

Education and emigration intentions: Evidence from a natural experiment in Turkey

Z. Eylem Gevrek*

University of Konstanz

Pinar Kunt

University of Konstanz

Heinrich W. Ursprung

University of Konstanz

Abstract

We exploit the 1997 change in education law in Turkey that raised compulsory schooling from 5 to 8 years to investigate the causal effect of education on emigration intentions. Our instrumental variables estimates indicate that an additional year of schooling increases the probability of reporting the intention to emigrate by 24 percentage points. Moreover, we provide evidence that the identified effect of education on emigration intentions does not operate through financial dissatisfaction or financial hardship but rather through dissatisfaction with a bleak political environment that better educated people are more keenly aware of.

JEL classification: I21, I25, J15

Keywords: Education, migration, political discontent

*Corresponding author. Department of Economics, Box D124, University of Konstanz, 78457 Konstanz, Germany. Phone ++49-7531-88-4183, Fax -4450, Email: zahide.gevrek@uni-konstanz.de.

1 Introduction

Studying the relation between education and migration has a long tradition in development economics (Hanson, 2010). Many development economists fear that a positive correlation between education and emigration may jeopardize the ultimate development strategy, if education efforts are thwarted by a resultant brain drain (Docquier and Rapoport, 2012). The Mediterranean mass migration wave prompted by the Arab spring has now brought the migration issue to the fore of the economic discourse at large. Migration is no longer seen as a mere potential problem for the origin countries but mainly as a potential source of negative externalities in the destination countries (Borjas, 2015; Clemens, 2011; Ehrlich and Kim, 2015). Among these negative externalities, the most salient worry is that many migrants lack the kind of education that helps them to integrate in their new social and political environment.

Although the determinants of international migration have been studied extensively, most studies employ econometric methods that can only establish correlations but not causal effects. To be sure, the root cause of the current displacement of refugees from Afghanistan, Iraq and Syria, are clearly the respective armed conflicts; sophisticated econometric methods of establishing causality are not required (Hatton and Williamson, 2006). When it comes to assessing the determinants of emigration from developing countries that do *not* face violent conflicts, establishing causality is however a serious empirical challenge. In the case of education as a potential determinant of migration, it is, for example, quite likely that some unobserved behavioral factors (risk attitudes, time preferences, and family background spring to mind) influence both education and migration behavior. A correlation between education and migration may therefore only reflect that these two variables are co-determined implying that the actual contribution of education *per se* on migration attitudes is nil. Technically speaking, education might be endogenous with respect to migration behavior.

In this study we use a unique survey data set and exploit a natural experiment to

identify the causal effect of education on the intention to emigrate from Turkey. Focusing on the intention to emigrate instead of *actual* emigration has the advantage of providing information about the pool of potential emigrants; the disadvantage is, of course, that the intention to emigrate is only a necessary but not a sufficient precondition for actual emigration. Creighton (2013) and van Dalen and Henkens (2013) find, however, that the desire to emigrate strongly correlates with future actual migration behavior. Other recent economic studies that investigate the determinants of emigration intentions include Ivlevs (2014), Cai et al. (2014), Dustmann and Okatenko (2014), Manchin and Orazbayev (2015), and Sirkeci and Esipova (2013).

The natural experiment that we exploit for our study is the 1997 education policy reform that increased compulsory schooling in Turkey from five to eight years. The respective law was passed unexpectedly in August 1997 and enacted very quickly in fall 1997, presumably in an attempt to favorably influence Turkey's impending bid to become recognized as a candidate for full membership in the EU. Exposed to the reform were children who had not yet completed the fifth grade in summer 1997. Academically more advanced children were not affected. To accommodate the dramatic reform-induced increase in the number of primary school students, new schools were built, new classrooms were added to existing schools, new primary school teachers were hired, and transportation was made available to children with remote domiciles. We use an individual's exposure to the reform as an instrumental variable for education. The exposure of an individual to the reform is determined both by his age in 1997 and by the reform's intensity in his region of birth. Taking advantage of the fact that government increased the number of primary school classrooms at a differential rate across the regions of Turkey, we define the reform's intensity as the ratio of the number of primary school classrooms to the primary school aged population in the region of birth when the individual turned 11. This identification strategy that makes use of reform intensity is reminiscent of Duflo (2001). Dincer et al. (2014) and Gunes (2015) also employ closely related identification strategies in the context of the Turkish compulsory school law of 1997 to investigate the effect of

education on a wide range of outcomes including fertility, women's empowerment, child health and mortality.

Our estimates suggest that that an additional year of schooling increases the probability that a person reports an intention to emigrate by 24 percentage points. As compared to estimates of other nonpecuniary effects of education (Banks and Mazzonna, 2012; Oreopoulos and Salvanes, 2011), this is a very sizable effect. In an attempt to shed light on the mechanism underlying the identified relationship between education and emigration intention, we also estimated the effect of education on being satisfied with one's health and financial status, and on being dissatisfied with the prevailing political situation in Turkey. We find no statistically significant effect of education on the respondents' satisfaction with their health and financial status. We do however find a substantively and statistically significant effect of education on dissatisfaction with the political situation. These results indicate that the education-induced intention to emigrate from Turkey does not operate through financial dissatisfaction or financial hardship; the education-induced intention to emigrate more likely operates through dissatisfaction with a bleak political environment that better educated people are more keenly aware of.

We believe that our paper is the first study that examines the causal effect of education on the intention to emigrate. The literature on the causal relationship between education and migration is scant and has so far focused on within-country migration. Exploiting the 1959 Norwegian school reform, which increased compulsory education from 7 to 9 years, Machin et al. (2012) find that one additional year of schooling causes an increase in the annual within-country mobility rate of 15 percent. Since the reform-induced increase from 7 to 9 years of compulsory schooling is modest for nowadays Western European standards, the estimates probably capture the effect on individuals in the lowest tier of the educational distribution. A higher tier of the distribution is explored by Malamud and Wozniak (2012) who estimate the causal effect of college education on interstate migration in the United States. Their study exploits variation in college attendance induced by the avoidance behavior of men who faced the highest risk of being drafted in the Vietnam

War years. They find that additional years of college attendance significantly increase interstate mobility.

The remainder of this paper is organized as follows. Section 2 provides background information on the 1997 education reform in Turkey. Section 3 describes the survey data. Section 4 introduces the econometric model used in the empirical analysis. Section 5 presents the results. Section 6 explores the possible mechanisms behind the relationship between education and the intention to emigrate and Section 7 concludes.

2 Education Reform

In August 1997, the Turkish government extended compulsory schooling from 5 to 8 years for all children who had not yet completed fifth grade in spring 1997. The reform involved a substantial investment in school infrastructure. Between 1997 and 2003, 63,127 new classrooms were constructed at the primary school level (grade 1-8). According to data published by the Turkish Statistical Institute this amounts to a 19 percent increase in the number of primary school classrooms as compared to the 1996 level, or an additional 7 new classrooms per 1000 primary school-aged children (ages 6-13). Figures 1 and 2 convey an impression of the enormous expansion in school infrastructure induced by the school reform. Parallel to the expansion in infrastructure, the government recruited new primary school teachers, provided transportation for children with remote domiciles, and provided free textbooks and school meals for children from low-income families. The share of public investments realized by the Ministry of National Education measured against the entire government investment budget more or less doubled in the post reform years (Turkish Statistical Institute, 2006). The annual total cost of the reform (investment and running expenses) was estimated to exceed three billion US dollars during the 1997-2004 period (Dulger, 2004; OECD, 2007).

The investment in school infrastructure varied however considerably across regions

and sub-regions.¹ The government was likely to devote more resources to regions with low pre-reform primary school enrollment rates to decrease the inequality in educational outcomes. As a consequence, the cross regional gap in primary school enrollment rate decreased between 1997 and 2000.²

The education reform had a large effect on primary school enrollment. The number of students in grades 1-8 increased rapidly from 9.1 million in 1997 to 10.5 million in 2000 (Turkish Statistical Institute, 2006), reflecting the high pre-reform drop-out rate in the then non-compulsory school grades 6 to 8. Figure 3 shows the trend in the gross primary school enrollment rate in grades 6-8 between 1990 and 2003.³ In the pre-reform period, the enrollment rate lingered around 65% and started to rise sharply after the implementation of the reform. By the year 2003 the gross primary school enrollment rate in the grades 6-8 reached almost 100% percent.

There is thus every indication that the government succeeded in accomplishing the most important objective of the reform, to wit, expanding primary school access. Despite, or perhaps because of the very substantial efforts in expanding primary school access, the government did not manage to implement any significant quality reforms; in particular, the 1968 national curriculum was maintained with only very minor adjustments (Dulger, 2004; Gunes, 2015).⁴ Gunes (2015) explicitly claims that the reform did not seriously aim at improving the quality of education; the additional classrooms were built to accommodate

¹Turkey is divided in 12 main regions and 26 sub-regions. In our empirical analysis, the geographic variables are measured at the sub-regional level. We merged however 6 small sub regions with neighboring larger sub-regions to arrive at a minimum of 50 observations per sub-region. Our 20 sub-regions are Istanbul, West Marmara, Izmir, Aydin, Manisa, East Marmara, Ankara, Konya, Antalya, Adana, Hatay, Central Anatolia, Zonguldak and Kastamonu, Samsun, East Black Sea, Northeast Anatolia, Malatya, Van, Gaziantep, Sanliurfa and Mardin.

²For example, during 1997-2000, the enrollment rate in the sub-region with the lowest pre-reform enrollment rate (i.e., Van) increased by 37 percent, while the enrollment rate in the sub-region with the highest pre-reform enrollment rate (i.e., Ankara) increased by 7 percent. The difference between the enrollment rates of the two sub-regions declined from 20 percentage points to 7 percentage points during the same period.

³The gross primary school enrollment rate in grades 6-8 is defined as the ratio of the number of students enrolled in grades 6-8 to the population of children aged 11-13.

⁴In the 2004 Implementation Completion Report (ICR) of the World Bank, the Basic Education Program is, for example, evaluated as "unsatisfactory" in meeting the objective of "training of teachers, principals and inspectors".

increased enrollment and not to reduce the class size. Supporting this claim, Figure 5 shows that the ratio of the number of primary school classrooms to the number of primary school students did not increase after the reform; this ratio rather decreased in the first reform year, presumably because the construction of new classrooms could not keep up with the increase in primary school children, and later on returned to the pre-reform level.

3 Data

Our study relies on KONDA Barometer survey data. KONDA, one of the largest research and consultancy firms in Turkey, conducts representative opinion polls concerning political and social matters on a monthly basis. KONDA has, for example, a strong track record in accurately predicting election outcomes, and their predictions receive significant attention in national and international media.⁵ Each survey includes a standard set of core questions on age, gender, education, province of birth, province of residence, ethnic origin, religion, employment status, and household income.⁶ The surveys from which our data are taken (April 2013 and January 2014) contain an additional module of questions on life satisfaction.

We make use of the following survey question to construct our dependent variable that measures the respondents' intention to migrate: *To what extent do you agree or disagree with the following statement: "Even if I had the opportunity to move permanently to another country, I would prefer to continue living in Turkey."* The possible answers range from 1 (strongly disagree) to 5 (strongly agree). We transform the respondents' answers into a dummy variable that equals one if the respondent either strongly disagrees or disagrees, and zero otherwise.

We explore three channels, all associated with some dimension of subjective well-being, through which education could conceivably affect the expressed emigration intentions:

⁵For more information on KONDA, please visit the following webpage: <http://www.konda.com.tr/en/>

⁶The surveys typically interview 1800 to 3600 individuals aged 18 or older. Interviews are conducted face-to-face in the respondents' homes.

the respondent's satisfaction with their health, financial circumstances, and the actual political situation. The following survey questions allow us to measure the respective levels of satisfaction: "To what extent do you agree or disagree with the following statements: (i) In general, I am satisfied with my health; (ii) I am satisfied with my financial situation; (iii) There are positive developments in Turkish politics: the political situation in Turkey is improving". The possible answers range again from 1 (strongly disagree) to 5 (strongly agree). Our indicator variables "health satisfaction" and "financial satisfaction" take the value of one if the respondent's level of agreement with the respective statement is either "strongly agree" or "agree", and zero otherwise. The indicator variable "political dissatisfaction" takes the value of one if the respondent's level of disagreement with the statement (iii) above is either "strongly disagree" or "disagree", and zero otherwise.

In another survey question, the respondents are asked whether they were able to make ends meet with their income last month. This question relates, of course, also to the financial dimension of subjective well-being and can therefore be used as an alternative measure for financial satisfaction. The possible answers to that question are as follows: (i) Yes, I could even save some money, (ii) I could barely manage to make ends meet last month, (iii) I could not make ends meet last month, (iv) I could not pay all the bills and borrowed some money. Our binary variable "financial comfort" takes the value of one if the respondent's answer is (i) and zero otherwise.

Using the survey information on the province of birth, we construct a region of birth variable that assigns each individual's birthplace to one of the 20 sub-regions of Turkey. Matching the survey responses with Population Census and National Education Statistics data (Formal Education: 1991-2003), we arrive at our set of data that contains the number of primary school classrooms, primary school enrollment, and number of children of primary school age in the respondent's region of birth over the 1991-2003 period.⁷ For inter-census years, the number of primary school aged children in each region is imputed using the information from the Population Censuses of the years 1985, 1990, 2000, and

⁷Children of primary school age are 6-13 years old.

2007.⁸

We restrict our analysis to individuals born between 1980 and 1992. As the compulsory schooling law required students enrolled in the fifth grade in Fall 1997 to stay in school until the completion of the 8th grade, individuals born between 1987 and 1992 were affected by the law and therefore constitute the treatment group, while individuals born between 1980 and 1985 were exempted from the mandate and therefore constitute the control group. The justification for our delineation of the treatment and the control group is provided in Section 4.

Table A.2 in the Appendix reports summary statistics for our entire sample and by treatment status. In our entire sample, the proportion of individuals with at least eight years of schooling amounts to 77 percent. In the treatment group, about 90 percent have at least eight years of schooling, whereas only about 65 percent have a primary school diploma in the control group. Individuals with emigration intentions make up 29 percent of the entire sample. The proportion of individuals who expressed an intention to emigrate is almost 50 percent higher among those who were affected by the reform as compared to those who were not affected (23,6 percent versus 34,8 percent). The entire sample is almost equally divided between males and females. Females are slightly underrepresented in the treatment group. Our data set is unique in the sense that it includes information on the respondents' religious affiliation as well as their ethnic background. Consistent with the fact that in Turkey the vast majority of the population is Sunni Muslim, Alevi Muslims and members of other religious affiliations constitute only 5 percent of the sample.⁹ Kurds are the largest ethnic minority in Turkey. In our sample, 17 percent of individuals have Kurdish ethnicity.¹⁰ This figure squares well with the estimate of the CIA World Factbook, indicating that 18% of the Turkish population is Kurdish.

⁸National Education Statistics and Population Censuses are provided by the Turkish Statistical Institute.

⁹In the survey, the respondents are asked to report their religious affiliation as one of the following: Sunni Muslim, Alevi Muslim, and Other.

¹⁰The question on ethnicity lists the following possible responses: Turkish, Kurdish, Arabic, and Other.

4 Identification Strategy

Estimating the causal effect of education on emigration intentions is not an easy task because unobserved factors affecting both education and emigration intentions may produce an omitted variable bias. Individuals who have relatives abroad may, for example receive remittances that relax their liquidity constraint and thereby encourage investment in human capital. Since individuals who have social networks abroad are more likely to have the intention to emigrate than those without such connections, social ties to emigrants may influence both education and the intention to emigrate (Manchin and Orazbayev, 2015). Risk aversion may serve as a second example: potential emigrants tend to be less risk-averse than people without the intention to emigrate, and risk aversion also appears to be correlated with the level of education (Outreville, 2015).¹¹ Unfortunately, our data set does not contain information on social networks, individuals' risk attitudes, etc.

We exploit the change in the compulsory schooling law in Turkey in 1997 as a source of exogenous variation in education to identify the causal effect of education on emigration intentions. Following the identification strategy employed by Duflo (2001), we use an individual's exposure to the reform as an instrumental variable for education. The exposure of an individual to the education reform depends both on his age in 1997 when the law was enacted and on the reform's intensity in his region of birth.¹² Students who had not yet completed fifth grade in summer 1997 were affected by the education reform. As children normally start the first grade at the age of 6 in Turkey, individuals younger than 11 in 1997 (i.e., born after 1986) constitute the treatment group, while those older than 11 in 1997 (i.e., born before 1986) constitute the control group.

¹¹Using data from the German Socio-Economic Panel, Jaeger et al. (2010) examine the relationship between migration and risk attitudes. They find that less risk averse individuals are more likely to migrate internally.

¹²Duflo (2001) capitalizes on a primary school construction program in Indonesia to investigate the causal effect of education on wages. Exploiting regional differences in program intensity measured by the number of new schools constructed per primary school aged child, Duflo (2001) uses the interaction between the year of birth and the program intensity as an instrumental variable for education. A number of studies use similar identification strategies to estimate the causal effect of education on a range of outcomes, including fertility, infant and child health, and women's empowerment (Chou et al., 2010; Dincer et al., 2014; Gunes, 2015; Osili and Long, 2008).

We exclude from our analysis people who were born in 1986 because it is unclear which members of the 1986 cohort were actually exposed to the reform. According to the law that regulates primary school enrollment, children who are 72 months old by the end of the calendar year should start primary school in September.¹³ However, the age requirement for primary school enrollment is not strictly enforced. Since parents have the right to delay their child’s primary school entry for one year because of maturity concerns, some individuals who were born in 1986 might have been enrolled in the first grade in September 1993 rather than September 1992 although they satisfied the age requirement to start primary school in September 1992. These individuals finished only fourth grade in summer 1997 and were therefore subject to the reform, whereas most of their contemporaries were not.¹⁴

Taking advantage of the fact that the number of newly built primary school classrooms varied across regions and years, we measure each individual’s exposure to the reform (the reform intensity) with the ratio of the number of primary school classrooms (i.e., classrooms for grades 1-8) to the number of primary school aged children (i.e., children aged between 6 and 13) in the individual’s region of birth at the time when he or she turned 11.¹⁵ The reform intensity variable allows us to exploit within-region variation in the number of primary school classrooms per child across cohorts.¹⁶

To address the potential endogeneity of education, we employ the method of instrumental variables. The first-stage regression is specified as follows:

¹³This law was published in Official Gazette of the Republic of Turkey (No: 21308) on 7 August 1992.

¹⁴We form the treatment and control groups under the assumption that students started the first grade at the age of six and did not experience grade retention until the fifth grade. It is quite possible that some individuals who were at the fourth grade in summer 1996 may have repeated this grade and ended up being at the fourth grade in summer 1997 and thus were exposed to the reform. Given the fact that we incorrectly assign those individuals to the control group, grade retention potentially leads to a downward bias in the estimated effect of education.

¹⁵Using the same education reform as a source of exogenous variation in education, Dincer et al. (2014) and Gunes (2015) also exploit geographic differences in the intensity of the reform to investigate the causal effects of education on several outcomes. Dincer et al. (2014) use the variation in the number of primary school teachers across regions to capture the intensity of reform, while Gunes (2015) uses the variation in the number of primary school classrooms across provinces.

¹⁶To be more specific, consider two individuals who were born in the same region in 1987 and 1988 respectively. Person A, born in 1987, is matched with the 1998 value of the reform-intensity measure in his region of birth, while person B, born in 1988, is matched with the respective 1999 value.

$$\begin{aligned}
Educ_{ijt} = & \varphi + \alpha_j + \gamma_t + \beta_1(CSL_i * Classroom_{j\tau}) + \beta_2 Classroom_{j\tau} + \beta_3(R_{j1996} * \gamma_t) \\
& + \delta X_{ijt} + \mu S_{2013} + \varepsilon_{ijt}
\end{aligned} \tag{1}$$

where $Educ_{ijt}$ is a binary variable indicating whether the individual i has at least eight years of schooling and α_j represents region-of-birth fixed effects that control for time-invariant region-specific unobserved factors affecting schooling. We include year-of-birth fixed effects, denoted by γ_t , to account for changes in social policy and economic conditions. The compulsory schooling law (CSL) indicator variable takes the value of 1 if the individual was born between 1987 and 1992 and it takes the value of zero if the individual was born between 1980 and 1985. $Classroom_{j\tau}$ denotes the ratio of the number of primary school classrooms (for grades 1-8) to the number of children aged between 6 and 13 in the region of birth (hereafter referred to as classroom-child ratio). We take the classroom-child ratio in the year in which the individual enters middle school (i.e., $\tau = t + 11$) to exploit variations in the reform's intensity across treatment cohorts and regions. Since we do not know where the individual started and completed primary education, the construction of the reform's intensity variable is based on the assumption that primary education was completed in the region of birth.¹⁷

R_{j1996} denotes the gross primary school enrollment rate in the region of birth in 1996.¹⁸ We interact the year-of-birth dummies with the gross primary school enrollment rate in the region of birth in 1996. The rationale behind including these interaction terms is that the number of new classrooms built in a region may be associated with the pre-reform enrollment rate in that region because the government is likely to have allocated more resources to regions where the pre-reform enrollment rate was low. X_{ijt} is a vector of

¹⁷The reform's intensity is measured with an error if the region of birth is different from the region of education. To alleviate this measurement error, we exclude in a robustness test individuals who did not live in the region where they were born at the time of the survey.

¹⁸The gross primary school enrollment rate is the ratio of the number of students enrolled in grades 1-8 to the number of children aged between 6 and 13 in the region of birth.

individual characteristics, including ethnicity, religion, and gender. We also include a survey-year dummy for 2013, denoted by S_{2013} , to control for time effects.

In the first stage (equation 1), we endogenize education using exposure to the reform as an instrument. In the second stage, we use the predicted values of education obtained from the first stage regression to estimate the following second stage regression:

$$Y_{ijt} = \varphi + \alpha_j + \gamma_t + \theta Educ_{ijt} + \pi_1 Classroom_{j\tau} + \pi_2 (R_{j1996} * \gamma_t) + \lambda X_{ijt} + \pi_3 S_{2013} + \nu_{ijt} \quad (2)$$

where Y_{ijt} is a binary variable indicating whether the individual has the intention to emigrate. The identifying assumption requires that our instrument, which is the interaction between the CSL dummy and the reform intensity in the region of birth when the individual turned 11, affects the intention to emigrate only through its impact on education (i.e., the instrument is not correlated with the structural error term, ν_{ijt} , in equation 2). Standard errors are corrected for clustering at the region-of-birth and treatment status.

5 Results

5.1 The effect of the compulsory schooling law on education

Table 1 reports the estimated effect of the compulsory schooling law (CSL) on the probability of having at least eight years of schooling (i.e., the probability of completing at least primary school). We use four specifications to estimate the first-stage regression. The number of covariates steadily increases across columns in Table 1. In all specifications, the coefficient of our instrument has the expected positive sign and is statistically significant at the 1 percent level. Our preferred estimates derive from the fourth specification. They indicate that a larger classroom-child ratio induces more schooling across treatment cohorts:

A one percentage point increase in the classroom-child ratio increases the probability of completing at least eight years of schooling by 9.3 percentage points. Given that the average classroom-child ratio at the national level increased between 1996 and 2003 by 0.7 percentage points, this finding implies that the CSL led to a 6.5 percentage point increase in the probability of having at least eight years of schooling, which corresponds to a 10 percent increase as compared to the pre-reform mean presented in Table A.1. Since the first-stage F-statistic exceeds 10, we do not appear to face a weak instruments problem.¹⁹ Moreover, we find that the probability of completing at least eight years of schooling is 17 percentage points lower for females than for males and 10 percentage points higher for people of other religions than for Sunni Muslims.

To support the credibility of our identification strategy, we run a control experiment in which the CSL dummy in equation 1 captures a placebo treatment. In this experiment, individuals born between 1980 and 1985 are assigned to the treatment group, while those born between 1975 and 1979 constitute the control group; neither the treatment nor the control group was thus exposed to the reform. The rationale behind comparing successive cohorts who were not exposed to the reform is to provide supportive evidence that in the absence of reforms any increase in educational attainment would not have been systematically different across regions. If the educational outcomes were increasing faster in regions in which the government devoted more resources prior to the reform, then we would find a positive and statistically significant coefficient for our instrument in the placebo experiment. The upper panel of Table 2 reports the baseline estimates presented in column 4 of Table 1. The results of the control experiment reported in the lower panel of Table 2 indicate that the estimated coefficient of the instrument is statistically insignificant, suggesting that our results are not driven by the presence of a pre-existing differential trend in educational attainment across regions.

We also changed the specification of the first stage regression (equation 1) by replacing

¹⁹As a rule of thumb, in the case of a single endogenous regressor, the instrument is considered to be weak if the first-stage F-statistic is less than 10 (Staiger and Stock, 1997).

the interaction between the CSL dummy and the intensity of the reform with twelve different interactions that are obtained by using the twelve year-of-birth dummies instead of the CSL dummy. Figure 6 plots the estimated coefficients of these interaction terms (i.e., each point on the solid line shows the coefficient of the interaction between the intensity of the reform and the corresponding year-of-birth dummy). The dashed lines represent 95 percent confidence intervals. Figure 6 indicates that the confidence intervals contain zero for each birth cohort in the control group, justifying the proposition that the control group is not affected by the reform. Moreover, the coefficients of the interaction terms become larger across treatment cohorts and they are jointly significant at the 1 percent significance level.

Given that the coefficients of the interactions terms presented in Figure 6 are statistically insignificant for the control group, we check whether the first-stage results are robust to replacing the CSL dummy in equation 1 with year-of-birth dummies for the treatment group. The resulting specification (equation 3) allows us to estimate the effect of the reform on each treatment cohort separately and to use the interactions between year-of-birth dummies and reform intensity as instruments for education.

$$\begin{aligned}
 Educ_{ijt} = & \varphi + \alpha_j + \gamma_t + \sum_{k=1987}^{1992} \theta_k (Year_{ik} * Classroom_{j\tau}) + \beta_2 Classroom_{j\tau} + \beta_3 (R_{j1996} * \gamma_t) \\
 & + \delta X_{ijt} + \mu S_{2013} + \varepsilon_{ijt}
 \end{aligned} \tag{3}$$

where $Year_{ik}$ takes the value of 1 if the individual i was born in year k and zero otherwise. The resulting F-statistic associated with the instruments remains larger than 10.²⁰

Following Duflo (2001), we examine the effect of the reform on various levels of education. For each level of education, we run separate regressions in which the dependent variable takes the value of one if the individual completed at least "s" levels of education

²⁰Estimation results based on equation 3 are available upon request.

and zero otherwise. The indicator "s" refers to the following four levels of education: elementary education, middle school education, high school education, and college education.²¹ Each point on the solid line in Figure 7 represents the estimated coefficient of the instrument for the respective highest level of education. The dashed lines are 95 percent confidence intervals. Figure 7 shows that the education reform has the largest impact on the probability of completing middle school. It appears to have little or no effect on the other levels of education, suggesting that the reform raised educational attainment in Turkey mainly through increasing middle school completion.

5.2 The effect of the compulsory schooling law on emigration intentions

The first panel of Table 3 shows the ordinary least squares (OLS) estimates of the effect of education on emigration intentions. Estimations are carried out through four specifications. Each specification adds more controls to the set of explanatory variables. Treating education as exogenous, we find that having at least eight years of schooling is associated with a higher probability of reporting an intention to emigrate. The instrumental variables (IV) estimates reported in the third panel of Table 3 suggest that primary school completion has a positive and statistically significant impact on the propensity to report emigration intentions. An additional year of schooling increases the probability of report-

²¹The reference categories for the educational attainments "s" are as follows: illiterate (i.e., those who cannot write and read), less than elementary education (i.e., less than 5 years of schooling), elementary education (i.e., 5 years of schooling), middle school diploma (i.e., 8 years of schooling), high school diploma, and college education.

ing emigration intentions by about 24 percentage points.²² The Wooldridge exogeneity test-statistic is statistically significant, implying that OLS produces inconsistent estimates (Wooldridge, 1995).

Consistent with the IV results, the reduced-form estimates presented in the second panel of Table 3 indicate that the effect of our instrument on the probability of reporting emigration intentions is positive and statistically significant at the 1 percent level, suggesting that an increase in schooling induced by the education reform increases the probability of reporting emigration intentions. The IV results also show that members of ethnic and religious minority groups and females are more likely to express an intention to emigrate, perhaps because members of minority groups are in general less satisfied with their life and this possibly also applies to women who are marginalized in Turkish society.²³

We conduct several robustness tests to buttress the validity and strength of the IV estimate of the effect of education on the probability of reporting emigration intentions. Table 4 shows the results of the robustness tests. Column A replicates the baseline estimates presented in column 4 of Table 3. The baseline specification excludes the 1986 cohort because we argue that it is unclear which members of that cohort were actually exposed to the reform. Including the 1986 cohort and assigning the CSL dummy for this cohort either the value of 0.3 (column B) or 0.5 (column C) does not alter our main

²²There are several possible explanations for why the IV estimate exceeds the OLS estimate. First, unobserved factors that have (i) a negative effect on the intention to migrate and are (ii) positively correlated with education would result in a downward bias in the OLS estimate. The literature on the relationship between subjective well-being and the intention to migrate indicates, for example, that people who are satisfied with their life are less likely to consider emigration (Otrachshenko and Popova, 2014; Graham and Markovitz, 2011; Chindarkar, 2014; Cai et al., 2014), and several studies suggest a positive and statistically significant association between education and self-reported life satisfaction (e.g., Blanchflower and Oswald, 2004; Easterlin, 2001; Ferrer-i-Carbonell, 2005; Graham and Pettinato, 2002). Second, classical measurement error in schooling exerts a downward bias on the OLS estimate. Third, in the presence of heterogeneous treatment effects, the IV estimator may identify the average effect of education for the subpopulation of individuals who changed their educational attainment because of the reform i.e., the IV estimate may well capture the average effect of the additional three years of schooling among those who would not have completed these extra three years of schooling in the absence of the reform. The effect of primary school completion on the probability of reporting emigration intentions is likely to be larger for this subpopulation at the lower end of the education distribution than for the entire population.

²³Among 145 countries, Turkey is stuck at rank 130 in the 2015 WEF Global Gender Gap Report.

findings. In a second step, we use the alternative specification of the first-stage regression given by equation 4. This specification allows us to employ interactions between the intensity of reform and six treatment cohort dummies as instruments for education. The results reported in column D reveals that the estimated coefficient of primary school completion on the intention to migrate remains positive and statistically significant at the 1 percent level when we change the specification of the first-stage regression. We then go on to restrict our sample to individuals who lived in their region of birth at the time of the survey because those individuals are more likely to have completed their primary school education in their birth region, implying that in this subsample the reform intensity variable is less likely to be measured with error. The results reported in column E indicate that the lack of information about the respondents' migration history is not likely to have caused a severe bias. The estimated coefficient of education does not change much and remains statistically significant at the 5 percent level when we exclude those who, at the time of the survey, did not live in their region of birth. Finally, we cluster standard errors at the region-of-birth level (column F) and at the region-of-birth*year-of-birth level (column G). The estimates indicate that the effect of education on the intention to emigrate remains statistically significant at conventional levels.

5.3 Channels through which education could affect emigration intentions

We explore several potential mechanisms that may explain why the intention to emigrate is influenced by the level of education. The literature on the determinants of emigration intentions reveals that not only objective measures, such as income and employment status, are important drivers of emigration intentions, but also survey-elicited subjective measures of well-being. A number of studies find, for example, a negative correlation between life satisfaction and the intention to migrate (Cai et al., 2014; Chindarkar, 2014; Graham and Pettinato, 2002; Hiskey et al., 2014; Otrachshenko and Popova, 2014)

Our data allow us to investigate whether satisfaction (or rather dissatisfaction) in economics affairs, health, and politics are likely to link education with emigration intentions. Economic studies of migration behavior have traditionally focused on financial motives, in particular on differences in expected earnings between the country of origin and the country of destination. We capture this economic dimension by using the variable *financial satisfaction* that we gleaned from the KONDA barometer survey as expounded in Section 3. If education engenders dissatisfaction with the financial situation in the country of origin, perhaps because individuals with a good education expect to be much better able to cash in on their education abroad, this dissatisfaction may well give rise to an intention to emigrate, thereby establishing the sought-after link between education and emigration intention. Our study is indeed the first one that investigates the causal effect of education on financial satisfaction.

A similar argument can be made for a link between education, health, and emigration intentions. Many studies have attempted to establish a causal relationship between education and measures of actual health outcomes by exploiting various instances of increases in compulsory schooling. Most studies find that education is causally linked to health, but not all.²⁴ Another strand of literature points to the importance of health status in shaping emigration intentions (Ivlevs 2015). Van Dalen (2013), for example, finds that self-rated health is positively associated with the propensity to emigrate from the Netherlands. We explore in our Turkish context this mechanism linking education to migration intentions via health status. The health status is captured by the binary variable *health satisfaction*, which is described in Section 3. Apart from our intention to identify potential links between education and migration intentions, this exercise also contributes to the literature on the causal effect of education on health outcomes in Turkey.

The last mechanism that we investigate presumes that in many settings education is related to dissatisfaction with the political situation which, in turn, may constitute a contributing factor to a person's consideration of emigration. The influence of political

²⁴Cutler and Lleras-Muney (2012) provides a review of the literature

institutions on emigration has been analyzed before. Epstein et al. (1999), for example, show how exclusive institutions of political rent-seeking may give rise to an unravelling exodus of possibly the most productive people. Dustmann and Okatenko (2014) use data from the Gallup World Poll and find that satisfaction with local amenities and confidence in the home country's institutions are important predictors of migration intentions. Hiskey et al. (2014) use survey data from 22 Latin American countries and also find strong evidence that both the quality of the democratic system and its ability to fulfill basic governance responsibilities influence emigration behavior. We investigate whether the nexus between education and political dissatisfaction may constitute a possible mechanism linking education to the intention to migrate. Disenchantment with the political situation in Turkey in the years 2013-2014 is measured with our variable *political dissatisfaction*.

Table 5 presents the proportion of respondents expressing financial satisfaction, health satisfaction, and political dissatisfaction. Respondents who expressed an intention to migrate, on average, report lower levels of health and financial satisfaction than respondents who have no emigration intentions, and the proportion of respondents who are dissatisfied with the political situation is higher among the respondents who expressed an intention to migrate than among those who did not. These descriptive statistics provide supportive evidence for the hypothesis that discontent encourages the intention to migrate.

We estimate the effect of education on financial satisfaction, health satisfaction, and political dissatisfaction. The IV results presented in columns 1 and 2 of Table 6 indicate that education does not have a statistically significant impact on how people are satisfied with their health and financial status, suggesting that the effect of education on emigration intentions does not operate through health and financial satisfaction.²⁵ To check the robustness of our result indicating that the impact of education on emigration intentions is unlikely to work through financial discontent, we also estimate the effect of education on financial comfort, which is a binary variable based on the survey question asked re-

²⁵Cesur et al. (2014) exploits the 1997 education reform in Turkey to examine the causal effect of education on several health outcomes. They also find that education has no effect on self-reported health.

spondents whether they were able to make ends meet with their income last month. The results reported in column 3 of Table 6 indicate that education has no impact on the probability of being financially comfortable. Education is thus unlikely to affect emigration intentions through its impact on pecuniary aspects of a person's well-being.

The IV results reported in column 4 of Table 6 show however clearly that education has a positive and statistically significant impact on discontentment with the political situation. An additional year of schooling increases the probability of being dissatisfied with the political situation by about 26 percentage points, providing evidence that education is likely to affect emigration intentions through its impact on dissatisfaction with the political situation.

6 Conclusion

In the economics of migration literature, it is commonly claimed that migrants are favorably self-selected, i.e. on average better educated, than otherwise similar people who do not migrate (e.g. Chiswick, 1999). Even though the empirical evidence has shown that the invoked *ceteris paribus* clause is indeed of great importance (e.g. McKenzie and Rapoport, 2010), the basic proposition of migrants being positively self-selected remains unchallenged. Much less clear is whether the proposed correlation between education and migration reflects a causal relationship or whether migration is driven by some other, perhaps unobserved, characteristics that also drive education.

In this paper, we capitalize on a natural experiment in Turkey to investigate the causal effect of education on the intention to migrate. In 1997, compulsory school attendance was increased in Turkey from five to eight years. Students who had not yet completed the fifth grade in summer 1997 were required to stay in school until the end of the eighth grade while academically more advanced students were exempt from the mandate. To meet this dramatic rise in the primary school enrollment, government made a substantial investment in school infrastructure. Taking advantage of the fact that the reform-induced

increase in the number of primary school classrooms differ across the regions of Turkey, we measure the reform's intensity as the ratio of the number of primary school classrooms to the number of primary school-aged children in the region of birth at age 11 and exploit variation in the reform's intensity across regions and cohorts to construct an instrumental variable for education.

Our instrumental variable estimates indicate that education indeed has a causal influence on emigration intentions. An additional year of schooling increases the probability of expressing the intention to emigrate from Turkey by about 24 percentage points. We also investigate mechanisms that are likely to translate education into emigration intentions: satisfaction with financial circumstances, health status, and the political situation in Turkey. We find that satisfaction with financial circumstances and health status are not causally related to education. Satisfaction, or rather dissatisfaction, with the political situation, however, turns out to be causally related to education. This result provides some evidence supporting the proposition that the mechanism linking education to the intention to emigrate may well be that education evokes a more critical appreciation of the political situation encountered in the source country which then translates into emigration desire.

References

- Banks, James and Fabrizio Mazzonna. 2012. "The Effect of Education on Old Age Cognitive Abilities: Evidence from a Regression Discontinuity Design." *Economic Journal*, 122(560), 418-48.
- Blanchflower, David G. and Andrew J. Oswald. 2004. "Well-Being over Time in Britain and the USA." *Journal of Public Economics*, 88(7-8), 1359-86.
- Borjas, George J. 2015. "Immigration and Globalization: A Review Essay." *Journal of Economic Literature*, 53(4), 961-74.
- Cai, Ruohong; Neli Esipova; Michael Oppenheimer and Shuaizhang Feng. 2014. "International Migration Desires Related to Subjective Well-Being." *IZA Journal of Migration*, 3(1), 1-20.
- Cesur, Resul; Bahadir Dursun, and Naci Mocan. 2014. "The Impact of Education on Health and Health Behavior in a Middle-Income, Low-Education Country." NBER Working Papers 20764.
- Cesur, Resul and Naci Mocan. 2013. "Does Secular Education Impact Religiosity, Electoral Participation and the Propensity to Vote for Islamic Parties? Evidence from an Education Reform in a Muslim Country." NBER Working Papers 19769.
- Chindarkar, Namrata. 2014. "Is Subjective Well-Being of Concern to Potential Migrants from Latin America?" *Social Indicators Research*, 115(1), 159-82.
- Chiswick, Barry. 1999. "Are Immigrants Favorably Self-Selected?." *American Economic Review*, 89(2): 181-185.
- Chou, Shin-Yi; Jin-Tan Liu; Michael Grossman and Ted Joyce. 2010. "Parental Education and Child Health: Evidence from a Natural Experiment in Taiwan." *American Economic Journal: Applied Economics*, 2(1), 33-61.
- Clemens, Michael A. 2011. "Economics and Emigration: Trillion-Dollar Bills on the Sidewalk?" *The Journal of Economic Perspectives*, 25(3), 83-106.

- Creighton, Mathew J. 2013. "The Role of Aspirations in Domestic and International Migration." *The Social Science Journal*, 50(1), 79-88.
- Cutler, David M. and Adriana Lleras-Muney. 2012. "Education and Health: Insights from International Comparisons," NBER Working Papers 17738.
- Dincer, Mehmet Alper; Neeraj Kaushal and Michael Grossman. 2014. "Women's Education: Harbinger of Another Spring? Evidence from a Natural Experiment in Turkey." *World Development*, 64, 243-58.
- Docquier, Frederic and Hillel Rapoport. 2012. "Globalization, Brain Drain, and Development." *Journal of Economic Literature*, 50(3), 681-730.
- Duflo, Esther. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review*, 91(4), 795-813.
- Dulger, Ilhan. 2004. "Case study on Turkey rapid coverage for compulsory education program." World Bank conference "Scaling up poverty reduction: A global learning process", Shanghai (2004).
- Dustmann, Christian and Anna Okatenko. 2014. "Out-Migration, Wealth Constraints, and the Quality of Local Amenities." *Journal of Development Economics*, 110, 52-63.
- Easterlin, Richard A. 2001. "Life Cycle Welfare: Evidence and Conjecture." *Journal of Socio-Economics*, 30(1), 31-61.
- Ehrlich, Isaac and Jinyoung Kim. 2015. "Immigration, Human Capital Formation and Endogenous Economic Growth," NBER Working Papers: 21699.
- Epstein, Gil S.; Arye L. Hillman and Heinrich W. Ursprung. 1999. "The King Never Emigrates." *Review of Development Economics*, 3(2), 107-21.
- Ferrer-i-Carbonell, Ada. 2005. "Income and Well-Being: An Empirical Analysis of the Comparison Income Effect." *Journal of Public Economics*, 89(5-6), 997-1019.
- Graham, Carol and Stefano Pettinato. 2002. "Frustrated Achievers: Winners, Losers and Subjective Well-Being in New Market Economies." *Journal of Development Studies*, 38(4), 100-40.

- Gunes, Pinar Mine. 2015. "The Role of Maternal Education in Child Health: Evidence from a Compulsory Schooling Law." *Economics of Education Review*, 47, 1-16.
- Hanson, Gordon H. 2010. "International Migration and Development." in: Dani Rodrik, M. Rosenzweig (Eds.), *Handbook of Development Economics*, vol. 5, North-Holland, Amsterdam (2010), 4363-4414.
- Hatton, Timothy J. and Jeffrey G. Williamson. 2006. "Refugees, Asylum Seekers, and Policy in Europe," R. J. Langhammer and F. Foders, *Labor Mobility and the World Economy*. Berlin, Heidelberg: Springer Berlin Heidelberg, 249-84.
- Hiskey, Jonathan; Jorge Daniel Montalvo and Diana Orc s. 2014. "Democracy, Governance, and Emigration Intentions in Latin America and the Caribbean." *Studies in Comparative International Development*, 49(1), 89-111.
- Ivlevs, Artjoms. 2014. "Happiness and the Emigration Decision." *IZA World of Labor*, 96.
- Ivlevs, Artjoms. 2015. "Happy moves? Assessing the link between life satisfaction and emigration intentions." *Kyklos*, 68 (3), 335-356.
- David A.; Thomas Dohmen; Armin Falk; David Huffman; Uwe Sunde and Holger Bonin. 2010. "Direct Evidence on Risk Attitudes and Migration." *Review of Economics and Statistics*, 92(3), 684-89.
- Machin, Stephen; Kjell G. Salvanes and Panu Pelkonen. 2012. "Education and Mobility." *Journal of the European Economic Association*, 10(2), 417-50.
- Malamud, Ofer and Abigail Wozniak. 2012. "The Impact of College on Migration: Evidence from the Vietnam Generation." *Journal of Human Resources*, 47(4), 913-50.
- Manchin, Miriam and Sultan Orazbayev. 2015. "Social networks and the intention to migrate." Working paper.
- McKenzie, David and Hillel Rapoport. 2010. "Self-Selection Patterns in Mexico-U.S. Migration: The Role of Migration Networks." *Review of Economics and Statistics*, 92(4), 811-821.
- OECD. 2007. "Reviews of National Policies for Education: Basic Education in Turkey."

- Oreopoulos, Philip and Kjell G. Salvanes. 2011. "Priceless: The Nonpecuniary Benefits of Schooling." *Journal of Economic Perspectives*, 25(1), 159-84.
- Osili, Una Okonkwo and Bridget Terry Long. 2008. "Does Female Schooling Reduce Fertility? Evidence from Nigeria." *Journal of Development Economics*, 87(1), 57-75.
- Otrachshenko, Vladimir and Olga Popova. 2014. "Life (Dis)Satisfaction and the Intention to Migrate: Evidence from Central and Eastern Europe." *Journal of Socio-Economics*, 48, 40-49.
- Outreville, J. Francois. 2015. "The Relationship between Relative Risk Aversion and the Level of Education: A Survey and Implications for the Demand for Life Insurance." *Journal of Economic Surveys*, 29(1), 97-111.
- Sirkeci, Ibrahim and Neli Esipova. 2013. "Turkish Migration in Europe and Desire to Migrate to and from Turkey." *Border Crossing: Transnational Working Papers*, 1301.
- Staiger, Douglas and James H. Stock. 1997. "Instrumental variables regression with weak instruments." *Econometrica.*, 65, 557-586.
- Turkish Statistical Institute. 2006. "National education statistics formal education 2003/04." Ankara: Prime Ministry, Republic of Turkey.
- van Dalen, Hendrik P. and Kene Henkens. 2013. "Explaining Emigration Intentions and Behaviour in the Netherlands, 2005-10." *Population Studies*, 67(2), 225-41.
- WEF. 2015. "Global Gender Gap Report." <https://www.weforum.org/reports/global-gender-gap-report-2015/>
- Wooldridge, Jeffrey M. 1995. "Score diagnostics for linear models estimated by two stage least squares." in : G. S. Maddala, P. C. B. Phillips, and T. N. Srinivasan (Eds.), *Advances in Econometrics and Quantitative Economics: Essays in honor of Professor C. R. Rao*, Oxford: Blackwell, pp 66-87.

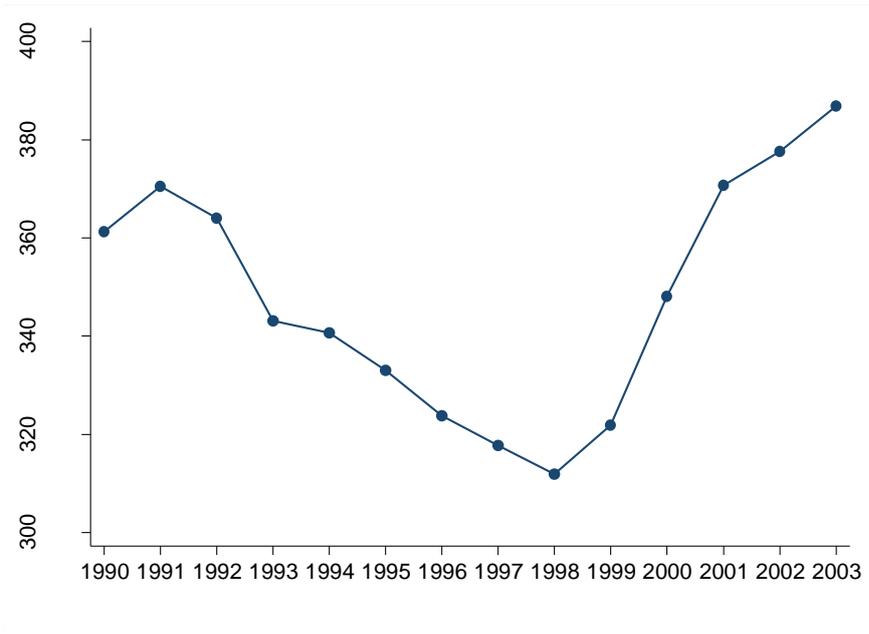


Figure 1. Number of primary school classrooms (grades 1-8)

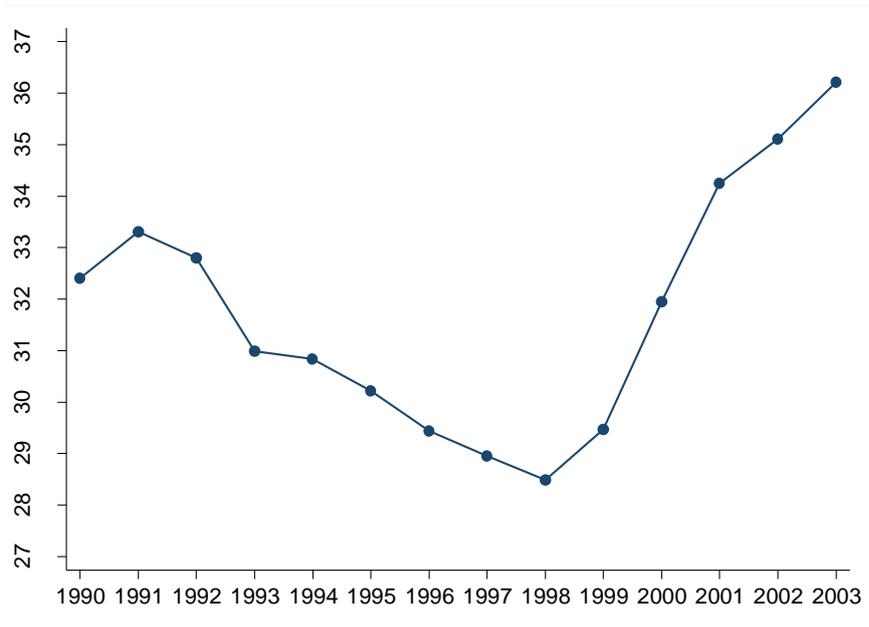


Figure 2. Number of primary school classrooms per 1000 primary school-aged children

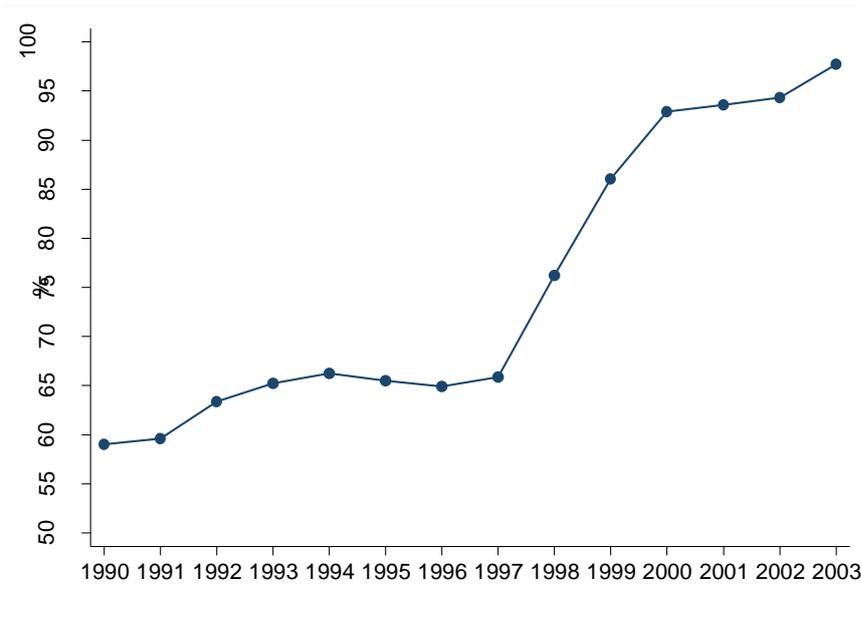


Figure 3. Gross enrollment rate in grades 6-8

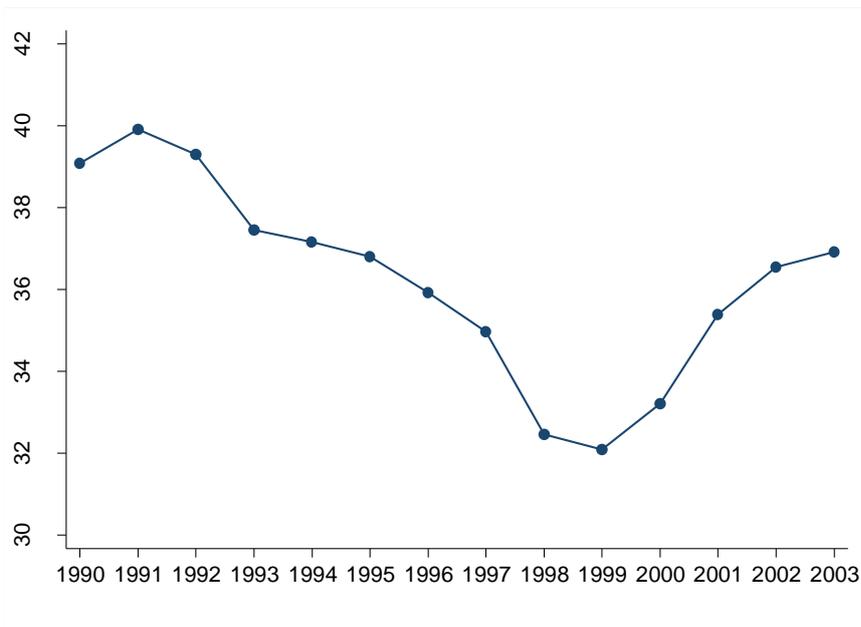


Figure 4. Number of primary school classrooms per 1000 primary school students

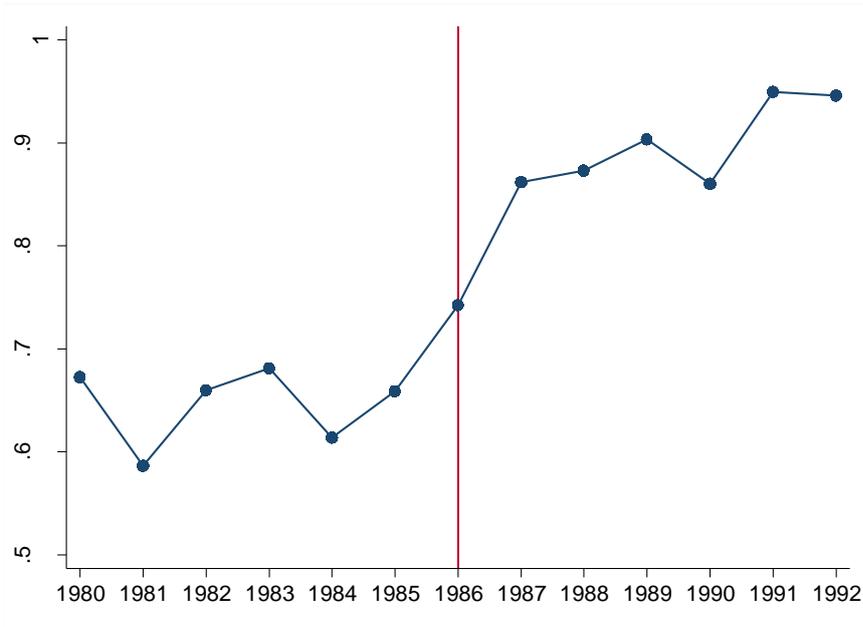


Figure 5. Proportion of individuals with at least eight years of education by birth cohorts

Table 1. Effect of the compulsory schooling law on education

	(1)	(2)	(3)	(4)
CSL* Classroom-child ratio	10.36*** (1.97)	9.67*** (1.73)	9.41*** (1.72)	9.31*** (1.77)
Female		-0.17*** (0.03)	-0.17*** (0.03)	-0.17*** (0.03)
Kurdish			-0.07 (0.05)	-0.07 (0.05)
Other Ethnicity			-0.05 (0.05)	-0.06 (0.05)
Alevi Muslim				0.01 (0.05)
Other Religion				0.10* (0.06)
F-statistic	27.76	31.39	29.81	27.77
Mean of the dependent variable	0.77	0.77	0.77	0.77
Mean of the classroom-child ratio	0.03	0.03	0.03	0.03
N	1,404	1,404	1,404	1,404

Notes: * Significant at 0.1 level. ** Significant at 0.05 level. *** Significant at 0.01 level. The dependent variable is a binary variable indicating whether the individual has at least eight years of schooling. The compulsory schooling law (CSL) indicator variable takes the value of 1 if the individual born between 1987 and 1992 and it takes the value of zero if the individual born between 1980 and 1985. Classroom-child ratio is obtained by dividing the number of primary school classrooms (for grades 1-8) by the number of children aged between 6 and 13 in the region of birth at the time when the individual turned 11. The reference categories for ethnic origin and religion are Turkish and Sunni Muslim, respectively. All the regressions control for year-of-birth fixed effects, region-of-birth fixed effects, Classroom-child ratio, interactions between the year-of-birth dummies and the gross primary school enrollment rate in the region of birth in 1996 as well as the survey year dummy. The F-statistic is associated with the hypothesis that the coefficient of the instrument (the interaction between CSL dummy and Classroom-child ratio) is zero. Standard errors reported in the parenthesis are corrected for clustering at the region-of-birth and treatment status.

Table 2. Effect of the compulsory schooling law on education in the control experiment

Panel A: Baseline Estimates

Treatment: born 1987-92

Control: born 1980-85

N=1407

CSL* Classroom-child ratio	9.31*** (1.77)
----------------------------	-------------------

Panel B: Estimates from Control Experiment:

False Treatment: born 1980-85

Control: born 1975-79

N=1278

CSL* Classroom-child ratio	0.14 (2.55)
----------------------------	----------------

Notes: * Significant at 0.1 level. ** Significant at 0.05 level. *** Significant at 0.01 level.

Panel A shows the estimated coefficient of the instrument (the interaction between CSL dummy and Classroom-child ratio) from the baseline specification presented in column 4 of Table 1. The first-stage regression specification in panel B differs from that in panel A in only one respect: in the latter, the compulsory schooling law (CSL) indicator variable takes the value of 1 if the individual born between 1980 and 1985 and it takes the value of zero if the individual born between 1975 and 1979. Standards errors reported in the parenthesis are corrected for clustering at the region-of-birth and treatment status.



Figure 6. Coefficients of the interactions between year-of-birth dummies and reform intensity

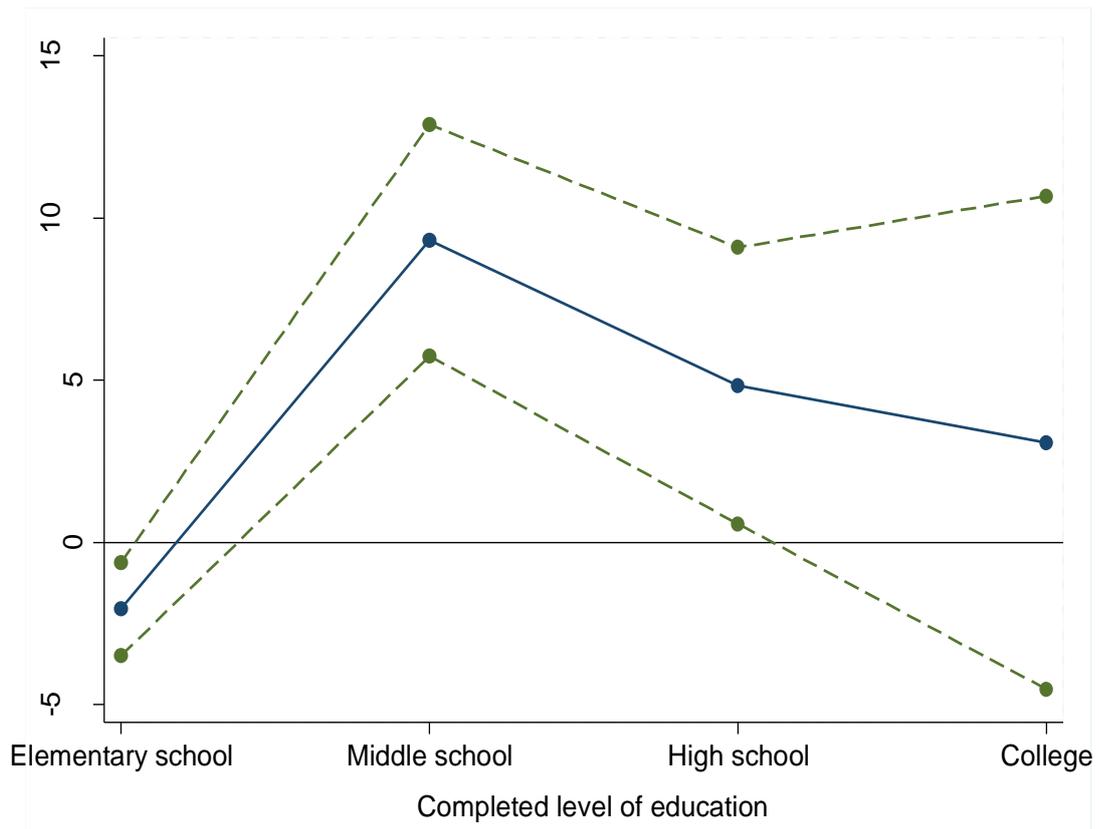


Figure 7. Coefficients of the interactions between CSL dummy and reform intensity

Table 3. Effect of education on the intention to emigrate

	(1)	(2)	(3)	(4)
Panel A: OLS estimates				
Education	0.07** (0.03)	0.06* (0.03)	0.06** (0.03)	0.06** (0.03)
Panel B: Reduced form estimates				
CSL* Classroom-child ratio	6.66*** (2.35)	6.67*** (2.35)	6.87*** (2.35)	6.84*** (2.32)
Panel C: IV estimates				
Education	0.64*** (0.21)	0.68*** (0.22)	0.73*** (0.23)	0.73*** (0.24)
Female		0.09* (0.05)	0.10* (0.05)	0.10* (0.05)
Kurdish			0.11*** (0.04)	0.11*** (0.04)
Other Ethnicity			0.13* (0.08)	0.12 (0.08)
Alevi Muslim				0.15*** (0.05)
Other Religion				-0.07 (0.08)
Wooldridge test statistic	6.34**	6.30**	6.84**	6.99**
N	1,404	1,404	1,404	1,404

Notes: * Significant at 0.1 level. ** Significant at 0.05 level. *** Significant at 0.01 level. The dependent variable is a binary variable indicating whether the individual has the intention to emigrate. The estimations are carried out through four specifications. Column (1) does not include any controls. Column (2) control for gender. Column (3) and Column (4) add ethnic origin and religion, respectively to the set of explanatory variables. Education is a binary variable indicating whether the individual has at least eight years of schooling. The compulsory schooling law (CSL) dummy variable takes the value of 1 if the individual born between 1987 and 1992 and it takes the value of zero if the individual born between 1980 and 1985. Classroom-child ratio is obtained by dividing the number of primary school classrooms (for grades 1-8) by the number of children aged between 6 and 13 in the region of birth at the time when the individual turned 11. The reference categories for ethnic origin and religion are Turkish and Sunni Muslim, respectively. All the regressions control for year-of-birth fixed effects, region-of-birth fixed effects, Classroom-child ratio, interactions between the year-of-birth dummies and the gross primary school enrollment rate in the region of birth in 1996 as well as the survey year dummy. Standard errors reported in the parenthesis are corrected for clustering at the region-of-birth and treatment status.

Table 4. Robustness checks- IV estimates of the effect of education on the intention to emigrate

	A Baseline	B Include 1986 cohort CSL= 0.3	C Include 1986 cohort CSL=0.5	D Alternative first-stage specification	E Exclude internal migrants	F Clustered at region-of -birth	G Clustered at region-of- birth*year-of birth
Education	0.73*** (0.24)	0.76*** (0.26)	0.73*** (0.25)	0.80*** (0.20)	0.79** (0.35)	0.73* (0.40)	0.73** (0.30)
Female	0.10* (0.05)	0.10* (0.06)	0.10* (0.06)	0.11** (0.05)	0.09 (0.07)	0.10 (0.08)	0.10 (0.06)
Alevi	0.15*** (0.05)	0.11** (0.05)	0.12** (0.05)	0.15*** (0.05)	0.08 (0.05)	0.15** (0.07)	0.15*** (0.06)
Other religion	-0.07 (0.08)	-0.01 (0.08)	-0.01 (0.08)	-0.08 (0.08)	-0.08 (0.10)	-0.07 (0.09)	-0.07 (0.08)
Kurdish	0.11*** (0.04)	0.09** (0.04)	0.09** (0.04)	0.11*** (0.04)	0.05 (0.06)	0.11* (0.06)	0.11** (0.04)
Other ethnicity	0.12 (0.08)	0.10 (0.08)	0.10 (0.08)	0.13* (0.08)	0.10 (0.09)	0.12* (0.07)	0.12 (0.10)
Observations	1404	1,540	1,540	1404	1,000	1,404	1,404

Notes: * Significant at 0.1 level. ** Significant at 0.05 level. *** Significant at 0.01 level. The dependent variable is a binary variable indicating whether the individual has the intention to emigrate. Column A shows the baseline estimates presented in column 4 of Table 3. Column B and Column C include the 1986 cohort. CSL dummy takes the value of 0.3 for this cohort in Column B and the value of 0.5 in Column C. Column D uses the first-stage specification presented in equation 3. Column E restricts the sample to individuals who lived in their region of birth at the time of the survey. Standard errors reported in parentheses are clustered at the region-of-birth and treatment status level in Columns A-E, at the region-of-birth level in Column F, and at the region-of-birth and year-of-birth level in Column G. Education is a binary variable indicating whether the individual has at least eight years of schooling. The reference categories for ethnic origin and religion are Turkish and Sunni Muslim, respectively. All the regressions control for year-of-birth fixed effects, region-of-birth fixed effects, Classroom-child ratio, interactions between the year-of-birth dummies and the gross primary school enrollment rate in the region of birth in 1996 as well as the survey year dummy.

Table 5. Mean values of variables by emigration intention status

	Emigration intention=0	Emigration intention=1	t-statistics
Education	0.744	0.832	-3.545
Health satisfaction	0.918	0.890	1.674
Financial satisfaction	0.781	0.626	6.081
Financial comfort	0.759	0.729	1.146
Political dissatisfaction	0.514	0.684	-5.888
Female	0.506	0.459	1.619
Sunni Muslim	0.952	0.927	1.852
Alevi Muslim	0.026	0.051	-2.377
Other Religion	0.022	0.022	0.021
Turkish	0.793	0.773	0.815
Kurdish	0.164	0.173	-0.420
Other Ethnicity	0.043	0.054	-0.843
N	994	410	

Notes: The description of variables is provided in Section 3. Column 2 and Column 3 present the mean values of variables among the respondents who expressed an intention to migrate and among those who did not, respectively. The last column presents t-statistics for two-tailed tests on the difference in mean values of the two samples.

Table 6. Effect of education on health satisfaction, financial satisfaction, financial comfort, and political dissatisfaction

	Health satisfaction	Financial satisfaction	Financial comfort	Political dissatisfaction
OLS estimates				
Education	0.06** (0.02)	0.04 (0.03)	0.04 (0.03)	0.14*** (0.04)
Reduced form estimates				
CSL* Classroom-child ratio	-2.31 (2.29)	-2.80 (2.41)	0.20 (3.00)	7.11*** (2.44)
IV estimates				
Education	-0.24 (0.27)	-0.29 (0.26)	0.02 (0.31)	0.78** (0.32)
Female	-0.05 (0.05)	-0.02 (0.05)	0.00 (0.07)	0.09* (0.06)
Kurdish	-0.11** (0.05)	-0.05 (0.05)	0.00 (0.06)	0.28*** (0.07)
Other Ethnicity	-0.07* (0.04)	-0.09 (0.06)	-0.13 (0.08)	-0.00 (0.10)
Alevi Muslim	-0.03 (0.05)	-0.11** (0.05)	0.04 (0.05)	-0.07 (0.05)
Other Religion	0.06 (0.07)	-0.20** (0.09)	-0.08 (0.06)	-0.11 (0.07)
Wooldridge test statistic	1.69	1.83	0.00	5.40**
Mean of the dependent variable	0.91	0.73	0.75	0.56
N	1,405	1,405	1,393	1,396

Notes: * Significant at 0.1 level. ** Significant at 0.05 level. *** Significant at 0.01 level. The dependent variable is Health satisfaction in column (1), Financial satisfaction in column (2), Financial comfort in column (3), and Political dissatisfaction in column (4). The description of dependent variables is provided in Section 3. Education is a binary variable indicating whether the individual has at least eight years of schooling. The compulsory schooling law (CSL) dummy variable takes the value of 1 if the individual born between 1987 and 1992 and it takes the value of zero if the individual born between 1980 and 1985. Classroom-child ratio is obtained by dividing the number of primary school classrooms (for grades 1-8) by the number of children aged between 6 and 13 in the region of birth at the time when the individual turned 11. The reference categories for ethnic origin and religion are Turkish and Sunni Muslim, respectively. All the regressions control for year-of-birth fixed effects, region-of-birth fixed effects, Classroom-child ratio, interactions between the year-of-birth dummies and the gross primary school enrollment rate in the region of birth in 1996 as well as the survey year dummy. Standard errors reported in the parenthesis are corrected for clustering at the region-of-birth and treatment status.

Appendix A.

Table A.1. Mean values of variables by treatment status

	All	Control	Treatment
Education	0.770	0.646	0.894
Emigration intention	0.292	0.236	0.348
Female	0.492	0.519	0.465
Sunni Muslim	0.944	0.947	0.942
Alevi Muslim	0.033	0.030	0.037
Other Religion	0.022	0.023	0.021
Turkish	0.787	0.804	0.770
Kurdish	0.167	0.151	0.183
Other Ethnicity	0.046	0.046	0.047
N	1404	703	701

Notes: The description of variables is provided in Section 3. Individuals born between 1980 and 1985 constitute the control group while those born between 1987 and 1992 constitute the treatment group.