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DISCUSSION PAPER SERIES

IZA DP No. 14001

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Pelin Akyol
Bilkent University

Murat Güray Kırdar
Boğaziçi University, American University of Beirut and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Does Education Really Cause Domestic Violence? Replication and Reappraisal of "For Better or For Worse? Education and the Prevalence of Domestic Violence in Turkey"

Using the 2008 Turkish National Survey of Domestic Violence against Women, 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". EK's findings are an artifact of the way the authors create two key variables: the variable that classifies women into rural vs. urban childhood location and the variable measuring financial control behavior. EK misclassify the variable on childhood rural status. We find that once this variable is defined properly, the evidence for all their findings vanishes. EK make use of *two of the three variables* related to financial control behavior in the dataset. We show that using all three variables—or any other combination of two of the three variables—generates no evidence of a policy effect on financial control behavior. Even after ignoring these problems, the evidence EK provide in their paper is highly specification sensitive and the standard checks of the continuity assumption in RDD fail for their key outcomes. Moreover, the results obtained from the analysis of urban areas—not provided in their paper—are inconsistent with the instrumental violence hypothesis. In addition, EK claim—using RDD graphs with high-order polynomials but no estimation results—that the policy has no effect on men's schooling, contrary to the findings of the previous literature. However, we show a clear and substantial policy effect on men's schooling, resulting in the failure of their exclusion restriction assumption.

JEL Classification: J12, J16, I25

Keywords: education, domestic violence, regression discontinuity, financial control behavior, women's employment

Corresponding author:
Murat Güray Kırdar
Department of Economics
Boğaziçi University
Bebek
Istanbul 34342
Turkey

E-mail: murat.kirdar@boun.edu.tr

1. Introduction

Economists have contributed to the investigation of the causes of domestic violence by examining the effect of better employment, income opportunities 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), public and cash transfers to women (Hidrobo and Fernald 2013; Hidrobo 2016), dowry payments (Bloch and Rao 2002; Srinivasan and Bedi 2007), 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 generates an increase in 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.

We show that EK's findings are an artifact of the way the authors created two key variables: the dependent variable measuring financial control behavior and the variable that classifies women into rural vs. urban childhood location. As we show in detail, when *either one* of these variables is defined properly, the evidence for their findings vanish. We also show that even if one uses EK's choices for these two variables, key RDD assumptions in their main findings for rural areas fail and one finds results for urban areas that conflict with the instrumental violence hypothesis. In addition, we show that the exclusion restriction assumption required in the interpretation of their results as the causal effects of women's schooling fails. We also demonstrate several other problems in EK's econometric analysis and we underline some unconventional empirical decisions they have made which produced results that are both fragile and inconsistent with the body of work that analyzed the same reform using much larger and more detailed data sets.

First, we describe how EK misclassify the survey respondents into urban vs rural childhood location (the correction of which eliminates the results). Remarkably, the fact that no information on the rural and urban status of the childhood place of residence exists in the 2008 TNSDVW is not mentioned in the paper. 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] Specifically, the authors define "district centers" as rural areas for movers. This is surprising because 96% of these "district centers" are urban areas in the 2008 TNSDVW data they employ. As a consequence, EK overestimate the true level of childhood rural areas by about 20 percentage points. In fact, EK classify the district centers of big metropolitan cities as rural areas. The municipality of Besiktas in Istanbul and the municipality of Cankaya in Ankara are two glaring examples of 100% urban areas with the highest human development index of the county, which are classified as rural by EK. Moreover, their definition of rural areas suffers from sample selection as it is more likely to include movers than stayers. We show that once childhood rural areas are defined properly—or in any way other than authors' selection, the evidence for all their findings vanishes. In addition, the coefficient magnitudes are much smaller.

Then, restricting our analysis to the EK's flawed sample of childhood rural areas, we demonstrate several substantial problems in their empirical analysis. First, we explain the problem in EK's construction of the variable for partners' financial control behavior. EK's evidence for the instrumental violence hypothesis critically hinges on finding an effect on financial control behavior. In the survey they use, three consecutive questions on financial control behavior exist. EK choose to make use of *two of these three questions*, despite the fact the third, left-out, question is the most relevant and prevalent indicator of financial control of

[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.

the spouse. We show that using all three questions—or any combination of two of the three variables other than the authors' selection—generates no evidence of a policy effect on financial control behavior. Moroever, the mangitude of the coefficient estimates are substantially smaller.

Even after ignoring these problems in the definitions of key variables, we show that the continuity assumption of RDD fail for their key outcomes. For instance, with placebo cutoff points, the continuity assumption for the financial control behavior variable fails 30 percent of the time with a 10-percent statistical significance level.

We also demonstrate major issues regarding the failure of the exclusion restriction assumption and sample selection bias. 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 which analyzed the same reform and reported the impact of the reform on educational attainment of both sexes.[2] This point is relevant because the existence of the reform's impact on men's education would violate the exclusion restriction (i.e. 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 used in EK as well. In addition, we show that the policy substantially increases the incidence of ever having a relationship for women with rural childhood residence—the only group for which EK establish evidence—resulting in a potential sample selection problem in their 2SLS estimates.

After demonstrating these major flaws in EK's analysis with their own rural sample, we examine the robustness of their findings. First, we show that even using the same empirical specification of EK, small variations in the model, such as omitting some covariates and not using the sample weights, make the results very fragile. The variables for which the results are most sensitive are the same variables that fail the tests for the continuity assumption of RDD. Second, we replicate the EK analysis for their urban sample, which is not provided in EK, and 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 (16.8% in the dataset), EK assign missing status rather than zero. In their local polynomial estimations, they first calculate an

[2] 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.

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 0.12 and 0.24 standard deviations, respectively.

A brief background information and descriptions of data and methods are provided in Section 2. In Section 3, 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 4, 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. Section 5 concludes.

2. Brief Background, Data and Methodology

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 is 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 afterwards 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.[3] Therefore, children born in 1987 and afterwards 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 (early and late school start age) was quite common. As a result, some children among the 1986 birth cohort are affected by the policy, while 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 the Ministry of National Education (MONE) of Turkey,

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

which was 15 percent in 1996 and 1997 before the policy, jumped to 37.3 percent in 1998 and remained at around 30 percent 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 around 6.75 to 7.67 million—a 13.7-percent increase—compared to the 1.8-percent increase in the preceding 3-year interval and the 0.5-percent increase in the succeeding 3-year interval. The number of students in rural areas rose from around 2.35 to 2.8 million over the same period, which is equivalent to a 20-percent increase compared to the 7-percent fall in the preceding 3-year interval and the 1.4-percent fall in the succeeding 3-year interval (Kirdar et al., 2016).

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. In other words, the bandwidth that they use in their regression is arbitrary and does not represent their specification and data.[4] Moreover, they use the same bandwidth in their sharp and fuzzy RDD.

Rather than choosing an arbitrary bandwidth as EK do, we use their specification with several alternative bandwidths, thereby assessing the robustness of their findings. 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

[4] Their approach is not local also in the sense that running a 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.

optimal bandwidths conditional on the specification used, unlike in EK.[5] However, we approach the results of local polynomial methods with caution *in this context and with the size of the data in hand*. The fuzziness in the treatment status of the 1986 and 1987 birth cohorts 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. Hence, we view the results of local polynomial approaches —in this context—only as supporting evidence.

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, dummies for birth months, and a dummy for rural childhood residence. Standard errors are clustered at the month-year of birth as in EK.

While EK's estimation results utilize a number of covariates and sampling weights, their RDD graphs use neither these covariates nor sampling weights. Hence, some of their estimation results do not match their graphs. In these cases (e.g. financial control behavior) in the paper, the graphs are either omitted or given for the combined rural/urban sample. Another surprising feature of the tables in the EK paper is that they do not report that they use sampling weights. It would be important to check the robustness of the findings without these weights, which we do.

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, 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 0.12 and 0.24 standard deviations, respectively, as they refer to a z-score.

3. Flawed Definition of Rural Areas during Childhood

We start this section reminding the reader that the definition of rural and urban status of childhood place of residence is critical in the EK study because the evidence provided in EK

[5] 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.

holds only for women who lived in rural areas during childhood. We also remind the reader that rural areas are defined as locations with a population below 10,000 in the dataset used in the study. Below, we provide brief background information on the administrative units of Turkey, in particular with regard to province center/district center/village status of locations.[6]

3.1 Brief Background Information

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.[7] Some provinces, however, do not have province centers. These provinces contain the major cities of the country such as Istanbul, Ankara, and Izmir. For instance, 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.

According to the 2008 TNSDVW, most district centers are urban areas. Using the information on the type of the current location of residence in the form of province center/ district center/ village and the status of the current location of residence as rural vs. urban area, we can calculate the urban and rural fractions of different types of locations. According to this, while 100% of province centers and 96.0% of district centers are urban areas, only 20% of villages are classified as urban areas. In other words, all province centers and the big majority of district centers have a population above 10,000 whereas only 20% of villages do. Villages that are classified as urban areas are generally in the peripheries of large cities. District centers that are classified as rural areas are small district centers with a population below 10,000.

3.2 Analysis with a Proper Definition of Rural Areas, as well as Alternative Ones

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*

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[6] The last category is precisely "subdistrict or village". For ease of reference, we call this only "village" throughout the paper.

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

rural residence, as the age for starting junior high school in Turkey is 12 years old.” Remarkably, the fact that no information on the rural and urban status of the childhood place of residence exists in the 2008 TNSDVW is not mentioned in the paper. In fact, EK combine information from two separate questions to generate their “childhood rural areas” definition. For women who moved to a different location after age 12, a question elicits their childhood location of residence in the form “village/district center/province center”.[8] For women who have been living in the same location since age 12, 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 “villages and district centers” as rural areas whereas they take “rural identifier” available in the dataset for stayers. In fact, 68 percent of the women in the data are movers.[9]

However, taking “district centers” as rural areas is tremendously problematic because, as discussed above, 96% of district centers in the survey are urban areas. In fact, district centers include parts of major cities such as Istanbul, Ankara, and Izmir. According to the 2008 TNSDVW, of the 15- to 49-year-old women in 2008, 1.6% in Istanbul, 3.0% in Ankara, and 19.6% in Izmir provinces live in rural areas. However, all of these women either live in district centers or in villages (as no province center exists in these provinces). Therefore, when EK assign district centers as rural areas for movers, *they are assigning the residents of the major urban areas of the country to rural areas.* In fact, a woman who lived in Besiktas/Istanbul (the district with the highest development index in Turkey) during childhood but now lives in a different location is classified as living in a rural area during childhood with the EK definition of rural areas during childhood.

In fact, with the 2008 TNSDVW survey, it is possible to generate a proper and consistent definition of rural areas. While, as mentioned above, childhood rural/urban status is not available in the survey, it is possible to generate a village/district center/province center status for the current place of residence. Although this information 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

[8] 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?”

[9] 59 percent of the women in the sample with 10-year intervals around the cutoff and 57.5 percent of the women in the sample with 5-year intervals around the cutoff are movers.

and stayers. Using this we define rural areas as villages only and call this proper and consistent definition in our analysis.[10]

According to the definition of EK, 59.8 percent of the women in the 2008 TNSDWW have rural childhood region of residence. However, we calculate that only 40.8 percent of women in the TNSDVW live in villages during childhood.[11] This fraction of women living in villages during childhood that we calculate is in accordance with the numbers in the Turkish Demographic and Health Surveys.[12] Moreover, in the 2008 TNSDVW, the percentage of women living in villages (24.6%) is slightly higher than the fraction living in rural areas (22.0%). This means that the fraction of women living in villages during childhood that we calculate as 40.8 percent above is an upper bound for the fraction of women with a rural childhood residence. Hence, by taking the fraction of women living in rural areas during childhood as 59.8 percent, EK overestimate this fraction *at least by 19 percentage points.*[13]

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 (which we use) and of villages and district centers together (which EK use) in Table 1. 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 and to 60-month bandwidths around the cutoff, as in EK, we see that the number 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 1 are the same as those in panels (A)-(C) of Table 1 of EK.

Table 1 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

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[10] With regard to the above example on major cities, our definition of villages as rural areas implies that of the 15- to 49-year-old women in 2008, 9.0% in Istanbul, 7.8% in Ankara, and 25.3% in Izmir are living in rural areas.

[11] When we take the 5-year intervals around the cutoff as in most analyses in EK, 51.8 percent of the women in their sample lived in rural areas during childhood, whereas this fraction is only 32.9 percent with our definition.

[12] In fact, of women aged 15–49, 46 percent lived in villages during childhood according to the 2008 TDHS and 36.7 percent according to the 2013 TDHS.

[13] According to Turkish censuses, the fraction of women living in rural areas was 48.6 percent in 1985 and 35.5 percent in 2000. The women in the EK sample are born in the 1980s and 1990s.

mean 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 in EK's rural sample, movers are more educated than stayers and their sample is more likely to include movers by construction.[14] 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.

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 2, with various bandwidths from 10 to 3 years on each side of the cutoff. Note that the arbitrarily selected bandwidths of EK lie within the range of our bandwidths. Table 2 has three 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 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.[15]

As can be seen in panel (I), our findings are consistent with those in EK when we use their definition.[16] However, the picture completely changes when we move into the panel (II)—with a proper definition of rural areas. For the psychological violence and employment variables, statistical significance vanishes for all bandwidths. Furthermore, the magnitude of the

[14] 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.

[15] Although this definition is also improper for rural areas as in EK definition, it is still better than that of EK as it at least uses a consistent definition across movers and stayers.

[16] 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 percent level. Our estimate with a bandwidth of 72 months is 0.228, which is also statistically significant at the 10 percent 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.

coefficients is much closer to 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 4.2 and Section 4.5.

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.[17]

Next, for the same four different definitions of rural areas during childhood, Table A1 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 triangle kernel and accounting for covariates and sampling weights. As discussed earlier, this approach is not right in the sense that 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).[18]

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

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

[18] This is different from that in EK who use the reduced form bandwidths also for fuzzy RDD.

evidence for the policy effect on women's employment and hence their evidence for instrumental violence hypothesis vanish. 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 vanishes once appropriate definitions of rural areas are used—as in Table 2. The CCFT method results 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 2. 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.

4. Replications with their Flawed Rural Sample

In the rest of this paper, our analysis uses the exact EK data and lists the several serious problems in their empirical analysis with their data of choice.

4.1 Cherry Picking to Find an Effect on Financial Control Behavior

In the questionnaire for women in the 2008 TNSDVW, there are three questions on financial control behavior: questions 502-A, 502-B, 502-C. The first one asks the respondent whether she was ever prevented from working or was forced to quit her job because of her partner. This is the most common type of financial violence experienced by women in the whole sample and EK sample; in fact, 31 percent of the women experience this type of violence. The second one elicits whether the woman was ever denied money for household needs by her partner despite the fact that the partner had sufficient money for other expenses, and only 6.5 percent of the women experience this type of violence. The third one asks the woman if the partner ever took her income despite her will, 4 percent of the women experience this. Surprisingly, EK choose to use only the latter two questions in their definition of financial control behavior, but not the first one. In this section, we investigate the robustness of their results for alternative definitions of financial control behavior definition.

Figure 1 shows RDD graphs for EK's definition of financial control behavior in panel (A) and for the complete definition of financial control behavior—which includes all three items— in panel (B). Both panels give the plots separately for EK's rural and urban areas. Our RDD graphs adjust for covariates and sampling weights as in the regressions of EK. Panel (A) suggests a positive effect of the policy on financial control behavior with EK's definition of rural areas. However, surprisingly, the graph for urban areas shows an equally big but a negative effect of the policy on financial control behavior. (EK do not present the graph for urban areas in their paper.) In panel (B), on the other hand, the negative policy effect on financial control behavior in urban areas completely vanishes and the positive effect in rural areas becomes much smaller—when we use all three questions on financial control behavior in the survey.

Table 3 provides RDD estimation results on financial control behavior for rural areas and for urban areas, separately, using alternative definitions. While Panel (I) gives the results with the EK definition, the definition in panel (II) combines questions 502-A with 502-B, the definition in panel (III) combines questions 502-A with 502-C, and the definition in panel (IV) uses all three items (as is apt). We gradually zoom in around the cutoff starting with 10 years on each side of the cutoff and narrowing down to 2 years, with linear polynomials on each side.[19] Table 3 shows a policy effect on financial control behavior for rural areas with EK's arbitrary definition—in fact, a large effect that is around 0.2 to 0.3 standard deviations.[20] However, when we take the proper definition of financial control behavior in panel (IV), the policy effect reduces substantially. Moreover, it becomes statistically insignificant for all bandwidths of 7 years or lower on each side of the cutoff and remains only marginally significant for very wide bandwidths. In addition, no evidence of a policy effect exists in panels (II) and (III) with the other possible combinations of two items out of three.

For financial control behavior, EK take a bandwidth of 71 months in their paper. The coefficient estimate in Table 3 with a bandwidth of 72 months is 0.232 (which is 0.235 in EK's paper with 71 months) when we use EK's definition of financial control behavior, whereas it is 0.135 and not statistically significant at the conventional levels when we take the proper definition of financial control behavior. Furthermore, the coefficient estimate for financial control behavior

[19] Here, we cannot provide results using the nonparametric CCFT method due to convergence problems. However, Table 3 provides results with very narrow bandwidths.

[20] The policy effect in urban areas, for bandwidths of 6 years and lower, are negative and large in magnitude—as Figure 1 suggests—but not statistically significant at the conventional levels.

is even lower in panel (II), at 0.035, and in panel (III), at 0.056, with the other two combinations of two items out of three. The patterns are similar for other bandwidths.

4.2 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 all 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 not 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 4 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 A2 in the Appendix.

In Table 4 and Table A2, we have 54 estimates for sample (A) (6 bandwidths for 9 alternative cutoffs). For psychological violence in panel (A) of Table A2, of the 54 estimates, 5 yield a statistically significant result at least at the 10 percent level, which is expected as 5/54 is less than 10 percent. However, for financial control behavior in panel (A) of Table 4, 16 of the 54 estimates yield statistically significant results—which is roughly 30 percent. 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 4 shows that for the employment variable, 12 of the 54 estimates—more than 22 percent—yields statistically significant results, 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.[21]

Panel (B) of Table 4 indicates similar results. For the financial control behavior variable, 5 of the 21 estimates (24 percent) and for the employment variable 9 of the 21 estimates (43 percent) are statistically significant at least at the 10 percent level. We would put less emphasis on panel (B), though, as the bandwidths are narrower than what EK take.

4.3 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,[22] 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* 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.

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[21] 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 percent) for the financial control behavior variable and 8 of the 27 estimates for the employment variable (30 percent) yield statistically significant results. In essence, the odds become even lower in this case.

[22] In this dataset, the information on month-of-birth information 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.

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 alternative bandwidths. In addition to the 60-month bandwidths on each side which EK use, we also take 72-month bandwidths and 48-month 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). All panels indicate clear jumps at the cutoff for both men and women. Moreover, the 95-percent 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 percent before the policy—which is actually around 80 percent as can be seen in Figure 2. It is actually below 80 percent when the 1986 birth cohort, which includes many treated individuals, is omitted.

An important feature of the data that is not discussed at all in EK is the fuzziness of the treatment status of the 1986 and 1987 birth cohorts—discussed at length 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.[23] 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 bigger for both men and women. In the EK study, however, fitting high-order polynomials in a relatively narrow bandwidth where fuzziness exists around the cutoff hides the policy effect because the high-order polynomials capture the policy effect due to the fuzziness.

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

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

month-of-birth information is not available for men.[24] 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-percent confidence intervals do not overlap in the graph for men.

Next, we discuss our estimation results. Table 5 shows the results of parametric RDD with the 2014 THLFS that takes various bandwidths from 2 to 10 years.[25] While panel (1) in Table 5 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 percentage points for men—this is the effect that EK claim not to exist—and by 15 percentage points for women.

It is also interesting to observe how the results change as we narrow the bandwidth gradually in panel (1) of Table 5. 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 fuzziness in the treatment status of the 1986 and 1987 birth cohorts. As the bandwidth gets narrow, the fuzziness increases the slopes of the time trends and decreases the policy effect. In line with these observations, panel (2) of Table 5 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 percentage points and of women by 19.8 percentage points.

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 (Cattaneo et al., 2017). As can be seen from the results in Table A3 in the Appendix, the evidence of a policy effect persists up to 5th degree polynomials for both men and women.

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[24] 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.

[25] 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.

Next, we use the local polynomial approach of CCFT. The results are given in Table A4 in the Appendix. The estimated effect is about 3-4 percentage points for men and about 8 percentage points for women. These magnitudes are consistent with those in Table 5 that use similarly narrow bandwidths. Although we do not think that it is apt to use such narrow bandwidths for sharp RDD in this context—where the treatment variable is quite fuzzy around the cutoff, it is reassuring that even these results indicate a clear policy effect on men's schooling.

In Table 6, 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.[26] 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 A5 in the Appendix, 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).[27] 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.[28]

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[26] 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.

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

[28] 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

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.[29] Although this policy has a substantial effect on men's schooling, which is almost as large as that for women (almost about a year), EK try to establish "null policy effect on men's schooling" as an "alternative fact" in the literature. 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 allow us to zoom in around the cutoff more.[30] Since the number of clusters (year-of-birth groups) is relatively small, we use the Wild-cluster bootstrap. The results are given in Table 7, 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

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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 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 percent for women vs. 70 percent 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.

[29] 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.

30 EK could use this data; however, they prefer to use only the 2014 THLFS because it has month-of-birth information, albeit with serious shortcomings. First, month-of-birth information in the 2014 HLFS is missing for 12.6 percent of the observations; and of the remaining observations, 19.2 percent are born in January. Moreover, while 24.3 percent of individuals born in January have no school degree, this percentage is 12.3 for those born in December.

regardless of the use of a donut-hole.[31] 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 percentage points, the increase for men is about 15 percentage points. 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 2013 waves of TDHS, given in Table A6 in the Appendix, are very similar to our results based on the THLFS.

4.4 Selection Bias – Policy Effect on Ever Having a Relationship in EK's Rural Areas

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 show that the policy did not have an effect on ever-had-a-relationship status. *Remarkably, they do not present this analysis for rural areas*—although their key findings are only for rural areas.

In Figure 4, we provide RDD graphs on the policy effect on ever having a relationship. Unlike EK, we do it for the rural sample of EK, as well as the total sample, and we adjust for covariates and sampling weights—as in their regressions. We provide the RDD graphs for alternative bandwidths, in addition to EK's choice of a 60-month bandwidth on either side. Figure 4, regardless of the bandwidth, indicates a jump in the ever-had-a-relationship status for rural areas, but not for all areas.

Next, we present the estimates on the policy effect on the ever-had-a relationship status using both global (parametric) and local polynomial (nonparametric) approaches. In the parametric approach, given in Table 8, 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 and for the full sample—using the EK specification

[31] 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.

with covariates and sampling weights. Table 8 clearly shows that the policy increases the ever-had-a-relationship status in rural areas by about 7–9 percentage points, whereas no such evidence exists for the full sample.

In Table A7 of the Appendix, we present nonparametric RDD results using the CCFT optimal bandwidths—only as complementary evidence, though. Despite the narrow bandwidths chosen by the CCFT method, the results indicate a positive policy effect on the ever-had-a-relationship status for rural areas and for the total sample.[32] At the same time, the coefficients are much larger in rural areas—in accordance with those in Table 8. The results in Table A7 for both the rural sample and the total sample, where the bandwidths range from 16 to 30 months on either side of the cutoff are actually in line with the results in the final column of Table 8 where 24-month bandwidths are taken.[33]

In essence, both the nonparametric and parametric results—regardless of the bandwidth—indicate a clear policy effect on ever having a relationship for the rural sample. On the other hand, the same evidence for the total sample vanishes for bandwidths wider than 36 months. The effect and its magnitude for the rural sample indicate a potentially important sample selection bias to the degree that women who are pushed to have a relationship with the policy are different from the sample of women who already have a relationship in terms of their propensity to face domestic violence, which is highly plausible.

4.5 Robustness of EK's Findings Ignoring the Major Flaws in their Analysis

In this subsection, we show that even if we ignore the major flaws in EK's analysis—discussed above—and use the empirical model as specified by the authors, their results are sensitive to small variations in model specification. In the replications here, we conduct the analysis for urban areas in the EK sample, as well as the rural areas—which provides us important clues about the robustness of their findings.

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[32] Although parametric results with bandwidths ranging from 3 to 10 years indicate no policy effect in all areas, parametric results with 2-year bandwidths and nonparametric results, which use bandwidths less than 2 years, find a policy effect. This highlights the potential danger of relying only on nonparametric results with this data.

[33] One difference is that the parametric results in Table 8 essentially uses a uniform bandwidth, whereas the triangular bandwidth in Table A7 puts more emphasis on points around the cutoff.

4.5.1 Highly Sensitive Results to the Use of Covariates and Sampling Weights

Figure 5 shows the RDD graphs for the three key variables using bandwidths of 60 months on each side as in EK. Panel (I) in Figure 5 does not adjust for covariates and sampling weights whereas panel (II) does. Both panels suggest that psychological violence increases with the policy in rural areas, as EK claim. On the other hand, the evidence of a policy effect on financial control behavior in rural areas, which EK find, is much stronger when covariates and sampling weights are used. In the paper, a RDD graph for the financial control behavior variable is not presented—although it is given for the other violence variables (Figure 6 in their text). Essentially, their RDD graph, which does not adjust for covariates and sampling weights, would indicate a smaller policy effect on financial control behavior than what their estimates reveal.

Examining the policy effect on financial control behavior and employment together in Figure 5—in light of the instrumental violence hypothesis— we see that while the graph in panel (I) indicates a positive policy effect on employment in rural areas, it does not indicate a positive policy effect on financial control behavior. Only after controlling for covariates and sampling weights, as shown in panel (II), evidence for the instrumental violence hypothesis emerges.

Next, we present our estimation results. Table 9 displays the policy effect on the three key variables of interest. Unlike EK, we provide the results for urban areas, as well as the rural areas and all areas. In panel (I), we use the same specification that EK use whereas we exclude the covariates and sampling weights in panel (II). The results are given for alternative bandwidths ranging from 3 to 7 years, centering on the 60-month bandwidth that EK use in their graphs.

Regarding psychological violence, panel (A) of Table 9 indicates a policy effect in rural areas, as suggested by Figure 5. This is in line with the findings of EK. However, the evidence for the policy effect on financial control behavior, claimed by EK, is much weaker as can be seen in panel (B).[34] In addition, when additional covariates and sampling weights are not used, no statistical evidence of a policy effect on financial violence in rural areas exists for any bandwidth—which is in line with Figure 5.

Finally, panel (C) of Table 9 shows the policy effect on women's employment status. With the EK specification, statistical evidence of a positive policy effect on employment in rural areas exists for three of the five bandwidths. For urban areas, although statistical evidence does not

[34] Only three of the five bandwidths reveal statistically significant evidence and two of these three are at the 10 percent level.

exist, the policy effect on employment is positive and large in magnitude except for that with the narrowest bandwidth. These patterns are similar when sampling weights and additional covariates are not used.

In essence, these results only partially confirm EK's findings—even after ignoring the major flaws in their analysis. With their sample, we find that the policy has a positive effect on psychological violence in rural areas. However, we also show that the evidence EK provide on the policy effect on financial violence in rural areas is very tenuous. It holds only for certain bandwidths and only at the 10-percent statistical significance level and it vanishes once covariates and sampling weights are not used. Their results on women's employment are similarly tenuous, though not as much as that for financial control behavior. The sensitivity of the findings to the specification for the financial control behavior variable, and for the employment outcome to a lesser degree, is consistent with the findings in Section 4.2 on the failure of the continuity assumption for these variables.

4.5.2 Evidence for Urban Areas that Contradicts the Instrumental Violence Hypothesis

Figure 5 shows another surprising pattern: the RDD graphs for employment and financial control behavior in urban areas indicate evidence that contradicts the instrumental violence hypothesis. In both panels, regardless of the use of covariates and sampling weights, while the policy increases employment in urban areas, it decreases financial control behavior.[35]

As can be seen in panel (B) of Table 9, the policy effect on financial control behavior in urban areas is always negative and large in magnitude and statistically significant in one of the five bandwidths with their specification. More importantly, it turns statistically significant for four of the five bandwidths when sampling weights and their choice of covariates are not used. In fact, this evidence for a negative effect on financial violence in urban areas is as strong as the evidence for a positive effect on financial violence in rural areas over the two panels. Moreover, the policy has either a small positive or no effect on employment in urban areas. Therefore, EK's argument that—according to the instrumental theories of violence—a change in financial violence is brought about by a change in household income (through a higher probability of

[35] Another interesting feature in panel (I) of Figure 5 is that the graphs on financial control behavior and employment variables illustrate a high frequency of the lowest z-values (basically zeros before taking the z-transformation), which is particularly strong for the variable for financial control behavior. We cannot see this in panel (II) as it plots the residuals after a regression on covariates.

employment) is clearly contradicted by the evidence in urban areas. While we find evidence for the policy reducing financial control behavior in urban areas, as much as the rise it causes in rural areas, the policy has no effect on women's employment in urban areas.

4.5.3 Sensitivity to Exclusion of Observations with Missing Month of Birth

EK assign treatment status according to the month and year of birth of an individual. However, in the 2008 TNSDVW data set, the month-of-birth information is missing for 15.4 percent of the total sample and for 20.4 percent of the rural sample, which is not mentioned in their paper. In addition, the individuals for whom this information is missing are different in important ways. While individuals with missing month-of-birth information have on average 2.3 years of schooling, individuals with complete month-of-birth information have 6.7 years of schooling. Therefore, in a robustness check, we randomly assign birth months for individuals with missing information. In doing so, we account for the differences in the distribution of birth months by educational attainment as less-educated individuals are more likely to report to be born in January.[36]

In Table 10, we present the results for psychological violence, financial control behavior (with EK definition) and employment outcomes. For comparison purposes, we also provide the estimates with the EK sample—which drop nonrandom observations as discussed above. For psychological violence and employment variables, the estimated coefficients with the full sample are, luckily, only slightly smaller. However, for the financial control variable of EK, the estimated coefficients are more than 20 percent smaller for all bandwidths. This finding provides further clues as to why the EK's estimated effects on financial control behavior are large.

4.5.4 Correction of the Mistake in the Construction of Schooling Variable

EK make a mistake in cleaning the data on years of schooling. In the survey, a question first elicits whether the respondent 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

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[36] We first calculate the distribution of months for each years-of-schooling level. Then, we take a weighted average of these distributions where weights are the fractions of each year-of-schooling level among the individuals with missing month-of-birth information.

Appendix presents the results for the policy effect on years of schooling as defined in EK and with our correction. The mistake of EK results in slightly smaller coefficient estimates for the policy effect on years of schooling. Hence, luckily, this is not likely to pose a substantial problem in the 2SLS estimates of EK.

5. Conclusion

In their paper published in AEJ: Applied, Erten and Keskin (EK) made a number of unsavory decisions when creating their key variables and analyzing the data. Correcting any single one of these eliminates their results and obliviates any evidence for the instrumental violence hypothesis promoted by their paper. For example, childhood urban-rural residence of survey respondents is grossly misclassified and the evidence for their findings vanish once it is corrected. Similarly, EK seem to have cherry-picked the particular combination of three available variables to create their measure of financial control behavior. Specifically, they chose two of the three questions posed to survey respondents to define financial control, leaving out the most relevant question. We show that using all three questions or any other combination of two of the three eliminates their results, even if one uses their specification and all other variables the way EK created.

In addition, they fail to recognize the substantial policy effect on partners' education level—which causes the exclusion restriction to fail—and 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. Even after all these choices, the evidence they provide in their paper is highly sensitive to the use of covariates and sampling weights, and the standard checks of the continuity assumption of RDD fail for their critical variables. Moreover, the evidence for urban areas—which is not provided in their paper—is inconsistent with the key theory for which they are trying to find evidence for.

The flawed definition of rural areas and the arbitrary selection of financial control variables in the paper could not have been noticed by the readers who are not familiar with the data set or the context because these issues are never mentioned by the authors anywhere in their paper. There are additional, more subtle but consequential empirical problems. For example, 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, that is mostly not cited in the paper—they present no estimation results and instead display misleading graphs in support of their claim. Second, a test of the sample selection problem in rural areas—the only region for which they find an effect is not available in the paper. However, the policy significantly increases the fraction of women who have ever had a relationship in their rural sample. Third, no results for urban areas are provided in the paper; in fact, doing so would provide evidence that would contradict the instrumental violence hypothesis. Fourth, the authors do not conduct checks of the continuity assumption under placebo cutoffs—which has become standard in the RDD literature. Fifth, in RDD estimation, the authors use optimal bandwidths that do not match their specification and they use the same bandwidths for sharp and fuzzy RDDs. Finally, the interpretation of their results on the policy effect on domestic violence variables as percentage-point changes is not correct because domestic violence variables are z-scores. Hence, they must be interpreted as standard-deviation changes.

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Table 1: 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 2: 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 3: Policy Effect on Financial Control Behavior with a Proper Definition

	Number of Years on Each Side of the Cutoff								
	10	9	8	7	6	5	4	3	2
I) EK's Definition of Financial Control Behavior (Refused to give her money and Took her money)									
A) Rural Sample									
Policy Effect	0.214*	0.241**	0.250**	0.252**	0.232*	0.192	0.263*	0.254	0.340
	[0.120]	[0.117]	[0.115]	[0.115]	[0.120]	[0.131]	[0.157]	[0.169]	[0.230]
Observations	2,138	1,922	1,728	1,530	1,313	1,090	867	653	432
B) Urban Sample									
Policy Effect	-0.071	-0.042	-0.022	0.007	-0.075	-0.144*	-0.143	-0.190	-0.177
	[0.086]	[0.090]	[0.094]	[0.101]	[0.094]	[0.085]	[0.097]	[0.142]	[0.137]
Observations	1,569	1,442	1,297	1,143	991	839	676	517	359
II) Prevent her from working and Refused to give her money									
A) Rural Sample									
Policy Effect	0.122	0.112	0.102	0.079	0.035	-0.007	0.036	-0.041	0.089
	(0.105)	(0.106)	(0.109)	(0.111)	(0.117)	(0.129)	(0.160)	(0.184)	(0.236)
Observations	2,189	1,972	1,778	1,581	1,361	1,127	900	680	454
B) Urban Sample									
Policy Effect	0.020	0.036	0.078	0.113	0.032	0.041	0.027	0.123	0.129
	(0.105)	(0.109)	(0.113)	(0.125)	(0.129)	(0.137)	(0.160)	(0.194)	(0.258)
Observations	1,646	1,518	1,373	1,218	1,064	900	731	553	382
III) Prevent her from working and Took her money									
A) Rural Sample									
Policy Effect	0.198	0.184	0.163	0.128	0.056	0.006	0.046	-0.062	-0.063
	(0.128)	(0.126)	(0.127)	(0.128)	(0.134)	(0.145)	(0.182)	(0.211)	(0.269)
Observations	1,986	1,788	1,612	1,430	1,225	1,013	806	603	406
B) Urban Sample									
Policy Effect	0.009	0.025	0.041	0.060	0.002	0.031	0.002	0.023	0.068
	(0.098)	(0.102)	(0.104)	(0.111)	(0.118)	(0.126)	(0.143)	(0.168)	(0.199)
Observations	1,542	1,426	1,289	1,149	1,007	856	688	520	357
IV) Full Definition of Financial Control Behavior (all three elements)									
A) Rural Sample									
Policy Effect	0.200*	0.202*	0.198*	0.181	0.135	0.089	0.143	0.065	0.168
	[0.111]	[0.109]	[0.109]	[0.110]	[0.115]	[0.126]	[0.157]	[0.175]	[0.227]
Observations	2,202	1,985	1,790	1,592	1,371	1,137	906	685	455
B) Urban Sample									
Policy Effect	0.001	0.022	0.048	0.078	0.002	-0.005	-0.032	-0.007	0.031
	[0.083]	[0.087]	[0.090]	[0.099]	[0.100]	[0.102]	[0.119]	[0.145]	[0.169]
Observations	1,658	1,530	1,385	1,230	1,075	911	738	558	387

Notes: Here, we use the EK 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 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 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 4: 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 5: 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 6: 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 7: 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 8: Policy Effect on Ever Having a Relationship – Global to Local Approach

	Number of Years on Each Side of the Cutoff								
	10	9	8	7	6	5	4	3	2
A) EK Rural Sample									
Policy Effect	0.065**	0.073**	0.077**	0.089**	0.084**	0.071*	0.114**	0.068	0.146**
	[0.032]	[0.033]	[0.034]	[0.036]	[0.037]	[0.041]	[0.048]	[0.058]	[0.068]
Observations	2,621	2,399	2,197	1,990	1,711	1,405	1,100	817	540
B) EK Urban Sample									
Policy Effect	-0.014	-0.012	-0.013	-0.008	-0.050	0.013	-0.014	0.068	0.107
	[0.044]	[0.045]	[0.045]	[0.046]	[0.047]	[0.050]	[0.054]	[0.062]	[0.066]
Observations	2,287	2,157	2,007	1,843	1,583	1,303	1,016	770	517
C) Total Sample									
Policy Effect	0.021	0.026	0.028	0.039	0.018	0.038	0.048	0.064	0.112**
	[0.029]	[0.030]	[0.030]	[0.031]	[0.031]	[0.034]	[0.036]	[0.043]	[0.048]
Observations	4,908	4,556	4,204	3,833	3,294	2,708	2,116	1,587	1,057

Notes: Here, we use the EK 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 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 9: Replication of EK – with and without covariates and sampling weights

	Panel I - Covariates and Sampling Weights as in EK					*Panel II - No Covariates and Sampling Weights*				
	Number of Years on Each Side of the Cutoff					Number of Years on Each Side of the Cutoff				
	7	6	5	4	3	7	6	5	4	3
A) Psychological Violence										
Rural Sample	0.129**	0.137**	0.152**	0.179**	0.135*	0.116**	0.126**	0.133**	0.149**	0.096
	[0.055]	[0.060]	[0.063]	[0.069]	[0.077]	[0.056]	[0.058]	[0.063]	[0.067]	[0.073]
Observations	1,642	1,417	1,176	931	704	1,646	1,419	1,178	932	705
Urban Sample	-0.058	-0.062	-0.057	-0.041	-0.083	0.000	-0.004	0.031	0.096	0.113
	[0.076]	[0.078]	[0.082]	[0.092]	[0.121]	[0.064]	[0.068]	[0.073]	[0.078]	[0.094]
Observations	1,332	1,160	974	780	589	1,339	1,167	979	784	593
Total Sample	0.038	0.044	0.054	0.078	0.049	0.064	0.067	0.087*	0.124**	0.099*
	[0.044]	[0.047]	[0.050]	[0.056]	[0.068]	[0.040]	[0.042]	[0.047]	[0.051]	[0.056]
Observations	2,974	2,577	2,150	1,711	1,293	3,002	2,601	2,171	1,727	1,306
B) Financial Control Behavior										
Rural Sample	0.252**	0.232*	0.192	0.263*	0.254	0.182	0.146	0.124	0.166	0.213
	[0.115]	[0.120]	[0.131]	[0.157]	[0.169]	[0.125]	[0.130]	[0.144]	[0.172]	[0.197]
Observations	1,530	1,313	1,090	867	653	1,533	1,315	1,092	868	654
Urban Sample	0.007	-0.075	-0.144*	-0.143	-0.190	-0.087	-0.168*	-0.193**	-0.217**	-0.249*
	[0.101]	[0.094]	[0.085]	[0.097]	[0.142]	[0.090]	[0.091]	[0.097]	[0.109]	[0.135]
Observations	1,142	991	839	676	517	1,149	998	844	680	521
Total Sample	0.139*	0.100	0.047	0.082	0.069	0.061	0.006	-0.018	-0.005	0.007
	[0.082]	[0.084]	[0.087]	[0.101]	[0.117]	[0.073]	[0.076]	[0.084]	[0.097]	[0.115]
Observations	2,672	2,304	1,929	1,543	1,170	2,698	2,327	1,949	1,558	1,182
C) Employment										
Rural Sample	0.085***	0.076**	0.074**	0.037	0.036	0.073**	0.071**	0.068*	0.027	0.030
	[0.031]	[0.032]	[0.036]	[0.034]	[0.038]	[0.031]	[0.033]	[0.037]	[0.040]	[0.044]
Observations	1,645	1,420	1,179	934	707	1,649	1,422	1,181	935	708
Urban Sample	0.047	0.060	0.046	0.073	0.040	0.049	0.046	0.040	0.033	0.021
	[0.051]	[0.056]	[0.063]	[0.069]	[0.076]	[0.035]	[0.037]	[0.040]	[0.044]	[0.049]
Observations	1,341	1,169	981	786	594	1,348	1,176	986	790	598
Total Sample	0.063**	0.065**	0.056*	0.039	0.007	0.065***	0.062**	0.057**	0.032	0.026
	[0.026]	[0.028]	[0.032]	[0.033]	[0.036]	[0.024]	[0.024]	[0.027]	[0.029]	[0.032]
Observations	2,986	2,589	2,160	1,720	1,301	3,014	2,613	2,181	1,736	1,314

Notes: The data comes 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, 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 in panel (I) 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 in panel (1), as in Erten and Keskin, but not in panel (2). 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.

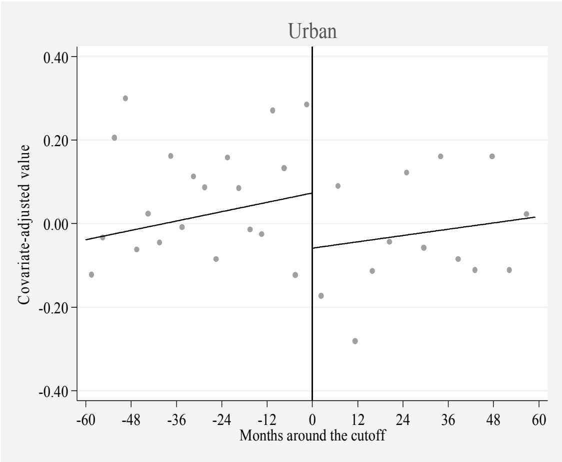
Table 10: Policy Effect on Key Variables of Interest with Full Data (Randomized Month of Birth conditional on Years of Schooling for Missing Values)

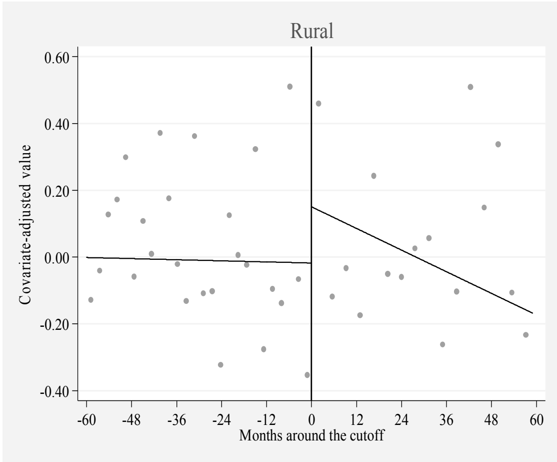
		Number of Years on Each Side of the Cutoff							
		10	9	8	7	6	5	4	3
A) Psychological Violence									
EK Sample		0.093*	0.106**	0.117**	0.128**	0.137**	0.151**	0.178**	0.135*
		[0.052]	[0.052]	[0.053]	[0.055]	[0.060]	[0.063]	[0.068]	[0.077]
	No obs.	2,253	2,036	1,840	1,642	1,417	1,176	931	704
Full Sample		0.089*	0.100*	0.106**	0.118**	0.131**	0.143**	0.161**	0.124*
		[0.050]	[0.051]	[0.051]	[0.053]	[0.057]	[0.061]	[0.066]	[0.072]
	No obs.	2,544	2,302	2,056	1,831	1,578	1,310	1,035	777
B) Financial Control Behavior									
EK Sample		0.212*	0.238**	0.247**	0.249**	0.230*	0.190	0.260*	0.252
		[0.119]	[0.116]	[0.114]	[0.113]	[0.118]	[0.129]	[0.155]	[0.167]
	No obs.	2,138	1,922	1,728	1,530	1,313	1,090	867	653
Full Sample		0.167	0.188*	0.198*	0.198**	0.181*	0.144	0.195	0.186
		[0.104]	[0.103]	[0.101]	[0.100]	[0.104]	[0.117]	[0.137]	[0.146]
	No obs.	2,408	2,167	1,925	1,700	1,456	1,208	957	713
C) Employment									
EK Sample		0.044	0.062**	0.071**	0.085***	0.076**	0.074**	0.037	0.036
		[0.031]	[0.031]	[0.031]	[0.031]	[0.032]	[0.036]	[0.034]	[0.038]
	No obs.	2,256	2,039	1,843	1,645	1,420	1,179	934	707
Full Sample		0.041	0.056*	0.069**	0.080***	0.070**	0.068**	0.038	0.041
		[0.029]	[0.029]	[0.029]	[0.029]	[0.030]	[0.034]	[0.032]	[0.035]
	No obs.	2,548	2,306	2,060	1,835	1,581	1,313	1,038	780

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. The running variables are given in panel 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, a dummy for whether the interview language was Turkish, a dummy for whether the respondent lives in a rural area and dummies for 26 NUTS-2 region of residence during childhood. In the regressions with the full sample, the month-of-birth dummies also include a dummy for missing month of birth. The regressions are weighed using the sample weights, as in EK. Standard errors are clustered at the year of birth level. Statistical significance *** at the 1 percent level, ** at the 5 percent level, * at the 10 percent level.

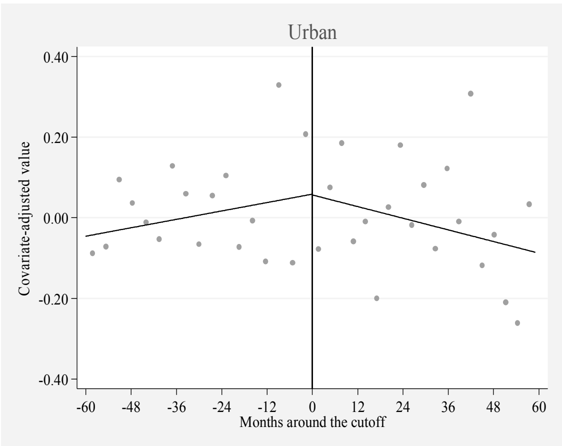
Figure 1: Policy Effect on Financial Control Behavior under Alternative Definitions in EK's Rural Areas

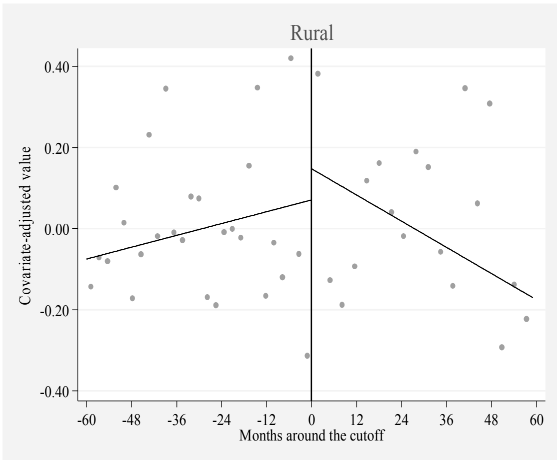
1) EK Definition of Financial Control Behavior – Only Two of the Three Questions





2) Full Definition of Financial Control Behavior – All Three Questions

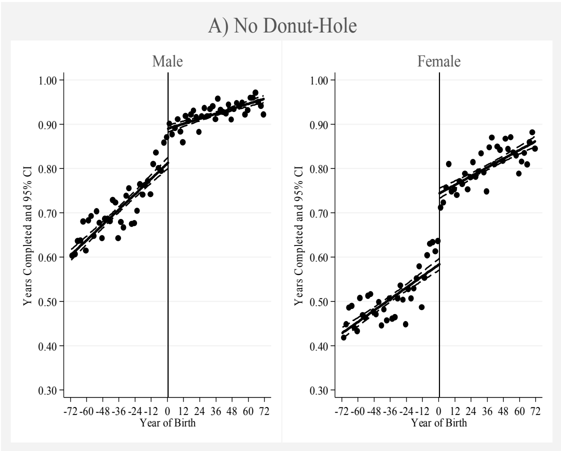


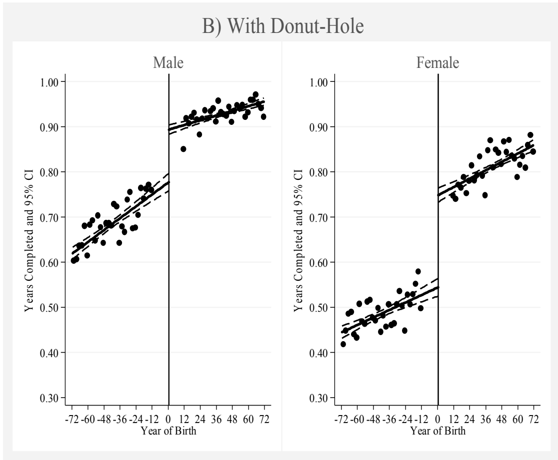


Notes: The dependent variable is adjusted for the covariates that EK use. Sampling weights are used. "Rdplot" package of CCT is used.

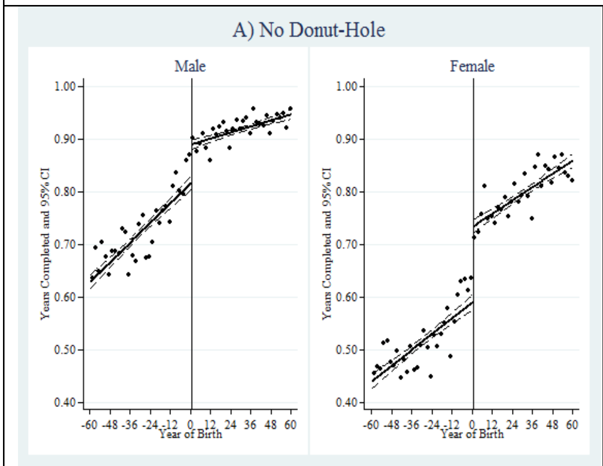
Figure 2: Policy Effect on Junior-High School Completion, 2014 THLFS

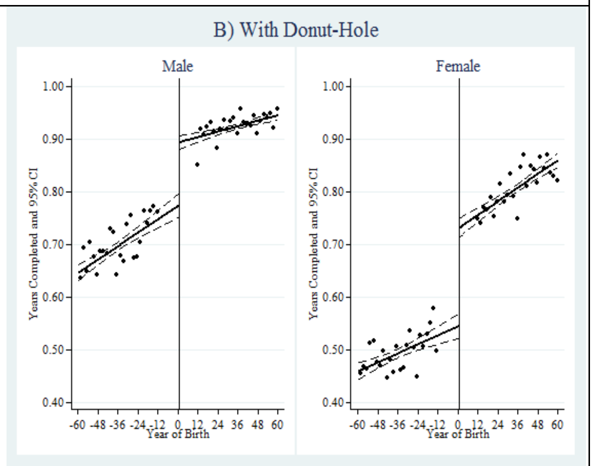
1) Bandwidth = 72 months



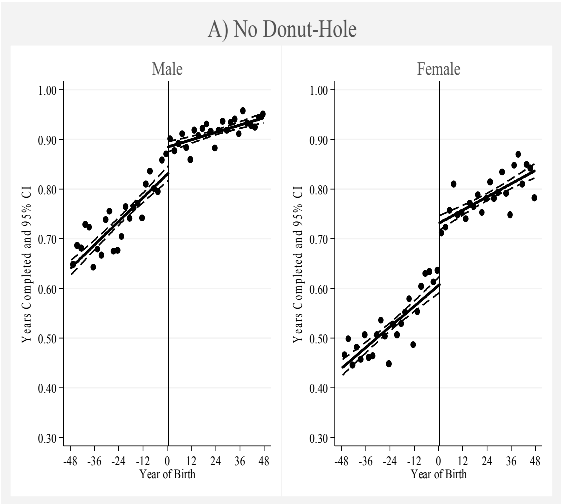


2) Bandwidth = 60 months





3) Bandwidth = 48 months



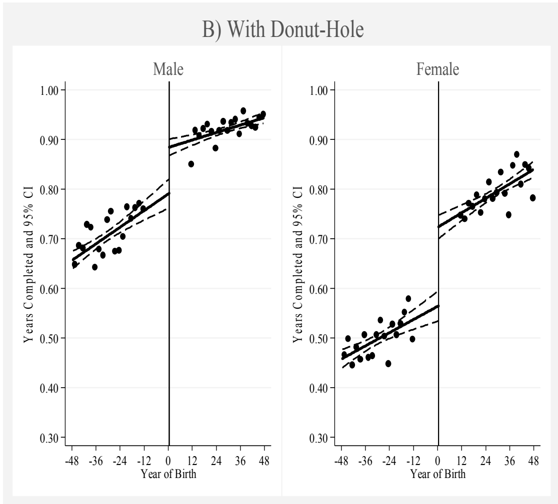
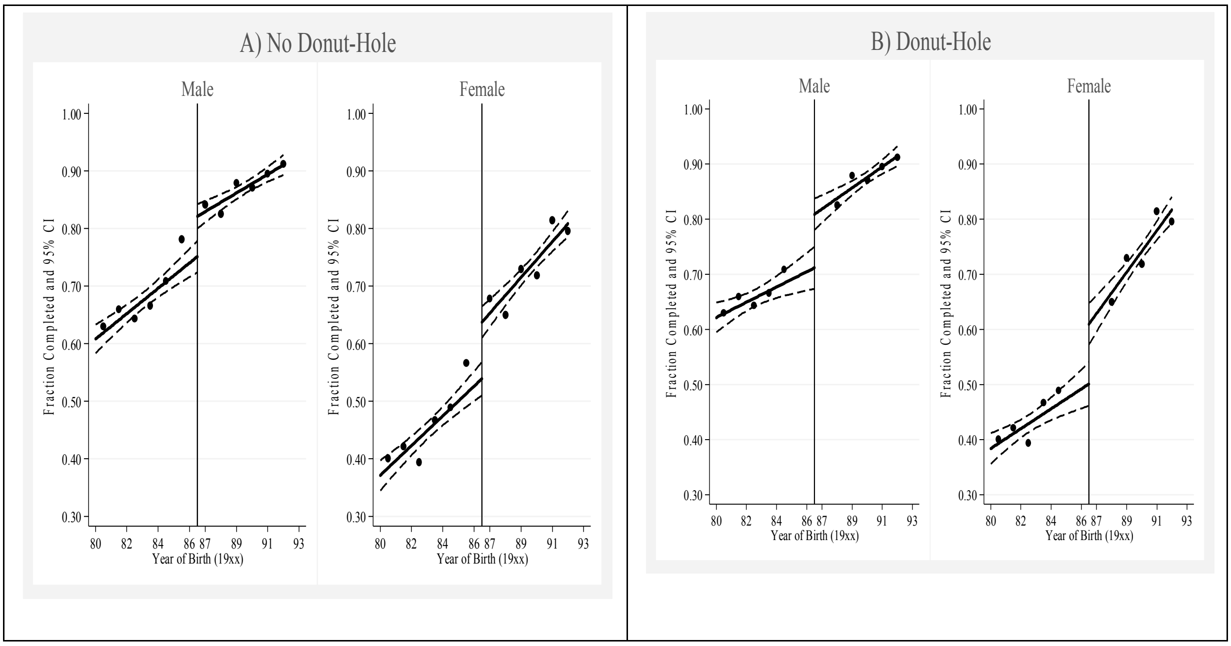


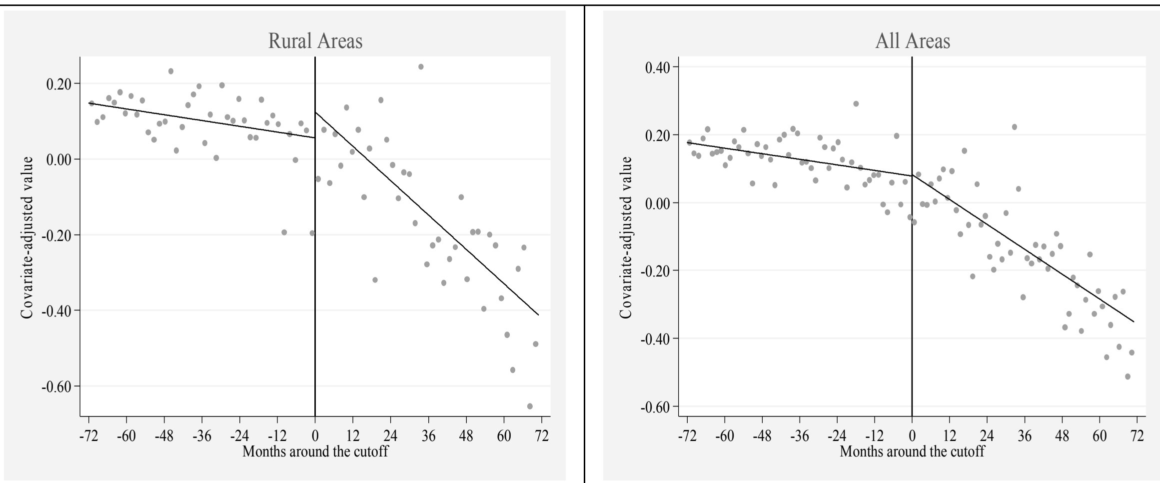
Figure 3: Fraction Completing Middle School, 2008 TNSDVW



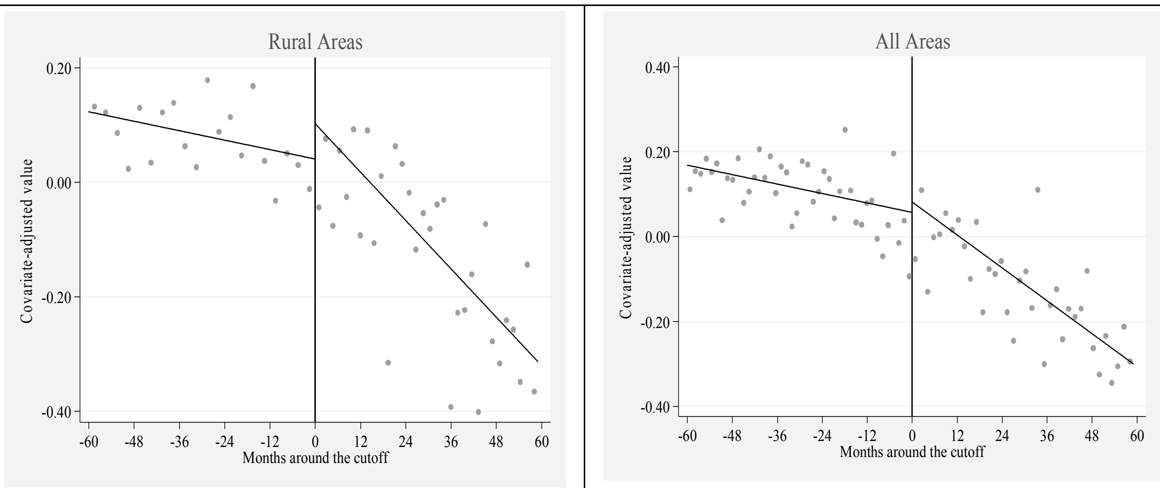
Notes: The data come from the 2008 TNSDVW. The sample is restricted to individuals aged 16 and above (as most individuals complete grade 8 at age 14 or 15).

Figure 4: Policy Effect on Ever Having a Relationship in EK's Rural Areas and All Areas

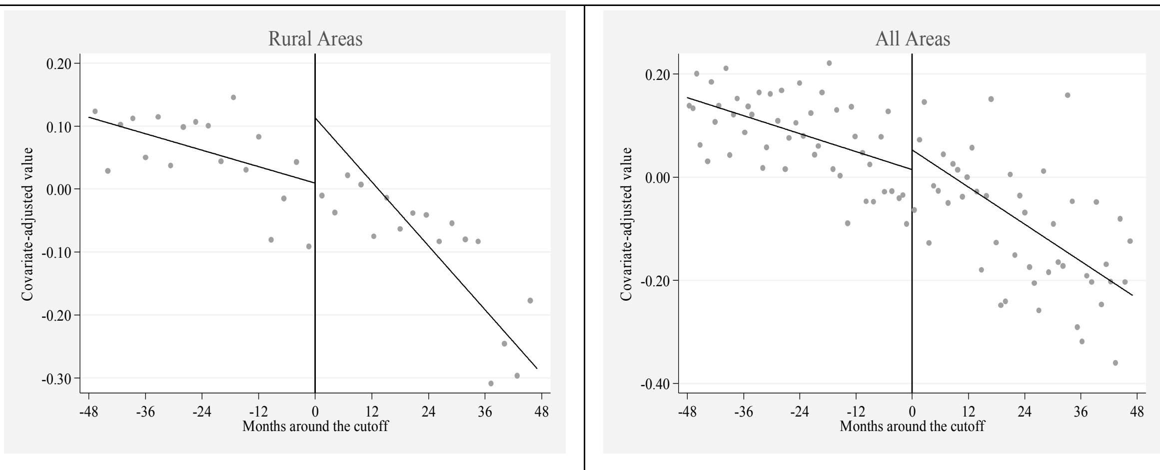
A) Bandwidth = 72 months



B) Bandwidth = 60 months



C) Bandwidth = 48 months

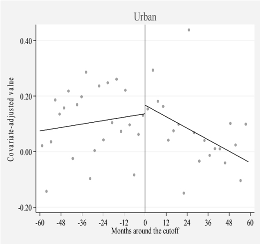


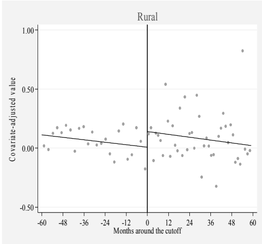
Notes: The dependent variable is adjusted for the covariates that EK use. Sampling weights are used. "Rdplot" package of CCT is used.

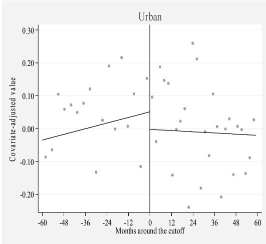
Figure 5: Key Outcome Variables in EK

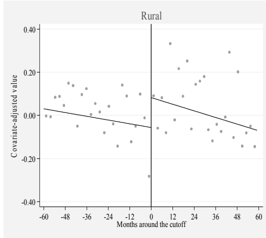
I)No Covariates, No Sampling Weights		II) Covariates, Sampling Weights	

A) Psychological Violence

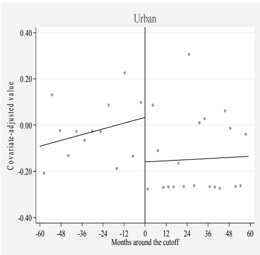


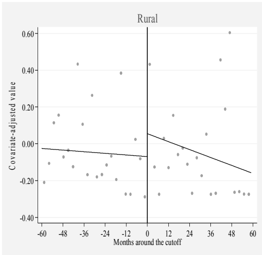


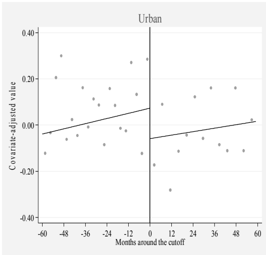


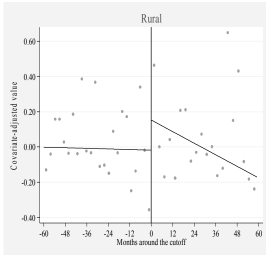


B) Financial Control Behavior

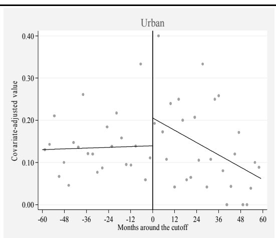


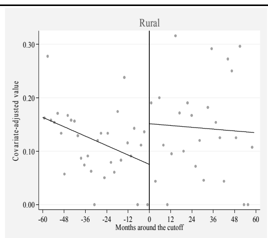


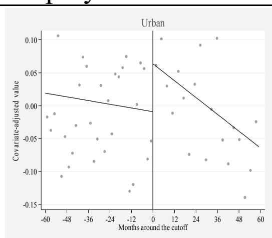


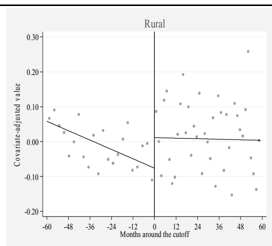


C) Employed









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

APPENDIX

Table A1: Policy Effect on Key Variables of Interest in "Rural Areas" with 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: 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 A3: 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 A4: Policy Effect on Middle School Completion by Gender – Nonparametric approach of CCFT, 2014 THLFS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A) Men				*B) Women*			
Conventional	0.038***	0.041***	0.031*	0.039**	0.089***	0.077***	0.083***	0.075***
	(0.013)	(0.011)	(0.016)	(0.016)	(0.010)	(0.007)	(0.014)	(0.012)
Bias-corrected	0.032**	0.037***	0.029*	0.039**	0.083***	0.074***	0.080***	0.074***
	(0.013)	(0.011)	(0.016)	(0.016)	(0.010)	(0.007)	(0.014)	(0.012)
Robust	0.032**	0.037***	0.029	0.039**	0.083***	0.074***	0.080***	0.074***
	(0.014)	(0.012)	(0.018)	(0.017)	(0.012)	(0.009)	(0.017)	(0.015)
Observations	169,355	169,355	169,355	169,355	174,882	174,882	174,882	174,882
BW Type	MSE	CER	MSE	CER	MSE	CER	MSE	CER
BW loc. poly.	24.67	17.51	47.80	32.31	22.73	16.11	43.96	29.67
BW bias	53.02	53.02	67.19	67.19	45.03	45.03	68.50	68.50
Order of local poly.	1	1	2	2	1	1	2	2
Order Bias	2	2	3	3	2	2	3	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 A5: 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 A6: 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 A7: Policy Effect on Ever Having a Relationship – CCFT Method

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A) Rural Sample				*B) Total Sample*			
Conventional	0.143***	0.141***	0.133**	0.217***	0.104***	0.071***	0.095***	0.068*
	(0.036)	(0.037)	(0.054)	(0.074)	(0.029)	(0.027)	(0.035)	(0.038)
Bias-corrected	0.160***	0.149***	0.129**	0.217***	0.112***	0.075***	0.088**	0.065*
	(0.036)	(0.037)	(0.054)	(0.074)	(0.029)	(0.027)	(0.035)	(0.038)
Robust	0.160***	0.149***	0.129*	0.217***	0.112***	0.075***	0.088**	0.065*
	(0.044)	(0.046)	(0.067)	(0.081)	(0.035)	(0.028)	(0.040)	(0.037)
Observations	6,463	6,463	6,463	6,463	10,667	10,667	10,667	10,667
BW Type	MSE	CER	MSE	CER	MSE	CER	MSE	CER
BW loc. poly.	21.66	15.81	26.11	18.22	21.99	16.05	30.27	21.12
BW bias	33.47	33.47	39.23	39.23	34.87	34.87	42.74	42.74
Order of local poly.	1	1	2	2	1	1	2	2
Order Bias	2	2	3	3	2	2	3	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 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
1) 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]
No Obs.	2,075	1,878	1,704	1,521	1,320	1,095	869	659
Results with Corrected Data	1.768***	1.889***	1.873***	1.786***	1.945***	1.828***	2.139***	2.523***
	[0.365]	[0.371]	[0.376]	[0.383]	[0.403]	[0.453]	[0.502]	[0.594]
No Obs.	2,256	2,039	1,843	1,645	1,420	1,179	934	707
2) 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]
No Obs.	1,717	1,595	1,453	1,306	1,139	954	763	574
Results with Corrected Data	0.890**	0.868**	0.764*	0.991**	0.799*	0.581	0.413	0.480
	[0.377]	[0.394]	[0.402]	[0.413]	[0.419]	[0.451]	[0.505]	[0.570]
No Obs.	1,771	1,643	1,497	1,341	1,169	981	786	594

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 -- instead of zero. 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.

Table A9: Policy Effect on Key Variables of Interest by Childhood Rural and Urban Areas – Our Findings

	Number of Years on Each Side of the Cutoff							
	10	9	8	7	6	5	4	3
A) Psychological Violence								
Rural Sample	0.000	-0.004	0.011	0.021	0.007	-0.010	-0.064	-0.107*
	[0.047]	[0.049]	[0.049]	[0.052]	[0.051]	[0.049]	[0.043]	[0.051]
Wild-cluster Bootstrap p-value	0.996	0.956	0.855	0.718	0.932	0.908	0.547	0.375
Observations	1,732	1,558	1,382	1,244	1,066	880	685	518
Urban Sample	-0.030	-0.019	-0.019	-0.025	-0.022	-0.018	0.035	-0.022
	[0.026]	[0.025]	[0.026]	[0.027]	[0.024]	[0.023]	[0.034]	[0.022]
Wild-cluster Bootstrap p-value	0.309	0.463	0.472	0.351	0.287	0.480	0.391	0.375
Observations	2,649	2,445	2,218	1,974	1,719	1,446	1,159	867
B1) Financial Control Behavior (EK)								
Rural Sample	0.014	-0.000	0.006	0.012	-0.025	-0.068	-0.099	-0.275*
	[0.124]	[0.126]	[0.128]	[0.134]	[0.134]	[0.139]	[0.144]	[0.119]
Wild-cluster Bootstrap p-value	0.895	1.000	0.964	0.919	0.898	0.788	0.719	0.344
Observations	1,625	1,452	1,280	1,142	972	802	629	471
Urban Sample	0.039	0.086	0.106*	0.127**	0.067	-0.006	0.085	0.196**
	[0.061]	[0.051]	[0.052]	[0.057]	[0.063]	[0.081]	[0.051]	[0.064]
Wild-cluster Bootstrap p-value	0.609	0.217	0.147	0.150	0.476	0.951	0.508	0.281
Observations	2,421	2,218	1,992	1,749	1,515	1,283	1,032	777
B2) Financial Control Behavior (full)								
Rural Sample	0.082	0.055	0.041	0.039	-0.019	-0.067	-0.074	-0.231
	[0.103]	[0.103]	[0.103]	[0.110]	[0.105]	[0.106]	[0.121]	[0.130]
Wild-cluster Bootstrap p-value	0.549	0.673	0.739	0.798	0.898	0.676	0.711	0.344
Observations	1,675	1,501	1,327	1,189	1,015	837	656	495
Urban Sample	0.051	0.083	0.109	0.129	0.067	0.030	0.105	0.250**
	[0.076]	[0.075]	[0.074]	[0.076]	[0.086]	[0.107]	[0.065]	[0.069]
Wild-cluster Bootstrap p-value	0.591	0.408	0.288	0.301	0.621	0.874	0.469	0.188
Observations	2,534	2,330	2,104	1,860	1,623	1,375	1,112	831
C) Employment								
Rural Sample	-0.009	0.010	0.011	0.032	0.019	0.034**	0.001	-0.014
	[0.035]	[0.028]	[0.030]	[0.019]	[0.017]	[0.012]	[0.022]	[0.030]
Wild-cluster Bootstrap p-value	0.820	0.746	0.738	0.217	0.358	0.039	0.984	0.750
Observations	1,735	1,561	1,385	1,247	1,068	882	687	520
Urban Sample	0.038*	0.039*	0.041*	0.045*	0.054**	0.052*	0.048*	0.014
	[0.021]	[0.022]	[0.022]	[0.023]	[0.024]	[0.023]	[0.021]	[0.023]
Wild-cluster Bootstrap p-value	0.097	0.099	0.115	0.134	0.109	0.074	0.117	0.594
Observations	2,660	2,456	2,229	1,984	1,729	1,454	1,166	873

Notes: The data come from the 2008 Turkish National Survey on Domestic Violence against Women. Childhood rural areas are defined as villages and urban areas as district or province centers. 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 year of birth, the regressions also control for birth-month dummies, a dummy for whether the interview language was Turkish, a dummy whether the type of residence at age 12 is a village, a dummy for whether the respondent lives in a rural area 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 year of birth level. Since the number of clusters is small, 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.