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Compulsory schooling reform and intimate partner violence in Turkey

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ABSTRACT

We examine how Turkey's 1997 compulsory schooling policy affects intimate partner violence (IPV) using the 2008 and 2014 Turkish National Survey of Domestic Violence Against Women and regression discontinuity design. We find conclusive evidence that the policy *reduces* physical violence against rural women, whereas this evidence is suggestive for the sample of all women. For the urban sample, we reveal large negative, but statistically insignificant, effects on sexual violence and partners preventing women from working. We find null policy effects on psychological violence for the sample of all women. The policy appears to have been protective against IPV for women overall. In addition, we show that the policy effects are realized through changing partner characteristics as well as women's increased schooling. Our results contradict previous evidence for Turkey, and we demonstrate that the previous evidence misclassifies two key variables.

1. Introduction

One in every four women aged 15–49 years who have ever been in a relationship has been subjected to some form of intimate partner violence (IPV) in their lifetime, according to the World Health Organization (WHO, 2021). Violence can have negative effects on women's mental, physical, and reproductive health, which can directly or indirectly affect women's wellbeing, labor market outcomes, and their children's outcomes (Carrell and Hoekstra, 2010; Carrell and Hoekstra, 2012). Given the high prevalence of domestic violence and its economic consequences, economists have contributed to the investigation of the causes of domestic violence.[1]

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[1] These causes include the effect of improved women's employment, income opportunities, higher wages, and autonomy (Tauchen et al. 1991; Farmer and Tiefenthaler 1997; Panda and Agarwal 2005; Bowlus and Seitz 2006; Aizer 2010; Eswaran and Malhotra 2011; Chin 2012; Heath 2014; Anderberg et al. 2016; Cools and Kotsadam 2017; Anderberg et al. 2021), public and cash transfers for women (Hidrobo and Fernald 2013; Hidrobo 2016, Angelucci and Heath 2020), dowry payments (Bloch and Rao 2002; Srinivasan and Bedi 2007, Calvi and Keskar 2021), unexpected emotional cues (Card and Dahl 2011), divorce laws (Brassiolo 2016), restrictions on alcohol sales (Luca et al. 2015), civil conflict and war service (La Mattina 2017, Cesur and Sabia 2016), and battered women's service usage (Farmer and Tiefenthaler 1996).

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Women's empowerment is a significant factor that affects domestic violence outcomes. The findings in the literature regarding the relationship between the empowerment of women and domestic violence outcomes are mixed. One strand of the literature supports the hypothesis that improved outside options, better income opportunities, and enhanced labor market outcomes for women decrease domestic violence (Aizer, 2010; Bowlus and Seitz, 2006; Farmer and Tiefenthaler, 1997; Tauchen et al., 1991; Peterman et al., 2017; Anderberg et al., 2016). This perspective contends that women remain in a marriage only if the utility of staying in the marriage is above a certain threshold, which is referred to as reservation utility. As women are empowered through better education or improved labor market outcomes, their reservation utility will increase and their partner will decrease violence to ensure this reservation utility.[2] In contrast, some research supports the view that improvements in the educational, social, and economic status of women may lead to increased exposure to violence, as the partner may use violence as an instrument to extract resources from women or control women's behavior (Bloch and Rao, 2002; Eswaran and Malhotra, 2011; Heath, 2014; Bulte and Lensink, 2019). Another hypothesis for explaining the relationship between women's empowerment and domestic violence outcomes that has some support in the literature is the male backlash hypothesis. According to this hypothesis, when women are in a more advantaged position in terms of education, income, or labor market outcomes, this can be perceived by their partners as a threat to traditional gender norms, and men use violence to restore these gender roles.[3]

Although ample and growing literature investigates the effects of women's income and employment opportunities, as means of empowerment, on domestic violence outcomes, research on the impact of education on domestic violence outcomes is scarce. We contribute to this important line of inquiry by examining the effect of the 1997 compulsory schooling reform in Turkey on domestic violence outcomes. This reform increased compulsory schooling from 5 to 8 years and had a large impact on schooling outcomes. Aydemir et al. (2022) report that the reform raised the middle school completion rate by around 17 percentage points among men and 21 percentage points among women. The effect was particularly strong in rural areas and had spillover effects on high school completion (Kirdar et al., 2016).

We use data from the 2008 and 2014 rounds of the Turkey National Survey of Domestic Violence against Women (TNSDVW).[4] For identification of the effect of the compulsory schooling policy, we exploit the month-year of birth cutoff in the exposure to the 1997 compulsory schooling reform in Turkey via regression discontinuity design (RDD). In the estimation, we use both parametric and local polynomial approaches. We estimate only the reduced form policy effect, but not the effect of women's schooling via a two-stage least squares (2SLS) estimation because of the failure of the exclusion restriction assumption. We also carefully assess the potential policy effects on sample selection (via impact on the composition of the sample) and the quality of women's responses provided in the survey.

We find null policy effects on psychological violence and almost null effects on women's employment. While we estimate a positive policy effect on financial control behavior, it is not statistically significant at conventional levels.[5] Conversely, we find suggestive evidence of a negative effect on physical violence for the total sample, and this evidence becomes conclusive for women who grew up in rural areas. In addition, we estimate large and adverse, but imprecisely estimated, policy effects on sexual violence and partners preventing women from working for the urban sample.

A key finding of our study is that the increased schooling of women via the policy is not the only cause of these observed policy effects on IPV outcomes. Examining the impact of the policy on partners' characteristics, we find that the policy also increases partners' schooling and the probability that women (rather than their parents) choose their partners. We also find suggestive evidence that the age gap between partners decreases in the rural sample.

Our finding that education is a protective factor against IPV is supportive of the hypothesis that the empowerment of women via improved outside options decreases domestic violence (Aizer, 2010; Farmer and Tiefenthaler, 1996; Tauchen et al., 1991; Peterman et al., 2017). In contrast, we find no evidence that a partner may use violence as an instrument to extract resources from women or control women's behavior (Bloch and Rao, 2002; Eswaran and Malhotra, 2011).

Much of the evidence for women's empowerment via improved outside options decreasing domestic violence comes from developed country settings (e.g., Gelles, 1976; Famer and Tiefenthaler, 1997; Bowlus and Seitz, 2006; Aizer, 2010). An exception is Haushofer et al. (2018), who find that unconditional cash transfers lead to a reduction in IPV in Kenya, using a randomized controlled trial. Our study provides causal evidence from a socially conservative middle-income country. The above studies examine the impact of women's resources and economic opportunities on IPV. In contrast, women's empowerment in our context is related to compulsory schooling reform.

Several studies in the context of developing countries demonstrate the detrimental effects of women's labor force participation, income, and assets on IPV (Bloch and Rao, 2002; Eswaran and Malhotra, 2011; Heath, 2014; Alesina et al., 2021; Guarneiri and Rainer, 2021). These findings are consistent with Alesina et al. (2021) who assert that in more patriarchal and conservative societies, where divorce is often not socially accepted, economic opportunities for women can increase IPV. Our paper differs from this literature in three important ways. First, while many of these studies are based on associations,[6] our paper uses a plausibly exogenous variation in schooling. Second, the contexts differ; for instance, in the context of their paper (India), Block and Rao (2002) report that a bride is a

[2] Similarly, better-quality shelter services, generous divorce laws, and improved property rights are found to reduce domestic violence through the same channel (Farmer and Tiefenthaler, 1996; Brassiolo, 2016; Amaral, 2017).

[3] See, e.g., Macmillan and Gartner (1999), Alesina et al. (2021), Guarnieri and Rainer (2021), and Tur-Prats (2021).

[4] Given the modest size of each round of the TNSDVW and the low frequency of some of the events analyzed in this study, the use of two different rounds of the TNSDVW increases the power of our empirical analysis.

[5] A limitation of the financial control behavior variable is that it is based on only two events, each of which has a very low incidence in the data.

[6] An exception is Bulte and Lensink (2019), who provide causal evidence that increased income via a training program raises IPV in Vietnam.

"potential hostage," whereas divorce is possible in our context. Third, our study focuses on how women's increased schooling and changing partner characteristics resulting from compulsory schooling reform affect IPV, whereas the above literature is predominantly interested in the associations between IPV and labor force participation, income, and assets.

Our findings are highly contradictory to those of Erten and Keskin (2018, henceforth EK), who, using the 2008 round of the TNSDVW, find that women's education increases both employment and IPV—supporting the instrumental violence hypothesis (IVH). Their evidence holds *only* for women who lived in "rural areas during childhood" but not for all women. Reexamining their analysis, we demonstrate that the evidence they provide results from a misclassification of two key variables: childhood rural areas and women's employment status. When either of these two variables is properly defined, the authors' evidence for the IVH vanishes. In addition, we demonstrate two other serious flaws in EK's analysis: (i) the policy alters the composition of their sample and the survey response quality (failure of the exclusion restriction assumption), and (ii) the main identification assumption of the RDD fails for some key outcomes.

The remainder of this paper is organized as follows. Section 2 provides brief background information, and Section 3 discusses our data and methods. We present our results in Section 4. Section 5 revisits EK's analysis and provides details regarding each of the above-mentioned problems using a transparent and methodical approach. Section 6 concludes.

2. Brief background information

Before 1997, basic education in Turkey was comprised of 5 years of compulsory primary school and 3 years of optional middle school. In 1997, with law number 4306, the duration of compulsory schooling was extended from 5 to 8 years, merging primary and middle schools into "primary education," and the primary school diploma after 5 years of education was abolished. Since then, a middle school diploma has been awarded to those completing 8 years of schooling. The new law went into effect immediately in the fall of 1998, affecting those who were in the fourth grade or a lower grade level at the end of the 1996–1997 school year; hence, the policy affected children who started school in the 1993–1994 school year or later. According to the relevant law at that time, children were required to begin school in the fall semester of the year in which they turned 72 months old.[7] Therefore, children born in 1987 and afterward were affected by the policy; however, it is important to note that the rule on school start age was not strictly enforced at that time and noncompliance with the reform (early and late school start age) was quite common. As a result, some children in the 1986 birth cohort were affected by the policy and some in the 1987 birth cohort were not.

To implement the new policy successfully, the government increased public spending on education. The public investment budget share of Turkey's Ministry of National Education (MONE), which was 15% in 1996 and 1997 prior to policy implementation, jumped to 37.3% in 1998 and remained at around 30% until 2000 (Kirdar et al., 2016). As a result, the reform had a substantial impact on enrollment rates. Statistics from Turkey's MONE show that from the 1997–1998 school year to the 2000–2001 school year, the number of students in urban areas increased from 6.75 to 7.67 million—a 13.7% increase—compared to the 1.8% increase in the preceding 3-year interval and the 0.5% increase in the succeeding 3-year interval. The number of students in rural areas rose from 2.35 to 2.8 million over the same period, a 20% increase, compared to the 7% fall in the preceding 3-year interval and the 1.4% fall in the succeeding 3-year interval (Kirdar et al., 2016).

The impact of the compulsory schooling reform differed in urban and rural areas due to differences in schooling infrastructure investment. The MONE used two key instruments in implementing the reform, including bussing rural children to nearby schools and the construction of boarding schools. With the reform, several primary schools in rural areas that were unable to provide the facilities for all grades from 1 to 8 were closed, and the students in these schools were bussed to nearby schools (in more centrally located villages) that housed all grades from 1 to 8. Consequently, the number of students bussed to schools rose from 127,683 to 621,986 students between the 1996–1997 and 1999–2000 school years (Kirdar et al., 2016). The other key instrument was the construction of free-of-charge boarding schools that housed all grades 1 to 8. The number of students enrolled in these schools increased from 34,465 to 281,609 between the 1996–1997 and 2001–2002 school years (Kirdar et al., 2016). These two instruments substantially decreased the cost of schooling for grades 6–8 (the new compulsory grade levels) in rural areas, as these schooling levels were not locally available in many rural areas before the policy. In urban areas, lower secondary school attendance was already relatively high at the time of the reform, meaning that physical capacity already existed. The capacity in urban areas was used more efficiently after the reform via a double-shift system, in which some children attended school in the morning and some in the afternoon. In addition, merging primary and lower secondary schools increased efficiency with which the existing capacity could be used. Consequently, the total number of classrooms increased in both urban and rural areas (Kirdar et al., 2016).

3. Data and methodology

3.1. Data

The data we use are obtained from the TNSDVW of 2008 and 2014.[8] We pool the two rounds of the TNSDVW to increase the power of our empirical analysis, which is particularly important due to the modest sample size of each round of the TNSDVW and the low incidence of some of the events analyzed. The two rounds used the same sampling and collection methods, the only difference being

[7] *Resmi Gazete*; Friday, August 7, 1992, Section 14.

[8] The data are available only for 2008 and 2014.

Table 1
Descriptive statistics.

VARIABLES	All Sample Mean	Std. Dev.	Obs.	Rural Mean	Std. Dev.	Obs.	Urban Mean	Std. Dev.	Obs.
A) Physical Violence									
Partner ever slapped her	0.235	0.424	4,496	0.263	0.441	1,669	0.220	0.414	2,827
Partner ever pushed her	0.135	0.342	4,496	0.138	0.345	1,669	0.134	0.341	2,827
Partner ever hit her	0.069	0.253	4,494	0.074	0.261	1,668	0.066	0.248	2,826
Partner ever kicked her	0.061	0.240	4,495	0.072	0.259	1,669	0.056	0.230	2,826
Partner ever chocked her	0.041	0.199	4,496	0.038	0.191	1,669	0.043	0.203	2,827
B) Sexual Violence									
Partner ever forced her into sex	0.050	0.218	4,496	0.053	0.223	1,669	0.049	0.216	2,827
Partner ever forced her into sex with fear	0.062	0.241	4,496	0.067	0.251	1,669	0.059	0.236	2,827
Partner ever humiliated her during sex	0.026	0.160	4,495	0.028	0.165	1,669	0.025	0.157	2,826
C) Psychological Violence									
Partner ever humiliated her	0.294	0.456	4,496	0.303	0.460	1,669	0.289	0.453	2,827
Partner ever insulted her	0.155	0.362	4,493	0.159	0.366	1,668	0.154	0.361	2,825
Partner ever threatened her	0.116	0.321	4,496	0.107	0.310	1,669	0.121	0.326	2,827
Partner intervenes with friends	0.178	0.383	4,492	0.148	0.355	1,666	0.194	0.395	2,826
Partner intervenes with family	0.092	0.288	4,491	0.082	0.274	1,666	0.097	0.296	2,825
Partner insists on knowing where she is	0.731	0.444	4,489	0.730	0.444	1,668	0.732	0.443	2,821
Partner ignores her	0.150	0.357	4,489	0.148	0.355	1,668	0.151	0.358	2,821
Partner is angry about other men	0.566	0.496	4,458	0.588	0.492	1,651	0.555	0.497	2,807
Partner is suspicious that she is unfaithful	0.065	0.247	4,454	0.054	0.226	1,655	0.071	0.257	2,799
Partner intervenes with healthcare	0.233	0.423	4,474	0.319	0.466	1,660	0.188	0.391	2,814
Partner intervenes with clothing choices	0.458	0.498	4,493	0.450	0.498	1,668	0.462	0.499	2,825
D) Financial Control Behavior									
Partner ever prevents her from working	0.286	0.452	3,900	0.269	0.444	1,389	0.294	0.456	2,511
Partner ever took her money	0.035	0.184	3,394	0.037	0.189	1,252	0.034	0.182	2,142
Partner ever refused to give her money	0.061	0.239	3,932	0.065	0.246	1,520	0.058	0.234	2,412
E) Domestic Violence Outcome Indices									
Physical Violence Index	-0.129	0.665	4,496	-0.108	0.674	1,669	-0.140	0.659	2,827
Psychological Violence Index	0.035	0.528	4,496	0.037	0.502	1,669	0.033	0.540	2,827
Financial Violence Index	-0.083	0.736	4,185	-0.074	0.752	1,578	-0.087	0.727	2,607
Sexual Violence Index	-0.079	0.679	4,496	-0.066	0.665	1,669	-0.086	0.686	2,827
F) Education									
Years of schooling	8.225	3.815	4,509	6.420	3.245	1,671	9.151	3.754	2,838
Middle school completion	0.594	0.491	4,509	0.381	0.486	1,671	0.703	0.457	2,838
G) Relationship and Partner Characteristics									
Schooling gap	1.054	3.361	4,438	1.563	3.485	1,644	0.793	3.265	2,794
Partners' middle school completion	0.683	0.465	4,438	0.548	0.498	1,644	0.752	0.432	2,794
Age gap between Partners	3.800	3.935	3,564	3.679	4.062	1,446	3.874	3.855	2,118
Women's consent in the marriage decision	0.579	0.494	3,574	0.482	0.500	1,452	0.638	0.481	2,122
Partner worked last week or usually has a job	0.868	0.338	4,509	0.889	0.314	1,671	0.858	0.349	2,838
H) Labor Market Outcomes									
Woman worked last week	0.175	0.380	4,509	0.162	0.369	1,671	0.182	0.386	2,838
Woman worked last week or usually has a job	0.258	0.438	4,509	0.279	0.449	1,671	0.248	0.432	2,838
I) Gender and Domestic Violence Attitudes									
A woman should not argue with her partner if she disagrees with him.	0.377	0.485	4,481	0.496	0.500	1,657	0.316	0.465	2,824
A woman should be able to spend her money as she wishes	0.695	0.460	4,474	0.644	0.479	1,654	0.721	0.448	2,820
Men can beat their partners in certain situations	0.195	0.396	4,408	0.258	0.438	1,626	0.162	0.369	2,782
Men should do housework	0.720	0.449	4,490	0.645	0.479	1,664	0.758	0.428	2,826
A child can be beaten for discipline	0.277	0.447	4,489	0.365	0.482	1,664	0.231	0.422	2,825
Men are responsible for a woman's behavior	0.393	0.488	4,433	0.500	0.500	1,638	0.338	0.473	2,795
A woman is obliged to have sexual intercourse with her husband	0.142	0.349	4,451	0.187	0.390	1,649	0.119	0.323	2,802
Gender Attitudes Index	0.051	0.519	4,509	-0.122	0.545	1,671	0.140	0.482	2,838
J) Other Variables									
Data Wave 2014	0.394	0.489	4,509	0.390	0.488	1,671	0.396	0.489	2,838
Response Quality	0.867	0.339	4,473	0.833	0.373	1,660	0.885	0.319	2,813
Non-Turkish Speaker	0.004	0.066	4,500	0.010	0.097	1,669	0.002	0.042	2,831
Childhood Region: Rural	0.339	0.473	4,509	1.000	0.000	1,671	0.000	0.000	2,838

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence against Women. The sample includes women who have ever had a relationship and who are born within a 72-month bandwidth of the 1987 January cutoff.

that the sample size is smaller in 2014 than in the 2008 survey. The 2008 survey covers 17,168 households and 12,795 women, and the 2014 survey covers 11,247 households and 7,462 women.

The survey includes several questions to elicit information on physical, sexual, financial, and psychological violence experienced by women from their partners. It is constructed by using a questionnaire developed by WHO as a model. To ensure the safety of both the women interviewed and the interviewers and to reduce misreporting, the ethical guidelines developed by the WHO were used in every stage of the study (WHO, 2001). During the fieldwork, the research was referred to as the "Turkey Women and Family Survey," and the research topic was not revealed to others. If more than one woman was in the 15–59 age group in a household, only one woman was selected randomly, and interviews were conducted in a private setting. Finally, as the interviews were conducted with women, only females served as interviewers.

The datasets include information on the level of education, birth month and year, childhood region, type of childhood region, marriage history, presence and intensity of forms of spousal violence, and characteristics and behavioral patterns of the husbands/partners. The type of childhood place of residence is an essential variable in this paper as we conduct our empirical analysis in terms of the rural and urban status of the childhood place of residence. The TNSDVW dataset includes a variable for rural areas, defined as locations with populations under 10,000. However, the childhood rural area status based on this definition is *not* available for a significant portion of the women in the dataset. The TNSDVW also provides information regarding the province center, district center, and subdistrict/village status of childhood locations for all women, and we define childhood rural areas as subdistrict/villages.

Table 1 presents descriptive statistics for women who ever had a relationship and who were born within 72 months around the cutoff. We also present descriptive statistics for the rural and urban status of women's childhood regions. Several questions in the TNSDVW dataset extract information regarding various types of violence. We construct an index for each of the physical, psychological, financial,[9] and sexual violence categories by averaging the z-scores for each kind of violence in the corresponding category. A higher value of these indices represents higher exposure to domestic violence. Panel A of Table 1 shows that the most common type of physical violence women are exposed to is being slapped or pushed. One in every four women has been slapped by their partners. As demonstrated in panel B, partners forcing women into sex with fear is the most common form of sexual violence experienced by women.

Panel C shows that the share of women who ever experienced psychological violence is quite large. In fact, 73% of the women in our sample indicate that their partners insisted on knowing where they were. In panel D, we present the descriptive statistics of three types of financial control behavior. The most common type of financial control behavior is preventing women from working, with 28.6% of women reporting this type of violence. The other two types of financial control behavior, forcibly taking women's money and refusing to give money, are experienced by around 3.5% and 6.1% of women, respectively. We construct the variable for financial control behavior using these two variables as a measure of partners extracting resources from women, as in EK (2008). We examine the incidence of partners preventing women from working separately. Panel E presents the descriptive statistics for the aggregate domestic violence measures.

Panel F presents the average years of schooling and the share of women who have at least a middle school degree in each sample. Women who grew up in rural areas have, on average, 6.42 years of schooling, while those who grew up in urban areas have 9.15 years of education. Panel G presents relationship and partner characteristics. Women who grew up in urban areas are more likely to have more educated partners. The schooling gap between partners, defined as the difference in the years of schooling between men and women, is also lower in urban areas. Moreover, women whose childhood region is urban are more likely to make their marriage decision. We create two dummy variables to capture the employment status of women and their partners: "women (partner) worked last week" and "women (partner) worked last week or usually has a job." In our analysis, we use the latter definition, which is more consistent with the standard definition of employment.[10] We present the labor market outcomes of partners in panel G and the labor market outcomes of women in panel H. There is a significant difference between the two employment definitions for the women's sample. Only 17.5% of women are employed when employment is defined as "worked last week," whereas 25.8% of women are employed when employment is defined as "worked last week or usually has a job." The latter percentage is more consistent with national statistics.

Panel I presents the share of women who match the various perspectives listed in the table on gender and domestic violence. We also construct a gender index by averaging the z-scores of these gender and domestic violence attitudes, revealing that women who grew up in urban childhood regions have more equal gender views. Finally, panel J presents the remaining covariates used in the empirical analysis and response quality variable. At the end of the survey, interviewers are asked to evaluate the quality of responses. The options include poor, medium, good, and very good. We define a response quality variable as equal to 1 if the quality was evaluated to be good or very good and 0 otherwise. The quality of the responses of women who grew up in urban areas appears to be better than those who grew up in rural areas.

3.2. Identification method and estimation

To determine the effect of the education policy, we use the month-year of birth cutoff in a RDD. In estimating the reduced form impacts of the policy, we use the following sharp RDD specification,

$$y_i = \beta_0 + \beta_1 T_i + I(T_i = 0) f(x_i) + I(T_i = 1) g(x_i) + X_i \Gamma + u_i, \tag{1}$$

[9] The financial violence index is formed by using z-scores of the variables constructed from the answers to the following two questions: "Did your partner refuse to give you money?" and "Did your partner take your income?"

[10] For instance, the Turkish Household Labor Force Survey (THLFS) defines individuals as employed if they worked in the reference week or if they did not work in the reference week but have a job that they can return to.

where y denotes the outcome variable for woman i. The treatment variable (T) takes the value of 1 when the month-year of birth is after January 1987 and 0 otherwise. The indicator function, I(.), is 1 when the statement inside the parenthesis is true and 0 otherwise. The functions f(.) and g(.) capture the time trends in the outcome variable on the left- and right-hand-side of the cutoff, respectively. The running variable (x) is the month-year of birth, which is normalized at the cutoff value. In Eq. (1), X denotes the set of control variables, u stands for the error terms, and β_1 shows the reduced form effect of the policy on the outcome variable.

The control variables (X) include birth month dummies, a dummy variable for whether the childhood region was a rural area, a dummy variable for whether the interview language was Turkish, survey year fixed effect, and dummies for 26 NUTS-2 regions of residence at age 12.[11] The regressions are weighed using sample weights, and standard errors are clustered at the month-year of birth level, as suggested by Lee and Card (2008).

In the estimation, we use both parametric and nonparametric (local polynomial) approaches. In our parametric approach, we use several alternative bandwidths with split linear trends on each side of the cutoff.[12] In particular, we start with an 8-year bandwidth on each side of the cutoff and gradually zoom in around the cutoff by narrowing the bandwidth by 1 year increments. Hence, we show estimates for five different bandwidths from 8 to 4 years on each side. The reason for stopping at 4-year bandwidths will be made clear when we present the policy effect on schooling outcomes. With our narrowest bandwidth, we retain 96 clusters in our data.

We also conduct nonparametric RDD estimation using two different optimal bandwidth selection methods, Imbens and Kalyanaraman (2012, IK) and Calonico et al. (2017, CCFT). [13] When using IK optimal bandwidths (b), we also assess the robustness of our findings with alternative bandwidths of b/2, 3b/2, and 2b. When using CCT optimal bandwidths, we examine robustness using both bias-corrected and robust standard errors. In addition to alternative methods of calculating the optimal bandwidth offered by these approaches, we also check the robustness of our findings to the use of alternative kernels in both approaches.

The use of nonparametric methods, which typically only choose points that are very close to the cutoff under the hypothetical infinite sample, is challenging in our context, due to the discrete nature of the running variable and imperfect compliance for the 1986 and 1987 birth cohorts that are immediately around the cutoff. We find that the local polynomial estimates are sensitive to alternative bandwidths and bandwidth types in the CCFT approach. Hence, in this context, we view the results of local polynomial approaches only as complementary evidence. In fact, Lee and Lemieux (2010) argue that "[n]onparametric estimation does not represent a 'solution' to functional form issues raised by RD designs. It is, therefore, helpful to view it as a complement to—rather than a substitute for—parametric estimation."

3.2.1. Checks of the identification assumption

We next examine the fundamental identifying assumption in the RDD that potential outcome distributions are smooth around the cutoff. Although this assumption is not directly testable, three tests are commonly used in the literature to assess its plausibility, including (i) the continuity of the score density around the cutoff, (ii) the absence of treatment effects on pre-treatment covariates, and (iii) the absence of treatment effects at artificial cutoff values.

Continuity of the score density around the cutoff requires that households not manipulate the running variable to be on one particular side of the cutoff. In this case, we would expect a higher mass on that side of the cutoff. While this is unlikely in our context because the running variable (month-year of birth) occurs before the policy, we examine this possibility formally using the McCrary test (McCrary, 2008). The results are given for the full sample in Figure OA1 and by rural and urban status in Figure OA2 in Online Appendix A, indicating no evidence of such manipulation.

We next check the policy effect on the pre-treatment covariates at the cutoff. In the absence of sorting around the cutoff, we would expect no jump at the cutoff for the pre-treatment covariates. The results in Tables OA1–OA3 in Online Appendix A, with each table applying a different bandwidth,[14] show that the assumption regarding the absence of a jump at the cutoff for the pre-treatment covariates holds.[15]

Finally, we examine the absence of treatment effects at artificial cutoffs, first separating the data into (i) a sample consisting of women who were not affected by the policy, which we call sample (A), and (ii) a sample of those who were affected by the policy, which we call sample (B). The results for several artificial cutoffs on either side of the actual cutoff are presented in Table OA4 of Online Appendix A for physical violence and sexual violence and in Table OA5 of Online Appendix A for financial control behavior and psychological violence.[16] Of the 75 estimates for each dependent variable, only three for physical violence, 12 for sexual violence, nine

[11] We use a missing dummy variable for observations with missing information on the childhood NUTS-2 region.

[12] Gelman and Imbens (2019) suggest using low-order polynomials for trends in RDD.

[13] We use the "rd" Stata command (Nichols, 2011) to implement the estimation with IK bandwidths and the "rdrobust" Stata command (Calonico et al., 2017) for the estimation with CCFT bandwidths.

[14] The bandwidths are 60, 72, and 84 months, respectively, in Online Appendix A Tables OA1, OA2, and OA3.

[15] Evidence of a jump at the cutoff does not emerge for any of the variables with the full sample and the rural and urban samples in Online Appendix A Tables OA2 and OA3. In Online Appendix A Table OA1, no jump is evident for any of the variables with the full sample and urban sample; however, for three of the 15 variables, we reject the absence of a jump at the 10% level for the rural sample.

[16] With sample A, we start with the alternative cutoff of January 1985, allowing at least two years on each side of the cutoff, and gradually shift the alternative cutoff to the left by one year until January 1977. For each alternative cutoff, we take bandwidths ranging from 2 to 7 years. With sample B, we start with the alternative cutoff value of January 1989, gradually shifting it to the right by six months this time, as the maximum bandwidth on the right-hand side of the actual cutoff is only 7 years. For sample B, the bandwidths range only from 2 to 4 years due to the shorter maximum bandwidth.

for financial control behavior, and three for psychological violence are statistically significant at the 10% level. Examining the rejections of the 75 estimates at the 5% level, two for physical violence, four for sexual violence, three for financial control behavior, and none for psychological violence are statistically significant. Consequently, we can conclude that there could be concerns regarding the continuity assumption for the sexual violence variable.

4. Results

4.1. Preliminary checks: policy effect on ever having a relationship, reporting missing month of birth, and response quality

Before we present our main results, we assess whether the policy caused compositional changes in our sample due to its potential effects on the status of ever having a relationship or on reporting missing information for the month of birth (which generates our running variable). Although such compositional changes would not bias our reduced form estimates, they would bias the 2SLS estimates and would alter the interpretation of the reduced form estimates. The observed effects would partly result from compositional changes rather than the direct impact of the policy on schooling attainment. We also examine the policy effect on the response quality measure in the survey.[17] A potential impact on the response quality would also change our interpretation, as it could indicate that the policy changed the likelihood that women would report domestic violence rather than the probability that they actually faced it.

Fig. 1 displays the RDD graphs for ever having a relationship in panel (I), for reporting missing month of birth in panel (II), and for the response quality in panel (III). Each panel presents the results for the total, rural, and urban samples. Our RDD graphs throughout the text provide suggestive evidence, presenting the data for a particular bandwidth (72 months, which lies in the middle of our bandwidth range in the estimations). In our estimations, we carefully assess the robustness of our findings to alternative bandwidths. Fig. 1 shows no significant jumps at the cutoff for any of the variables. In all plots, but those for ever having a relationship in the urban and total samples, the mean value of the outcome variable on the right-hand side of the cutoff lies within the 95% confidence interval on the left-hand side of the cutoff.

Table 2 presents the RDD estimates for the same three outcome variables. Panel (I) presents no evidence of a policy effect on ever having a relationship. Moreover, the magnitudes of the coefficient estimates are small, except for those with the narrowest bandwidth. The RDD results with the local polynomial approach in Appendix Table A1 show that the policy increases the likelihood of ever having a relationship for the total and urban samples with the optimal CCFT bandwidths but not the optimal IK bandwidths. The optimal bandwidth for the total sample is only about 2 years with the CCFT approach and about 4 years with the IK approach; hence, we can conclude that no evidence of a policy effect exists for ever having a relationship, except for the estimates based on the very narrow bandwidths of the CCFT approach.

Panel (II) of Table 2 shows the policy effect on the probability of reporting missing month of birth information. The coefficients are all negative; however, they are not statistically significant at conventional levels, except for one at the 10% level. We do not conduct local polynomial RDD estimation for the missing birth month variable because the running variable here is the year of birth. Finally, panel (III) presents the policy effect on the response quality measure. We observe no policy effect for wider bandwidths (6- to 8-year bandwidths), whereas the impact for the total sample is statistically significant at the 10% level for 4- and 5-year bandwidths. Moreover, the policy effect gradually grows in magnitude as we narrow the bandwidth. Estimates from the local polynomial approach in Appendix Table A1 show a positive policy effect on response quality for the total sample with the CCFT optimal bandwidths (about 33 months) but not with the IK optimal bandwidths. Therefore, we find weak evidence of a policy effect on response quality only for narrow CCFT bandwidths.

In essence, our checks on sample selection and response quality raise only minor concerns (based on weak statistical evidence) for the response quality variable that are only applicable to narrow bandwidths. In this case, the evidence suggests that the policy improves the response quality for the total sample; hence, we must acknowledge that the interpretation of our results on IPV outcomes with narrow bandwidths could be influenced by a policy effect on the response quality. It is important to note that none of our findings are solely based on very narrow bandwidths; we make a conclusion only if the result remains robust under alternative bandwidths.[18]

4.2. Main results

In this section, we first present the impact of the education reform on schooling outcomes and partner characteristics and then examine the effect on IPV outcomes. Finally, we explore how women’s attitudes on gender and domestic violence change with the policy.

4.2.1. Policy effect on schooling

Fig. 2 shows the RDD graphs for two schooling outcomes, years of schooling and middle school completion, revealing a substantial

[17] A potential effect on response quality could result from either a direct policy effect on actual response quality—because more educated women provide better responses—or a change in the sample’s composition at the cutoff due to a policy effect on the composition women due to the reasons discussed above.

[18] When we take the 2008 data individually, the exclusion restriction assumptions fail for the total sample. We reveal large policy effects on response quality and reporting missing month of birth information, which are robust across all bandwidths (see Online Appendix A Table OA6). Consequently, we do not conduct our analysis individually for each year in this study.

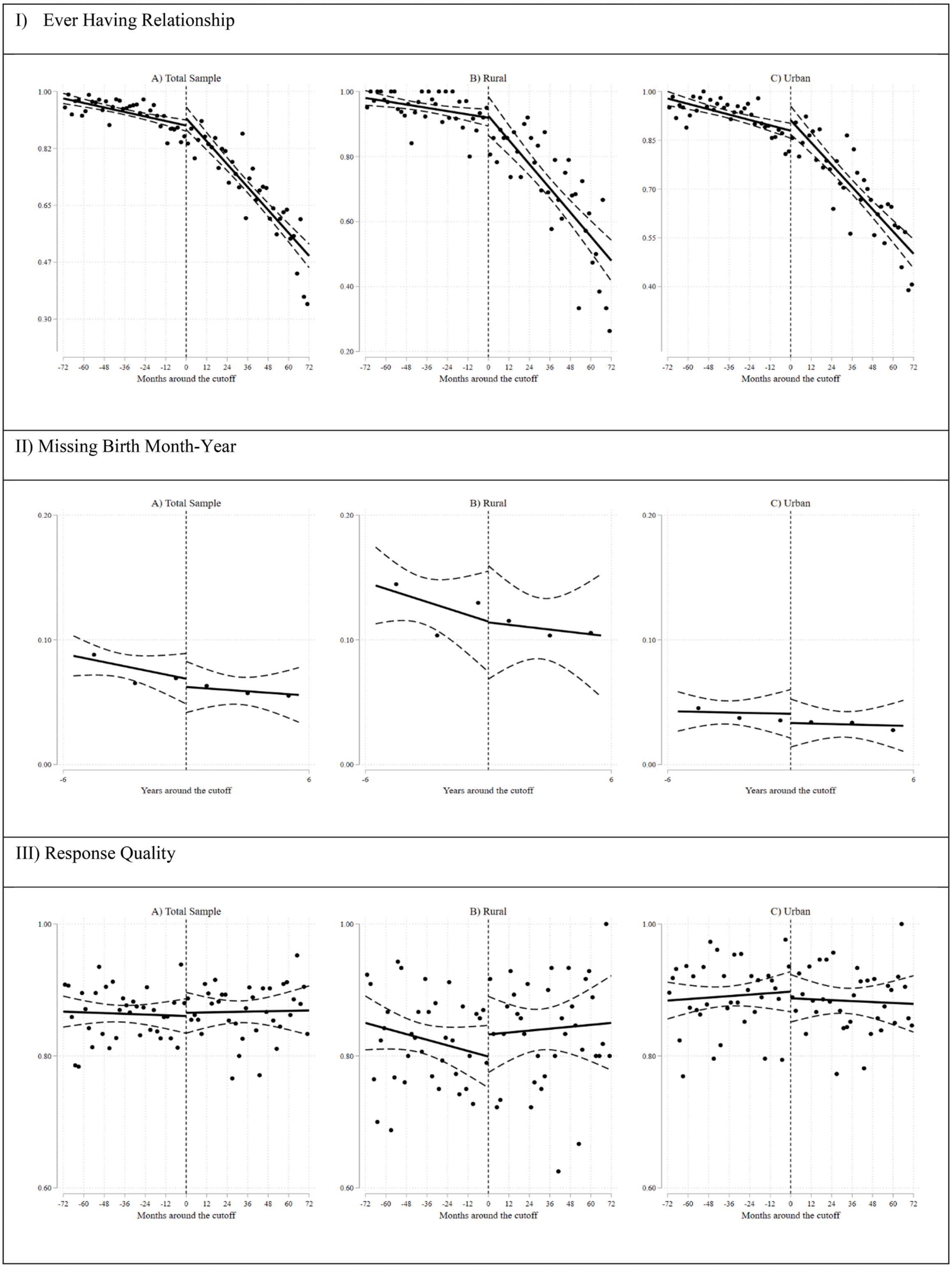

Fig. 1. Policy Effect on Ever Having a Relationship, Missing Month-Year of Birth, and Response Quality.

Table 2
Policy effect on ever having a relationship, missing month-year of birth, and response quality

	Number of Years on Each Side of the Cutoff				
	8	*7*	*6*	*5*	*4*
	I) Ever Having a Relationship (Running Variable: Month-Year of Birth)				
A) Total Sample					
Policy	0.004	0.025	0.013	0.022	0.035
	[0.021]	[0.022]	[0.023]	[0.025]	[0.027]
Observations	6,908	6,212	5,329	4,415	3,461
B) Rural Sample					
Policy	-0.015	0.008	0.018	0.001	0.022
	[0.030]	[0.031]	[0.032]	[0.035]	[0.041]
Observations	2,523	2,272	1,941	1,581	1,239
C) Urban Sample					
Policy	0.009	0.028	0.007	0.032	0.044
	[0.026]	[0.027]	[0.028]	[0.030]	[0.033]
Observations	4,385	3,940	3,388	2,834	2,222
	II) Birth Month is Missing (Running Variable: Year of Birth)				
A) Total Sample					
Policy	-0.020	-0.022*	-0.019	-0.016	-0.025
	[0.012]	[0.012]	[0.013]	[0.016]	[0.014]
Wild Bootstrap p-value	0.322	0.287	0.431	0.640	0.398
Observations	6,195	5,604	4,841	4,055	3,212
B) Rural Sample					
Policy	-0.033	-0.040	-0.028	-0.022	-0.041
	[0.030]	[0.031]	[0.034]	[0.041]	[0.041]
Wild Bootstrap p-value	0.459	0.382	0.607	0.733	0.563
Observations	2,453	2,222	1,905	1,568	1,236
C) Urban Sample					
Policy	-0.010	-0.009	-0.009	-0.007	-0.012
	[0.008]	[0.008]	[0.007]	[0.007]	[0.006]
Wild Bootstrap p-value	0.558	0.610	0.539	0.601	0.422
Observations	3,742	3,382	2,936	2,487	1,976
	III) Response Quality Good or Very Good (Running Variable: Month-Year of Birth)				
A) Total Sample					
Policy	0.004	0.009	0.025	0.039*	0.048*
	[0.018]	[0.019]	[0.021]	[0.023]	[0.024]
Observations	5,792	5,163	4,462	3,743	2,968
B) Rural Sample					
Policy Effect	0.010	0.002	0.029	0.050	0.062
	[0.037]	[0.040]	[0.043]	[0.047]	[0.052]
Observations	2,160	1,929	1,659	1,370	1,084
C) Urban Sample					
Policy	-0.004	0.005	0.012	0.027	0.041
	[0.022]	[0.023]	[0.025]	[0.026]	[0.029]
Observations	3,632	3,234	2,803	2,373	1,884

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence Against Women. In Panel I), the sample includes all women. In Panels II) and III), the sample includes women who have ever had a relationship. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. In addition to the policy dummy and split linear time trends on either side of the cutoff where the running variable is month-year of birth in Panels I) and III) and year of birth in Panel II), the regressions also control for birth-month dummies in Panels I) and III), a dummy for whether the childhood region was a rural area, a dummy for whether the interview language was Turkish, survey year fixed effect, and dummies for 26 NUTS-2 region of residence at age 12. The regressions are weighed using the sample weights. Standard errors are clustered at the month-year of birth level in Panels I) and II) and clustered at the year of birth level in Panel III). Statistical significance *** at the 1 % level, ** at the 5 % level, * at the 10 % level.

jump at the cutoff in both outcomes for the total sample. This jump is also much more pronounced for the rural sample, as reported in earlier studies (Kirdar et al., 2016). Table 3 shows the policy effect on years of schooling and middle school completion status for the total, rural, and urban samples. Panel (I) shows that the policy increases the years of schooling by about 1 year, and this effect is stronger in rural areas than in urban areas. The policy effect in rural areas ranges from 1.3 years with narrow bandwidths to 1.7 years with wider bandwidths, whereas the policy impact in urban areas is about 0.7 to 0.9 years. The patterns of the policy effect on middle school completion are similar. Quantitatively, the policy increases middle school completion by more than 20 ppt with medium to wider bandwidths and about 15–20 ppt with narrow bandwidths.

A notable feature of our data is the 1986 and 1987 birth cohorts' imperfect compliance with the policy, as discussed in other papers (see, e.g., Kirdar et al., 2016, 2018). Due to common early and late school start in Turkey among these birth cohorts, many individuals in the 1986 birth cohort are affected by the policy and some individuals in the 1987 birth cohort are not affected. Consequently, as demonstrated in Table 3, the estimated policy effect decreases but remains statistically significant as we gradually narrow the bandwidth, while the linear time trends grow in magnitude. For instance, for the middle school completion outcome, the pre-policy (post-policy) trend coefficient increases from 0.01 (0.02) for 8-year bandwidths and to 0.04 (0.03) for 3-year bandwidths. As the

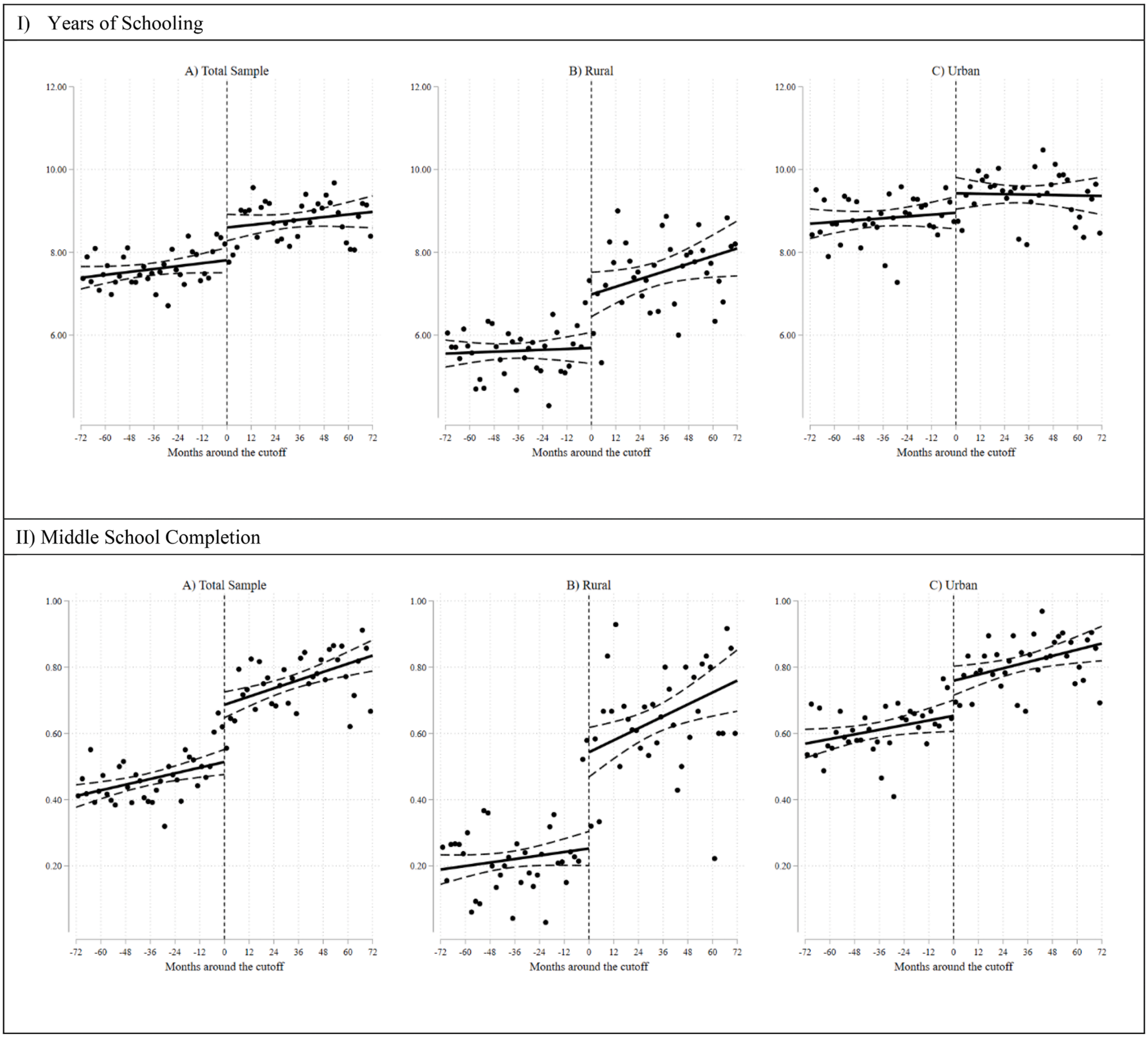

Fig. 2. Policy Effect on Schooling Outcomes.

bandwidth narrows, the slopes of the time trends increase and the estimated policy effect diminishes because the relative significance of the 1986 and 1987 cohorts in the data rises. Therefore, our reduced form estimates on IPV outcomes with narrow bandwidths will also be similarly influenced by the imperfect compliance status of these two birth cohorts.

Due to the imperfect compliance just around the cutoff discussed above, as demonstrated in Table 3, the estimated policy effect on middle school completion is about 21–22 pp with 6- to 8-year bandwidths and about 17–18 pp with 4- and 5-year bandwidths, but only about 14 pp with 3-year bandwidths. Moreover, the policy effect on middle school completion for the urban sample drops from 14.4 pp with 8-year bandwidths, and to 7.5 pp with 3-year bandwidths, losing statistical significance at conventional levels. In other words, there is no evidence of a first stage impact of the policy on middle school completion for the urban sample with 3-year bandwidths. Therefore, in our reduced form parametric RDD estimates of the policy effect on IPV outcomes, the narrowest bandwidth we use is 4 years on each side of the cutoff to ensure the existence of a first stage.

The local polynomial RDD estimates on schooling outcomes in Appendix Table A1 are consistent with those in Table 3. For the total sample, the policy effect on years of schooling is about 0.8 years with the CCFT optimal bandwidth (26 months) but about 1 year with the IK optimal bandwidth (93 months).[19] The CCFT approach uses such narrow bandwidths in our context that there is no statistical

[19] Similarly, the policy effect on middle school completion is about 13 ppt with the CCFT optimal bandwidth (27 months) and 22 ppt with the IK optimal bandwidth (79 months).

Table 3
Policy effect on schooling outcomes—sample of women who have ever had a relationship

	Number of Years on Each Side of the Cutoff					
	8	*7*	*6*	*5*	*4*	*3*
	I) Years of Schooling					
A) Total Sample						
Policy	1.067***	1.174***	1.194***	0.958***	0.998***	0.988***
	[0.219]	[0.224]	[0.244]	[0.270]	[0.296]	[0.359]
Pre-policy trend	0.002	0.000	-0.001	0.004	0.003	0.006
	[0.003]	[0.003]	[0.003]	[0.005]	[0.006]	[0.011]
Post-policy trend	0.001	-0.001	0.000	0.007	0.006	0.003
	[0.004]	[0.004]	[0.005]	[0.006]	[0.008]	[0.012]
Observations	5,829	5,198	4,492	3,767	2,988	2,288
B) Rural Sample						
Policy	1.680***	1.682***	1.698***	1.345***	1.262**	1.385**
	[0.362]	[0.384]	[0.413]	[0.447]	[0.510]	[0.606]
Observations	2,172	1,941	1,668	1,376	1,089	837
C) Urban Sample						
Policy	0.749***	0.913***	0.919***	0.749**	0.809**	0.767*
	[0.269]	[0.272]	[0.294]	[0.328]	[0.361]	[0.406]
Observations	3,657	3,257	2,824	2,391	1,899	1,451
	II) Middle School Completion					
A) Total Sample						
Policy	0.216***	0.214***	0.214***	0.181***	0.171***	0.141***
	[0.028]	[0.030]	[0.033]	[0.035]	[0.040]	[0.047]
Pre-policy trend	0.001**	0.001**	0.001	0.002**	0.002**	0.004**
	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]	[0.002]
Post-policy trend	0.002***	0.002***	0.002***	0.003***	0.003**	0.003*
	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]	[0.001]
Observations	5,829	5,198	4,492	3,767	2,988	2,288
B) Rural Sample						
Policy	0.357***	0.343***	0.347***	0.294***	0.275***	0.279***
	[0.051]	[0.053]	[0.057]	[0.062]	[0.073]	[0.084]
Observations	2,172	1,941	1,668	1,376	1,089	837
C) Urban Sample						
Policy	0.144***	0.148***	0.143***	0.121***	0.112**	0.075
	[0.035]	[0.037]	[0.040]	[0.044]	[0.052]	[0.060]
Observations	3,657	3,257	2,824	2,391	1,899	1,451

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence Against Women. The sample includes women who have ever had a relationship. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. In addition to the policy dummy and split linear time trends on either side of the cutoff where the running variable is month-year of birth, the regressions also control for birth-month dummies, a dummy for whether the childhood region was a rural area, a dummy for whether the interview language was Turkish, survey year fixed effect, and dummies for 26 NUTS-2 region of residence at age 12. The regressions are weighed using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance *** at the 1 % level, ** at the 5 % level, * at the 10 % level.

evidence of a policy impact on years of schooling for the rural sample or on middle school completion for the urban sample, although the coefficient estimates remain sizable. This lack of evidence of an effect on certain schooling outcomes with very narrow bandwidths is assumed to be related to the imperfect compliance status of the 1986 and 1987 birth cohorts, as noted above.[20] For this reason, we consider the local polynomial approach, particularly that using the CCFT optimal bandwidth, as only complementary evidence in our context. In addition, as discussed in Section 4.1, problems related to the policy altering the response quality are evident only for narrow bandwidths.

4.2.2. Policy effect on partners' characteristics

The compulsory school reform could affect women's IPV outcomes not only through its effect on women's education, but also through partners' characteristics; therefore, we also examine the policy effect on partners' schooling. Several previous studies report a significant impact of the policy on men's schooling outcomes, using RDD and the 1987 January cutoff in men's month-year of birth. However, the impact on partners' schooling differs because the running variable in this study is women's month-year of birth, and some women who are on the right-hand side of the cutoff may have partners who are not affected by the policy (when the male partner is sufficiently older than the female partner to be on the other side of the cutoff).

Panel (I) of Fig. 3 provides some suggestive evidence of a positive policy impact on partners' middle school completion, which is

[20] Imperfect compliance in the treatment status of the 1986 and 1987 birth cohorts immediately around the cutoff creates considerable curvature in the potential outcomes, forcing the optimal CCFT bandwidth to be narrow, in the tradeoff between bias and precision.

more apparent for the rural sample. Panel (I) of Table 4 presents the RDD estimates on the policy impact. For the total sample, we observe a positive policy effect on middle school completion, which is statistically significant for all bandwidths except two narrow ones. Quantitatively, the policy increases partners' middle school completion by about 5–7 percentage points. The RDD estimates with the local polynomial approach in Appendix Table A1 corroborate the positive effect. The estimated impact is 8.2 percentage points with the CCFT optimal bandwidth (36 months) and 6.5 percentage points with the IK optimal bandwidth (100 months). The impact for the rural sample in Table 4 is somewhat larger in magnitude overall; however, precision is lower than that for the total sample, presumably due to the smaller sample size. The local polynomial RDD estimates in Appendix Table A1 indicate a positive and statistically significant impact, representing a 13.4 pp increase with the CCFT optimal bandwidth (35 months) and a 9.2 pp increase with the IK optimal bandwidth (98 months). The impact for the urban sample in Table 4 is somewhat lower than the total sample and less precise overall. Similarly, the RDD estimates with a local polynomial approach in Appendix Table A1 are imprecisely estimated. In sum, the evidence suggests a positive policy effect on partners' schooling, which is larger and more precisely estimated for the total and rural samples.

Given the rise in both women's and their partners' education, we could expect a change in marital sorting patterns. For this purpose, we examine how schooling and age gaps between partners change with the policy. The RDD graphs in panel (II) of Fig. 3 indicate a fall in the gap between partners' years of schooling, which is more pronounced for rural areas. Regarding the age gap, the RDD graph in panel (III) suggests that a fall occurs at the cutoff for the rural sample but not for the total or urban samples. Finally, panel (IV) indicates a jump in the fraction of women whose consent are taken in marriages.

Panel (II) of Table 4 shows the parametric RDD estimates regarding the policy effect on the schooling gap. The results indicate that the policy decreases the schooling gap for the total and rural samples. The gap in partners' years of schooling narrows by 0.6 to 0.8 years for the total sample and by about 1 year for the rural sample. The coefficients for the urban sample are also negative but smaller in absolute magnitude and less precisely estimated. For all samples, the evidence is weaker with the very narrow bandwidths in Table 4 and the similarly narrow CCFT optimal bandwidths in Appendix Table A1. However, with the IK optimal bandwidths, the evidence of a narrowing gap persists for the total and rural samples, as in the parametric estimates.

Panel (III) of Table 4 presents the parametric RDD estimates for the policy effect on the age gap between partners. The estimated coefficients are negative and large (about half a year) for the rural sample, and the policy effect on schooling is also stronger for rural areas; however, the results are not statistically significant. The absolute magnitudes of the coefficients for rural areas gradually rise as we narrow the bandwidth. Moreover, the effect becomes statistically significant with the CCFT optimal bandwidths in Appendix Table A1 (35 months). For the total and urban samples, we achieve no conclusive result in the parametric estimates in Table 4; however, for the total sample, the coefficient estimate gradually becomes more negative as the bandwidth narrows. It is negative and statistically significant with the CCFT optimal bandwidth in Appendix Table A1 (23 months). In sum, while there is no evidence of a policy effect on the age gap for the total and urban samples, there is suggestive evidence of the policy reducing the age gap for the rural sample, which is consistent with the findings of Kirdar et al. (2018) that the policy delays teenage marriage among girls.

Finally, panel (IV) of Table 4 shows the parametric RDD estimates for the policy impact on the incidence of women's consent in marriage decisions. Regardless of the bandwidth, there is compelling evidence that the policy increases the probability that women's consent is given in marriage. This result is primarily driven by the rural sample, for which both the coefficient magnitudes and statistical significance are much higher. The nonparametric results in Appendix Table A1 confirm these findings.

The evidence for the rise in partner's schooling and the likelihood that the bride's consent is given in a marriage and the suggestive evidence for the reduction in the partners' age gap in the rural sample imply that mechanisms other than the increased schooling of women have a role in the observed effects on IPV outcomes. In addition, the evidence for the reduction in the schooling gap and the suggestive evidence for the fall in the age gap imply that women's bargaining power in the household increases. This rise in women's bargaining power would contribute to the decline in physical violence we find. In addition, Akyol and Mocan (2020) show that the reform reduced women's propensity to marry a first cousin or a blood relative and increased the likelihood that women meet their husbands outside of family networks. Using data on migration history from the DHS of Turkey, Aydemir et al. (2022) find that the policy changes women's propensity to migrate for marriage purposes. These results imply that the reform changed marriage sorting patterns, which can also affect IPV outcomes.

Another mechanism through which the education policy could affect IPV outcomes is via its effect on partners' employment status. Figure OA3 in Online Appendix A suggests that there is no jump at the cutoff for partners' employment for any sample, and the RDD estimates in Table OA7 in Online Appendix A show no evidence of a policy effect on partners' employment. This lack of a policy effect on men's employment status is consistent with Aydemir and Kirdar (2017), who find a null effect of the policy on men's employment status using the Turkish Household Labor Force Surveys (THLFS).

4.2.3. Policy effects on IPV outcomes

Fig. 4 shows the policy effect on physical, sexual, and psychological violence that women experienced from their partners. Panel (I) suggests a drop in physical violence at the cutoff for the total and rural samples; the mean value at the cutoff on the right-hand side is outside the 95% confidence interval on the left-hand side. In contrast, panels (II) and (III) indicate no evidence of a jump at the cutoff for sexual violence and psychological violence.

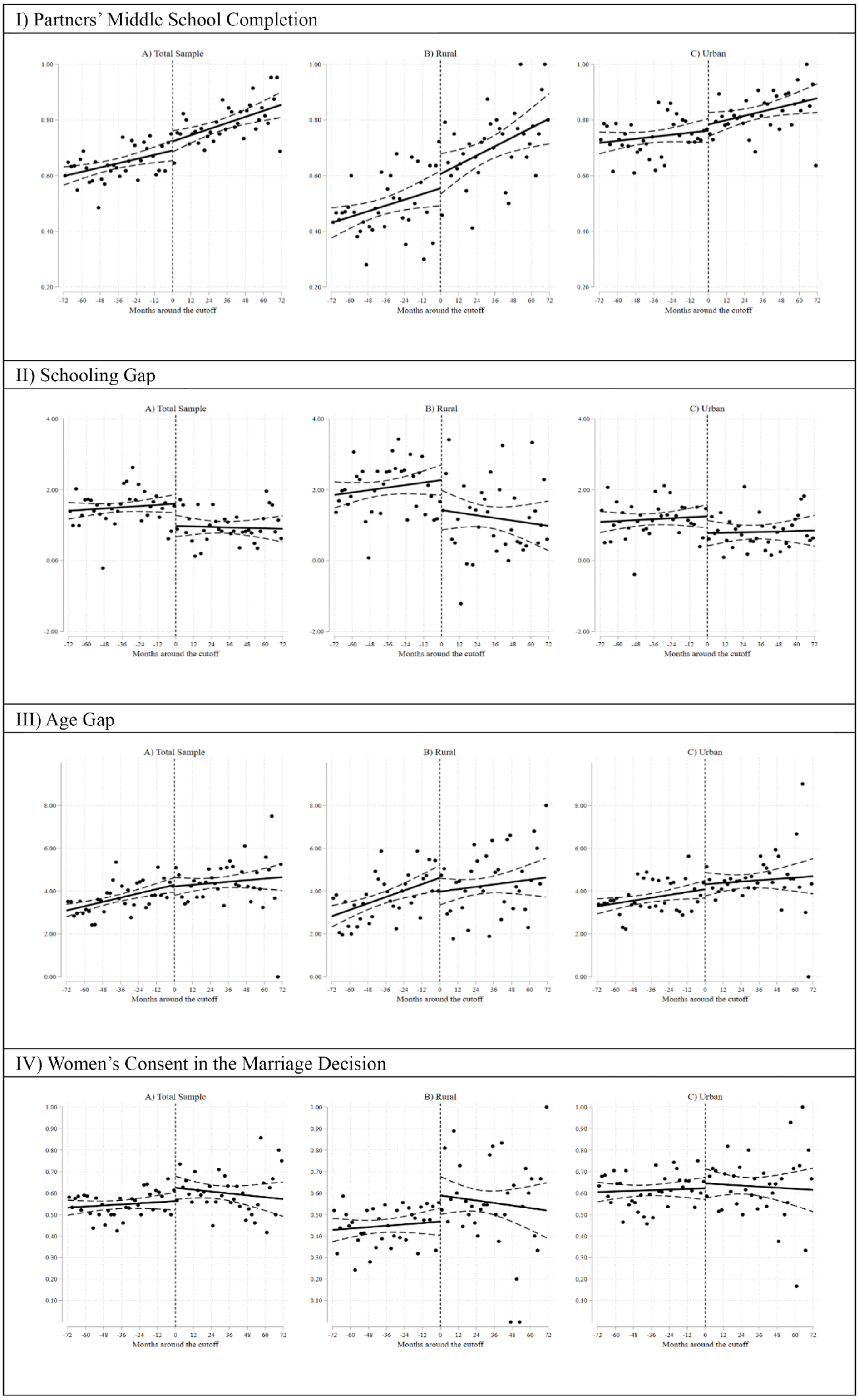

Fig. 3. Policy Effect on Outcomes Related to Partners' Characteristics.

Table 4
Policy effect on partner characteristics

	Number of Years on Each Side of the Cutoff									
	8	*7*	*6*	*5*	*4*	*8*	*7*	*6*	*5*	*4*
	I) Partners' Middle School Completion					**II) Schooling Gap**				
A) Total Sample										
Policy	0.065**	0.060*	0.064*	0.057	0.046	-0.595***	-0.744***	-0.778***	-0.601**	-0.627*
	[0.031]	[0.032]	[0.034]	[0.037]	[0.038]	[0.222]	[0.230]	[0.252]	[0.273]	[0.320]
Observations	5,741	5,116	4,422	3,714	2,950	5,741	5,116	4,422	3,714	2,950
B) Rural Sample										
Policy	0.087*	0.064	0.059	0.046	0.071	-1.082***	-1.169***	-1.215***	-0.991**	-0.724
	[0.052]	[0.055]	[0.058]	[0.062]	[0.068]	[0.344]	[0.360]	[0.383]	[0.397]	[0.442]
Observations	2,139	1,910	1,641	1,356	1,073	2,139	1,910	1,641	1,356	1,073
C) Urban Sample										
Policy	0.051	0.057	0.064	0.055	0.034	-0.347	-0.540**	-0.539*	-0.405	-0.475
	[0.038]	[0.039]	[0.043]	[0.045]	[0.049]	[0.260]	[0.269]	[0.303]	[0.330]	[0.389]
Observations	3,602	3,206	2,781	2,358	1,877	3,602	3,206	2,781	2,358	1,877
	III) Age gap					**IV) Women's Consent in Marriage Decision**				
A) Total Sample										
Policy	0.215	0.123	-0.061	-0.276	-0.228	0.056**	0.077***	0.093***	0.067**	0.080**
	[0.284]	[0.303]	[0.298]	[0.317]	[0.360]	[0.028]	[0.029]	[0.031]	[0.033]	[0.035]
Observations	4,723	4,158	3,556	2,963	2,358	4,743	4,173	3,566	2,972	2,364
B) Rural Sample										
Policy	-0.185	-0.368	-0.451	-0.631	-0.580	0.094*	0.119**	0.128**	0.090	0.108*
	[0.449]	[0.469]	[0.491]	[0.515]	[0.604]	[0.051]	[0.052]	[0.056]	[0.060]	[0.063]
Observations	1,902	1,688	1,445	1,189	950	1,912	1,697	1,451	1,194	953
C) Urban Sample										
Policy	0.489	0.438	0.197	-0.011	0.047	0.045	0.064	0.083*	0.057	0.063
	[0.358]	[0.376]	[0.376]	[0.400]	[0.475]	[0.038]	[0.041]	[0.044]	[0.049]	[0.054]
Observations	2,821	2,470	2,111	1,774	1,408	2,831	2,476	2,115	1,778	1,411

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence Against Women. The sample includes women who have ever had a relationship. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. In addition to the policy dummy and split linear time trends on either side of the cutoff where the running variable is month-year of birth, the regressions also control for birth-month dummies, a dummy for whether the childhood region was a rural area, a dummy for whether the interview language was Turkish, survey year fixed effect, and dummies for 26 NUTS-2 region of residence at age 12. The regressions are weighed using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance *** at the 1 % level, ** at the 5 % level, * at the 10 % level.

Table 5 presents the estimated policy effects on physical, sexual, and psychological violence. Panel (I) reveals that the policy effect on physical violence has a negative sign that is large in magnitude; however, only one of the five bandwidths has statistical significance at the 10% level. When we restrict the sample to rural areas, robust evidence emerges that the policy reduces physical violence. Quantitatively, the policy decreases the average z-score of physical violence by about 0.12 standard deviations for women who lived in rural areas during childhood. For urban areas, we observe no evidence of a policy effect on physical violence. The local polynomial RDD estimates in Appendix Table A2 confirm these findings. The results using both the CCFT and IK optimal bandwidths indicate that the policy reduces physical violence for women with rural childhood residence. Quantitatively, the policy effect with the IK bandwidth is similar to the parametric estimates, whereas the estimated impact with the CCFT bandwidth is somewhat larger.

Panel (II) of Table 5 shows no statistical evidence of a policy effect on sexual violence for the total sample, although the coefficients are consistently negative and not small. For the urban sample, these negative coefficients are even larger in absolute magnitude. Nonetheless, they remain statistically insignificant at conventional levels. The local polynomial RDD estimates in Appendix Table A2 are similar in the sense that the coefficient magnitudes for the total and urban samples are large and negative, but they are not statistically significant at conventional levels.

Panel (III) of Table 5 presents the policy effect on psychological violence. The coefficients for the total sample are about zero. Although the coefficients for the rural sample are somewhat larger, they remain small (0.23–0.38 standard deviations) and statistically insignificant. The local polynomial RDD estimates in Appendix Table A2 confirm the null policy effects on psychological violence for the total sample and the positive but statistically insignificant effects for the rural sample. Essentially, we estimate null policy impacts on psychological violence for the total sample. There is also no evidence of a policy effect for the samples of rural and urban status.

Fig. 5 displays the RDD graphs for financial control behavior, the incidence of partners preventing women from working, and women's employment status. Panels (I) and (II) suggest no evidence of a jump at the cutoff for financial control behavior or partners preventing women from working; however, the graph for women's employment in panel (III) suggests a small jump at the cutoff for the

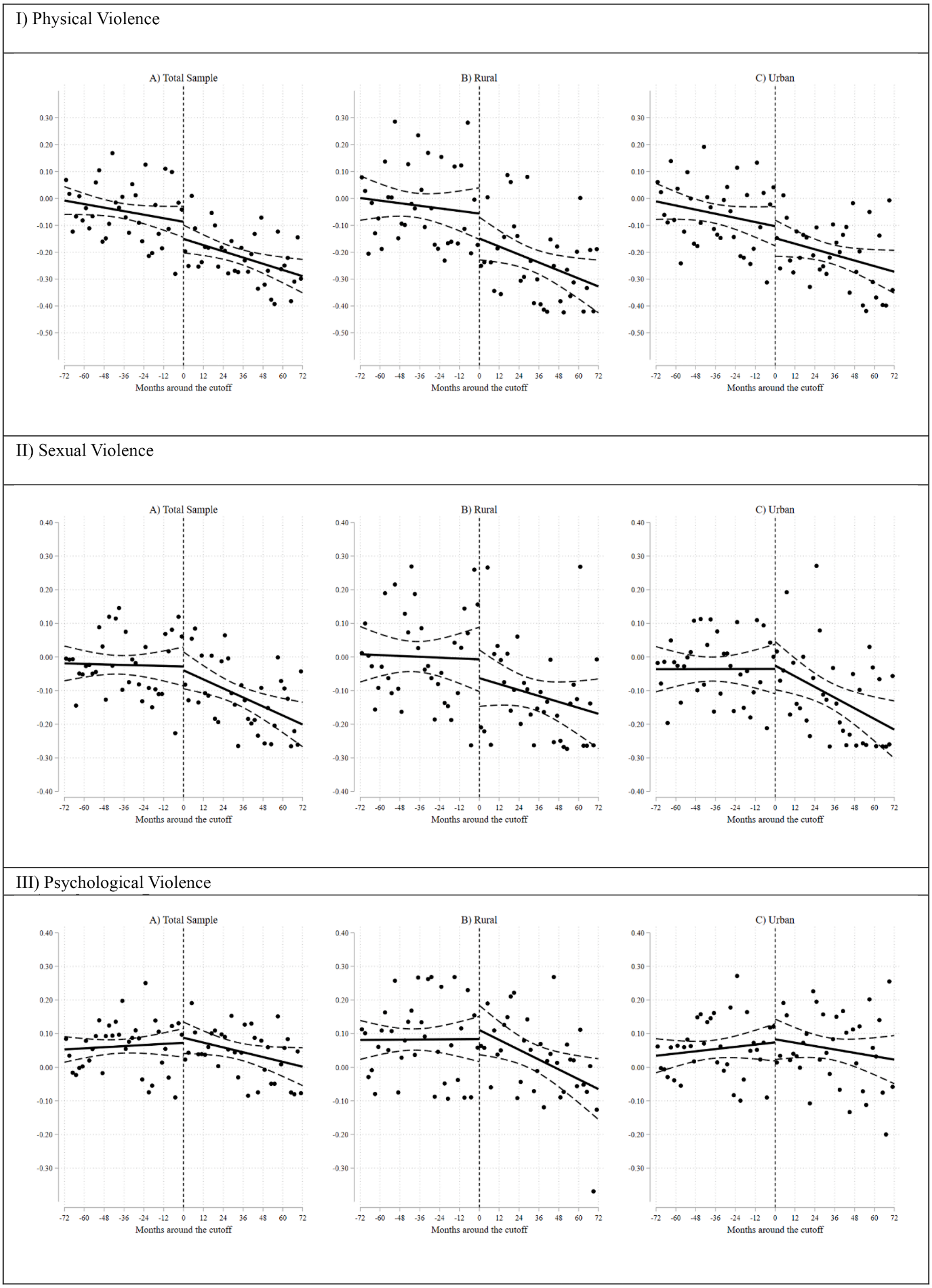

Fig. 4. Policy Effect on Physical, Sexual, and Psychological Violence.

Table 5
Policy Effect on physical, sexual, and psychological violence

	Number of Years on Each Side of the Cutoff				
	8	*7*	*6*	*5*	*4*
	I) Physical Violence				
A) Total Sample					
Policy	-0.043	-0.048	-0.045	-0.075*	-0.076
	[0.036]	[0.038]	[0.042]	[0.044]	[0.048]
Observations	5,815	5,185	4,479	3,756	2,978
B) Rural Sample					
Policy	-0.118**	-0.148**	-0.114*	-0.118*	-0.128*
	[0.059]	[0.061]	[0.062]	[0.065]	[0.075]
Observations	2,170	1,939	1,666	1,374	1,087
C) Urban Sample					
Policy	-0.014	0.002	-0.009	-0.058	-0.053
	[0.043]	[0.045]	[0.050]	[0.053]	[0.056]
Observations	3,645	3,246	2,813	2,382	1,891
	II) Sexual Violence				
A) Total Sample					
Policy	-0.030	-0.032	-0.031	-0.055	-0.030
	[0.041]	[0.042]	[0.048]	[0.053]	[0.059]
Observations	5,814	5,184	4,479	3,756	2,978
B) Rural Sample					
Policy Effect	-0.008	-0.006	-0.019	-0.042	-0.015
	[0.062]	[0.061]	[0.069]	[0.079]	[0.084]
Observations	2,170	1,939	1,666	1,374	1,087
C) Urban Sample					
Policy	-0.047	-0.048	-0.036	-0.061	-0.033
	[0.047]	[0.050]	[0.056]	[0.061]	[0.067]
Observations	3,644	3,245	2,813	2,382	1,891
	III) Psychological Violence				
A) Total Sample					
Policy	-0.006	-0.009	-0.001	-0.007	0.001
	[0.032]	[0.034]	[0.038]	[0.041]	[0.046]
Observations	5,815	5,185	4,479	3,756	2,978
B) Rural Sample					
Policy	0.023	0.034	0.038	0.037	0.026
	[0.049]	[0.051]	[0.056]	[0.060]	[0.068]
Observations	2,170	1,939	1,666	1,374	1,087
C) Urban Sample					
Policy	-0.017	-0.023	-0.011	-0.022	-0.010
	[0.039]	[0.040]	[0.044]	[0.045]	[0.053]
Observations	3,645	3,246	2,813	2,382	1,891

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence Against Women. The sample includes women who have ever had a relationship. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. In addition to the policy dummy and split linear time trends on either side of the cutoff where the running variable is month-year of birth, the regressions also control for birth-month dummies, a dummy for whether the childhood region was a rural area, a dummy for whether the interview language was Turkish, survey year fixed effect, and dummies for 26 NUTS-2 region of residence at age 12. The regressions are weighed using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance *** at the 1 % level, ** at the 5 % level, * at the 10 % level.

total and urban samples but not for the rural sample.

Panel (I) of Table 6 presents the RDD estimates for the policy effect on financial control behavior. There is no evidence of a policy impact for the total sample, although the coefficients are not small. The policy effect for the rural sample is stronger; however, it is statistically significant only for the narrowest bandwidth and at the 10% level. The local polynomial RDD estimates in Appendix Table A2 are consistent with these findings.[21] In sum, we find no evidence of a policy effect on financial control behavior for the total and urban samples. We also achieve no conclusive result regarding the policy effect on financial control behavior for the rural sample, as the result depends on the bandwidth selection. This is the only outcome (the policy effect on financial control behavior for the rural sample) for which the data indicate some suggestive evidence of an adverse policy effect. However, it is important to note that each of the two events that define the financial control behavior variable has a very low incidence, as shown in Table 1; therefore, minimal

[21] There is no evidence of a policy effect on financial control behavior for the total and urban samples, regardless of the optimal bandwidth selection method; however, for rural areas, evidence of a positive policy effect emerges with the CCFT optimal bandwidth (26 months), which is consistent with the parametric estimates based on similarly narrow bandwidths. We also find no statistical evidence of a policy effect with the IK optimal bandwidth, which is much wider.

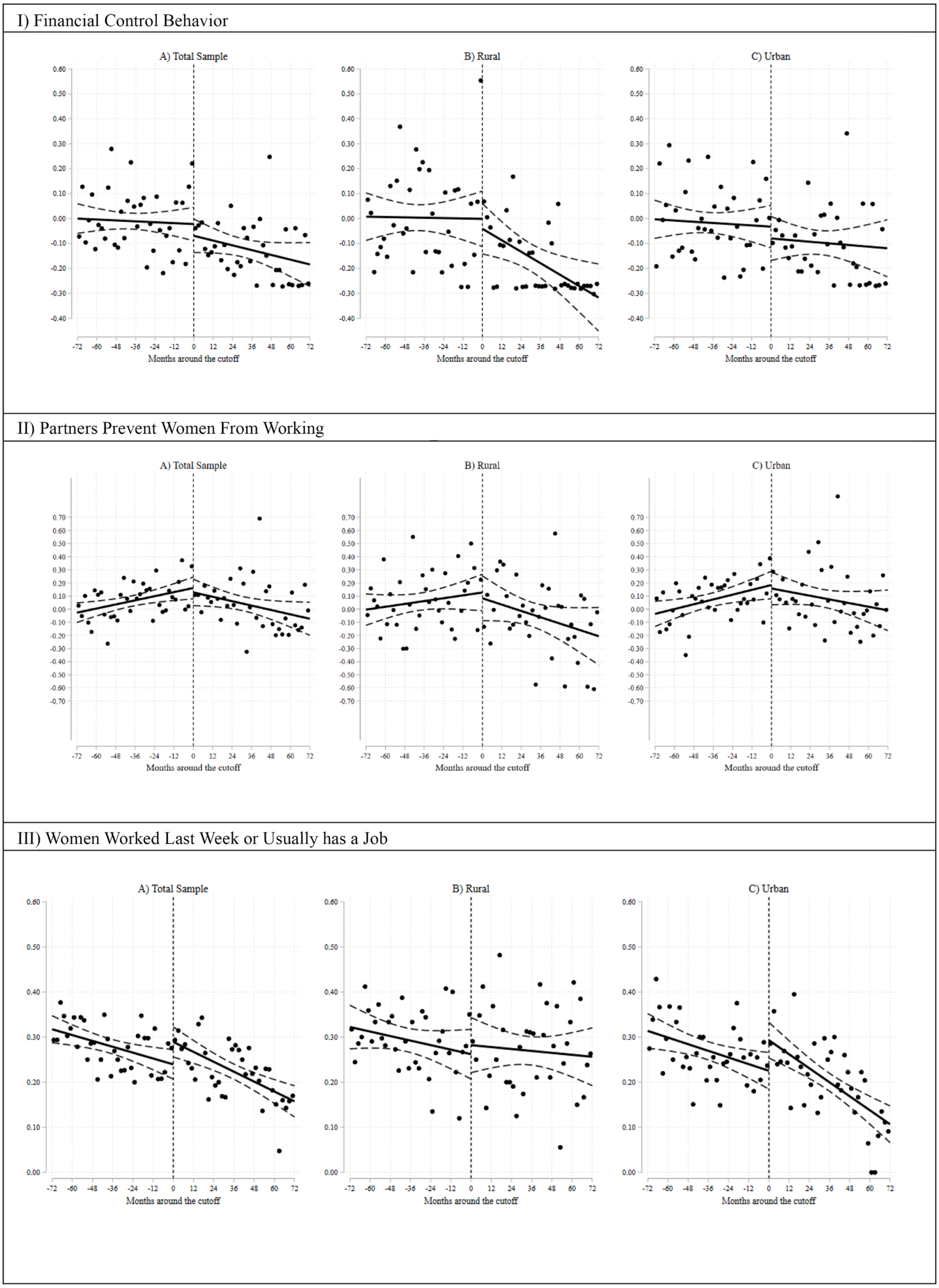

Fig. 5. Policy Effect on Financial Control Behavior and Women's Employment.

Table 6
Policy effect on financial control behavior and employment outcomes

Number of Years on Each Side of the Cutoff					
	8	*7*	*6*	*5*	*4*
	I) Financial Control Behavior				
A) Total Sample					
Policy Effect	0.077	0.073	0.062	0.047	0.060
	[0.053]	[0.055]	[0.059]	[0.063]	[0.072]
Observations	5,455	4,837	4,169	3,505	2,792
B) Rural Sample					
Policy Effect	0.085	0.115	0.134	0.175	0.230*
	[0.089]	[0.091]	[0.099]	[0.109]	[0.124]
Observations	2,059	1,833	1,575	1,301	1,039
C) Urban Sample					
Policy Effect	0.069	0.054	0.028	-0.005	-0.013
	[0.057]	[0.059]	[0.065]	[0.064]	[0.072]
Observations	3,396	3,004	2,594	2,204	1,753
	II) Partner Prevents her from Working				
A) Total Sample					
Policy	-0.047	-0.050	-0.082	-0.090	-0.129
	[0.078]	[0.080]	[0.086]	[0.092]	[0.112]
Observations	5,071	4,507	3,885	3,272	2,608
B) Rural Sample					
Policy	0.023	0.002	-0.005	0.005	-0.027
	[0.120]	[0.123]	[0.135]	[0.145]	[0.166]
Observations	1,820	1,625	1,387	1,143	910
C) Urban Sample					
Policy	-0.069	-0.069	-0.108	-0.123	-0.164
	[0.092]	[0.095]	[0.103]	[0.109]	[0.132]
Observations	3,251	2,882	2,498	2,129	1,698
	III) Women Worked Last Week or Usually have a job				
A) Total Sample					
Policy Effect	0.030	0.030	0.026	0.004	-0.021
	[0.020]	[0.021]	[0.021]	[0.022]	[0.023]
Observations	5,829	5,198	4,492	3,767	2,988
B) Rural Sample					
Policy Effect	-0.013	0.002	-0.009	-0.019	-0.032
	[0.038]	[0.039]	[0.041]	[0.044]	[0.046]
Observations	2,172	1,941	1,668	1,376	1,089
C) Urban Sample					
Policy Effect	0.056**	0.047*	0.041	0.013	-0.009
	[0.027]	[0.028]	[0.029]	[0.031]	[0.034]
Observations	3,657	3,257	2,824	2,391	1,899

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence Against Women. The sample includes women who have ever had a relationship. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. In addition to the policy dummy and split linear time trends on either side of the cutoff where the running variable is month-year of birth, the regressions also control for birth-month dummies, a dummy for whether the childhood region was a rural area, a dummy for whether the interview language was Turkish, survey year fixed effect, and dummies for 26 NUTS-2 region of residence at age 12. The regressions are weighed using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance *** at the 1 % level, ** at the 5 % level, * at the 10 % level.

random events could have a substantial influence on the results.

Notably, the incidence of partners preventing women from working is much higher, as can be seen in Table 1. Panel (II) of Table 6 presents the policy effect on this variable, revealing that the coefficients for the total sample are all negative and large in magnitude, which suggests that the policy decreases the incidence of partners preventing women from working; however, it is statistically insignificant at conventional levels. The magnitudes of the negative coefficients are even larger for the urban sample (where about 30% of women reported facing this behavior). The nonparametric results in Appendix Table A2 are consistent with these findings. Given the large magnitude of the negative coefficients, t-values that are above 1 in most cases, and given the consistency of the coefficient magnitudes and precision across different bandwidths, we can assert that there is suggestive evidence that the policy reduces the incidence of partners preventing women from working for the urban sample.

Panel (III) of Table 6 shows no evidence of a policy effect on women's employment for the total sample. Moreover, while the coefficients with 6- to 8-year bandwidths are positive but statistically insignificant, those with narrower (4- or 5-year) bandwidths are either about zero or negative. The coefficients for the rural sample are negative for all bandwidths but one. The coefficients for the urban sample are positive and statistically significant with wider bandwidths but substantially smaller with narrower bandwidths. The results of nonparametric RDD are consistent with these patterns.[22] In essence, we find no evidence of a policy effect on women's employment for the total and rural samples. For the urban sample, the results are mixed and not robust. Although evidence of a positive impact exists for some bandwidths, the coefficient estimates are highly varied and even negative with other bandwidths.

We also examine a dataset better suited for this purpose, the THLFS. Using the 16 rounds of this dataset from 2004 to 2019, which gives us a sample size above 1 million observations with 10-year bandwidths, we estimate the policy effect using alternative bandwidths. We also calculate wild cluster bootstrapped standard errors due to the small number of clusters resulting from using year of birth as the running variable. Table OA8 in Online Appendix A shows no evidence of a policy effect on women's employment.[23] This is consistent with our findings using the TNSDVW.

Considered together, our findings reveal no evidence for the IVH. First, there is no evidence of a policy effect on women's employment for the total and rural samples. Moreover, we find no adverse effects of the compulsory schooling reform on any of the IPV outcomes examine. In addition, when we dig deeper using rural and urban status, the lack of evidence for IVH does not change. The bandwidths for which we find a positive policy impact on women's employment for urban areas reveal no evidence of an adverse effect on IPV outcomes. Conversely, for the few (and very narrow) bandwidths for which we find a positive policy impact on financial control behavior in rural areas, the policy effect on women's employment is negative, contradicting the IVH.

Our findings show that women are in a more advantaged position in terms of education (the school gap lowers); however, we find no adverse effects of schooling reform on IPV outcomes. Hence, our results are inconsistent with the male backlash hypothesis. In contrast, our findings support the hypothesis that improved outside options for women decrease domestic violence (Aizer, 2010; Farmer and Tiefenthaler, 1997; Tauchen, Witte and Long, 1991; Peterman et al., 2017; Anderberg et al., 2016). The reform not only increased women's education but also reduced women's educational gaps with respect to partners and increased women's agency in choosing a partner.

4.2.4. Policy effect on gender and domestic violence attitudes

Women's reported responses on actual domestic violence outcomes used in the previous section could certainly suffer from measurement error; therefore, we also examine the policy effect on women's attitudes toward gender and domestic violence. Table OA9 of Online Appendix A presents the parametric RDD estimates. We find evidence of a negative policy effect on the opinion that "a woman can spend money without partner's consent" with narrow bandwidths. Similarly, only with narrow bandwidths does evidence indicate that the policy reduces the incidence of opinions that "a child can be beaten for discipline" and "a woman's behavior is a man's responsibility" and increases the incidence of the opinion that "there should be a division of labor at home." All of these findings, except for the opinion that "a woman can spend money without partner's consent," indicate that women's bargaining power and status in the household improve with the policy. In addition, for outcomes for which statistical evidence does not exist, the coefficients' signs are in line with improving women's status. These findings on attitudes are consistent with our conclusions based on the reported domestic violence outcomes overall. It is essential to note that the above findings regarding gender and domestic violence attitudes have statistical validity only with (very) narrow bandwidths and are therefore not robust.

4.3. Further robustness checks

4.3.1. Alternative aggregations of IPV outcomes

In this section, instead of using z-scores, we construct IPV outcomes in different ways. First, we use binary variables for each IPV category (physical, sexual, and psychological violence and financial control behavior), where the variable takes the value of 1 if the woman reports experiencing any of the items in the list of that IPV category and 0 otherwise. The results in Online Appendix Table OA10 show that the qualitative patterns of the coefficients are similar; however, the precision of the estimates is much lower. This is not surprising because we lose significant information with this definition; namely, the intensity and variety of violence women are exposed to.

In a second set of IPV outcomes, we define the outcome for each category equal to the different number of acts of violence the women were exposed to in that category. Finally, to account for the potential that some items in the list of IPV questions may be more correlated with each other than others, we apply principal components analysis to generate the outcome for each IPV category. In Online Appendix A, we present the results for the former group in Table OA11 and the latter group in Table OA12. The estimates are

[22] Appendix Table A2 shows no evidence of a policy effect on women's employment for the total and rural samples with either of the optimal bandwidth methods. However, for the urban sample, the results using the IK optimal bandwidth (which is a wide bandwidth) indicate a positive policy impact, which is consistent with the parametric estimates with wider bandwidths. In contrast, the results with the CCFT optimal bandwidths indicate no evidence of a policy impact also for the urban sample.

[23] Due to the imperfect compliance status of the 1986 and 1987 birth cohorts, we also conduct a donut-hole RDD (excluding these two cohorts). The estimates presented using a donut-hole in Online Appendix Table OA8 reveal suggestive, but inconclusive, evidence of a small positive effect (less than 1 ppt) on women's employment. We also examine the policy effect on other employment outcomes, revealing stronger policy effects on women's full-time and wage employment. According to the estimates using the donut-hole, the policy increases full-time and wage employment by about 1.5 ppt. Although the policy has a null or small effect on employment, it changes the type of employment. It appears that women move into better jobs as a result of the policy.

quite similar to our main estimates in Tables 5 and 6, and our main findings hold.

4.3.2. Alternative definition of psychological violence

The two elements of our definition of financial control behavior (partner ever took her money and partner ever refused to give her money) can also be considered an aspect of psychological violence; hence, in an alternative definition, we add each of these variables into the several acts of the psychological violence index. The results in Online Appendix Table OA13 for this broader definition of psychological violence are similar to those presented in Table 5. The policy effect is almost null for the total sample, null for the urban sample, and positive but statistically insignificant for the rural sample.

5. Revisiting Erten and Keskin (2018)

As previously noted, a recent paper by EK (2018) examines the effect of *women's education* on domestic violence outcomes in Turkey, using use the same compulsory schooling reform as the source of exogenous variation in schooling and the 2008 TNSDVW as the data source.[24] EK find that women's increased education causes a rise in employment. In addition, the authors report that women's education increases psychological violence against women and partners' financial control over women. EK argue that these findings support the *instrumental-violence hypothesis (*IVH*),* which is the use of violence to achieve an underlying goal such as retrieving financial resources. The authors detect these relationships only in one sub-sample of data: women who spent their childhood in rural areas.

Our study improves upon the EK paper by examining a wider range of outcomes and providing a more careful execution of data analysis and empirical methods and a more in-depth examination of the underlying mechanisms.[25] In addition, we reach starkly different conclusions. Unlike EK's findings, our results reveal no evidence for the IVH, which holds for both urban and rural areas. In addition, we demonstrate that it is essential to include examinations of the policy effect on partner characteristics in the observed effect on IPV outcomes in this context, not just the effect accruing from women's higher education level, as done by EK.[26] Since our findings are highly contradictory to EK's, we revisit their empirical analysis, providing the details of our comprehensive replication and reanalysis of their study in Online Appendix B. Here we provide a summary of this analysis.

First, we show that EK's findings are an artifact of the manner in which the authors created a key variable, that which classifies women into rural vs. urban childhood locations. EK define district centers as rural areas, which generates two major problems. First, more than 80% of non-metropolitan district centers are urban areas according to the urban and rural definition in the TNSDVW. Second, many women in central districts of metropolitan areas are reported to live in district centers; hence, EK consider many women living in the country's most developed regions to have lived in rural areas. We show that it is possible to generate a meaningful definition of rural areas—using villages—demonstrating the consequences of EK's flawed definition. EK significantly overestimate the fraction of childhood rural areas—by 35%—and the characteristics of women in rural areas are considerably different from actual rural women. In fact, when we properly define childhood rural areas, we observe a decrease in the magnitude of the estimated effects of the reform on psychological violence and women's employment, but not on financial control behavior; and the impacts on all three variables become (statistically) insignificantly different from zero (see Online Appendix B.1 for details).

In the second part of our reexamination of EK, maintaining EK's definition of childhood rural areas, we demonstrate several serious problems in their analysis. First, we demonstrate that the policy substantially increases the incidence of ever having a relationship for EK's rural sample (the only group for which EK establish evidence), resulting in a potential sample selection problem in their 2SLS estimates. In addition, we find that the policy increases response quality and decreases the incidence of missing information for the month of birth (the running variable) for EK's rural and total samples (see Online Appendix B.2.1 for details). In other words, a failure of the exclusion restriction assumption in EK's analysis is evident for their total sample, in addition to the misclassified rural sample. In contrast, the exclusion restriction assumption holds for our total sample (Table 2) because we use the 2014 round of the TNSDVW as well as the 2008 round. This difference contributes to our diverging results for the total sample. Second, we show that the key identification assumption of the RDD fails for some of their key outcomes, particularly for the financial control behavior variable, which is constructed based on two events that had a very low probability of occurrence (see Online Appendix B.2.2 for details).

Third, we demonstrate serious problems with EK's employment definition, as the authors use different definitions of employment status for men and women. When we define women's employment in the same way that EK define men's employment, we observe a substantial decrease in the magnitude of the policy effect on women's employment and the effects become (statistically) insignificantly different from zero. In addition, using the THLFS, we demonstrate that EK's definition underestimates the actual employment rate of the women in their sample by 41%. Another remarkable issue with their findings on women's employment is the sheer size of the policy effect. The authors estimate that the policy increases the employment of women who grew up in rural areas by a striking 63% (and by 37% for all women), although female employment in Turkey has been low and stagnant for decades (see Online Appendix

[24] A recent study, Zhou and Su (2021), also use compulsory schooling reform in China as an instrument to investigate the effects of education on domestic violence, demonstrating that education decreases physical and sexual violence.

[25] Abdurrahimov and Akyol (2018) conduct a similar analysis using EK's methodology but 2014 data.

[26] In their abstract, EK (2018) claim, "The increase in education among rural women led to an increase in self-reported psychological violence and financial control behavior, without changes in physical violence, partner characteristics, or women's attitudes towards such violence." Although establishing the effect of women's education rather than just the effect of a policy is more generalizable, it requires stronger assumptions (Imbens and Angrist, 1994). The exclusion restriction condition in this setting requires that the policy affect domestic violence variables only through women's education; however, we find that the policy alters partner characteristics.

B.2.3 for details).

Fourth, EK report that the policy increases women's schooling, but not men's, although several earlier studies showed strong evidence that the policy increases men's schooling.[27] The authors' claim regarding the absence of evidence for men is based on an RDD graph using *third-order* polynomials for trends in a context where imperfect compliance exists for the 1986 and 1987 birth cohorts immediately surrounding the cutoff, although they use *linear* polynomials throughout the rest of the paper. We demonstrate that proper RDD graphs and estimations based on the two datasets EK use, in addition to other larger datasets, indicate a robust and large impact on men's schooling, as reported in previous literature (see Online Appendix B.2.4 for details).

6. Conclusion

This study examined how Turkey's 1997 compulsory schooling policy, which substantially increased both men's and women's schooling, affected IPV against women. For this purpose, we use the 2008 and 2014 Turkish National Survey of Domestic Violence Against Women and RDD. We find conclusive evidence that the policy *reduced* physical violence against rural women; this evidence becomes suggestive for the total sample of all women. For the urban sample, we also find large and negative but statistically insignificant effects on sexual violence and partners preventing women from working. Conversely, we find null policy effects on psychological violence. Hence, the policy has had a protective for women against IPV.

As noted, our findings highly contradict those in a previous paper by EK (2018), wherein, using a similar RDD methodology and only the 2008 round of the TNSDVW, the authors determine that *women's education* increases both employment and IPV, supporting the IVH. Their evidence is limited to *only* women who lived in "rural areas during childhood." Reexamining this study, we demonstrate that the evidence provided is a result of the misclassification of two key variables of childhood rural areas and women's employment status. If either of these two variables is defined properly, the evidence for the IVH vanishes. We demonstrate other severe problems in their analysis, including the policy altering the composition of their sample and survey response quality and the failure of the key identification assumption of the RDD for some key outcomes.

A primary finding of our study is that the policy effects are not only realized through women's increased schooling but also through altered partner characteristics. The policy also increased partners' schooling and the probability that women choose their own partners (rather than their parents). Moreover, we find suggestive evidence that the age gap between partners decreased for the rural sample. Therefore, it is essential to include these channels in the observed policy effect on IPV in this context, not just the impact accrued from women's higher education level, as EK claim.

Few previous studies establish a causal relationship between schooling and IPV. Nevertheless, education is one of the factors that can empower women through improved outside options, better income opportunities, and improved labor market outcomes. Some studies find that women's empowerment via improved outside options decreases domestic violence (Aizer, 2010; Farmer and Tiefenthaler, 1996; Tauchen et al., 1991; Peterman et al., 2017). Conversely, other studies show that women's improved outside options increase IPV because partners use violence as an instrument to extract resources from women or control women's behavior (Bloch and Rao, 2002; Eswaran and Malhotra, 2011). Our findings support the hypothesis that the empowerment of women via improved outside options decreases domestic violence.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.euroecorev.2022.104313.

Appendix

None

[27] See, e.g., Kirdar et al., 2010, 2012, 2016, 2018; Aydemir and Kirdar, 2017; Aydemir et al., 2019; Cesur and Mocan, 2018; Dursun et al., 2018; Torun, 2018.

Table A1
Local polynomial RDD estimates I

VARIABLES	(1) Ever having a Relationship	(2) Response Quality Good or Very Good	(3) Years of Schooling	(4) Middle School Completion	(5) Partners' Middle School	(6) Partners' Schooling Gap	(7) Partners' Age Gap	(8) Women's Consent in Marriage Decision
	Total Sample							
	A) Reduced Form-CCFT bandwidths							
Conventional	0.077***	0.063*	0.828**	0.132***	0.082**	0.159	-1.050**	0.098***
	[0.029]	[0.032]	[0.352]	[0.048]	[0.042]	[0.327]	[0.412]	[0.033]
Bias-corrected	0.097***	0.066**	0.700**	0.112**	0.084**	0.377	-1.189***	0.112***
	[0.029]	[0.032]	[0.352]	[0.048]	[0.042]	[0.327]	[0.412]	[0.033]
Robust	0.097***	0.066*	0.700*	0.112**	0.084*	0.377	-1.189**	0.112***
	[0.033]	[0.039]	[0.411]	[0.053]	[0.051]	[0.355]	[0.497]	[0.039]
BW loc. poly.	25.20	33.47	26.06	27.35	36.49	21.78	23.37	31.42
BW bias	50.58	61.09	44.21	54.30	65.15	49.73	45.66	54.47
	B) Reduced Form-IK bandwidths							
Optimal BW (b)	0.028	0.008	1.094***	0.216***	0.065**	-0.562***	-0.201	0.087***
	[0.026]	[0.016]	[0.222]	[0.031]	[0.030]	[0.192]	[0.361]	[0.031]
b/2	0.089***	0.024	0.961***	0.146***	0.041	-0.688***	-1.050**	0.082**
	[0.033]	[0.022]	[0.294]	[0.044]	[0.039]	[0.260]	[0.501]	[0.036]
3b/2	0.018	0.003	0.994***	0.223***	0.063**	-0.551***	-0.050	0.050*
	[0.023]	[0.015]	[0.188]	[0.027]	[0.025]	[0.175]	[0.298]	[0.028]
2b	0.003	-0.003	0.905***	0.217***	0.062***	-0.524***	0.223	0.049*
	[0.021]	[0.014]	[0.179]	[0.023]	[0.023]	[0.172]	[0.285]	[0.026]
BW loc. poly.	48.53	130.4	92.99	78.81	100.5	128.2	47.88	67.01
	Rural Sample							
	A) Reduced Form-CCFT bandwidths							
Conventional	0.018	0.077	0.901	0.263***	0.134*	0.064	-1.079*	0.206***
	[0.041]	[0.061]	[0.562]	[0.075]	[0.074]	[0.577]	[0.568]	[0.062]
Bias-corrected	0.007	0.088	0.763	0.233***	0.139*	0.382	-1.089*	0.239***
	[0.041]	[0.061]	[0.562]	[0.075]	[0.074]	[0.577]	[0.568]	[0.062]
Robust	0.007	0.088	0.763	0.233***	0.139	0.382	-1.089	0.239***
	[0.051]	[0.075]	[0.651]	[0.086]	[0.092]	[0.676]	[0.681]	[0.069]
BW loc. poly.	23.69	33.62	26.92	32.03	34.88	21.86	34.79	30.78
BW bias	42.78	60.45	46.99	63.04	61.80	45.56	60.72	56.97
	B) Reduced Form-IK bandwidths							
Optimal BW (b)	0.010	0.001	1.693***	0.363***	0.092*	-1.246***	-0.403	0.126**
	[0.031]	[0.036]	[0.362]	[0.049]	[0.052]	[0.294]	[0.473]	[0.057]
b/2	0.024	0.082*	1.264**	0.292***	0.061	-1.176***	-0.462	0.155**
	[0.044]	[0.048]	[0.508]	[0.070]	[0.068]	[0.385]	[0.583]	[0.068]
3b/2	-0.029	-0.006	1.784***	0.385***	0.086*	-1.162***	-0.100	0.092*
	[0.027]	[0.029]	[0.324]	[0.044]	[0.044]	[0.282]	[0.406]	[0.050]
2b	-0.046*	-0.024	1.718***	0.392***	0.073*	-1.054***	0.035	0.092**
	[0.025]	[0.028]	[0.305]	[0.042]	[0.042]	[0.287]	[0.399]	[0.047]
BW loc. poly.	83.91	104.7	95.80	101.4	98.39	140	82.31	66.52
	Urban Sample							
	A) Reduced Form-CCFT bandwidths							
Conventional	0.127***	0.054	0.979**	0.093	0.054	-0.156	-0.280	0.095
	[0.038]	[0.034]	[0.410]	[0.064]	[0.050]	[0.429]	[0.496]	[0.060]
Bias-corrected	0.148***	0.055	0.930**	0.068	0.045	-0.039	-0.289	0.098
	[0.038]	[0.034]	[0.410]	[0.064]	[0.050]	[0.429]	[0.496]	[0.060]
Robust	0.148***	0.055	0.930*	0.068	0.045	-0.039	-0.289	0.098
	[0.042]	[0.040]	[0.522]	[0.069]	[0.060]	[0.507]	[0.664]	[0.075]
BW loc. poly.	26.69	34.18	27.23	27.43	39.93	32.47	28.92	26.37
BW bias	54.59	58.29	46.15	54.17	73.10	57.50	52.76	45.64
	B) Reduced Form-IK bandwidths							
Optimal BW (b)	0.045	-0.003	0.651***	0.150***	0.057	-0.279	-0.016	0.069*
	[0.031]	[0.022]	[0.242]	[0.038]	[0.035]	[0.251]	[0.473]	[0.041]
b/2	0.123***	0.039	0.805**	0.088	0.030	-0.562	-1.154**	0.039
	[0.041]	[0.029]	[0.325]	[0.055]	[0.045]	[0.371]	[0.551]	[0.058]
3b/2	0.020	0.007	0.484**	0.148***	0.042	-0.243	-0.070	0.041
	[0.028]	[0.019]	[0.213]	[0.030]	[0.030]	[0.217]	[0.402]	[0.036]
2b	-0.002	0.008	0.469**	0.124***	0.046	-0.154	0.392	0.039
	[0.026]	[0.018]	[0.199]	[0.027]	[0.028]	[0.207]	[0.383]	[0.035]
BW loc. poly.	50.33	95.26	118.4	82.42	111.7	101.3	39.62	82.79

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence Against Women. Except column 1, the sample includes women who have ever had a relationship. We use CCFT optimal bandwidths in panel (A) and IK optimal bandwidths in panel (B). These optimal bandwidths are calculated conditional on covarites and sampling weights, and estimation is done accordingly using the "rdrobust" command of CCFT and "rd" command for IK optimal bandwidths. CCFT bandwidths are MSE-optimal and the degree of local polynomials is one (two for bias correction). Covariates include birth-month dummies, a dummy for whether the childhood region was a rural area, a dummy for whether the interview language was Turkish, survey year fixed effect, and dummies for 26 NUTS-2 region of residence at age 12. Statistical significance *** at the 1 % level, ** at the 5 % level, * at the 10 % level.

Table A2
Local polynomial RDD estimates II

VARIABLES	(1) Ever having a Relationship	(2) Response Quality Good or Very Good	(3) Years of Schooling	(4) Middle School Completion	(5) Partners' Middle School Completion	(6) Partners' Schooling Gap	(7) Partners' Age Gap	(8) Women's Consent in Marriage Decision
Total Sample								
A) Reduced Form-CCFT bandwidths								
Conventional	0.077***	0.063*	0.828**	0.132***	0.082**	0.159	-1.050**	0.098***
	[0.029]	[0.032]	[0.352]	[0.048]	[0.042]	[0.327]	[0.412]	[0.033]
Bias-corrected	0.097***	0.066**	0.700**	0.112**	0.084**	0.377	-1.189***	0.112***
	[0.029]	[0.032]	[0.352]	[0.048]	[0.042]	[0.327]	[0.412]	[0.033]
Robust	0.097***	0.066*	0.700*	0.112**	0.084*	0.377	-1.189**	0.112***
	[0.033]	[0.039]	[0.411]	[0.053]	[0.051]	[0.355]	[0.497]	[0.039]
BW loc. poly.	25.20	33.47	26.06	27.35	36.49	21.78	23.37	31.42
BW bias	50.58	61.09	44.21	54.30	65.15	49.73	45.66	54.47
B) Reduced Form-IK bandwidths								
Optimal BW (b)	0.028	0.008	1.094***	0.216***	0.065**	-0.562***	-0.201	0.087***
	[0.026]	[0.016]	[0.222]	[0.031]	[0.030]	[0.192]	[0.361]	[0.031]
b/2	0.089***	0.024	0.961***	0.146***	0.041	-0.688***	-1.050**	0.082**
	[0.033]	[0.022]	[0.294]	[0.044]	[0.039]	[0.260]	[0.501]	[0.036]
3b/2	0.018	0.003	0.994***	0.223***	0.063**	-0.551***	-0.050	0.050*
	[0.023]	[0.015]	[0.188]	[0.027]	[0.025]	[0.175]	[0.298]	[0.028]
2b	0.003	-0.003	0.905***	0.217***	0.062***	-0.524***	0.223	0.049*
	[0.021]	[0.014]	[0.179]	[0.023]	[0.023]	[0.172]	[0.285]	[0.026]
BW loc. poly.	48.53	130.4	92.99	78.81	100.5	128.2	47.88	67.01
Rural Sample								
A) Reduced Form-CCFT bandwidths								
Conventional	0.018	0.077	0.901	0.263***	0.134*	0.064	-1.079*	0.206***
	[0.041]	[0.061]	[0.562]	[0.075]	[0.074]	[0.577]	[0.568]	[0.062]
Bias-corrected	0.007	0.088	0.763	0.233***	0.139*	0.382	-1.089*	0.239***
	[0.041]	[0.061]	[0.562]	[0.075]	[0.074]	[0.577]	[0.568]	[0.062]
Robust	0.007	0.088	0.763	0.233***	0.139	0.382	-1.089	0.239***
	[0.051]	[0.075]	[0.651]	[0.086]	[0.092]	[0.676]	[0.681]	[0.069]
BW loc. poly.	23.69	33.62	26.92	32.03	34.88	21.86	34.79	30.78
BW bias	42.78	60.45	46.99	63.04	61.80	45.56	60.72	56.97
B) Reduced Form-IK bandwidths								
Optimal BW (b)	0.010	0.001	1.693***	0.363***	0.092*	-1.246***	-0.403	0.126**
	[0.031]	[0.036]	[0.362]	[0.049]	[0.052]	[0.294]	[0.473]	[0.057]
b/2	0.024	0.082*	1.264**	0.292***	0.061	-1.176***	-0.462	0.155**
	[0.044]	[0.048]	[0.508]	[0.070]	[0.068]	[0.385]	[0.583]	[0.068]
3b/2	-0.029	-0.006	1.784***	0.385***	0.086*	-1.162***	-0.100	0.092*
	[0.027]	[0.029]	[0.324]	[0.044]	[0.044]	[0.282]	[0.406]	[0.050]
2b	-0.046*	-0.024	1.718***	0.392***	0.073*	-1.054***	0.035	0.092**
	[0.025]	[0.028]	[0.305]	[0.042]	[0.042]	[0.287]	[0.399]	[0.047]
BW loc. poly.	83.91	104.7	95.80	101.4	98.39	140	82.31	66.52
Urban Sample								
A) Reduced Form-CCFT bandwidths								
Conventional	0.127***	0.054	0.979**	0.093	0.054	-0.156	-0.280	0.095
	[0.038]	[0.034]	[0.410]	[0.064]	[0.050]	[0.429]	[0.496]	[0.060]
Bias-corrected	0.148***	0.055	0.930**	0.068	0.045	-0.039	-0.289	0.098
	[0.038]	[0.034]	[0.410]	[0.064]	[0.050]	[0.429]	[0.496]	[0.060]
Robust	0.148***	0.055	0.930*	0.068	0.045	-0.039	-0.289	0.098
	[0.042]	[0.040]	[0.522]	[0.069]	[0.060]	[0.507]	[0.664]	[0.075]
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	[0.031]	[0.022]	[0.242]	[0.038]	[0.035]	[0.251]	[0.473]	[0.041]
b/2	0.123***	0.039	0.805**	0.088	0.030	-0.562	-1.154**	0.039
	[0.041]	[0.029]	[0.325]	[0.055]	[0.045]	[0.371]	[0.551]	[0.058]
3b/2	0.020	0.007	0.484**	0.148***	0.042	-0.243	-0.070	0.041
	[0.028]	[0.019]	[0.213]	[0.030]	[0.030]	[0.217]	[0.402]	[0.036]
2b	-0.002	0.008	0.469**	0.124***	0.046	-0.154	0.392	0.039
	[0.026]	[0.018]	[0.199]	[0.027]	[0.028]	[0.207]	[0.383]	[0.035]
BW loc. poly.	50.33	95.26	118.4	82.42	111.7	101.3	39.62	82.79

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence Against Women. Except column 1, the sample includes women who have ever had a relationship. We use CCFT optimal bandwidths in panel (A) and IK optimal bandwidths in panel (B). These optimal bandwidths are calculated conditional on covarites and sampling weights, and estimation is done accordingly using the "rdrobust" command of CCFT and "rd" command for IK optimal bandwidths. CCFT bandwidths are MSE-optimal and the degree of local polynomials is one (two for bias correction). Covariates include birth-month dummies, a dummy for whether the childhood region was a rural area, a dummy for whether the interview language was Turkish, survey year fixed effect, and dummies for 26 NUTS-2 region of residence at age 12. Statistical significance *** at the 1 % level, ** at the 5 % level, * at the 10 % level.

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