Compulsory Schooling Reform and Household Saving Behavior

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Abstract

The development of financial markets is crucial for economic growth and financial stability. In combination with low rates of financial market participation, ever-evolving global and local financial systems raise questions on the determinants of household financial behavior and the types of intervention needed to encourage it. Exploiting a legislative change that increased mandatory schooling from five to eight years in Turkey, we investigate the impact of the reform on household saving decisions. We find that while the reform increased the education level of both household heads and spouses, its effect on household saving behavior is limited. These results suggest the limitation of the scope of policies that only increase the years of schooling for financial market participation and highlight the importance of the inclusion of financial training in education curricula.

Keywords: Compulsory Schooling Law, Financial Markets, Saving, Household Data

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1. Introduction

Individuals around the world have had – to varying degrees – increasingly facilitated access to a globalized financial system. On the one hand, paired with rapid technological development, this transformation has granted opportunities for the continuous creation of new and exciting financial products. On the other hand, individuals are forced to face what amounts to an avalanche of ever-evolving and complicated information. Admittedly, navigation of the financial market has always been a task for the more cognitively endowed; however, the recent increase in the variety and speed of transactions has also increased the barriers to entry into a market where typical households have been historically underrepresented. These facts, along with the instabilities international markets have experienced since the financial crisis of 2008, have shifted the attention of scientists and governments toward the construction of policies promoting financial market participation. A natural target of these policies, which is also expected to provide individuals with the knowledge necessary to make informed financial decisions, is education.

Education and its expansion have always been considered the cornerstone of combined microand macro-economic policymaking. This is not in naught: several studies have found evidence of labor market and other nonpecuniary returns to primary or secondary education, especially when it is the target of institutionally driven policies (Angrist and Krueger, 1991; Oreopoulos, 2006). When it comes to financial behavior, a low level of education has been proposed in the literature as one of the reasons for the low participation of households in the financial market, as it is vitally tied to one's cognitive capabilities (Hanushek and Woessamn, 2008). Agarwal and Mazumder (2013) emphasize the importance of cognitive ability (measured using math test scores) in sound financial decision-making. The mathematical and economic training received with schooling can increase relevant cognitive capacity and, thus, the probability of participating in the financial market. Human capital accumulation has also been linked to higher engagement with riskier assets (Ehrlich, Hamlen and Yin, 2008). From an alternative perspective, because of its potential positive effect on earnings, education is expected to increase the probability of participating in the financial market by creating wealth accumulation opportunities (Cooper and Zhu, 2016).

In this paper, we use data from Turkey to investigate the effect of education-expanding policies on household saving behavior. More specifically, we analyze how the households' saving decisions are impacted by the exposure of household heads to policies that increase compulsory

schooling. The choice of location is deliberate: Turkey has a modern financial market; however, it still registers one of the lowest private saving rates among economies of similar size and has been declining since 1998 (IMF, 2016). As late as 2018, a survey from ING bank found that only 16% of its respondents declared to have any savings, with the majority preferring to keep it 'under the pillow'. This is indicative of a low financial market participation rate, despite national statistics showing that upper secondary educational attainment of the population aged 25 and older has been above 25% since 2008, even reaching as high as 47.1% in 2022 (TÜİK, 2023). For the purposes of this paper, we exploit a change in compulsory schooling laws in 1997, which increased mandatory schooling from five to eight years within a regression discontinuity design (RDD). The reform, known as the 'Basic Education Program', was enacted swiftly and unexpectedly, thus generating an exogenous increase in schooling for those younger than 11 years old in 1997, leaving older ones unaffected. Using a nationally representative micro data set containing detailed information on household background characteristics and their financial market behavior, we first look into the effect of the policy change on educational attainment for the impacted cohort. Then, we examine whether this reform had any impact on our variables of interest.

Our results confirm the well-documented finding that this reform had a significant impact on schooling. We find that the 1997 compulsory schooling law change increases junior high school completion rates for both household heads and spouses by around 6.2 and 11.4 percentage points, respectively, as shown in Figure 1. This impact does not extend to higher forms of education, which is also consistent with the literature.¹ However, the reform has a limited impact on the financial behavior of the households in our sample. We find overall saving behavior to be unaffected by the reform, in both its broad definition and in relation to the households' interaction with the financial institutions in the country (in our case, these are only banks). The only form of saving that is impacted by the 1997 reform is gold holdings, which experience increases calculated as anywhere from 2.7 to 3.9 percentage points, depending on the bandwidth used in the analysis.

Our work contributes to the growing literature that analyses the impact of education reforms on the financial market participation of households. Cole, Paulson and Shastry (2014) is one of the first papers that uses exogenous variation generated by changes in compulsory schooling

¹ See Cesur and Mocan (2018), Dursun, Cesur and Mocan (2018), Akar, Akyol and Okten (2022), Akyol and Mocan (2022).

legislation to measure the relationship between education and financial behavior, highlighting the issue of financial illiteracy and its effect on financial market participation. This aspect of the analysis has been studied extensively: Cole, Paulson and Shastry (2016) find that traditional, high school-level mathematics education encourages financial participation, where targeted financial courses fail. Other studies have found that financially illiterate individuals tend to have little to no retirement plans, borrow more, and hold fewer financial assets (Lusardi and Mitchell, 2007; van Rooij, Lusardi and Alessie, 2011; Lusardi and Tufano, 2015). We add to this literature by analyzing the policy-related aspect of this question in the context of a developing country – Turkey - which has a shallow but quickly developing financial market. Closely related to our study is that of Black, Devereux, Lundborg and Majlesi (2018), who use exogenous variation in compulsory primary education to estimate the impact of additional schooling on equity market participation in Sweden, where they detect decreased risk aversion as a potential channel of increased stock market participation. Earlier unpublished works that study the effect of changes in education laws on financial behavior using household-level data are those of Garsia and Tessada (2013) and Park and Son (2015), who define educational attainment as high school and university completion, respectively, and show that education increases financial market participation.

This study also relates to the extensive literature that uses changes in compulsory schooling laws to determine the impact of education on employment and earnings (Angrist and Krueger, 1991; Harmon and Walker, 1995; Oreopoulos, 2006), health, happiness and social standing (Oreopoulos, 2007; Oreopoulos and Salvanes, 2011; Kemptner, Jürges and Reinhold, 2011; Clark and Royer, 2013), fertility (Black, Devereux and Salvanes, 2008; Cygan-Rehm and Maeder, 2013), cognitive ability (Crespo, Lòpez-Noval and Mira, 2014), criminal activity (Lochner and Moretti, 2004; Machin, Olivier and Vujić, 2011). Moreover, previous studies have used the same reform to establish the impact of changes in compulsory schooling laws on several outcomes of interest in Turkey, including, but not limited to, labor market outcomes (Kırdar, Dayıoğlu and Koç, 2016; Aydemir and Kırdar, 2017; Torun, 2018), religiosity (Cesur and Mocan, 2018), health and happiness (Dursun and Cesur, 2016; Dursun, Cesur and Kelly, 2017; Dursun, Cesur and Mocan, 2018); fertility and gender equality (Dinçer, Kaushal and Grossman, 2014; Kırdar, Dayıoğlu and Koç, 2018), prosocial behavior (Akar, Akyol and Okten, 2022), and consanguineous marriage (Akyol and Mocan, 2022).

The rest of the paper is organized as follows. Section 2 provides a brief overview of the 1997 reform. Section 3 describes our empirical approach, while Section 4 presents our results and robustness analyses. Section 5 discusses potential mechanisms, and Section 6 concludes.

2. The Basic Education Program

The Republic of Turkey has followed a centralized governmental structure since its establishment in 1923. Under this form of governance, the responsibility for the management of the country's educational institutions falls to the Ministry of National Education. Until 1997, compulsory schooling in Turkey consisted of only 5 years of primary education. After its completion, students could choose to drop out or continue their education in general, vocational or religious schools. The Basic Education Law passed in 1997 extended compulsory education in Turkey to 8 years. Under the new law, starting from the 1997-1998 academic year, all students enrolling in the fifth grade would be mandated to continue their education for three more years in order to get a junior high school diploma. Hence, each individual's exposure to the reform was determined by their school starting age, with only those born after January 1987 being impacted by the changes in the legislation. The vast majority of the eligible population did comply with the new law despite the compliance with the age restrictions not being perfect (Kırdar, Dayıoğlu and Koç, 2018).

While the government supported the program to decrease poverty and social disparity, a central motivation behind the policy change was to curb religious education in the country (Aydemir and Kırdar, 2017). The Islamist party had won the parliamentary majority in 1995, and secular groups, mainly in the military and judiciary, introduced a new set of laws to prevent the expansion of Islamist influence. The extension of compulsory schooling was included in the new legislation. In addition, vocational and religious schools at the secondary level were closed, and the traditional diploma rewarded at the end of the fifth year was abolished. Extensive, country-wide funding was provided for the expansion of the education system in both infrastructure and staffing in order to handle the expected increase in active students. This intervention was also independent of the macroeconomics of Turkey at the time; thus, it did not coincide with other policies that would also have an effect on schooling outcomes (Aydemir and Kırdar, 2017).

The Basic Education program had a significant impact on the student population in Turkey. As a result of the reform and the incentives created to promote compliance, enrollment in grades one through eight increased with over 1.1 million students, raising the primary school enrollment rate from 85.63% in 1997 to 96.30% in 2002. The increase in enrollment rates was higher for females than for males, with the enrollment of females in rural areas experiencing an increase of 162% in the first year alone (Dülger, 2004).

The 1997 Basic Education Program brought an unexpected change in compulsory schooling laws that only affected the years of schooling of the impacted cohort and had no impact on the quality of schooling (Aydemir and Kırdar, 2017), as it did not initiate a change in the curricula or the courses taught in schools (Dülger, 2004). The 1997 law change is widely accepted to have exogenously impacted the educational attainment of the exposed cohort and thus, it can be treated as a natural experiment (Angrist and Krueger, 1991; Acemoğlu and Angrist, 2000; Lochner and Moretti, 2004).²

3. Empirical Methodology

3.1 Data and Descriptive Statistics

We use data from the 2021 Family Structure Survey, conducted by the Turkish Statistical Institute jointly with the Ministry of Family and Social Policies, which has information on both the household and individual level for household members. The sample contains responses from 19,428 households and 42,043 individuals aged 15 and older on a variety of questions intended to determine their lifestyle and family structure, including their background information, labor market position, welfare, financial behavior, and marital status. The survey was conducted on a household basis, with household members interviewed privately.

In the individual response part of the survey, members of the household, particularly the household head and their spouse, are asked questions regarding their financial decisions. Specifically, they are asked if i) they have any personal interest income and ii) any real estate income that they have personally earned in the last year. Correspondingly, in the household questionnaire, one representative from each household is asked if the household has any savings, and if the answer is 'YES', polar questions are asked to extract the details. With relation to saving behavior, the household representative is asked whether the household savings are i) kept in a bank

² See Cesur and Mocan (2018), Dursun, Cesur and Mocan (2018), Akar, Akyol and Okten (2022) for examples of using this reform as an instrument for junior high school completion.

account, ii) stocks, iii) bonds, iv) fund certificates or v) gold. Using the responses in the household portion of the survey, we define our outcome variables as dummies that take 1 if the household representative admits to owning the specific asset, and 0 otherwise. In order to avoid issues that may arise from the variation in the compositions of households, we only concentrate on household heads and their spouses (if available) to define our individual-level variables.³

Table 1 reports the descriptive statistics of the household heads and their spouses. The treatment status is defined as exposure to the 1997 reform and is determined by the month and year of birth. As discussed in Section 2, individuals born before January 1987 would have already completed their fifth grade by the summer of 1997 and would have been allowed to drop out under the new law voluntarily. Hence, they make up our control group. The treatment group consists of individuals born after January 1987: those born in 1987 would be the ones to enroll in the fifth grade in the 1997-1998 academic year, making them the first cohort to be impacted by the reform.

In Panel A of Table 1, we present descriptive statistics for the educational outcomes of household heads and their spouses,⁴ by the treatment status of the household head. 82.8% of the household heads in the sample have completed at least junior–high, which entails eight years of schooling. The proportion is larger for the treated sample, which has been exposed to the change in education laws: 90.7% of treated household heads have received their junior-high diploma, compared to 76.6% in the untreated group. The same statistics are somewhat lower for the spouses' subsample, however, the difference in educational attainment between treatment and control groups persists: while 87.1% of spouses of the household heads in the treatment group have received at least eight years of schooling, only 75.0% of the spouses of the untreated household heads have done so.

Panel B of Table 1 reports the statistics for households' saving behavior by the treatment status of the household heads. Households with treated (i.e., exposed to the reform and presumably more educated) household heads seem to save slightly less. Overall, there appears to be little difference between the treatment and control groups for most measures of household financial market participation.

³ Table A.1 in the Appendix includes detailed explanations for each variable.

⁴ The difference in sizes of these two sub-samples is explained by the fact that a portion of the household heads is unmarried, and there are some inconsistencies in the information provided for the spouses' subsample.

In Panel C, we show the summary statistics for the labor market outcomes of the household heads and respective spouses in our sample: notice that there is a large difference between household heads and their spouses in both employment status and having wage income (only 38.7% of spouses are employed versus 80.2% of household heads). This is in line with Turkey's female labor force participation rate, which is reported by the Turkish Statistical Institute to be around 35%, and as we show in Panel D, almost 75% of the sample of the household heads is male, compared to only about 17% of the spouses. In addition, Panel D of Table 1 provides a brief overview of the marriage market outcomes of the individuals in our sample and their ages. Notably, most of the household heads in our sample are in a cohabiting relationship (82.9% is 'married'), with the rate being slightly lower for the treated subsample (82.1% vs. 83.4%). This could also be related to the lower average age of the treated cohort, which decreases their propensity of being married or in a long-term relationship.⁵

3.2 Identification

Our identification strategy exploits the month-year birth cutoff in household head's exposure to the reform. We use the following sharp RDD specification to estimate the reduced-form effect of exposure to the education reform:

$$y_i = \alpha_0 + \alpha_1 T_i + \alpha_2 I (T_i = 0) h(x_i) + \alpha_3 I (T_i = 1) g(x_i) + X_i \Phi + \varepsilon_i$$
(1)

The dependent variable y_i represents the financial market behavior of household *i*. In our main analysis, y_i is an indicator of whether household *i* has any savings and if such operations are conducted through a financial intermediary (i.e., a bank). Alternatively, y_i is a measure of the type of saving declared by household *i*, whether it is in the form of bank accounts, gold, foreign exchange, retirement funds or other financial instruments.

 T_i is our treatment indicator and takes 1 if the head of household *i* was born after the monthyear cutoff of January 1987. I(.) is an indicator function that takes 1 if the expression inside the parenthesis is true (0, otherwise). h(.) and g(.) are functions that capture the time trends in the outcome variable before and after the cutoff date, respectively. The running variable *x* is the month-year of birth, normalized around the cutoff. Hence, Equation (1) accounts for the potential differential trends in the outcomes of both the treatment and the control group.

⁵ See Table A.2 in the Appendix for the mean ages of married household heads.

 X_i is a vector of control variables, used to account for the background characteristics of the head of household *i*. It includes indicators for the month of birth, current region of residence, childhood region of residence, whether their formative years were spent in a rural region, their sex and its interactions with the trend variables, and the interaction between the indicators of childhood region and having been brought up in a rural setting. The aim is to capture the characteristics of the household head which may impact their financial decisions and are shaped by local socio-economic factors and/or earlier exposure to economic incentives or knowledge related to financial markets.

We follow a similar strategy to estimate the impact of the reform on schooling for the household heads in our sample and their spouses, with educational attainment as our dependent variable. The right-hand-side of the equation is identical to that of Equation (1). The dependent variable is an indicator of the educational attainment of individual i, which in this case may be the head of household i or their spouse.⁶ We will use three different definitions of educational attainment in our analysis: having attained at least a junior-high, high school or university diploma.

We use several alternative bandwidths with split linear trends on each side of the cutoff. Starting with a 10-year (i.e., 120-month) bandwidth around the cutoff of January 1987, we methodically narrow the range of observations by decreasing the bandwidth by 12 months in each of the subsequent estimations, allowing us to present results for 6 different choices of bandwidth. The standard errors for these regressions are clustered at the month-year-of-birth level, as suggested by Lee and Card (2008). We also check the robustness of our results using nonparametric approaches and non-linear time trends.

3.3 Preliminary checks

The validity of our estimation strategy depends on the assumption that our outcome distributions are smooth around the cutoff. We test the plausibility of this assumption by conducting two commonly used tests: continuity of the running variable around the cutoff and absence of treatment effects on pre-treatment covariates.

For the score density to be continuous around the cutoff value, the household heads in the sample must not be able to manipulate the running variable in order to be on a particular side of

⁶ The definition of the treatment indicator T_j is adjusted accordingly.

the cutoff. Given the definition of our running variable (month and year of birth), this type of manipulation is unlikely, so we expect the continuity of the score density to hold by default. However, for completeness, we examine the density of our running variable using the test developed by Cattaneo et al. (2018). The results presented in Figure 2 indicate no evidence of such manipulation in our sample, confirming the continuity assumption.⁷

We next check the continuity of the pre-treatment covariates around the cutoff. Random treatment assignment requires that individuals around the cutoff have similar characteristics, which, in turn, implies that the distributions of these characteristics should be continuous around the value of the cutoff. Hence, we need to check whether the reform had any impact on our pre-treatment covariates, which include the household heads' childhood and current region of residence and sex. The results in Table 2 for different bandwidths indicate no evidence of sorting around the cutoff, confirming our continuity assumption.

4. Results

4.1 Schooling outcomes

We start by first examining the schooling effect of the reform. Figure 1 presents the RDD graphs for the household head's and spouses' middle school completion status. Table 3 presents the results of this estimation for the three different definitions of the outcome variable (having completed at least junior high school – i.e., received at least eight years of schooling – high school or university) for the sample of household heads and that of the spouses, using different bandwidths around the cutoff date. The estimates reported in Table 3 suggest that the reform has a significant impact on at least junior high school completion for all individuals in our dataset: depending on our choice of bandwidth, the propensity of having at least a junior-high diploma increased by at least 6.0 percentage points for household heads and 8.3 percentage points for the spouses, with the later predictively having been more affected due to their higher concentration of females.⁸ These results are consistent with what is found in the literature (Kırdar, Dayıoğlu and Koç, 2016; Cesur and Mocan, 2018; Dursun, Cesur and Mocan, 2018; Akar, Akyol and Okten, 2022; Akyol and Kırdar, 2022). Also consistent with the literature, the reform seems to be weakly related to at least

⁷ The difference is insignificant, with p-value 0.866.

⁸ See Panel D in Table 1.

high school and university completion for either subsample, indicated by the weakly significant point estimates in both Panels.

4.2 Saving Outcomes

We now turn our attention to the impact on household financial behavior of the reform. Table 4 reports the results of our estimates from Equation (1) for each household saving measure. The estimation results in Panel A show a lack of the impact of the reform on the households' overall propensity to save, or on doing so through a financial institution, defined as *Any saving* and *Bank saving*, respectively. Our results in Panel B suggest that out of the several financial instruments presented, household heads' exposure to the treatment affects only their households' gold savings (*Gold*). The effect captured here is positive, indicating an increase in gold holdings of 2.7 to 3.9 percentage points, particularly for the estimations with bandwidths narrower than 8 years.⁹

The composition of the sample used in these estimations needs more careful consideration, as it includes a diverse range of households. An important distinction is related to the marital status of the household head. It is not unreasonable to think that there may be intrinsic differences that lead to different preferences on marriage, and also result in dissimilar financial behavior. In our sample, the existence of such unobserved factors is hinted at by the small difference in mean ages between the married and unmarried household heads (0.25 years) and by the disparity in the gender structure of these subsamples: married household heads are predominantly male, while in the unmarried subsample, the distribution of sexes is more even (80.5% vs. 47.5%).¹⁰ Moreover, the 1997 reform has a limited impact on the marriage decisions of the household heads in our sample.¹¹ Therefore, we re-estimate Equation (1) for the married and unmarried subsamples separately.

The results in panels A and B of Table 5 show that there is indeed a difference in how household saving behavior responds to the treatment status of the household head between these two subsamples. While the insignificance of the impact of the reform on our overall saving measures persists here as well, we see that the 1997 reform impacts the gold savings of households with married household heads only, as the same impact cannot be detected for unmarried

⁹ See Figure 4 for a graphical representation of this effect.

¹⁰ See Panel D of Table A.2 in the Appendix.

¹¹ See Table A.3 in the Appendix. We use the term 'marriage' to refer to a cohabiting relationship, even though the members of the couple may not be legally married.

household heads. This conclusion raises questions about the channels through which this change in education policy may influence household saving behavior.

4.4 Robustness Checks

In this section, we present some of the alternative estimations we conducted to investigate the robustness of our results. We begin by changing our model specification to account for the potential existence of quadratic time trends on both sides of the cutoff value. Alternatively, we check the robustness of our original findings using triangular kernels. We report these results in Table 6 and Table 7, respectively. As indicated in these tables, our results are robust to both of these alternative specifications.

We further check the validity of our methodology by examining the absence of treatment effects at placebo cutoff values. We first separate the data into the control (household heads born before January 1987) and treatment (household heads born after January 1987) groups, and then estimate Equation (1) for these subsamples separately at several placebo cutoff values. Table 8 reports the results of the placebo estimations for our overall measures of saving and borrowing (*Any saving* and *Any debt*), and for the form of saving, our baseline analysis detected to be impacted by the reform (*Gold*) at several choices of bandwidth. Out of the 16 different estimations we run for our dependent variables, only one placebo reform causes a significant impact on the overall borrowing propensity of the treated households. However, the significance level does not exceed 5%, and given the ineffectiveness of the other placebo reforms, we are confident in the continuity of our dependent variables.

As a final check, we conduct nonparametric (local polynomial) estimations for all of our outcome variables using the optimal bandwidth selection method proposed by Calonico, Cattaneo, Farrell and Titiunik (Calonico et.al., 2017). Table 9 reports the results of this nonparametric estimation for our main outcome variables. The optimal bandwidths calculated using the CCFT algorithm range from approximately 59 to 74 months around the cutoff date of January 1987 (i.e., 5-6 years). The estimates calculated using this approach are very similar in both significance and magnitude to the results of our baseline estimation: the 1997 reform does not seem to impact the overall saving and borrowing behavior of the households in our sample, but it increases their propensity to hold gold.

5. Potential Mechanisms

There are several channels through which changes in compulsory schooling laws can impact household saving decisions, so it is an interesting exercise to look into the reasons behind the weak impact of such a legislative change on household financial behavior in Turkey. The central component of the impact – or lack thereof – of the policy under investigation on saving behavior in this sample is indeed the increase in schooling. Education can impact financial market participation through its positive effect on overall cognitive ability or by increasing individuals' financial literacy, even only through general mathematics courses (Cole et. al., 2016): the ability to perform numerical calculations and process complicated information is crucial when navigating financial markets (Christelis, Japelli and Padula, 2010; Yoong, 2010), as is the degree of risk aversion and trust in the financial system (Balloch, Nicolae and Philip, 2015; Black et. al., 2018). In the case of Turkey, there is anecdotal evidence that students receive almost no economics or financial illiteracy, which in turn discourages participation in the financial market, and could explain why we fail to capture a stronger impact of the reform of 1997 on the financial behavior of Turkish households.

Confronted with the almost ubiquitous insignificance of our estimates, it becomes even more important for us to understand the few significant results we do find, i.e., the positive impact of the reform on gold savings. A potential transmission mechanism is improved labor market outcomes, either in terms of higher earnings or increased employment of the household heads. However, the results presented in Table 10 put the validity of this explanation into question. In Panel A, we report that the 1997 reform has a negative impact on the probability of employment for the household heads in our sample, and does not significantly affect their probability of earning a regular wage. This result, while surprising, is not out of line with the literature,¹² and makes sense in the context of the economic situation in Turkey at the time of our survey. The 2021 Family Structure Survey was conducted from August to November of 2021, a period in which Turkey was still dealing with the impacts of the Covid19 pandemic and the labor market had yet to recover. For our purposes, this means that we cannot explain the increase in households' gold investments

¹² See Aydemir and Kırdar (2017) for an analysis of the labor market impacts of the 1997 Basic Education Program.

as a simple outcome of the increase in household heads' incomes. It does, however, clarify why we do not see an impact of the reform on the extensive margin.

Alternatively, the reform could impact household savings by improving the labor market outcomes of the spouses. After all, we isolated the positive impact of the reform on gold savings as coming solely from the subsample of married household heads.¹³ If the treatment status of the household head increases their spouse's income or employment probability, then we could be dealing with an improvement in the household's financial position, paving the way for increased gold savings. However, the results in Panel B of Table 10 belie the existence of such a channel. The household head being impacted by the 1997 reform does not have any impact on their spouse's employment propensity or their likelihood of earning a regular wage. We go one step further and check the impact of the reform on the employment probability of either of the members of a couple. As we show in Panel C of Table 10, the lack of impact persists.¹⁴ This result is congruent with the lack of the impact of the reform on overall savings, but it does not explain the reasons behind the increase in gold holdings.

Ultimately, what we could be looking at here is a re-allocation of households' disposable incomes: in 2021, the Turkish Central Bank initiated a policy of lower interest rates, in the hopes of regulating the rising inflation rates. What happened instead was a mass exodus of foreign capital. Turkey entered a period of currency crisis, and the inflation rate ballooned. It is possible that, in the face of economic uncertainty, households end up changing their income distributions by saving using an instrument that is generally regarded – mostly culturally – as a safe way of hedging against inflation risk. This type of behavior could also explain the ineffectiveness of the 1997 reform on the other types of saving: the impact of the 1997 reform that we capture is only at the junior-high level, which can provide an advantage when making simple saving decisions, but arguably does not imply the necessary schooling level for undertaking more complex investments. So, we can argue that while the Basic Education Program increased educational attainment to then positively impact at least a type of saving behavior, it did not do so enough to allow for more

¹³ See results in Table 5.

¹⁴ Moreover, the household head's treatment status does not impact their spouse's educational attainment (see results in Table 11).

sophisticated financial market participation. Unfortunately, the dataset does not provide the necessary information to test this hypothesis.

6. Conclusion

This paper aims to investigate the effects of compulsory schooling reform, which substantially increased educational attainment, on household financial behavior in a developing country – Turkey. We exploit the 1997 compulsory education reform to investigate the relationship between policies that target educational attainment and household financial market participation with respect to several measures of saving.

Our results suggest that the change in education policy in 1997 increased junior high school completion rates of Turkish household heads by at least 6.0 percentage points. We find that there is no effect of this legislative change on household overall saving decisions, but the household head having been impacted by the reform is associated with an increase of at least 2.7 percentage points in the propensity to have savings in gold. These results are preserved for the subsample with married household heads, but we see no impact of the reform on the saving behavior of the "unmarried" subsample. These results are robust to a series of sensitivity tests.

It is important to clarify that our empirical strategy identifies the reduced form effect of the exposure to the reform of the household heads on household financial behavior outcomes, which is the policy-related impact of compulsory schooling laws and is not equivalent to the impact of education. However, the household head's treatment status does not increase the spouse's labor market outcomes or schooling levels.¹⁵ Thus, we can view our analysis as providing suggestive evidence on the nature of the causal relationship between education and household financial decisions. In addition, estimating the impact of this compulsory schooling reform on financial outcomes is of independent interest: the reform of 1997 had a larger impact on the educational outcomes of particularly disadvantaged groups (Dülger, 2004), so any income-relevant externality of this policy is of value to the policy maker.

¹⁵ See Panel B in Table 10 and Table 11.

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Figures and Tables



Figure 1: Proportion of individuals with at least junior high school degree



Figure 2: McCrary density test of discontinuity around the cutoff



Figure 3: Household saving behavior outcomes by birth cohort of household heads



Figure 4: Types of saving by birth cohort of household heads

		Heads			Spouses	
	All	Treatment	Control	All	Treatment	Control
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Mean	Mean	Mean	Mean	Mean
	Sd.	Sd.	Sd.	Sd.	Sd.	Sd.
A – Educational outcomes						
Junior high school	0.828	0.907	0.766	0.803	0.871	0.750
	(0.378)	(0.290)	(0.423)	(0.398)	(0.335)	(0.433)
High school	0.611	0.642	0.586	0.548	0.596	0.512
	(0.488)	(0.479)	(0.493)	(0.498)	(0.491)	(0.500)
University	0.367	0.413	0.332	0.336	0.387	0.297
	(0.482)	(0.493)	(0.471)	(0.472)	(0.487)	(0.457)
B – Saving						
Any saving	0.166	0.166	0.167			
	(0.372)	(0.372)	(0.373)			
Bank saving	0.042	0.038	0.046			
-	(0.201)	(0.190)	(0.208)			
Bank account	0.034	0.034	0.035			
	(0.182)	(0.182)	(0.183)			
Gold	0.060	0.067	0.055			
	(0.238)	(0.250)	(0.228)			
C – Labor market outcomes						
Employed	0.802	0.799	0.804	0.387	0.379	0.393
	(0.399)	(0.401)	(0.397)	(0.487)	(0.485)	(0.489)
Wage earner	0.693	0.698	0.689	0.339	0.328	0.347
-	(0.461)	(0.459)	(0.463)	(0.473)	(0.470)	(0.476)
Private sector	0.212	0.230	0.198	0.136	0.142	0.132
	(0.409)	(0.421)	(0.399)	(0.343)	(0.349)	(0.339)
D – Individual characteristics						
Age	34.83	31.46	37.45	32.36	29.40	34.61
-	(3.462)	(1.762)	(1.788)	(4.984)	(4.088)	(4.391)
Male	0.749	0.751	0.747	0.174	0.186	0.164
	(0.434)	(0.433)	(0.435)	(0.379)	(0.390)	(0.371)
Married	0.829	0.821	0.834	```	``´´	` '
	(0.377)	(0.383)	(0.372)			
Observations	4213	1840	2373	3378	1459	1919

Table 1: Descriptive statistics (by treatment status of household heads)

Notes: Detailed explanations on the definitions of the variables can be found in Table A.1 in the Appendix.

	Number of months around the cutoff						
	(1)	(2)	(3)	(4)	(5)	(6)	
	120	108	96	84	72	60	
Childhood region: Village	0.027	0.035	0.027	0.026	0.024	0.020	
	(0.022)	(0.024)	(0.024)	(0.026)	(0.028)	(0.030)	
Observations	6579	6077	5494	4909	4277	3587	
Male	0.015	0.013	0.010	0.007	-0.003	0.009	
	(0.020)	(0.021)	(0.022)	(0.023)	(0.026)	(0.028)	
Observations	7033	6500	5879	5254	4570	3831	
Region 1	-0.012	-0.009	-0.007	-0.010	0.001	-0.011	
	(0.016)	(0.016)	(0.018)	(0.019)	(0.020)	(0.020)	
Region 2	-0.002	-0.011	-0.008	-0.012	-0.012	-0.011	
	(0.011)	(0.011)	(0.012)	(0.012)	(0.013)	(0.014)	
Region 3	-0.009	-0.010	-0.021	-0.026	-0.029	-0.023	
	(0.016)	(0.016)	(0.017)	(0.018)	(0.019)	(0.020)	
Region 4	-0.017	-0.014	-0.020	-0.015	-0.011	-0.003	
	(0.014)	(0.014)	(0.015)	(0.016)	(0.017)	(0.019)	
Region 5	0.026*	0.024	0.037**	0.036**	0.039**	0.042**	
	(0.016)	(0.016)	(0.017)	(0.018)	(0.019)	(0.020)	
Region 6	-0.000	-0.008	-0.013	-0.005	-0.013	-0.008	
	(0.015)	(0.015)	(0.016)	(0.017)	(0.018)	(0.020)	
Region 7	-0.019*	-0.014	-0.008	-0.011	-0.015	-0.022*	
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)	
Region 8	-0.000	0.001	0.000	-0.005	-0.005	-0.006	
	(0.010)	(0.011)	(0.011)	(0.012)	(0.013)	(0.014)	
Region 9	0.007	0.011	0.014	0.018	0.016	0.018	
	(0.009)	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)	
Region 10	-0.007	-0.002	-0.006	-0.010	-0.019	-0.019	
	(0.010)	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)	
Region 11	0.008	0.007	0.013	0.017	0.019	0.016	
	(0.011)	(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	
Region 12	0.025	0.025	0.021	0.024	0.030	0.028	
	(0.018)	(0.019)	(0.020)	(0.022)	(0.022)	(0.024)	
Observations	7033	6500	5879	5254	4570	3831	

Table 2: Effect of household head's treatment exposure on pre-treatment covariates

Notes: Detailed explanations on the definitions of the variables can be found in Table A.1.

		Num	ber of month	s around the	cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)
	120	108	96	84	72	60
A – Household head	ds					
Junior high school	0.068***	0.060***	0.065***	0.060***	0.062**	0.044
	(0.020)	(0.020)	(0.022)	(0.023)	(0.025)	(0.027)
High school	0.006	-0.007	0.002	0.002	0.001	-0.006
	(0.021)	(0.021)	(0.024)	(0.024)	(0.025)	(0.028)
University	0.026	0.008	0.004	0.008	-0.005	-0.011
	(0.022)	(0.024)	(0.025)	(0.027)	(0.028)	(0.029)
Observations	6478	5983	5414	4836	4213	3535
B – Spouses						
Junior high school	0.137***	0.134***	0.119***	0.111***	0.114***	0.083***
	(0.017)	(0.018)	(0.018)	(0.019)	(0.020)	(0.023)
High school	0.065***	0.056**	0.043*	0.046*	0.047	0.010
	(0.023)	(0.024)	(0.025)	(0.026)	(0.029)	(0.031)
University	0.056**	0.040*	0.013	0.008	-0.003	-0.023
	(0.022)	(0.023)	(0.023)	(0.024)	(0.024)	(0.024)
Observations	5982	5543	5032	4427	3820	3209

Table 3: Effect of the 1997 Basic Education Program on educational attainment

Notes: Columns (1) to (6) report the effect of the reform on educational attainment, measured from estimations conducted using separate samples defined according to the specified bandwidths. Panel A reports the results of these estimations for the sample of household heads. Panel B reports the results of these estimations for the sample of spouses. The dependent variables are *Junior high school*, *High school* and *University*, which are dummies that take 1 if the individual has completed the implied level of schooling. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.

		Nu	mber of mor	ths around th	e cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)
	120	108	96	84	72	60
A – Overall saving						
Any saving	0.002	-0.001	0.012	0.016	0.021	0.015
	(0.018)	(0.018)	(0.019)	(0.020)	(0.020)	(0.023)
Bank saving	-0.000	-0.000	0.006	0.007	0.004	0.009
	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)	(0.012)
B – Types of saving						
Account	0.007	0.008	0.014*	0.015	0.011	0.016
	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)
Retirement	0.005	0.008	0.007	0.010	0.015**	0.016**
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)
Real estate	-0.005	-0.006	-0.010	-0.008	-0.005	-0.008
	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)
Gold	0.017	0.018	0.027**	0.037***	0.039***	0.037***
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.014)
Foreign currency	0.001	-0.000	0.001	0.005	0.011	0.013
	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)
Domestic currency	-0.007	-0.005	-0.000	-0.001	-0.004	0.000
	(0.013)	(0.013)	(0.014)	(0.015)	(0.016)	(0.018)
Observations	6478	5983	5414	4836	4213	3535

Table 4: Effect of household head's treatment exposure on household saving

Notes: Columns (1) to (6) report the effect of the exposure to the reform of the household head on household saving behavior, measured from estimations conducted using separate samples defined according to the specified bandwidths. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.

		Numb	er of months	around the	cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)
	120	108	96	84	72	60
A – Married sample						
Any saving	0.013	0.013	0.030	0.035	0.042*	0.045*
	(0.019)	(0.020)	(0.020)	(0.022)	(0.022)	(0.026)
Bank saving	-0.001	-0.000	0.005	0.006	0.002	0.010
	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)	(0.011)
Gold	0.027**	0.030**	0.039***	0.048***	0.048***	0.044***
	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.015)
Observations	5233	4857	4428	3978	3491	2958
B – Unmarried samp	le					
Any saving	-0.040	-0.066	-0.069	-0.070	-0.086	-0.131**
	(0.047)	(0.048)	(0.050)	(0.054)	(0.056)	(0.062)
Bank saving	0.001	-0.004	0.004	0.005	0.008	-0.014
	(0.027)	(0.029)	(0.029)	(0.032)	(0.037)	(0.041)
Gold	-0.021	-0.033	-0.034	-0.017	-0.022	-0.004
	(0.031)	(0.034)	(0.037)	(0.038)	(0.041)	(0.047)
Observations	1245	1126	986	858	722	577

Table 5: Effect of household head's treatment exposure on household saving (by marital status)

Notes: Columns (1) to (6) report the effect of the exposure to the reform of the household head on household saving behavior, measured from estimations conducted using separate samples defined according to the specified bandwidths. Panel A reports the results for the 'Married' subsample, which includes household heads that declare to be in a marital or long-term cohabitating relationship. Panel B reports the results for the 'Unmarried' subsample. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.

Table 6: Results with quadratic trends

		Number of months around the cutoff							
	(1)	(2)	(3)	(4)	(5)	(6)			
	120	108	96	84	72	60			
Any saving	0.003	-0.000	0.012	0.015	0.022	0.016			
	(0.018)	(0.018)	(0.019)	(0.020)	(0.020)	(0.023)			
Bank saving	0.001	0.001	0.007	0.006	0.004	0.009			
	(0.008)	(0.009)	(0.009)	(0.010)	(0.011)	(0.011)			
Gold	0.019*	0.019	0.028**	0.037***	0.040***	0.039***			
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.013)			
Observations	6478	5983	5414	4836	4213	3535			

Notes: Columns (1) to (6) report the effect of the exposure to the reform of the household head on household financial behavior, measured from estimations conducted using separate samples defined according to the specified bandwidths. Panel A reports the results for our saving measures. Panel B reports the results for our borrowing measures. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy – and an additional control to account for the marital status of the household head. Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively. Source: 2021 Family Structure Survey Table 7: Results with triangular kernel weights

		Number of months around the cutoff							
	(1)	(2)	(3)	(4)	(5)	(6)			
	120	108	96	84	72	60			
Any saving	0.013	0.016	0.019	0.021	0.019	0.022			
	(0.018)	(0.018)	(0.019)	(0.019)	(0.020)	(0.021)			
Bank saving	0.004	0.006	0.006	0.006	0.007	0.007			
	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)			
Gold	0.029***	0.033***	0.037***	0.039***	0.039***	0.040***			
	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)			
Observations	6420	5910	5333	4757	4119	3435			

Notes: Columns (1) to (6) report the effect of the exposure to the reform of the household head on household financial behavior, measured from estimations conducted using separate samples defined according to the specified bandwidths. Panel A reports the results for our saving measures. Panel B reports the results for our borrowing measures. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.

		Nun	nber of months	s around the c	utoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Cutoff	120	108	96	84	72	60
A – Any sav	ving					
Jan. '83	0.024	0.024	0.021	0.015	0.020	0.018
	(0.021)	(0.021)	(0.022)	(0.023)	(0.024)	(0.025)
Jan. '84	-0.002	-0.006	-0.014	-0.011	-0.015	-0.008
	(0.023)	(0.023)	(0.025)	(0.025)	(0.025)	(0.027)
Jan. '85	-0.026	-0.033	-0.031	-0.034	-0.028	-0.028
	(0.033)	(0.032)	(0.034)	(0.033)	(0.033)	(0.035)
Jan. '86	-0.011	-0.006	-0.003	0.004	0.010	0.006
	(0.020)	(0.020)	(0.022)	(0.024)	(0.027)	(0.029)
B-Gold						
Jan. '83	0.000	-0.003	-0.003	-0.008	-0.008	-0.009
	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)
Jan. '84	-0.002	-0.002	-0.006	-0.006	-0.004	-0.000
	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.017)
Jan. '85	-0.006	-0.009	-0.007	-0.005	-0.001	0.006
	(0.018)	(0.018)	(0.019)	(0.018)	(0.019)	(0.020)
Jan. '86	-0.003	-0.001	0.003	0.010	0.019	0.012
	(0.016)	(0.016)	(0.016)	(0.017)	(0.018)	(0.019)

Table 8-1: Placebo cutoff values: Control group

Notes: Columns (1) to (6) report the effect of the exposure to the reform of the household head on household financial behavior, measured from estimations conducted using separate samples defined according to the specified bandwidths under different hypothetical cutoff values. The sample consists of individuals born before the original cutoff January 1987. Panels A and B report the results for our saving and borrowing measures. Panel C reports the results for savings in the form of gold holdings. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.

		Num	ber of month	s around the	cutoff	
_	(1)	(2)	(3)	(4)	(5)	(6)
Cutoff	120	108	96	84	72	60
A – Any saving						
Jan. '88	0.007	-0.000	-0.015	-0.014	-0.025	-0.025
	(0.043)	(0.044)	(0.044)	(0.047)	(0.047)	(0.046)
Jan. '89	0.055	0.053	0.049	0.042	0.045	0.049
	(0.035)	(0.036)	(0.037)	(0.036)	(0.036)	(0.038)
Jan. '90	0.038	-0.014	0.039	0.037	0.029	0.034
	(0.031)	(0.037)	(0.031)	(0.032)	(0.032)	(0.031)
Jan. '91	0.012	0.008	0.009	0.005	0.003	-0.008
	(0.030)	(0.030)	(0.030)	(0.030)	(0.031)	(0.031)
B – Gold						
Jan. '88	0.012	0.007	0.002	0.009	0.001	0.001
	(0.023)	(0.023)	(0.024)	(0.025)	(0.024)	(0.025)
Jan. '89	0.004	0.001	-0.004	-0.007	-0.003	-0.001
	(0.025)	(0.025)	(0.025)	(0.025)	(0.026)	(0.026)
Jan. '90	0.004	0.002	-0.002	-0.008	-0.014	-0.009
	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)	(0.020)
Jan. '91	0.006	0.006	0.002	-0.003	-0.010	-0.020
	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)	(0.018)

Table 8-2: Placebo cutoff values: Treatment group

Notes: Columns (1) to (6) report the effect of the exposure to the reform of the household head on household financial behavior, measured from estimations conducted using separate samples defined according to the specified bandwidths under different hypothetical cutoff values. The sample consists of individuals born after the original cutoff January 1987. Panels A and B report the results for our saving and borrowing measures. Panel C reports the results for savings in the form of gold holdings. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.

Table 9: Nonparametric estimation results

		Variables	
	(1)	(2)	(3)
	Any saving	Bank saving	Gold
Conventional	0.010	0.000	0.028***
	(0.019)	(0.010)	(0.010)
Bias-corrected	0.012	0.001	0.028***
	(0.019)	(0.010)	(0.010)
Robust	0.012	0.001	0.028**
	(0.023)	(0.012)	(0.011)
BW loc. poly	58.80	66.45	63.32
BW bias	86.79	99.98	90.07

Notes: Columns (1) to (6) report the effect of the exposure to the reform of the household head on household financial behavior, measured from nonparametric estimations conducted using the optimal bandwidth calculated by the CCFT algorithm. CCFT bandwidths are MSE-optimal and the degree of local polynomials is one (two for bias correction). Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.

		Nur	nber of mont	hs around the	e cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)
	120	108	96	84	72	60
A - Household hea	ıds					
Employed	-0.018	-0.027*	-0.033**	-0.037**	-0.048***	-0.041**
	(0.014)	(0.014)	(0.015)	(0.016)	(0.018)	(0.018)
Wage earner	-0.023	-0.028	-0.039**	-0.043**	-0.044**	-0.029
	(0.018)	(0.019)	(0.020)	(0.020)	(0.022)	(0.024)
Private sector	0.004	-0.004	-0.008	-0.008	-0.014	-0.011
	(0.018)	(0.018)	(0.019)	(0.020)	(0.021)	(0.024)
Interest income	-0.004	-0.006	-0.010*	-0.007	-0.004	-0.002
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)
Observations	6478	5983	5414	4836	4213	3535
B - Spouses						
Employed	0.014	0.018	0.025	0.020	0.018	0.045
	(0.021)	(0.021)	(0.022)	(0.024)	(0.025)	(0.031)
Wage earner	-0.016	-0.016	-0.009	-0.008	-0.008	0.006
	(0.023)	(0.024)	(0.025)	(0.028)	(0.028)	(0.037)
Private sector	0.013	0.005	0.003	0.005	0.000	0.004
	(0.018)	(0.019)	(0.020)	(0.022)	(0.023)	(0.029)
Interest income	-0.001	-0.001	-0.002	-0.002	-0.002	-0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)
Observations	5056	4377	3994	3591	3147	2172
C - Either						
Employed	-0.003	-0.007	-0.002	0.001	-0.003	-0.003
	(0.014)	(0.015)	(0.015)	(0.015)	(0.016)	(0.018)
Observations	5,233	4,857	4,428	3,978	3,491	2,958

Table 10: Effect of household head's treatment exposure on labor market outcomes

Notes: Notes: Columns (1) to (6) report the effect of the reform on labor market outcomes, measured from estimations conducted using separate samples defined according to the specified bandwidths. Panel A reports the results of these estimations for the sample of household heads. Panel B reports the results of these estimations for the sample of spouses. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.

		Number of months around the cutoff							
_	(1)	(2)	(3)	(4)	(5)	(6)			
	120	108	96	84	72	60			
Junior high school	0.025	0.020	0.011	-0.003	-0.005	0.021			
	(0.016)	(0.017)	(0.019)	(0.020)	(0.021)	(0.022)			
High school	0.015	-0.000	-0.009	-0.024	-0.022	-0.013			
	(0.023)	(0.024)	(0.026)	(0.027)	(0.027)	(0.030)			
University	0.048*	0.038	0.037	0.024	0.013	0.001			
	(0.025)	(0.026)	(0.028)	(0.029)	(0.030)	(0.034)			
Observations	5056	4698	4289	3853	3378	2862			

Table 11: Effect of household head's treatment exposure on spouse's education

Notes: Columns (1) to (6) report the effect of the exposure to the reform of the household head on their spouse's educational attainment, measured from estimations conducted using separate samples defined according to the specified bandwidths. The dependent variables are *Junior high school*, *High school* and *University*. *Junior high school*, *High school* and *University*. *Junior high school*, *High school* and *University* are dummies that take 1 if the individual has completed the implied level of schooling. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.

Appendix

Table A.1: Variables definitions

VARIABLE	DEFINITION					
	INDIVIDUAL LEVEL VARIABLES					
Junior high school	A dummy variable that takes the value 1 if the household head and/or spouse has completed at least junior high school (8 years of schooling); 0, otherwise.					
High school	A dummy variable that takes the value 1 if the household head and/or spouse has completed at least high school (12 years of schooling); 0, otherwise.					
University	A dummy variable that takes the value 1 if the household head and/or spouse has completed at least university (16 years of schooling); 0, otherwise.					
Childhood-Village	A dummy variable that takes the value 1 if the respondent spent the first 15 years in a rural area; 0, otherwise.					
Treatment	A control variable determined by the dummy variable "Reform", which takes 1 if the respondent is in the treatment group; 0, otherwise.					
Control	A control variable determined by the dummy variable "Reform", which takes 1 if the respondent is in the control group; 0, otherwise.					
Employed	A dummy variable that takes the value 1 if the household head and/or spouse is employed in any capacity in the last week; 0, otherwise.					
Wage earner	A dummy variable that takes the value 1 if the household head and/or spouse's main income is in the form of a salary or wage taken in the last year; 0, otherwise.					
Private sector	A dummy variable that takes the value 1 if the household head and/or spouse is employed in the private sector; 0, otherwise.					
Male	A dummy variable that takes 1 if the household head and/or spouse identifies as male; 0, otherwise					
Married	A dummy variable that takes the value 1 if the household head and/or spouse is either married or cohabiting; 0, otherwise.					
Unmarried	A dummy variable that takes the value 0 if the household head and/or spouse is married; 1, otherwise.					
	HOUSEHOLD LEVEL VARIABLES					
Any saving	A dummy variable that takes the value 1 if the household representative admits that the respective household has saving in any form; 0, otherwise.					
Bank saving	A dummy variable that takes the value 1 if the household representative admits that the respective household has saving in either a bank account, stocks, bonds or funds; 0, otherwise.					
Account	A dummy variable that takes the value 1 if the household representative admits that the respective household has saving in a bank account; 0, otherwise.					

Continues in the next page

Retirement	A dummy variable that takes the value 1 if the household representative admits that the respective household has saving in a
	private pension; 0, otherwise.
	A dummy variable that takes the value 1 if the household
Real estate	representative admits that the respective household owns real estate;
	0, otherwise.
	A dummy variable that takes the value 1 if the household
Gold	representative admits that the respective household has saving in
	gold; 0, otherwise.
	A dummy variable that takes the value 1 if the household
Foreign currency	representative admits that the respective household has saving in
2	foreign currency; 0, otherwise.
	A dummy variable that takes the value 1 if the respondent admits
Domestic currency	that the respective household has savings in the form of domestic
-	currency; 0, otherwise.

	Married			Unmarried		
	All	Treatment	Control	All	Treatment	Control
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Mean	Mean	Mean	Mean	Mean
	Sd.	Sd.	Sd.	Sd.	Sd.	Sd.
A – Educational outcomes						
Junior high school	0.826	0.903	0.768	0.834	0.924	0.758
-	(0.379)	(0.296)	(0.422)	(0.373)	(0.265)	(0.429)
Highschool	0.600	0.621	0.583	0.665	0.742	0.601
-	(0.490)	(0.485)	(0.493)	(0.472)	(0.438)	(0.490)
University	0.345	0.375	0.323	0.474	0.590	0.377
-	(0.476)	(0.484)	(0.468)	(0.500)	(0.493)	(0.485)
B – Saving		· · ·			· · · ·	
Any saving	0.162	0.157	0.165	0.190	0.207	0.176
	(0.368)	(0.364)	(0.371)	(0.392)	(0.406)	(0.381)
Bank saving	0.039	0.032	0.043	0.058	0.061	0.056
	(0.193)	(0.177)	(0.204)	(0.234)	(0.239)	(0.230)
Bank account	0.032	0.030	0.033	0.047	0.052	0.043
	(0.175)	(0.172)	(0.178)	(0.212)	(0.222)	(0.204)
Gold	0.059	0.066	0.054	0.065	0.073	0.059
	(0.236)	(0.248)	(0.226)	(0.247)	(0.260)	(0.235)
C – Labor market outcomes						
Employed	0.813	0.796	0.825	0.751	0.815	0.697
	(0.390)	(0.403)	(0.380)	(0.433)	(0.389)	(0.460)
Wage earner	0.690	0.687	0.693	0.704	0.748	0.667
	(0.462)	(0.464)	(0.461)	(0.457)	(0.435)	(0.472)
Private sector	0.206	0.212	0.201	0.244	0.313	0.186
	(0.404)	(0.409)	(0.401)	(0.430)	(0.464)	(0.389)
D – Individual characteristics						
Age	34.88	31.55	37.42	34.63	31.06	37.61
	(3.404)	(1.743)	(1.787)	(3.722)	(1.796)	(1.790)
Male	0.805	0.793	0.815	0.475	0.559	0.405
	(0.396)	(0.405)	(0.388)	(0.500)	(0.497)	(0.491)
Observations	3491	1511	1980	722	329	393

Table A.2: Descriptive statistics (by marital status of the household head)

Notes: Detailed explanations on the definitions of the variables can be found in Table A.1.

	Number of months around the cutoff							
	(1)	(2)	(3)	(4)	(5)	(6)		
	120	108	96	84	72	60		
Treatment	0.060***	0.056***	0.043**	0.036*	0.031	0.034		
	(0.017)	(0.018)	(0.018)	(0.018)	(0.019)	(0.021)		
Observations	6478	5983	5414	4836	4213	3535		

Table A.3: Effect of household head's treatment exposure on their marital status

Notes: Columns (1) to (6) report the effect of the exposure to the reform of the household head on their marital status, measured from estimations conducted using separate samples defined according to the specified bandwidths. The dependent variable *Married* is defined as the household head being in a cohabiting relationship. *Treatment* is a dummy variable that indicates whether the household head has been exposed to the 1997 reform. Control variables include a dummy for having spent the first 15 years in a village, an indicator variable for the male sex, childhood and current residence fixed effects and their interactions, control and treatment time trends and their interaction with the male dummy.

Standard errors are clustered at month-year-of-birth level.

*, **, *** denote significance levels of 10%, 5% and 1%, respectively.