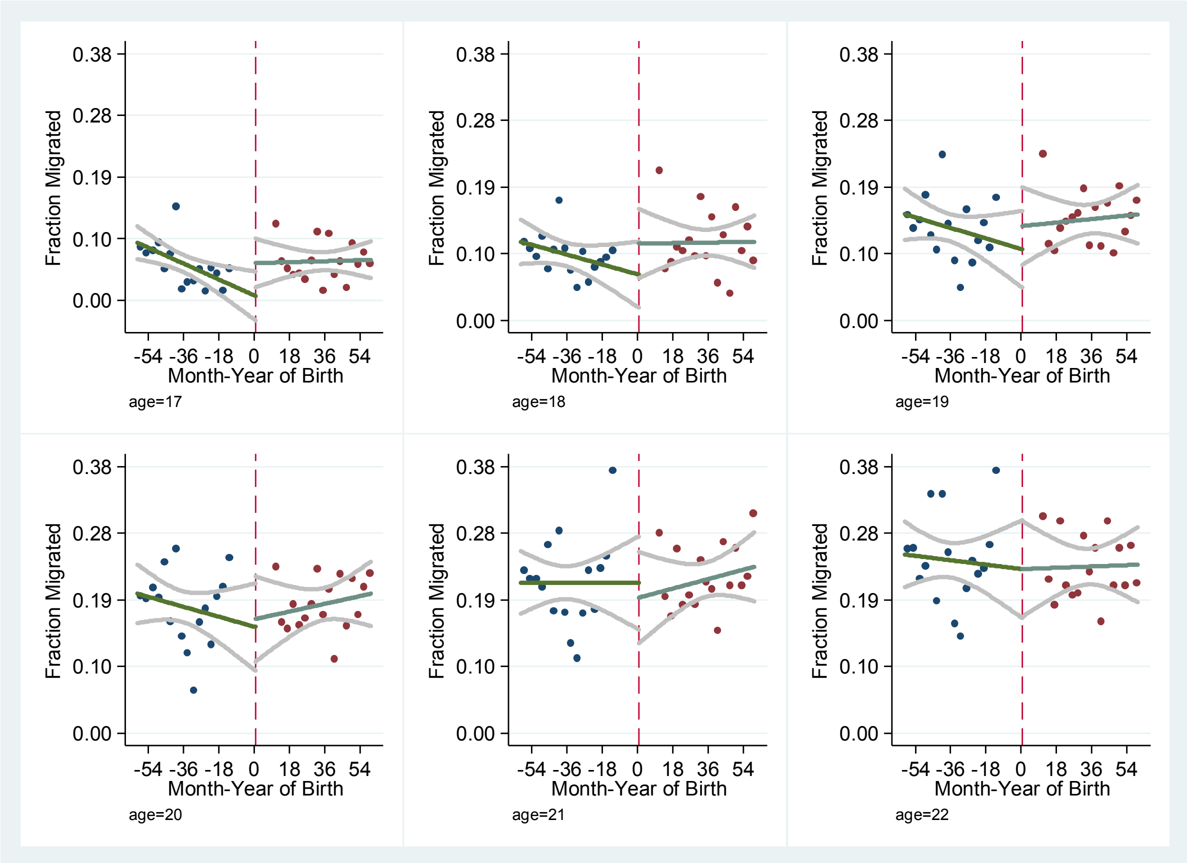


Figure A18: Fraction Migrated for Education by Age, 5-year intervals with Donut-Hole

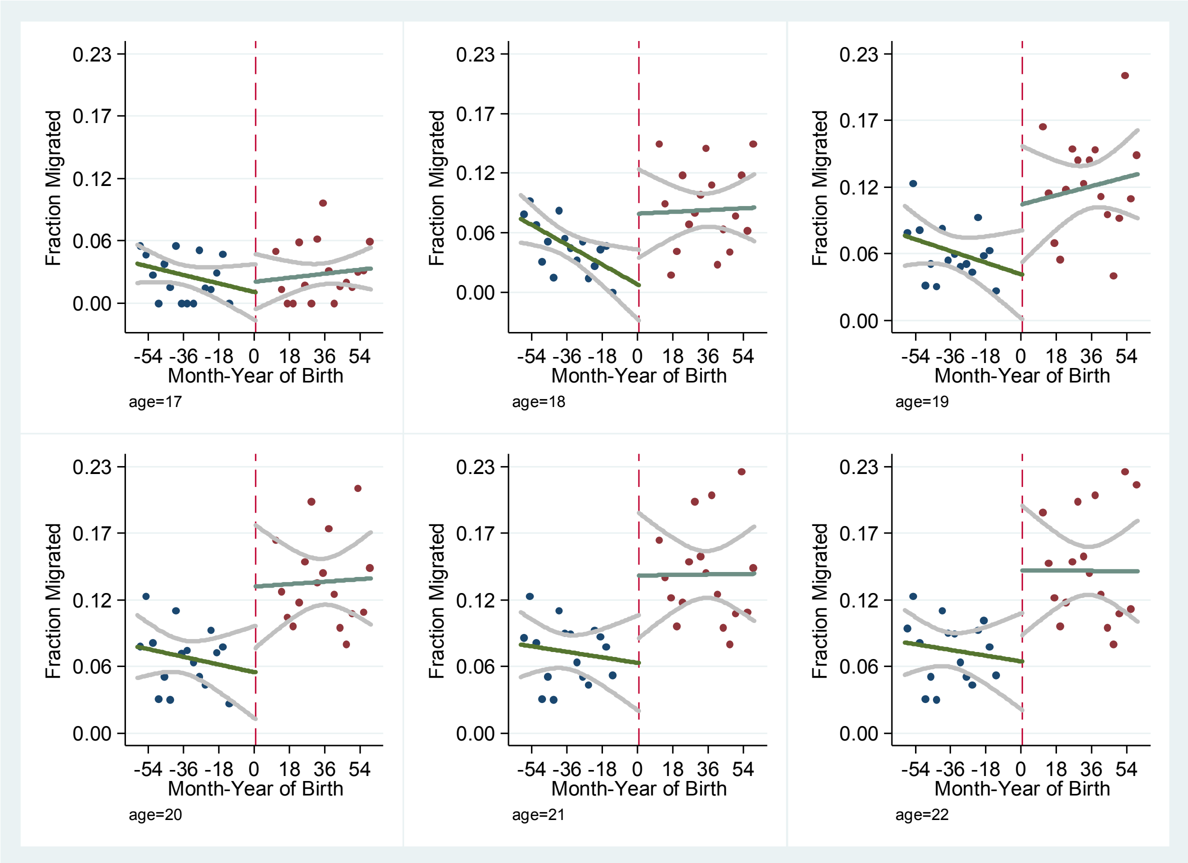


Figure A19: Fraction Migrated for Employment by Age, 5-year intervals with Donut-Hole

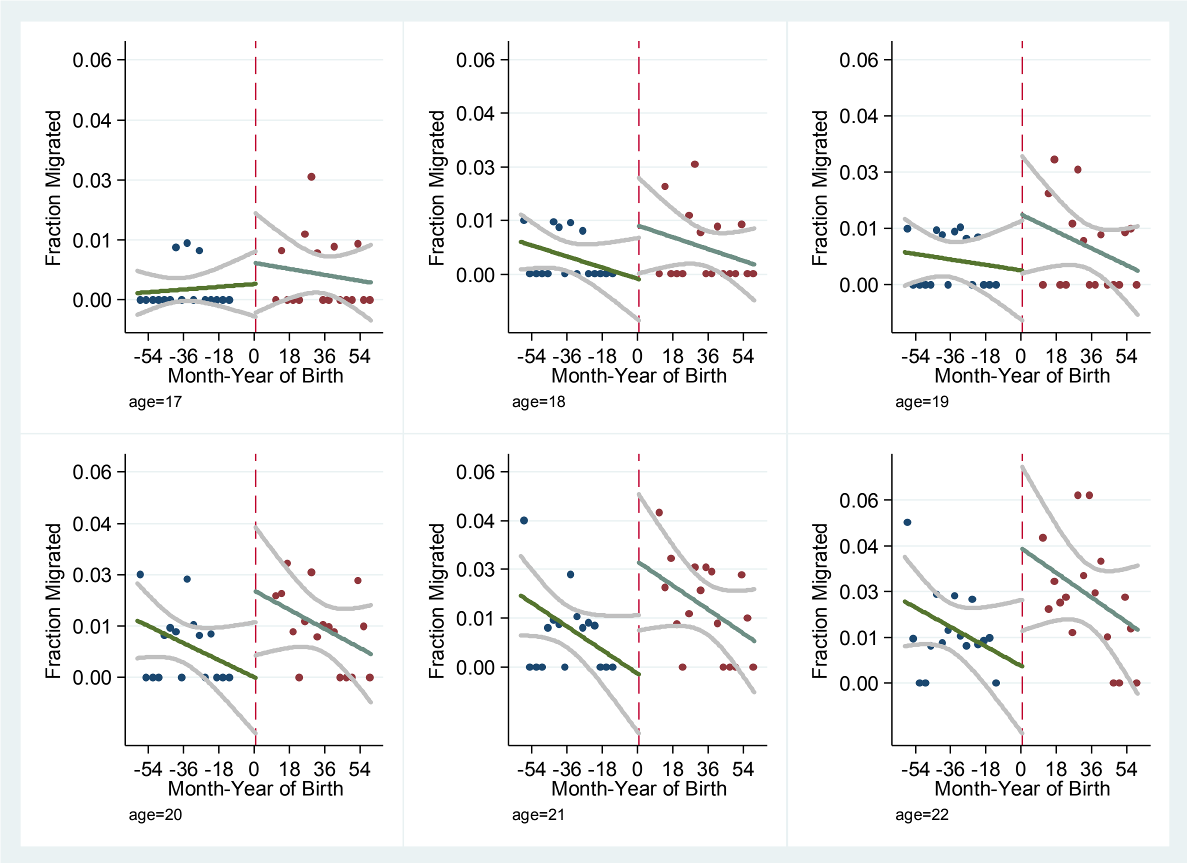


Figure A20 - Policy implementation and other aggregate indicators

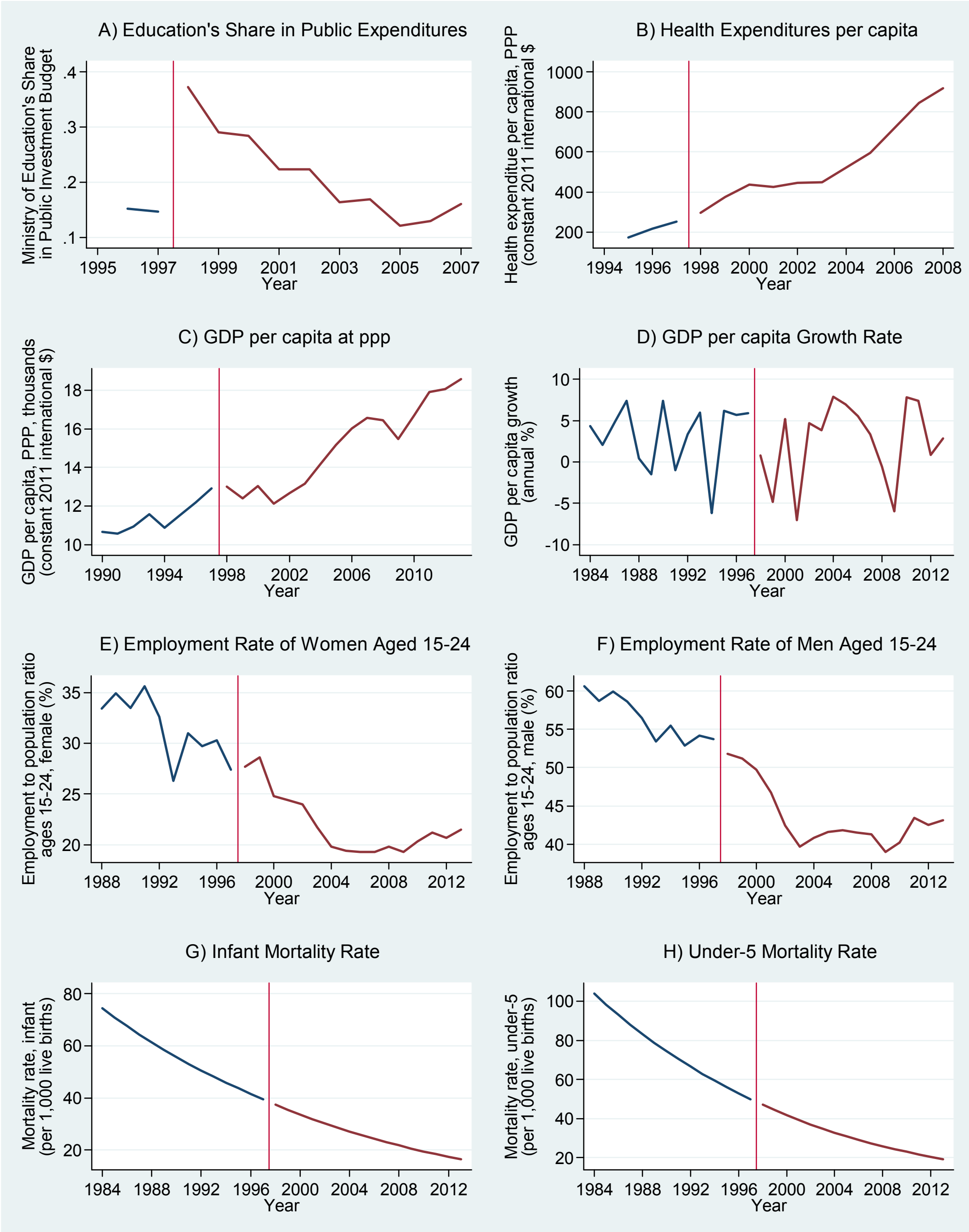
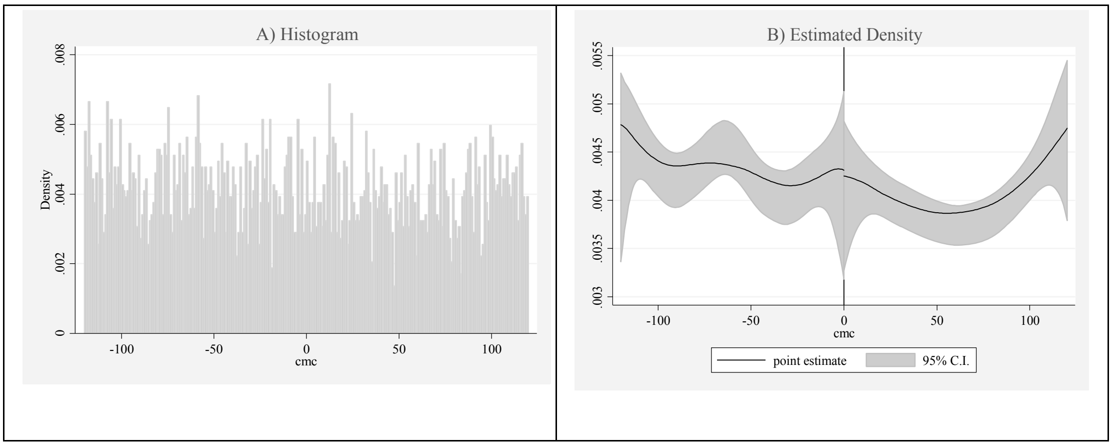


Figure A21: Histogram and Estimated Density of the Running Variable



Supplementary Tables

Table A1: Check of Discontinuity at the Cutoff for Other Covariates

	Age=16		Age=18		Age=20		Age=22		Age=24	
	RD Effect	p-value	RD Effect	p-value	RD Effect	p-value	RD Effect	p-value	RD Effect	p-value
Month of Birth										
January	0.085	0.372	0.087	0.381	0.092	0.406	0.114	0.367	0.145	0.355
February	0.017	0.811	0.017	0.812	0.023	0.766	0.029	0.746	0.051	0.654
March	0.017	0.828	0.014	0.860	0.010	0.915	0.023	0.812	0.040	0.721
April	0.002	0.977	-0.003	0.972	0.008	0.932	0.009	0.930	0.030	0.796
May	0.015	0.843	0.010	0.897	0.022	0.796	0.031	0.756	0.058	0.662
June	0.010	0.879	0.010	0.888	0.012	0.864	-0.008	0.927	-0.005	0.958
July	0.004	0.955	-0.001	0.993	-0.003	0.964	-0.019	0.809	-0.019	0.844
August	-0.042	0.581	-0.048	0.539	-0.056	0.497	-0.052	0.547	-0.067	0.507
September	-0.013	0.858	-0.015	0.839	-0.017	0.824	-0.034	0.677	-0.085	0.372
October	-0.031	0.692	-0.030	0.703	-0.038	0.647	-0.043	0.627	-0.077	0.443
November	-0.022	0.683	-0.019	0.728	-0.029	0.620	-0.039	0.530	-0.049	0.476
December	-0.042	0.516	-0.023	0.723	-0.023	0.740	-0.010	0.897	-0.022	0.793
NUTS-1 Level Region										
Region 1	-0.012	0.351	-0.015	0.266	-0.019	0.198	-0.026	0.103	-0.026	0.162
Region 2	-0.005	0.464	-0.002	0.709	-0.002	0.765	-0.003	0.650	-0.007	0.395
Region 3	0.004	0.574	0.008	0.290	0.008	0.384	0.006	0.547	0.017	0.193
Region 4	0.004	0.597	0.002	0.770	0.000	0.963	-0.002	0.866	-0.007	0.435
Region 5	0.006	0.375	0.003	0.644	0.003	0.656	-0.002	0.816	-0.004	0.604
Region 6	0.006	0.402	0.003	0.722	-0.001	0.860	0.004	0.681	0.009	0.481
Region 7	-0.003	0.772	-0.002	0.862	0.002	0.871	-0.003	0.804	-0.029	0.073
Region 8	-0.007	0.351	-0.008	0.276	0.000	0.990	0.008	0.358	0.011	0.316
Region 9	-0.001	0.905	-0.004	0.694	0.001	0.915	-0.001	0.931	-0.006	0.599
Region 10	-0.005	0.632	-0.006	0.609	0.001	0.935	0.009	0.551	0.012	0.554
Region 11	0.004	0.520	0.006	0.373	0.002	0.792	0.002	0.839	0.015	0.109
Region 12	-0.009	0.494	-0.008	0.559	-0.004	0.802	-0.007	0.650	-0.008	0.696
Type of Location										
Province Center	-0.028	0.266	-0.037	0.142	-0.029	0.291	-0.019	0.517	-0.020	0.529
District Center	0.017	0.457	0.018	0.454	0.030	0.230	0.038	0.179	0.023	0.446
Rural	0.010	0.678	0.019	0.453	-0.001	0.957	-0.019	0.519	-0.003	0.936
Mother Tongue										
Turkish	0.019	0.475	0.018	0.510	0.054	0.089	0.036	0.293	0.033	0.444
Kurdish	-0.017	0.536	-0.015	0.581	-0.048	0.127	-0.033	0.345	-0.028	0.521
Arabic	-0.002	0.837	-0.004	0.617	-0.006	0.492	0.000	0.971	-0.003	0.823
Other	-0.001	0.776	0.001	0.766	0.000	0.935	-0.003	0.453	-0.002	0.632
Father Literate	-0.012	0.426	-0.014	0.342	-0.007	0.676	0.004	0.812	0.002	0.929
Mother Literate	-0.002	0.931	0.004	0.897	0.028	0.347	0.029	0.369	-0.010	0.775

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the 20-year interval around the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts. (b) The dependent variable is given in column (1). The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate OLS regression of the dependent variable on the policy dummy and split linear time trends on each side of the cutoff. The standard errors are clustered at the month-and-year-of-birth level.

Table A2: Continuity-based Analysis for Alternative Cutoffs

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Years Shifted		*-8*	*-6*	*-4*	*-2*	*0*	*+2*	*+4*	*+6*	*+8*
Age=16		0.009	-0.001	-0.003	-0.027	0.011	0.038**	0.009	-0.005	-0.033
		[0.034]	[0.022]	[0.023]	[0.019]	[0.014]	[0.019]	[0.024]	[0.019]	[0.026]
	No obs.	3,035	3,035	3,035	3,035	5,855	2,820	2,820	2,820	2,820
Age=17		0.007	-0.004	-0.022	-0.015	0.071***	0.065**	0.035	0.005	-0.058*
		[0.036]	[0.032]	[0.038]	[0.029]	[0.019]	[0.031]	[0.031]	[0.024]	[0.031]
	No obs.	3,035	3,035	3,035	3,035	5,819	2,784	2,784	2,784	2,784
Age=18		0.035	-0.002	-0.019	-0.040	0.078***	-0.028	0.007	-0.001	-0.035
		[0.035]	[0.032]	[0.033]	[0.031]	[0.024]	[0.041]	[0.039]	[0.039]	[0.065]
	No obs.	3,035	3,035	3,035	3,035	5,503	2,468	2,468	2,468	2,468
Age=19		0.026	-0.029	-0.040	-0.027	0.066***	0.000	-0.028	-0.051	--
		[0.030]	[0.034]	[0.033]	[0.032]	[0.025]	[0.044]	[0.038]	[0.048]	--
	No obs.	3,035	3,035	3,035	3,035	5,206	2,171	2,171	2,171	--
Age=20		0.020	-0.056	-0.017	-0.013	0.077***	0.038	-0.034	-0.064	--
		[0.034]	[0.037]	[0.035]	[0.036]	[0.026]	[0.042]	[0.045]	[0.058]	--
	No obs.	3,035	3,035	3,035	3,035	4,921	1,886	1,886	1,886	--
Age=21		0.051	-0.040	-0.028	-0.007	0.054*	0.001	-0.023	--	--
		[0.036]	[0.039]	[0.039]	[0.036]	[0.029]	[0.051]	[0.051]	--	--
	No obs.	3,035	3,035	3,035	3,035	4,651	1,616	1,616	--	--
Age=22		0.088**	-0.060	-0.016	-0.040	0.041	0.024	-0.139***	--	--
		[0.041]	[0.041]	[0.041]	[0.040]	[0.030]	[0.053]	[0.044]	--	--
	No obs.	3,035	3,035	3,035	3,035	4,389	1,354	1,354	--	--
Age=23		0.102**	-0.062	-0.010	-0.014	0.014	0.011	--	--	--
		[0.043]	[0.039]	[0.038]	[0.034]	[0.030]	[0.057]	--	--	--
	No obs.	3,035	3,035	3,035	3,035	4,128	1,093	--	--	--
Age=24		0.116**	-0.074*	-0.011	-0.022	-0.009	-0.004	--	--	--
		[0.048]	[0.041]	[0.038]	[0.034]	[0.033]	[0.076]	--	--	--
	No obs.	3,035	3,035	3,035	3,035	3,860	825	--	--	--

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the 20-year interval around the cutoff -- 1977 to 1996 birth cohorts in column (5), but to the pre-treatment interval of 1977-1986 in columns (1)-(4) and to the post-treatment interval of 1987-1996 in columns (6)-(9). Note that the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the oldest birth cohort is born in 1993. (b) The dependent variable is ever-migrated status after age 15 at each given age. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987 in column (5). In other columns, the cutoff is shifted according to the number of years givien in the "years shifted" row. For instance, when years shifted is "-2", the cutoff is taken as January 1985. Each cell comes from a separate OLS regression of the dependent variable on the policy dummy and split linear time trends on each side of the cutoff. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A3 – First-Stage Results: Policy Effect on Middle School Degree Attainment, THLFS data

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
A) Age Interval: 15-38						
policy	0.137***	0.111***	0.099***	0.178***	0.143***	0.135***
	[0.023]	[0.019]	[0.023]	[0.025]	[0.026]	[0.028]
Observations	774,693	774,693	774,693	818,352	818,352	818,352
R-squared	0.152	0.154	0.154	0.216	0.217	0.217
F-statistic	35.21	33.99	19.07	50.47	30.26	23.30
Bootstrap p-value	0.000	0.000	0.007	0.000	0.000	0.021
B) Age Interval: 18-35						
policy	0.132***	0.111***	0.099***	0.169***	0.143***	0.135***
	[0.025]	[0.020]	[0.023]	[0.026]	[0.027]	[0.028]
Observations	552,267	552,267	552,267	591,529	591,529	591,529
R-squared	0.125	0.127	0.127	0.164	0.165	0.165
F-statistic	28.66	29.85	19.05	42.30	28.69	22.83
Bootstrap p-value	0.000	0.002	0.006	0.000	0.000	0.027
C) Age Interval:21-32						
policy	0.122***	0.108***	0.099***	0.157***	0.142***	0.135***
	[0.028]	[0.022]	[0.023]	[0.029]	[0.028]	[0.028]
Observations	362,378	362,378	362,378	387,609	387,609	387,609
R-squared	0.098	0.100	0.100	0.123	0.124	0.124
F-statistic	19.14	23.26	18.60	29.91	26.28	22.62
Bootstrap p-value	0.001	0.003	0.002	0.000	0.001	0.020
D) Age Interval: 23-30						
policy	0.109***	0.104***	0.099***	0.145***	0.138***	0.135***
	[0.030]	[0.024]	[0.022]	[0.030]	[0.029]	[0.028]
Observations	244,301	244,301	244,301	258,241	258,241	258,241
R-squared	0.083	0.084	0.084	0.104	0.104	0.104
F-statistic	13.71	18.48	19.39	22.96	23.12	23.37
Bootstrap p-value	0.005	0.005	0.000	0.001	0.004	0.019
Age Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Age Group Dummies*Year Trend		Yes			Yes	
Age Dummies*Year trend			Yes			Yes

Notes: The samples are drawn from 2009-17 THLFS. The sample in each panel is restricted to the age group given in panel headings. The dependent variable is middle school degree which is equal to 1 if individual has completed at least middle school, 0 otherwise. The key variable of interest is policy dummy which is equal to 1 when year of birth is 1987 or later. All specifications include age and year dummies as control variables. The specifications in columns (2) and (5) also include interactions of year trend with dummies for age groups (15-17, 18-20, 21-23, 24-27, 28+). The specifications in columns (3) and (6) also include the interactions of year trend with age dummies. The standard errors are clustered at the year of birth level. Due to the relatively small number of clusters, wild-cluster bootstrap p-values are also provided. Statistical significance is *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table A4: First-Stage Results – Policy Effect on Middle School Completion with TDHS data, Global Approach (10-year intervals on each side of the cutoff)

	(1)	(2)	(3)	(4)	(5)	(6)
Type of Poly.	A) Single			B) Split		
Degree of Poly.	First	Second	Third	First	Second	No. Obs
Age=16	0.158***	0.157***	0.138***	0.157***	0.120***	5,834
	[0.025]	[0.025]	[0.034]	[0.025]	[0.039]	
Age=17	0.158***	0.157***	0.137***	0.157***	0.118***	5,798
	[0.025]	[0.025]	[0.034]	[0.025]	[0.039]	
Age=18	0.157***	0.151***	0.141***	0.152***	0.117***	5,483
	[0.026]	[0.025]	[0.035]	[0.025]	[0.040]	
Age=19	0.157***	0.146***	0.140***	0.147***	0.108***	5,186
	[0.027]	[0.026]	[0.035]	[0.026]	[0.041]	
Age=20	0.155***	0.131***	0.140***	0.135***	0.110**	4,902
	[0.028]	[0.028]	[0.036]	[0.027]	[0.043]	
Age=21	0.157***	0.127***	0.139***	0.132***	0.108**	4,632
	[0.028]	[0.031]	[0.036]	[0.029]	[0.046]	
Age=22	0.157***	0.108***	0.124***	0.116***	0.141***	4,370
	[0.028]	[0.034]	[0.036]	[0.030]	[0.048]	
Age=23	0.154***	0.124***	0.122***	0.124***	0.141***	4,112
	[0.028]	[0.039]	[0.039]	[0.033]	[0.053]	
Age=24	0.146***	0.140***	0.119***	0.138***	0.157***	3,845
	[0.028]	[0.044]	[0.045]	[0.038]	[0.059]	

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the 20-year interval around the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.

(b) The dependent variable is middle-school completion status. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell comes from a separate OLS regression of middle-school completion status at the specified age on the policy dummy and the specified time trends in the running variable as well as other control varibles. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A5 - Effect of Middle School Completion on Ever-Migrated Status for Marriage (TDHS data), Global Approach

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	2SLS						
Type of Poly.		A) Single			B) Split			
Degree of Poly.		First	Second	Third	First	Second	Baseline	No. Obs
Age=16	-0.029***	0.043	0.041	-0.004	0.040	0.001	0.029	5,834
	[0.006]	[0.053]	[0.052]	[0.082]	[0.052]	[0.109]		
Age=17	-0.040***	0.277***	0.276***	0.246*	0.276***	0.266	0.058	5,798
	[0.009]	[0.090]	[0.091]	[0.137]	[0.091]	[0.182]		
Age=18	-0.034***	0.316***	0.323***	0.391**	0.316**	0.425	0.095	5,483
	[0.012]	[0.117]	[0.125]	[0.181]	[0.124]	[0.259]		
Age=19	-0.037**	0.203*	0.181	0.370*	0.178	0.496	0.130	5,186
	[0.014]	[0.122]	[0.140]	[0.190]	[0.137]	[0.325]		
Age=20	-0.053***	0.187	0.239	0.308	0.210	0.351	0.165	4,902
	[0.016]	[0.132]	[0.175]	[0.202]	[0.160]	[0.309]		
Age=21	-0.055***	0.189	0.296	0.318	0.242	0.337	0.195	4,632
	[0.017]	[0.155]	[0.249]	[0.249]	[0.214]	[0.406]		
Age=22	-0.042**	0.090	0.198	0.256	0.075	0.272	0.222	4,370
	[0.018]	[0.157]	[0.294]	[0.271]	[0.246]	[0.295]		
Age=23	-0.034*	-0.141	0.040	0.033	-0.134	0.157	0.247	4,112
	[0.019]	[0.167]	[0.287]	[0.291]	[0.272]	[0.301]		
Age=24	-0.030	-0.198	0.206	0.185	-0.057	0.015	0.265	3,845
	[0.020]	[0.191]	[0.317]	[0.387]	[0.286]	[0.306]		

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to 10-year intervals on each side of the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.
(b) The dependent variable is ever-migrated status (for any reason) since age 15. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell in column (1) comes from a separate OLS regression of ever-migrated status on middle-school completion status and a third-degree polynomial in the running variable in addition to other control variables, whereas each cell in columns (2) to (6) comes from a separate 2SLS regression of ever-migrated status on middle-school completion status -- which is instrumented by the policy dummy -- and the specified time trends in month-year of birth as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A6 - Effect of Middle School Completion on Ever-Migrated Status for Education (TDHS data), Global Approach

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	2SLS						
Type of Poly.		A) Single			B) Split			
Degree of Poly.		First	Second	Third	First	Second	Baseline	No. Obs
Age=16	0.020***	0.054*	0.054*	0.037	0.054*	0.027	0.009	5,834
	[0.004]	[0.030]	[0.030]	[0.044]	[0.030]	[0.057]		
Age=17	0.045***	0.163**	0.165**	0.184*	0.164**	0.236	0.028	5,798
	[0.006]	[0.068]	[0.069]	[0.109]	[0.070]	[0.153]		
Age=18	0.082***	0.275***	0.282***	0.285*	0.278***	0.309	0.059	5,483
	[0.008]	[0.098]	[0.107]	[0.148]	[0.106]	[0.215]		
Age=19	0.114***	0.285***	0.310**	0.432**	0.306**	0.628**	0.084	5,186
	[0.008]	[0.109]	[0.132]	[0.174]	[0.126]	[0.307]		
Age=20	0.131***	0.332***	0.458***	0.501***	0.424***	0.671**	0.092	4,902
	[0.009]	[0.113]	[0.167]	[0.181]	[0.149]	[0.319]		
Age=21	0.141***	0.321***	0.442**	0.483**	0.406**	0.753**	0.096	4,632
	[0.010]	[0.115]	[0.194]	[0.188]	[0.164]	[0.376]		
Age=22	0.148***	0.307***	0.554**	0.508**	0.490**	0.526**	0.095	4,370
	[0.011]	[0.118]	[0.256]	[0.222]	[0.205]	[0.253]		
Age=23	0.153***	0.345***	0.625**	0.634**	0.572***	0.439	0.096	4,112
	[0.011]	[0.117]	[0.253]	[0.255]	[0.217]	[0.272]		
Age=24	0.152***	0.385***	0.465**	0.477*	0.432**	0.635*	0.094	3,845
	[0.011]	[0.125]	[0.216]	[0.275]	[0.196]	[0.334]		

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to 10-year intervals on each side of the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.
(b) The dependent variable is ever-migrated status (for any reason) since age 15. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell in column (1) comes from a separate OLS regression of ever-migrated status on middle-school completion status and a third-degree polynomial in the running variable in addition to other control variables, whereas each cell in columns (2) to (6) comes from a separate 2SLS regression of ever-migrated status on middle-school completion status -- which is instrumented by the policy dummy -- and the specified time trends in month-year of birth as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A7 - Effect of Middle School Completion on Ever-Migrated Status for Employment (TDHS data), Global Approach

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	2SLS						
Type of Poly.		A) Single			B) Split			
Degree of Poly.		First	Second	Third	First	Second	Baseline	No. Obs
Age=16	0.000	-0.002	-0.003	0.015	-0.003	0.022	0.003	5,834
	[0.003]	[0.014]	[0.014]	[0.019]	[0.014]	[0.023]		
Age=17	0.003	0.043*	0.042*	0.055*	0.042*	0.055	0.005	5,798
	[0.003]	[0.024]	[0.024]	[0.033]	[0.024]	[0.039]		
Age=18	0.008*	0.046	0.038	0.120**	0.039	0.150*	0.008	5,483
	[0.004]	[0.032]	[0.035]	[0.052]	[0.035]	[0.078]		
Age=19	0.007	0.072**	0.073*	0.097*	0.074*	0.123	0.010	5,186
	[0.004]	[0.036]	[0.044]	[0.059]	[0.043]	[0.099]		
Age=20	0.015**	0.079	0.100	0.110	0.091	0.064	0.017	4,902
	[0.006]	[0.050]	[0.074]	[0.083]	[0.068]	[0.136]		
Age=21	0.019***	0.114**	0.140	0.129	0.136	0.094	0.022	4,632
	[0.007]	[0.054]	[0.094]	[0.093]	[0.085]	[0.163]		
Age=22	0.023***	0.173***	0.262*	0.209	0.254*	0.029	0.029	4,370
	[0.007]	[0.065]	[0.157]	[0.128]	[0.138]	[0.141]		
Age=23	0.030***	0.197***	0.125	0.127	0.177	0.029	0.037	4,112
	[0.008]	[0.067]	[0.130]	[0.132]	[0.128]	[0.136]		
Age=24	0.035***	0.182*	0.090	0.141	0.219	0.153	0.044	3,845
	[0.009]	[0.093]	[0.167]	[0.218]	[0.172]	[0.177]		

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to 10-year intervals on each side of the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.

(b) The dependent variable is ever-migrated status (for any reason) since age 15. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell in column (1) comes from a separate OLS regression of ever-migrated status on middle-school completion status and a third-degree polynomial in the running variable in addition to other control variables, whereas each cell in columns (2) to (6) comes from a separate 2SLS regression of ever-migrated status on middle-school completion status -- which is instrumented by the policy dummy -- and the specified time trends in month-year of birth as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A8 - Effect of Middle School Completion on Ever-Migrated Status with Parents (TDHS data), Global Approach

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	2SLS						
Type of Poly.		A) Single			B) Split			
Degree of Poly.		First	Second	Third	First	Second	Baseline	No. Obs
Age=16	0.011*	-0.063	-0.069	-0.111	-0.069	-0.150	0.019	5,834
	[0.006]	[0.053]	[0.051]	[0.077]	[0.050]	[0.103]		
Age=17	0.011	-0.030	-0.037	-0.052	-0.036	-0.070	0.025	5,798
	[0.007]	[0.055]	[0.054]	[0.080]	[0.054]	[0.103]		
Age=18	0.007	-0.072	-0.092	-0.111	-0.090	-0.166	0.029	5,483
	[0.008]	[0.060]	[0.063]	[0.090]	[0.062]	[0.129]		
Age=19	0.010	-0.102	-0.150**	-0.139	-0.145**	-0.215	0.034	5,186
	[0.009]	[0.067]	[0.075]	[0.099]	[0.072]	[0.162]		
Age=20	0.014	-0.115	-0.230**	-0.139	-0.216**	-0.180	0.039	4,902
	[0.009]	[0.071]	[0.097]	[0.101]	[0.088]	[0.157]		
Age=21	0.015	-0.113	-0.306**	-0.169	-0.276***	-0.008	0.044	4,632
	[0.010]	[0.076]	[0.122]	[0.108]	[0.105]	[0.148]		
Age=22	0.022**	-0.124	-0.167	-0.142	-0.183	-0.155	0.046	4,370
	[0.010]	[0.085]	[0.155]	[0.139]	[0.129]	[0.167]		
Age=23	0.028**	-0.052	0.079	0.085	0.045	-0.052	0.054	4,112
	[0.011]	[0.089]	[0.127]	[0.129]	[0.115]	[0.170]		
Age=24	0.030**	-0.012	0.036	0.070	-0.018	0.105	0.059	3,845
	[0.012]	[0.101]	[0.143]	[0.202]	[0.152]	[0.148]		

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to 10-year intervals on each side of the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.

(b) The dependent variable is ever-migrated status (for any reason) since age 15. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell in column (1) comes from a separate OLS regression of ever-migrated status on middle-school completion status and a third-degree polynomial in the running variable in addition to other control variables, whereas each cell in columns (2) to (6) comes from a separate 2SLS regression of ever-migrated status on middle-school completion status -- which is instrumented by the policy dummy -- and the specified time trends in month-year of birth as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A9 - Effect of Middle School Completion on Ever-Migrated Status with Spouse (TDHS data), Global Approach

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	2SLS						
Type of Poly.		A) Single			B) Split			
Degree of Poly.		First	Second	Third	First	Second	Baseline	No. Obs
Age=16	-0.002	-0.013	-0.013	-0.027	-0.013	-0.031	0.002	5,834
	[0.002]	[0.018]	[0.017]	[0.022]	[0.017]	[0.028]		
Age=17	-0.004*	-0.035	-0.036	-0.075*	-0.036	-0.103*	0.005	5,798
	[0.002]	[0.025]	[0.025]	[0.042]	[0.025]	[0.058]		
Age=18	-0.006	-0.049	-0.047	-0.051	-0.046	-0.054	0.009	5,483
	[0.004]	[0.041]	[0.042]	[0.061]	[0.042]	[0.079]		
Age=19	-0.009*	-0.031	-0.003	-0.001	-0.009	0.008	0.014	5,186
	[0.005]	[0.047]	[0.052]	[0.073]	[0.052]	[0.113]		
Age=20	-0.008	-0.040	-0.027	-0.058	-0.030	-0.142	0.021	4,902
	[0.006]	[0.054]	[0.066]	[0.077]	[0.064]	[0.137]		
Age=21	-0.019**	-0.093	-0.155	-0.119	-0.152	-0.238	0.033	4,632
	[0.009]	[0.074]	[0.102]	[0.102]	[0.093]	[0.194]		
Age=22	-0.019**	-0.107	-0.132	-0.077	-0.190	-0.167	0.041	4,370
	[0.009]	[0.084]	[0.139]	[0.124]	[0.128]	[0.160]		
Age=23	-0.017*	-0.065	-0.046	-0.053	-0.140	-0.029	0.049	4,112
	[0.010]	[0.091]	[0.149]	[0.150]	[0.145]	[0.185]		
Age=24	-0.011	-0.038	0.121	0.118	0.033	0.162	0.058	3,845
	[0.011]	[0.091]	[0.148]	[0.186]	[0.143]	[0.208]		

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to 10-year intervals on each side of the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts.

(b) The dependent variable is ever-migrated status (for any reason) since age 15. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell in column (1) comes from a separate OLS regression of ever-migrated status on middle-school completion status and a third-degree polynomial in the running variable in addition to other control variables, whereas each cell in columns (2) to (6) comes from a separate 2SLS regression of ever-migrated status on middle-school completion status -- which is instrumented by the policy dummy -- and the specified time trends in month-year of birth as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A10: First-Stage Results – Policy Effect on Middle School Completion by *Type of Place Residence before Age 15* (TDHS data), Global Approach

	Type of Place of Residence before Age 15			
	A) Province or District Center		B) Village	
	2SLS	No Obs.	2SLS	No Obs.
	(1)	(2)	(3)	(4)
Age=16	0.128***	3,666	0.205***	2,168
	[0.030]		[0.042]	
Age=17	0.128***	3,641	0.204***	2,157
	[0.030]		[0.042]	
Age=18	0.122***	3,432	0.192***	2,051
	[0.030]		[0.044]	
Age=19	0.120***	3,211	0.190***	1,975
	[0.031]		[0.045]	
Age=20	0.108***	3,020	0.180***	1,882
	[0.032]		[0.048]	
Age=21	0.105***	2,833	0.189***	1,799
	[0.034]		[0.052]	
Age=22	0.098***	2,668	0.157***	1,702
	[0.036]		[0.055]	
Age=23	0.111***	2,507	0.142**	1,605
	[0.037]		[0.063]	
Age=24	0.117***	2,330	0.145**	1,515
	[0.041]		[0.072]	

Notes: (a) The data come from the 2013 TDHS. Using the migration history of each woman from age 15 to her current age in 2013, the data are put into person-age format. The sample is restricted to the 20-year interval around the cutoff -- 1977 to 1996 birth cohorts. However, since the oldest person in this interval is 17-years old, the right hand side of the interval around the cutoff becomes incrementally shorter with age at higher ages. For instance, at age 20, the sample includes the 1977-1993 birth cohorts. The samples in panel (A) include women who lived in a province or district center before age 15, whereas the samples in panel (B) include women who lived in a village before age 15.

(b) The dependent variable is ever-migrated status (for any reason) since age 15. The running variable is month-year of birth. The policy dummy is one when month-year of birth is greater than January 1987. Each cell in columns (1) and (3) comes from a separate 2SLS regression of ever-migrated status on middle school completion -- which is instrumented by the policy dummy -- and split linear time trends in month-year of birth on either side of the cutoff as well as other control variables. Other control variables include dummy controls for month of birth, type of location of residence in childhoold in terms of size (large city, small city, village) and region (26 NUTS-2 level regions), mother tongue (Kurdish, Arabic, and other), and mother's and father's literacy status. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A11: First-Stage Results – Policy Effect on Degree Attainment by Region (THLFS data), Women aged 15-38

	(1) İstanbul	(2) Ege and Marmara	(3) Central Anatolia	(4) Mediterannean	(5) Black Sea	(6) East and Southeast Anatolia
	Middle School Completion					
Policy	0.066*	0.128***	0.107***	0.058***	0.118***	0.029
	[0.035]	[0.013]	[0.021]	[0.012]	[0.012]	[0.022]
Observations	55,138	119,377	88,340	53,416	52,319	102,629
R-squared	0.190	0.260	0.277	0.214	0.310	0.189
	High School Completion					
Policy	-0.011	0.047***	0.019**	0.012	0.045***	0.017
	[0.010]	[0.010]	[0.009]	[0.021]	[0.006]	[0.015]
Observations	55,138	119,377	88,340	53,416	52,319	102,629
R-squared	0.118	0.125	0.123	0.102	0.128	0.069
Age Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Age Dummies * Year Trend	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The samples are drawn from 2009-17 THLFS. The sample is restricted to the age group 15-38. The dependent variable in panel A is middle school degree which is equal to 1 if individual has completed at least middle school, 0 otherwise. The dependent variable in panel B is high school degree which is equal to 1 if individual has completed at least high school, 0 otherwise.The key variable of interest is policy dummy which is equal to 1 when year of birth is 1987 or later. All specifications include age and year dummies as control variables, aswell as, the interactions of year trend with age dummies. The standard errors are clustered at the year of birth level. Statistical significance is *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.