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ABSTRACT

This article exploits the rollout of the 1997 Basic Education Program in Turkey, a reform that extended compulsory school attendance, to estimate the causal effects of education on women's awareness of laws designed to reduce gender inequality and prevent domestic violence. The study implements a regression-discontinuity design and finds that the additional years of schooling improved women's legal awareness. Women exposed to the education reform were more likely to have heard about the new laws and services through newspapers, journals, or books. However, despite these improvements, the study finds no evidence of a significant change in the risk of women experiencing domestic violence or their ability to quit abusive relationships.

KEYWORDS

Legal knowledge, information acquisition, education, domestic violence, regression discontinuity, Turkey

JEL Codes: J12, J16, I25

HIGHLIGHTS

- Multiple barriers undermine women's access to the justice system in Turkey.
- Education reform helped remove one such barrier: women's legal awareness of their rights.
- However, legal awareness did not translate into significant changes in incidence of domestic violence.
- Raising awareness is a necessary first step but not enough to empower women to access to legal institutions.

INTRODUCTION

Intimate partner violence is a worldwide problem with major social, economic, and health consequences. Nearly one out of every three women

suffers from physical or sexual intimate partner violence in their lifetimes. In developing countries, where women generally have weak bargaining power within the household, such intimate partner violence is even more prevalent.[1] To reduce violence against women, many governments in developing countries have implemented comprehensive legal reforms with the goal of legally empowering women.[2] Some of these reforms have entailed not only the introduction of new laws to provide protection, accommodation, and financial support for abused women but also the establishment of new institutions designed to provide legal services to the victims of violence.[3] Such legal reforms also have the potential to change how women interact with the legal system when they experience domestic violence.[4]

Although legislative reform has accelerated in recent decades, multiple barriers still undermine women's access to the justice system.[5] One major barrier that is often cited is women's limited knowledge about their rights and of the ways in which laws enforce those rights. A common presumption among development practitioners is that the acquisition of information about new legal reforms is a precondition for improving women's access to the justice system and increasing their bargaining power within the household.

Indeed, building inclusive justice systems was recently included among the "Sustainable Development Goals" of the United Nations to ensure gender equality (UN 2017).[6] However, to date, there is no causal evidence in the existing literature to support this hypothesis.

In this article, we examine whether increasing women's educational attainment improves legal awareness of laws designed to prevent domestic violence. Ultimately, however, knowledge is a means to an end. Therefore, we also assess whether such knowledge acquisition has consequences for related outcomes, including the ability to quit an abusive relationship and the risk of experiencing domestic violence. Improved education for women may enhance their ability to access and process new information on recently introduced laws and services. In turn, an improvement in legal awareness may increase women's bargaining power within the household and reduce their exposure to intimate partner violence. To the best of our knowledge, our study provides the first causal evidence on the impact of education on legal knowledge and its consequences for domestic violence.[7] Since the early 2000s, Turkey has made substantial changes in laws pertaining to gender equality and the prevention of domestic violence. These changes were partly motivated by the process of Turkey's negotiations on accession to the European Union (EU; Aldikacti-Marshall 2008). For example, in 2004, the penal code recognized sexual violence against women as a criminal act against an individual and extended the definition of rape to include rape within marriage. In 2012, the Turkish parliament introduced the Law to Protect Family and Prevent

Violence against Women, which improved the protection of victims against renewed violence from perpetrators by improving the capabilities of police to take effective action. In addition, it regulated the establishment of Violence Prevention and Supervision Centers (VPSCs) to provide legal and financial support and new shelters to accommodate women and children experiencing domestic violence.[8]

We refer to these recent changes in the gender-equality legislation as "new" laws and services to distinguish them from the preexisting laws and services that date back to the 1920s.[9] We exploit the rollout of the 1997 Basic Education Program in Turkey, which increased compulsory school attendance from five to eight years, to study the impact of increased exposure to education on women's awareness of these legal reforms and domestic violence outcomes. We employ the 2014 National Survey on Domestic Violence against Women in Turkey (NSDVW 2014), which includes information on women's legal knowledge as well as different forms of intimate partner violence. Our dataset is novel in its coverage of both indicators of women's legal knowledge on specific topics and the particular outcomes that legal reforms were intended to regulate.

Since unobservable characteristics such as ability, socioeconomic status, and upbringing may affect both education and legal knowledge, simple correlations that do not account for reverse causality or omitted variable bias may be misleading. To isolate the causal impact of education on legal knowledge in the Turkish context, we employ a regression-discontinuity (RD) design that allows us to estimate a meaningful treatment effect by comparing legal awareness indicators for younger women exposed to the reform and older women who were not exposed to it. As the required age for beginning junior high school in Turkey is 12, the extension of compulsory schooling in 1997 implied that individuals born after January 1987 were required to complete eight years of education while older cohorts born before January 1987 could drop out after five years (Cesur and Mocan 2014; Dinçer, Kaushal, and Grossman 2014). Our identifying assumption is that these two cohorts, born one month apart, exhibit no systematic differences other than whether they were exposed to the education reform.

We find that the reform induced an average increase of roughly one year of additional schooling for women. Our main finding suggests that the reform-induced increase in women's schooling improved women's legal awareness of new laws and services designed to reduce gender inequalities and prevent domestic violence. Moreover, we find no evidence of a significant impact of the education reform on women's knowledge of preexisting laws, dating back to 1920s, related to gender equality and domestic violence. These findings imply that having additional years of schooling provides women with an advantage in acquiring new information about recently introduced laws and services, while preexisting laws almost a

century old are more likely to be common knowledge by 2014. We also find that the reform had a significant impact on the source from which women acquired information about new laws and services. In particular, we find that the reform had a positive impact on having heard about the new laws and services through newspapers, journals, or books. In contrast, we find no significant effect of the reform on other sources of information, including TV, family and friends, and formal institutions.

Despite the substantial improvement in women's legal awareness of laws and services concerning gender equality and domestic violence, we find no evidence indicating that the reform had a significant impact on domestic violence measures or divorce-related outcomes. In particular, we find no evidence of a significant effect on physical violence, sexual violence, psychological violence, or financial control behavior exercised by intimate partners. We also find no evidence that the reform had a significant impact on ever leaving home after experiencing domestic violence, ever getting divorced, or having a second marriage.

One possible threat to our identification strategy might arise from the use of self-reported data on domestic violence outcomes and legal awareness. If more-educated women are more or less likely to report violent episodes or their knowledge of laws and services, the results might suffer from reporting bias.[10] Although we cannot rule out this possibility, any such bias is likely to be limited in this context for two reasons. First, we find no evidence that women's education had a significant impact on their gender attitudes, as shown in Table A4 in the Online Appendix. In particular, we find no evidence that additional years of schooling had a significant impact on women's likelihood of reporting that they approve of intimate partner violence in certain situations. Second, unlike domestic violence or other more sensitive topics, the subject of legal awareness is not a socially stigmatized issue. As such, any social desirability bias attributable to reporting legal awareness is likely to be minimal. Our study is related to several strands of the existing literature. First, our work contributes to the prior research that has explored the relationships between education and political knowledge, civic understanding, and engagement. Thomas S. Dee (2004) finds that education has a significant effect on both voting behavior and civic awareness in the United States. Similarly, Kevin Milligan, Enrico Moretti, and Philip Oreopoulos (2004) show that higher education predicts higher political awareness and interest by exploiting compulsory schooling laws in the United States and the United Kingdom. In a developing country context, Willa Friedman et al. (2016) find that an increase in women's schooling in Kenya led to an increase in political knowledge and reduced acceptance of political authority among young women.

Our work is also closely related to the earlier studies suggesting a causal relationship between education and individuals' acquisition of new information as well as their ability to actively exploit this new information

to their own benefit. The most notable line of this body of work investigates the relationship between education and health outcomes. Adriana Lleras-Muney and Frank R. Lichtenberg (2002) propose that more-educated people have better health outcomes as a result of being more willing and able to use newer health technologies.

Similarly, Sherry Glied and Adriana Lleras-Muney (2008) show that more-educated people have better chances of survival in cases of disease with more technological progress in treatment. However, David M. Cutler and Adriana Lleras-Muney (2006) underscore an important fact: better ability to reach or process knowledge does not always translate into action. Thus, education does not always guarantee necessary changes in actual health behavior. Our findings are in line with those of the education-health literature: while education plays an important role in improving legal knowledge, which is a first step toward raising awareness of women's rights, it may not be enough to change the overall well-being of women if knowledge cannot guarantee access to legal institutions.

Our study is also related to the extended literature on the causal effects of compulsory schooling laws on returns to education in the labor market (Angrist and Krueger 1991), health outcomes (Lleras-Muney 2005; Clark and Royer 2013), fertility behavior (Black, Devereux, and Salvanes 2008; McCrary and Royer 2011), and other outcomes. Previous studies have examined the effects of the same 1997 compulsory schooling reform on other outcomes of interest in Turkey. These studies include, but are not limited to, Resul Cesur and Naci H. Mocan (2014) and Selim Gulesci and Erik Meyersson (2012), who find a negative effect of the reform on women's religiosity; Mehmet Alper Dinçer, Neeraj Kaushal, and Michael Grossman (2014) and Pinar Mine Güneş (2016), who find a negative effect on fertility and child mortality; Abdurrahman Aydemir and Murat G. Kirdar (2017) and Murat G, Kırdar, Meltem Dayıoğlu, and İsmet Koç (2018), who find an increase in women's earnings and a decline in teenage marriage; Bilge Erten and Pinar Keskin (2018), who find an increase in the psychological violence; and Erten and Keskin (2020), who find a decline in the intergenerational transmission of violence against children.[11] Although our findings complement these studies, our paper differs significantly through its focus on legal knowledge and its relationship to domestic violence outcomes.

More specifically, this study is closely related to our earlier work on education and the prevalence of domestic violence in Turkey. However, by investigating an alternative and potentially important channel – women's awareness of laws intended to empower them – this paper contributes to our understanding of the effectiveness of compulsory schooling in a context of significant limitations on women's rights. First, using an earlier version of the same data source – the 2008 TNSDVW – Erten and Keskin (2018) demonstrate that increased women's education led

to an improvement in the labor market outcomes of women (that is, those younger than 21 years old in 2008) and the increase in women's personal income generated incentives for male partners to use violence as an instrument for extracting rents from women. However, in subsequent work (Erten and Keskin 2020), using the current dataset (2014 NSDVW), we find no evidence of a significant impact of women's education on the labor market outcomes of women (that is, those younger than 27 years old in 2014), given the completed fertility outcomes and increase in childcare responsibilities for this age group. As a result, the underlying mechanism for the greater exposure to psychological violence and financial control behavior is no longer present. Hence, we find no change in these indicators of intimate partner violence for this age group (Erten and Keskin 2020).

Altogether, the findings from our previous work on the effects of the reform on labor market outcomes and the prevalence of domestic violence imply that since the reform did not significantly change the employment of women in their late 20s, it did not result in an increase in violent behavior towards these women. In the absence of labor-market-induced effects of increased schooling on domestic violence, we would expect that if legal awareness improves women's bargaining power, we would observe a significant reduction in the degree of violence they face from their partners. The fact that we found no such effect suggests that improvements in legal awareness do not always translate into the empowerment of women within the household.

THE CONTEXT

Compulsory schooling reform in Turkey

We study the consequences of the compulsory schooling reform that took place in Turkey in 1997. Prior to the reform, the Turkish education system consisted of five years of primary school, three years of junior high school, and three years of high school. Only the first five years of primary school education were compulsory, and the two higher levels were voluntary. In 1997, the Turkish parliament introduced Law No. 4306, which extended compulsory schooling to eight years, merging primary and junior high school into primary education. The law effectively eliminated the option to attend religious junior high schools and replaced the primary school diploma awarded at the end of fifth grade with a primary education diploma received upon successful completion of the eighth grade. The law was passed on August 16, 1997. It was referred to as the Basic Education Program, and it applied to all students who did not already have a primary school diploma at the beginning of the 1997–98 school year.

According to the Turkish law on school starting age, a child begins compulsory schooling in September of the year when he or she turns six

years old. The 1997 Basic Education Program, which made eight years of primary education compulsory, was effectively implemented in the 1997–98 school year. This implied that students who had completed the fifth grade in 1997 could drop out while those who had completed fourth grade in 1997 were required to continue school through eighth grade. The combination of the school starting age law and the 1997 Basic Education Program implied that children born before January 1987 could drop out after five years whereas those born after January 1987 had to complete eight years of education. Although there may have been cases that do not strictly fit this rule, due to either imperfect compliance with the age of starting school or grade repetition, the official requirements were such that students born after January 1987 were more likely to comply with the new compulsory schooling law than were older cohorts.[12] The introduction of the 1997 Basic Education Program allows us to isolate the effect of compulsory schooling from other policy changes that may have occurred during this period, as one has no reason to expect other policy changes to differentially affect individuals born before or after January 1987.

The Basic Education Program required massive investments in schooling infrastructure, which increased the share of education in the public investment budget from 15 percent in 1997–37 percent in 1998. Referred to as a "big bang" approach to education reform, the program entailed the restoration of old schools and the construction of new schools, the hiring of 103,000 additional teachers (a 41 percent increase) and the construction of 80,000 new classrooms (a 36 percent increase) between 1996 and 2003. The Turkish government also sought to improve computer literacy by distributing more than 56,000 computers to rural primary schools. A standardized bus system was implemented in 2000 to transport students from rural areas to nearby schools, and a program was established to distribute free books and meals to low-income students. Overall, the Basic Education Program successfully increased enrollment in primary education. The net schooling ratio rose from 84.74 percent in 1997 to 93.54 percent in 2000. The enrollment of girls increased substantially, and the ratio of girls to boys in primary and secondary education rose from 90 percent in 1995 to 97 percent in 2005.

DATA AND EMPIRICAL METHODOLOGY

Data

We use data from the National Survey on Domestic Violence against Women in Turkey (NSDVW) of 2014. This survey is a nationally representative household survey containing information on the respondents' experiences of domestic violence, their educational background and demographics, and their knowledge of the laws and services designed for

the victims of domestic violence as well as their sources of information about these laws and services. One of the main objectives of the survey was "to identify problematic areas regarding the legal regulations for combating violence (especially problems regarding the implementation of Law No. 6284 on the Protection of the Family and the Prevention of Violence against Women)" (Yuksel-Kaptanoglu and Tarim 2015). Hence, to assess the effectiveness of the laws designed to prevent domestic violence, the survey contained specific questions about women's legal awareness and the sources of information through which they gained such awareness.

The survey was conducted among 15,072 households between April and July 2014. The survey covered women between 15 and 59 years of age, and one woman per household was randomly selected for the interview. Special effort was made to avoid having anyone else in the room while the interviews were conducted. The respondents were informed that their answers would be kept strictly confidential. The survey also includes the birth month and year of each respondent, which allows us to use an RD approach. In Online Appendix C, we present Table A1 to provide detailed information about summary statistics on the major indicators of women from the 2014 NSDVW survey. We construct measures of women's exposure to intimate partner violence within the last twelve months in order to examine whether women's awareness about the recent laws, particularly the 2012 Law to Prevent Domestic Violence, had any significant impact on their experience of domestic violence. Online Appendix C provides detailed information on the construction of indices used in the analysis.

Identification

The combination of the 1997 compulsory schooling law and the law on school starting age implies that individuals born after January 1987 were required to complete eight years of schooling whereas those born earlier could drop out after five years, as explained earlier. We exploit this discontinuity in an RD design to estimate the causal effect of education on awareness of new laws and services pertaining to gender equality and the prevention of domestic violence. Our identifying assumption is that these two cohorts born one month apart do not exhibit any systematic differences other than being exposed to the compulsory schooling law or not. As long as this assumption holds, this approach represents a treatment assignment that is as good as random. In our RD design, we assign treatment based on an individual's month and year of birth, implying that those born after January 1987 are assigned to treated status.

We employ an RD design by exploiting discontinuity in the birth date and using this discontinuity as an instrument for years of schooling following the previous research (Clark and Royer 2013; Erten and Keskin 2018). We report both reduced-form (RF) estimates (that is, sharp RD) and two-stage

least-squares estimates (that is, fuzzy RD) for all of the outcome variables of interest. Our specification follows a basic RD form:

$$Y_i = \alpha + \beta t_i + f(x_i) + \varepsilon_i \qquad (1)$$

$$\forall x_i \in (c - h, \ c + h)$$

Where y_i is the dependent variable, t_i is the treatment status, x_i is the forcing variable, and h is the bandwidth around the cutoff point c. We allow the slope to vary on each side of the cutoff. The control function, $f(x_i)$, is a continuous n-order polynomial function of the forcing variable on each side of the cutoff point. We use local linear regressions in our RD estimations (Imbens and Lemiuex 2008) and conduct optimal bandwidth selection using the Guido Imbens and Karthik Kalyanaraman (2009) procedure.[13] This approach implies the selection of an optimal bandwidth for each outcome variable examined.[14] Following David S. Lee and David Card (2008), we cluster standard errors at the month-year-of-birth level to accommodate specification error in the forcing variable.

In all of our specifications, we include the following control variables: a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, childhood-region fixed effects, and interactions of childhood-region fixed effects with an indicator for rural childhood regions.[15]

Preliminary checks

We test the validity of our RD design by using two standard checks (Imbens and Lemiuex 2008). First, we examine whether the density of the forcing variable, the month-year of birth, is continuous at the discontinuity. We perform a McCrary density test on the density of the forcing variable. This test yields an insignificant estimate, as shown in Figure A1, available in the Online Appendix.

Second, we investigate whether the predetermined covariates are balanced around the discontinuity. In Figure 1, each graph illustrates local averages of the outcome in one-month bins plotted against the forcing variable, with overlaid linear regression lines using raw data on each side of the threshold. The gray lines represent 95 percent confidence intervals. The predetermined characteristics include regional dummy variables capturing whether the respondent's childhood region is western, southern, central, northern, or eastern Turkey and whether the respondent's interview language is not Turkish. The graphs do not indicate any significant jumps at the cutoff point. We also report regression-based tests of whether the control variables exhibit any significant jumps at the discontinuity. In Online Appendix D, Table A2 shows that the control
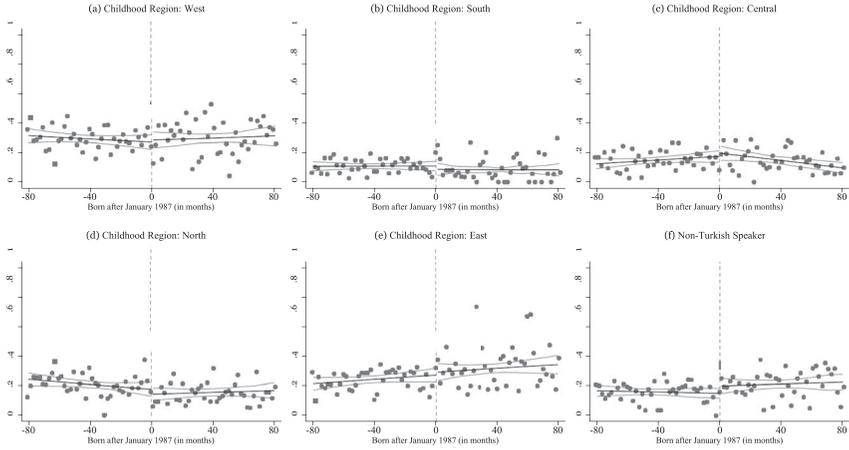
Figure 1 Balanced covariates
Notes: Data are from the 2014 NSDVW. The figures plot predetermined covariates in monthly bins against the month-year of birth of being born in January 1987. The vertical line in each graph represents the cutoff point, January 1987. Gray lines show 95 percent confidence intervals around the mean level. Variable definitions are listed in Online Appendix A.

variables do not exhibit any evidence of a statistically significant jump at the discontinuity, with the exception of one childhood region. A SUR test of the coefficients' joint significance results in a *p*-value of 0.21. Overall, we conclude that the predetermined covariates appear to be balanced around the discontinuity.

Because all of the domestic violence-related questions are relevant only to women who have been in a relationship, our RD analysis for these outcomes is based on the sample of women who have had a relationship. One concern is the extent to which the treatment had an effect on relationship status and, therefore, on selection into the sample of analysis. To address this concern, we examine whether the reform had a significant effect on ever having a relationship or on ever being married. Figure A2 in the Online Appendix plots the local averages of these outcomes in monthly bins against the forcing variable. We find some evidence of a trend break driven by an increasing decline in the propensity of women ever having a relationship in the younger sample. However, since there is no evidence of a discontinuous jump around the cutoff, we conclude that the reform had no significant impact on the probability of ever having a relationship or being married. In Online Appendix D, Table A2 indicates no evidence of a significant effect of the reform on relationship status. The estimated coefficients are also null. Hence, there is no reason to expect that the
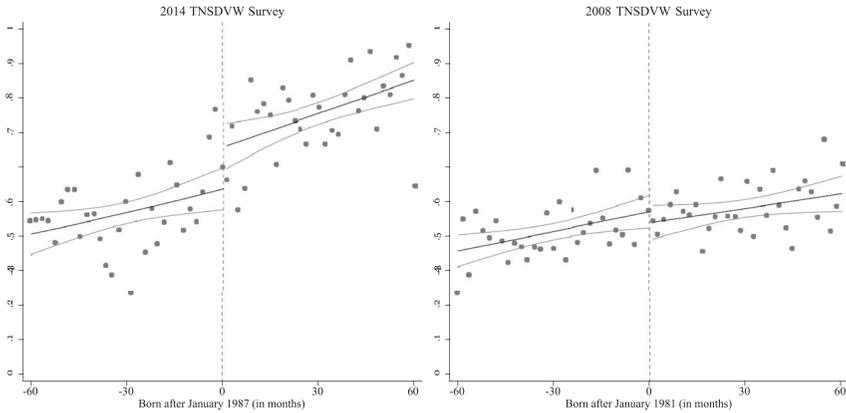
Figure 2 RD treatment effects on junior high school completion: treatment and placebo
Notes: Data are from the 2014 and 2008 NSDVW, respectively. The figures plot a dummy variable equal to one of the respondent completed junior high school in monthly bins. Gray lines show 95 percent confidence intervals around the mean level.

reform affected the probability of selection into the sample of women who have had a relationship or were ever married.[16]

EFFECTS OF THE COMPULSORY SCHOOLING LAW

Schooling outcomes

We proceed by testing the effect of the compulsory schooling law on schooling outcomes. Figure 2 provides a graphical illustration of the RD design by comparing the treatment and placebo effects using the 2014 and 2008 NSDVW surveys. The left-hand graph plots women'sjunior high school completion rates in monthly bins against the month and year of birth, with a threshold of January 1987, using the 2014 NSDVW survey. As explained earlier, the education reform required those born after this date to complete junior high school, while those in the older cohorts had the option of dropping out after completing primary school. The left-hand graph shows a clear jump at the discontinuity, with an approximately 15–20 ppt increase in the probability of completing junior high school. Using data from the 2008 NSDVW, we conduct a placebo test to examine the validity of the RD design. The right-hand graph of Figure 2 shows the same relationship using the 2008 HLFS survey with the same age cutoff, comparing 27- and 28-year-old women. The age cutoff corresponds to being born before or after January 1981. The right- hand graph shows no evidence of a break in completing junior high school for women of the

same age in the 2008 NSDVW data. Thus, the jump that we observe around the discontinuity of the reform implementation in the 2014 survey is not likely to be driven by some underlying relationship between age and school completion but is rather an outcome of the reform.

While the RD graphs reveal a positive treatment effect of being exposed to the compulsory schooling reform on educational attainment, the results could be further refined with regression analysis. Table 1 presents the RD treatment effects on years of schooling and the completion of different levels of education for all of the women surveyed in the 2014 NSDVW. The last column in each row reports the outcome means for the relevant sample. Column 4 shows the optimal bandwidth estimated by the Imbens and Kalyanaraman (2009) algorithm in months on each side of the discontinuity. In all specifications, we include as controls a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, childhood-region fixed effects, and interactions of childhood-region fixed effects with an indicator for rural childhood regions.

The first row of Table 1 reports estimates of the RD treatment effects on women's years of schooling. The optimal bandwidth, calculated using the Imbens and Kalyanaraman (2009) algorithm, is eighty-nine months around the discontinuity. Using a local linear specification, column 1 reports an RD estimate of 0.70 years for the treatment effect on years of schooling, which is statistically significant at the 5 percent level. In terms of magnitude, an increase of 0.70 years in the years of schooling corresponds to an 8.3 percent increase relative to the mean. To check for robustness, we add alternative specifications by allowing the bandwidth to vary and report the linear RD estimates with 0.75 and 1.5 times the optimal bandwidth in columns 2 and 3, respectively. The estimated effects remain significant within an approximate range of 0.7 to one year.

The remaining rows of Table 1 report the RD treatment effects on different levels of school completion. The second row displays the estimated RD treatment effects for an indicator variable capturing whether the respondent completed junior high school or higher. The linear specification with the optimal bandwidth in column 1 reports an RD estimate of 19 ppt corresponding to 32 percent relative to the mean. In alternative specifications, the estimates remain significant. The third row indicates that the linear RD estimate of the treatment effect on completing high school is 13 ppt, and it remains significant in alternative specifications. This finding implies that the reform had long-term effects in enabling some women to continue beyond junior high school. As expected, all RD estimates for whether the respondent completed primary school are insignificant.

Table 1 RD treatment effects of the reform on schooling outcomes

	(1) Linear RD ĥ bandwidth	*(2)* Linear RD 0.75ĥ bandwidth	*(3)* Linear RD 1.5ĥ bandwidth	*(4)* Bandwidth	*(5)* N	*(6)* Mean
Years of schooling	0.704**	0.769**	1.031***	89	2,492	8.48
	(0.327)	(0.365)	(0.267)			
Completed education: Junior high school	0.192***	0.186***	0.186***	118	3,308	0.60
	(0.031)	(0.037)	(0.027)			
High school	0.125***	0.081*	0.078**	65	1,837	0.40
	(0.044)	(0.048)	(0.038)			
Primary school	− 0.020	− 0.031	− 0.020	93	2,630	0.91
	(0.024)	(0.028)	(0.020)			

Notes: Data are from the 2014 NSDVW. Columns 1–3 report local linear regressions using the optimal bandwidth ĥ, 0.75 and 1.5 ĥ, respectively. The optimal bandwidth, reported in column 4, is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 5 reports the number of observations used in estimations with the optimal bandwidth ĥ, and column 6 reports the outcome mean within the optimal bandwidth ĥ. All results are reported for the full sample of women. The variables are described in Online Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

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These results for primary school completion provide a robustness check, showing that the reform did not influence the likelihood of completing primary school, which was already mandatory prior to 1997.[17]

Education and legal awareness

One of the major benefits of education is that additional years of schooling reduce the costs of obtaining new information and using it. Theodore W. Schultz (1975) shows that additional years of schooling improve information acquisition and learning. More-educated individuals gain not only the ability to acquire new information by having access to multiple sources of information but also the ability to process that information to make better decisions. Without adequate levels of education, it is generally difficult for individuals to process and act upon new information, even if it is accessible. Studies in the education-health literature have demonstrated that more-educated individuals more quickly employ new medical technologies (Glied and Lleras-Muney 2008) and utilize new public information for making better health decisions (Aizer and Stroud 2010). In a similar vein, education may enhance the ability to acquire information about changes in laws and services related to gender equality and improve the ability to process this new legal knowledge to make better decisions. One of the implications is that the more educated may gather information about new laws and services more quickly compared to the less educated while there may not be differential effects on the awareness of preexisting legislation by education level.

We test whether the compulsory schooling reform had a significant impact on legal awareness of gender equality and the prevention of domestic violence. Figure 3 plots the local averages of the two summary indices of women's legal awareness of laws in month-of-birth bins around the discontinuity, January 1987. The graph on the left shows no evidence of a significant break in the awareness of preexisting laws index whereas the right-hand graph shows evidence of a clear jump in the awareness of new laws index. This finding about the preexisting laws is not surprising as these laws are almost a century old and probably *common knowledge*; thus, there is no reason why more educated women should be able to know about them more. While these graphs illustrate a positive RD treatment effect on legal awareness of new laws with no evidence of a significant effect for legal awareness of preexisting laws, the results can be elaborated with regression analysis.

Table 2 reports the RD estimates of the effect of the compulsory schooling reform on women's knowledge of laws and services. In Panel A, the OLS estimates in column 1 indicate the presence of a positive correlation between years of schooling and indicators of awareness of preexisting laws. The magnitudes of the correlations suggest that one
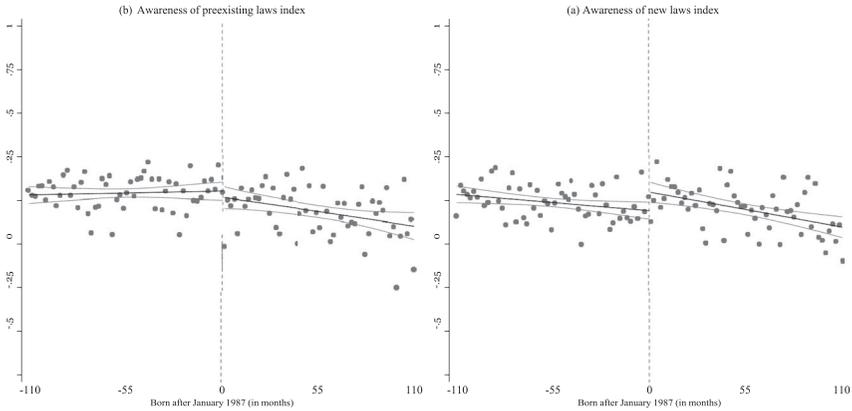
Figure 3 RD treatment effects on information about laws
Notes: Data are from the 2014 NSDVW. The figures plot awareness of preexisting laws index and awareness of new laws index in monthly bins. Gray lines show 95 percent confidence intervals around the mean level.

additional year of schooling corresponds to a 0.7 ppt higher probability of having heard about the law stating that men and women must be at least 17 years old to get married, a 0.3 ppt higher probability of being aware that it is illegal for parents and other relatives to allow their children below the age of 15 to be married and that doing so is punishable by imprisonment, a 0.3 ppt higher probability of being aware that it is illegal to have a religious marriage before an official one, and a 0.8 ppt higher likelihood of knowing that polygamy is illegal. Similarly, the correlations show that one additional year of schooling corresponds to a 0.8 ppt higher probability of having heard about the law stating that the perpetrator of violence is kept away from the common residence to protect the victims of violence, a 1.8 ppt higher likelihood of being aware that the perpetrator is prevented from approaching or contacting the victims of violence, and a 1.1 ppt higher likelihood of knowing that if the victim's life is in danger, the victim is provided temporary protection services.

The RD estimates in Panel A of Table 2 in columns 2 and 3 show no evidence of a significant effect of the reform on awareness of any of the preexisting laws. We also find no evidence that the reform had a significant impact on the awareness of preexisting laws index, a summary measure that aggregates information from individual components of preexisting laws.

In Panel B of Table 2, the OLS estimates in column 1 indicate that one additional year of schooling is positively associated with a 0.5 ppt higher probability of being aware that marital rape is illegal, a 1.6 ppt higher likelihood of knowing that a woman is not required to have her husband's permission to work outside the home, a 0.8 ppt higher likelihood of being

Table 2 Effects of education on information about laws and services

	(1) OLS	(2) RF	(3) IV	(4) Bandwidth	(5) N	(6) Mean
Panel A: Preexisting laws						
Men and women must be at least 17 years old to get married.	0.007***	− 0.034	− 0.043	99	2,761	0.88
	(0.002)	(0.025)	(0.038)			
It is illegal for parents and other relatives to allow their children below the age of 15 to be married, and doing so is punishable by imprisonment.	0.003**	0.003	0.003	131	3,606	0.92
	(0.001)	(0.019)	(0.019)			
It is illegal to have a religious marriage before official marriage.	0.003*	0.001	0.001	138	3,765	0.83
	(0.002)	(0.026)	(0.024)			
It is illegal for a man to marry more than one woman.	0.008***	0.001	0.001	129	3,580	0.86
	(0.002)	(0.023)	(0.024)			
The perpetrator is kept away from the common residence to protect the victims of violence.	0.008***	0.001	0.001	108	3,003	0.88
	(0.002)	(0.024)	(0.029)			
The perpetrator is prevented from approaching or contacting the protected victims of violence.	0.018***	− 0.032	− 0.026	157	4,162	0.80
	(0.002)	(0.027)	(0.024)			
If the victim's life is in danger, the victim is provided temporary protection services.	0.011***	− 0.016	− 0.018	122	3,399	0.87
	(0.002)	(0.022)	(0.025)			
Awareness of preexisting laws index	0.023***	− 0.026	− 0.026	131	3,630	0.02
	(0.003)	(0.035)	(0.037)			

(*Continued*).

Table 2 Continued.

	(1) OLS	(2) RF	(3) IV	(4) Bandwidth	(5) N	(6) Mean
Panel B: New laws						
(a) 2001 Civil Code and 2005 Penal Code						
It is illegal for a man to have sex with his wife without her consent.	0.005** (0.002)	0.060* (0.034)	0.061* (0.036)	126	3,503	0.72
A woman is not required to have her husband's permission to work outside the home.	0.016*** (0.003)	0.092** (0.040)	0.132* (0.078)	94	2,630	0.45
After spouses get divorced, they share the property that they obtained during their marriage.	0.008*** (0.001)	− 0.008 (0.021)	− 0.009 (0.022)	126	3,503	0.92
(b) 2012 Law to Prevent Domestic Violence Shelters are provided for people who experienced domestic violence and their children.	0.011*** (0.002)	0.039* (0.023)	0.048* (0.028)	111	3,100	0.86
If the perpetrator is addicted to alcohol or drugs, he is provided with diagnosis and treatment opportunities.	0.003 (0.003)	0.095*** (0.031)	0.085*** (0.033)	142	3,880	0.59
The victim of violence is provided with childcare services.	− 0.004 (0.003)	0.062* (0.036)	0.074 (0.049)	116	3,246	0.38

Table 2 Continued.

	(1) OLS	(2) RF	(3) IV	(4) Bandwidth	(5) N	(6) Mean
Services are provided to victims of violence by the Violence Prevention and Supervision Centers (VPSC).	0.003 (0.002)	0.055*** (0.020)	0.049** (0.021)	166	4,280	0.12
The victims of violence are provided temporary financial support.	−0.004 (0.003)	0.055 (0.039)	0.051 (0.039)	138	3,785	0.48
The perpetrator is prevented from selling the shared home.	0.003 (0.003)	0.062 (0.040)	0.076 (0.052)	111 137	3,096 3,765	0.43 −0.01
Awareness of new laws index	0.012*** (0.003)	0.110*** (0.036)	0.102*** (0.038)			

Notes: Data are from the 2014 NSDVW. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth ĥ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 and 3 report reduced-form (RF) RD treatment effects and two-stage least-squares (IV) RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. The variables are described in Online Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

aware that spouses share the property that they obtained during marriage after they get divorced, and a 1.1 ppt higher probability of knowing that shelters are provided for victims of domestic violence and their children.

The RD estimates in columns 2 and 3 of Panel B in Table 2 indicate that the reform had a positive impact on the awareness of new laws. The RF RD estimate in column 2 of the last row shows that the reform had a positive and significant impact of 11 ppt on the awareness of new laws index. The IV estimate in column 3 is consistent with the RF estimate, and both the RF and IV estimates are precisely estimated, being significant at the 1 percent level.[18] In terms of magnitudes, the RD treatment effect on awareness of new laws index corresponds to 0.21 standard deviations.

Moreover, the RD estimates for several specific articles listed in Panel B of Table 2 are also significant and positive. The first-row RF estimates in column 2 indicate that the reform had a positive impact of 6 ppt on the likelihood of being aware of the amendment to the 2005 Penal Code that marital rape is illegal, which corresponds to a 8.3 percent increase relative to the sample mean. The IV estimate in column 3 is also precisely estimated and confirms this effect. In the second row, the RD estimates indicate that the reform had a significant and positive effect of 9.2 ppt on legal awareness of the amendment to the 2001 Civil Code stating that a woman is not required to have her husband's permission to work outside the home. In terms of magnitude, this corresponds to a 20 percent increase relative to the sample mean.

Regarding awareness related to laws on domestic violence, the RD estimates show that the reform had a significant impact of 3.9 ppt on being aware of an article introduced in the 2012 Law to Prevent Domestic Violence, which states that shelters are provided for people who experience domestic violence and their children. This corresponds to a 5 percent increase relative to the sample mean. Similarly, the RD estimates in the fifth row indicate that the reform had a positive effect of 9.5 ppt on increasing awareness of other articles of the same law asserting that if the perpetrator is addicted to alcohol or drugs, he is provided with diagnosis and treatment opportunities. This corresponds to a 16 percent increase relative to the sample mean. In the sixth row, the RD estimates show that the reform had a positive impact of 6.2 ppt on having heard about the provision that the victim of violence is provided with childcare services, corresponding to a 16 percent increase relative to the mean. Finally, the RD estimates in the seventh row show that the reform had a large and significant impact on one of the most distinguishing features of the 2012 Law to Prevent Domestic Violence. The RF estimates in column 2 indicate that the reform had a positive impact of 5.5 ppt on being aware that services are provided to the victims of violence by the VPSCs corresponding to a 46 percent increase relative to the sample mean. Overall, the large magnitudes of these RD treatment effects imply that the reform was particularly successful in raising

women's awareness of several new laws and services introduced in the post-2000 period to reduce gender inequalities and prevent domestic violence in Turkey.

Another question of interest that we explore is whether the reform had a significant effect on sources of information through which women gained an awareness of the new laws and services. Figure 4 provides a graphical illustration of the RD treatment effects on having heard about new laws from different sources. At the discontinuity in the upper-left graph, we see no evidence of a significant jump in having heard about the new laws from TV index. However, in the upper-right graph of Figure 4, we see clear evidence of a significant upward shift in having heard about the new laws from newspapers, journals, or books index. In contrast, the bottom graphs of Figure 4 show no evidence of a significant jump at the discontinuity for having heard about the new laws from family and friends index or the having heard about the new laws from formal institutions index.

To examine the effects of the reform on sources of information about new laws in a more refined analysis, we provide the results of RD regressions in Table 3. The OLS estimates in column 1 indicate the presence of a positive correlation between years of schooling and the indices of having heard about the new laws from newspapers, journals, or books and from formal institutions and a negative correlation between years of schooling and having heard about the new laws from family and friends (social networks) index. The magnitudes of the correlations imply that one additional year of schooling corresponds to a 7.9 ppt higher likelihood of having heard about the new laws from newspapers, journals, or books, a 3.9 ppt higher likelihood of having heard about them from formal institutions, and a 1.8 ppt lower likelihood of having heard about them from family and friends. In contrast, we find no evidence of a significant correlation between education and receiving information about new laws from TV, which is unsurprising because TV usage is very widespread across all educational groups and socioeconomic backgrounds in Turkey.

The RD results in the first row of Table 3 show no evidence of a significant effect of the reform on having heard about the new laws from TV. In the second row, the RF estimate indicates that the reform had a significant and positive impact of 13.6 ppt on having heard about the new laws from newspapers, journals, or books, corresponding to 0.15 standard deviations of the outcome. The IV estimate is also consistent with this finding and is marginally significant. The RD estimates in the subsequent rows indicate no evidence of a significant effect of the reform on having heard about the new laws from friends and family or from formal institutions.

As a robustness check, Table A5 in Online Appendix D reports the RD estimates using a static bandwidth of 118 months around the cutoff, which is the optimal bandwidth estimated for junior high school completion. The findings in the table show that the RD estimates for awareness of the new
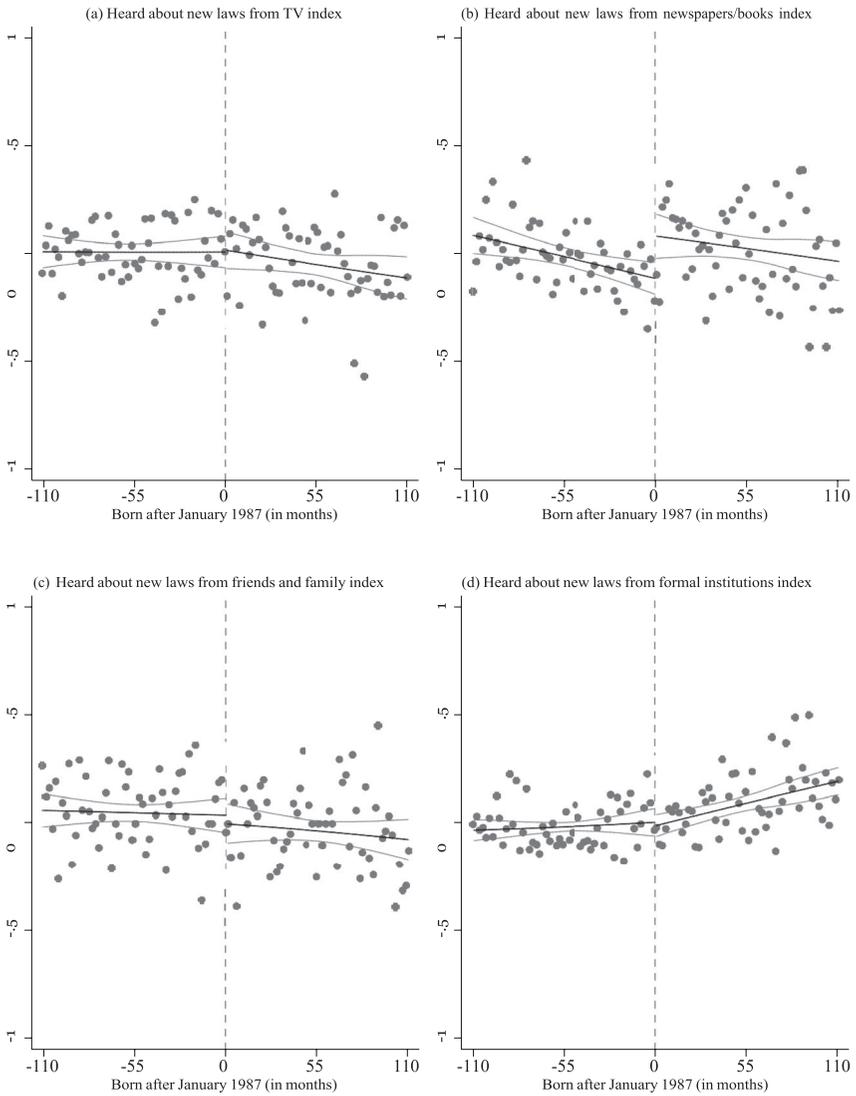
Figure 4 RD treatment effects on sources of information about new laws
Notes: Data are from the 2014 NSDVW. The figures plot z-score indices of having heard about the new laws from TV, newspapers/journals/books, friends and family, and formal institutions in monthly bins. Gray lines show 95 percent confidence intervals around the mean level.

laws index and having heard about the new laws from newspapers, journals, or books index are robust to this specification. The RD results for the awareness of preexisting laws index and indices of having heard about the

Table 3 Effects of education on sources of information about new laws and services

	(1) OLS	(2) RF	(3) IV	(4) Bandwidth	(5) N	(6) Mean
Heard about the new laws and services from the following sources: TV	− 0.000 (0.005)	− 0.035 (0.058)	− 0.046 (0.079)	114	3,165	− 0.01
Newspapers, journals, or books	0.079*** (0.005)	0.136** (0.069)	0.162* 0.162*	116	3,250	0.05
Friends and family (social networks)	− 0.018*** (0.005)	− 0.031 (0.062)	− 0.031 (0.060)	134	3,709	0.01
Formal institutions	0.039*** (0.004)	− 0.022 (0.037)	− 0.025 (0.045)	120	3,331	0.02

new laws from TV, friends and family, and formal institutions also remain insignificant in this specification.

Altogether, our results provide evidence that additional years of schooling allowed women to acquire information about new laws and services pertaining to gender equality and domestic violence through reading newspapers, journals, or books. We also find no evidence of a significant impact of education on awareness of preexisting laws and services dating back to the 1920s. In the next section, we explore whether the reform-induced improvement in legal knowledge led to changes in domestic violence and the ability to quit abusive relationships.

Education and domestic violence

If having additional years of schooling improves women's legal awareness of new laws designed to enhance gender equality and reduce domestic violence, it may lead to a change in domestic violence outcomes. For example, if women are more aware that they are strictly protected from the abusing partner, they might be more likely to ask for protection from the police. Similarly, if women have heard about laws that provide temporary financial assistance and childcare services, they might have a stronger fallback position and, thus, a more credible threat of leaving the abusive relationship. The favorable shift in women's bargaining position might then result in a decline in domestic violence as well as an increase in probability of leaving home or filing for divorce after experiencing domestic violence (Farmer and Tiefenthaler 1996; Aizer 2011).

We examine this possibility by testing whether the reform had a significant impact on domestic violence and divorce outcomes. Table 4 presents the results. The OLS estimates in column 1 indicate that there is a negative correlation between years of schooling and all measures of

domestic violence as well as the probability of having a second marriage. The magnitudes of the correlations suggest that one additional year of schooling is associated with 2.7 ppt less physical violence, 2.2 ppt less sexual violence, 2.0 ppt less psychological violence, 1.5 ppt less financial control exercised by the intimate partner, and a 0.4 ppt lower probability of having a second marriage.

The RD estimates in columns 2 and 3 of Table 4 indicate no evidence of a significant impact of the reform on domestic violence or divorce outcomes. More specifically, we find no evidence that the reform had a significant impact on physical violence, sexual violence, psychological violence, or financial control indices. The RD estimates for the overall domestic violence index, which aggregates information across of these indicators of abuse, also shows no evidence of a significant impact of the reform on overall risk of experiencing domestic violence. As related evidence, we find very similar results when we use indicator variables of exposure to domestic violence as opposed to z-score indices. Online Appendix Table A8 shows no evidence of a significant effect of the reform on the probability of experiencing physical violence, sexual violence, psychological violence, or financial control measured by dummy variables, or underlying individual components of these indicators. Moreover, we also find no evidence of a significant effect of the reform on ever leaving home, ever getting divorced, or having a second marriage.

We conducted several robustness checks. First, Table A5 in Online Appendix D presents the RD results using a static bandwidth of 118 months around the cutoff, which is the optimal bandwidth estimated for junior high school completion. The findings in the table show that the RD estimates for the physical violence index and divorce-related outcomes are robust to this specification. Second, to accommodate a more flexible specification, we report the RD estimates using a quadratic control function in Table A6 in the Online Appendix. The estimated RD treatment effects are similar to those reported in our main tables. Third, Figure A5 in the Online Appendix presents the RD estimates using a quadratic control function and a bandwidth of eighty-nine months around the discontinuity, which is the optimal bandwidth estimated for years of schooling. We observe qualitatively similar RD treatment effects to those in the main results presented in the paper. Finally, Online Appendix Table A7 also shows that the effect of reform on schooling is slightly larger for women raised in rural regions, which is roughly 1.1 years of additional schooling. This table also illustrates that our main results hold for women raised in rural regions.[19]

Moreover, we also examine whether legal awareness plays a mediating role in the relationship between education and domestic violence related outcomes. Online Appendix Table A9 presents results from an IV regression where we instrument legal awareness about new laws with the reform exposure in order to examine the second-stage effects on domestic

Table 4 Effects of education on domestic violence and divorce outcomes

	(1) OLS	(2) RF	(3) IV	(4) Bandwidth	(5) N	(6) Mean
Physical violence index	− 0.020***	− 0.096	− 0.125	105	2,647	0.05
	(0.006)	(0.077)	(0.106)			
Sexual violence index	− 0.024***	− 0.123	− 0.163	107	2,718	0.03
	(0.005)	(0.076)	(0.112)			
Psychological violence index	− 0.023***	− 0.052	− 0.058	141	3,350	0.03
	(0.005)	(0.067)	(0.071)			
Financial control behavior	− 0.019***	− 0.026	− 0.035	119	2,884	0.00
	(0.004)	(0.066)	(0.089)			
Overall domestic violence index	− 0.022***	− 0.072	− 0.087	121	2,995	− 0.00
	(0.003)	(0.045)	(0.057)			
Ever left home	0.002	0.090	0.245	88	648	0.37
	(0.007)	(0.072)	(0.390)			
Ever divorced	− 0.002	− 0.011	− 0.016	94	2,646	0.05
	(0.001)	(0.017)	(0.026)			
Second marriage	− 0.004***	− 0.005	− 0.004	157	4,172	0.03
	(0.001)	(0.010)	(0.009)			

Notes: Data are from the 2014 NSDVW. See Table 2 for table notes.

violence and divorce related outcomes. The results from this exercise show no evidence of a significant impact of reform-induced legal awareness on domestic violence or ability to quit abusive relationships. This is not surprising given that the reduced-form estimates presented in Table 4 shows no evidence of a significant impact of the reform on domestic violence related outcomes. We conclude that legal awareness did not play a significant mediating role in the relationship between education and domestic violence outcomes in this context.

Furthermore, we examine whether the reform had any heterogeneous effects on legal awareness and domestic violence by mother's exposure to domestic violence. One potential mechanism could be that women whose mothers faced domestic violence could be more sensitive to the topic, which might increase their awareness of legal reforms. In Online Appendix Table A10, we observe no evidence that the reform had a differential impact on awareness about new laws or physical violence experienced by women whose mothers were subject to violence at home.

Finally, it is possible that increases in women's education can alter their fertility or other marriage market outcomes. In Online Appendix Table A11, we test whether the reform has any significant effects on a number of these outcomes including number of children, having ever used contraceptive, having ever given birth, whether the partner disapproves of contraception, pregnancy age, marriage age, husband's age, and husband's religiosity. We find no evidence that the reform had a significant impact on these outcomes in our context. Lastly, as shown in Erten and Keskin (2020) and discussed in the introduction, Online Appendix Table A12 indicates that the reform had no significant effects on the labor market outcomes of the women within our sample in this study.

CONCLUSION

In this article, we provide evidence of the impact of an extension of compulsory schooling that exogenously raised the average educational attainment on the acquisition of legal information in Turkey. Employing an RD design allows us to estimate the causal effects of the education reform on women's legal awareness of laws on gender equality and domestic violence as well as actual domestic violence and divorce outcomes. To the best of our knowledge, this is the first quantitative analysis that examines the impact of education on an unexamined dimension of information acquisition, specifically, legal awareness, and the source of such information acquisition. Given that knowledge of legal rights and availability of services has been widely accepted as a necessary condition for women to gain access to the legal system, we also assess whether the education reform had a significant impact on women's risk of experiencing domestic violence or ability to quit abusive relationships.

Our findings reveal that the reform had a positive impact on women's legal knowledge of new laws and services introduced to reduce gender inequalities and prevent domestic violence; however, this improvement in legal awareness did not translate into significant changes in domestic violence or divorce outcomes. Interestingly, we find no evidence of a significant impact of the education reform on women's knowledge of preexisting laws dating back to the early twentieth century. We also show that the reform significantly increased the likelihood of acquiring information about new laws and services through newspapers, journals, or books. Although our findings suggest a complementarity between education and legal information provision, we find no evidence indicating that the reform had a significant impact on domestic violence measures or divorce-related outcomes. In particular, we find no evidence of a change in the prevalence of physical, sexual, or psychological violence or financial control behavior exercised by intimate partners. We also find no evidence of a reform-induced change in ever leaving home after experiencing domestic violence, ever getting divorced, or having a second marriage.

Our results indicate that although education has a significant and large impact on the acquisition of legal information, this improvement in legal awareness does not necessarily translate into the differences in outcomes that these laws were intended to have. These findings parallel those documented in the education-health literature, indicating that education generally affects the acquisition of health-related information; however, the improvements in health-specific knowledge account for only small differences in behavior and health outcomes. Similar to the findings in the health literature relating to new technologies and new advances in medical knowledge, we detect significant effects of education only on knowledge related to newly introduced changes in gender-equality legislation, as opposed to preexisting legislation that has long been in place. Our findings also suggest that while improving legal knowledge could be an important step toward raising awareness of women's rights, it may not be sufficient to strengthen women's bargaining power within the household to reduce the prevalence of domestic violence.

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NOTES

[1] Abused women are more likely to report physical, mental, and reproductive health problems (Campbell 2002), and their children are more likely to suffer from socio-emotional and cognitive problems (Carlson 2000).

[2] See United Nations (2010) for an overview of different types of legislation designed and implemented to prevent violence against women across different countries.

[3] Over the past decade, several countries, including Brazil, Ecuador, El Salvador, Mexico, Peru, and Turkey, have launched networks of violence prevention centers, or women"s justice centers, which are institutions designed to provide legal, medical, and financial support to victims of domestic violence (Kavanaugh, Sviatschi, and Trako 2017).

[4] The interaction of domestic violence victims with the legal system has been receiving growing attention in the literature. Recent studies show that having more women policy officers significantly improves the quality of policing provisions and improves the reporting of crime against women (Amaral, Bhalotra, and Prakash 2018; Amaral 2019).

[5] Friedemann-Sánchez (2012), using evidence from Colombia, highlights that the main barrier to the assertion of legal rights in developing countries is often a lack of legal awareness and understanding of a person's rights.

[6] The need to provide legal literacy programs was also recognized in the Being Platform for Action organized by the United Nations in 1995.

[7] The scope of legal knowledge (for example, whether individuals know their legal rights and obligations) differs from political knowledge (for example, whether individuals follow politics in the media or participate in political parties) and civic knowledge (for example, whether individuals are aware of their civil rights and duties). Dee (2004) examines the effects of education on civic awareness and engagement, and Milligan, Moretti, and Oreopoulos (2004) examine the effects of education on political awareness and interest, as we explain in greater detail below.

[8] Appendix B provides further details of the legislation on gender equality and domestic violence in Turkey.

[9] This classification is useful in distinguishing post-2000 laws that were passed during the EU negotiations to fulfill the accession criteria on gender equality from the pre-2000 laws that have largely been in place since the 1920s. The post-2000 laws have provided a much stronger legal framework to prevent intimate partner abuse and protect women's rights in marriage, work, and other social relations, as we explain in detail in Online Appendix B.

[10] It is important to note that it would be ideal to use administrative data from hospital or police reports on domestic violence given that they are objective measures of violence. However, this type of information is likely to be misleading, especially in a developing country setting where only a selected group of women has access to hospitals or police stations after experiencing a violent episode. For example, in our dataset, only 6.5 percent of all women who faced physical violence visited a hospital after the assault, and only 9.8 percent of them filed a police report. It is also not possible to capture the degree of psychological or financial violence using administrative reports.

[11] A more recent compulsory schooling law was implemented in 2012. Erten and Keskin (2019) examined the heterogeneous treatment effects of this reform on education and labor market outcomes by poverty and religiosity in Turkey.

[12] See Cesur and Mocan (2014) for details of the laws, which stipulated that Turkish students who were 72 months old by the end of a calendar year could start school in September of that year (Resmi Gazete, Number 21308). As a result, children born before January 1987 could begin primary school education in 1992 and unintentionally avoid the eight-year requirement that was adopted on August 18, 1997, and effectively implemented in the 1997–98 school year.

[13] The optimal bandwidth approach relies on the central intuition that there is a tradeoff between bias and variance. This implies that as one approaches the threshold point and narrows down the bandwidth, the bias will be reduced because the treatment and control groups become more similar while, at the same time, the variance of estimates will increase given the smaller sample size (Lee and Lemieux 2010). Taking this into account, the optimal bandwidth procedures have the objective of selecting a bandwidth that minimizes bias while maximizing precision.

[14] In addition, we use specifications that adopt the optimal bandwidth from the first-stage results for junior high school completion, which is estimated as 118 months around the discontinuity; these are included in the Online Appendix tables. This static bandwidth approach complements the former results for which we use the optimal bandwidth.

[15] We use fixed effects for twelve regions where the respondents lived until the age of 12 when they were subjected to the education reform.

[16] The balanced covariates checks for the sample of women who ever had a relationship provide similar results. Online Appendix Figure A3 provides no evidence of significant jumps around the discontinuity for predetermined covariates, and Online Appendix Table A3 presents RD estimates and shows that a SUR test of joint significance also fails to reject the null hypothesis that the coefficients of predetermined covariates are jointly equal to zero.

[17] One obvious question is whether the 1997 reform had an effect on men's schooling outcomes. Online Appendix Figure A4 plots the average junior high school completion for women and men using the 2014 Household Labor Force Survey (HLFS). The graph on the left shows evidence of a clear jump for the junior high school completion of women whereas the right-side graph shows no evidence of a significant jump for the same outcome for men. This result implies that the reform had a much smaller effect on men, possibly because the junior high school completion rate for men was already close to 90 percent prior to the reform. Unfortunately, we cannot replicate the same exercise for men using the NSDVW 2014 since the month of birth for husbands is not reported in the dataset.

[18] For the IV estimates, the first-stage F-statistic is 16.46, indicating again a strong first-stage relationship between the reform and schooling outcomes.

[19] The only exception is the reform's effect on the ways in which the women learned about the new laws. Education does not have a significant effect on rural women's likelihood of learning about the reforms from newspapers, journals, or books. This can be explained by the limited supply of these news outlets in rural Turkey.

SUPPLEMENTAL DATA

Supplemental data for this article can be accessed at https://doi.org/10.1080/13545701.2022.2061029.

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