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Does Education Really Cause Domestic Violence? Revisiting the Turkish Data*

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Abstract

Using the 2008 Turkish National Survey of Domestic Violence against Women (NSDVW) and the 1997 compulsory schooling policy as an instrument for schooling, Erten and Keskin (2018, henceforth EK), published in *AEJ–Applied Economics*, find that *women's education* increases the psychological violence and financial control behavior that they face from their partners. The authors also claim that the incidence of financial control behavior rises because women become more likely to be employed—supporting the instrumental violence hypothesis. They present this evidence *only* for women who live in what they call "rural areas during childhood".

We first show that the evidence EK provide—which exists only for childhood rural areas—is a result of their misclassification of the rural areas variable. We show that once this variable is defined properly, the evidence for their findings vanishes. Second, ignoring the misclassification of the rural status variable, we demonstrate a number of serious flaws in their empirical analysis: (i) selection bias resulting from the policy altering the composition of women in their sample, (ii) failure of the main identification assumption of RDD for some key outcomes, (iii) failure of the exclusion restriction assumption, (iv) inconsistency in the definition of employment variable across men and women (and a problematic definition of employment of women), (v) elementary mistakes in data cleaning, RDD estimation, and interpretation of the estimates. In addition, the evidence for urban areas contradicts the hypothesis they claim to hold for rural areas.

Then, we examine the policy effect on domestic violence outcomes using both 2008 and 2014 TNSDVW datasets. We find null policy effects on psychological violence and almost null effects on women's employment, and positive but statistically insignificant effects on partners' financial control behavior. Hence, our findings do not support the instrumental violence hypothesis, and this holds true for the rural sample as well. The only robust evidence the data provide is that the policy reduces physical violence for women with rural childhood residence.

JEL Classification: I21, I28, J12, J16, J24, O15, O18

Keywords: intimate partner violence, education, compulsory schooling, psychological violence, financial control behavior, women's employment

1. Introduction

Economists have contributed to the investigation of the causes of domestic violence by examining the effect of better employment, income opportunities, higher wages, and autonomy of women (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 to 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), restriction on alcohol sales (Luca et al. 2015), civil conflict and war service (La Mattina 2017, Cesur and Sabia 2016) and service usage of battered women (Farmer and Tiefenthaler 1996).

A recent paper, published in the *American Economic Journal – Applied Economics* by Erten and Keskin (2018, henceforth EK), contributes to this important line of inquiry by examining the effect of women's education on certain domestic violence outcomes in Turkey. The authors use the 1997 compulsory schooling reform as the source of exogenous variation in schooling and employ the 2008 Turkey National Survey of Domestic Violence against Women (TNSDVW) as the data source. EK find that increased education of women causes a rise in women's employment. The authors report that women's education leads to a rise in psychological violence against women and increases partners' financial control over women. EK argue that these findings support the *instrumental-violence hypothesis,* which is the use of violence to achieve an underlying goal such as retrieving financial resources. The authors report that these relationships are detected only in one sub-sample of data: women who have ever had a relationship and who spent their childhood in rural areas.

It is important to re-visit EK's analysis using the same data set but with full transparency of the empirical methods and procedures due to the following reasons. First, the research question has substantive importance, and the results have potentially important implications for public policy in developing countries. Second, some of the secondary findings of EK—such as that the reform had no impact on men's education—contradict a large body of research that analyzed the same Turkish reform.

In the first part of our analysis, we show that EK's findings are an artifact of the way the authors created a key variable: the variable that classifies women into rural vs. urban childhood locations. As we show in detail, when this variable is defined properly, the statistical evidence for their findings—which exists only for their rural areas—vanish. In the second part of our

analysis, maintaining EK's flawed childhood rural areas definition, we demonstrate several serious problems in EK's empirical analysis. Finally, in the third part of our analysis, we ask the same questions using both 2008 and 2014 rounds of the TNSDVW and reach substantially different results from EK.

In the first part of our analysis, we describe how EK misclassify the survey respondents into urban vs. rural childhood locations. Remarkably, the paper does not mention the fact that no information on the rural and urban status of the childhood place of residence exists in the 2008 TNSDVW. The data require EK to combine different variables for movers (who change their location between age 12 and the time of the survey) and stayers in defining this variable.[1] This causes two major problems: (i) their definition of childhood rural areas is *inconsistent* across movers and stayers because EK's rural areas for movers (*district centers*, subdistricts or villages during childhood) and rural areas for stayers (locations with a population below 10,*000*) are very different, (ii) they significantly overestimate the fraction of rural areas among movers, and hence the fraction of overall rural areas (by about 16 percentage points (ppt)).

The reason for their overestimation of the fraction of rural areas is that they define "district centers" as rural areas for movers. This is highly problematic for two reasons: (i) more than 80% of non-metropolitan district centers are rural areas according to the rural definition in the 2008 TNSDVW, (ii) many women in central districts of metropolitan areas are reported to live in district centers; hence, many women living in the most developed regions of the country are taken by EK to live in rural areas. Moreover, their definition of rural areas suffers from sample selection as it is more likely to include movers than stayers.

Although it is not possible to generate a rural/urban identifier during childhood for movers in the TNSDVW, it is possible to generate province center/district center/village status during childhood for both movers and stayers. In fact, we generate a well-defined variable for "villages during childhood" and show that it approximates rural areas very well. We show that once childhood rural areas are defined properly—or in any way other than authors' selection, 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 effects on all three variables become (statistically) insignificantly different from zero.

[1] For stayers, a rural vs. urban identifier is available in the dataset, where rural areas are defined as locations with a population below 10,000.

In essence, the evidence EK provide for rural areas simply results from the severe misclassification of the rural variable. In addition, their results do not apply to the full sample of women. Among the three key variables that EK use to support the instrumental violence hypothesis, no statistical evidence of a policy effect on two (psychological violence and financial control behavior) are provided by EK's estimates for the total sample.[2] Only for women's employment, EK provide statistical evidence at the 5% level for the total sample. However, we show that this results from the particular definition EK choose for women's employment—which is also highly problematic, as discussed below.

In the second part of our analysis, maintaining EK's flawed definition of childhood rural areas, we demonstrate several serious problems in their empirical analysis. First, we show 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 month of birth information—the running variable in EK analysis—for EK's rural and total samples. Second, we show that the key identification assumption of RDD—the continuity of potential outcomes over the running variable—fails for some of their key outcomes. For instance, with placebo cutoff points, the continuity assumption for the financial control behavior variable in EK's rural sample fails 30% of the time with a 10% statistical significance level.

Third, we demonstrate major issues regarding the failure of the exclusion restriction assumption. EK's assertion that the reform had no impact on men's education—which is based on misleading RDD graphs with no estimation results provided—is the polar opposite of the well-established result from dozens of studies that analyzed the same reform and reported the impact of the reform on the educational attainment of both sexes.[3] This point is relevant because the existence of the reform's impact on men's education would violate the exclusion restriction (that the reform had an impact on spousal violence only through its influence on women's education). We show that the reform registered an impact on men's education in the sample of EK.

Fourth, we demonstrate several problems with their employment definition (one of the three key variables in their analysis). EK use different definitions for the employment status of men and

[2] For the full sample, their t-value for the psychological violence variable is less than one, and the effect on financial control behavior is not statistically significant at the 10% level (Table 4 in EK).

[3] 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.

women. We show that when women's employment is defined in the same way EK define men's employment, we observe a substantial decrease in the magnitude of the estimated effects of the reform on women's employment, and the effects become (statistically) insignificantly different from zero. In addition, using the Turkish Household Labor Force Surveys (THLFS), we show that EK's definition of women's employment 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. They estimate that the policy increases employment of women who grew up in rural areas by a striking 63% (and by 37% for all women). It is difficult to understand how female employment in Turkey, which has been low and stagnant for decades, could give such a drastic response. Using the THLFS, which is certainly more apt for studying this issue, we demonstrate that the policy effect on women's employment is either null or small (at most 1 ppt)—compared to the 5.6 ppt they estimate for the full sample.

Fifth, EK provide the results for their rural and total samples—but not for their urban sample. When we replicate the EK analysis for their urban sample, we find evidence that contradicts the instrumental violence hypothesis. Finally, EK's data generation, estimation, and interpretation of their results have problems. The years of schooling variable, which is the endogenous variable in EK's 2SLS regressions, is not constructed properly. For those who have never been to school (17% in the dataset), EK assign missing status rather than zero. Similarly, the partner's years of schooling variable suffers from the same mistake. In their local polynomial RDD estimations, they first calculate an optimal bandwidth given their dependent and running variables. Then, in the range of this bandwidth, they run a regression also controlling for several additional covariates and using sampling weights—although the bandwidth calculation in their first stage does not account for these additional covariates and sampling weights. EK construct z-scores for their domestic violence variables; however, in the interpretation of their results, they claim that the policy increases psychological violence by 12 ppt and financial control behavior by 24 ppt—which must be interpreted in terms of standard deviations.

In the third part of our analysis, we carry out our own analysis also using an RDD and both 2008 and 2014 rounds of the TNSDVW. Unlike EK, we focus on the policy effect rather than the effect of women's schooling—because of the failure of the exclusion restriction assumption. We find null policy effect on psychological violence, contrary to the findings of EK. While we estimate a positive policy effect on financial control behavior, it is not statistically significant at the conventional levels and much smaller in magnitude than what EK estimate. Moreover, no

evidence is observed for a policy effect on women's employment; hence, no evidence exists for the instrumental violence hypothesis either. When we restrict our analysis to rural areas—the only sample for which EK provide evidence for the instrumental violence hypothesis—we find that the policy effect on financial control behavior grows in magnitude and becomes even statistically significant for some bandwidths. However, we estimate a null policy effect on the employment of women who grew up in rural areas—hence, rejecting the instrumental violence hypothesis again. For the rural sample, no evidence of a policy effect on psychological violence exists—also contrary to the EK's findings. A new finding is that 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.

Section 2 provides brief background information, and Section 3 gives descriptions of data and methods. In Section 4, we explain the problem in the definition of rural vs. urban areas during childhood in EK and show how the results change with a proper definition. In Section 5, given the flawed definition of rural and urban areas in EK, we provide details of each of the above-mentioned problems in a transparent and methodical way to show whether and how they alter the results. In Section 6, we present the results of our own analysis, and Section 7 concludes.

2. Brief Background Information

Before 1997, basic education in Turkey was comprised of five years of compulsory primary school and three years of optional middle school. In 1997, with law number 4306, the duration of compulsory schooling was extended from five to eight years, merging primary and middle schools into "primary education", and the primary school diploma after five years of schooling was abolished. Since then, a middle school diploma has been awarded to those who have completed eight years of schooling. The new law went into effect immediately in the fall of 1998, affecting those who were in the fourth or a lower grade level at the end of the 1996–97 school year. Hence, children who started school in the 1993–94 school year or afterward were bound by the policy. According to the relevant law at that time, a child is supposed to start school in the fall semester of the year that she/he turns 72 months old.[4] Therefore, children born in 1987 and afterward are 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

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

(early and late school start age) was quite common. As a result, some children among the 1986 birth cohort are affected by the policy and some among the 1987 birth cohort are not.

To implement the new policy successfully, public spending on education was increased. The public investment budget share of Turkey's Ministry of National Education (MONE), which was 15% in 1996 and 1997 before the policy, jumped to 37.3% in 1998 and remained at around 30% until 2000 (Kirdar et al., 2016). The reform had a substantial impact on enrollment rates. Statistics from the MONE of Turkey show that from the 1997–98 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, which is equivalent to 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).

3. Data and Methodology

The data EK use come from Turkey's National Survey on Domestic Violence against Women (NSDVW) of 2008, a nationally representative survey covering 17,168 households. In each household, only one woman, 15-59-year-old, is chosen randomly and interviewed from July to September. In the data collection process, the Ethical and Safety guidelines developed by the World Health Organization were followed to collect the data in the most reliable way. The survey includes 12,795 women for whom the information is available on the level of education, birth month and year, childhood region, type of childhood region, marriage history, presence and intensity of spousal violence, and behavioral patterns of the husband/partner.

In terms of methodology, EK state that they use local polynomial (nonparametric) RDD methods. However, what they actually do is that using the Imbens-Kalyanaraman (IK, 2012) algorithm, they first estimate a bandwidth given their dependent and running variables. Then, in the range of this bandwidth, they run a regression also controlling for several additional covariates and using sampling weights—although the bandwidth calculation in their first stage does not account for these additional covariates and sampling weights.[5] In other words, the

[5] They also use the Calonico-Cattaneo-Farrell-Titiunik (CCFT) algorithm in the robustness checks given in their appendix. However, this is also subject to the same problematic two-stage approach.

bandwidth that they use in their regression is arbitrary and does not represent their specification and data.[6] Moreover, they use the same bandwidth in their sharp and fuzzy RDD.[7]

We use both parametric and nonparametric (local polynomial) approaches, where the running variable is month-year of birth. In our parametric approach—rather than choosing an arbitrary bandwidth as EK do—we use several alternative bandwidths, thereby assessing the robustness of our findings. In particular, we show the estimates for 8 different bandwidths from 10-years to 3-years on each side, incremented by one year at each time. Note that "optimal bandwidths" of EK lie within this bandwidth range. For instance, in their main results for domestic violence variables in Table 4 of their text, EK's optimal bandwidths range from 59 to 140 months (about 5 to 12 years).

In addition, we replicate their findings using "their optimal IK bandwidths" and use the more recent local polynomial approach of Calonico-Cattaneo-Farrell-Titiunik (CCFT, 2016)—where we choose actual optimal bandwidths conditional on the specification used, unlike in EK.[8] However, we approach the results of local polynomial methods with caution *in this context and with the size of the data in hand*. The imperfect compliance for the 1986 and 1987 birth cohorts, which are immediately around the cutoff, and the relative sparsity of the observations around the cutoff in this small dataset yield the application of a local polynomial approach—that typically chooses very narrow bandwidths—potentially dangerous. We find that the local polynomial estimates are quite sensitive to alternative bandwidths and bandwidth types in the CCFT approach. Hence, we view the results of local polynomial approaches—in this context—only as supporting evidence.

EK also use the 2014 Turkish Household Labor Force Survey (THLFS) as a complementary dataset to analyze the policy effect on men's schooling. This is because while the main dataset does not have the month of birth information for men, this particular round of the THLFS has.

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[6] Their approach is not local also in the sense that running an OLS in the second stage, they are essentially using a uniform kernel; hence, not putting more weight on the data points closer to the cutoff—which local regressions typically do.

7 It is important to note that EK's unorthodox implementation of the RDD routine is not because the frontier in the RDD literature has been recently shifting. At the time of the publication of their paper, Stata (which the authors use) had already "rd" and "rdrobust" commands, which the authors could use to estimate the reduced form or the 2SLS estimates in one step with an optimal bandwidth that reflects their specification. The Stata command "rd" has been available since 2011 (Nichols, 2011) and "rdrobust" since 2014 (Calonico et al., 2014).

[8] We use the "rd" Stata command (Nichols, 2011) in implementing the estimation with IK bandwidths and "rdrobust" Stata command (Calonico et al., 2017) in the estimation with CCFT bandwidths.

However, in the 2014 THLFS, the information on month-of-birth is missing for 12.6% of the observations; and among the non-missing observations, 19.2% are born in January. Moreover, month-of-birth is highly correlated with education; while 24.3% of individuals born in January have no degree, this percentage is 12.3 for those born in December.[9] Despite all these data quality issues, EK still use this month-year of birth information in the 2014 THLFS. An alternative would be to pool all annual rounds of the THLFS, which results in over a million observations, and use year of birth as the running variable.[10] In our analysis of the policy effect on men's schooling and on men's and women's employment outcomes (for which THLFS is a better data source), we take this approach (as well as replicating their approach). One might be worried about year of birth as the running variable because this could force the researcher to compare cohorts that are born further apart from each other. However, the bandwidths we take are 3 to 10 years with the parametric approach (regardless of the running variable)—which lie within the range of the optimal bandwidths of EK (which is from 5 to 12 years).[11]

In our RDD specifications, in addition to the policy dummy, we use split linear time trends on each side of the cutoff. In addition, we use certain covariates and sampling weights, as in EK. The covariates that we (and EK) use include a dummy for the Turkish language, dummies for 26 NUTS-2 level regions of childhood residence,[12] dummies for birth months, and a dummy for rural childhood residence. Standard errors are clustered at the month-year of birth as in EK.

4. Flawed Definition of Rural Areas during Childhood

We start this section by reminding the reader that the definition of the rural and urban status of childhood place of residence is critical in the EK study because *the statistical evidence provided in EK holds only for women who lived in rural areas during childhood*.

[9] Information on the month of birth in the TNSDVW (as well as other datasets gathered by the Institute of Population Studies of Hacettepe University, such as the Turkish Demographic and Health Survey) seems to be much cleaner. Turkish Household Labor Force Surveys are conducted by the Turkish Statistical Institute.

[10] Month of birth information is not available in other rounds of the THLFS, which is available annually.

[11] Another issue when the running variable is year of birth is the few clusters problem in the estimation of standard errors. For this reason, we calculate Wild-cluster bootstrapped standard errors (Cameron et al, 2008, 2015).

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

4.1 Understanding EK's Definition of Childhood Rural Areas

The only information that EK provide on the way they generate childhood rural and urban status in their paper is as follows: *"Our data contain information on the type of region in which each woman has lived through the age of 12 (e.g., whether in a village, a district, or a province) from the 2008 NSDVW survey. This information allows us to construct an indicator of pre-reform rural residence, as the age for starting junior high school in Turkey is 12 years old."* In fact, EK combine information from two separate questions to generate their "childhood rural areas" definition. This is illustrated in Table 1. For women whose current place of residence is different from their childhood place of residence (who we call "movers"), a question elicits their childhood location of residence in the form "village/district center/province center".[13] For women whose current residence is identical to their childhood residence (who we call "stayers"), the survey includes a rural/urban identifier—where rural refers to locations with a population below 10,000. What EK do is that for movers, they take "district centers, village or subdistricts" as rural areas, whereas they take "rural identifier" available in the dataset for stayers.

Table 1. Characterization of EK's Definition of Rural Areas, 2008 TNSDVW

	Childhood Place ≠ Current Place **(MOVERS)**	Childhood Place = Current Place **(STAYERS)**
Variable Definition	Province center, district center, village or subdistrict	Rural/Urban
EK Choice	District center, village or subdistrict	Rural

We show that this childhood rural/urban definition by EK is highly problematic because most district centers—which they take as rural areas in the definition for movers, who constitute 68% of the women in the data—are actually urban areas.[14] This causes two major problems: (i) their definition of childhood rural areas is *inconsistent* across movers and stayers because their rural areas for movers (*district centers*, subdistricts or villages during their childhood) and their rural areas for stayers (locations with a population below *10,000*) are very different, (ii) they significantly overestimate the fraction of rural areas among movers and, hence, the fraction of

[13] The question is as follows: "Until you were 12 years old, where did you live for most of the time? Was this place then a province center, a district center, a subdistrict or a village? Or was it abroad?"

[14] In fact, 59% of the women in the sample with 10-year intervals around the cutoff and 57.5% of the women in the sample with 5-year intervals around the cutoff are movers.

overall rural areas. In essence, their definition of childhood rural areas is both inconsistent (across movers and stayers) and meaningless (because it includes many urban areas).

4.2 Why is EK's Definition Highly Problematic?

We first provide brief background information on the administrative units of Turkey, in particular with regard to province center/district center/village status of locations. Turkey has 81 provinces, which are divided into several districts. Typically, the largest city in each province is designated as the province center. This province center is also the center of the district it belongs to. Other districts are also designated a district center, again typically which is the most populated city/town in that district. Each district has several villages.[15] Some provinces, however, do not have province centers. These provinces contain the major cities of the country, such as Istanbul, Ankara, and Izmir. Technically, Istanbul does not have a province center (because the city essential covers a substantial fraction of the land area of the province), but several district centers combine to make up the city of 15 million residents.

Turkish Statistical Institute, which is in charge of collecting the censuses, defines a location as urban if the population of that location is above 20,000. On the other hand, the Hacettepe Institute of Population Studies, which provides the dataset used in this study (as well as the Turkish Demographic and Health Surveys), uses a population threshold of 10,000 for urban areas. Table 2 shows the fraction of urban areas for province centers/district centers/villages according to the 1985, 1990, and 2000 censuses. Here, we also calculate the share urban according to the 10,000 threshold for district centers. As can be seen from this table, the majority of district centers are, in fact, urban areas. With the 10,000 population threshold that EK uses in their definition for stayers, more than 80% of district centers are urban areas. It is important to note here that these district centers do not include the central districts of metropolitan areas.[16]

Table 2. Type of Location of Residence and Rural/Urban Status (%) – Turkish Censuses

[15] This administrative structure was valid until 2013, after which some changes were made.

[16] In fact, in the 1985 Census, the central districts of metropolitan areas (three provinces had such centers in 1985) are classified as district centers. In that case, the share of urban areas jumps from 56.95% to 77.36%. Hence, we make a correction for the 1985 Census definition to align with those of 1990 and 2000 censuses.

	1985 Census		1990 Census		2000 Census	
	Urban (20,000 threshold)	Urban (10,000 threshold)	Urban (20,000 threshold)	Urban (10,000 threshold)	Urban (20,000 threshold)	Urban (10,000 threshold)
Province Center	95.86		97.57		97.64	
District Center	56.95	82.01	58.33	79.89	65.38	84.73
Village/Subdistrict	2.62		4.04		5.3	

Notes: Metropolitan district centers are defined as province centers.

In other words, while EK defines non-metropolitan district centers as rural areas, more than 80% of them are urban areas. The second problem arises from the assumption of EK that women living in the central districts of metropolitan areas live in province centers. This is not true in the 2008 TNSDVW, as shown in Table 3. According to the information on childhood type of location and province of location, 38.1% of Ankara, 31.1% of Istanbul, and 45.2% of Izmir residents report that they live in district centers. Any researcher slightly familiar with the context would realize that these percentages are impossibly high because most of the population of these provinces live in the central metropolitan cities of these provinces. In fact, according to the 2000 Census, only 9.4% of Ankara, 3.6% of Istanbul and 15.6% of Izmir residents live in district centers.

Table 3: Type of Location of Residence during Childhood for Provinces with Major Metropolitan Centers (%)

A) 2008 TNSDVW

	Childhood Location of Residence (Movers)					Current Location of Residence (Everybody)	
	Province Center	District Center	Village/ Subdistrict	EK Rural		Urban	Rural
Ankara	36.4	38.1	25.5	63.6	Ankara	90.5	9.5
Istanbul	60.5	31.1	8.5	39.5	Istanbul	87.8	12.2
Izmir	27.6	45.2	27.1	72.4	Izmir	78.7	21.3
Turkey	22.5	24.0	53.6	77.5	Turkey	73.7	26.3

B) 2000 Census

	Province Center	District Center	Village/ Subdistrict		Urban	Rural
Ankara	79.0	9.4	11.6	Ankara	86.3	13.7
Istanbul	87.2	3.6	9.2	Istanbul	95.5	4.5
Izmir	66.4	15.6	18.0	Izmir	77.6	22.4

The reason for this overestimation of district centers in the 2008 TNSDVW is the following. In the questionnaire, movers are asked to report whether they lived in a province center, a district center or a village during childhood. (Although, administratively, no province center exists for these metropolitan centers, respondents in the survey are unfortunately asked to report a province center or a district center.) For instance, a woman living in Besiktas/Istanbul (the district with the highest development index in Turkey) could report living in a province center or a district center. If she reports it as a district center, this major part of Istanbul would be classified as a rural area with the EK definition. Even though, in the implementation guidelines, interviewers are instructed to check these responses and revise accordingly, the fact that more than 31% of Istanbul, 38% of Ankara, and 45% of Izmir residents are reported to live in district centers during childhood clearly indicates that a substantial fraction of women living in central districts of metropolitan areas are classified as rural with the EK definition.

In essence, EK's treatment of district centers is problematic for two reasons: (i) more than 80% of non-metropolitan district centers are urban areas according to the rural definition in the 2008 TNSDVW, (ii) many women in central districts of metropolitan areas are reported to live in district centers; hence, many women living in the most developed regions of the country are taken by EK to live in rural areas.

4.3 Why not use Villages as a Proxy for Rural Areas?

Table 4 illustrates the data available in the survey from a different angle. Information on childhood location of residence is available only in the form of province center/district center/village for movers. In constrast, information on the current location of residence is available both in the form of the province center/district center/village and in the form of rural/urban residence—for both movers and stayers.[17] Therefore, while it is not possible to generate a rural/urban identifier during childhood for movers, it is possible to generate province center/district center/village status during childhood for both movers and stayers.

Table 4. Information on Rural/Urban Status and Type of Location in the 2008 TNSDVW

[17] Although the information on the province center/district center/village status of the current location is not provided as a single variable in the data, it is possible to construct it using the information on the district center and village codes. Therefore, we can have the village/district center/province center status for both movers and stayers.

	During Childhood	Current Location
Rural/Urban	No	Yes: STAYERS AND MOVERS. (2)
Province/District/Village	Yes: ONLY FOR MOVERS. (1)	**Yes: STAYERS AND MOVERS. (3)**

Notes: Data pieces (1) and (2) are used in EK, but not (3).

Here, we show that it is possible to generate a well-defined variable for "villages during childhood", which approximates rural areas very well. First, as shown in Table 4, the information on childhood village status is available for both movers and stayers. Hence, it is possible to generate a consistent definition—unlike the one by EK. Second, the fraction of residents in villages is a very good approximation to the fraction living in rural areas. As shown in Table 2, while most province and district centers are urban areas, almost all villages are rural areas. Moreover, in the 2008 TNSDVW, the percentage of women living in villages (26.3%) is very similar to the fraction living in rural areas (26.6%). If EK defined rural areas as villages rather than villages and district centers, they would calculate the childhood rural areas for movers as 8.5% in Istanbul, 25.5% in Ankara, and 27.1% in Izmir. These percentages, as can be seen from Table 3, are much more in line with the fraction living in rural areas based on the current location of residence.

Essentially, it is difficult to understand the choice of EK in defining the childhood rural residence. While villages are a very good approximation to rural areas, district centers are certainly not.

4.4 Consequences of EK's Definition of Rural Areas

According to the definition of EK, 64.3% of the women in the 2008 TNSDVW have rural childhood residence. However, using the information on village/district center/province center status for both movers and stayers, shown in Table 4, we calculate that only 47.7% of women in the TNSDVW lived in villages during childhood. This percentage that we calculate is consistent with the numbers in the Turkish Demographic and Health Surveys.[18] Moreover, in the 2008 TNSDVW, as discussed above, the percentage of women living in villages (26.3%) is very similar to the fraction living in rural areas (26.6%). Hence, taking the fraction of women living in rural areas during childhood as 64.3%, EK overestimate this fraction by *about 16 ppt.*[19]

[18] In fact, of women aged 15–49, 46% lived in villages during childhood according to the 2008 TDHS.

[19] According to Turkish censuses, the fraction of individuals living in rural areas was 48.5% in 1990 and 40.6% in 2000. (These are upper bounds as the rural definition has a 20,000 threshold, unlike the 2008 TNSDVW). The

In addition, EK not only overestimate the fraction of rural areas but also generate an arbitrary childhood rural sample by using different definitions for movers and stayers. In order to understand the sample selection caused by defining district centers as rural areas for movers, we compare certain characteristics of rural areas with those of villages and of villages and district centers together (which EK use) in Table 5. While we cannot make this comparison for the childhood location of residence (as rural/urban information is not available for childhood), we can make this comparison for the current location of residence. When we restrict the sample to women who ever had a relationship within 60-month bandwidths around the cutoff, as in EK, we see that the numbers of observations for rural areas (458) and for villages (461) are very similar, whereas the number of observations for villages and district centers is 1,520. The variables given in Table 5 are the same as those in panels (A)-(C) of Table 1 of EK.

Table 5 clearly shows that villages provide a good approximation to rural areas, whereas the combination of villages and district centers is substantially different from rural areas. While the mean value of years of schooling is 6.98 in rural areas, it is 6.97 in villages but 8.04 in villages and district centers. This implies that movers are more educated than stayers in EK's rural sample, and their sample is more likely to include movers by construction.[20] Similarly, the sample of villages and district centers is much different from rural areas in terms of other educational outcomes (particularly in high school completion), employment outcomes (much lower employment in agriculture), partner's schooling, and asset ownership, among others.

4.5 Results with Alternative Definitions of Rural Areas

Next, we examine how the results presented in EK change with alternative definitions for rural areas during childhood. Since the evidence presented in EK goes via the variables of psychological violence, financial control behavior, and employment, we focus on them. Estimation results are given in Table 6, with various bandwidths from 3 to 10 years on each side of the cutoff. Note that the arbitrarily selected bandwidths of EK lie within the range of our bandwidths. Table 6 has four panels. In panel (I), we use the EK definition of rural areas—which will call improper and inconsistent. In panel (II), we take the proper but inconsistent definition of villages for movers and rural areas for stayers. In panel (III), the proper and

women in the EK sample were born in the 1980s and 1990s. While these numbers are also consistent with the fraction living in villages during childhood, they are significantly lower than the 64.3% with the EK definition.

[20] Aydemir, Kirdar, and Torun (2019) show that women affected by the reform are more likely to migrate, which makes EK's rural definition also endogenous to the reform.

consistent definition of villages as rural areas for both movers and stayers is used. Finally, in panel (IV), we examine the results when we take villages and district centers—consistently over time—as the areas of interest.[21]

As shown in panel (I), our findings are consistent with those in EK when we use their definition.[22] However, the picture completely changes when we move into panel (II)—with a proper definition of rural areas. For the psychological violence and employment variables, we observe a decrease in the magnitude of the estimated effects of the reform, and the effects become (statistically) insignificantly different from zero. For the financial control variable, statistical significance also vanishes; it remains only for two of the eight bandwidths. However, the magnitudes of the coefficients do not fall much for this variable. We will discuss the reasons for this later in Section 5.2.

When we examine panel (III), with the proper and consistent definition, the results are similar to those in panel (II). In this case, however, statistical evidence for a policy effect on financial control behavior does not exist for any of the bandwidths. Moreover, the coefficients on policy effect on employment are even smaller for all bandwidths. The coefficient estimate of 0.074 for both 5-year and 6-year intervals with the EK sample falls to 0.040 and 0.029, respectively—although the evidence on this variable is crucial for EK's evidence on the instrumental violence hypothesis. Finally, in panel (IV), we take the sample as district centers and villages. Although this definition certainly does not stand for rural areas, it is consistent across movers and stayers—unlike the definition in EK. This sample does not provide support for EK's findings, either.[23]

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[21] Although this definition is also improper for rural areas like the EK definition, it is still better than the EK definition because it at least uses a consistent definition across movers and stayers.

[22] Evidence for a policy effect on psychological violence exists for all bandwidths, and evidence for a policy effect on financial control behavior and on employment each exists for six of the eight bandwidths. For psychological violence, in Table 4 in their text, EK reports a reduced form estimate of 0.123 with a bandwidth of 75 months, while our estimate is 0.128 with 84 months and 0.136 with 72 months. For financial control behavior, EK's estimate of policy effect is 0.235 with a bandwidth of 71 months, which is statistically significant only at the 10% level. Our estimate with a bandwidth of 72 months is 0.228, which is also statistically significant at the 10% level, where it is 0.190 and not statistically significant at the conventional levels with a bandwidth of 60 months. Finally, EK's estimate of the policy effect on employment is 0.082 with a bandwidth of 78 months. Our estimate is 0.086 with a bandwidth of 84 months and 0.074 with a bandwidth of 72 months. Hence, the results agree.

[23] For each of the three key variables, statistical significance at the 10% level exists only for 2 of the 9 bandwidths. Moreover, these significant cases are all for different bandwidths.

Next, for the same four different definitions of rural areas during childhood, Table A1 in Appendix A presents the results of various local polynomial approaches. In panel (A), we use the IK optimal bandwidths of EK. Using these optimal bandwidths, we estimate local linear regressions on both sides of the cutoff using a triangular kernel and accounting for covariates and sampling weights. As discussed earlier, this approach is not right because the optimal bandwidths do not take covariates and sampling weight into consideration. We carry out this exercise mainly to replicate the EK results. In panel (B), we use the CCFT local polynomial approach. Here, unlike in panel (A), the bandwidths are actually optimal; in other words, they are selected according to the specification that allows for covariates and sampling weights. In panel (C), we use the CCFT approach to estimate fuzzy RDD, where the optimal bandwidths are generally somewhat wider than those in panel (B).

As can be seen in panel (1A) for "EK rural areas" and "EK optimal bandwidths", their findings and the evidence for the instrumental violence hypothesis hold. Psychological violence, financial control behavior and employment all increase. However, panel (1B) shows that even with their rural sample, once we use proper optimal bandwidths using the CCFT method, the evidence for the policy effect on women's employment and hence their evidence for instrumental violence hypothesis vanishes. Moreover, the 2SLS results in panel (1C) indicate no evidence of an effect of an additional year of schooling on psychological violence or employment. Panels (2A) and (3A) show that even with their optimal bandwidths, the evidence for their findings completely disappears once appropriate definitions of rural areas are used—as in Table 6. The CCFT method estimates in panels (2) and (3) indicate evidence of an effect of financial control behavior but not on women's employment. Therefore, EK's instrumental violence hypothesis fails again.

Here, it is also important to note that, with the proper and consistent definition of rural areas during childhood in panel (3), the results with CCFT bandwidths indicate a policy effect on financial control behavior—although for no bandwidth ranging from 3 to 10 years does such evidence exist in Table 6. The bandwidth that the CCFT approach chooses for this variable is very narrow: 19 months (36 months for the bias). This highlights the importance of examining the results of local polynomial approaches with optimal bandwidths along with the results of parametric approaches with alternative bandwidths.

5 Replications with their Flawed Rural Definition

In this section, we use the exact EK data and list the several serious problems in their empirical analysis with their data of choice.

5.1 Selection Bias – Policy Effect on Ever Having a Relationship, Reporting Missing Month of Birth, and Response Quality

EK reduce their sample to those who have ever had a relationship because the variables on domestic violence are elicited only for these individuals in the dataset. EK provide an RDD graph of ever-had-a-relationship status and marital status in Figure 3 in their text and estimation results in their Online Appendix—which shows that the policy did not affect ever-had-a-relationship status. *Remarkably, they do not present this analysis for rural areas*—although their key findings are only for rural areas. This is important because the existence of a policy effect on ever-having a relationship would change the composition of women in the sample—and, hence, potentially result in a selection bias in their 2SLS estimates. Another issue that would also cause a potential selection bias is a policy effect on providing missing month-year of birth information, which is the running variable in EK analysis. In this subsection, we first examine these two issues that would change the composition of women at the cutoff.

At the end of the survey, interviewers provide an assessment of the quality of the respondents' responses. The options include poor, acceptable, good or very good. We coded response quality as 1 if it is good or very good, and 0 otherwise. We also examine the existence of a policy effect on this variable about response quality. 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.

In Figure 1, we provide RDD graphs for the policy effect on: (i) ever-had-a relationship status, (ii) reporting missing birth-month information, (iii) response quality. Unlike EK, we do it for the rural and urban sample of EK, as well as the total sample, and we adjust for covariates and sampling weights as in their regressions. Panel (A) of Figure 1 indicates a jump in the ever-had-a-relationship status for EK's rural areas, but not for urban or all areas. Panel (B) suggests substantial drops in the fraction reporting missing birth-month information for EK's rural

sample and the total sample.[24] Finally, panel (C) shows a significant rise in response quality for EK's rural sample and to a lesser degree for the total sample.

Next, using both global (parametric) and local polynomial (nonparametric) approaches, we present the estimates for the policy effect on: (i) ever-had-a relationship status, (ii) missing birth-month information, (iii) response quality. In the parametric approach, given in Table 7, we use split linear polynomials on either side of the cutoff and move gradually, one year at a time, from a global bandwidth of 10-year-intervals on each side to local 3-year-intervals. We do it for rural areas, urban areas, and the full sample—using the EK specification with covariates and sampling weights.

For the rural sample, panel (I) of Table 7 shows that the policy increases the ever-had-a-relationship status in rural areas by about 7–9 ppt, and panel (II) indicates that policy decreases the probability of not reporting month-year of birth information by about 5–9 ppt. For the total sample, the policy effect on ever having a relationship is large and positive but statistically insignificant. However, statistical evidence exists of a policy effect on having missing month of birth information; the policy reduces the probability of reporting missing month of birth information by about 4 ppt. In addition, the results presented in panel (III) show a large positive policy effect on response quality for the rural sample; the probability of providing a good response rises by about 10 ppt. For the total sample, similarly, we observe evidence of a large and positive policy impact on response quality. In Table A2 of Appendix A, we present nonparametric RDD results using the CCFT optimal bandwidths. The results are overall consistent with those in Table 7. A difference is that evidence of a policy effect on ever having a relationship emerges also for the total sample.

In essence, both the nonparametric and parametric results—regardless of the bandwidth—indicate a policy effect on ever having a relationship, reporting a missing month of birth information, and response quality for the rural sample and on reporting a missing month of birth information and response quality for the total sample. Moreover, the large magnitudes of these effects indicate a potentially important sample selection bias to the degree that women who are pushed by the policy to have a relationship or report a non-missing month of birth information are different from the sample of women who already have a relationship in terms of their propensity to face domestic violence.

[24] For the missing month of birth variable in panel (B), the running variable is year of birth.

This selection problem would cause a bias in the 2SLS estimates—due to the change in the sample composition—but not in the reduced form policy effects. However, it would change the interpretation of the policy effect. The observed policy effects on IPV outcomes would partly result from the changes in the sample composition—not from an increase in education. Similarly, the observed positive policy effect on response quality would change the interpretation of the policy effect. EK's findings on the policy effect on IPV outcomes might be resulting from the fact that more educated women might be reporting IPV experiences better.

5.2 Failure of the Continuity Assumption of RDD

The fundamental identifying assumption in RDD is that potential outcome distributions are smooth around the cutoff. Although this assumption is not directly testable, three diagnostics are commonly used in the literature to test its plausibility: (i) continuity of the score density around the cutoff, (ii) null treatment effects on pre-treatment covariates, and (iii) null treatment effects at artificial cutoff values. While EK conducts the first two diagnostics, they do not carry out the third—which we do here.

For this purpose, we first split the data into two: (i) a sample that includes individuals who are not affected by the policy—those who are born in 1986 or earlier (call this sample A), and (ii) a sample that includes those who are affected by the policy—those who are born in 1987 and afterward (call this sample B). In each case, we take several alternative cutoffs. With sample A, we start with the alternative cutoff of January 1985, so that there remain at least 2 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 and gradually shift 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 from 2 to 4 years only due to the shorter maximum bandwidth. The results are presented in Table 8 for financial control behavior and employment. Here, we focus on these two of the three key variables because the analysis in the previous section showed that EK results for these variables are not robust. The results for the psychological violence variable are left to Table A3 in Appendix A.

In Table 8 and Table A3, we have 54 estimates for sample (A) (6 bandwidths for 9 alternative cutoffs). For psychological violence in panel (A) of Table A3, of the 54 estimates, 5 yield a statistically significant result at least at the 10% level, which is expected as 5/54 is less than 10%. However, for financial control behavior in panel (A) of Table 8, 16 of the 54 estimates

yield statistically significant results—which is roughly 30%. This is unlikely to be random. A problem with the financial control behavior data is that there are many zeros—which yields the results very volatile and raises serious doubts about the continuity of the potential outcome distribution for this variable. Similarly, panel (A) of Table 8 shows that for the employment variable, 12 of the 54 estimates—more than 22%—yields statistically significant results at the 10% level, which casts doubt on the continuity assumption for this variable. There are also a notable number of zeros for this variable, not as much as that for financial violence but more than that for psychological violence.[25]

Panel (B) of Table 8 indicates similar results. For the financial control behavior variable, 5 of the 21 estimates (24%) and for the employment variable 9 of the 21 estimates (43%) are statistically significant at least at the 10% level. We would put less emphasis on panel (B), though, as the bandwidths are narrower than what EK take.

5.3 Failure of the Exclusion Restriction Assumption -- Policy Effect on Men's Schooling

In the abstract, EK 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 in a single country is more generalizable, it requires stronger assumptions. It is well known since Imbens and Angrist (1994) that the main condition for the validity of an instrumental variable is the exclusion restriction condition—which, in this setting, requires that the policy affect domestic violence variables only through women's education. This, in turn, requires that the policy have no effect on the schooling of these women's husbands or partners.

In this published paper using 2SLS estimation, remarkably, the authors do not make any reference to the exclusion restriction assumption. At the same time, they show a RDD graph on the policy effect on men's schooling using a different dataset, the 2014 Turkish Household Labor Force Survey (THLFS),[26] because this dataset includes information on the month of birth of men, whereas the original dataset does not (it includes only for women). EK use *third-degree*

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[25] These findings are not driven by the narrow bandwidths. If we were to take the bandwidths from 5 to 7 years only—this is the range of most of EK's bandwidths—10 of the 27 estimates (37%) for the financial control behavior variable and 8 of the 27 estimates for the employment variable (30%) yield statistically significant results.

[26] In this dataset, the information on month-of-birth is missing for 12.6 percent of the observations; and among non-missing observations, 19.2 percent are born in the month of January. Moreover, while 24.3 percent of individuals born in January have no degree, this percentage is 12.3 for those born in December.

polynomials on each side of the cutoff in their RDD graphs for schooling—although they use linear polynomials for all other graphs in their paper. In addition, no estimation results are provided on the policy effect on men's schooling using the THLFS in EK.

In this section, we show that the RDD graph in EK's paper (Figure 4B in their text) on the policy effect on men's junior high school completion is misleading. Our RDD graphs, in fact, show a clear jump at the cutoff. Moreover, unlike EK who provide no estimation results at all, we present estimation results using both parametric and nonparametric methods. In fact, with the THLFS, it is easier to reach meaningful results with nonparametric methods due to its much bigger sample size. Both parametric and nonparametric methods indicate conclusive and large effects on men's schooling. The details are given next.

Figure 2 provides RDD graphs with two alternative bandwidths. Unlike EK, who take third order polynomials on both sides of the cutoff, we take linear trends, as it is apt with these relatively narrow bandwidths (Gelman and Imbens, 2019). Both panels indicate a clear jump at the cutoff for both men and women. Moreover, the 95% confidence intervals do not overlap. A surprising feature of EK's paper on this issue is their claim that men's junior-high school completion was already above 90% before the policy—which is actually around 80% as can be seen in Figure 2. It is actually below 80% when the 1986 birth cohort, which includes many treated individuals, is omitted.

An important feature of the data is imperfect compliance with the policy among the 1986 and 1987 birth cohorts—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 of the individuals in the 1986 birth cohort are affected by the policy, and some individuals in the 1987 birth cohort are not affected—contrary to the cutoff rule. This is actually quite visible in panel (A) of Figure 2. Many of the observations points for the 1986 birth cohort are above the fitted line, and many of those for the 1987 birth cohort are below.[27] Hence, in panel (B) of Figure 2, we show the same graph when the 1986 and 1987 birth cohorts (the donut-hole) are omitted. As expected, in this case, the jumps are much larger for both men and women. In their analysis of schooling outcomes with the 2014 HLFS data, EK fit high-order polynomials (unlike in their other graphs) in a relatively narrow bandwidth where the potential outcome displays a high-level of curvature around the cutoff due to the imperfect compliance. Consequently, their high-order polynomials capture the policy effect.

[27] Figure 1 in Aydemir et al. (2019) also shows this pattern very clearly.

We also use the 2008 TNSDVW dataset to examine the policy effect on schooling outcomes. Figure 3 replicates Figure 4A in EK. The running variable is year-of-birth in these figures as month-of-birth information is not available for men.[28] Figure 3 indicates a clear jump at the cutoff for men, as well as women. Even when a donut-hole is not used, the 95% confidence intervals do not overlap in the graph for men.

Next, we discuss our estimation results. Table 9 shows the results of parametric RDD with the 2014 THLFS that takes various bandwidths from 2 to 10 years.[29] While panel (1) in Table 9 uses the full data, panel (2) takes the donut-hole sample excluding the 1986 and 1987 birth cohorts. As can be seen from panel (1), evidence of a policy effect on junior high school completion exists for both men and women regardless of the bandwidth and the use of the donut hole. With the 60-month bandwidth that EK use in their graph and *without* a donut-hole, the policy increases junior high completion by 8.7 ppt for men—this is the effect that EK claim not to exist—and by 15 ppt for women.

It is also interesting to observe how the results change as we narrow the bandwidth gradually in panel (1) of Table 9. First, the policy effect gradually diminishes in magnitude, although it remains statistically significant. At the same time, the linear time trends grow in magnitude. For instance, the pre-policy trend coefficient increases from 0.02 for 10-year bandwidth to 0.06 for 2-year bandwidths. These facts illustrate the effect of imperfect compliance among the 1986 and 1987 birth cohorts. As the bandwidth gets narrow, the slopes of the time trends increase and the estimated policy effect diminishes as the relative importance of the 1986 and 1987 cohorts in the data increases. In line with these observations, panel (2) of Table 9 illustrates that the estimated policy effects are much larger for both men and women when a donut-hole is taken. With the 60-month bandwidth, the policy increases the junior high school completion of men by 13.7 ppt and of women by 19.8 ppt.

In the above approach, we gradually narrow the bandwidth while holding the order of the polynomials for the time trends constant. Next, we do just the opposite. We start with wide bandwidths by taking 10-year intervals on each side and assess the robustness of our findings by gradually increasing the order of polynomials. While using high order polynomials might be dangerous with narrow bandwidths, they might be needed with a more global approach

[28] We restrict the sample to individuals aged 16 and above because almost all individuals would finish junior high school by this age. Hence, there are only six data points on each side of the cutoff.

[29] This time, we can take wider ranges as we are not limited by 7 years on the right-hand side of the cutoff. The data come from 2014 instead of 2008, when the oldest affected birth cohort is 27 years old.

(Cattaneo et al., 2017). As can be seen from the results in Table A4 in Appendix A, the evidence of a policy effect persists up to 5th degree polynomials for both men and women.

Next, we use the local polynomial approach of CCFT. The results are given in Table A5 in Appendix A. The estimated effect is about 3-4 ppt for men and about 8 ppt for women. These magnitudes are consistent with those in Table 9 that use similarly narrow bandwidths.

In Table 10, we present the estimation results on the policy effect on junior high school completion using the 2008 TNSDVW dataset. We take six different bandwidths; we start with five years on each side and gradually widen it by one year each time.[30] Since the running variable is year of birth, we encounter the few clusters problem. Therefore, we also calculate Wild-cluster bootstrap p-values (Cameron et al., 2008; Cameron and Miller, 2015). The results indicate a clear policy effect on junior high school completion for both men and women for all bandwidths but the narrowest one. The results of the same analysis with a donut-hole, given in Table A6 of Appendix A, provide evidence of a policy effect for all bandwidths.

Essentially, both datasets used in the EK study, the 2008 TNSDVW and the 2014 THLFS, indicate substantial policy effects on men's junior high school completion. The claim of EK that the policy increases women's schooling but not men's is especially striking given the existing literature on this issue at the time of the publication of this paper. Several earlier papers (as well as recent ones) show strong evidence that the policy increases men's schooling (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); however, EK fail to cite any of these papers, but one (on another issue).[31] Kirdar et al. (2016) detail the substantial investment in schooling infrastructure that was made with the reform, which included the bussing of half a million students to nearby schools and the construction of about 600 boarding schools in remote rural areas in addition to the extension of classroom capacity, hiring of new teachers, and so forth. It is highly difficult to comprehend why the policy would affect female children but not males.[32]

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[30] The bandwidth for the right-hand side is capped at six years because there are at most six years on this side of the cutoff.

[31] However, most of these authors are listed in EK's acknowledgements in the paper.

[32] The policy effect on schooling outcomes could theoretically be larger for either gender. Alderman and Gertler (1997) show that—under the same assumptions on market incentives and parental preferences that yield higher educational attainment for boys than girls—the price elasticity of schooling demand is higher for girls. These assumptions certainly hold in the Turkish setting as well. Orazem and King (2007), in their review article, report that empirical studies in the context of South Asia and Middle East—where girls have lower educational attainment—generally find a higher price elasticity of schooling demand for girls. Hence, we expect the response

The other adverse consequence of EK's misleading information is that other papers, some of which are their own, also refer to this misleading information (Erten and Keskin, 2020; Gulesci et al., 2020) to rule out the exclusion restriction problem in establishing causal relationships between women's education and certain outcomes.[33] Therefore, we examine the policy impact on men's and women's schooling also using other data sources to settle this issue once and for all.

First, we pool all THLFS data between 2004 and 2015, resulting in hundreds in thousands of observations, which allows us to zoom in around the cutoff more. Since the number of clusters (year-of-birth groups) is relatively small, we use the Wild-cluster bootstrap. The results are given in Table 11, with and without the donut-hole and for various bandwidths that gradually zoom in around the cutoff, for grade 8 (junior high school) completion, high school completion, and years of schooling. The results provide very strong evidence that the policy has a strong impact on all three schooling outcomes for both men and women. Moreover, this evidence holds regardless of the use of a donut-hole.[34] Furthermore, the results are robust to the correction for the small number of clusters. Quantitatively, the policy effect on completed years of schooling is quite similar for men and women. While the effect on women is about 0.7 to 0.9 years, the estimated effect for men is only about 0.05 years lower—except with the estimates using the narrowest bandwidth where the difference is about 0.15 years. While the policy increases the grade 8 completion rate of women by about 20 ppt, the increase for men is about 15ppt. On the other hand, the policy effect on the high school completion rate is stronger for men, which is in accordance with the findings of Kırdar et al. (2016). The second dataset we use for this purpose is the Turkish Demographic and Health Surveys (TDHS). The results based on the 2008 and

to the fall in schooling costs resulting from the compulsory schooling policy to be larger for girls than for boys. On the other hand, several factors contribute to a lower demand for the schooling of girls in Turkey. First, due to the distinctly lower labor-market participation rates for women in Turkey (25% for women vs. 70% for men in 2008 [TurkStat, 2012]), the higher earnings capacity resulting from schooling would be less important for girls. Moreover, the value of future earnings would be discounted more for girls as daughters are more likely to move away from their parents after marriage. It is not obvious whether the opportunity cost of schooling would be higher for boys or girls because while boys are more likely to work in the market, the value of girls' home production would be higher. On the other hand, the cost of traveling away from home to go to school as well as the cost of attending schools would be much higher for girls than for boys.

[33] Obviously, nullifying the exclusion restriction problem is critical in these papers in showing the effect of "maternal education" on the outcomes of interest. On the other hand, in their study on teenage marriage and births in Turkey using the same instrument, Kirdar et al. (2018) show the policy effect on both men's and women's schooling, discuss the failure of the exclusion restriction assumption and limit their study to the policy effect only.

[34] When a donut-hole is not used, the coefficient magnitudes are lower, as expected. Also as expected, the use of a donut-hole makes a bigger difference when the bandwidth is narrow.

2013 waves of TDHS, given in Table A7 of Appendix A, are very similar to our results based on the THLFS.

5.4 Problems in EK's Definition of the Employment Variable

In this subsection, we demonstrate a series of problems with EK's definition of employment. First, EK use different definitions for the employment of men and women. Second, their definition for women's employment results in values that are way off compared to the official employment rates provided by TurkStat. Third, their estimates on the policy effect on women's employment are unbelievably large. These are detailed next.

In the employment variable, EK use different definitions for men and women. They define women as employed if they "worked last week", whereas they define men as employed if they "worked last week" or "usually have a job".[35] According to Table 1 of EK, 13% of women in their rural sample, 15% of women in their urban sample, and 14% of all women are employed. However, the THLFS shows that for EK's sample of 1982 to 1991 birth cohorts in 2008, 29.5% of women in rural areas, 21.6% of women in urban areas, and 23.8% of all women are employed. In other words, based on the total sample, they underestimate the true employment level by 41% (14% vs. 23.8%). On the other hand, when we use the extended definition of employment including both "worked last week" and "usually have a job" (which EK use only for men), we calculate that 21.7% of women are employed in the TNSDVW dataset—which is much more in line with the 23.8%, provided by the THLFS.

Table 12 shows the results on the policy effect on employment using these two alternative definitions. Panel (I) presents the estimates for EK's rural areas. We do not know the reasons for EK's choice of inconsistent definitions of employment across men and women. However, the results in panel (I) show that their choice happens to help their argument on the instrumental violence hypothesis: using only "worked last week" results in evidence of a policy effect on women's employment (panel B for women), whereas the coefficients become much closer to zero and the statistical evidence vanishes when the extended definition is used (panel A for women). On the other hand, the extended definition for men—which they choose—results in null policy effects on men's employment (panel A for men); however, if EK were to use the

[35] See the data cleaning file of EK, 1b-Woman-data-cleaning.do, line number 347 and 357 for the definitions of men's and women's employment, respectively.

definition they use for women, they would find much larger policy effects on men's employment (panel B for men)—raising concerns about the exclusion restriction assumption.

A highly surprising element of EK's findings is the sheer magnitude of the policy effect on women's employment. EK estimate the policy effect on employment as 5.6 ppt for the total sample and 8.2 ppt for the rural sample (Table 6 in their text). In other words, given the mean employment rates reported in Table 1 of their text, EK find that the policy increases women's employment by 37% for the total sample and by a tremendous 63% for their rural sample. Low and stagnant female employment (and labor force participation) in Turkey has been a fundamental labor market issue that researchers and policy-makers in Turkey have addressed (see, e.g., Tunali et al. (2021)). Therefore, it is certainly surprising that female employment gives such a tremendous response to the education reform, especially for women with rural origin.

To better understand the policy effect on employment effects, we turn to a dataset better suited for this purpose, the Turkish Household Labor Force Surveys. 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 and with and without a donut-hole (as in Section 5.3). Here, we also calculate Wild-cluster bootstrapped standard errors due to the small number of clusters resulting from year-of-birth being the running variable. As stated in Section 3, the range of our bandwidths here (3 to 10 years) lies within the range of the "optimal bandwidths" of EK (5 to 12 years).

Table 13 shows that no evidence of a policy effect on women's employment exists when a donut-hole is not taken in the sample. With a donut-hole, suggestive but not conclusive evidence emerges. However, the magnitude of the policy effect—less than 1ppt—is substantially smaller than what EK estimate. We also examine the policy effect on other employment outcomes. The policy effect on full-time employment and wage-employment of women is stronger. According to the estimates with a donut-hole, the policy increases full-time employment and wage employment by about 1.5 ppt. The policy effect reaches almost 2 ppt when we take full-time wage employment as the outcome. In essence, the policy seems to have a small effect on employment. However, it also changes the type of employment. It seems that women move into better jobs as a result of the policy.

The small policy effects on women's employment with the THLFS dataset are more similar to the results with the TNSDVW sample when employment is defined using both "worked last week" and "usually has a job". The Turkish Household Labor Force Survey defines individuals

as employed if they worked in the reference week or have a job that they could return even if they were temporarily absent in the reference week. Therefore, EK's definition of employment for men is more consistent with the definition in the standard labor force surveys.

5.5 Evidence for Urban Areas that Contradicts the Instrumental Violence Hypothesis

EK provide their results for the total sample and for their rural sample in their text—but not for their urban sample. In this subsection, we examine the results for the key variables using both the EK rural and urban samples. Figure 4 shows a surprising pattern: the RDD graphs for urban areas provide suggestive evidence that contradicts the instrumental violence hypothesis. Figure 4 suggests that the policy increases employment in urban areas, as well as in rural areas. However, while a jump is observed for rural areas in psychological violence and in financial control behavior at the cutoff, a drop is in fact observed for both of these variables at the cutoff for urban areas.

The corresponding estimates are provided in Table 14 for EK's rural areas in panel (A) and urban areas in panel (B). The results in panel (A) are in fact consistent with EK's findings—ignoring the flawed definition of EK's rural areas and the problems demonstrated in the previous subsections. However, as can be seen in panel (B), the policy effect on women's employment for urban areas is almost as large as that for rural areas. On the other hand, the policy effects on both psychological violence and financial control behavior have negative signs. Although they are not statistically significant at the conventional levels, their absolute magnitudes are quite large. Given these findings, it is certainly difficult to understand why an increase in women's employment leads to such different results in psychological violence and financial control behavior between rural and urban areas.

5.6 Mistake in the Construction of Schooling Variable

EK make a mistake in cleaning the data on years of schooling of both women and their partners. In the survey, a question first elicits whether the respondent (or her partner) has ever been to school. Then, for those who have ever been to schooling, more detailed questions on educational attainment are asked. For those who have never been to school, EK assign missing status to the years of schooling variable, rather than zero. In this subsection, we examine the potential effects of this mistake. Table A8 in the Appendix presents the results for the policy effect on years of schooling of women and their partners as defined in EK and with our correction. The mistake of EK results in only slightly smaller coefficient estimates for the policy effect on years of

schooling of women and their partners in the rural sample. Hence, luckily, this is not likely to pose a substantial problem in the 2SLS estimates of EK.

5.7 Misinterpretation of the Estimates

EK construct z-scores for domestic violence variables. In particular, they generate a z-score for each component of a dimension of domestic violence and average the z-scores across the components of each dimension. However, in their results, they interpret the coefficient estimates of the policy effect as percentage-point effects. For instance, in the introduction (page 5), they claim "…, the reform had negative effects, increasing the psychological violence experienced by women by 12 ppt…, increasing financial control behavior by 24 ppt." However, these must be interpreted as standard deviations as they refer to a z-score.

6 Our Analysis

Since the EK analysis suffers from several serious problems and the above analysis essentially examines the effect of fixing only one problem at a time, in this section, we redo the analysis. In our analysis, we use both the 2008 and 2014 cycles of TNSDVW to increase the potential precision of our estimates. This is particularly important in this context, given the modest size of each round of the TNSDVW and the low frequency of some of the events we analyze. Here, we estimate only the policy effect—but not the effect of women's schooling—because of the failure of the exclusion restriction assumption, as shown in the previous section. In our analysis by rural/urban status, we define rural areas as villages—which is the appropriate definition as shown in Section 4. We define employment as "worked last week" or "usually has a job" because, as shown in Section 5.4, this is more consistent with the employment definition in the THLFS.

6.1 Preliminary Checks

First, we assess the validity of the key identification assumption of RDD and the assumption that the policy does not bring about compositional changes in our sample due to its potential effect on the status of ever having a relationship.

6.1.1 Checks of the Identification Assumption

Here, we check the fundamental identifying assumption in 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: (i) continuity of the score density around the cutoff, (ii) absence of treatment effects on pre-treatment covariates, (iii) absence of treatment effects at artificial cutoff values.

Continuity of the score density around the cutoff requires that households not manipulate the running variable in order to be on one particular side of the cutoff. In that 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) comes before the policy, we examine this possibility formally using the McCrary test. The results, given in Figure B1 in Appendix B for the full sample and in Figure B2 by rural and urban status, indicate no evidence of such manipulation.

Next, we check the policy effect on 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 Table B1 in Appendix B show that evidence of a jump at the cutoff emerges for none of the variables with the full sample and the rural sample. For the urban sample, evidence of a jump that is statistically significant at the 10% level is observed only for one of the 15 variables, which is expected.

Finally, we check the absence of treatment effects at artificial cutoffs. We use the same methodology used in Section 5.2 (which indicated the failure of this assumption for financial control behavior in the EK sample). The results for several artificial cutoffs on either side of the actual cutoff are provided in Table B2 in Appendix B for physical violence and sexual violence and in Table B3 in Appendix B for financial control behavior and psychological violence. Of the 75 estimates for each dependent variable, only 3 for physical violence, 12 for sexual violence, 9 for financial control behavior, 3 for psychological violence are statistically significant at the 10% level. When we examine the rejections at the 5% level, we observe that of the 75 estimates, 2 for physical violence, 4 for sexual violence, 3 for financial control behavior, and none for psychological violence is statistically significant. Therefore, we can conclude that we might have some concerns about the continuity assumption only for the sexual violence variable.

6.1.2 Policy Effect on Ever Having a Relationship, Reporting Missing Month of Birth, and Response Quality

In Section 5.1, we showed that the policy changed the composition of women and response quality in the EK rural and total samples. Here, we conduct the same analysis for our sample. Table B4 in Appendix B shows the estimates for the policy effect on the status of ever having a

relationship, reporting missing month of birth information, and response quality for the full sample, as well as for the rural and urban samples. We find no evidence of a policy effect on ever having a relationship for any sample. Moreover, this is not simply a result of high standard errors; the coefficients are much closer to zero than those estimated for the EK samples (Table 7). For response quality, we observe no evidence of a policy effect for any sample, either.[36] Only for the incidence of reporting missing month of birth information, we observe evidence of a policy effect for the total sample and urban sample. The magnitude of this effect for the total sample, however, is about only half as large as the effect we estimate for the EK total sample in Table 7. Therefore, we acknowledge that the interpretation of our results for the total and urban samples might be influenced by a small policy effect on the composition of the sample.

6.2 Main Results

In this section, we first show the effect of the education reform on schooling outcomes, then move on to examine the reform effect on intimate partner violence (IPV) outcomes, as well as on partner characteristics (which might provide some clues about our results on IPV outcomes).

6.2.1 Policy Effect on Schooling

Table 13 shows the policy effect on years of schooling and middle school completion status for the total, rural, and urban samples. As can been in panel (A), the policy increases the years of schooling by about one year. The 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. The policy effect in urban areas is about 0.7 to 0.9 years. The patterns about 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 to 20 ppt with narrow bandwidths. The fact that the policy effect is weaker with narrow bandwidths is consistent with our findings in Section 5.3 in the way that imperfect compliance in the treatment status of birth cohorts immediately surrounding the cutoff reduces the estimated policy effect.

6.2.2 Policy Effect on Intimate Partner Violence and Employment Outcomes

Table 16 displays the policy effect on physical violence and sexual violence, and Table 17 shows the policy effect on psychological violence, financial control behavior, and employment. Panel

[36] Only for two bandwidths for the total sample does statistical evidence emerge.

(I) of Table 16 shows that the policy effect on physical violence has a negative sign and is large in magnitude. However, its statistical significance is weak; only 3 out of 8 bandwidths provide statistical evidence, and that is at the 10% level. When we restrict the sample to rural areas, however, 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. We observe no evidence for urban areas regarding the policy effect on physical violence.

As can be seen in panel (II) of Table 16, no statistical evidence of a policy effect on sexual violence exists 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 and more precisely estimated. Nonetheless, they are still not statistically significant at the conventional levels, except for the one with the widest bandwidth.

Panel (I) of Table 17 shows the policy effect on psychological violence. We would like to remind the reader that this is one of the variables for which EK report a positive policy effect. However, the coefficients we estimate for the total sample are about zero except for those estimated with the two widest bandwidths. Essentially, we estimate null impacts of the policy on psychological violence for the total sample. No evidence of a policy effect exists for the samples by rural and urban status, either.

Panel (II) of Table 17 presents the policy effect on financial control behavior. As shown in panel (A), no evidence of a policy impact exists for the total sample. In panel (B), the policy effect of the rural sample becomes somewhat stronger. However, it is statistically significant only for the two narrowest bandwidths. Hence, no conclusive evidence is observed for a policy effect on financial control behavior for the rural sample, either.

Finally, panel (III) of Table 17 shows the policy effect on women's employment. Here, we define employment as having worked in the last week or usually having a job—which is the definition that EK use for their male sample. As discussed earlier, this is also more consistent with the definition used in labor force surveys. As can be seen in panel (III), we observe no evidence of a policy effect on women's employment. Moreover, while the coefficients with wider bandwidths are positive, those with narrower bandwidths (5 years or less) are either zero or negative. The coefficients for the rural sample are negative for all bandwidths but one. The coefficients for the urban sample are mostly positive (and even statistically significant for some) for wider bandwidths but zero or negative for narrower bandwidths. The estimated policy effect on women's employment seems to be more positive in urban areas than in rural areas—contrary

to the findings of EK. In essence, the finding here that no evidence of a policy effect on women's employment exists is consistent with our findings in Section 5.4 based on the Turkish Household Labor Force Surveys (unless a donut-hole is taken).

In sum, our findings provide no evidence for the instrumental violence hypothesis—unlike the findings of EK. We estimate null policy effects on psychological violence, no evidence of a policy effect on women's employment (with either small or zero effects), and no evidence of a policy effect on financial control behavior. Moreover, when we dig deeper by actual rural and urban status, this does not change. The only statistically robust evidence is the negative policy effect on physical violence in rural areas.

Our findings are, in fact, more consistent with the results documented in a different paper by EK (Erten and Keskin, 2021), which uses the 2014 round of the same dataset. In that paper, EK find that the reform increased women's legal awareness of new laws and services designed to reduce gender inequalities and prevent domestic violence. In that paper, they also investigate the effects of the reform on violence outcomes. Although they do not find a significant impact of the reform on domestic violence outcomes, the impacts they find are negative and sizeable.

6.2.3 Policy Effect on Partner's Characteristics

Given our finding that the policy also increases men's schooling, the policy potentially changes also marital sorting patterns. This is important because a change in marital sorting patterns would also contribute to the observed IPV outcomes, such as psychological violence. Therefore, in this subsection, we examine the effect of the reform on partner's characteristics.

Table 18 presents the results. Panel (I) gives the results on the partner's years of schooling. For the total sample, we observe a positive effect of the policy on partner's schooling; however, this is statistically significant only for the wide and very narrow bandwidths. The impact for the rural sample is even larger in magnitude; the policy increases the schooling of men in rural areas by about 0.5 years. However, the coefficients are not statistically significant at the conventional levels. Given the large magnitude, consistency across different bandwidths, and marginal statistical insignificance of the estimates, we can point to suggestive but not conclusive evidence of a positive policy impact on partner's schooling.

Panel (II) of Table 18 presents the policy effect on the age gap between partners. The estimated coefficients are negative and large for the rural sample (the policy effect on schooling is stronger for rural areas); however, they are not statistically significant. We observe no consistent patterns

for the total and urban samples. Panel (III) shows that no evidence exists for a policy effect on the partner's employment. This finding is consistent with that of Aydemir and Kirdar (2017), who find a null effect of the policy on men's employment status using the THLFS.

In essence, we find large but statistically insignificant effects of the policy on the partner's schooling for the rural and total samples and on the age gap for the rural sample. Despite the large coefficients, the precision of our estimates is low due to the sample size. Therefore, we cannot rule out that the observed policy effects on IPV behavior partly result from the policy effect on the partner's characteristics. Certainly, the increase in partner's education and the reduction in the age gap could 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 to get forced into marriage against their consent. Moreover, women who are exposed to the reform are more likely to have met their husbands outside of family networks. Aydemir et al. (2019) find that the policy changed women's migration propensity for marriage purposes. These results imply that the reform changed marriage sorting patterns, which can affect IPV outcomes.

6.2.4 Estimates with a Local Polynomial Approach

In this subsection, we present our RDD results with a local polynomial approach. We provide this only as a robustness check mainly because of absent or weak first-stages (policy impact on schooling) with the CCFT bandwidths. Table B5 in the Appendix shows that in the estimation of the policy effect on years of schooling and junior high school completion for the total and urban samples, the CCFT method chooses bandwidths of about 30 months and provides no evidence of a policy effect on either variable with the robust estimates (which is the improvement this method makes over the IK method). Similarly, for the rural areas, we observe no evidence of a policy effect on years of schooling—although evidence of a policy effect on middle school completion exists due to the wider optimal bandwidth for this outcome (about 44 months). Nevertheless, the results with the IK optimal bandwidths (which are much wider than the CCFT optimal bandwidths) provide evidence of a positive policy effect on both schooling outcomes for all samples (Table B5, Appendix B).

We think that the lack of a policy effect on schooling outcomes with our data and the CCFT approach is the following. Due to the imperfect compliance of the 1986 and 1987 birth cohorts, our setting is, in fact, fuzzy RDD. However, in estimating the policy effect, we use a reduced form (a sharp RDD) where there is much curvature immediately around the cutoff. In order to

minimize the bias that might result from this curvature with linear polynomials, the CCFT method chooses very narrow bandwidths.

When we examine IPV outcomes, in Table B5, we observe that neither method provides evidence of a policy effect increasing psychological violence for the full sample (contrary to the claims of EK). This is not due to precision; the effect is either null or negative. This result is also consistent with our findings using parametric methods. The estimates for financial control behavior are also similar to our findings with parametric methods, both in terms of magnitude and statistical significance. Although the estimated coefficients are positive, they are not statistically significant. Finally, neither of the optimal bandwidth approaches provides evidence of a policy effect on women's employment. While the CCFT method results in a negative coefficient, the IK method results in a positive coefficient; both are, however, small and statistically significant. In essence, these results confirm that the policy has no effects on psychological violence and women's employment, and the estimated impact on financial control behavior is positive but quite imprecisely estimated. Hence, no evidence for the instrumental violence hypothesis exists.

Both local polynomial approaches indicate negative and marginally statistically insignificant effects on physical violence, which is consistent with our parametric findings. The CCFT method also provides evidence that the policy reduces sexual violence (unlike the parametric methods and IK method); however, the optimal bandwidth chosen is only 33 months.

When we examine the results for rural areas, we observe that they are overall similar to those with a parametric approach. No evidence of a policy effect on psychological violence or women's employment exists. However, statistical evidence of a positive policy effect on financial control behavior emerges, which is similar to the findings with narrow bandwidths with the parametric approach. In addition, strong evidence for the policy reducing physical violence exists, as in the findings with the parametric approach. For urban areas, no evidence of the policy affecting any measure of IPV exists, except for the policy reducing sexual violence with the CCFT bandwidths of 32 months. However, this finding is not confirmed by IK bandwidths or parametric methods.

7 Conclusion

In their paper published in AEJ: Applied, Erten and Keskin (EK) claim to establish evidence for the instrumental violence hypothesis—based on their findings that women's schooling increases

psychological violence and financial control behavior while also raising women's employment. Their evidence comes from women who have ever had a relationship and who lived in what they call "rural areas" during their childhood. In this paper, we show that the results in EK are fragile and possibly incorrect due to a number of reasons.

EK misclassify the variable on rural and urban status of childhood place of residence. We show that once this variable is defined properly, the statistical evidence for their findings—which exists only for rural areas—vanishes. Even if we ignore the misclassification of the rural status variable, we demonstrate that a number of serious flaws exists in their empirical analysis: (i) selection bias resulting from the policy altering the composition of women ever having a relationship in rural areas, (ii) failure of the main identification assumption of RDD for some key outcomes, (iii) failure of the exclusion restriction assumption, (iv) inconsistency in the definition of employment variable across men and women, (v) elementary mistakes in data cleaning, RDD estimation, and interpretation of the estimates. In addition, the evidence for urban areas contradicts the hypothesis they claim to hold for rural areas.

We conduct a similar analysis—examining the policy effect rather than the effect of women's schooling due to the failure of the exclusion restriction assumption—using the 2008 and 2014 rounds of the TNSDVW. Contrary to EK's findings, we find no evidence for the instrumental violence hypothesis. We estimate null policy effects on psychological violence, no evidence of a policy effect on women's employment (with either small or zero effects), and no evidence of a policy effect on financial control behavior. When we conduct the analysis by actual rural and urban status, we find no evidence for the instrumental violence hypothesis, either. However, we do find robust evidence for the policy reducing physical violence in rural areas.

Our study makes a number of important points. First, it highlights the importance of having adequate knowledge of the context in empirical studies. The definition the authors make for rural areas is meaningless and inconsistent in construction. They define non-metropolitan district centers as rural areas—although more than 80% of them are urban. They define a considerable fraction of women living in the metropolitan district centers—some of which are the most developed parts of the country—as living in rural areas. A separate but related issue is concerning their estimates on the policy effect on women's employment. The authors are willing to accept a 13% employment rate in childhood rural areas (which are mostly agricultural). They calculate that the policy increases women's employment by an astounding 63% in rural areas. It is hard to grasp how female employment—which has been low and stagnant for decades in Turkey—would give such a drastic response.

Second, our study highlights the importance of knowing and acknowledging the earlier work on the topic. The authors fail to recognize the substantial policy effect on men's education—which causes the exclusion restriction to fail. In fact, they claim to find no policy effect on men's schooling, although several previous papers on the effects of the same compulsory schooling policy (not cited in the paper) demonstrated the substantial policy effect on men.

Third, the authors are not careful about checking the identification assumptions. The authors miss the substantial policy effect on the status of having ever a relationship in rural areas, which potentially causes a serious sample selection bias in their 2SLS estimates. In addition, some of the standard checks of the continuity assumption of RDD fail for their critical variables.

Fourth, the authors make several unorthodox choices in their empirical analysis. When the authors try to argue that the policy had no impact on men's schooling—a claim that flies in the face of a large body of existing research—they present no estimation results and instead display misleading graphs (with high-order polynomials that are not used anywhere else in the paper) in support of their claim. In addition, the authors use different definitions of employment for men and women. In fact, if they were to use their definition for men also for women, they would find no evidence for a policy effect on women's employment; and, if they were to use their definition for women also for men, they would find a large policy effect on men's employment—resulting in the potential failure of the exclusion restriction problem. Moreover, when the authors present their results for subgroups, they are not comprehensive. They present it for rural areas and for the total sample—but not for urban areas. However, we find that the evidence for urban areas—not provided in their paper—is inconsistent with the instrumental violence hypothesis.

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Tables and Figures

Tables 1 to 4 (which are small tables) are included in the text.

Table 5: A Comparison of Rural Areas with Villages and with Villages and District Centers

	Mean Values			Number of Observations		
	Rural	Village	Village or District Center	Rural	Village	Village or District Center
Years of Schooling	6.983	6.967	8.040	458	461	1,520
Completed Primary School	0.865	0.873	0.917	458	461	1,520
Completed Junior High School	0.509	0.508	0.590	458	461	1,520
Completed High School	0.213	0.199	0.353	458	461	1,520
Employed	0.209	0.196	0.150	458	461	1,520
Employed in Non-agriculture	0.081	0.089	0.114	458	461	1,520
Employed in Services	0.069	0.075	0.095	458	461	1,520
Employed in Agriculture	0.128	0.107	0.035	458	461	1,520
Social Security	0.033	0.037	0.067	458	461	1,519
Personal Income Index	-0.067	-0.065	-0.072	458	461	1,520
Marriage Age	19.834	19.914	20.156	312	320	1,077
Marriage Decision by Herself	0.557	0.515	0.567	314	322	1,080
Partner is Employed	0.828	0.863	0.842	458	461	1,520
Partner's Schooling	8.570	8.469	9.076	442	445	1,482
Schooling Difference between Partners	1.579	1.475	0.998	442	445	1,482
Age Difference between Partners	3.766	3.919	3.994	312	320	1,077
Husband's Age	23.579	23.813	24.145	314	322	1,080
Husband's Religiosity Index	-0.009	0.017	-0.004	452	454	1,508
Partner Witnessed Violence toward His Mother	0.281	0.323	0.308	314	316	1,128
Partner Experienced Violence from His Family	0.734	0.741	0.736	356	354	1,255
Ever Divorced	0.007	0.006	0.006	458	461	1,520
Had a Second Marriage	0.004	0.004	0.004	458	461	1,520
Asset Ownership Index	-0.050	-0.025	0.085	458	461	1,520

Notes: The data come from the 2008 TNSDVW. The above variables are the same as those in Table 1 (panels A-C) of EK. The sample is restricted to those who have ever had a relationship and to a 60-month bandwidth on each side of the cutoff -- as in EK. Schooling variables are corrected for the cleaning mistake in EK.

Table 6: Policy Effect on Key Variables of Interest with Alternative Definitions of Rural Areas during Childhood

	Number of Years on Each Side of the Cutoff							
	10	9	8	7	6	5	4	3
I) EK Sample: Improper and Inconsistent Rural Definition								
For Movers: Rural defined as ***District Centers*** *and Villages during Childhood*								
For Stayers: Rural defined using Survey Variable "Rural" at the time of Survey								
A) Psychological Violence	0.093*	0.106**	0.118**	0.129**	0.137**	0.152**	0.179**	0.136*
	[0.051]	[0.052]	[0.052]	[0.054]	[0.059]	[0.062]	[0.068]	[0.075]
Obs.	2,253	2,036	1,840	1,642	1,417	1,176	931	704
B) Financial Control Behavior	0.214*	0.241**	0.250**	0.252**	0.232*	0.192	0.264*	0.254
	[0.120]	[0.117]	[0.115]	[0.114]	[0.119]	[0.130]	[0.156]	[0.168]
Obs.	2,138	1,922	1,728	1,530	1,313	1,090	867	653
C) Employment	0.045	0.062**	0.072**	0.086***	0.074**	0.074**	0.037	0.036
	[0.031]	[0.031]	[0.031]	[0.031]	[0.032]	[0.036]	[0.035]	[0.040]
Obs.	2,256	2,039	1,843	1,645	1,420	1,179	934	707
II) Proper but Inconsistent Rural Definition								
For Movers: Rural defined as Villages during Childhood								
For Stayers: Rural defined using Survey Variable "Rural" at the time of Survey								
A) Psychological Violence	0.058	0.059	0.082	0.099	0.098	0.102	0.097	0.073
	[0.058]	[0.059]	[0.061]	[0.061]	[0.065]	[0.069]	[0.076]	[0.085]
Obs.	1,504	1,351	1,215	1,093	941	777	609	468
B) Financial Control Behavior	0.255*	0.251	0.250	0.259*	0.240	0.236	0.242	0.288
	[0.153]	[0.153]	[0.152]	[0.154]	[0.164]	[0.179]	[0.209]	[0.228]
Obs.	1,410	1,258	1,124	1,002	858	707	558	427
C) Employment	0.025	0.050	0.045	0.066	0.057	0.048	0.027	0.025
	[0.042]	[0.042]	[0.042]	[0.042]	[0.041]	[0.047]	[0.049]	[0.059]
Obs.	1,506	1,353	1,217	1,095	943	779	611	470
III) Proper and Consistent Rural Definition								
For Movers: Rural defined as Villages during Childhood								
For Stayers: Rural defined as Villages at the Time of Survey								
A) Psychological Violence	0.067	0.067	0.090	0.107*	0.105	0.106	0.093	0.113
	[0.059]	[0.060]	[0.061]	[0.061]	[0.066]	[0.069]	[0.080]	[0.095]
Obs.	1,478	1,327	1,194	1,078	927	765	599	456
B) Financial Control Behavior	0.246	0.236	0.238	0.245	0.243	0.250	0.277	0.345
	[0.155]	[0.155]	[0.155]	[0.158]	[0.170]	[0.186]	[0.218]	[0.235]
Obs.	1,391	1,241	1,110	994	851	702	555	421
C) Employment	0.017	0.038	0.035	0.054	0.040	0.029	0.003	0.001
	[0.042]	[0.043]	[0.042]	[0.043]	[0.045]	[0.051]	[0.057]	[0.067]
Obs.	1,480	1,329	1,196	1,080	929	767	601	458
IV) Sample of Villages and District Centers -- for both movers and stayers								
A) Psychological Violence	0.039	0.041	0.048	0.065	0.077	0.101*	0.136**	0.087
	[0.044]	[0.044]	[0.045]	[0.045]	[0.049]	[0.052]	[0.056]	[0.064]
Obs.	2,816	2,564	2,325	2,080	1,800	1,501	1,194	899
B) Financial Control Behavior	0.133	0.153	0.167*	0.165*	0.151	0.103	0.144	0.168
	[0.100]	[0.099]	[0.099]	[0.098]	[0.106]	[0.111]	[0.131]	[0.147]
Obs.	2,615	2,365	2,128	1,884	1,622	1,355	1,085	816
C) Employment	0.039	0.050	0.051	0.064**	0.064*	0.060	0.027	-0.012
	[0.031]	[0.031]	[0.032]	[0.032]	[0.034]	[0.038]	[0.038]	[0.045]
Obs.	2,823	2,571	2,332	2,086	1,806	1,507	1,200	904

Notes: The data come from the 2008 Turkish National Survey on Domestic Violence against Women. In the survey, if a woman has been residing in the same place that she lived during her childhood (stayer), she is asked about her current location only; whereas if a woman changed her location after age 12 (mover), she is asked about her location during childhood. Accordingly, the samples in four separate panels are defined as given in panel headings. The sample is restricted to women who have ever had a relationship as in EK. 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, the regressions in both panels include split linear time trends on either side of the cutoff where the running variable is month-year of birth. As in EK, the regressions also controls for birth-month dummies, a dummy for whether the interview language was Turkish, a dummy for whether the respondent currently lives in a rural area (mistakenly called village in EK), and dummies for 26 NUTS-2 region of residence during childhood. The regressions are weighed using the sample weights, as in EK. Standard errors are clustered at the month-year of birth level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 7: Policy Effect on Ever Having a Relationship, Missing Month-Year of Birth, and, Response Quality

	Number of Years on Each Side of the Cutoff							
	10	*9*	*8*	*7*	*6*	*5*	*4*	*3*
I) Ever Having a Relationship (Running Variable: Month-Year of Birth)								
A) Rural Sample								
Policy Effect	0.065**	0.073**	0.077**	0.089**	0.084**	0.071*	0.114**	0.068
	[0.032]	[0.033]	[0.034]	[0.036]	[0.037]	[0.041]	[0.048]	[0.058]
Observations	2,621	2,399	2,197	1,990	1,711	1,405	1,100	817
B) Urban Sample								
Policy Effect	-0.014	-0.012	-0.013	-0.008	-0.050	0.013	-0.014	0.068
	[0.044]	[0.045]	[0.045]	[0.046]	[0.047]	[0.050]	[0.054]	[0.062]
Observations	2,287	2,157	2,007	1,843	1,583	1,303	1,016	770
C) Total Sample								
Policy Effect	0.021	0.026	0.028	0.039	0.018	0.038	0.048	0.064
	[0.029]	[0.030]	[0.030]	[0.031]	[0.031]	[0.034]	[0.036]	[0.043]
Observations	4,908	4,556	4,204	3,833	3,294	2,708	2,116	1,587
II) Birth Month is Missing (Running Variable: Year of Birth)								
A) Rural Sample								
Policy	-0.047*	-0.042	-0.060**	-0.069***	-0.071**	-0.068**	-0.084**	-0.091**
	[0.025]	[0.027]	[0.023]	[0.023]	[0.023]	[0.029]	[0.029]	[0.034]
Wild Bootstrap p-value	0.264	0.315	0.124	0.064	0.087	0.234	0.227	0.344
Observations	2,549	2,307	2,059	1,837	1,582	1,314	1,036	779
B) Urban Sample								
Policy	-0.005	-0.004	-0.005	0.004	0.008	0.018*	0.001	0.001
	[0.008]	[0.008]	[0.008]	[0.006]	[0.007]	[0.009]	[0.004]	[0.006]
Wild Bootstrap p-value	0.559	0.628	0.620	0.536	0.261	0.107	0.734	0.938
Observations	1,850	1,714	1,559	1,398	1,219	1,024	818	615
C) Total Sample								
Policy	-0.031**	-0.028*	-0.039***	-0.041***	-0.040***	-0.032*	-0.048***	-0.049**
	[0.013]	[0.015]	[0.011]	[0.012]	[0.012]	[0.015]	[0.013]	[0.014]
Wild Bootstrap p-value	0.145	0.223	0.04	0.043	0.092	0.311	0.141	0.344
Observations	4,399	4,021	3,618	3,235	2,801	2,338	1,854	1,394
III) Response Quality Good or Very Good (Running Variable: Month-Year of Birth)								
A) Rural Sample								
Policy Effect	0.084**	0.087**	0.093**	0.092**	0.096**	0.137***	0.167***	0.215***
	[0.034]	[0.036]	[0.037]	[0.040]	[0.046]	[0.051]	[0.055]	[0.070]
Observations	2,243	2,027	1,832	1,634	1,412	1,173	929	705
B) Urban Sample								
Policy Effect	-0.001	-0.004	-0.024	-0.000	-0.013	0.007	0.075*	0.042
	[0.029]	[0.030]	[0.032]	[0.031]	[0.034]	[0.038]	[0.043]	[0.044]
Observations	1,762	1,635	1,489	1,334	1,163	975	781	590
C) Total Sample								
Policy Effect	0.045*	0.044*	0.040	0.053**	0.051*	0.078**	0.121***	0.120**
	[0.023]	[0.024]	[0.025]	[0.026]	[0.030]	[0.034]	[0.037]	[0.045]
Observations	4,005	3,662	3,321	2,968	2,575	2,148	1,710	1,295

Notes: The data come from the 2008 Turkish National Survey on Domestic Violence against Women. 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 Panel I) and III) and year of birth in Panel II), the regressions also control for birth-month dummies in Panel I) and III), a dummy for whether the childhood region was a rural area, a dummy for whether the interview language was Turkish, 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 Panel I) and III) and clustered at the year of birth level in Panel II). Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 8: Checking the Continuity Assumption of RDD via Alternative Cutoffs – EK Rural Areas

	A) Birth Year <= 1986							B) Birth Year >=1987		
	Maximum Number of Years on Each Side of the Cutoff							Max. Number of Years		
Cutoff	7	6	5	4	3	2	Cutoff	4	3	2
I) Financial Control Behavior										
Jan-85	0.044	0.081	0.097	0.068	0.035	0.176	Jan-89	0.160	0.142	0.068
	[0.080]	[0.086]	[0.091]	[0.092]	[0.094]	[0.112]		[0.172]	[0.181]	[0.198]
Jan-84	-0.049	-0.013	0.010	0.026	-0.067	-0.241**	Jul-89	0.082	0.088	-0.353***
	[0.077]	[0.079]	[0.082]	[0.089]	[0.094]	[0.111]		[0.134]	[0.144]	[0.130]
Jan-83	0.032	0.052	0.106	0.144*	0.202**	0.275**	Jan-90	0.093	0.103	0.014
	[0.073]	[0.073]	[0.073]	[0.073]	[0.078]	[0.111]		[0.145]	[0.148]	[0.172]
Jan-82	-0.125*	-0.123*	-0.129*	-0.122	-0.079	-0.112	Jul-90	0.275*	0.279*	0.447**
	[0.065]	[0.067]	[0.068]	[0.077]	[0.073]	[0.093]		[0.165]	[0.150]	[0.171]
Jan-81	-0.111*	-0.116*	-0.151**	-0.167**	-0.181**	-0.109	Jan-91	0.019	0.042	0.039
	[0.065]	[0.067]	[0.069]	[0.069]	[0.082]	[0.088]		[0.125]	[0.110]	[0.220]
Jan-80	0.042	0.017	0.029	-0.001	0.063	0.098	Jul-91	-0.167	-0.170	-0.580**
	[0.065]	[0.070]	[0.075]	[0.085]	[0.089]	[0.129]		[0.100]	[0.134]	[0.225]
Jan-79	0.109	0.118*	0.088	0.147	0.093	0.013	Jan-92	-0.092	-0.118	-0.139
	[0.068]	[0.070]	[0.078]	[0.090]	[0.111]	[0.144]		[0.095]	[0.113]	[0.338]
Jan-78	0.171**	0.142*	0.164**	0.112	0.071	0.094				
	[0.069]	[0.074]	[0.079]	[0.094]	[0.113]	[0.131]				
Jan-77	0.100	0.112	0.046	-0.027	-0.068	-0.103				
	[0.075]	[0.077]	[0.081]	[0.082]	[0.092]	[0.113]				
II) Employment										
Jan-85	-0.047	-0.035	-0.011	-0.001	0.044	0.027	Jan-89	-0.055	-0.110	-0.092
	[0.038]	[0.036]	[0.035]	[0.037]	[0.038]	[0.042]		[0.082]	[0.087]	[0.095]
Jan-84	-0.091***	-0.073**	-0.069**	-0.043	-0.038	0.022	Jul-89	0.024	0.019	-0.074
	[0.033]	[0.034]	[0.034]	[0.037]	[0.038]	[0.047]		[0.066]	[0.070]	[0.085]
Jan-83	-0.088***	-0.092***	-0.082**	-0.087**	-0.082*	-0.125**	Jan-90	0.094	0.096	0.073
	[0.032]	[0.033]	[0.035]	[0.037]	[0.047]	[0.055]		[0.068]	[0.073]	[0.087]
Jan-82	0.021	0.016	0.014	0.036	0.048	0.111*	Jul-90	0.145**	0.168**	0.252***
	[0.035]	[0.035]	[0.036]	[0.040]	[0.042]	[0.056]		[0.071]	[0.079]	[0.090]
Jan-81	0.019	0.005	-0.002	-0.013	-0.015	-0.092	Jan-91	0.086	0.092	0.087
	[0.035]	[0.037]	[0.039]	[0.042]	[0.050]	[0.057]		[0.082]	[0.096]	[0.097]
Jan-80	0.077**	0.073**	0.058	0.039	0.020	0.077	Jul-91	-0.150**	-0.204**	-0.326***
	[0.034]	[0.036]	[0.040]	[0.044]	[0.049]	[0.064]		[0.070]	[0.077]	[0.087]
Jan-79	0.05	0.059	0.037	-0.001	-0.004	-0.081	Jan-92	-0.181**	-0.260***	-0.274***
	[0.036]	[0.039]	[0.042]	[0.049]	[0.061]	[0.069]		[0.081]	[0.084]	[0.088]
Jan-78	0.053	0.043	0.049	0.063	0.036	0.080				
	[0.034]	[0.036]	[0.040]	[0.045]	[0.053]	[0.063]				
Jan-77	-0.020	-0.027	-0.008	-0.003	0.019	-0.009				
	[0.034]	[0.036]	[0.039]	[0.045]	[0.051]	[0.055]				

Notes: The data come from the 2008 Turkish National Survey on Domestic Violence against Women. The sample is restricted to the birth cohorts unaffected by the policy in panel (A) and to the birth cohorst affected by the policy in panel (B). In both panels, we take counterfactual policy cutoffs by gradually shifting the cutoff point, as specified in columns (1) and (8). The cutoffs are chosen so as to keep at least 2 years of data on each side of the cutoff. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. The bandwidths in panel (B) are much narrower because the data has only 7 years on the right hand side of the cutoff. 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 interview language was Turkish, and dummies for 26 NUTS-2 region of residence at age 12. The regressions are weighed using the sample weights, as in Erten and Keskin. Standard errors are clustered at the birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 9: Policy Effect on Middle School Completion by Gender – 2014 THLFS

1) Full Sample

A) Men

	Number of Years on Each Side of the Cutoff								
	10	9	8	7	6	5	4	3	2
Policy	0.133***	0.128***	0.118***	0.108***	0.090***	0.087***	0.068***	0.047***	0.034**
	[0.010]	[0.010]	[0.010]	[0.011]	[0.010]	[0.011]	[0.012]	[0.013]	[0.016]
Pre-policy trend	0.002***	0.002***	0.002***	0.002***	0.003***	0.003***	0.004***	0.005***	0.006***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]
Post-policy trend	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001**
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]
Observations	64,408	56,827	49,749	43,574	37,499	31,113	24,783	18,560	12,302

B) Women

	Number of Years on Each Side of the Cutoff								
	10	9	8	7	6	5	4	3	2
Policy	0.186***	0.185***	0.182***	0.177***	0.168***	0.150***	0.132***	0.113***	0.099***
	[0.010]	[0.011]	[0.012]	[0.012]	[0.013]	[0.014]	[0.015]	[0.014]	[0.015]
Pre-policy trend	0.002***	0.002***	0.002***	0.002***	0.002***	0.002***	0.003***	0.004***	0.006***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]
Post-policy trend	0.001***	0.001***	0.001***	0.001***	0.002***	0.002***	0.002***	0.002***	0.002**
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]
Observations	69,250	61,612	54,434	47,332	40,336	33,329	26,493	19,914	13,146

2) Donut-Hole Sample

A) Men

	Number of Years on Each Side of the Cutoff								
	10	9	8	7	6	5	4	3	2
Policy	0.169***	0.167***	0.160***	0.150***	0.129***	0.137***	0.115***	0.080**	0.096***
	[0.010]	[0.011]	[0.012]	[0.013]	[0.014]	[0.015]	[0.019]	[0.032]	[0.033]
Pre-policy trend	0.001***	0.001***	0.001***	0.002***	0.002***	0.002***	0.002***	0.004***	0.004*
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.002]
Post-policy trend	0.000***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001	--
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	--
Observations	58,226	50,645	43,567	37,392	31,317	24,931	18,601	12,378	6,120

B) Women

	Number of Years on Each Side of the Cutoff								
	10	9	8	7	6	5	4	3	2
Policy	0.217***	0.219***	0.222***	0.223***	0.217***	0.198***	0.177***	0.150***	0.140*
	[0.012]	[0.013]	[0.014]	[0.016]	[0.017]	[0.021]	[0.030]	[0.043]	[0.074]
Pre-policy trend	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.002***	0.003***	0.006
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]	[0.004]
Post-policy trend	0.001***	0.001***	0.001***	0.001***	0.001***	0.002***	0.002***	0.002**	--
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]	--
Observations	62,677	55,039	47,861	40,759	33,763	26,756	19,920	13,341	6,573

Notes: The data come from the 2014 Turkish Household Labor Force Survey. In panel (2), 1986 and 1987 birth cohorts (the donut-hole) are excluded from the sample. 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, the regressions also control for birth-month dummies. Standard errors are clustered at the birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 10: Policy Effect on Middle School Completion by Gender –2008 TNSDVW

	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidth on the left	10 years	9 years	8 years	7 years	6 years	5 years
Bandwidth on the right	6 years	6 years	6 years	6 years	6 years	5 years
A) MALE SAMPLE						
Policy	0.076**	0.071**	0.083**	0.070**	0.059**	0.051*
	[0.026]	[0.027]	[0.031]	[0.028]	[0.027]	[0.028]
Wild-cluster Bootstrap p-value	0.032	0.043	0.058	0.059	0.075	0.197
Pre-policy trend	0.019***	0.021***	0.017**	0.022***	0.027***	0.032***
	[0.005]	[0.005]	[0.006]	[0.005]	[0.006]	[0.009]
Post-policy trend	0.016***	0.016***	0.016***	0.016***	0.016***	0.016***
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.004]
Observations	10,232	9,832	9,153	8,676	8,008	6,672
R-squared	0.072	0.072	0.061	0.063	0.061	0.050
B) FEMALE SAMPLE						
Policy	0.105***	0.101***	0.111***	0.098**	0.088**	0.066*
	[0.032]	[0.032]	[0.036]	[0.033]	[0.032]	[0.035]
Wild-cluster Bootstrap p-value	0.015	0.019	0.018	0.022	0.041	0.293
Pre-policy trend	0.023***	0.024***	0.021***	0.026***	0.031***	0.039***
	[0.004]	[0.005]	[0.006]	[0.005]	[0.007]	[0.009]
Post-policy trend	0.031***	0.031***	0.031***	0.031***	0.031***	0.035***
	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]	[0.010]
Observations	10,378	9,907	9,294	8,767	8,119	6,752
R-squared	0.11	0.107	0.095	0.095	0.09	0.077

Notes: The data come from the 2008 TNSDVW. Since the sample is restricted to individuals aged 16 and older (most individuals complete middle school at age 14 or 15), the youngest birth cohort in the sample is born in 1992. This yields at most 6 points on the right hand side of the cutoff. The estimates in each column come from a separate regression using a sample defined according to the bandwidths specified in the column headings. Standard errors are clustered at the birth-year level. Since the number of cluster is small (ranging from 16 in column (1) to 10 in column (6)), we also provide wild-cluster bootstrap p-values. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 11: Policy Effects on Schooling Outcomes for Men and Women – THLFS (2004-2015)

	With Donut-Hole					Without Donut-Hole				
	Female		Male				Female		Male	
(1)	(2)	(3)	(4)	(5)		(6)	(7)	(8)	(9)	(10)
A) Policy Effect on Completing Grade 8										
1973-1985, 1988-2000	0.207***	991,501	0.156***	915,650		1973-1986, 1987-2000	0.173***	1,086,737	0.131***	999,024
	[0.011]		[0.006]				[0.022]		[0.015]	
Wild Bootstrap p-value	0.000		0.000			Wild Bootstrap p-value	0.000		0.000	
1976-1985, 1988-1997	0.205***	820,293	0.154***	757,132		1976-1986, 1987-1997	0.166***	915,529	0.124***	840,506
	[0.008]		[0.005]				[0.022]		[0.017]	
Wild Bootstrap p-value	0.000		0.000			Wild Bootstrap p-value	0.000		0.000	
1979-1985, 1988-1994	0.204***	617,489	0.158***	563,405		1979-1986, 1987-1994	0.155***	712,725	0.117***	646,779
	[0.008]		[0.007]				[0.025]		[0.021]	
Wild Bootstrap p-value	0.004		0.002			Wild Bootstrap p-value	0.000		0.000	
1982-1985, 1988-1991	0.210***	368,850	0.144***	332,569		1982-1986, 1987-1991	0.133***	464,086	0.089***	415,943
	[0.006]		[0.008]				[0.027]		[0.021]	
Wild Bootstrap p-value	0.010		0.020			Wild Bootstrap p-value	0.012		0.016	
B) Policy Effect on Completing High School										
1976-1985, 1988-1997	0.093***	685,709	0.103***	621,703		1976-1986, 1987-1997	0.065***	776,010	0.076***	700,293
	[0.018]		[0.020]				[0.021]		[0.020]	
Wild Bootstrap p-value	0.000		0.000			Wild Bootstrap p-value	0.000		0.000	
1978-1985, 1988-1995	0.089***	584,011	0.099***	526,268		1978-1986, 1987-1995	0.061***	674,312	0.072***	604,858
	[0.018]		[0.020]				[0.020]		[0.018]	
Wild Bootstrap p-value	0.000		0.000			Wild Bootstrap p-value	0.000		0.004	
1980-1985, 1988-1993	0.085***	460,695	0.093***	413,443		1980-1986, 1987-1993	0.054**	550,996	0.063***	492,033
	[0.019]		[0.024]				[0.018]		[0.019]	
Wild Bootstrap p-value	0.014		0.006			Wild Bootstrap p-value	0.012		0.008	
1982-1985, 1988-1991	0.074**	317,559	0.064*	282,411		1982-1986, 1987-1991	0.042**	407,860	0.043**	361,001
	[0.022]		[0.030]				[0.015]		[0.017]	
Wild Bootstrap p-value	0.074		0.266			Wild Bootstrap p-value	0.060		0.124	
C) Policy Effect on Years of Schooling										
1980-1985, 1988-1993	0.878***	343,332	0.838***	316,540		1980-1986, 1987-1993	0.504**	399,489	0.539***	368,724
	[0.172]		[0.141]				[0.184]		[0.121]	
Wild Bootstrap p-value	0.020		0.014			Wild Bootstrap p-value	0.010		0.008	
1981-1985, 1988-1992	0.820***	288,405	0.770***	266,124		1981-1986, 1987-1992	0.457**	344,562	0.477***	318,308
	[0.178]		[0.151]				[0.173]		[0.117]	
Wild Bootstrap p-value	0.028		0.018			Wild Bootstrap p-value	0.034		0.012	
1982-1985, 1988-1991	0.697***	229,786	0.616***	213,106		1982-1986, 1987-1991	0.377**	285,943	0.376***	265,290
	[0.104]		[0.135]				[0.124]		[0.093]	
Wild Bootstrap p-value	0.018		0.112			Wild Bootstrap p-value	0.052		0.026	
1983-1985, 1988-1990	0.721***	169,698	0.527***	157,373		1983-1986, 1987-1990	0.344**	225,855	0.312***	209,557
	[0.069]		[0.028]				[0.102]		[0.074]	
Wild Bootstrap p-value	0.064		0.000			Wild Bootstrap p-value	0.070		0.064	

Notes: The sample includes observations from 2004-2015 Turkish Household Labor Force Surveys. The sample is restricted to ages 15 and above in panel (A), to ages 18 and above in panel (B), and to ages 22 and above in panel (C) in order to prevent censoring in each schooling outcome. As a result, while the youngest birth cohort is the 2000 birth cohort in panel (A), it is the 1997 birth-cohort in panel (B) and the 1993 birth-cohort in panel (C). In each panel, we use alternative bandwidths gradually zooming in around the cutoff. The policy dummy is one when year of birth is greater 1987. Each cell comes from a separate regression of the specified schooling outcome on the policy dummy as well as the specified time trends. The number of observations is given in columns (3), (5), (8), and (10). Standard errors are clustered at the year-of-birth level. However, as the number of clusters is relatively few, we also calculate p-values using the wild-cluster bootstrap estimation of Cameron et al. (2008). Statistical significance is *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 12: Policy Effect on Employment by Gender, TNDSVW 2008 – EK Sample

	Number of Years on Each Side of the Cutoff							
	10	9	8	7	6	5	4	3
	I) 2008 EK Rural Sample							
Women								
	A) Worked Last Week or Usually has a job							
Policy	0.002	0.012	0.031	0.036	0.017	0.018	-0.005	-0.001
	[0.037]	[0.039]	[0.039]	[0.038]	[0.038]	[0.041]	[0.044]	[0.053]
No Obs.	2,256	2,039	1,843	1,645	1,420	1,179	934	707
	B) Worked Last Week							
Policy	0.045	0.062**	0.072**	0.086***	0.074**	0.074**	0.037	0.036
	[0.031]	[0.031]	[0.031]	[0.031]	[0.032]	[0.036]	[0.035]	[0.040]
No Obs.	2,256	2,039	1,843	1,645	1,420	1,179	934	707
Men								
	A) Worked Last Week or Usually has a job							
Policy	0.013	0.008	0.010	0.019	-0.014	-0.020	-0.018	0.023
	[0.042]	[0.043]	[0.044]	[0.044]	[0.046]	[0.048]	[0.051]	[0.054]
No Obs.	2,256	2,039	1,843	1,645	1,420	1,179	934	707
	B) Worked Last Week							
Policy	0.036	0.032	0.034	0.053	0.037	0.032	0.035	0.173**
	[0.046]	[0.048]	[0.049]	[0.049]	[0.051]	[0.053]	[0.062]	[0.067]
No Obs.	2,256	2,039	1,843	1,645	1,420	1,179	934	707
	II) 2008 EK Urban Sample							
Women								
	A) Worked Last Week or Usually has a job							
Policy	0.073	0.086	0.063	0.073	0.063	0.048	0.053	0.038
	[0.051]	[0.053]	[0.055]	[0.057]	[0.061]	[0.068]	[0.073]	[0.082]
No Obs.	1,771	1,643	1,497	1,341	1,169	981	786	594
	B) Worked Last Week							
Policy	0.055	0.059	0.045	0.046	0.059	0.044	0.072	0.040
	[0.047]	[0.049]	[0.050]	[0.051]	[0.056]	[0.063]	[0.069]	[0.076]
No Obs.	1,771	1,643	1,497	1,341	1,169	981	786	594
Men								
	A) Worked Last Week or Usually has a job							
Policy	0.005	0.008	0.003	0.002	0.011	-0.042	-0.059	-0.092
	[0.053]	[0.053]	[0.054]	[0.055]	[0.059]	[0.061]	[0.069]	[0.085]
No Obs.	1,771	1,643	1,497	1,341	1,169	981	786	594
	B) Worked Last Week							
Policy	0.053	0.053	0.039	0.033	0.046	-0.006	0.012	0.008
	[0.056]	[0.058]	[0.059]	[0.060]	[0.066]	[0.068]	[0.076]	[0.092]
No Obs.	1,771	1,643	1,497	1,341	1,169	981	786	594

Notes: The data come from Erten and Keskin (2018). 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 interview language was Turkish, a dummy for rural place of residence during childhood, and dummies for 26 NUTS-2 region of residence during childhood. The regressions are weighed using the sample weights, as in Erten and Keskin. Standard errors are clustered at the month-year of birth level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 13: Policy Effect on Women's Various Employment Outcomes, THLFS (2004-2019)

	Without Donut-Hole				With Donut-Hole			
	Number of Years Around Cutoff				Number of Years Around Cutoff			
	10	8	6	4	10	8	6	4
A) Employed								
	0.004	0.004	0.004	0.001	0.008*	0.008*	0.010*	0.007**
	[0.005]	[0.005]	[0.005]	[0.002]	[0.004]	[0.004]	[0.005]	[0.003]
Wild Bootstrap p-value	0.578	0.559	0.551	0.594	0.078	0.105	0.197	0.313
No obs.	1,003,841	837,468	646,524	441,752	886,229	719,856	528,912	324,140
B) Full-time Employed								
	0.009	0.008	0.007	0.005	0.016***	0.014**	0.016**	0.016***
	[0.006]	[0.006]	[0.005]	[0.003]	[0.005]	[0.005]	[0.006]	[0.002]
Wild Bootstrap p-value	0.207	0.273	0.266	0.000	0.009	0.038	0.094	0.031
No obs.	1,003,841	837,468	646,524	441,752	886,229	719,856	528,912	324,140
C) Wage Employed								
	0.007	0.007	0.006	0.005	0.013**	0.014**	0.014*	0.016***
	[0.007]	[0.006]	[0.006]	[0.003]	[0.006]	[0.006]	[0.006]	[0.002]
Wild Bootstrap p-value	0.404	0.347	0.407	0.023	0.059	0.069	0.145	0.031
No obs.	1,003,841	837,468	646,524	441,752	886,229	719,856	528,912	324,140
D) Full-time Wage Employed								
	0.009	0.009	0.008	0.006	0.016**	0.017**	0.018**	0.019***
	[0.007]	[0.006]	[0.006]	[0.003]	[0.006]	[0.006]	[0.006]	[0.002]
Wild Bootstrap p-value	0.266	0.264	0.267	0.016	0.018	0.037	0.092	0.000
No obs.	1,003,841	837,468	646,524	441,752	886,229	719,856	528,912	324,140
E) Full-time Wage Employed, Permanent Job								
	0.010	0.009	0.008	0.006	0.017**	0.017**	0.018**	0.018***
	[0.007]	[0.007]	[0.006]	[0.003]	[0.006]	[0.006]	[0.006]	[0.002]
Wild Bootstrap p-value	0.296	0.277	0.296	0.055	0.015	0.030	0.097	0.000
No obs.	1,003,841	837,468	646,524	441,752	886,229	719,856	528,912	324,140

Notes: The sample includes observations from 2004-2019 Turkish Household Labor Force Surveys. The sample is restricted to ages 18 and above. In each panel, we use alternative bandwidths gradually zooming in around the cutoff. The policy dummy is one when year of birth is greater 1987. Each cell comes from a separate regression of the specified schooling outcome on the policy dummy as well as the specified time trends. Standard errors are clustered at the year-of-birth level. However, as the number of clusters is relatively few, we also calculate p-values using the wild-cluster bootstrap estimation of Cameron et al. (2008). Statistical significance is *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 14: Evidence for Urban Areas that Contradicts the Instrumental Violence Hypothesis

	Number of Years on Each Side of the Cutoff							
	10	9	8	7	6	5	4	3
A) RURAL SAMPLE								
				I) Psychological Violence				
Policy	0.093*	0.106**	0.118**	0.129**	0.137**	0.152**	0.179**	0.136*
	[0.051]	[0.052]	[0.052]	[0.054]	[0.059]	[0.062]	[0.068]	[0.075]
Observations	2,253	2,036	1,840	1,642	1,417	1,176	931	704
				II) Financial Control Behavior				
Policy	0.214*	0.241**	0.250**	0.252**	0.232*	0.192	0.264*	0.254
	[0.120]	[0.117]	[0.115]	[0.114]	[0.119]	[0.130]	[0.156]	[0.168]
Observations	2,138	1,922	1,728	1,530	1,313	1,090	867	653
				III) Women: Worked Last Week				
Policy	0.045	0.062**	0.072**	0.086***	0.074**	0.074**	0.037	0.036
	[0.031]	[0.031]	[0.031]	[0.031]	[0.032]	[0.036]	[0.035]	[0.040]
Observations	2,256	2,039	1,843	1,645	1,420	1,179	934	707
B) URBAN SAMPLE								
				I) Psychological Violence				
Policy	-0.066	-0.062	-0.059	-0.057	-0.061	-0.053	-0.039	-0.083
	[0.070]	[0.072]	[0.073]	[0.076]	[0.078]	[0.082]	[0.092]	[0.121]
Observations	1,761	1,633	1,487	1,332	1,160	974	780	589
				II) Financial Control Behavior				
Policy	-0.068	-0.040	-0.018	0.009	-0.074	-0.142*	-0.144	-0.189
	[0.085]	[0.090]	[0.094]	[0.101]	[0.094]	[0.086]	[0.097]	[0.142]
Observations	1,568	1,441	1,296	1,142	991	839	676	517
				III) Women: Worked Last Week				
Policy	0.055	0.059	0.045	0.046	0.059	0.044	0.072	0.040
	[0.047]	[0.049]	[0.050]	[0.051]	[0.056]	[0.063]	[0.069]	[0.076]
Observations	1,771	1,643	1,497	1,341	1,169	981	786	594

Notes: The data come from the 2008 TNSDVW. 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, the regressions in both panels include split linear time trends on either side of the cutoff where the running variable is month-year of birth. The regressions also controls for birth-month dummies, a dummy for whether the interview language was Turkish, a dummy for rural place of residence at age 12, 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 birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 15: Policy Effect on Years of Schooling and Middle School Completion-Sample of Women who have ever had a Relationship, 2008 and 2014 TNSDVW

	Number of Years on Each Side of the Cutoff							
	10	*9*	*8*	*7*	*6*	*5*	*4*	*3*
	I) Years of Schooling							
A) Total Sample								
Policy Effect	1.026***	1.073***	1.055***	1.162***	1.173***	0.939***	0.964***	0.939***
	[0.196]	[0.205]	[0.213]	[0.218]	[0.238]	[0.262]	[0.287]	[0.348]
Observations	7,025	6,437	5,829	5,198	4,492	3,767	2,988	2,288
B) Rural Sample								
Policy Effect	1.705***	1.754***	1.683***	1.697***	1.696***	1.348***	1.232**	1.348**
	[0.330]	[0.343]	[0.350]	[0.371]	[0.400]	[0.431]	[0.494]	[0.588]
Observations	2,652	2,417	2,172	1,941	1,668	1,376	1,089	837
C) Urban Sample								
Policy Effect	0.654***	0.718***	0.735***	0.893***	0.893***	0.724**	0.776**	0.717*
	[0.236]	[0.250]	[0.264]	[0.267]	[0.287]	[0.320]	[0.353]	[0.395]
Observations	4,373	4,020	3,657	3,257	2,824	2,391	1,899	1,451
	II) Junior High School Completion							
A) Total Sample								
Policy Effect	0.225***	0.222***	0.216***	0.214***	0.214***	0.181***	0.171***	0.141***
	[0.026]	[0.027]	[0.028]	[0.030]	[0.033]	[0.035]	[0.040]	[0.047]
Observations	7,025	6,437	5,829	5,198	4,492	3,767	2,988	2,288
B) Rural Sample								
Policy Effect	0.365***	0.362***	0.357***	0.343***	0.347***	0.294***	0.275***	0.279***
	[0.048]	[0.049]	[0.051]	[0.053]	[0.057]	[0.062]	[0.073]	[0.084]
Observations	2,652	2,417	2,172	1,941	1,668	1,376	1,089	837
C) Urban Sample								
Policy Effect	0.150***	0.150***	0.144***	0.148***	0.143***	0.121***	0.112**	0.075
	[0.031]	[0.033]	[0.035]	[0.037]	[0.040]	[0.044]	[0.052]	[0.060]
Observations	4,373	4,020	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 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, 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 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 16: Policy Effect on Intimate Partner Violence Outcomes, 2008 and 2014 TNSDVW

	Number of Years on Each Side of the Cutoff							
	10	*9*	*8*	*7*	*6*	*5*	*4*	*3*
	I) Physical Violence							
A) Total Sample								
Policy Effect	-0.062*	-0.063*	-0.043	-0.048	-0.045	-0.075*	-0.076	-0.073
	[0.032]	[0.034]	[0.036]	[0.038]	[0.042]	[0.044]	[0.048]	[0.052]
Observations	7,010	6,422	5,815	5,185	4,479	3,756	2,978	2,280
B) Rural Sample								
Policy Effect	-0.112**	-0.107*	-0.118**	-0.148**	-0.114*	-0.118*	-0.128*	-0.154**
	[0.055]	[0.056]	[0.059]	[0.061]	[0.062]	[0.065]	[0.075]	[0.076]
Observations	2,650	2,415	2,170	1,939	1,666	1,374	1,087	835
C) Urban Sample								
Policy Effect	-0.040	-0.046	-0.014	0.002	-0.009	-0.058	-0.053	-0.049
	[0.038]	[0.040]	[0.043]	[0.045]	[0.050]	[0.053]	[0.056]	[0.069]
Observations	4,360	4,007	3,645	3,246	2,813	2,382	1,891	1,445
	II) Sexual Violence							
A) Total Sample								
Policy Effect	-0.053	-0.043	-0.030	-0.032	-0.031	-0.055	-0.030	-0.089
	[0.037]	[0.039]	[0.041]	[0.042]	[0.048]	[0.053]	[0.059]	[0.065]
Observations	7,008	6,420	5,814	5,184	4,479	3,756	2,978	2,280
B) Rural Sample								
Policy Effect	-0.017	-0.011	-0.008	-0.006	-0.019	-0.042	-0.015	-0.087
	[0.056]	[0.059]	[0.062]	[0.061]	[0.069]	[0.079]	[0.084]	[0.097]
Observations	2,650	2,415	2,170	1,939	1,666	1,374	1,087	835
C) Urban Sample								
Policy Effect	-0.076*	-0.064	-0.047	-0.048	-0.036	-0.061	-0.033	-0.092
	[0.043]	[0.045]	[0.047]	[0.050]	[0.056]	[0.061]	[0.067]	[0.076]
Observations	4,358	4,005	3,644	3,245	2,813	2,382	1,891	1,445

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence against Women. 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, 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 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 17: Policy Effect on Outcomes related to Instrumental Violence Hypothesis

	Number of Years on Each Side of the Cutoff							
	10	*9*	*8*	*7*	*6*	*5*	*4*	*3*
	I) Psychological Violence							
A) Total Sample								
Policy Effect	-0.023	-0.022	-0.006	-0.009	-0.001	-0.007	0.001	-0.009
	[0.030]	[0.031]	[0.032]	[0.034]	[0.038]	[0.041]	[0.046]	[0.050]
Observations	7,010	6,422	5,815	5,185	4,479	3,756	2,978	2,280
B) Rural Sample								
Policy Effect	0.010	0.023	0.034	0.038	0.037	0.026	0.074	0.075
	[0.046]	[0.049]	[0.051]	[0.056]	[0.060]	[0.068]	[0.077]	[0.078]
Observations	2,415	2,170	1,939	1,666	1,374	1,087	835	835
C) Urban Sample								
Policy Effect	-0.031	-0.035	-0.017	-0.023	-0.011	-0.022	-0.010	-0.044
	[0.037]	[0.038]	[0.039]	[0.040]	[0.044]	[0.045]	[0.053]	[0.057]
Observations	4,360	4,007	3,645	3,246	2,813	2,382	1,891	1,445
	II) Financial Control Behavior							
A) Total Sample								
Policy Effect	0.041	0.066	0.077	0.073	0.062	0.047	0.060	0.073
	[0.049]	[0.051]	[0.053]	[0.055]	[0.059]	[0.063]	[0.072]	[0.083]
Observations	6,617	6,050	5,455	4,837	4,169	3,505	2,792	2,144
B) Rural Sample								
Policy Effect	0.067	0.091	0.085	0.115	0.134	0.175	0.230*	0.279**
	[0.083]	[0.086]	[0.089]	[0.091]	[0.099]	[0.109]	[0.124]	[0.135]
Observations	2,526	2,300	2,059	1,833	1,575	1,301	1,039	797
C) Urban Sample								
Policy Effect	0.029	0.053	0.069	0.054	0.028	-0.005	-0.013	-0.007
	[0.053]	[0.053]	[0.057]	[0.059]	[0.065]	[0.064]	[0.072]	[0.091]
Observations	4,091	3,750	3,396	3,004	2,594	2,204	1,753	1,347
	III) Women Worked Last Week or Usually has a job							
A) Total Sample								
Policy Effect	0.017	0.026	0.030	0.030	0.026	0.004	-0.021	-0.023
	[0.018]	[0.019]	[0.020]	[0.021]	[0.021]	[0.022]	[0.023]	[0.026]
Observations	7,025	6,437	5,829	5,198	4,492	3,767	2,988	2,288
B) Rural Sample								
Policy Effect	-0.022	-0.016	-0.013	0.002	-0.009	-0.019	-0.032	-0.036
	[0.035]	[0.037]	[0.038]	[0.039]	[0.041]	[0.044]	[0.046]	[0.054]
Observations	2,652	2,417	2,172	1,941	1,668	1,376	1,089	837
C) Urban Sample								
Policy Effect	0.039	0.050*	0.056**	0.047*	0.041	0.013	-0.009	-0.006
	[0.025]	[0.026]	[0.027]	[0.028]	[0.029]	[0.031]	[0.034]	[0.036]
Observations	4,373	4,020	3,657	3,257	2,824	2,391	1,899	1,451

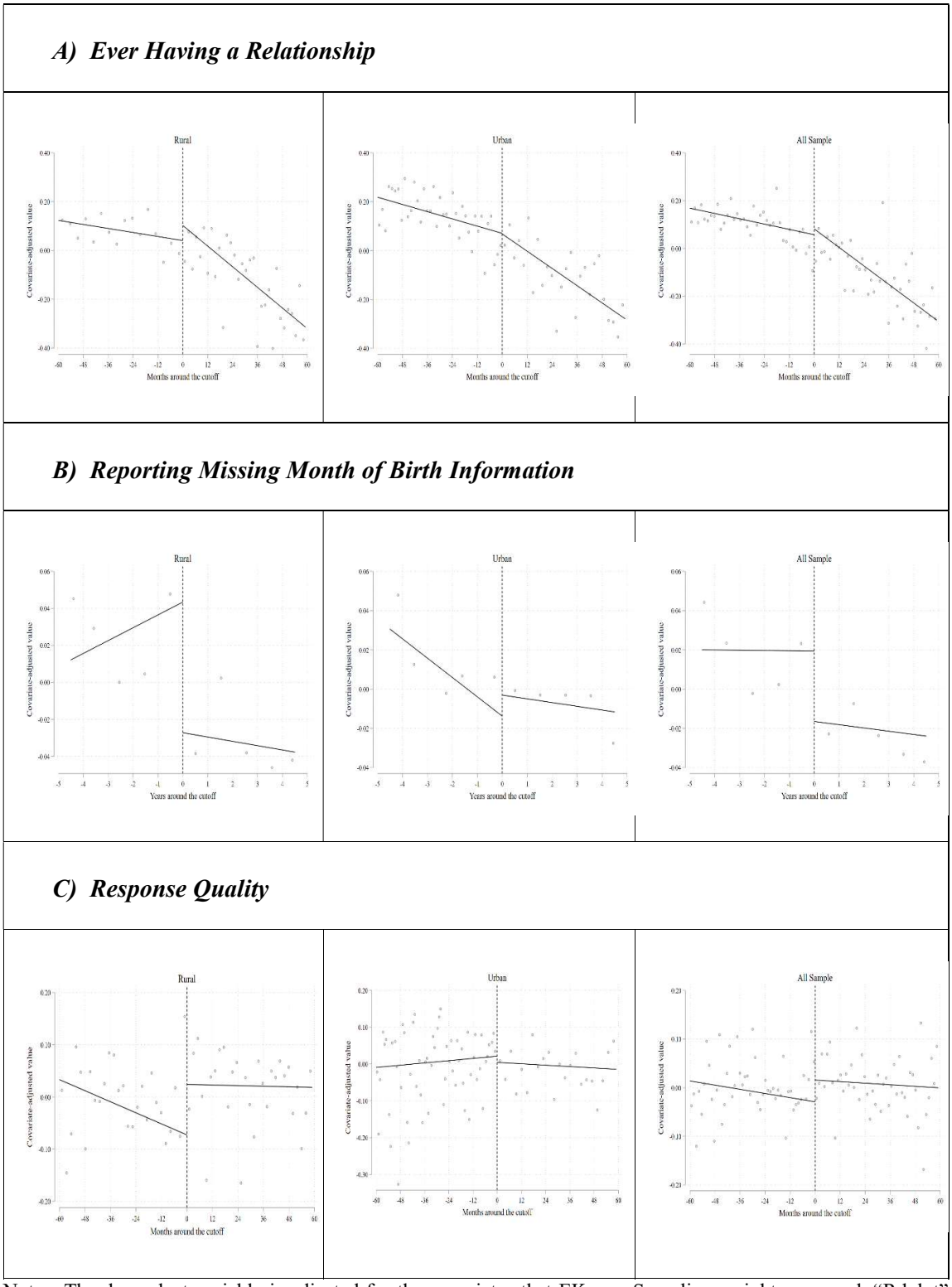
Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence against Women. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. In addition to the policy dummy and split linear 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, 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 percent level, ** at the 5 percent level, * at the 10 percent level.

Table 18: Policy Effect on Partner Characteristics

	Number of Years on Each Side of the Cutoff							
	10	*9*	*8*	*7*	*6*	*5*	*4*	*3*
	I) Partner's Years of Schooling							
A) Total Sample								
Policy	0.452**	0.441**	0.410*	0.360	0.359	0.311	0.327	0.647*
	[0.208]	[0.220]	[0.230]	[0.238]	[0.256]	[0.278]	[0.292]	[0.336]
Observations	6,923	6,341	5,741	5,116	4,422	3,714	2,950	2,260
B) Rural Sample								
Policy	0.500	0.545	0.534	0.438	0.419	0.328	0.500	0.954*
	[0.320]	[0.334]	[0.350]	[0.360]	[0.376]	[0.389]	[0.430]	[0.511]
Observations	2,614	2,382	2,139	1,910	1,641	1,356	1,073	825
C) Urban Sample								
Policy	0.419	0.398	0.360	0.326	0.341	0.315	0.307	0.555
	[0.265]	[0.281]	[0.296]	[0.309]	[0.335]	[0.357]	[0.383]	[0.438]
Observations	4,309	3,959	3,602	3,206	2,781	2,358	1,877	1,435
	II) Age gap							
A) Total Sample								
Policy	0.337	0.277	0.215	0.123	-0.061	-0.276	-0.228	-0.462
	[0.277]	[0.278]	[0.284]	[0.303]	[0.298]	[0.317]	[0.360]	[0.387]
Observations	5,803	5,274	4,723	4,158	3,556	2,963	2,358	1,818
B) Rural Sample								
Policy	-0.089	-0.188	-0.185	-0.368	-0.451	-0.631	-0.580	-1.003
	[0.408]	[0.422]	[0.449]	[0.469]	[0.491]	[0.515]	[0.604]	[0.605]
Observations	2,355	2,135	1,902	1,688	1,445	1,189	950	734
C) Urban Sample								
Policy	0.629*	0.578	0.489	0.438	0.197	-0.011	0.047	-0.071
	[0.351]	[0.353]	[0.358]	[0.376]	[0.376]	[0.400]	[0.475]	[0.518]
Observations	3,448	3,139	2,821	2,470	2,111	1,774	1,408	1,084
	III) Partner Worked Last Week or Usually has a job							
A) Total Sample								
Policy	0.006	0.015	0.022	0.035	0.025	0.013	0.002	-0.000
	[0.019]	[0.020]	[0.021]	[0.022]	[0.024]	[0.025]	[0.027]	[0.031]
Observations	7,025	6,437	5,829	5,198	4,492	3,767	2,988	2,288
B) Rural Sample								
Policy	0.004	0.007	0.011	0.031	0.006	-0.003	-0.044	-0.029
	[0.031]	[0.032]	[0.033]	[0.035]	[0.036]	[0.036]	[0.040]	[0.049]
Observations	2,652	2,417	2,172	1,941	1,668	1,376	1,089	837
C) Urban Sample								
Policy	0.007	0.021	0.028	0.038	0.033	0.020	0.026	0.020
	[0.025]	[0.026]	[0.027]	[0.028]	[0.031]	[0.033]	[0.035]	[0.040]
Observations	4,373	4,020	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 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, 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 percent level, ** at the 5 percent level, * at the 10 percent level.

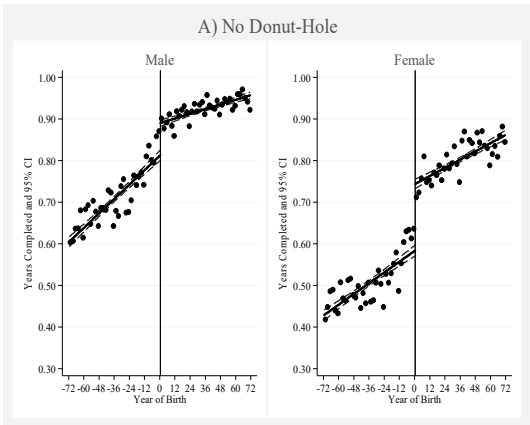
Figure 1: Policy Effects on Sample Selection and Response Quality

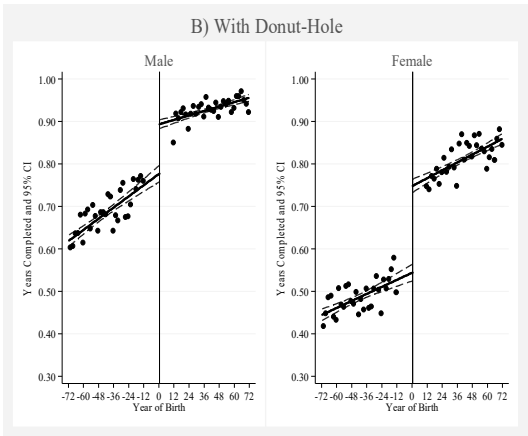


Notes: The dependent variable is adjusted for the covariates that EK use. Sampling weights are used. "Rdplot" package of CCT is used.

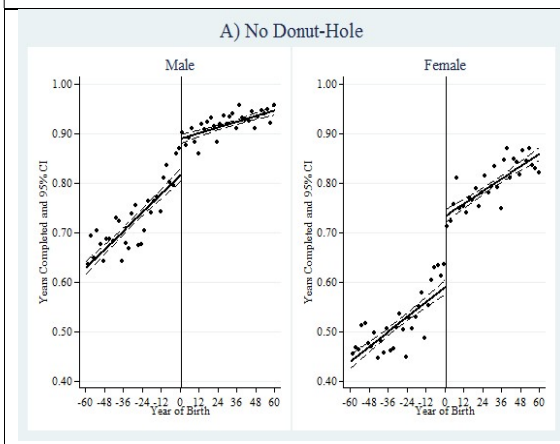
Figure 2: Policy Effect on Junior-High School Completion, 2014 THLFS

1) Bandwidth = 72 months





2) Bandwidth = 60 months



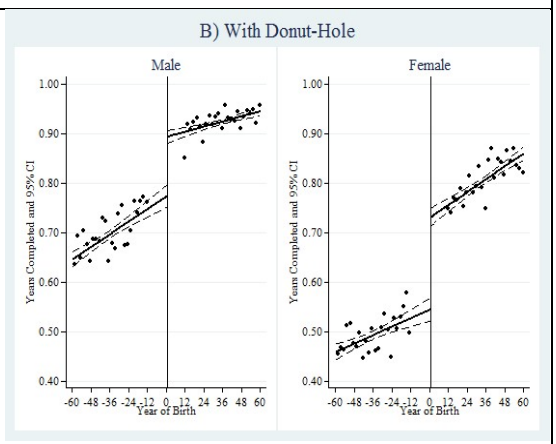
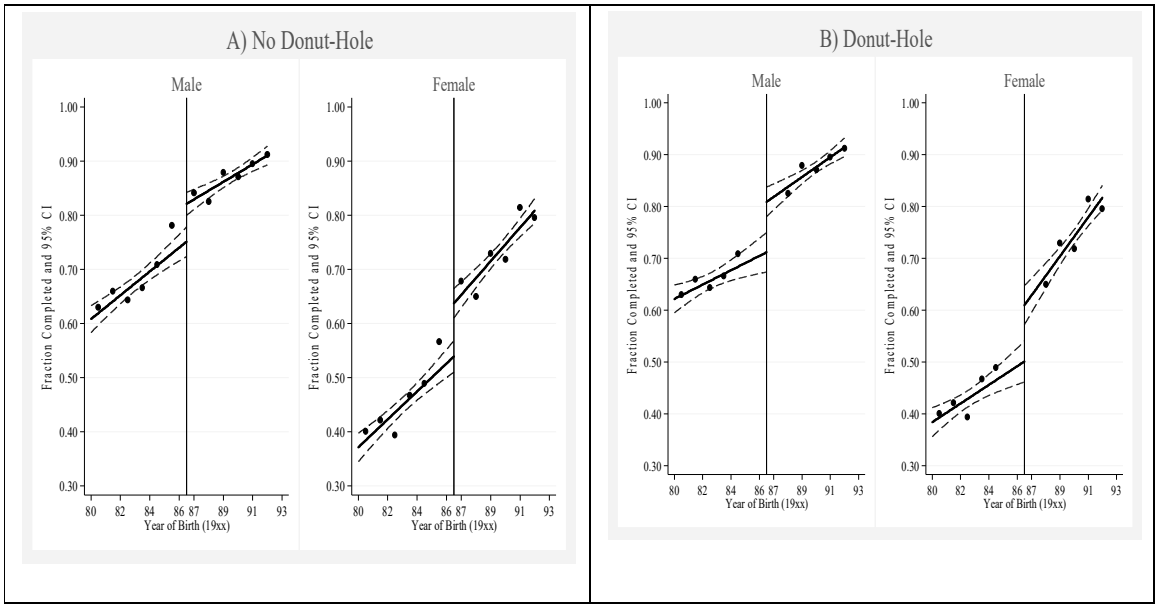
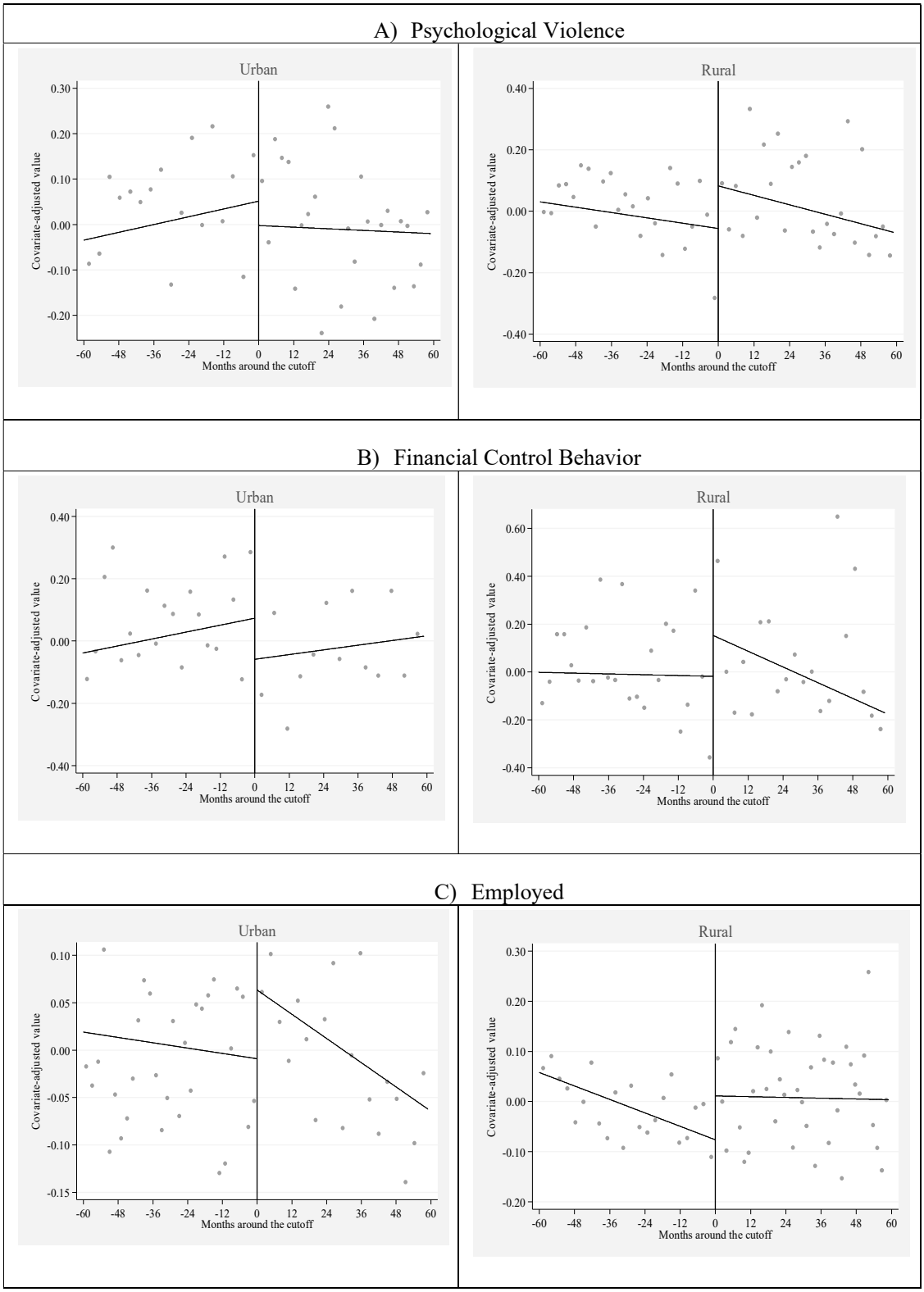


Figure 3: Fraction Completing Middle School, 2008 TNSDVW



Notes: The data come from the 2008 TNSDVW. The sample is restricted to individuals aged 16 and above (as most individuals complete grade 8 at age 14 or 15).

Figure 4: Key Outcome Variables in EK



Notes: The sample is the same as that in Erten and Keskin (2018). The dependent variables are adjusted for the covariates that EK use, and sampling weights are used as in EK. "Rdplot" package of CCT is used.

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APPENDIX A

Table A1: Policy Effect on Key Variables of Interest with Alternative Definitions of Rural Areas during Childhood using Optimal Bandwidths

A) Reduced Form - IK Bandwiths				B) Reduced Form - CCFT Bandwidths				C) 2SLS - CCFT Bandwidths			
	Psych. Violence	Financial Control Behavior	Emp.		Psych. Violence	Financial Control Behavior	Emp.		Psych. Violence	Financial Control Behavior	Emp.
1) EK Sample: Meaningless and Inconsistent Rural Definition											
For Movers: Rural defined as ***District Centers*** *and Villages at Age 12*											
For Stayers: Rural defined using Survey Variable "Rural" at the time of Survey											
Optimal BW (b)	0.138**	0.240*	0.059**	Conventional	0.134*	0.320**	0.027	Conventional	0.067	0.156*	0.008
	(0.058)	(0.138)	(0.029)		(0.069)	(0.134)	(0.040)		(0.044)	(0.095)	(0.019)
b/2	0.134*	0.250	0.020	Bias-corrected	0.132*	0.399***	0.024	Bias-corrected	0.071	0.177*	0.003
	(0.073)	(0.158)	(0.038)		(0.069)	(0.134)	(0.040)		(0.044)	(0.095)	(0.019)
3b/2	0.120**	0.240*	0.061**	Robust	0.132	0.399**	0.024	Robust	0.071	0.177	0.003
	(0.053)	(0.124)	(0.028)		(0.083)	(0.171)	(0.050)		(0.053)	(0.109)	(0.024)
2b	0.098*	0.223*	0.045	BW loc. poly.	31.87	16.35	22.46	BW loc. poly.	30.68	27.69	25.04
	(0.051)	(0.123)	(0.028)	BW bias	49.79	28.40	35.97	BW bias	48.64	46.58	43.47
BW loc. poly.	75.14	70.71	77.57								
2) Meaningful but Inconsistent Rural Definition											
For Movers: Rural defined as Villages at Age 12											
For Stayers: Rural defined using Survey Variable "Rural" at the time of Survey											
Optimal BW (b)	0.098	0.275	0.055	Conventional	0.083	0.424***	0.057	Conventional	0.064	0.220	0.024
	(0.060)	(0.196)	(0.038)		(0.064)	(0.155)	(0.037)		(0.055)	(0.156)	(0.041)
b/2	0.093	0.331*	0.040	Bias-corrected	0.095	0.490***	0.057	Bias-corrected	0.081	0.278*	0.029
	(0.073)	(0.192)	(0.055)		(0.064)	(0.155)	(0.037)		(0.055)	(0.156)	(0.041)
3b/2	0.079	0.266	0.045	Robust	0.095	0.490**	0.057	Robust	0.081	0.278	0.029
	(0.056)	(0.172)	(0.037)		(0.075)	(0.191)	(0.046)		(0.064)	(0.175)	(0.048)
2b	0.062	0.269	0.043	BW loc. poly.	31.84	18.78	23.32	BW loc. poly.	39.01	37.55	32.96
	(0.055)	(0.164)	(0.038)	BW bias	50.44	35.94	38.82	BW bias	58.42	62.94	52.01
BW loc. poly.	93.61	59.16	84.30								
3) Meaningful and Consistent Rural Definition											
For Movers: Rural defined as Villages at Age 12											
For Stayers: Rural defined as Villages at the Time of Survey											
Optimal BW (b)	0.108*	0.344*	0.041	Conventional	0.148**	0.573***	-0.001	Conventional	0.131	0.402	0.003
	(0.062)	(0.208)	(0.040)		(0.068)	(0.169)	(0.051)		(0.120)	(0.302)	(0.067)
b/2	0.117	0.481**	0.008	Bias-corrected	0.172**	0.645***	-0.012	Bias-corrected	0.168	0.532*	0.006
	(0.082)	(0.187)	(0.055)		(0.068)	(0.169)	(0.051)		(0.120)	(0.302)	(0.067)
3b/2	0.085	0.281	0.038	Robust	0.172*	0.645***	-0.012	Robust	0.168	0.532	0.006
	(0.057)	(0.181)	(0.038)		(0.089)	(0.199)	(0.059)		(0.139)	(0.329)	(0.076)
2b	0.070	0.268	0.042	BW loc. poly.	23.62	19.80	28.87	BW loc. poly.	42.04	39.97	35.76
	(0.055)	(0.169)	(0.038)	BW bias	40.05	37.08	48.04	BW bias	66.06	69.87	57.77
BW loc. poly.	100.5	56.86	110.7								
4) Sample of Villages and District Centers -- for both movers and stayers											
Optimal BW (b)	0.090*	0.153	0.050	Conventional	0.136**	0.348***	-0.044	Conventional	0.044	0.204	-0.027
	(0.048)	(0.118)	(0.031)		(0.062)	(0.125)	(0.038)		(0.041)	(0.144)	(0.025)
b/2	0.083	0.161	0.009	Bias-corrected	0.159**	0.427***	-0.061	Bias-corrected	0.039	0.269*	-0.043*
	(0.062)	(0.135)	(0.037)		(0.062)	(0.125)	(0.038)		(0.041)	(0.144)	(0.025)
3b/2	0.061	0.159	0.041	Robust	0.159**	0.427***	-0.061	Robust	0.039	0.269	-0.043
	(0.045)	(0.106)	(0.029)		(0.074)	(0.149)	(0.047)		(0.048)	(0.166)	(0.032)
2b	0.042	0.138	0.035	BW loc. poly.	24.02	16.27	25.82	BW loc. poly.	31.09	23.98	25.79
	(0.044)	(0.104)	(0.029)	BW bias	39.03	29.77	42.66	BW bias	53.23	39.78	40.58
BW loc. poly.	77.46	73.38	105								

Notes: The data come from the 2008 Turkish National Survey on Domestic Violence against Women. In the survey, if a person has been residing in the same place that she lived at age 12 (stayer), she is asked about her current location only; whereas if a person has changed her location after age 12 (mover), she is asked about her location at age 12. Accordingly, the samples in four separate panels are defined as given in panel headings. The sample is restricted to women who have ever had a relationship as in EK. In panel (A)s, IK optimal bandwidths that do not account for covariates or sampling weights are used to be consistent with EK. In the estimation, unlike EK, we use the "rd" command that allows for covariates and sampling weights. In panel (B)s, we use CCFT optimal bandwidths. These optimal bandwidths are calculated conditional on covarites and sampling weights and estimation is done accordingly using the "rdrobust" command of CCFT. In panel (C)s, the same approach as in panel (B) is taken, but a fuzzy RDD is used. CCFT bandwidths are MSE-optimal and the degree of local polynomials is one (two for bias correction). Covariates include dummies for birth months, for birth region of residence and Turkish language. Standard errors are clustered at the birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table A2: Policy Effect on Ever Having a Relationship, Response Quality and Missing Month-Year of Birth – CCFT Method

	(1)	(2)	(3)	(4)	(5)	(6)
	A) Rural Sample			B) Total Sample		
VARIABLES	Ever Having a Relationship	Response Quality Good or Very Good	Birth Month is Missing	Ever Having a Relationship	Response Quality Good or Very Good	Birth Month is Missing
Conventional	0.103**	0.135*	-0.079***	0.107***	-0.003	-0.042***
	[0.040]	[0.070]	[0.024]	[0.029]	[0.040]	[0.014]
Bias-corrected	0.114***	0.096	-0.103***	0.129***	-0.035	-0.055***
	[0.040]	[0.070]	[0.024]	[0.029]	[0.040]	[0.014]
Robust	0.114**	0.096	-0.103***	0.129***	-0.035	-0.055***
	[0.047]	[0.080]	[0.020]	[0.034]	[0.046]	[0.011]
Observations	6,463	6,026	7,478	10,661	9,625	11,349
BW loc. poly.	38.68	23.64	5.851	29.46	18.73	5.479
BW bias	51.82	39.14	7.024	49.11	35.59	6.559
Order of local poly.	1	1	1	1	1	1
Order Bias	2	2	2	2	2	2

Notes: The data come from the 2008 Turkish National Survey on Domestic Violence against Women. In columns (2), (3), (5) and (6), the sample is restricted to women who have ever had a relationship. In columns (3) and (6), running variable is the year of birth, in other columns, it is month-year of birth. The optimal bandwidths are calculated conditional on covarites and sampling weights and estimation is done accordingly using the "rdrobust" command of CCFT. CCFT bandwidths are MSE-optimal and the degree of local polynomials is one (two for bias correction). Covariates include birth-month dummies (except in columns (3) and (6)), a dummy for whether the childhood region was a rural area, a dummy for whether the interview language was Turkish and dummies for 26 NUTS-2 region of residence at age 12. Sampling weights are used. Standard errors are clustered at the birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table A3: Checking the Continuity Assumption of RDD for Psychological Violence Variable in Rural Areas via Alternative Cutoffs

Dependent Variable: Psychological Violence										
	A) Birth Year <= 1986							B) Birth Year >=1987		
	Maximum Number of Years on Each Side of the Cutoff							Number of Years		
Cutoff	7	6	5	4	3	2	Cutoff	4	3	2
Jan-85	0.015	0.022	0.038	0.038	0.074	0.149*	Jan-89	-0.104	-0.116	-0.065
	[0.053]	[0.056]	[0.059]	[0.060]	[0.062]	[0.076]		[0.102]	[0.106]	[0.130]
Jan-84	-0.051	-0.038	-0.035	-0.036	-0.084	-0.144	Jul-89	-0.180**	-0.237***	-0.281***
	[0.053]	[0.055]	[0.059]	[0.061]	[0.064]	[0.090]		[0.082]	[0.089]	[0.104]
Jan-83	0.028	0.029	0.047	0.057	0.052	0.056	Jan-90	-0.077	-0.041	-0.069
	[0.052]	[0.052]	[0.054]	[0.059]	[0.065]	[0.093]		[0.087]	[0.089]	[0.130]
Jan-82	-0.002	0.021	0.022	0.023	0.055	0.068	Jul-90	-0.001	0.113	0.229**
	[0.051]	[0.053]	[0.054]	[0.059]	[0.070]	[0.079]		[0.086]	[0.090]	[0.112]
Jan-81	-0.035	-0.05	-0.055	-0.066	-0.095*	-0.09	Jan-91	0.082	0.159	0.165
	[0.045]	[0.047]	[0.050]	[0.052]	[0.054]	[0.068]		[0.095]	[0.099]	[0.123]
Jan-80	0.044	0.008	-0.013	-0.013	-0.001	0.069	Jul-91	0.071	0.099	0.001
	[0.043]	[0.044]	[0.046]	[0.051]	[0.054]	[0.063]		[0.123]	[0.127]	[0.142]
Jan-79	0.071*	0.051	0.015	-0.004	0.038	-0.042	Jan-92	-0.109	-0.122	-0.294**
	[0.042]	[0.043]	[0.045]	[0.048]	[0.056]	[0.059]		[0.118]	[0.113]	[0.121]
Jan-78	0.077*	0.082*	0.074	0.058	-0.004	0.06				
	[0.041]	[0.046]	[0.048]	[0.048]	[0.054]	[0.067]				
Jan-77	0.031	0.049	0.075	0.025	-0.006	-0.102				
	[0.043]	[0.047]	[0.052]	[0.055]	[0.066]	[0.080]				

Notes: The data come from the 2008 Turkish National Survey on Domestic Violence against Women. The sample is restricted to the birth cohorts unaffected by the policy in panel (A) and to the birth cohorst affected by the policy in panel (B). In both panels, we take counterfactual policy cutoffs by gradually shifting the cutoff point, as specified in columns (1) and (8). The cutoffs are chosen so as to keep at least 2 years of data on each side of the cutoff. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. The bandwidths in panel (B) are much narrower because the data has only 7 years on the right hand side of the cutoff. 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 interview language was Turkish, a dummy for rural place of residence at age 12, and dummies for 26 NUTS-2 region of residence at age 12. The regressions are weighed using the sample weights, as in Erten and Keskin. Standard errors are clustered at the birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table A4: Policy Effect on Middle School Completion by Gender – Alternative Degrees of Polynomials with 10-year bandwidths on each side of the Cutoff, 2014 THLFS

A) MALE SAMPLE					
Degree of Split Polynomials	Linear	Quadratic	Cubic	Fourth-order	Fifth-order
Policy	0.133***	0.077***	0.044***	0.035*	0.045**
	[0.010]	[0.012]	[0.015]	[0.020]	[0.022]
Observations	64,408	64,408	64,408	64,408	64,408
R-squared	0.124	0.125	0.126	0.126	0.126
B) FEMALE SAMPLE					
Degree of Split Polynomials	Linear	Quadratic	Cubic	Fourth-order	Fifth-order
Policy	0.186***	0.155***	0.099***	0.073***	0.072***
	[0.010]	[0.014]	[0.016]	[0.019]	[0.024]
Observations	69,250	69,250	69,250	69,250	69,250
R-squared	0.162	0.162	0.163	0.163	0.163

Notes: The data come from the 2014 Turkish Household Labor Force Survey. Each cell comes from a separate regression using 10-year intervals around the cutoff. Different orders of polynomials that are split on each side of the cutoff are used--as specified in column headings. In addition to the policy dummy and split time trends on either side of the cutoff, the regressions also control for birth-month dummies. Standard errors are clustered at the birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table A5: Policy Effect on Middle School Completion by Gender – Nonparametric approach of CCFT, 2014 THLFS

	(1)	(2)	(3)	(4)
	A) Men		*B) Women*	
Conventional	0.038***	0.031*	0.089***	0.083***
	(0.013)	(0.016)	(0.010)	(0.014)
Bias-corrected	0.032**	0.029*	0.083***	0.080***
	(0.013)	(0.016)	(0.010)	(0.014)
Robust	0.032**	0.029	0.083***	0.080***
	(0.014)	(0.018)	(0.012)	(0.017)
Observations	169,355	169,355	174,882	174,882
BW Type	MSE	MSE	MSE	MSE
BW loc. poly.	24.67	47.80	22.73	43.96
BW bias	53.02	67.19	45.03	68.50
Order of local poly.	1	2	1	2
Order Bias	2	3	2	3

Notes: The data come from the 2014 Turkish Household Labor Force Survey. Nonparametric RD method of CCFT (rdrobust) is used. In each column, a different data-driven bandwidth is taken. These bandwidths differ by whether they are MSE-optimal or CER-optimal and the degree of local polynomials. A triangular kernel is used. Covariates include dummies for birth months, a dummy for whether the interview language was Turkish, a dummy for rural place of residence at age 12 (only in panel B), and dummies for 26 NUTS-2 region of residence at age 12. Sampling weights are used, as in Erten and Keskin. Standard errors are clustered at the month-year of birth level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table A6: Policy Effect on Middle School Completion by Gender – 2008 TNDSVW with a Donut-Hole

	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidth on the left	10 years	9 years	8 years	7 years	6 years	5 years
Bandwidth on the right	6 years	6 years	6 years	6 years	6 years	5 years
A) MALE SAMPLE						
Policy	0.091***	0.086***	0.113***	0.097***	0.089***	0.083**
	[0.024]	[0.026]	[0.027]	[0.023]	[0.023]	[0.028]
Wild-cluster Bootstrap p-value	0.016	0.033	0.008	0.000	0.000	0.031
Pre-policy trend	0.015***	0.017**	0.009*	0.014***	0.017***	0.018**
	[0.004]	[0.006]	[0.005]	[0.003]	[0.004]	[0.007]
Post-policy trend	0.019***	0.019***	0.019***	0.019***	0.019***	0.020***
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.004]
Observations	9,020	8,620	7,941	7,464	6,796	5,460
R-squared	0.078	0.078	0.068	0.071	0.07	0.059
B) FEMALE SAMPLE						
Policy	0.104***	0.101**	0.123***	0.109***	0.100***	0.056*
	[0.031]	[0.033]	[0.034]	[0.031]	[0.030]	[0.025]
Wild-cluster Bootstrap p-value	0.028	0.033	0.010	0.011	0.016	0.094
Pre-policy trend	0.019***	0.020***	0.013**	0.018***	0.021***	0.028***
	[0.004]	[0.005]	[0.004]	[0.003]	[0.004]	[0.008]
Post-policy trend	0.038***	0.038***	0.038***	0.038***	0.038***	0.048***
	[0.007]	[0.007]	[0.007]	[0.007]	[0.007]	[0.007]
Observations	9,099	8,628	8,015	7,488	6,840	5,473
R-squared	0.121	0.119	0.108	0.109	0.105	0.092

Notes: The data come from the 2008 TNSDVW. Since the sample is restricted to individuals aged 16 and older (most individuals complete middle school at age 14 or 15), the youngest birth cohort in the sample is born in 1992. In addition, the 1986 and 1987 birth-cohorts (the donut-hole) are excluded. The estimates in each column come from a separate regression using a sample defined according to the bandwidths specified in the column headings. Standard errors are clustered at the birth-year level. Since the number of cluster is small (ranging from 16 in column (1) to 10 in column (6)), we also provide wild-cluster bootstrap p-values. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table A7: Policy Effects on Schooling Outcomes –TDHS Data

	A) With Donut-Hole					B) Without Donut-Hole				
	Female		Male				Female		Male	
(1)	(2)	(3)	(4)	(5)		(6)	(7)	(8)	(9)	(10)
A) Policy Effect on Completing Grade 8										
1975-1985, 1988-1998	0.196***	14,503	0.155***	14,529		1975-1986, 1987-1998	0.165***	15,988	0.105***	15,975
	[0.019]		[0.025]				[0.022]		[0.034]	
Wild Bootstrap p-value	0.000		0.002			Wild Bootstrap p-value	0.000		0.018	
1978-1985, 1988-1995	0.180***	11,449	0.153***	11,445		1978-1986, 1987-1995	0.145***	12,934	0.088**	12,891
	[0.021]		[0.031]				[0.024]		[0.038]	
Wild Bootstrap p-value	0.000		0.004			Wild Bootstrap p-value	0.002		0.036	
B) Policy Effect on Completing High School										
1975-1985, 1988-1995	0.060***	11,975	0.090***	11,919		1975-1986, 1987-1995	0.051***	13,460	0.056**	13,365
	[0.013]		[0.021]				[0.010]		[0.024]	
Wild Bootstrap p-value	0.000		0.006			Wild Bootstrap p-value	0.002		0.074	
1978-1985, 1988-1995	0.066***	10,115	0.083***	10,119		1978-1986, 1987-1995	0.052***	11,600	0.044*	11,565
	[0.015]		[0.023]				[0.012]		[0.024]	
Wild Bootstrap p-value	0.000		0.002			Wild Bootstrap p-value	0.000		0.154	
C) Policy Effect on Years of Schooling										
1975-1985, 1988-1991	0.904***	9,099	0.818***	8,977		1975-1986, 1987-1991	0.674***	10,205	0.480**	10,063
	[0.155]		[0.210]				[0.139]		[0.214]	
Wild Bootstrap p-value	0.074		0.122			Wild Bootstrap p-value	0.042		0.158	
1978-1985, 1988-1991	0.894***	7,239	0.674**	7,177		1978-1986, 1987-1991	0.641***	8,345	0.326	8,263
	[0.180]		[0.219]				[0.146]		[0.196]	
Wild Bootstrap p-value	0.076		0.176			Wild Bootstrap p-value	0.040		0.178	

Notes: The sample includes observations from both the 2008 and 2013 DHS. The sample is restricted to ages 15 and above in panel (A), to ages 18 and above in panel (B), and to ages 22 and above in panel (C) in order to prevent censoring in each schooling outcome. As a result, while the youngest birth cohort is the 1998 birth cohort in panel (A), it is the 1995 birth-cohort in panel (B) and the 1991 birth-cohort in panel (C). The oldest birth-cohort in the samples is the 1975 birth cohort. In each panel, the estimates are given for two separate time intervals around the cutoff--as indicated in columns (1) and (6). The policy dummy is one when year of birth is greater 1987. Each cell comes from a separate regresion of the specified schooling outcome on the policy dummy as well as the specified time trends. Standard errors are clustered at the year-of-birth level. However, as the number of clusters is relatively few, we also calculate p-values using the wild-cluster bootstrap estimation of Cameron et al. (2008). Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A8: Policy Effect on Years of Schooling with Corrected Data on Years of Schooling

	Number of Years on Each Side of the Cutoff							
	10	*9*	*8*	*7*	*6*	*5*	*4*	*3*
	I) Years of Schooling							
A) EK Rural Sample								
EK Results	1.733***	1.845***	1.834***	1.744***	1.822***	1.708***	1.904***	2.126***
	[0.338]	[0.346]	[0.356]	[0.358]	[0.380]	[0.420]	[0.455]	[0.534]
Observations	2,075	1,878	1,704	1,521	1,320	1,095	869	659
Results with Corrected Data	1.773***	1.892***	1.880***	1.792***	1.958***	1.838***	2.149***	2.496***
	[0.366]	[0.372]	[0.376]	[0.383]	[0.402]	[0.449]	[0.494]	[0.586]
Observations	2,256	2,039	1,843	1,645	1,420	1,179	934	707
B) EK Urban Sample								
EK Results	0.840**	0.821**	0.742**	0.892**	0.716*	0.553	0.481	0.409
	[0.342]	[0.359]	[0.367]	[0.382]	[0.390]	[0.424]	[0.481]	[0.544]
Observations	1,717	1,595	1,453	1,306	1,139	954	763	574
Results with Corrected Data	0.856**	0.836**	0.737*	0.958**	0.772*	0.562	0.419	0.489
	[0.371]	[0.387]	[0.396]	[0.408]	[0.416]	[0.449]	[0.500]	[0.564]
Observations	1,771	1,643	1,497	1,341	1,169	981	786	594
	II) Partner's Years of Schooling							
A) EK Rural Sample								
EK Results	0.264	0.298	0.304	0.083	0.162	0.327	0.550	0.708
	[0.421]	[0.430]	[0.443]	[0.444]	[0.476]	[0.491]	[0.549]	[0.663]
Observations	2,194	1,981	1,789	1,597	1,379	1,147	905	687
Results with Corrected Data	0.338	0.368	0.357	0.147	0.209	0.369	0.682	0.907
	[0.404]	[0.410]	[0.421]	[0.423]	[0.451]	[0.476]	[0.524]	[0.629]
Observations	2,233	2,016	1,820	1,623	1,401	1,164	920	696
B) EK Urban Sample								
EK Results	0.739*	0.635	0.545	0.600	0.563	0.383	0.472	1.178**
	[0.407]	[0.421]	[0.437]	[0.452]	[0.475]	[0.497]	[0.542]	[0.517]
Observations	1,723	1,599	1,455	1,301	1,135	956	769	579
Results with Corrected Data	0.748*	0.654	0.565	0.585	0.556	0.386	0.469	1.075**
	[0.406]	[0.418]	[0.434]	[0.448]	[0.469]	[0.490]	[0.530]	[0.514]
Observations	1,743	1,616	1,470	1,315	1,145	963	775	585

Notes: The data come from the 2008 Turkish National Survey on Domestic Violence against Women. "EK results" are based on a years-of-schooling variable where missing status is assiged to those who have never been to school. 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, the regressions in both panels include 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 interview language was Turkish, and dummies for 26 NUTS-2 region of residence at age 12. The regressions are weighed using the sample weights as in Erten and Keskin. Standard errors are clustered at the birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

APPENDIX B – Checks of RDD Assumptions in Our Analysis

Figure B1: McCrary Density

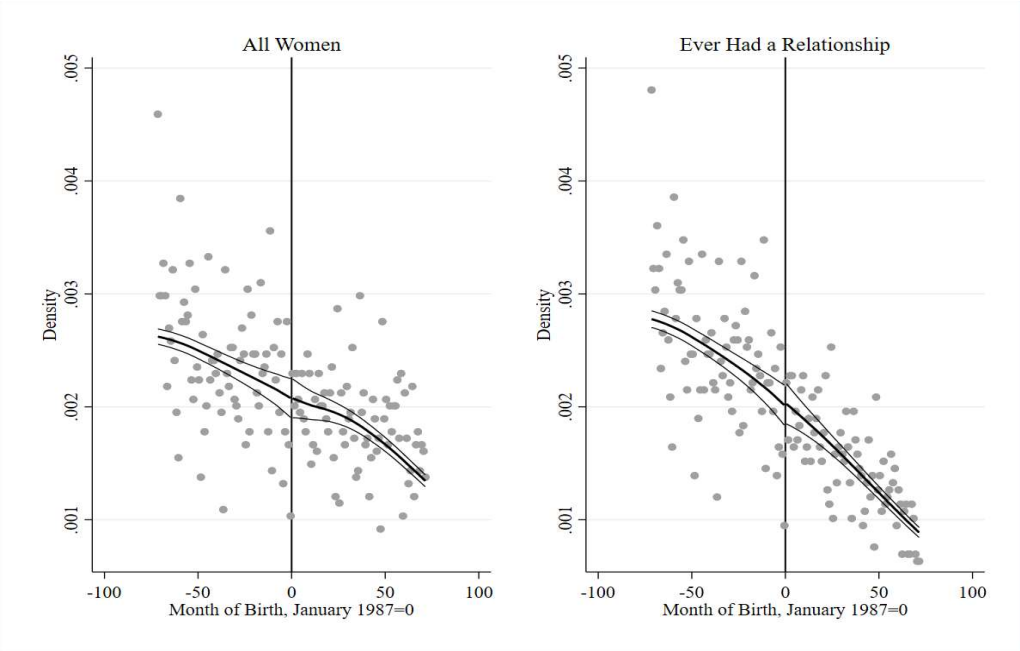
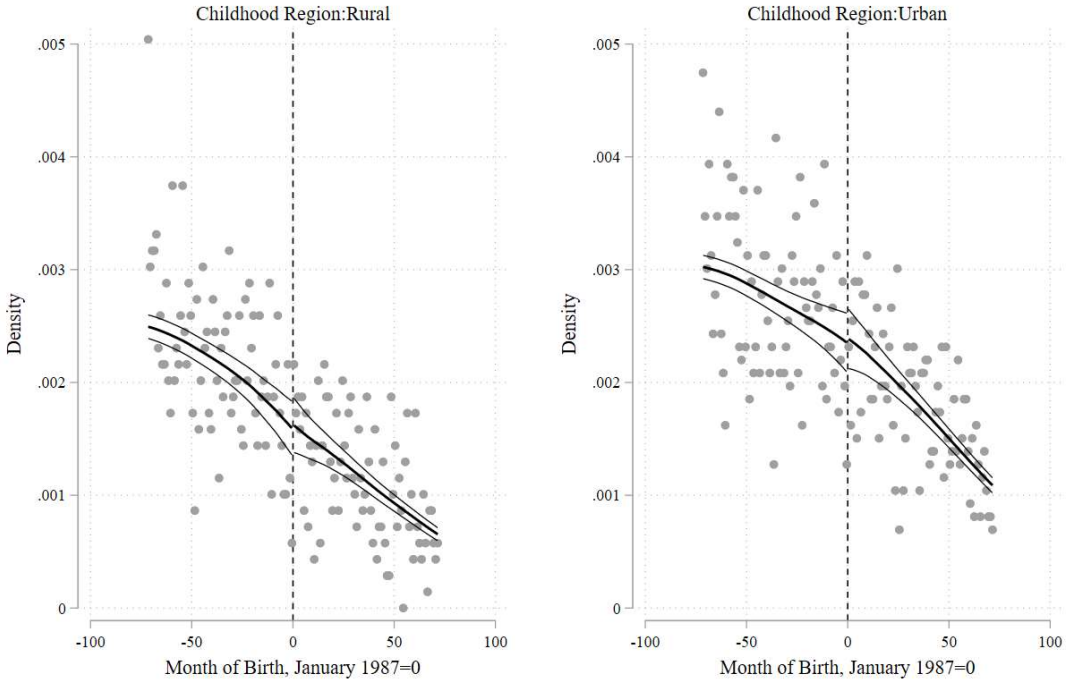


Figure B2: McCrary Density Test by Rural/Urban Status



Notes: The sample includes women who have ever had a relationship.

Table B1: Balanced Covariates

			Childhood Region			
	All Sample		**Rural**		**Urban**	
VARIABLES	RD Effect	p-value	RD Effect	p-value	RD Effect	p-value
Survey Wave	0.023	0.514	0.032	0.55	0.03	0.485
Interview Language: Non-Turkish	0.003	0.391	0.008	0.44	0.005	0.126
Childhood Region: Rural	0.016	0.634				
Istanbul Region (TR1)	-0.032	0.318	0.022	0.197	-0.02	0.636
West Marmara Region (TR2)	0.002	0.862	0.005	0.86	0.005	0.64
Aegean Region (TR3)	0.017	0.486	-0.009	0.785	0.033	0.24
East Marmara Region (TR4)	-0.022	0.239	-0.034	0.204	-0.01	0.661
West Anatolia Region (TR5)	0.009	0.684	0.041	0.171	-0.029	0.277
Mediterranean Region (TR6)	0.005	0.829	0.04	0.3	-0.027	0.367
Central Anatolia Region (TR7)	0.008	0.615	0.001	0.973	0.019	0.233
West Black Sea Region (TR8)	-0.002	0.937	0.004	0.928	-0.026	0.357
East Black Sea Region (TR9)	0.004	0.836	-0.034	0.371	0.014	0.42
Northeast Anatolia Region (TRA)	-0.012	0.349	-0.028	0.265	0	0.994
Central East Anatolia Region (TRB)	0	0.989	-0.007	0.857	-0.011	0.493
Southeast Anatolia Region (TRC)	0.023	0.361	0	0.989	0.052	0.084

Note: 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 within the bandwidth of 72 months. The estimates come from a regression where the controls include the policy dummy and split linear time trends on either side of the cutoff where the running variable is the year of birth. The regressions are weighed using the sample weights. Standard errors are clustered at the month-year of birth level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table B2: Checking the Continuity Assumption of RDD via Alternative Cutoffs for Physical Violence and Sexual Violence

	A) Birth Year <= 1986							B) Birth Year >=1987		
	Maximum Number of Years on Each Side of the Cutoff							Max. Number of Years		
Cutoff	7	6	5	4	3	2	Cutoff	4	3	2
I) Physical Violence										
Jan-85	-0.025	-0.005	-0.018	-0.011	-0.060	-0.112	Jan-89	-0.010	-0.019	0.005
	[0.063]	[0.067]	[0.068]	[0.073]	[0.071]	[0.075]		[0.063]	[0.067]	[0.075]
Jan-84	0.008	0.019	0.045	0.033	0.047	0.054	Jul-89	-0.014	-0.028	-0.074
	[0.055]	[0.058]	[0.061]	[0.060]	[0.068]	[0.076]		[0.051]	[0.056]	[0.068]
Jan-83	-0.050	-0.039	-0.035	-0.005	0.010	0.030	Jan-90	-0.028	-0.029	-0.052
	[0.053]	[0.056]	[0.057]	[0.062]	[0.069]	[0.084]		[0.053]	[0.053]	[0.076]
Jan-82	-0.095**	-0.091*	-0.084	-0.079	-0.082	-0.102	Jul-90	0.024	0.033	0.079
	[0.047]	[0.049]	[0.053]	[0.055]	[0.067]	[0.084]		[0.051]	[0.056]	[0.073]
Jan-81	-0.023	-0.026	-0.007	-0.002	0.021	0.095	Jan-91	0.025	0.049	0.090
	[0.044]	[0.046]	[0.050]	[0.056]	[0.055]	[0.067]		[0.050]	[0.058]	[0.071]
Jan-80	-0.012	-0.010	-0.022	-0.013	-0.014	-0.099	Jul-91	-0.031	-0.013	-0.050
	[0.042]	[0.046]	[0.051]	[0.054]	[0.065]	[0.074]		[0.054]	[0.059]	[0.071]
Jan-79	0.096**	0.069	0.080	0.071	0.055	0.124	Jan-92	-0.028	-0.032	-0.087
	[0.044]	[0.046]	[0.050]	[0.054]	[0.056]	[0.075]		[0.046]	[0.049]	[0.060]
Jan-78	0.058	0.080	0.036	0.004	-0.005	-0.066				
	[0.049]	[0.055]	[0.057]	[0.062]	[0.070]	[0.088]				
Jan-77	0.062	0.065	0.064	0.042	0.013	0.029				
	[0.045]	[0.049]	[0.055]	[0.057]	[0.069]	[0.085]				
II) Sexual Violence										
Jan-85	-0.085	-0.079	-0.097	-0.090	-0.123*	-0.088	Jan-89	0.024	0.013	0.033
	[0.057]	[0.060]	[0.062]	[0.068]	[0.068]	[0.076]		[0.046]	[0.054]	[0.049]
Jan-84	-0.049	-0.042	-0.035	-0.072	-0.093	-0.098	Jul-89	-0.082*	-0.098**	-0.062
	[0.052]	[0.054]	[0.057]	[0.058]	[0.063]	[0.073]		[0.042]	[0.043]	[0.051]
Jan-83	0.025	0.023	0.039	0.063	0.113*	0.161**	Jan-90	-0.038	-0.045	-0.068
	[0.049]	[0.050]	[0.051]	[0.054]	[0.058]	[0.069]		[0.041]	[0.037]	[0.055]
Jan-82	-0.042	-0.044	-0.050	-0.007	-0.023	-0.110	Jul-90	-0.028	-0.005	-0.019
	[0.043]	[0.046]	[0.048]	[0.051]	[0.057]	[0.067]		[0.046]	[0.041]	[0.055]
Jan-81	0.004	-0.014	0.008	-0.017	-0.012	0.079	Jan-91	-0.005	0.035	0.087
	[0.041]	[0.042]	[0.046]	[0.048]	[0.051]	[0.058]		[0.056]	[0.059]	[0.066]
Jan-80	-0.029	0.005	-0.039	-0.065	-0.064	-0.114*	Jul-91	0.072	0.130**	0.124**
	[0.037]	[0.040]	[0.043]	[0.047]	[0.053]	[0.058]		[0.051]	[0.061]	[0.060]
Jan-79	0.049	0.041	0.069*	0.074*	0.059	0.084*	Jan-92	-0.056	-0.032	-0.036
	[0.033]	[0.037]	[0.040]	[0.042]	[0.046]	[0.050]		[0.047]	[0.056]	[0.059]
Jan-78	0.028	0.022	0.044	0.068*	0.069	0.025				
	[0.035]	[0.036]	[0.037]	[0.040]	[0.045]	[0.049]				
Jan-77	-0.031	-0.008	-0.041	-0.038	-0.029	-0.046				
	[0.036]	[0.038]	[0.041]	[0.045]	[0.047]	[0.058]				

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence against Women. The sample is restricted to the birth cohorts unaffected by the policy in panel (A) and to the birth cohorst affected by the policy in panel (B). In both panels, we take counterfactual policy cutoffs by gradually shifting the cutoff point, as specified in columns (1) and (8). The cutoffs are chosen so as to keep at least 2 years of data on each side of the cutoff. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. The bandwidths in panel (B) are much narrower because the data has only 7 years on the right hand side of the cutoff. 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 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 birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table B3: Checking the Continuity Assumption of RDD via Alternative Cutoffs for Financial Control Behavior and Psychological Violence

	A) Birth Year <= 1986							B) Birth Year >=1987		
	Maximum Number of Years on Each Side of the Cutoff							Max. Number of Years		
Cutoff	7	6	5	4	3	2	Cutoff	4	3	2
I) Financial Control Behavior										
Jan-85	-0.044	-0.026	-0.039	-0.057	-0.090	-0.100	Jan-89	0.150**	0.137*	0.129
	[0.055]	[0.062]	[0.065]	[0.070]	[0.075]	[0.081]		[0.075]	[0.077]	[0.089]
Jan-84	0.013	0.042	0.062	0.057	0.012	0.017	Jul-89	0.143*	0.149*	0.050
	[0.073]	[0.073]	[0.075]	[0.078]	[0.078]	[0.088]		[0.073]	[0.080]	[0.090]
Jan-83	-0.005	-0.008	0.023	0.051	0.089	0.061	Jan-90	0.042	0.033	-0.060
	[0.061]	[0.062]	[0.062]	[0.062]	[0.071]	[0.081]		[0.072]	[0.081]	[0.095]
Jan-82	-0.065	-0.073	-0.082	-0.045	-0.050	-0.031	Jul-90	0.071	0.040	0.032
	[0.051]	[0.053]	[0.054]	[0.056]	[0.062]	[0.072]		[0.062]	[0.067]	[0.080]
Jan-81	-0.080*	-0.064	-0.068	-0.110*	-0.076	-0.018	Jan-91	0.023	-0.009	0.047
	[0.046]	[0.048]	[0.052]	[0.059]	[0.064]	[0.070]		[0.059]	[0.066]	[0.087]
Jan-80	-0.008	-0.016	-0.007	-0.018	-0.026	-0.030	Jul-91	-0.063	-0.102*	-0.116
	[0.044]	[0.046]	[0.052]	[0.057]	[0.060]	[0.064]		[0.053]	[0.061]	[0.077]
Jan-79	0.063	0.067	0.060	0.114**	0.094	0.042	Jan-92	-0.059	-0.048	-0.054
	[0.045]	[0.050]	[0.054]	[0.057]	[0.064]	[0.078]		[0.059]	[0.074]	[0.085]
Jan-78	0.070	0.071	0.091	0.050	0.085	0.082				
	[0.049]	[0.052]	[0.055]	[0.062]	[0.071]	[0.088]				
Jan-77	0.014	0.018	-0.028	-0.050	-0.135**	-0.085				
	[0.052]	[0.054]	[0.054]	[0.058]	[0.059]	[0.080]				
II) Psychological Violence										
Jan-85	-0.023	-0.011	-0.022	-0.016	-0.020	-0.017	Jan-89	0.019	0.027	0.068
	[0.059]	[0.059]	[0.059]	[0.063]	[0.063]	[0.070]		[0.052]	[0.055]	[0.061]
Jan-84	0.006	0.005	0.019	0.005	-0.002	-0.006	Jul-89	-0.016	-0.029	-0.032
	[0.044]	[0.046]	[0.048]	[0.052]	[0.061]	[0.077]		[0.048]	[0.055]	[0.066]
Jan-83	0.004	0.007	0.006	0.028	0.024	0.024	Jan-90	-0.043	-0.033	-0.077
	[0.044]	[0.045]	[0.047]	[0.049]	[0.056]	[0.077]		[0.050]	[0.055]	[0.076]
Jan-82	-0.015	-0.013	-0.012	-0.018	-0.009	-0.025	Jul-90	-0.016	-0.011	-0.018
	[0.037]	[0.039]	[0.041]	[0.046]	[0.053]	[0.070]		[0.049]	[0.053]	[0.077]
Jan-81	-0.007	-0.003	-0.003	-0.010	-0.017	0.033	Jan-91	0.018	0.017	0.075
	[0.032]	[0.033]	[0.036]	[0.038]	[0.045]	[0.053]		[0.051]	[0.059]	[0.076]
Jan-80	0.004	-0.013	-0.017	-0.023	-0.024	-0.073*	Jul-91	-0.002	0.034	0.045
	[0.028]	[0.031]	[0.033]	[0.036]	[0.038]	[0.042]		[0.053]	[0.058]	[0.074]
Jan-79	0.053*	0.039	0.028	0.046	0.054	0.092*	Jan-92	-0.031	-0.015	-0.060
	[0.029]	[0.030]	[0.033]	[0.036]	[0.041]	[0.046]		[0.049]	[0.056]	[0.075]
Jan-78	0.021	0.026	0.014	-0.013	-0.010	-0.046				
	[0.030]	[0.033]	[0.034]	[0.037]	[0.042]	[0.052]				
Jan-77	0.023	0.031	0.030	0.020	-0.021	0.009				
	[0.032]	[0.034]	[0.038]	[0.040]	[0.048]	[0.059]				

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence against Women. The sample is restricted to the birth cohorts unaffected by the policy in panel (A) and to the birth cohorst affected by the policy in panel (B). In both panels, we take counterfactual policy cutoffs by gradually shifting the cutoff point, as specified in columns (1) and (8). The cutoffs are chosen so as to keep at least 2 years of data on each side of the cutoff. The estimates in each column come from a separate regression using the sample defined according to the bandwidths specified in the column headings. The bandwidths in panel (B) are much narrower because the data has only 7 years on the right hand side of the cutoff. 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 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 birth-month level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table B4: Policy Effects on Ever Having a Relationship, Missing Birth-Month Information Status, and Response Quality

	Number of Years on Each Side of the Cutoff							
	10	*9*	*8*	*7*	*6*	*5*	*4*	*3*
I) Ever Having a Relationship (Running Variable: Month-Year of Birth)								
A) Total Sample								
Policy Effect	-0.020	-0.008	0.004	0.025	0.013	0.022	0.035	0.056*
	[0.019]	[0.020]	[0.021]	[0.022]	[0.023]	[0.025]	[0.027]	[0.032]
Observations	8,266	7,599	6,908	6,212	5,329	4,415	3,461	2,627
B) Rural Sample								
Policy Effect	-0.027	-0.018	-0.015	0.008	0.018	0.001	0.022	-0.004
	[0.027]	[0.028]	[0.030]	[0.031]	[0.032]	[0.035]	[0.041]	[0.048]
Observations	3,075	2,808	2,523	2,272	1,941	1,581	1,239	939
C) Urban Sample								
Policy Effect	-0.019	-0.006	0.009	0.028	0.007	0.032	0.044	0.084**
	[0.024]	[0.025]	[0.026]	[0.027]	[0.028]	[0.030]	[0.033]	[0.038]
Observations	5,191	4,791	4,385	3,940	3,388	2,834	2,222	1,688
II) Birth Month is Missing (Running Variable: Year of Birth)								
A) Total Sample								
Policy	-0.020**	-0.018*	-0.024***	-0.027***	-0.023**	-0.022**	-0.031***	-0.026**
	[0.009]	[0.010]	[0.008]	[0.008]	[0.008]	[0.010]	[0.008]	[0.010]
Wild Bootstrap p-value	0.101	0.221	0.049	0.041	0.095	0.220	0.117	0.375
Observations	6,401	5,885	5,334	4,807	4,157	3,476	2,751	2,091
B) Rural Sample								
Policy	-0.024	-0.018	-0.032	-0.037	-0.026	-0.027	-0.042	-0.035
	[0.028]	[0.030]	[0.030]	[0.030]	[0.032]	[0.037]	[0.037]	[0.041]
Wild Bootstrap p-value	0.515	0.614	0.430	0.335	0.563	0.667	0.492	0.531
Observations	2,678	2,445	2,189	1,975	1,689	1,391	1,095	840
C) Urban Sample								
Policy	-0.015**	-0.014*	-0.017**	-0.016**	-0.016**	-0.014**	-0.019**	-0.016
	[0.006]	[0.007]	[0.006]	[0.006]	[0.007]	[0.006]	[0.006]	[0.008]
Wild Bootstrap p-value	0.09	0.131	0.07	0.062	0.073	0.141	0.047	0.250
Observations	3,723	3,440	3,145	2,832	2,468	2,085	1,656	1,251
III) Response Quality Good or Very Good (Running Variable: Month-Year of Birth)								
A) Total Sample								
Policy Effect	0.010	0.009	0.006	0.017	0.032	0.045*	0.057**	0.058
	[0.019]	[0.020]	[0.021]	[0.021]	[0.024]	[0.026]	[0.028]	[0.036]
Observations	5,945	5,448	4,951	4,423	3,830	3,204	2,539	1,936
B) Rural Sample								
Policy Effect	0.008	0.012	0.018	0.013	0.037	0.060	0.078	0.115
	[0.036]	[0.038]	[0.040]	[0.042]	[0.046]	[0.051]	[0.058]	[0.072]
Observations	2,329	2,119	1,910	1,712	1,471	1,215	959	736
C) Urban Sample								
Policy Effect	0.004	0.002	-0.006	0.012	0.017	0.031	0.049	0.033
	[0.022]	[0.023]	[0.024]	[0.024]	[0.027]	[0.029]	[0.031]	[0.034]
Observations	3,616	3,329	3,041	2,711	2,359	1,989	1,580	1,200

Notes: The data come from the 2008 and 2014 Turkish National Survey on Domestic Violence against Women. 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 Panel I) and III) and year of birth in Panel II), the regressions also control for birth-month dummies in Panel 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 Panel I) and II) and clustered at the year of birth level in Panel III). Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

Table B5: Policy Effect on All Outcomes, Nonparametric Approach of CCFT and IK, 2008 and 2014 TNDSVW

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Years of Schooling	Junior High School Completion	Physical Violence	Sexual Violence	Psychological Violence	Financial Control Behavior	Women Worked Last Week or Usually has a job
				All Sample			
				A) Reduced Form-CCFT bandwidths			
Conventional	0.552*	0.068	-0.068	-0.150**	0.001	0.066	-0.016
	[0.305]	[0.043]	[0.043]	[0.067]	[0.040]	[0.077]	[0.017]
Bias-corrected	0.373	0.041	-0.072*	-0.182***	0.002	0.071	-0.027
	[0.305]	[0.043]	[0.043]	[0.067]	[0.040]	[0.077]	[0.017]
Robust	0.373	0.041	-0.072	-0.182**	0.002	0.071	-0.027
	[0.353]	[0.048]	[0.052]	[0.080]	[0.048]	[0.094]	[0.021]
BW loc. poly.	30.90	29.48	58.84	33.29	56.29	38.52	41.64
BW bias	51.52	56.50	94.28	58.91	90.10	54.63	76.05
				B) Reduced Form-IK bandwidths			
Optimal BW (b)	1.093***	0.186***	-0.055	-0.040	-0.007	0.069	0.013
	[0.237]	[0.033]	[0.037]	[0.047]	[0.039]	[0.058]	[0.016]
b/2	0.853***	0.111**	-0.066	-0.091	-0.007	0.086	0.008
	[0.306]	[0.043]	[0.048]	[0.062]	[0.049]	[0.075]	[0.018]
3b/2	1.049***	0.208***	-0.060*	-0.040	-0.009	0.060	0.003
	[0.207]	[0.029]	[0.031]	[0.039]	[0.034]	[0.052]	[0.016]
2b	0.992***	0.213***	-0.066**	-0.037	-0.019	0.041	-0.012
	[0.191]	[0.026]	[0.028]	[0.035]	[0.030]	[0.048]	[0.016]
BW loc. poly.	92.99	78.81	94.20	87	68.52	87.34	158.1
				Rural Sample			
				A) Reduced Form-CCFT bandwidths			
Conventional	0.812*	0.252***	-0.199***	-0.039	0.053	0.320***	-0.037
	[0.486]	[0.069]	[0.062]	[0.081]	[0.057]	[0.118]	[0.041]
Bias-corrected	0.630	0.226***	-0.208***	-0.058	0.061	0.378***	-0.048
	[0.486]	[0.069]	[0.062]	[0.081]	[0.057]	[0.118]	[0.041]
Robust	0.630	0.226***	-0.208***	-0.058	0.061	0.378***	-0.048
	[0.573]	[0.080]	[0.078]	[0.097]	[0.067]	[0.131]	[0.049]
BW loc. poly.	32.07	43.71	33.22	60.59	52.90	38.27	50.64
BW bias	52.13	76.17	53.96	95.36	81.53	59.77	85
				B) Reduced Form-IK bandwidths			
Optimal BW (b)	1.552***	0.331***	-0.125**	-0.015	0.031	0.204*	-0.017
	[0.394]	[0.053]	[0.054]	[0.062]	[0.057]	[0.108]	[0.034]
b/2	1.155**	0.259***	-0.137**	-0.041	0.063	0.317**	-0.034
	[0.527]	[0.073]	[0.061]	[0.083]	[0.078]	[0.128]	[0.041]
3b/2	1.665***	0.355***	-0.112**	-0.013	0.027	0.134	-0.027
	[0.351]	[0.048]	[0.049]	[0.054]	[0.049]	[0.094]	[0.031]
2b	1.717***	0.370***	-0.097**	-0.012	0.010	0.098	-0.025
	[0.326]	[0.045]	[0.046]	[0.052]	[0.045]	[0.087]	[0.030]
BW loc. poly.	95.80	101.4	104.6	108.8	70.83	73.12	120.2
				Urban Sample			
				A) Reduced Form-CCFT bandwidths			
Conventional	0.568	0.007	-0.048	-0.164**	-0.026	-0.025	0.002
	[0.358]	[0.059]	[0.069]	[0.081]	[0.047]	[0.072]	[0.025]
Bias-corrected	0.446	-0.024	-0.043	-0.203**	-0.027	-0.049	-0.010
	[0.358]	[0.059]	[0.069]	[0.081]	[0.047]	[0.072]	[0.025]
Robust	0.446	-0.024	-0.043	-0.203**	-0.027	-0.049	-0.010
	[0.427]	[0.064]	[0.085]	[0.094]	[0.059]	[0.089]	[0.029]
BW loc. poly.	32.38	27.61	42.69	31.77	52.79	40.82	47.18
BW bias	52.80	52.71	68.47	56.31	82.77	55.34	85.20
				B) Reduced Form-IK bandwidths			
Optimal BW (b)	0.758***	0.126***	-0.021	-0.045	-0.023	0.039	0.023
	[0.257]	[0.042]	[0.042]	[0.054]	[0.040]	[0.052]	[0.025]
b/2	0.747**	0.055	-0.045	-0.103	-0.030	0.009	0.009
	[0.324]	[0.057]	[0.060]	[0.074]	[0.053]	[0.061]	[0.030]
3b/2	0.615***	0.138***	-0.040	-0.053	-0.030	0.022	0.039
	[0.225]	[0.035]	[0.036]	[0.045]	[0.036]	[0.047]	[0.024]
2b	0.534**	0.134***	-0.052	-0.051	-0.036	0.003	0.033
	[0.211]	[0.030]	[0.033]	[0.040]	[0.034]	[0.045]	[0.023]
BW loc. poly.	118.4	82.42	104.2	84.40	87.98	129.8	81.39