

Education increases patience: Evidence from a change in a compulsory schooling law

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ARTICLE INFO

JEL classification:
D90

Keywords:
Time preference
education
compulsory schooling

ABSTRACT

I investigate the causal effect of education on time preferences. To deal with the endogeneity of education, I exploit exogenous variation in education imposed by a Turkish school reform that raised compulsory education from five to eight years. I find that education causes individuals to make more patient inter-temporal choices but does not induce them to report being more patient. I also provide evidence that the effect of education on patient inter-temporal choices does not operate through changes in financial well-being.

1. Introduction

Time preferences are central to individual decision-making behavior such as saving, borrowing, and investment. In traditional economic models, preferences are assumed to be given (Stigler and Becker, 1977). But beginning in the 1990s, economic theories of endogenous preference formation began to proliferate (Hansson and Stuart, 1990; Rogers, 1994; Rubin and Ii 1979; Ursprung, 1988). Empirical evidence is, however, still scarce.

This paper contributes to our knowledge on the formation of time preferences by investigating the causal role of formal education. There are several reasons to expect that schooling affects time preferences. First, schooling might improve the ability to imagine the future. As Becker and Mulligan (1997) write: “Schooling can communicate images of the situations and difficulties of adult life, which are the future of childhood and adolescence. In addition, through repeated practice at problem solving, schooling helps children learn the art of scenario simulation. Thus educated people should be more productive at reducing the remoteness of future pleasures”. Second, education might influence time preferences by improving cognitive abilities. Numerous studies show that cognitive abilities are associated with a greater willingness to delay immediate rewards for larger but delayed gratifications (Benjamin et al., 2013; Burks et al., 2009; Dohmen et al., 2007;

Frederick, 2005; Shamosh et al., 2008), and recent evidence indicates that schooling enhances cognitive abilities (Carlsson et al., 2015; Dahmann, 2017; Falch and Sandgren Massih, 2011). Finally, schooling might influence time preferences by increasing earnings. A high income might reduce budgetary concerns and can make it easier for an individual to divert his or her attention to the future (Becker and Mulligan, 1997; Fisher, 1930; Mullainathan and Shafir, 2013).¹

Despite the many reasons to expect that education affects time preferences, a causal relationship is difficult to establish empirically. The most prominent concern is reverse causality - that is, patience at younger ages affects educational achievement - which will bias regression coefficients. A further concern is unobservable heterogeneity. Any correlation between education and time preferences might be a result of omitted variables such as individual ability and household characteristics, that commonly affect educational attainment and an individual's time preferences.

This paper exploits a change in the Turkish compulsory schooling law to identify a causal effect of schooling on patience. In 1997, the Turkish government raised mandatory formal schooling from 5 to 8 years for all children who had not yet completed the fifth grade in the end of the 1996/1997 Academic Year. To accommodate the expected increase in enrollment, additional schools and classrooms were built, new primary school teachers were recruited, and transportation was

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¹ In his seminal work, Fisher (1930) conjecture that “a small income, other things being equal, tends to produce a high rate of impatience, partly from the thought that provision for the present is necessary both for the present itself and for the future as well, and partly from the lack of foresight and self-control” (p.72). Becker and Mulligan's (1997) model on patience formation predicts that wealth causes patience: if consumers can invest in “being more patient”, the rich will have the greatest incentive to [invest], as they have both a low marginal utility of wealth (the cost of investing in time preference) and high future utilities (the returns to investing in time preference).” (p. 745). Mullainathan and Shafir (2013, p. 4) argue that scarcity, defined as “having less than you feel you need” impedes cognitive functioning, which in turn may favor behavioral mistakes and myopic behavior.

<https://doi.org/10.1016/j.ehb.2024.101375>

Received 2 June 2023; Received in revised form 6 March 2024; Accepted 10 March 2024

Available online 15 March 2024

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made available to children living in rural areas. During 1997 and 2003, more than 63,000 primary school classrooms were constructed leading a 19 percent increase in the number of primary school classrooms as compared to the 1996 level. Enrollment rates at the primary school level rose rapidly from 82 in 1997–96 percent in 2000.

The empirical analysis is based on a unique and nationally representative data set which contains survey-based information on time preferences of 1391 individuals in Turkey aged between 20 and 37 at the time of the survey. Time preferences are measured by i) revealed preferences elicited by a widely-used inter-temporal choice task (Frederick et al., 2002) with hypothetical rewards, and ii) the respondents' self-assessments of their patience. To estimate the causal effect of education on the two outcome measures, I use the instrumental variables methodology. I follow the approach by Gevrek et al. (2021) who exploit the same reform to estimate the effect of education on emigration intentions, and use an individual's exposure to the reform as instrument for education. The exposure of an individual to the reform is determined both by his age in 1997 and by the number of classrooms in his region of birth when he or she turned 11. Similar strategies were used to estimate the effect of education on earnings (Duflo, 2001), fertility outcomes (Dinçer et al., 2014; Osili and Long, 2008), and child health (Chou et al., 2010; Gunes, 2015).

The results show that education causes people to make more patient inter-temporal choices but does not induce them to report being more patient. A validation exercise of the two patience measures shows that only the number of patient inter-temporal choices produces correlations similar to those in related literature that uses incentivized procedures. I therefore interpret the identified effect of education on inter-temporal choices as evidence that education has a causal positive effect on patience.

To better understand the mechanisms underlying the identified relationship between education and patient inter-temporal choices, I estimate the effect of education on being financially comfortable. I find that being financially comfortable is not causally related to education. Education is thus unlikely to affect patient inter-temporal choices through its impact on financial well-being.

Several papers have attempted to establish causality between education and time preferences. Bauer and Chytilová (2010) use variation in the availability of schools across Ugandan villages to instrument education and find a negative association between education and the villagers' discount rates. Perez-Arce (2017) examines the effect of education by exploiting a random assignment to a college in Mexico and finds that college students who had to wait one year before enrolling make more patient inter-temporal choices than those who entered the university immediately. Alan and Ertac (2018) use a randomized educational intervention aimed at improving patience among elementary school children and find positive effects of the intervention on patient intertemporal decisions in incentivized experimental tasks. This paper extends this literature by using a policy change as a natural experiment to isolate the effect of schooling on time preferences.

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 empirical strategy. Section 5 presents the results. Section 6 concludes.

2. The 1997 education reform in Turkey

In August 1997, the Turkish government increased compulsory schooling from five to eight years which required all children between 6 and 13 years to attend elementary school for five years and middle school for three years.² Exposed to the reform were all children who had not yet completed the fifth grade in the end of the 1996/1997 Academic Year. To accommodate the expected increase in enrollment, the

government constructed new schools and classrooms, recruited teachers, and provided transportation for children living in rural areas. During 1997 and 2003, 63,127 new classrooms were constructed at the primary school level (grades 1–8) which represents a 19 percent increase in the number of primary school classrooms from 1996 and 7 additional classrooms per 1000 primary school aged children (6–13 years) (Turkish Statistical Institute, 2006, also see Fig. 1).

The reform primarily targeted children who would otherwise not enroll in middle school. Regions with relatively low pre-reform enrollment rates experienced therefore a greater expansion in the number of classrooms (see Figure A1 in the Appendix). My calculations suggest that across the 20 sub-regions of Turkey,³ the Spearman rank correlation between gross primary school enrollment rate in 1996 and the change in the number of primary school classrooms per child between 1996 and 2005 amounts to -0.65 and the p-value to 0.002.⁴

The reform led to an immediate increase in enrollment: the gross enrollment rate in grades 6–8 increased rapidly from 65 percent in 1997–93 percent in 2000 (see Fig. 2).⁵ The increase in the gross enrollment rate was much faster in regions with low pre-reform gross enrollment rates, i.e., regions that experienced a higher expansion in infrastructure, and regional gaps in enrollment decreased during this period.

Despite extensive efforts to enhance primary school accessibility, the reform did not initiate any significant changes in terms of quality. In particular, the 1968 national curriculum was maintained with slight modifications. In a comprehensive case study conducted for the World Bank on the implementation of the 1997 education reform, Dulger (2004) also states that, due to time limitations in the implementation process, the Ministry of National Education (MONE) primarily prioritized capacity-related concerns to accommodate new students. Fig. 3 supports this claim, by showing that the classroom-student ratio, i.e., the proportion of primary school classrooms to primary school students, did not experience growth following the implementation of the reform. Instead, this ratio declined during the initial year of the reform and

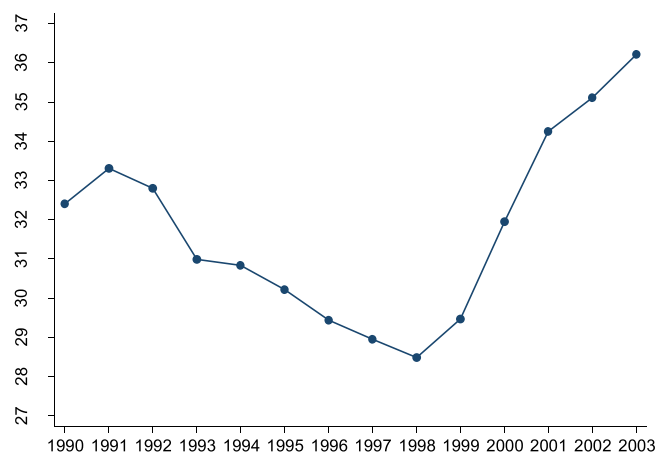


Fig. 1. Number of primary school classrooms (grades 1–8) per 1000 primary school-aged children.

³ The 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.

⁴ Gross primary school enrollment rate in grades 1–8 is calculated by dividing the number of students enrolled in grades 1–8 by the population of children aged 6–13.

⁵ The gross enrollment rate in grades 6–8 is defined as the number of students enrolled in grades 6–8 divided by the population of children aged 11–13.

² The Basic Education Law No: 4306.

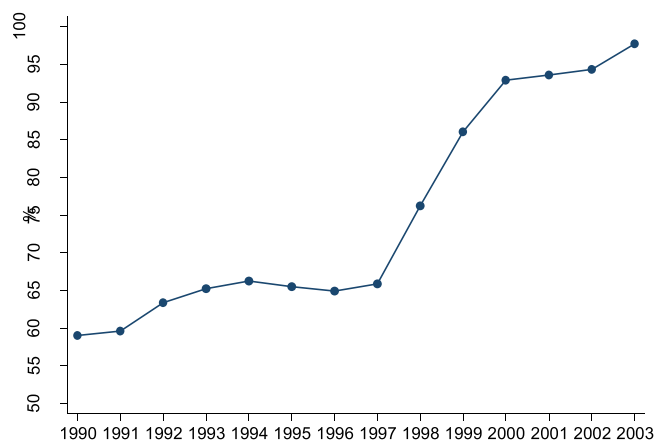


Fig. 2. Gross enrollment rate in grades 6-8.

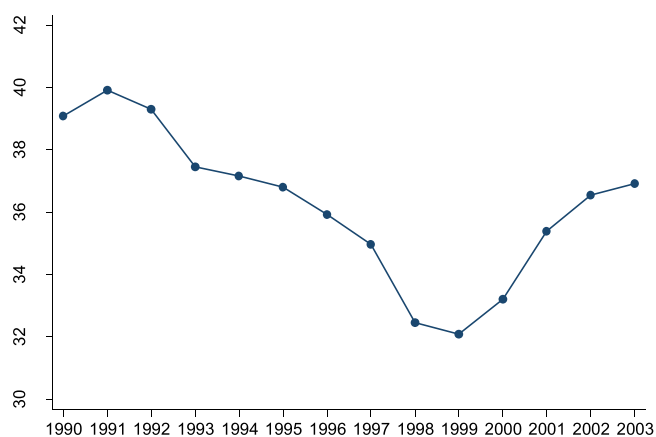


Fig. 3. Number of primary school classrooms (grades 1-8) per 1000 primary school students.

subsequently recovered to its state prior to the reform.

3. Data

The main data used in this paper comes from the KONDA Barometer survey of April 2015. KONDA, one of the largest research and consultancy firm in Turkey, regularly surveys representative population samples about social and economic matters.⁶ Each survey includes a standard set of core questions on age, gender, highest school degree, province of birth, current province of residence, ethnic origin, religion, employment status and financial status.⁷ To study the effect of education on patience, I arranged for including in the regular questionnaire two additional survey items that were designed to measure the respondents' time preferences.

The first item included the well-known choice task (Frederick et al., 2002). In this task, respondents face five hypothetical scenarios (see Table 8 in the Appendix for the actual choice sheet). In each scenario, the respondents were asked to indicate their preference between a smaller payment today and a larger payment to be received in one year. The delayed payment was a fixed amount of 200 TRY for each choice,

⁶ KONDA Barometer surveys have been used extensively in empirical research, e.g., Cesur and Mocan (2018), Gevrek et al. (2019).

⁷ Respondents are interviewed face-to-face in their homes.

and the amount of the earlier smaller payment decreased step by step (190 TRY, 180 TRY, 160 TRY, 130 TRY and 90 TRY, respectively).⁸ Based on the number of choices in which a respondent waits for a larger delayed payment instead of taking the smaller amount today, I constructed a measure of patience that ranges between 0 (never postponed) and 5 (always postponed). A decision to postpone indicates a patient choice; a high number of postponement decisions thus indicates a more patient person.⁹

One particular concern about this elicitation method is that it makes use of hypothetical rewards. It can be argued that in the absence of real monetary incentives respondents might not be motivated to give thoughtful answers; as a result, the elicited preferences may not reveal the respondent's actual preferences. There are, however, two aspects that favor this approach: first, using hypothetical rewards has the advantage to confront respondents with large monetary rewards, which are generally not feasible when using real monetary incentives (Frederick, et al., 2002). Second, studies that compare time preference measures elicited via hypothetical questions and incentivized choices generally find no differences (e.g., Chuang and Schechter, 2015; Falk et al., 2016; Johnson and Bickel, 2002; Madden et al., 2004).¹⁰ Consistent with this evidence is the correlation analysis that I report below. This analysis shows that the patience measure I use behaves in the same way as measures used from studies providing real monetary rewards.

The second survey item is taken from the German Socio-Economic Panel (SOEP) and is based on the respondents' self-assessments regarding their patience in general. The exact wording of the question is "Are you generally an impatient person, or someone who always shows great patience?" The possible answers ranged from "very impatient" to "very patient". I used the respondents' answers to construct a second measure of patience that ranges between 1 and 5, where 1 denotes "very impatient" and 5 "very patient".

My analysis focuses on respondents aged between 20 and 37 at the time of the survey. I assume that students follow the law that regulates primary school enrollment and start primary school by the calendar year of their 6th birthday.¹¹ Respondents aged 20-28 (aged 2-10 in 1997) are therefore considered to be exposed to the reform while respondents aged 29 and older (aged 11-19 in 1997) are exempt from it.¹² I restrict my analysis to respondents who answered the inter-temporal choice questions in a complete and consistent manner. That is, respondents who did not provide an answer in at least one of the five choice situations and those who chose the delayed payment when the earlier payment was

⁸ At the time of survey, 200 TRY was equivalent to roughly 70 USD and to 15 percent of the monthly equivalized household disposable income in Turkey in 2015 (TUIK 2015).

⁹ This time preference elicitation procedure also allows to infer the (bounds on the) discount rates. As the main goal of this paper is to examine the causal impact of education on time preferences rather than the changes in the magnitude of discount rates, the empirical analysis will be based on the number of patient choices. But note that respondents who make more impatient choices will have higher discount rates.

¹⁰ Exceptions include Kirby and Maraković (1996) and Coller and Williams (1999). Both studies find lower discount rates under hypothetical rewards than under real monetary incentives.

¹¹ The law that regulates primary school enrollment stipulates that a child starts primary school if he or she is 72 months old at the end of the respective calendar year (Official Gazette of the Republic of Turkey, # 21308, 7 August 1992).

¹² One should note, however, that the age requirement for enrollment is not strictly enforced. In particular, the entry can be delayed for one year if parents believe their child lacks school readiness. For the 1986 cohort, this implies that some members -especially those born in the last quarter- may be affected by the reform. I address this issue in Section 5.3. In a robustness check, I show that excluding respondents belonging to 1986 cohort does not alter the empirical results.

high but switched to the earlier payment when it was smaller are dropped from the analysis (sample selection issues are examined in Section 5).¹³ The final sample includes 1391 individuals, with 78 percent of the sample having at least 8 years schooling (Table 9 in the Appendix presents the descriptive statistics of the final sample).

Fig. 4 and Fig. 5 show the frequency distribution of answers to the intertemporal choice questions and the general patience question, respectively. The distribution of the number of postponement decisions suggests a high level of impatience in the sample: almost half of the respondents always chose 200 TRY today, i.e., the most impatient option. This is consistent with the level of impatience reported in other developing country studies (e.g., Chuang and Schechter, 2015; Rubalcava et al., 2009). The distribution of the responses to the general patience question yields a different picture, however: the proportion of respondents who rated themselves as “very impatient” is 9 percent. I find low but highly significant correlation (coefficient=0.104) between the number of postponement choices and self-reported levels of patience.

Table 1 provides information on how the two outcome measures are related with individual characteristics and actual outcomes. The OLS estimation results reported in columns 1–5 suggest that patience as measured by the number of postponement decisions in the intertemporal choice task is significantly correlated with gender, and not with age: the number of postponement decisions is higher for females than males. Moreover, the number of patient choices increases with years of schooling,¹⁴ and the relationship between the number of postponement decisions and financial outcomes is significant: the number of postponement decisions increases with household income, unemployed respondents make less patient inter-temporal choices than employed ones, and respondents who report that they were financially comfortable last month make more patient inter-temporal choices than those who report that they were not.¹⁵ These correlations are consistent with the findings of previous studies using incentivized procedures, suggesting

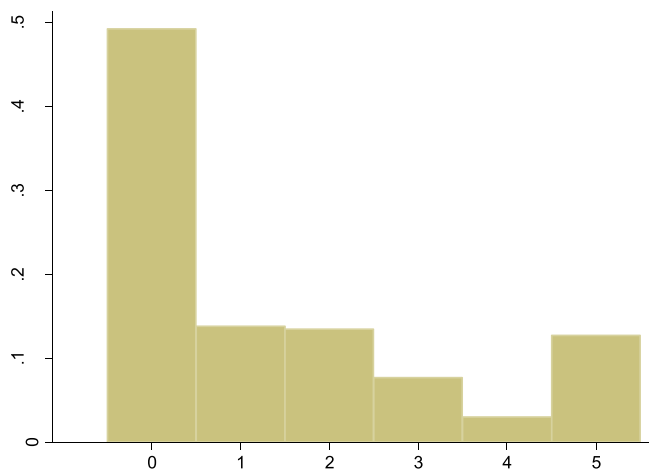


Fig. 4. Distribution of the number of postponement decisions in inter-temporal choice task.

¹³ The original sample without this restriction included 1620 individuals.

¹⁴ The KONDA surveys do not contain information on completed years of schooling but on the highest completed level of schooling. I generate the variable on completed years of schooling based on the Turkish Demographic and Health Surveys (TDHS), which include information on both completed degrees and completed years of schooling. I take 0 years for illiterates, 2 years for literates with no degrees, 5 years for elementary school graduates, 8 years for middle school graduates, 11 years for high school graduates, 15 years for college graduates, and 17 years for postgraduates.

¹⁵ Unfortunately, KONDA does not collect data on individual earnings. I therefore use (the logarithm of) household income.

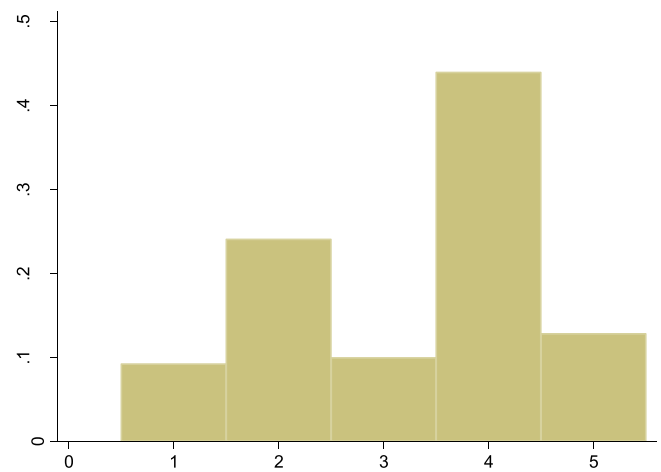


Fig. 5. Distribution of the respondents' self-assessed patience.

that that the use of hypothetical rewards is not a major concern (For similar correlations obtained from financially incentivized procedures see e.g., Ubfal, 2016; Harrison et al., 2002; Kirby and Maraković, 1996; Tanaka et al., 2016; Fisher et al., 2013; Wang et al., 2023). Also, Sunnis make less patient inter-temporal choices than Non-Muslims. Column 6–10 show the correlates of the patience measure derived from responses to the general patience question. Unlike the number of postponement choices, the level of self-reported patience increases with age and does not significantly differ for females and males. Respondents with Turkish ethnicity report lower patience levels than others. With the exception of being unemployed, the responses to the general patience question do not correlate with any of the actual outcome variables.

As one can easily see, except being unemployed, there is no factor that correlates with both measures of patience. While the number of postponement decisions shows similar patterns by age and gender as related work using incentivized procedures, the self-reported patience measure does not. Also, the number of postponement decisions correlates with a broader range of financial outcomes than self-reported patience, a finding that is consistent with Bradford et al. (2017) and Burks et al. (2012) that show that the elicited time preference measures perform better than self-reported patience in predicting financial outcomes. Combined with the evidence on the low correlation between the two measures of patience, the findings from Table 1 suggest that the two measures, while being related, capture different information on actual discounting behavior, in particular, the number of postponement decisions provides a better measure of time preference than self-assessed patience.

Using information on the respondent's region of birth and birth cohort, I matched the survey data with educational data on the number of primary school classrooms per child by region and cohort. I identified the region of birth by assigning each respondent's birth province to one of the 20 sub-regions of Turkey.¹⁶ Cohort of birth was calculated based on the age collected in the survey. Data on the number of primary school aged children was only available for the years 1985, 1990, 2000, and 2007. For inter-census years, I imputed the number of primary school aged children in each region assuming a linear trend.¹⁷

¹⁶ The Nomenclature of Territorial Units for Statistics divides Turkey in 26 statistical sub-regions. To obtain a minimum of 50 observations per sub-region, I adopt the following classification: I merge 6 small sub-regions with a neighboring larger sub-region. The 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.

¹⁷ National Education Statistics and Population Censuses are provided by the Turkish Statistical Institute.

Table 1
Correlates of the number of postponement decisions and self-reported patience: OLS.

	# of pp decisions	# of pp decisions	# of pp decisions	# of pp decisions	# of pp decisions	Self-reported patience	Self-reported patience	Self-reported patience	Self-reported patience	Self-reported patience
Female	0.320*** (0.080)	0.378*** (0.073)	0.365*** (0.086)	0.309*** (0.083)	0.349*** (0.088)	0.120 (0.078)	0.147* (0.071)	0.130 (0.078)	0.113 (0.078)	0.119 (0.078)
Age	-0.005 (0.012)	0.001 (0.012)	-0.006 (0.013)	-0.007 (0.013)	-0.005 (0.012)	0.028*** (0.008)	0.030*** (0.009)	0.027*** (0.008)	0.026*** (0.008)	0.028*** (0.008)
Turkish	-0.176 (0.187)	-0.211 (0.189)	-0.112 (0.204)	-0.177 (0.189)	-0.214 (0.196)	-0.246*** (0.075)	-0.263*** (0.081)	-0.232*** (0.070)	-0.245*** (0.076)	-0.255*** (0.074)
Kurdish	-0.156 (0.198)	-0.141 (0.192)	-0.049 (0.176)	-0.165 (0.194)	-0.162 (0.196)	-0.183 (0.132)	-0.176 (0.141)	-0.238* (0.125)	-0.186 (0.137)	-0.187 (0.123)
Sunni	-0.524** (0.220)	-0.429* (0.233)	-0.485* (0.249)	-0.515** (0.212)	-0.502** (0.226)	-0.194 (0.173)	-0.149 (0.184)	-0.111 (0.177)	-0.188 (0.174)	-0.193 (0.173)
Alevi	-0.357 (0.285)	-0.304 (0.282)	-0.311 (0.331)	-0.342 (0.282)	-0.350 (0.276)	-0.350 (0.249)	-0.325 (0.259)	-0.240 (0.244)	-0.318 (0.252)	-0.356 (0.250)
Years of schooling		0.037** (0.013)					0.017 (0.011)			
Household income (log)			0.171* (0.087)					-0.003 (0.088)		
Unemployed				-0.413** (0.158)					-0.260* (0.131)	
Financial comfort					0.524*** (0.166)					0.017 (0.088)
Observations	1386	1386	1322	1382	1379	1383	1383	1320	1379	1376

Notes: * Significant at 0.1 level. ** Significant at 0.05 level. *** Significant at 0.01 level. The reference categories for ethnic origin and religion are Other ethnic origin and Other religious affiliation (Non-Muslim), respectively. Household income is the logarithm of the total amount of money earned by every member of the household. Unemployed is a dummy variable that equals 1 if the respondent participates in the labor force but is currently unemployed and 0 if otherwise. Financial comfort is a dummy variable that equals 1 if the respondent was financially comfortable last month, and 0 if the respondent could barely manage to make ends meet, could not make ends meet, or could not pay all the bills and borrowed money. The sample consists of respondents aged between 20 and 37 at the time of the survey. The regression is estimated by OLS. Standard errors reported in the parenthesis are corrected for clustering at the region-of-residence.

4. Empirical strategy

An empirical analysis of the causal effect of schooling on time preferences is challenging as reverse causality and the unobserved factors affecting both education and patience may produce bias in the estimates. A solution to this difficulty requires an exogenous source of variation in education (Card, 1999). In this paper, I use the change in the compulsory schooling law in Turkey in 1997 as a source of exogenous variation in schooling. I follow the approach by Duflo (2001) and Gevrek et al. (2021) and use a respondent’s exposure to the reform as an instrument for education.

The exposure of a respondent to the reform is determined both by his age in 1997 and the reform intensity in his region of birth. The Compulsory Education Law was enacted on August 1997 and children who were at the 5th grade in the 1997/98 school year were the first group affected by the law. As children in Turkey normally attend primary school between the ages 6 and 11, I form a treatment group of respondents who were younger than 11 in 1997 (i.e., born after 1986) and were affected by the legislation and a corresponding comparison group of respondents who were older than 11 in 1997 (i.e., born before 1986) and were not affected.

Recall that the reform primarily targeted children who had not previously been enrolled in school. Regions with relatively low pre-reform enrollment rates experienced a greater expansion in the number of classrooms (see Fig. 1 and Figure A1). I take advantage of this additional source of variation and measure the intensity of the education reform as the number of primary school classrooms (grades 1–8) as a proportion to the number of primary school aged children (aged

between 6 and 13) in the respondent’s region of birth when she or he turns 11.¹⁸

Eq. (1) describes the baseline first-stage model

$$Educ_{ijt} = \phi + \alpha_j + \gamma_t + \delta X_{ijt} + \beta_1 (Young_i * Intensity_{jt}) + \beta_2 Intensity_{jt} + \beta_3 (R_{j1996} * \gamma_t) + \varepsilon_{ijt} \quad (1)$$

where $Educ_{ijt}$ is a binary variable indicating whether the respondent i born in region j in year t has at least eight years of schooling. α_j are region-of-birth fixed effects to deal with time-invariant, unobserved heterogeneity at the region-level (such as pre-reform school availability, teacher quality, and initial differences in the level of economic development). γ_t are year-of-birth fixed effects that control for national trends. X_{ijt} is a vector of individual characteristics that includes ethnicity, religion and gender. The *Young* dummy indicates whether an individual belongs to the cohort affected by the reform. It takes the value of 1 if the individual was between the ages of 2–10 in 1997 (or between the ages of 20 and 28 at the time of the survey), and it takes the value of zero if the individual was between the ages 11–19 in 1997 (or between the ages of 29 and 37 at the time of the survey) and therefore was not affected by the reform.¹⁹ $Intensity_{jt}$ denotes the ratio of the number of primary school classrooms (for grades 1–8) to the number of primary school aged children (6 and 13 years) in the region of birth in year $t = t + 11$, i.e., when the respondent turned 11. Since I do not have

¹⁸ Using solely the variation across cohorts as an instrument for schooling may introduce bias in the estimates since there might be economic and social factors and other policy changes coinciding with the education reform. Accounting for the reform intensity allows me to control for policy changes and unobserved national trends that may be correlated with the education reform.

¹⁹ The construction of treatment (Young) and comparison (Older) groups assumes that students did not experience grade retention. Grade retention may lead some individuals in the older cohort to be exposed to the reform, which will result in a downward bias in the estimated effect of education.

Table 2
Effect of the compulsory schooling law on education (first-stage).

	Model 1	Model 2	Model 3	Model 4
Young*Classes per child at age 11	9.556*** (1.912)	9.666*** (2.051)	9.593*** (2.021)	9.985*** (1.991)
F-statistic	24.98	22.20	22.52	25.16
Control variables:				
Gender	No	Yes	Yes	Yes
Ethnicity	No	No	Yes	Yes
Religion	No	No	No	Yes
Mean of the dependent variable	0.775	0.775	0.775	0.775
Mean of the classroom-child ratio	0.033	0.033	0.033	0.033
Observations	1419	1415	1408	1391

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

The dependent variable is a dummy variable that equals 1 if the respondent has at least 8 years of schooling and 0 otherwise. The sample consists of respondents aged between 20 and 37 at the time of the survey. Respondents aged between 20 and 28 years are classified as Young. The F-statistics test the hypothesis that the coefficient of the interaction between Young dummy and the number of classrooms per child at age 11 is zero. All the regressions control for year-of-birth fixed effects, region-of-birth fixed effects, number of classrooms per child at age 11, interactions between the Young dummy and the gross enrollment rate in grades 6–8 in the region of birth in 1996. Standard errors reported in the parenthesis are adjusted for clustering at the region-of-birth and treatment status.

information on the respondent’s region of schooling and when he or she started middle school, the *Intensity_{jt}* variable assumes that the respondents started 6th grade in the birth region at the age of 11.

The exclusion restriction requires that exposure to the reform should not directly affect the outcomes of interest, other than through its effect on educational attainment. In other words, the excluded instrument should be uncorrelated with the error term. Recall that the provision of additional classrooms was greater in regions that were lagging in education in the pre-reform period. To control for variation in reform intensity associated with pre-reform enrollment rates and other unobservable factors related to the pre-reform enrollment rates, Eq. (1) also includes a full set of interactions of year of birth dummies with the gross enrollment rate in grades 6–8 in 1996, R_{j1996} , in the region of birth.

The association between the reform and schooling for the treatment group *Young* is estimated by $\beta_1 + \beta_2$, and the same relationship for the control group is estimated by β_2 . As the control group was not exposed to the reform, β_2 measures the effect of other time varying factors associated with the reform intensity variable *Intensity_{jt}*. Under the assumption that the control and treatment groups are equally influenced by the other time varying factors correlated with the intensity variable, β_1 would estimate the impact of the compulsory education reform on the schooling of the treatment group *Young*.

The second-stage model is:

$$Y_{ijt} = \eta + \alpha_j + \gamma_t + \lambda X_{ijt} + \theta \widehat{Educ}_{ijt} + \pi_1 Intensity_{jt} + \pi_2 (R_{j1996} * \gamma_t) + v_{ijt} \quad (2)$$

where \widehat{Educ}_{ijt} is the predicted values of education obtained from the first-stage regression, and Y_{ijt} is a variable indicating the measure of patience for individual *i*. With heterogeneous treatment effects, the IV estimate, θ , identifies the effect of extended compulsory education only on compliers, i.e., individuals who would not have completed 8 years of schooling in the absence of the change in the compulsory schooling law (Angrist and Imbens, 1995). In all specifications, standard errors are corrected for clustering at the region-of-birth and treatment status.

5. Results

5.1. First-stage results

The first-stage results (Eq. (1)) are shown in Table 2. All models control for year-of-birth fixed effects, region-of-birth fixed effects, and the number of classrooms per child at age 11. I add additional controls successively in Models 2–4. Model 2 includes a dummy indicating the

²⁰ The results are consistent to excluding these interactions.

Table 3
Effect of the compulsory schooling law on education in the control experiment.

Panel A: Baseline Estimates	
Treatment: aged between 20 and 28 Control: aged between 29 and 37 N=1391	
CSL* Classroom-child ratio	9.31*** (1.77)
Panel B: Estimates from Control Experiment:	
False Treatment: aged between 29 and 37 Control: aged between 38 and 40 N=968	
CSL* Classroom-child ratio	0.505 (3.019)

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 *Young* dummy and *Classes per child at age 11*) from the baseline specification presented in column 4 of Table 1. Panel B shows the estimated coefficient of the interaction between an indicator variable that equals 1 for individuals aged 29–37 and zero for individuals aged 38–40 and *Classes per child age 11*. Standard errors reported in the parenthesis are corrected for clustering at the region-of-birth and treatment status.

respondents’ gender, Model 3 adds the respondents’ ethnic background, Model 4 includes the respondents’ religious affiliation. The estimate in Model 4 suggests that a one percentage point increase in the number of classrooms per child increases among the treated cohorts the probability of completing at least eight years of schooling by 9.98 percentage points. At the sample mean of 0.78, this represents a 12.8 percent increase in the probability of having at least eight years of schooling. The F-statistics across all specifications are larger than the critical F-ratio of 10, implying that weak instrument concerns are not warranted (Staiger and Stock, 1997).

One concern with the identification is that middle school completion may be higher for the younger cohorts because of reasons other than the education reform. In particular, the differences in schooling outcomes may be due to an existing differential trend in educational attainment across regions. I address this concern in Table 3 in which I compare the cohorts aged 29–37 to the cohorts aged 38 and 40 at the time of the survey. Neither group was exposed to the reform. However, if education increased faster in regions that received more resources prior to the reform, then I should find a significant coefficient for the younger unaffected cohorts. As shown in Panel A, the estimated coefficient of the instrument is insignificant.

I next generalize the identification strategy and use interaction term

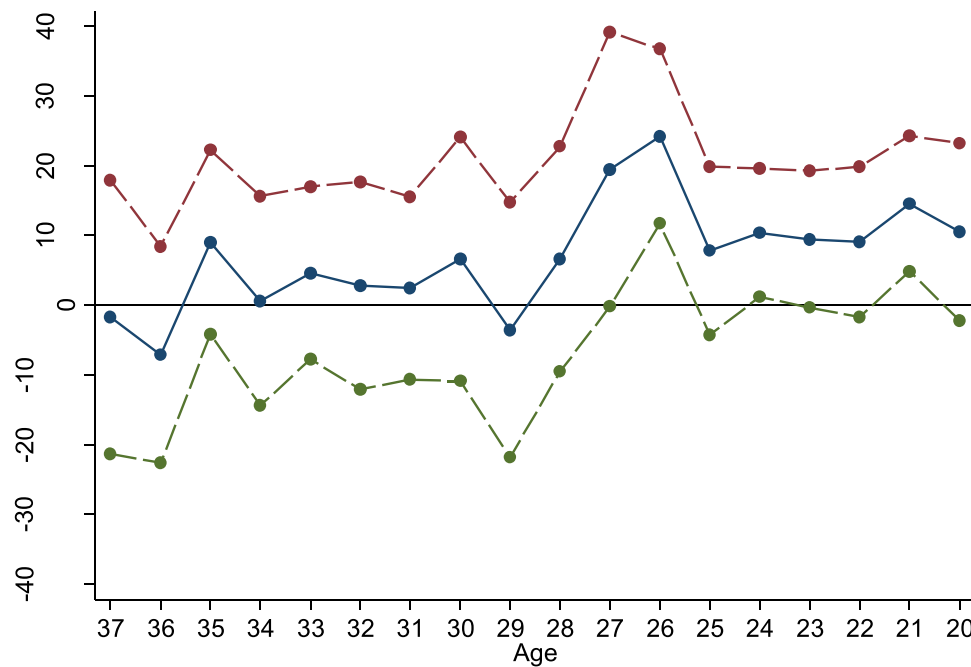


Fig. 6. Coefficients of the interactions between age dummies and reform intensity and 95% confidence intervals in the education equation.

analysis to estimate the following specification

$$Educ_{ijt} = \phi + \alpha_j + \gamma_t + \delta X_{ijt} + \zeta_k (Age_{ik} * Intensity_{jt}) + \beta_2 Intensity_{jt} + \beta_3 (R_{j1996} * Age_{ik}) + \epsilon_{ijt} \tag{3}$$

where Age_{ik} is a dummy that takes the value of 1 if the individual i is age k at the time of the survey and 0 otherwise. Fig. 6 plots the estimated coefficients of the 18 interaction terms along with the 95 percent confidence intervals. The coefficients fluctuate around 0 for respondents aged 29–37 and turn larger and statistically significant for respondents younger than 28. These findings show that the effects of the reform were restricted to the treatment group and that the empirical strategy is reasonable.²¹

To test the validity of the instrument, I follow Duflo (2001) and examine the effect of the reform on different levels of education. I run regressions using Eq. (1) in which the dependent variable is equal to one if the respondent completed at least “s” levels of education (elementary education, middle school education, high school education, and college education) and zero otherwise. Fig. 7 plots the estimated coefficients with corresponding 95 percent confidence intervals. It can be seen that the reform has the largest effect on the probability of completing middle school and it has a modest effect on the probability of completing high school. The estimated effects on the probability of completing elementary school and college are statistically insignificant. These findings suggest that the reform increased schooling through increasing middle school completion essentially and has been reported by other researchers as well (Dincer et al., 2014; Günes, 2015; Kirdar et al., 2018).

²¹ Dincer et al. (2014) and Günes(2015) obtain similar results for female sample.

5.2. Effect of education on patience

Before reporting the two-stage least squares (IV) estimates of the effect of education on patience, I begin by estimating ordinary least squares regressions. Table 4, Panel A, reports the OLS estimates of the effect of education on patience, measured by the number of postponement decisions in the question set on intertemporal choice and self-reported patience. The OLS estimates suggest that having at least 8 years of schooling has no statistically significant effect on neither of the two measures of patience. However, the OLS regressions do not account for the potential endogeneity of educational choices and may therefore yield biased results.²²

In Panel C, I report the IV results. Column 1 shows the IV estimate of the effect of completing 8 years of compulsory schooling on the number of postponement decisions. The IV estimate suggests that having 8 years of schooling has a positive and statistically significant impact on the number of postponement decisions. The coefficient indicates that having 8 years of schooling increases the number of patient inter-temporal choices by roughly 3. This effect amounts to 1,7 standard deviations. Consistent with this result is the reduced form estimate, reported in Panel B, that indicates a positive and statistically significant effect of the instrument on the number of postponement decisions. The Wooldridge exogeneity test-statistic is statistically highly significant, implying that OLS estimates that treat education as exogenous would not be consistent

²² Studies that find a significant relationship between education and time preferences typically measure education by years of schooling (see Dohmen et al., 2010 who also find insignificant correlations between different educational degrees and patience in a German sample). When I replace the dichotomous “8 years of schooling” variable with continuous (imputed) years of education variable in the model shown in Panel A, the estimated OLS coefficient is positive and significant at the 1 percent level, confirming previous studies. The reason why I use the dichotomous “8 years of schooling” variable rather than the continuous “(imputed) years of education” variable is because the reform affected educational attainment in Turkey mainly through increasing middle school completion (see Fig. 7). Moreover, estimating Eq. (1) by replacing the dependent variable with years of educations yields lower first-stage F-statistics. Lower F-statistics in estimations with years of education are also reported by Dincer et al. (2014).

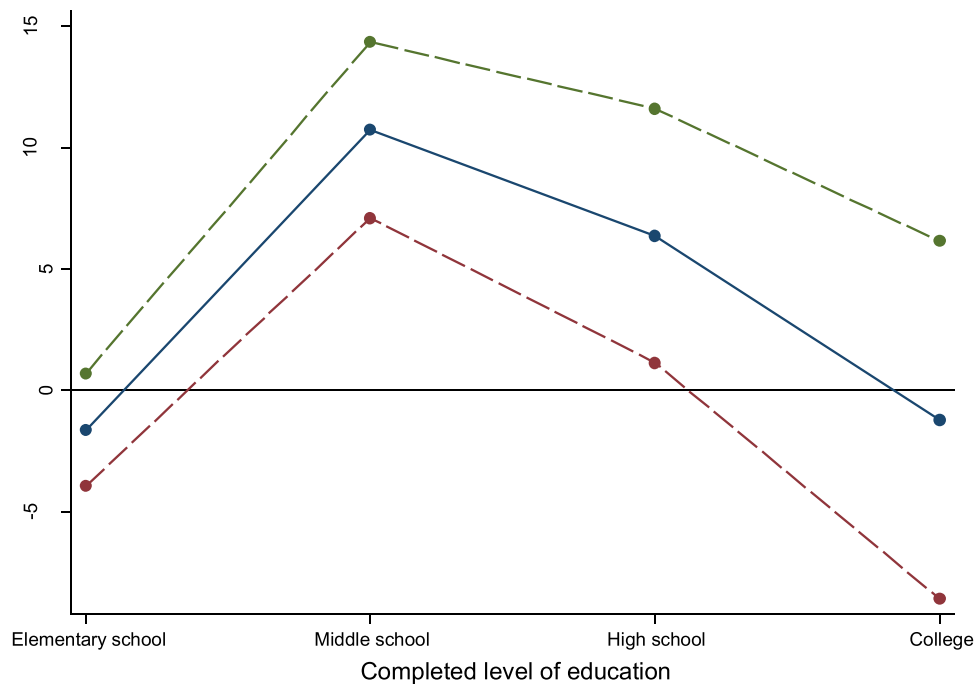


Fig. 7. Association between the probability of completing at least “m” level of education and reform intensity and 95% confidence intervals.

Table 4
Effect of education on patience.

	# of postponement choices	Self-reported patience
Panel A: OLS estimates		
At least 8 years of schooling	0.184 (0.115)	0.028 (0.071)
Panel B: Reduced form estimates		
Young* Intensity	29.720*** (8.803)	4.647 (12.205)
Panel C: IV estimates		
At least 8 years of schooling	2.976*** (1.127)	0.464 (1.140)
Female	0.740*** (0.171)	0.151 (0.173)
Turkish	-0.354 (0.233)	-0.276*** (0.097)
Kurdish	-0.081 (0.276)	-0.230** (0.112)
Sunni	-0.226 (0.367)	-0.120 (0.156)
Alevi	-0.137 (0.470)	-0.265 (0.183)
Wooldridge test statistic	9.728***	0.132
N	1391	1388

Notes: * Significant at 0.1 level. ** Significant at 0.05 level. *** Significant at 0.01 level. Each cell of panels A, B and C are obtained from separate regressions with the dependent variable shown in the column heading. The number of patient choices ranges between 0 and 5. The self-reported patience ranges between 1 and 5. The sample consists of respondents aged between 20 and 37 at the time of the survey. Respondents aged between 20 and 28 years are classified as Young. All the regressions control for year-of-birth fixed effects, region-of-birth fixed effects, number of classrooms per child at age 11, interactions between the Young dummy, the gross enrollment rate in grades 6–8 in the region of birth in 1996, gender, ethnic background and religious affiliation. Standard errors reported in the parenthesis are adjusted for clustering at the region-of-birth and treatment status.

and that IV estimation is appropriate (Wooldridge, 1995). The IV results also show that women make more patient intertemporal choices than men. This result is consistent with extensive empirical evidence indicating that females have lower discount rates than males (Bauer and Chytilová, 2013; Bettinger and Slonim, 2007; Castillo et al., 2011; Ditrich and Leipold, 2014; Kirby and Maraković, 1996; Ubfal, 2016).

There are two standard explanations for the larger IV estimates reported in Table 4. First, measurement error in schooling might introduce a downward bias in the OLS estimates, and eliminating this bias can yield larger IV estimates. Second, as pointed out earlier, to the extent that behavioral responses to a change in the compulsory schooling

reform vary across individuals, the IV estimator may capture the average effect of extended compulsory education for the subpopulation of individuals who altered their schooling choice because of the reform (Angrist and Imbens, 1995). Due to the increased marginal cost of education, these individuals would opt for fewer years of schooling in the absence of the reform. The effect of schooling on patient intertemporal choices is likely to be more pronounced for these individuals than for the entire population, leading to larger IV coefficients.

Column 2 presents the IV results when the dependent variable is the self-reported patience. The estimated effect of having 8 years of schooling is statistically insignificant. Consistent with this finding, the

Table 5

Effect of education on the probability of giving an incomplete/inconsistent answer in the inter-temporal choice task.

	Incomplete	Inconsistent
At least 8 years of schooling	-0.006 (0.137)	-0.054 (0.058)
Female	0.027 (0.026)	-0.005 (0.010)
Turkish	0.055* (0.033)	-0.014 (0.021)
Kurdish	0.146*** (0.038)	0.008 (0.019)
Sunni	-0.053 (0.040)	0.004 (0.016)
Alevi	-0.015 (0.047)	0.016 (0.026)
Observations	1620	1620

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

The sample consists of respondents aged between 20 and 37 at the time of the survey. All regressions are estimated using IV method and control for year-of-birth fixed effects, region-of-birth fixed effects, number of classrooms per child at age 11, interactions between the Young dummy and the gross enrollment rate in grades 6–8 in the region of birth in 1996, gender, ethnic background and religious affiliation. Standard errors reported in the parenthesis are adjusted for clustering at the region-of-birth and treatment status.

reduced-form estimate, reported in Panel B, shows no statistically significant relationship between the exposure to the reform and the responses to the general patience question. The results also show that respondents with Turkish ethnicities and respondents with Kurdish ethnicities self-report lower levels of patience than respondents from other ethnic minorities.

Overall, the results demonstrate that extended compulsory education leads to more patient inter-temporal choices but not to a higher self-reported patience. The validation exercise of the two measures in Section 3 has shown that only the number of patient intertemporal choices produces correlations similar to those in related literature that uses incentivized procedures. I therefore interpret the observed positive effect on patient inter-temporal choices as evidence that education causally affects time preferences.

5.3. Robustness checks

I next address several concerns regarding the empirical strategy: problems related to sample selection and data limitations.

The benchmark analysis estimates the effect of education in a selected sample of individuals who answered all five hypothetical choice questions in a consistent way: individuals who did not respond to at least one of the inter-temporal choice questions (12 percent of the entire sample) and individuals who gave inconsistent answers (2 percent of the entire sample) are excluded.

In Column 1 Table 5, I use IV regression to examine whether the probability of giving an incomplete answer is affected by schooling. I adopt an equation similar to Eq. (2) with one modification: now the dependent variable is a dummy that takes the value of 1 if the response is incomplete, and 0 otherwise. It can be seen that the coefficient of having 8 years of schooling is statistically insignificant (p-value=0.966).

To examine whether the probability of giving an inconsistent answer is affected by schooling, I now estimate Eq. (2) by replacing the dependent variable by a dummy that indicates whether the respondent gave an inconsistent answer or not. The results of this estimation are presented in Column 2, Table 5. The IV coefficient of completing 8 years of schooling is insignificant (p-value=0.357). Thus, I find no evidence that education affects the probability of giving an incomplete answer and the probability of giving an inconsistent answer. These findings suggest that restricting the sample on respondents who answer all five hypothetical choice questions in a consistent way is unlikely to induce a

Table 6

Effect of education on patience with different sample restrictions.

Dependent variable	12 cohorts instead 18	Exclude 1986 cohort	Non- migrants
# of postponement choices	1.987** (0.993)	2.582*** (0.990)	2.729*** (0.986)
N	936	1286	1236
Self-reported patience	-0.503 (1.206)	0.791 (1.143)	-0.332 (1.237)
N	933	1284	1233

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

All regressions control for year-of-birth fixed effects, region-of-birth fixed effects, number of classrooms per child at age 11, interactions between the Young dummy and the gross enrollment rate in grades 6–8 in the region of birth in 1996, gender, ethnic background and religious affiliation. Standard errors reported in the parenthesis are adjusted for clustering at the region-of-birth and treatment status.

correlation between the instrument and the error in Eq. (2) and therefore lend support to the validity of the IV estimates reported in Table 4.²³ While answering all questions consistently does not depend on schooling, it is significantly influenced by ethnicity. Compared to other minorities, both the probability of giving an incomplete and the probability of giving an inconsistent answer are higher among Kurds.

I also test whether the results depend on the selection of the analyzed birth cohorts. The main analysis used 18 cohorts, comprising 9 older and 9 young cohorts. In Table 6, Column 1, I repeated the regressions narrowing the cohort-window to 12 cohorts, comprising 6 older and 6 young cohorts (the sample consists of respondents aged between 34 and 23 at the of the survey). The IV estimates for this restricted sample largely support the previous findings, although there are some differences. The estimated effect of education on the number of patient inter-temporal choices decreases but is statistically significant at the 5 percent level. With respect to self-reported patience, the effect remains insignificant.

The construction of the treatment (Young) and control (Older) cohorts assumed that students follow the law that regulates primary school enrollment and start school the year they turn 6. As noted in footnote 11, however, the age requirement for enrollment is not strictly enforced. The entry can be delayed for one year if parents believe that their child lacks school readiness. For 1986 cohort, this implies that some students, especially those who born in the last quarter, might be affected by the policy. To test whether the results depend on the used specification of the treatment and control groups, I exclude respondents belonging to this group (see Column 2, Table 6). The results are similar compared to the whole sample.

As mentioned in the data section, the geographical information on where a respondent completed primary school was lacking. I therefore assumed that the respondent went to school in his or her birth region. If the respondent completed primary education in a different region than his or her birth region, however, both the reform’s intensity variable and the endogenous regressor “at least 8 years of schooling” will be measured with an error. To address this concern, I exclude respondents who were born in regions with the highest share of emigrants and do not currently live in their region of birth.²⁴ As shown in Column 3, Table 6, the IV estimates obtained from this restricted sample are similar to the

²³ I estimated Equation (2) where I replaced the dependent variable by either incomplete or inconsistent response. The IV estimate remains insignificant (p-value=0645).

²⁴ According to 2006 Migration and Internally Displaced Population Study (Institute of Population Studies) regions with the highest share of emigrants are Van, Sanliurfa and Mardin, Northeast Anatolia, Malatya and Gaziantep.

benchmark result, suggesting that a possible bias due to migration between the regions is not a major driver of the results.

5.4. Does the effect of schooling operate through changing financial well-being?

Overall, the results presented in the previous section suggest that extended compulsory education leads to more patient behavior. A potential channel by which the identified effect might operate is financial well-being. An increase in education may improve financial well-being, and this might make individuals more willing to delay immediate rewards, either because of a change in preferences (Fisher, 1930; Becker and Mulligan, 1997) or because richer people are less likely to be liquidity-constrained.²⁵ Indeed, a number of studies find a significant effect of changes in financial situation on inter-temporal choices (Carvalho et al., 2016; Dean and Sautmann, 2021; Harrison et al., 2005). With respect to the relationship between schooling and economic returns, evidence is mixed. For instance, while Oreopoulos (2006), Aakvik et al. (2010), and Meghir and Palme (2005) find that schooling positively affects earnings in Canada, Norway, and Sweden, Grenet (2013), Oosterbeek and Webbink (2007), and Pischke and Wachter (2008) document zero returns to schooling in France, Netherlands, and Germany. Exploiting the 1997 Turkish compulsory schooling reform, Aydemir and Kirdar (2017) find low returns to an additional year of schooling for men and higher returns for women. In particular, the authors report zero returns for middle school grade levels for both genders (p.30). Torun (2018) reports small effects of an additional year of schooling on earnings of men and large positive effects on earnings of women in Turkey.²⁶ The author also shows that an additional year of compulsory schooling does not affect the likelihood of being employed for either gender.

The dataset allows me to test whether financial well-being is a channel by which schooling affects time preferences. I capture financial well-being by the outcome variable *Financial comfort*. It is obtained from the respondents' answers to the following question: "Could you make ends meet last month".²⁷ The possible answers were "(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". The binary variable *Financial comfort* takes the value of one if the respondent's answer is (i) and zero otherwise.²⁸

Table 7 presents the effect of schooling on the probability of being financially comfortable. The OLS estimate suggests that completing 8 years of schooling has no effect on the probability of having financial comfort. Also, the IV estimate is insignificant and demonstrates that middle school completion does not have an impact on being financially

²⁵ The literature discusses two mechanisms through which a higher income could affect intertemporal choice behavior. One is through reducing liquidity constraints, as richer people can afford to delay consumption more than the poor. The second mechanism is through a change in preferences (see footnote 1). As Dean and Sautmann (2021) discuss, it is challenging to disentangle these mechanisms in choice data. See Andersen et al. (2008) and Andreoni and Sprenger (2012) for relevant discussions on these issues.

²⁶ The study does not report the returns to schooling at the middle school grade levels (6–8).

²⁷ Like other self-reported financial well-being measures, financial comfort is prone to heterogeneity in reporting, that is, for a given true but unobserved financial comfort, the respondents might report differently depending on their incentives to report bad financial situation (e.g., to gain a tax or subsidy advantage), tendency to complain, and comprehension of the survey questions. Reporting heterogeneity need not be a major concern, however, provided that the differences in reporting behavior are not systematic.

²⁸ The mean value for financial comfort is 0.18.

Table 7
Effect of education on financial well-being.

	Financial comfort
Panel A: OLS estimates	
At least 8 years of schooling	0.030 (0.024)
Panel B: Reduced form estimates	
Young* Intensity	-0.149 (2.045)
Panel C: IV estimates	
At least 8 years of schooling	-0.015 (0.199)
Female	-0.038 (0.037)
Turkish	0.128*** (0.038)
Kurdish	0.079* (0.043)
Sunni	-0.052 (0.067)
Alevi	-0.149** (0.060)
Wooldridge test statistic	0.050
N	1384

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

Each cell of panels A, B and C are obtained from separate regressions with the dependent variable shown in the column heading. Financial comfort is a dummy variable that equals 1 if the respondent was financially comfortable, and 0 if the respondent had financial difficulty. The sample consists of respondents aged between 20 and 37 at the time of the survey. Respondents aged between 20 and 28 years are classified as Young. All the regressions control for year-of-birth fixed effects, region-of-birth fixed effect, number of classrooms per child at age 11, interactions between the Young dummy and the gross enrollment rate in grades 6–8 in the region of birth in 1996. Standard errors reported in the parenthesis are adjusted for clustering at the region-of-birth and treatment status.

comfortable. Consistent with the IV estimate, the reduced form estimate of the effect of the class-child ratio is also insignificant.²⁹ Given these results and the finding of zero returns to schooling for middle school grade levels in Turkey (Aydemir and Kirdar, 2017), I conclude that the increase in patient inter-temporal choices observed in Table 4 is likely not to be driven by a change in financial well-being resulting from middle school completion.

6. Discussion and conclusion

In this paper, I use a change in the Turkish compulsory schooling law as a natural experiment

to examine the effect of schooling on time preferences. In 1997, the Turkish government extended compulsory schooling from 5 to 8 years for all children that had not yet finished the 5th grade in the end of the 1996/1997 Academic Year. I find that, on average, the Compulsory Schooling Law raised the probability that an affected child would complete 8 years of schooling by 9.98 percentage points.

Under the assumption that an individual's exposure to the reform is a valid instrument for educational attainment, I estimate the impact of schooling on two distinct measures of time preferences: i) the number of postponement decisions in a widely used inter-temporal choice task with hypothetical rewards, and ii) self-assessed patience. The estimates suggest that having 8 years of schooling increases the number of patient inter-temporal choices but has no impact on self-reported patience. A validation exercise shows that only the number of patient inter-temporal

²⁹ A mediation analysis provides consistent results: adding financial comfort as an additional control does not change the estimated coefficients of education of Table 4.

choices produces correlations that are similar to those in the related work using incentivized procedures. I therefore interpret the identified positive effect of education on patient inter-temporal choices as evidence that education causally affects time preferences.

In an attempt to better understand the mechanism that underlies the identified relationship between education and patience, I investigate the effect of middle school completion on being financially comfortable. I find that being financially comfortable is not causally related to educational attainment. This finding is consistent with the evidence provided by [Aydemir and Kirdar \(2017\)](#) who report zero returns to education at the middle school level in Turkey. Thus, a link between education and patience via increased financial well-being appears unlikely.

Given the data limitations, I cannot investigate the role of the alternative mechanisms that are likely to link education to patience. Recent literature provides suggestive evidence for cognitive abilities as a potential mechanism linking secondary education and patience. An additional secondary school year has been found to increase cognitive function in Sweden ([Falch and Sandgren Massih, 2011](#)), UK ([Gorman, 2023](#)), Norway ([Brinch and Ann Galloway 2012](#)), and in six other European countries ([Schneeweis et al., 2014](#)). Exploiting a broad set of educational reforms across 21 European countries, [Cappellari et al. \(2023\)](#) show that cognitive skill returns to schooling are driven by reforms extending preschool and secondary education. The authors find a small and not statistically effect of an additional year induced by primary school reforms. Further, [Falch and Sandgren Massih \(2011\)](#) examine changes in the distribution of IQ scores at the age of 10 and age 20 by educational attainment and find no significant increase for education levels below 10 years. Considering the existing evidence, it seems rather unlikely that the link between middle school completion and patience operates through increased cognitive abilities, although one should not rule out this mechanism without additional evidence from Turkey. While I cannot directly investigate whether schooling affects patience through focusing the students' attention on the future (see [Becker and Mulligan, 1997](#)), this channel is likely to be relevant for education at any level and might be behind the relationship between schooling and patience found in this paper.

A finding that deserves particular discussion is that the effect of education is significant for one measure of patience and not the other. [Section 3](#) provides two insights that help to explain this result. First, the correlation between the two measures of patience is low. Second, the number of postponement decisions delivers correlations that were similar to prior work using financially incentivized procedures, while self-reported patience does not. These two pieces of evidence suggest that the two measures, while being related, capture different information on a person's true time preference. In particular, respondents' self-assessments of their patience may be subject to a reference bias which arises when respondents rate their patience relative to their individual reference points. In the presence of such bias, self-reported patience measure will capture the respondents' patience relative to their reference points rather than their true patience, giving rise to misleading inferences about the causal effect of education on patience.

My findings have important implications for theoretical studies of economic decision making in general, and for human capital theory in particular. Human capital models consider education as an investment

in the future and assume causality to run from time preferences to schooling, and not the other way around. The schooling-induced increase in the number of patient inter-temporal choices that I observe challenges this view. Together with the evidence indicating that inter-temporal choices of children and adolescents are strong predictors of their educational achievement later in life ([Golsteyn et al., 2014](#); [Mischel et al., 1989](#)), this result implies a causality between schooling and time preferences that works in both directions.

The results also relate to the extensive body of research that studies the causal effects of education on life outcomes ([Oreopoulos and Salvanes, 2011](#)). A causal relationship between schooling and patience can explain why individuals with more education achieve more favorable outcomes in terms of health ([Dursun et al., 2018](#); [Kemptner et al., 2011](#); [Lleras-Muney, 2005](#)), financial outcomes ([Ajayi and Ross, 2020](#); [Cole et al., 2014](#)), and fertility ([Dincer et al., 2014](#); [Kirdar et al., 2018](#)), among other dimensions. Indeed, it has been often hypothesized that patience is a channel through which education affects health-related behaviors ([Fuchs, 1982](#); [Kemptner, et al., 2011](#)), decisions to engage crime ([Hjälmarsson and Lochner, 2012](#); [Lochner and Moretti, 2004](#)), and teenage contraceptive use ([Oreopoulos and Salvanes, 2011](#)). By providing empirical evidence on the causal relationship between education and patience, my findings contribute to our understanding of the mechanisms linking education to economic and social outcomes.

Finally, the results have also important implications for policy. They suggest that time preferences are malleable and that governments can, by extending compulsory schooling, contribute to altering the individuals' willingness to invest throughout their lives, e.g., in financial assets, human capital or health and improve lifetime outcomes. Whether the results are externally valid for policy changes beyond the compulsory education level is an open question for future research.

CRediT authorship contribution statement

Pınar Kunt Šimunović: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Data Availability

Data will be made available on request.

Acknowledgements

I am indebted to Bekir Ağırdir and Eren Pultar for their generous support in collecting the data. I am grateful to Heinrich Ursprung, Urs Fischbacher, Eylem Gevrek, and Luna Bellani for their advice and encouragement throughout this research. I thank the Editor and four anonymous reviewers for helpful comments and suggestions.

Author statement

The author confirms sole responsibility for the following: study conception and design, data collection, analysis and interpretation of results, and manuscript preparation.

Appendix

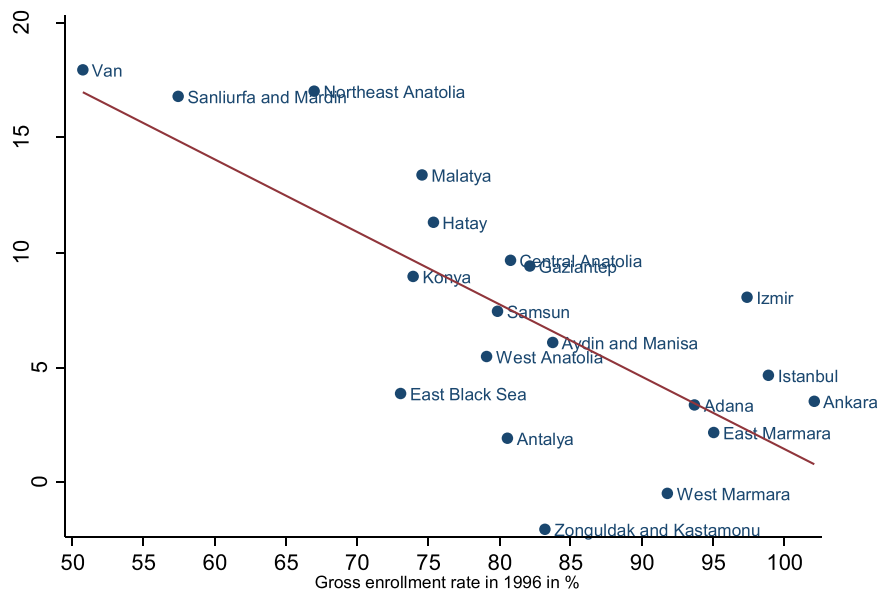


Figure A1. Correlation between the pre-reform primary school (grade 1–8) gross enrollment rate in 1996 and increase in primary school (grade 1–8) classrooms per 1000 children.

Table 8

Hypothetical choice question. Suppose you won a prize of 200 TRY. You have two options. Either you will receive 200 TRY in a year or you will receive an amount less than 200 TRY today. For EACH of the 5 situations I will read you know please indicate the option you choose.

190 TRY today, 200 TRY in one year	<input type="radio"/>	190 TRY today	<input type="radio"/>	200 TRY in one year
180 TRY today, 200 TRY in one year	<input type="radio"/>	180 TRY today	<input type="radio"/>	200 TRY in one year
160 TRY today, 200 TRY in one year	<input type="radio"/>	160 TRY today	<input type="radio"/>	200 TRY in one year
130 TRY today, 200 TRY in one year	<input type="radio"/>	130 TRY today	<input type="radio"/>	200 TRY in one year
90 TRY today, 200 TRY in one year	<input type="radio"/>	90 TRY today	<input type="radio"/>	200 TRY in one year

Table 9

Descriptive statistics

	N	Mean	Std. dev	Min	Max
At least 8 years of schooling	1391	0.775	0.418	0	1
Number of postponement choices	1391	1.398	1.760	0	5
Self-reported patience	1388	3.271	1.221	1	5
Female	1391	0.481	0.500	0	1
Turkish	1391	0.764	0.425	0	1
Kurdish	1391	0.160	0.367	0	1
Other ethnicity	1391	0.044	0.205	0	1
Sunni	1391	0.914	0.280	0	1
Alevi	1391	0.049	0.216	0	1
Other religion	1391	0.037	0.188	0	1

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