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Z. Eylem Gevrek, Pinar Kunt, Heinrich W. Ursprung

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Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

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Abstract

We exploit the 1997 school reform that increased compulsory schooling from 5 to 8 years to investigate the causal effect of education on emigration intentions. Our IV 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 but rather through displeasure at a bleak political environment that better educated people are more keenly aware of.

JEL-Codes: I210, I250, I310.

Keywords: education, migration, political discontent.

Z. Eylem Gevrek
Universidade Católica Portuguesa
Católica Porto Business School & CEGE
R. de Diogo Botelho 1327
Portugal – 4169-005 Porto
egevrek@porto.ucp.pt

Pinar Kunt
University of Konstanz
Department of Economics
Germany – 78457 Konstanz
pinar.kunt@uni-konstanz.de

Heinrich W. Ursprung
University of Konstanz
Department of Economics
Box 138
Germany – 78457 Konstanz
heinrich.ursprung@uni-konstanz.de

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1 Introduction

We use the natural experiment of the Turkish 1997 education policy reform that increased compulsory schooling from five to eight years together and a unique survey data set to identify the causal effect of education on the intention to emigrate from Turkey. Focusing on the self-reported 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 (2015), Cai et al. (2014), Dustmann and Okatenko (2014), Manchin and Orazbayev (2015), and Sirkeci and Esipova (2013). Our estimates suggest that that an additional year of schooling increases the probability of reporting 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.

We also estimate the causal effect of education on the respondents' self-reported satisfaction with their health, financial circumstances, and the prevailing political situation in Turkey. We use these three factors because they primarily determine life-satisfaction and many studies find that survey-elicited subjective measures of well-being are negatively correlated with the intention to migrate (Cai, et al., 2014; Chindarkar, 2014; Graham and Pettinato, 2002; Hiskey et al., 2014; Otrachshenko and Popova, 2014). These three factors therefore represent prime candidates for mechanisms that may underlie the identified relationship between education and emigration intentions. It turns out that we find no statistically significant effect of education on the respondents' satisfaction with their health and financial circumstances. We do however find a substantive 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 the respondents' self-rated health status or financial dissatisfaction, 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 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 her age in 1997 and by the reform's intensity in her region of birth. Taking advantage of the fact that the government needed to increase the number of primary school classrooms to accommodate the larger number

of primary school children, but was only able to do so at a differential rate across regions, we define the reform's intensity as the ratio of the number of primary school classrooms and 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.

We believe that our paper is the first study that uses a natural experiment to examine the causal effect of education on the *intention* to emigrate. We acknowledge however that Ivlevs and King (2012) have investigated a similar relationship using parental education as an instrument of own education. They find that in post-independence Kosovo an extra year of education increases the probability of *taking concrete steps to realize emigration intentions* by nine percentage points, a result that is not obscenely at variance with our finding if one takes into account that only a fraction of all expressions of emigration intentions are eventually followed up with concrete plans to actually endeavor emigration.

The literature on the causal relationship between education and migration is scant and has so far focused mainly 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. Section 5 presents the results and robustness tests. Section 6 elaborates on the question which mechanisms are likely to underlie the identified causal effect of education on emigration intentions, and Section 7 concludes.

2 The 1997 Education Reform in Turkey

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. Academically more advanced children were not affected. The law was passed unexpectedly and enacted very quickly in fall 1997 by the newly elected secular coalition government in an attempt to favorably influence Turkey's impending bid to become recognized as a candidate for full membership in the EU (Dulger 2004) and to limit the extent of religious education, in particular to prevent the Imam-Hatip (religious) schools from admitting students at a young age (Cesur and Mocan 2018).¹ To accommodate the dramatic reform-induced increase in the number of school children, new schools were built, new classrooms were added to existing schools, new primary school teachers were hired, transportation was made available to children with remote domiciles, and free textbooks and school meals were provided for children from low-income families.

The reform thus involved substantial funds that needed to be spread across time. Between 1997 and 2003, 63,127 new classrooms were constructed for primary school children (grade 1-8). According to data published by the Turkish Statistical Institute this amounted 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). Figure 1.1 conveys an impression of the enormous expansion in school infrastructure induced by the reform. Public investments realized by the Ministry of National Education as a share of 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).

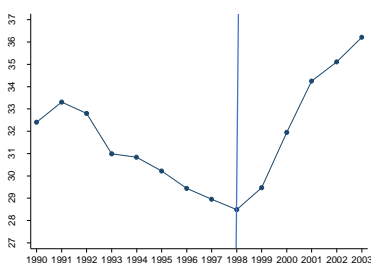


Figure 1.1 Number of primary school classrooms per 1000 children of primary school age

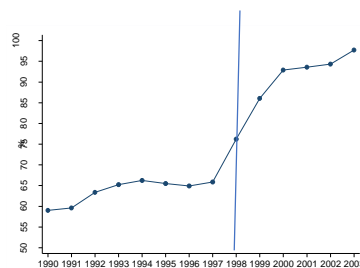


Figure 1.2 Gross enrollment rate in grades 6-8

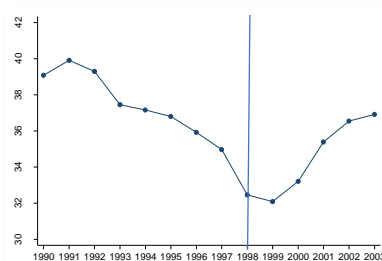


Figure 1.3 Number of primary school classrooms per 1000 children actually attending primary school

¹ See Cakir et al. (2004) and Cesur and Mocan (2013) for a detailed discussion of the political developments leading to the legislation of the 1997 education reform.

The investment in school infrastructure varied not only across time but also across regions and sub-regions.² 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 2000.³

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 1.2 shows the trend in gross primary school enrollment rates in grades 6-8 between 1990 and 2003.⁴ In the pre-reform period, the enrollment rate lingered around 65 percent 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.

The government thus 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).⁵ Gunes explicitly claims that the reform did not seriously aim at improving the quality of education; the additional classrooms were built to accommodate increased enrollment and not to reduce the class size (Gunes 2015, p. 4). Supporting this claim, Figure 1.3 shows that the ratio of the number of primary school classrooms to the number of primary school students (i.e., the classroom-student ratio) did not increase after the reform; this ratio rather decreased in the first reform year,

² Turkey 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.

³ For example, during 1997-2000, the enrollment rate in the sub-region with the lowest pre-reform enrollment rate (Van) increased by 37 percent, while the enrollment rate in the sub-region with the highest pre-reform enrollment rate (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.

⁴ The 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.

⁵ In 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” (p.7) (retrieved from <http://documents.worldbank.org/curated/en/2004/06/4653454/turkey-basic-education-program-project>).

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 predicting election outcomes, and their predictions receive significant attention in national and international media.⁶ 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.⁷ 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 life-satisfaction, through which education could conceivably affect emigration intentions: the respondent's satisfaction with their health, financial circumstances, and the actual political situation in Turkey. 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 circumstances; (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.

⁶See for information on their media coverage http://www.konda.com.tr/tr/uluslararasi_basin.php

⁷The surveys typically interview 1800 to 3600 individuals aged 18 or older. Interviews are conducted face-to-face in the respondents' homes.

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 across the 1991-2003 period.⁸ 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 2007.⁹

We restrict our analysis to individuals born between 1980 and 1992. As the compulsory schooling law required fifth grade students 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.1 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 at least eight years of schooling 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). Respondents in the treatment group reported on average to be more satisfied with their health status (88.8 percent versus 82.9 percent in the control group) and more dissatisfied with the prevailing political situation (42.6 percent a versus

⁸ Children of primary school age are 6-13 years old.

⁹ National Education Statistics and Population Censuses are provided by the Turkish Statistical Institute.

35.9 percent). The differences with respect to financial satisfaction and comfort are surprisingly small (about one and two percentage points, respectively, higher in the treatment group). The entire sample comprises about 50% men and women. Females are however 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.¹⁰ Kurds are the largest ethnic minority in Turkey. In our sample, 17 percent of the respondents have Kurdish ethnicity.¹¹ This figure squares well with the estimate of the CIA World Factbook, indicating that 18 percent of the Turkish population is Kurdish.

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 with no intention to emigrate, and risk aversion also appears to be correlated with the level of education (Outreville, 2015).¹² Unfortunately, our data set does not contain information on social networks, individuals' risk attitudes, etc. The same argument applies to estimating the causal effect of education on the other four dependent variables used in our study (i.e., political dissatisfaction, health satisfaction, financial satisfaction, and financial comfort). Commanding a critical mind, for example, varies positively with a person's academic success and renders him or her more discerning with respect to political machinations, and an upper-class family background fosters both academic success and financial comfort.

¹⁰ In the survey, the respondents are asked to report their religious affiliation as one of the following: Sunni Muslim, Alevi Muslim, and Other.

¹¹ The question on ethnicity lists the following possible responses: Turkish, Kurdish, Arabic, and Other.

¹² Using 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.

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 our dependent variables. 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 her age in 1997 when the law was enacted and on the reform's intensity in her region of birth.¹³ Students who had not yet completed fifth grade in summer 1997 were affected by the education reform. As children in Turkey are normally sent to school at the age of 6, 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.

We exclude from our analysis individuals 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.¹⁴ However, the age requirement for primary school enrollment was never strictly enforced. Since parents can delay their children's primary school entry for one year because of maturity concerns, some individuals who were born in 1986 might have been enrolled in September 1993 rather than in 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.¹⁵

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) and 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 11.¹⁶ The reform

¹³ Duflo (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. Other 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)

¹⁴ This law was published in Official Gazette of the Republic of Turkey (# 21308) on 7 August 1992.

¹⁵ We 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.

¹⁶ Using 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

intensity variable allows us to exploit within-region variation in the number of primary school classrooms per child across cohorts.¹⁷

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

$$Educ_{ijt} = \varphi + \alpha_j + \gamma_t + \beta_1(CSL_i * Classroom_{jt}) + \beta_2 Classroom_{jt} + \beta_3(R_{j1996} * \gamma_t) + \delta X_{ijt} + \mu S_{2013} + \varepsilon_{ijt} \quad (1)$$

where $Educ_{ijt}$ is a binary variable indicating whether the individual i born in region j and in year t 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. $Classroom_{jt}$ 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.¹⁸

R_{j1996} denotes the gross primary school enrollment rate in the region of birth in 1996.¹⁹ 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

across regions to capture the intensity of reform, while Gunes (2015) uses the variation in the number of primary school classrooms across provinces.

¹⁷ To 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 her region of birth, while person B, born in 1988, is matched with the respective 1999 value.

¹⁸ The 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 all individuals who did not live in the region where they were born at the time of the survey. The results from this robustness check are presented in column E of Table 3.

¹⁹ The 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.

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 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:

$$Y_{ijt} = \varphi + \alpha_j + \gamma_t + \theta \widehat{Educ}_{ijt} + \pi_1 Classroom_{jt} + \pi_2 (R_{j1996} * \gamma_t) + \lambda X_{ijt} + \pi_3 S_{2003} + v_{ijt} \quad (2)$$

where Y_{ijt} is a binary variable indicating, respectively, whether the individual has the intention to emigrate, is satisfied with her health status, financial circumstances, or the prevailing political situation. The identifying assumption requires that our instrument, i.e. the interaction between the compulsory schooling law (CSL) dummy and the reform intensity (as measured by the classroom variable) in the region of birth when the individual turned 11, affects the respective dependent variable only through its impact on education (i.e., the instrument is not correlated with the structural error term, v_{ijt} , in equation 2). Standards 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 from column 1 to column 4 in Table 1. In all four specifications, the coefficient of our instrument has the expected positive sign and statistically significant at the 1 percent level. Our preferred estimates derive from the fourth specification. They indicate that in the treatment cohorts a larger classroom-child ratio induces more schooling: 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 A1. Since the first-stage F-statistic exceeds 10, we do not appear to face a weak instruments problem.²⁰ 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.

Table 1. Effect of the compulsory schooling law on education
Dependent variable: having at least eight years of schooling

	(1)	(2)	(3)	(4)	(5) placebo treatment
CSL* Classroom-child ratio	10.36*** (1.97)	9.67*** (1.73)	9.41*** (1.72)	9.31*** (1.77)	0.14 (2.55)
Female		-0.17*** (0.03)	-0.17*** (0.03)	-0.17*** (0.03)	-0.26*** (0.04)
Kurdish			-0.07 (0.05)	-0.07 (0.05)	-0.02 (0.06)
Other Ethnicity			-0.05 (0.05)	-0.06 (0.05)	0.11 (0.10)
Alevi Muslim				0.01 (0.05)	-0.14** (0.06)
Other Religion				0.10* (0.06)	-0.08 (0.07)
F-statistic	27.76	31.39	29.81	27.77	0.00
Mean of the dependent variable	0.77	0.77	0.77	0.77	0.61
Mean of the classroom-child ratio	0.03	0.03	0.03	0.03	0.03
N	1,404	1,404	1,404	1,404	1278

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). Standard errors reported in the parenthesis are corrected for clustering at the region-of-birth and treatment status.

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,

²⁰ As 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).

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 results of the control experiment reported in column 5 of Table 1 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 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 2.1 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 2.1 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.

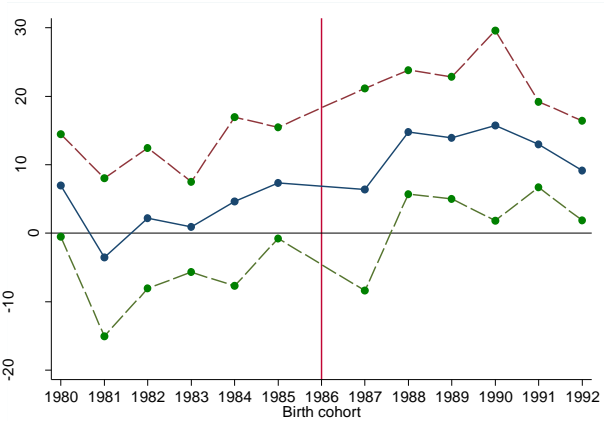


Figure 2.1 Coefficients of the interactions between year-of-birth dummies and reform intensity

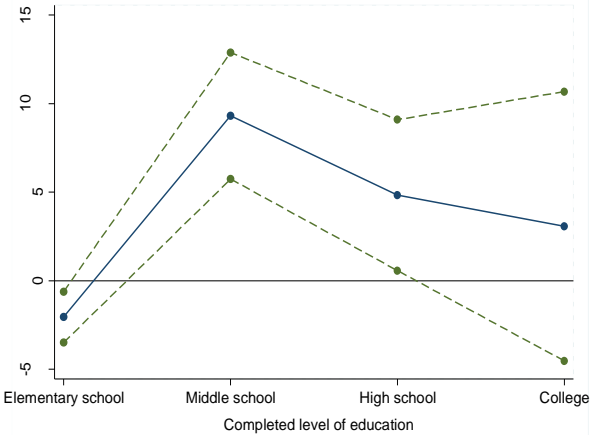


Figure 2.2 Coefficients of the interactions between CSL dummy and reform intensity

Given that the coefficients of the interactions terms presented in Figure 2.1 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.

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

where $Year_{ik}$ are the year-of-birth dummies for the treatment cohorts. $Year_{ik}$ 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 10.²¹

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 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.²² Each point on the solid line in Figure 2.2 represents the estimated coefficient of the instrument for the respective highest level of education. The dashed lines are 95 percent confidence intervals. Figure 2.2 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 2 reports the ordinary least squares (OLS) estimates of the effect of education on our main dependent variables: emigration intentions, political dissatisfaction, health satisfaction, financial satisfaction, and financial comfort which we use as an alternative way of capturing a respondent's financial circumstances. Treating education as exogenous, we find that having at least eight years of schooling is associated with a higher probability of

²¹ Estimation results based on equation 3 are available upon request.

²² The 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.

reporting an intention to emigrate, political discontent, satisfaction with one's health status, and financial comfort. The additional schooling does however not appear to be associated with financial satisfaction.

Table 2 about here

The instrumental variables (IV) estimates reported in Panel C of Table 2 suggest that primary school completion has a positive and statistically significant impact on the propensity to report emigration intentions and political discontent. An additional year of schooling increases the probability of reporting emigration intentions by about 24 percentage points and the probability of reporting political discontent by about 23 percentage points.²³ In both cases the Wooldridge (1995) exogeneity test-statistic is statistically significant, implying that OLS produces inconsistent estimates. Education now turns out to have no causal effect on the self-reported health status and financial comfort, and the OLS estimate of the effect on the respondents' financial satisfaction is confirmed by the IV estimate: education does not appear to have a causal effect on how content people are with their financial circumstances.

It may perhaps come at a surprise that we not find causal effects running from education to the respondents' self-assessed financial situation and health status. Notice, however, that the variables *Financial Satisfaction* and *Financial Comfort* both measure the subjectively perceived distance between the respondents' earnings and their financial expectations or consumer demand -- and education often raises both. This is presumably why we do not find a significant impact of education on being satisfied with one's financial circumstances. In the case of the variable *Health Satisfaction*, the reason for not finding any indication of an education-induced health effect may simply be that in our sample the respondents are still too young to be afflicted with a great deal of health problems.

²³ There 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.

Consistent with the IV results, the reduced-form estimates presented in the second panel of Table 2 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 Kurds are more likely to report emigration intentions and political discontent by 15 and 25 percentage points, respectively.²⁴ Moreover, Table 2 documents that 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.²⁵

5.3 Robustness tests

We conduct several robustness tests to buttress the validity and strength of the IV estimates of the effect of education on the probability of reporting emigration intentions, political dissatisfaction, health satisfaction, and financial satisfaction and comfort. Table 3 shows the results of the robustness tests. Column A replicates the baseline IV estimates of the education effects presented in the first row of panel C in Table 2.

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 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 reveal that the estimated coefficients of primary school completion on the intention to migrate and being dissatisfied with politics remain positive and statistically significant when we change the specification of the first-stage regression. With respect to the health and financial satisfaction, all effects remain insignificant.

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

²⁴ In Turkey, Kurdish people mostly live in south-eastern provinces where PKK's terrorist activities have been mainly concentrated. When we exclude respondents who live in south-eastern provinces from the analysis, the estimated effect of education on the probability of reporting emigration intentions remains statistically significant at the 10 percent level, decreases however somewhat in magnitude.

²⁵ Among 145 countries, Turkey is stuck at rank 130 in the 2015 WEF Global Gender Gap Report.

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 coefficients of education do not change much when we exclude those who, at the time of the survey, no longer lived in their region of birth.

Finally, we cluster standard errors at the region-of-birth level (column F). The estimated effects of education remain statistically significant at conventional levels.

6 Transformation mechanisms

The results presented in the previous section allow us to investigate which of our candidate mechanisms are likely to serve as a conduit between education and emigration intentions. Our candidate mechanisms are financial circumstances, health, and the political situation in Turkey. The descriptive statistics reported in Table 4 are suggestive of whether these three potential channels are really connected with emigration intentions. Contingent on whether the respective respondents expressed the intention to emigrate or not, Table 4 reports the share of respondents who expressed being satisfied with their health, their financial circumstances, and the political situation. Table 4 also reports the share of respondents who have enjoyed at least eight years of education. In the light of our result that education has a statistically and substantively significant causal positive effect on emigration intentions, it is not surprising that among the respondents who contemplate emigration the share of those who have eight years of schooling is larger (83.2%) than the share among the respondents who do not contemplate emigration (74.4%; Table 4, row 1).

Turning now to our three candidate mechanisms, it transpires that in our sample health satisfaction is negatively associated with emigration intentions (row 3). Van Dalen (2013), for example, finds that self-rated health is positively associated with the propensity to emigrate from the Netherlands.²⁶ It is, of course, possible that the different findings can be attributed to the fact that Turkish emigrants usually move to countries with better and more accessible health systems, whereas Dutch emigrants often move to countries that offer lower quality and less accessible health systems than the Netherlands. However, whatever the effect of health on emigration or emigration intentions may be, the mechanism linking education with emigration intentions certainly does not work through the respondents' health status. This is so because we have shown in the previous section (see Table 2) that education does not have a statistically significant effect on self-reported satisfaction with one's health (at least not for people in the

²⁶ Ivlevs (2015), who also examines emigration intentions, finds a similar result for a similar sample (post-socialist and select Western European countries).

age group of our survey respondents). Apart from our abortive attempt to identify the health status a potential link between education and migration intentions, our attempt can, however, pass for a valuable contribution to the literature on the causal effect of education on health outcomes in Turkey.²⁷

Table 4. Mean values of variables by emigration intention status

	no emigration intention	with emigration intention	t-statistic of difference
At least 8 years of education	0.744	0.832	-3.545***
N	994	410	
Dissatisfied with politics	0.343	0.516	-6.089***
N	989	405	
Satisfied with health status	0.872	0.824	2.325**
N	992	410	
Satisfied with financial situation	0.623	0.435	6.560***
N	993	409	
Financial comfort	0.276	0.228	1.858*
N	987	403	

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

For the very same reason, the sought-after mechanism that transforms education into emigration intentions cannot be a lack of financial satisfaction. It is true that in our sample the respondents who contemplate emigration are less likely to be content with their financial circumstances than respondents who do not contemplate emigration (Table 4, rows 4 and 5), and this finding is, of course, in line with most economic studies of migration behavior that have traditionally focused on financial push and pull motives (in particular on differences in expected earnings between the country of origin and the country of destination). But our IV estimates of the effect of education on being satisfied with one's financial circumstances clearly

²⁷ Most studies find that education is causally linked to health, but not all. Dursun et al. (2018) also exploit the 1997 education reform in Turkey to examine the causal effect of education on several health outcomes. They find that education has no statistically significant effect on men's self-reported health while education increases the probability of reporting an excellent health status for women. Cutler and Lleras-Muney (2012) provide a review of the literature.

show that this effect simply does not appear to exist. We realize of course that financial satisfaction does not capture all economic arguments that may be considered when contemplating emigration. Among the various factors of the commonly presumed economic calculus, financial dissatisfaction portrays at best a facet of the push component²⁸ The pull components, as so often in empirical migration studies, are not taken into account at all. The lack of a statistically significant relationship between the respondents' education and their being satisfied with their financial circumstances could also result from the education margin that we are able to examine in this paper. The 1997 education reform allows us to identify the effect of completing at least eight years of schooling in comparison to having at most five years of schooling. Exploring the causal effects of increased education at different levels of schooling (such as holding a high school diploma) on the intention to emigrate and on political and financial dissatisfaction goes beyond the scope of this study.

The last candidate mechanism on our list presumes that education may, in specific political settings, be related to dissatisfaction with the political situation which, in turn, may constitute a contributing push factor of a person's considering emigration. The influence of political push factors of emigration has been analyzed before. Epstein et al. (1999), for example, present a model that shows how exclusive institutions of political rent-seeking give rise to an unravelling exodus of the most productive people. Dustmann and Okatenko (2014) use data from the *Gallup World Poll* and find that in three geographic world regions (Sub-Saharan Africa, Asia, and Latin America) 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. Cooray and Schneider (2016) investigate the causal effects of corruption on the emigration rate of low, medium, and high skilled individuals in 20 OECD countries and find that when corruption increases, the emigration rate of high-skilled

²⁸ Using the Household Labor Force Survey conducted by the Turkish Statistical Institute and also exploiting the 1997 education reform to examine the effect of education on labor market outcomes, Torun (2018) demonstrates that extending primary schooling does not have a statistically significant effect on the wages of males while the effect is positive and statistically significant for females. He also finds that the 1997 education reform does not affect the probability of being employed for both males and females. Our data set does not contain information about the respondents' earnings but household income. We investigate the causal effect of education on household income. The results indicate that the causal effect of education on household income is not statistically significant at conventional levels. In this empirical analysis, the number of observations decreases to 1354 due to missing data on household income.

individuals also increases. Bygnes and Flipo (2017), finally, find that political dissatisfaction is an important motivator of recent intra-European migration. Our result reported in row 2 of Table 4 is perfectly in line with all of these findings; also in our sample political dissatisfaction is positively associated with emigration intentions.

In a sense, these diverse findings establish the last piece of evidence that is needed to identify an evidence-based mechanism that explains how education may give rise to emigration intentions. That education actually causes emigration intentions, we have established with our IV estimates for Turkey (Table 2, column 1, and Table 3, row 1). For the same sample, we have also established that education actually causes political dissatisfaction (Table 2, column 2, and Table 3, row 2). Now we see that various scholars have found empirical evidence for a relationship running from political dissatisfaction in different manifestations to emigration or emigration intentions – a finding that is reflected in our own dataset. We are thus confident in claiming that political dissatisfaction may constitute under certain circumstances, which are, for example, given in present-day Turkey, the conduit linking education to the intention to migrate.

Unfortunately, our data set does not allow us to explore possible mechanisms through which primary school completion may affect political dissatisfaction. However, exploiting the same reform as a source of exogenous variation in education and using large nationally representative samples, the following two studies provide evidence that the 1997 reform of compulsory schooling has had a statistically significant effect on a number of outcomes that can serve as potential mechanisms that link the additional years of schooling to political dissatisfaction. Dursun et al. (2018) find that the causal effect of extended primary education on the probability of using internet and computer on a daily basis and participating in social media activities is positive and statistically significant among males. Torun (2018) documents that additional years of schooling induced by the reform make women more likely to hold occupations that are characterized by higher complexity.

7 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 and a unique survey dataset 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 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 differs across regions, we measure the reform's intensity as the ratio of the number of primary school classrooms and the number of primary school-aged children in the region of birth at age 11, and then exploit the 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 do however, not content ourselves with this result. We also investigate whether education has a causal effect on the survey respondents' self-reported satisfaction with their health status, their financial circumstances, and the political situation in Turkey. These are interesting research questions in their own right, but the answers to these questions also serve to check whether health status, financial circumstances, and political discontent may represent mechanisms that are likely to translate education into emigration intentions. After all, many studies find the health status to be related to emigration or emigration intentions, and most studies find that financial incentives are important determinants of emigration.

Our IV estimates show that self-reported satisfaction with one's financial circumstances and health status are not causally related to the respondent's education. Satisfaction, or rather dissatisfaction, with the political situation turns however out to be causally related to education. This result, together with empirical evidence found in various recent studies, indicating that political discontent engenders emigration intentions, lends credibility to our proposition that at least one of the mechanism linking education to the intention to emigrate works through political discontent. Better educated people are better able to critically appreciate the political situation in their home countries and if this critical faculty engenders political discontent, the resulting disenchantment may well translate into emigration intentions.

We readily acknowledge that the proposed mechanism linking the identified causal relationship between education and emigration intentions need not be the only one, nor need it be at work in general. Perhaps even more importantly, it may be appropriate to emphasize that we have only identified a causal relationship between education and political discontent. Our belief that political discontent is not only correlated with but also causes emigration intentions, relies on not much more than an educated guess. The literature on this relationship is scant. Apart from the study by Cooray and Schneider (2016) we do not know of any convincing causality test of this relationship.

A final caveat concerns our conclusion that the effect of education on emigration intentions does not operate through financial dissatisfaction but rather through political discontent. This conclusion should by no means be misinterpreted as implying that the economic consequences of education have no causal effect on emigration. Education may, for example, increase a person's ability to function properly in a foreign country or to become better integrated which allows enjoying various social amenities that uneducated emigrants would miss out on. It may also provide attractive employment options that would otherwise not come forward. Our conclusion maintains only that education does not engender emigration intentions via financial dissatisfaction. But a person, even one that is perfectly satisfied with his or her financial circumstances, can always contemplate emigration if still better financial prospects loom abroad. We are, unfortunately, not able to test whether this very plausible mechanism is at work in our case.

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Table 2. Effect of education on the intention to emigrate, political dissatisfaction, health satisfaction, financial satisfaction, and financial comfort

Dependent variable	(1) Emigration intention	(2) Political dissatisfaction	(3) Health satisfaction	(4) Financial satisfaction	(5) Financial comfort
Panel A: OLS estimates					
Education	0.06** (0.03)	0.12*** (0.03)	0.08*** (0.03)	-0.01 (0.03)	0.06* (0.03)
Panel B: Reduced form estimates					
CSL* Classroom-child ratio	6.84*** (2.32)	6.14** (2.62)	-4.33 (2.83)	-3.49 (2.62)	-1.96 (2.80)
Panel C: IV estimates					
Education	0.73*** (0.24)	0.68** (0.29)	-0.46 (0.35)	-0.36 (0.27)	-0.21 (0.29)
Female	0.10* (0.05)	0.05 (0.05)	-0.10 (0.07)	-0.08 (0.06)	-0.04 (0.06)
Kurdish	0.15*** (0.05)	0.25*** (0.06)	-0.05 (0.06)	-0.10* (0.06)	-0.02 (0.07)
Other Ethnicity	-0.07 (0.08)	0.07 (0.11)	0.05 (0.09)	-0.07 (0.09)	-0.09 (0.08)
Alevi Muslim	0.11*** (0.04)	-0.08 (0.06)	-0.11* (0.06)	0.02 (0.06)	0.05 (0.06)
Other Religion	0.12 (0.08)	-0.03 (0.06)	-0.03 (0.06)	0.07 (0.07)	-0.06 (0.06)
Wooldridge test statistic	6.99**	3.61*	3.22*	1.57	0.77
Mean of the dependent variable	0.29	0.39	0.86	0.57	0.26
N	1,404	1,396	1,405	1,405	1,393

Notes: * Significant at 0.1 level. ** Significant at 0.05 level. *** Significant at 0.01 level. The dependent variable is Emigration intention in column (1), Political dissatisfaction in column (2), Health satisfaction in column (3), Financial satisfaction in column (4), Financial comfort in column (5). 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.

Table 3. Robustness checks- IV estimates of the effect of education on the intention to emigrate, political dissatisfaction, health satisfaction, financial satisfaction, and financial comfort

Dependent variable	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
Emigration intention	0.73*** (0.24)	0.76*** (0.26)	0.73*** (0.25)	0.80*** (0.20)	0.79** (0.35)	0.73** (0.30)
N	1404	1,540	1,540	1404	1,000	1,404
Political dissatisfaction	0.68** (0.29)	0.67** (0.31)	0.66** (0.30)	0.66** (0.26)	0.87* (0.48)	0.68* (0.37)
N	1,396	1,531	1,531	1,396	993	1,396
Health satisfaction	-0.46 (0.35)	-0.50 (0.37)	-0.46 (0.35)	-0.22 (0.25)	-0.17 (0.30)	-0.46 (0.47)
N	1,405	1,541	1,541	1,405	1,002	1,405
Financial satisfaction	-0.36 (0.27)	-0.38 (0.29)	-0.42 (0.28)	-0.16 (0.25)	-0.42 (0.33)	-0.36 (0.30)
N	1,405	1,541	1,541	1,405	1,002	1,405
Financial comfort	-0.21 (0.29)	-0.25 (0.31)	-0.26 (0.30)	-0.10 (0.30)	0.30 (0.46)	-0.21 (0.39)
N	1,393	1,528	1,528	1,393	993	1,393

Notes: * Significant at 0.1 level. ** Significant at 0.05 level. *** Significant at 0.01 level. Column A shows the baseline estimates presented in Panel C in Table 2. 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, and at the region-of-birth level in Column F. 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.

Appendix

Table A1. 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
Political dissatisfaction	0.393	0.359	0.426
Health satisfaction	0.858	0.829	0.888
Financial satisfaction	0.568	0.562	0.574
Financial comfort	0.263	0.252	0.273
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

Notes: In Column 1 N=1404 for all, except for Political dissatisfaction (N=1396), Health satisfaction (N=1405), Financial satisfaction (N=1405) and financial comfort (N=1393). In Column 2 N=703 for all, except for Political dissatisfaction (N=699), Health satisfaction (N=702), Financial satisfaction (N=703) and financial comfort (N=694). In Column 3 N=701 for all, except for Political dissatisfaction (N=697), Health satisfaction (N=703), Financial satisfaction (N=702) and financial comfort (N=699). 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.