

For Better or for Worse?: Education and the Prevalence of Domestic Violence in Turkey[†]

By BILGE ERTEN AND PINAR KESKIN*

We exploit a change in the compulsory schooling law in Turkey to estimate the causal effects of education on the prevalence of domestic violence. By adopting a regression discontinuity design, we find that the reform increased women's schooling by one year to one-and-a-half years and improved their labor market outcomes, with particularly strong effects for women raised in rural areas. 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. (JEL I21, I28, J12, J16, J24, O15, O18)

Intimate partner violence remains an important and widespread problem with adverse health and economic consequences in both developed and developing countries. In the United States, 22.3 percent of women have experienced severe physical violence by an intimate partner (Smith et al. 2017). The costs of domestic violence are staggering. In the United States alone, the total cost of domestic violence exceeds \$5.8 billion annually, and more than \$900 million of that cost is attributed to productivity losses (Department of Health and Human Services 2003). Intimate partner violence is even more prevalent in developing countries, particularly in those where women have weak bargaining power within the household (World Health Organization 2013). Women in violent relationships are more likely to report mental health problems, suicidal thoughts and attempts, alcohol and substance abuse, absence from work, sexually transmitted diseases, and unintended pregnancies (Campbell 2002). Similarly, children born or raised in such households are more likely to have lower birth weights (Aizer 2011), difficulties relating to their peers (Carlson 2000), delays in neuro-cognitive development (Huth-Bocks, Levendosky, and Semel 2001), and lower IQ

* Erten: Department of Economics, Northeastern University, 43 Leon Street, 312A Lake Hall, Boston, MA 02115 (email: b.erten@neu.edu); Keskin: Department of Economics, Wellesley College, Pendleton East, 106 Central Street, Wellesley, MA 02481 (email: pinar.keskin@wellesley.edu). For their comments and suggestions, we would like to thank S. Anukriti, Abdurrahman Aydemir, Alessandro Bonatti, Kristin Butcher, Resul Cesur, James Dana, Williams Dickens, Lakshmi Iyer, Murat Kirdar, Naci Mocan, Suresh Naidu, Cristian Pop-Eleches, Kartini Shastry, Huzeyfe Torun, and David Weil, as well as seminar participants at Bogazici University, DIAL, Galatasaray University, Koc University, Northeastern University, Sabanci University, Toulouse School of Economics, Tufts University, and Wellesley College. We are indebted to Mehmet Alper Dincer for generously answering our numerous questions. All errors are, of course, our own.

[†] Go to <https://doi.org/10.1257/app.20160278> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

(Koenen et al. 2003). These children may also impose negative achievement spillovers on their classmates (Carrell and Hoekstra 2010).

The economic empowerment of women is often considered a major tool in the fight against intimate partner violence. However, the impact of female economic empowerment on the level of domestic violence is a priori ambiguous. On the one hand, female empowerment through employment opportunities, income transfers, or access to welfare services may increase the resources available to women within the household, improve their outside options and/or bargaining status in their relationships, and decrease their exposure to violence (Farmer and Tiefenthaler 1996, Stevenson and Wolfers 2006, Aizer 2010, and Hidrobo and Fernald 2013). On the other hand, an increase in the resources available to women may strengthen men's incentives to use violence or threats of violence in order to control these newly obtained resources (Bloch and Rao 2002; Eswaran and Malhotra 2011; and Bobonis, Gonzalez-Brenes, and Castro 2013). Moreover, an increase in women's bargaining power, e.g., through better employment opportunities, may generate backlash from their partners, who may prefer that women do not work (Field et al. 2016). As a result, women may become more vulnerable to mistreatment.

In this paper, we examine the effect of a specific empowerment channel—education—on the prevalence of domestic violence in Turkey. Our findings reveal that increased female schooling improved women's labor market outcomes; however, this economic empowerment did not translate into empowerment within households. Our research addresses a number of limitations in existing studies. First, previous studies report a negative correlation between women's education and domestic violence (Ackerson et al. 2008, Vyas and Watts 2009, Eswaran and Malhotra 2011, and Anderberg et al. 2016), but fail to establish a causal relationship because they do not account for reverse causality or omitted variable bias.¹ Given that unobservables such as ability, discount rates, and upbringing might affect both education and domestic violence risk, establishing a causal relationship has been difficult. Second, although the quasi-experimental education-health literature addresses these concerns, none of the existing studies focuses on the causal effects of education on the incidence of domestic violence experienced by women.

Our paper is the first to causally examine whether education has a positive or negative impact on the probability of experiencing domestic violence. In particular, we assess whether an exogenous increase in the number of years of schooling affects

¹ Several studies report a negative correlation between education and domestic violence. Ackerson et al. (2008) find that, in India, the probability that women with no education experience intimate partner violence was more than five times higher than that of college-educated women, while the probability for wives of uneducated men was almost twice that for wives of college-educated men. Vyas and Watts (2009) show that, in low- and middle-income countries, college-educated women are less likely to experience domestic violence and more likely to leave their partners if they face violence. Tang and Lai (2008) review the domestic violence literature on China and conclude that the risk of spousal abuse is positively correlated with having low levels of education (for either the male or female partner), having grown up in rural areas, and abusing tobacco or drugs. In Turkey, a report from the Ministry of Family and Social Policy, *Domestic Violence against Women in Turkey*, shows that the prevalence of physical violence among women who did not complete primary school is 52 percent, which is more than twice the prevalence of physical violence among women with a high school degree or higher (25 percent) (Jansen, Üner, and Kardam 2009). Correlations also reveal that women without a primary school education are almost three times as likely to be subject to sexual violence (22.2 percent) than women with a high school degree or higher (8.7 percent). However, to date, no research has provided causal identification of the effect of education on domestic violence in any country context.

different dimensions of intimate partner violence indicators in a developing-country context with a high prevalence of domestic violence and relatively low levels of women's empowerment. We examine whether education has an effect on domestic violence through three mechanisms. First, additional years of schooling may alter women's attitudes toward gender norms and domestic violence by improving their access to information, informing them of their rights, and helping them develop a new sense of self-worth. If women's attitudes improve as a result of education, they might be less tolerant of domestic violence. Second, higher educational attainment may allow a woman to marry a "higher-quality" husband who may be more educated and less likely to use violence in his relationships. Third, more-educated women are more likely to have better employment opportunities and to earn higher returns on their education. According to traditional theories of household bargaining, such economic empowerment may strengthen women's outside options and reduce their risk of experiencing violence. However, as instrumental theories of violence illustrate, a potential unintended consequence of economic empowerment may be an increased incidence of domestic violence if male partners use violence or threats of violence as coercive instruments to extract resources from their partners. Our empirical analysis sheds light on these mechanisms that relate education to economic empowerment and empowerment within the household.

We study the consequences of the 1997 compulsory schooling law in Turkey, which increased mandatory school attendance from five to eight years, on domestic violence and related outcomes. We use a new and recently available nationally representative survey, the National Survey on Domestic Violence against Women in Turkey (NSDVW 2008). This survey includes detailed information on the prevalence of spousal violence, gender roles and violence attitudes, and labor and marriage market indicators. To isolate the causal impact of education, we employ a regression discontinuity (RD) design, which allows us to estimate a meaningful causal treatment effect by comparing domestic violence indicators for younger individuals who were exposed to the reform and older individuals who were not exposed to it in the context of Turkey. Because the required age for starting junior high school in Turkey is 12, the expansion of compulsory schooling in 1998 implied that individuals born before January 1987 could drop out after 5 years, whereas those born after January 1987 had to complete 8 years of education (Cesur and Mocan 2013; Dinçer, Kaushal, and Grossman 2014). Our identifying assumption is that these two cohorts, born one month apart, display no systematic differences other than being affected by the compulsory schooling law or not. In our RD design, we assign treatment according to the individual's month and year of birth, with those born after January 1987 assigned to the treated status.

To estimate the effects of education on domestic violence measures, we begin our analysis first by testing whether the reform had a positive effect on the level of education in our sample. Our RD estimates show that the reform increased female schooling by approximately 0.9 years and improved junior high school completion by 19 percentage points. In contrast, we find no evidence that the reform had a significant impact on the level of education attained by men in our sample. Furthermore, we find that the reform affected primarily women raised in rural areas that had previously lacked schooling infrastructure and held socially conservative

norms regarding the schooling of girls. For women who grew up in rural regions, the reform increased years of schooling by 1.8 years and junior high school completion by 34 percentage points. We find no evidence that the reform had a significant effect on the education of women raised in urban regions, who had, on average, eight years of schooling prior to the reform.

Second, our findings reveal that the reform had an adverse impact on the psychological violence and financial control behavior experienced by women raised in rural areas. However, we find no evidence that the reform had a significant effect on physical or sexual violence. Our RD estimates show that for women raised in rural areas, the reform had negative effects, increasing the psychological violence experienced by women by 12 percentage points. We also find that the reform had a negative effect on the financial control behavior faced by women raised in rural regions, increasing financial control behavior by 24 percentage points; such behaviors include taking a female partner's income or refusing to give her money for household expenditures.

We note upfront that self-reported data have potential shortcomings. Indeed, if education makes women better aware of their own rights, it is possible that more-educated women tend to report the actual incidence of violence at different rates than less-educated women. This, in turn, might cause us to overestimate violence among more-educated women. In our analysis, we address this issue by investigating whether education had an impact on women's attitudes toward violence. Based on our RD estimates, we find no evidence that the education reform had a significant impact on these attitudes.²

After quantifying the impacts of education on the prevalence of different forms of domestic violence, we explore the potential mechanisms underlying these effects. First, as already noted, we find no evidence that the reform had a significant effect on the domestic violence attitudes of women or on their partners' characteristics. However, our findings indicate that the increase in years of schooling had significant positive effects on labor market outcomes. Women who attained higher levels of education because of the reform were more likely to be employed and to work in the nonagricultural sector. As a result, these women also had higher personal incomes. Furthermore, we find larger RD treatment effects on these outcomes for women raised in rural regions, an additional positive effect on the likelihood of having social security benefits, and an increase in the likelihood of working in the service sector. We find no evidence of a significant RD treatment effect in urban areas. This result is not surprising, as the main compliers with the reform were women who grew up in rural areas. Since we assess the effects of education on a large number of outcomes, we adjust the standard errors for multiple hypothesis

²Ideally, one should use administrative data, such as hospital or police reports, when studying domestic violence because they are objective measures of violence and are not subject to self-reporting bias. However, this kind of information is likely to be flawed, especially in developing countries in which only a select group of women has access to hospitals or police stations after they experience a violent episode. For instance, in our dataset, only 4.2 percent of all women who reported an episode of physical violence visited a hospital after the attack, and only 4.4 percent of them filed a police report. It is even more difficult, if at all possible, to capture the extent of psychological or financial violence using administrative reports. Using a rare dataset that includes self-reported information on physical, psychological, sexual, and financial violence against women, we present the effects of increased education on different forms of violence that are otherwise impossible to observe.

testing following Simes (1986). Most of our findings are robust to this adjustment for multiple hypothesis testing.

The effects of the reform on domestic violence may be explained by an increase in the amount of household resources over which two partners can fight. In particular, the increase in women's income due to better schooling might have created incentives for men to use coercive instruments, such as threats of violence and other controlling behavior, to gain more influence over the household decision-making process. Indeed, our finding that the male partner exerts more financial control indicates that he has a motivation to extract rents by either taking the female partner's income or by refusing to contribute to household spending, thus forcing women to use their own income for joint purposes. These results are consistent with instrumental theories of violence that predict an increase in male partners' incentives to use coercive instruments in order to extract further rents as women's income increases. A common assumption in instrumental theories of violence is that bargaining power drastically shifts in favor of the male partner once a woman commits herself to marriage.³ Our results are, however, inconsistent with traditional models of household bargaining that predict an improvement in the female partner's position as her outside option increases with income. Crucially, a woman's outside options increase only to the extent that the threat of leaving the marriage is credible. In other words, women's actual degree of bargaining power is quite limited in societies where women's rights are weak and divorce is strongly stigmatized. Divorce rates in our context are minimal: at 1.4 per thousand people, Turkey has one of the lowest crude divorce rates in the Organisation for Economic Co-operation and Development (OECD 2014). Given the high social costs of divorce, women have strong incentives to stay in relationships in which they experience domestic violence, even as their labor market outcomes improve.

Two other mechanisms, in addition to those explored in instrumental theories of violence, are equally plausible explanations for our findings. First, an increase in women's schooling may lead to an increase in their ability or desire to work, and conflict with men's preference that their wives not work. Our finding that the reform increased female employment confirms that women prefer to work and reap the returns to their schooling. However, it remains plausible that men may prefer their partners not to work due to internalization of social norms against women's participation in the paid economy (Field et al. 2016).

³This shift allows the male partner to use either domestic violence (Bloch and Rao 2002) or threats of violence (Bobonis, Gonzalez-Brenes, and Castro 2013) as instruments of rent extraction because the option to leave the relationship has very high social costs for the woman and her family. In the more general model of Bloch and Rao (2002), the husband uses violence to signal his level of satisfaction with the marriage to his wife's parents. In this way, higher levels of income among the wife's parents induce the husband to use more violence to receive larger transfers from her parents. This also implies that a wife's higher income in such a context—where divorce is highly stigmatized—would also increase the incentives of the husband to use more violence in order to receive larger transfers from his wife. Bobonis, Gonzalez-Brenes, and Castro (2013) make an additional assumption that the wife strictly prefers threats of violence to physical violence and derive that, under that assumption, an increase in the wife's income induces the husband to use threats of violence but reduces his use of violence, as the opportunity cost of using violence increases with the increasing income of the wife. However, in the more general case, if the wife continues to cooperate even when she experiences violence from the husband, implying a less strict set of preferences, the husband has an incentive to use violence in order to receive larger transfers from the wife as her income increases. This implication would be even stronger if the probability of receiving a transfer is higher when the husband uses violence, rather than threats of violence, as a coercive instrument.

Second, previous studies have found that the reform led to a decline in female religiosity (Gulesci and Meyersson 2012, Cesur and Mocan 2013), without any evidence of a significant impact on the religious beliefs of men (Cesur and Mocan 2013). Thus, another plausible explanation for our results is that the reform rendered women less religious compared to their male partners, which could not only make women more assertive but also generate overall conflicting world views between partners on traditional gender roles. By constructing a proxy for male religiosity, we confirm the earlier findings in the literature that there was no significant change in men's level of religiosity. Taken together, these results might suggest that women may have become less religious than their male partners, and the resulting differences in worldviews may result in more intra-household conflict.

Our work contributes to the growing literature on the causal channels that affect the prevalence of domestic violence. One strand of this literature focuses on the heterogeneous effects of conditional cash transfer programs on domestic violence. Bobonis, Gonzalez-Brenes, and Castro (2013) analyze the effect of the Mexican Oportunidades program on domestic violence and find that beneficiary women are less likely to be victims of physical violence but are more likely to receive threats of violence. They interpret their findings using instrumental models of violence, which predict that, in the presence of asymmetric information related to the husband's gains from marriage, an increase in a woman's income could lead to an increase in the husband's threats of violence in order to extract rents from her. In another evaluation of the same program, Rivera, Hernández, and Castro (2006) find that beneficiary women do not face a distinctly greater risk of being physically abused by their partners than non-beneficiary women. In a randomized evaluation of the rural Progres program, Angelucci (2008) finds that, among households that received small transfers, alcohol-related domestic violence declined, whereas in households that received large transfers, the level of spousal abuse from husbands with particularly low levels of education increased. Our study contributes to this literature by examining the effects of a reform-induced increase in female income on the incidence of intimate partner violence.

A related body of empirical work focuses on other dimensions of the intra-household bargaining channels that affect domestic violence. Using exogenous changes in labor demand in female-intensive industries, Aizer (2010) shows that a decline in the male-female wage gap reduces violence against women. She interprets these findings using a household bargaining model that predicts that a relative improvement in female income reduces her exposure to spousal violence by increasing her bargaining power. Stevenson and Wolfers (2006) find that the adoption of unilateral divorce laws in the United States reduced female suicide rates, the incidence of domestic violence, and rates of females murdered by their partners; these effects resulted from an increase in the bargaining power of the abused partner. Our study contributes to this literature by examining the different channels through which education may affect intra-household bargaining, including attitudes and labor and marriage market effects in a setting in which divorce is a rarely exercised outside option.

Finally, our study relates to the extended literature on the causal effects of compulsory schooling laws on returns to education in the labor market (Angrist and Krueger 1991, Oreopolous 2006), health outcomes (Lleras-Muney 2005, Clark and

Royer 2013), fertility behavior (Black, Devereux, and Salvanes 2008; McCrary and Royer 2011), and other outcomes. We contribute to this growing literature by offering the first study to examine the effects of female schooling on the prevalence of domestic violence and providing detailed evidence from a developing country, Turkey. We acknowledge that previous studies have examined the effects of the same 1997 compulsory schooling reform on other outcomes of interest in Turkey. These studies include, but are not limited to, Cesur and Mocan (2013), who find a negative effect of the reform on women's propensity to self-identify as religious, wear a religious head cover, and cast a vote for Islamic parties; Gulesci and Meyersson (2012), who find a negative effect on women's religiosity and a positive effect on marriage decisions; and Dinçer, Kaushal, and Grossman (2014) and Güneş (2016), who find a negative effect on fertility and child mortality and a positive effect on the use of modern contraception. Although our findings complement these studies, our paper differs significantly in its focus on domestic violence outcomes and the channels through which education may affect these outcomes.

This paper is organized as follows. Section I provides a brief description of the 1997 compulsory schooling law in Turkey. Section II presents the data used in the analysis, the identification strategy used to estimate the causal effects of education on domestic violence, and preliminary checks for the RD analysis. Section III presents the main results, and Section IV discusses the evidence on potential causal channels. Section V concludes the paper.

I. Context and Overview of the 1997 Compulsory Schooling Law in Turkey

Prior to 1997, the education system consisted of a mandatory component of five years of primary school and a voluntary component of three years of junior high school and three years of high school. For the voluntary component, students could choose from either secular schools or vocational schools, which also included religious (imam-hatip) schools. Hence, a student who completed primary school in five years could drop out or continue studying in religious schools. Turkey's laws required education to be provided only in Turkish and in a co-educational setting and prohibited a headscarf from being worn in any type of school, including religious schools. In practice, however, religious schools permitted women to wear a headscarf during class time. During the 1990s, political Islam began to gain substantial support from the public, which led an Islamist party to win the 1995 national elections. This outcome exacerbated the ongoing conflict between the Islamic movement and secular political groups, including the military and the judiciary. The latter groups strongly criticized the lack of enforcement of the law in religious schools and in extracurricular religious study centers.

In 1997, the military decided to intervene with a set of decisions aiming to prevent the spread of the Islamist movement in Turkey. These decisions were announced by the National Security Council (NSC) on February 28, 1997, and they came to be known as the 1997 military memorandum, as they paved the way for the resignation of the Islamist party leader and the end of his coalition government. Among these decisions was the extension of compulsory schooling from five to eight years to be provided only in secular schools. On August 18, 1997, the Turkish parliament passed

Law No. 4306, which extended compulsory schooling to eight years, combining primary school and junior high school into primary education. This was referred to as the Basic Education Program, and it implied that students could no longer choose to attend religious junior high schools after completing five years of primary school. Religious junior high schools were closed, and students began to receive a diploma for successfully completing eighth grade.

The law for school starting age in Turkey implies that a child begins mandatory schooling in September of the year when he/she turns six years old. The 1997 compulsory schooling law, which became effective in the 1997–1998 school year, made eight years of primary education mandatory. Hence, if a student had completed fifth grade in 1997, he/she could drop out. In contrast, if a student had completed fourth grade in 1997, he/she was obliged to continue school through eighth grade. The combination of the school starting age law and the 1997 compulsory schooling law implied that children born before January 1987 could drop out after five years, whereas those born after January 1987 had to complete eight years of education. Although some cases might not have completely fit this rule because of imperfect compliance with the age of starting school or grade repetition, the official requirements were such that students born after January 1987 were more likely to comply with the new compulsory schooling law than the older cohorts were.⁴

Referred to as a “big bang” approach to education reform, the Basic Education Program emphasized the restoration of old schools and the construction of new schools. Thousands of new teachers were recruited and trained, and the Turkish government attempted to improve computer literacy by purchasing and distributing more than 56,000 computers to rural primary schools. A standardized bus system was implemented in 2000, and a program was established to distribute free books and meals to low-income students.

Although the Basic Education Program has been criticized for failing to substantially improve the quality of public school education, the program’s implementation significantly increased enrollment in primary schools. From 1997 to 2000, the net schooling ratio rose from 84.74 percent to 93.54 percent, and the number of students increased from 9,084,635 students to 10,480,721 students. During this period, the number of teachers also increased from 302,354 to 345,015. The enrollment of girls notably increased, and from 1995 to 2005, the ratio of girls to boys in primary and secondary education rose from 90 percent to 97 percent.

II. Data and Empirical Methodology

A. Data

We use data from Turkey’s National Survey on Domestic Violence against Women (NSDVW) of 2008, a nationally representative household survey that obtained

⁴Notably, as Cesur and Mocan (2013) explain in greater detail, Turkish students who are 72 months old by the end of a calendar year can start school in September of that year (Resmi Gazete Number 21308). Thus, children who were born before January 1987 could start primary school in 1992 and avoid the eight-year mandate that was adopted on August 18, 1997, taking effect in the 1997–1998 school year.

information on the presence and intensity of domestic violence and the parental history of violence, as well as other indicators of intra-household behavior. The survey, which was conducted among 24,048 households during the months of July and September 2008, covers data on socioeconomic indicators for households, demographics, labor market and marital histories, general health status, gender role attitudes, indicators for autonomy in decision making, indicators for the presence and intensity of domestic violence, and the parental history of domestic violence. The questions on domestic violence provide detailed information on physical, sexual, and psychological violence. The survey targeted women between 15 and 59 years old, including those who were ever married, those who were in a relationship (who had a boyfriend or were engaged), and those who had never been in a relationship. One woman per household was randomly selected for the interviews. There was no one else in the room when the interviews were conducted. The respondents were informed that their answers would be kept confidential. The survey also includes questions on the birth month and year of the women, and these data facilitate our use of an RD approach.

The domestic violence measures include binary variables on whether a woman had ever experienced physical, sexual, psychological violence, or financial control behavior from her intimate partner. Physical violence is measured by acts of slapping or throwing an object that would hurt; pushing, shoving, or pulling hair; hitting with the partner's fist or in a way that hurts; kicking, pushing on the ground, or beating; and choking or burning. Sexual violence is measured by forced sexual acts, forced sexual relations resulting from the fear of what the partner would do otherwise, and humiliating sexual acts. Psychological violence is defined by acts of insulting, humiliating, scaring or threatening, attempting to isolate a woman from her friends, trying to prevent contact from her family, insisting on knowing her location, ignoring her, becoming angry if she speaks to other men, being suspicious of infidelity, requiring her to seek permission regarding healthcare decisions, and controlling what she wears.⁵ Financial control behavior includes taking income from her and refusing to contribute money for household expenditures. To capture each dimension of domestic violence, we follow Duflo, Glennerster, and Kremer (2007) and Kling, Liebman, and Katz (2007) and construct four indices by averaging the *z*-scores of the underlying domestic violence indicators.⁶

Gender role attitudes are represented by whether the respondent agrees with each of the following statements: a woman should not argue with her partner if she disagrees with him; a woman should be able to spend her money as she desires; men

⁵We construct our psychological violence measure following the WHO definition "any act or omission that damages the self-esteem, identity, or development of the individual. It includes, but is not limited to, humiliation, threatening loss of custody of children, forced isolation from family or friends, threatening to harm the individual or someone they care about, repeated yelling or degradation, inducing fear through intimidating words or gestures, controlling behavior, and the destruction of possessions" (Ellsberg and Heise 2005). However, our results are robust to alternative definitions of psychological violence.

⁶We construct *z*-scores for each domestic violence variable using the mean and standard deviation of the variable, and we take the simple average of the *z*-scores to create four violence indices. For example, the physical violence index is constructed by averaging the *z*-scores of six underlying dummy variables indicating whether the respondent was subject to the following spousal acts of violence: slapping or throwing an object that would hurt; pushing, shoving, or pulling hair; hitting with the partner's fist or in a way that hurts; kicking, pushing on the ground, or beating; and choking or burning.

can beat their partners in certain situations; it may be necessary to beat children for discipline; men should also do housework, such as cooking and cleaning; men in the family are responsible for a woman's behavior; and it is a woman's duty to have sexual intercourse with her husband. To aggregate information from these attitudes, we also create a gender attitudes index by averaging the z -scores of each of the attitude variables.

Our data contain information on the type of region in which each woman has lived through the age of 12 (e.g., whether in a village, a district, or a province) from the 2008 NSDVW survey. This information allows us to construct an indicator of pre-reform rural residence, as the age for starting junior high school in Turkey is 12-years-old.

Table 1 presents summary statistics on the major indicators for women who have ever had a relationship from the 2008 NSDVW survey. We provide summary statistics for women between the ages of 16 and 26 because the estimated bandwidths in our local regression analyses fall into this range. Panel A of Table 1 indicates that the average length of female schooling for this age group was 8.5 years. The junior high school completion rate was 63 percent, and the high school completion rate was 38 percent. Ninety-five percent of the women had completed primary school. Column 4 tests for differences in the group means of women raised in rural areas and those of women raised in urban areas. Women raised in rural areas had 1.8 fewer years of schooling, 22 percentage points lower rates of junior high school completion, and 23 percentage points lower rates of high school completion. These results correspond to 21 percent fewer years of schooling, 34 percent lower rates of junior high school completion, and 61 percent lower rates of high school completion relative to the sample mean.

In panel B of Table 1, we report descriptive statistics for labor market outcomes, which are important mechanisms through which increased female schooling may empower women (Goldin 2006, Doepke and Tertilt 2009). Only 14 percent of the 16- to 26-year-old women in our sample were employed: 11 percent of them were employed in the nonagricultural sector, 10 percent in services, and 3 percent in the agricultural sector. These results are consistent with the overall pattern in Turkey, where female labor force participation remains rather low.⁷ Approximately 7 percent of the respondents worked in a job that had social security benefits. The last row in panel B of Table 1 reports summary statistics for a personal income index that is constructed by averaging the z -scores of indicator variables on whether the respondent earned personal income from the following six sources: rent from owning land, rent from owning a house, income from owning a company or workplace, income from owning a vehicle, having money in a bank, and income from other asset ownership.⁸ Higher index values indicate greater personal income. On average, compared with women raised in urban areas, women raised in rural areas were 7 percentage points less likely to work in the nonagricultural sector, 6 percentage points less

⁷In our entire survey dataset, female labor force participation is 16.6 percent, and female labor force participation in the nonagricultural sector is 11.6 percent.

⁸We construct a dummy variable for each indicator of personal income that takes the value of one if the respondent earns income and zero otherwise. We use the simple average of the z -scores of these six dummy variables to construct a personal income index for the respondent.

TABLE 1—SUMMARY STATISTICS FOR 16- TO 26-YEAR-OLD WOMEN WHO HAVE HAD A RELATIONSHIP

	Region of childhood			Difference	Observations (all/rural/urban) (5)
	All mean (SD) (1)	Rural mean (SD) (2)	Urban mean (SD) (3)	(2) – (3) est. (SE) (4)	
<i>Panel A. Education</i>					
Schooling	8.51 (3.32)	7.66 (3.10)	9.48 (3.29)	–1.82 (0.18)	2,078/1,102/962
Completed junior high school	0.63 (0.48)	0.53 (0.50)	0.75 (0.44)	–0.22 (0.03)	2,078/1,102/962
Completed high school	0.38 (0.49)	0.28 (0.45)	0.50 (0.50)	–0.23 (0.03)	2,078/1,102/962
Completed primary school	0.95 (0.21)	0.95 (0.22)	0.96 (0.19)	–0.01 (0.01)	2,078/1,102/962
<i>Panel B. Labor market outcomes</i>					
Employed	0.14 (0.35)	0.13 (0.34)	0.15 (0.36)	–0.02 (0.02)	2,190/1,186/990
Employed in non-agriculture	0.11 (0.32)	0.08 (0.27)	0.15 (0.36)	–0.07 (0.02)	2,190/1,186/990
Employed in services	0.10 (0.29)	0.07 (0.25)	0.13 (0.34)	–0.06 (0.02)	2,190/1,186/990
Employed in agriculture	0.03 (0.17)	0.05 (0.22)	0.00 (0.07)	0.05 (0.01)	2,190/1,186/990
Social security	0.07 (0.25)	0.04 (0.20)	0.10 (0.30)	–0.06 (0.02)	2,190/1,186/990
Personal income index	–0.08 (0.44)	–0.08 (0.43)	–0.07 (0.45)	–0.02 (0.03)	2,190/1,186/990
<i>Panel C. Partner and relationship characteristics</i>					
Marriage age ^a	20.16 (2.59)	20.00 (2.62)	20.40 (2.52)	–0.39 (0.17)	1,530/927/591
Marriage decision ^a	0.56 (0.50)	0.51 (0.50)	0.64 (0.48)	–0.12 (0.03)	1,535/930/593
Partner is employed	0.84 (0.37)	0.87 (0.34)	0.80 (0.40)	0.07 (0.02)	2,190/1,186/990
Partner's schooling	9.27 (3.41)	8.60 (3.35)	10.03 (3.32)	–1.44 (0.19)	2,131/1,153/964
Schooling difference between partners	0.86 (3.17)	1.08 (3.35)	0.60 (2.91)	0.47 (0.18)	2,034/1,078/942
Age difference between partners ^a	4.17 (3.56)	4.20 (3.59)	4.14 (3.52)	0.06 (0.24)	1,530/927/591
Husband's age ^a	24.32 (3.37)	24.20 (3.31)	24.54 (3.48)	–0.34 (0.24)	1,534/930/592
Husband's religiosity index ^a	–0.02 (0.64)	0.02 (0.63)	–0.06 (0.65)	0.08 (0.04)	2,170/1,179/980
Partner witnessed violence toward his mother	0.30 (0.46)	0.32 (0.47)	0.29 (0.45)	0.03 (0.03)	1,601/874/717
Partner experienced violence from his family members	0.73 (0.44)	0.75 (0.43)	0.71 (0.45)	0.03 (0.03)	1,788/979/797
Ever divorced ^a	0.01 (0.08)	0.01 (0.10)	0.00 (0.05)	0.01 (0.00)	2,190/1,186/990
Had a second marriage ^a	0.01 (0.07)	0.01 (0.07)	0.01 (0.08)	0.00 (0.00)	2,190/1,186/990
Asset ownership index	0.09 (0.40)	–0.01 (0.39)	0.19 (0.39)	–0.20 (0.02)	2,190/1,186/990

(continued)

TABLE 1—SUMMARY STATISTICS FOR 16- TO 26-YEAR-OLD WOMEN WHO HAVE HAD A RELATIONSHIP (*continued*)

	Region of childhood			Difference (2) – (3) est. (SE)	Observations (all/rural/urban)
	All mean (SD) (1)	Rural mean (SD) (2)	Urban mean (SD) (3)		
<i>Panel D. Gender and domestic violence attitudes</i>					
A woman should not argue with her partner if she disagrees with him	0.39 (0.49)	0.45 (0.50)	0.32 (0.47)	0.13 (0.03)	2,184/1,183/987
A woman should be able to spend her money as she wishes	0.68 (0.47)	0.65 (0.48)	0.71 (0.45)	–0.06 (0.03)	2,176/1,178/984
Men can beat their partners in certain situations	0.10 (0.30)	0.12 (0.32)	0.08 (0.26)	0.04 (0.02)	2,185/1,184/987
It may be necessary to beat children for discipline	0.29 (0.45)	0.33 (0.47)	0.24 (0.43)	0.09 (0.02)	2,181/1,181/986
Men should also do housework, e.g., cooking and cleaning	0.71 (0.45)	0.67 (0.47)	0.76 (0.43)	–0.09 (0.02)	2,182/1,180/988
Men in the family are responsible for a woman's behavior	0.41 (0.49)	0.45 (0.50)	0.36 (0.48)	0.08 (0.03)	2,155/1,167/974
It is a woman's duty to have sexual intercourse with her husband	0.22 (0.42)	0.26 (0.44)	0.18 (0.39)	0.08 (0.02)	2,149/1,163/972
Gender attitudes index	0.05 (0.53)	–0.03 (0.56)	0.15 (0.48)	–0.18 (0.03)	2,190/1,186/990
<i>Panel E. Domestic violence outcomes</i>					
Physical violence index	–0.20 (0.58)	–0.18 (0.57)	–0.21 (0.59)	0.04 (0.03)	2,180/1,183/983
Sexual violence index	–0.12 (0.59)	–0.12 (0.57)	–0.12 (0.62)	0.00 (0.03)	2,180/1,183/983
Psychological violence index	0.03 (0.51)	0.04 (0.49)	0.04 (0.54)	–0.01 (0.03)	2,180/1,183/983
Financial control index	–0.11 (0.70)	–0.09 (0.72)	–0.13 (0.67)	0.03 (0.04)	1,954/1,095/846
<i>Panel F. Covariates</i>					
Rural childhood region	0.54 (0.50)	1.00 (0.00)	0.00 (0.00)	1.00 (0.00)	2,176/1,186/990
Lives in a village	0.20 (0.40)	0.34 (0.47)	0.03 (0.18)	0.31 (0.02)	2,190/1,186/990
Non-Turkish speaker	0.00 (0.07)	0.01 (0.08)	0.00 (0.04)	0.01 (0.00)	2,183/1,184/985
Mother experienced domestic violence	0.26 (0.44)	0.29 (0.45)	0.24 (0.42)	0.05 (0.02)	2,104/1,139/951

Notes: The table presents the means, standard deviations, and number of observations. The sample includes women who have ever had a relationship and who were born within 60 months before or after January 1987. The variables with an ^a sign are available only for women who have been married. Columns 1–3 report means and standard deviations in parentheses. Column 4 reports differences in the group means between columns 2 and 3 with standard errors in parentheses. Column 1 has 14 observations less than column 4 because of 14 missing observations in the region of childhood variable. The variables are described in online Appendix A.

Source: 2008 National Survey on Domestic Violence against Women in Turkey

likely to work in services, 5 percentage points more likely to work in the agricultural sector, and 6 percentage points less likely to have access to social security benefits.

Panel C of Table 1 provides summary statistics for the type of partner or marriage market indicators. On average, the partners had completed 9.3 years of schooling,

and their employment probability was 84 percent. The average age of the respondent upon the first marriage was 20.2 years. Fifty-six percent had chosen to marry their husbands, whereas the other women had arranged marriages. On average, 30 percent of the partners had witnessed violence toward their own mothers, and 73 percent had experienced violence in the past from family members, including parents, siblings, or extended family.⁹ The last row in panel C reports summary statistics for an index that is constructed from averaging the *z*-scores of indicator variables on whether the respondent's household owns 24 different assets: a refrigerator, a gas/electric oven, a microwave oven, a blender/mixer, a dishwasher, a washing machine, an iron, a vacuum cleaner, a plasma TV (LCD), a television, cable TV, a satellite antenna, a video camera, a DVD/VCD player, a camera, a cellphone, a nonmobile telephone, a computer, internet, an air conditioner, a car, a taxi/mini-bus, a tractor, and a motorcycle.¹⁰ Higher index values indicate greater household wealth. On average, the partners of women raised in rural areas have approximately 1.4 fewer years of schooling, but they are 7 percentage points more likely to be employed. The average age of marriage for women raised in rural areas is 0.4 years lower than for women raised in urban areas. Women raised in rural areas are 12 percentage points less likely to have chosen to marry their husbands. Their level of household asset ownership is also lower than that of women raised in urban areas.

Panel D of Table 1 presents the descriptive statistics for gender and domestic violence attitudes. Thirty-nine percent of the women in our sample agree with the statement that a woman should not argue with her partner if she disagrees with him, and 68 percent agree that a woman should be able to spend her money as she wishes. Regarding attitudes toward domestic violence, 10 percent agree with the statement that men can beat their partners in certain situations, and 29 percent agree that it may be necessary to beat children for discipline. More than half of them, 71 percent, agree that men should also perform housework duties, including cooking and cleaning. Furthermore, 41 percent agree that men in the family are responsible for a woman's behavior, and 22 percent agree that it is a woman's duty to have sexual intercourse with her husband. Because these variables measure gender and domestic violence attitudes in different ways, we also construct a gender attitudes index to aggregate information by averaging the *z*-scores of attitudes variables following Kling, Liebman, and Katz (2007).¹¹ Higher index values indicate more progressive attitudes favoring gender equality.

⁹This information comes from the female respondents' responses about whether their partner witnessed or experienced violence while he was growing up. Hence, it is subject to measurement error and is reported only as a proxy for whether the partner witnessed or experienced violence growing up.

¹⁰We construct a dummy variable for each indicator of household wealth that takes the value of one if the respondent's household owns an asset and zero otherwise. We use the simple average of the *z*-scores of these 24 dummy variables to construct an asset index for the respondent's household.

¹¹We follow Kling, Liebman, and Katz (2007) to create a summary index for gender role attitudes. We recode the variables such that each dummy variable is given the value of one if the respondent disagrees with statements of gender inequality and domestic violence (a woman should not argue with her partner if she disagrees with him; men can beat their partners in certain situations; it may be necessary to beat children for discipline; men in the family are responsible for a woman's behavior; and it is a woman's duty to have sexual intercourse with her husband) and if the respondent agrees with statements of gender equality (a woman should be able to spend her money as she wishes; men should also do housework, including cooking and cleaning) and zero otherwise. We then construct *z*-scores for each attitude variable using the mean and standard deviation of the variable, and we use the simple average of *z*-scores to create a gender attitudes index.

Panel E of Table 1 provides descriptive statistics for the domestic violence measures. Following Duflo, Glennerster, and Kremer (2007) and Kling, Liebman, and Katz (2007), we aggregate information from each set of domestic violence measures to create four summary indices: a physical violence index, a sexual violence index, a psychological violence index, and a financial control index. This aggregation approach provides greater statistical power to identify effects in the same direction for a group of indicators that captures similar forms of violent behavior. We construct these indices by averaging the *z*-scores of each underlying measure of physical, sexual, psychological violence, and financial control behavior, as described above. Higher index values indicate higher levels of intimate partner violence. The raw means indicate less physical violence, less psychological violence, and less financial control behavior exercised by the partners of women raised in urban areas, but the differences between the rural and urban samples are not statistically significant.

The descriptive statistics for the components of each dimension of domestic violence are presented in Table A2 in online Appendix B. Twenty-four percent of the women aged 16–26 who have ever had a relationship experienced at least one type of physical violence from their intimate partner. The most prevalent forms of physical violence were spousal acts of slapping or throwing an object that would hurt (21 percent) and pushing, shoving, or pulling hair (11 percent). Less prevalent violent acts included hitting with the partner's fist or in a way that hurts (5 percent); kicking, pushing on the ground, or beating (5 percent); and choking or burning (3 percent). Moreover, 9 percent of the women experienced sexual violence, including forced sexual acts (4 percent), forced sex because of fear of what the husband would do otherwise (6 percent), and humiliating sexual acts (13 percent) by their intimate partner. A considerable share of women experienced at least one dimension of psychological violence, including being insulted (26 percent), being humiliated (13 percent), or being threatened or scared (15 percent) by their intimate partner, and their partners have insisted on knowing her location (78 percent), becoming angry if she speaks to other men (60 percent), intervening in her clothing choices (49 percent), requiring her to obtain his permission before seeking healthcare (25 percent), attempting to isolate her from her friends (20 percent), ignoring her (14 percent), attempting to prevent contact with her family (9 percent), and suspecting that she is cheating on him (7 percent). In addition, 6 percent experienced financial control behavior from their intimate partner, including taking her income despite her disapproval (3 percent) and refusing to give money for household spending (5 percent).

Finally, panel F of Table 1 reports summary statistics on the predetermined characteristics of the 16- to 26-year-old women in our sample who had ever had a relationship. Fifty-four percent of the women lived in a rural area until the age of 12, and one-fifth lived in villages. Less than 1 percent had a non-Turkish primary interview language, typically Kurdish or Arabic. On average, 26 percent of the respondents' mothers had experienced domestic violence.

B. Identification

The 1997 compulsory schooling law, coupled with the law on school starting age, implied that individuals born after January 1987 must complete eight years of

schooling, whereas those born earlier could drop out after five years, as explained in further detail in Section I. We use this break point in an RD design to estimate the causal effect of schooling on domestic violence. Our identifying assumption is that these two cohorts born one month apart do not exhibit any systematic differences other than being exposed to the compulsory schooling law or not. As long as this assumption holds, this approach represents a treatment assignment that is as good as random. In our RD design, we assign treatment according to the month and year of birth of the individual, with those born after January 1987 assigned to the treated status.

Following previous research (Oreopoulos 2006, Gulesci and Meyersson 2012, and Clark and Royer 2013), we use an RD design by exploiting discontinuity in the birth date and using this discontinuity as an instrument for years of schooling. We provide both reduced-form estimates (i.e., sharp RD) and two-stage least-squares estimates (i.e., fuzzy RD) for all of the outcome variables of interest. Our specification follows a basic RD form:

$$(1) \quad y_i = \alpha + \beta t_i + f(x_i) + \epsilon_i$$

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

where y_i is the dependent variable, t_i is the treatment status, x_i is the forcing variable, and h is the bandwidth around the cutoff point c . We allow the slope to vary on each side of the cutoff. The control function, $f(x_i)$, is a continuous n -order polynomial function of the forcing variable on each side of the cutoff point. We use local linear regressions in our RD estimations (Imbens and Lemiux 2008) and conduct optimal bandwidth selection using the Imbens and Kalyanaraman (2009) procedure. This approach implies the selection of an optimal bandwidth for each outcome variable examined. In addition, we use specifications that adopt the optimal bandwidth from the first-stage results for years of schooling, which is estimated as 61 months around the discontinuity, in tables focusing on heterogeneous effects. This static or constant bandwidth approach will complement the former results, for which we use the optimal bandwidth. In order to accommodate for specification error in the forcing variable, we follow Lee and Card (2008) by clustering standard errors at the month-year of birth level. As we evaluate the effects of education on a large number of outcomes, we adjust standard errors for multiple hypothesis testing following Simes (1986). Thus, for each outcome variable, we report results based on both standard p -values and p -values adjusted for multiple-hypotheses testing. We include the following control variables in all of our specifications: a dummy variable for whether the respondent's interview language was not Turkish, a dummy variable for having grown up in a rural region, month-of-birth fixed effects, and region fixed effects.¹² In subsamples based on childhood residence, we also control for whether the respondent currently lives in a village.

¹²We use fixed effects for 26 regions where the respondents lived until the age of 12, when they were subjected to the education reform.

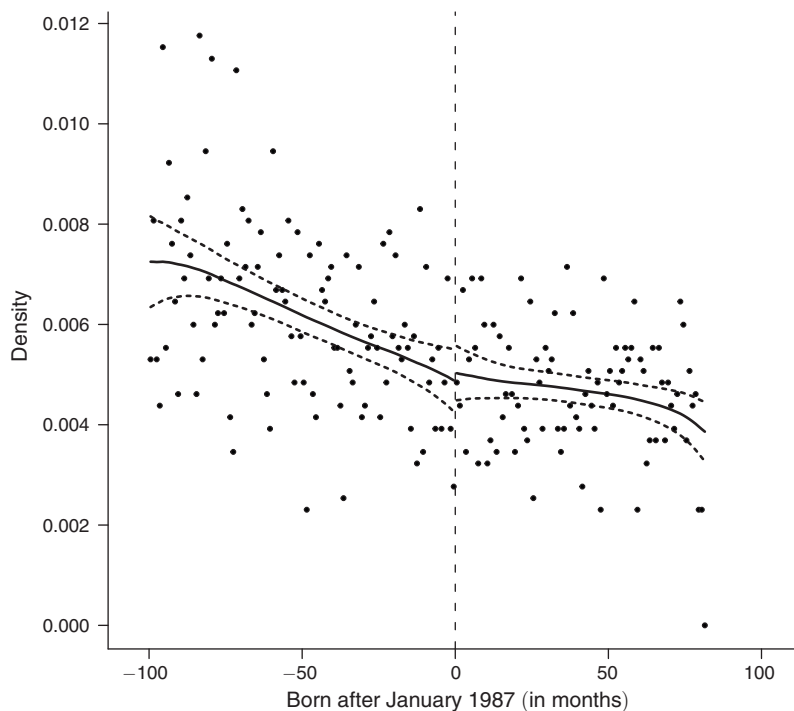


FIGURE 1. MCCRARY DENSITY TEST

Notes: The sample includes all women born between +80 to -100 months around the cutoff point, January 1987. The graph shows the results of the McCrary test of whether there is a discontinuity in the density of the forcing variable, the month of birth.

Source: Data are from the 2008 National Survey on Domestic Violence against Women in Turkey.

C. Preliminary Checks

We present two standard validity checks for the RD design (Imbens and Lemieux 2008). First, we examine whether the density of the forcing variable, the month-year of birth, is continuous at the discontinuity. We perform a McCrary density test on the density of the forcing variable. This yields an insignificant estimate, as shown in Figure 1.

Second, in Figure 2, we examine the control variables, used in subsequent regressions, at the discontinuity. Each graph represents local averages of the outcome in one-month bins plotted against the forcing variable, with overlaid smoothed linear regression lines using raw data on each side of the cutoff. The gray lines represent 95 percent confidence intervals. The predetermined characteristics that we plot are regional dummy variables for whether the respondent's childhood region is the West, South, Central, North, or East of Turkey, whether the respondent's childhood region is a rural area, whether the respondent's interview language is not Turkish, and whether the respondent's mother experienced domestic violence. The graphs do not indicate any significant jumps at the cutoff point. Overall, we conclude that the predetermined covariates appear balanced around the threshold. We also present regression-based tests of whether the control variables exhibit any significant jumps

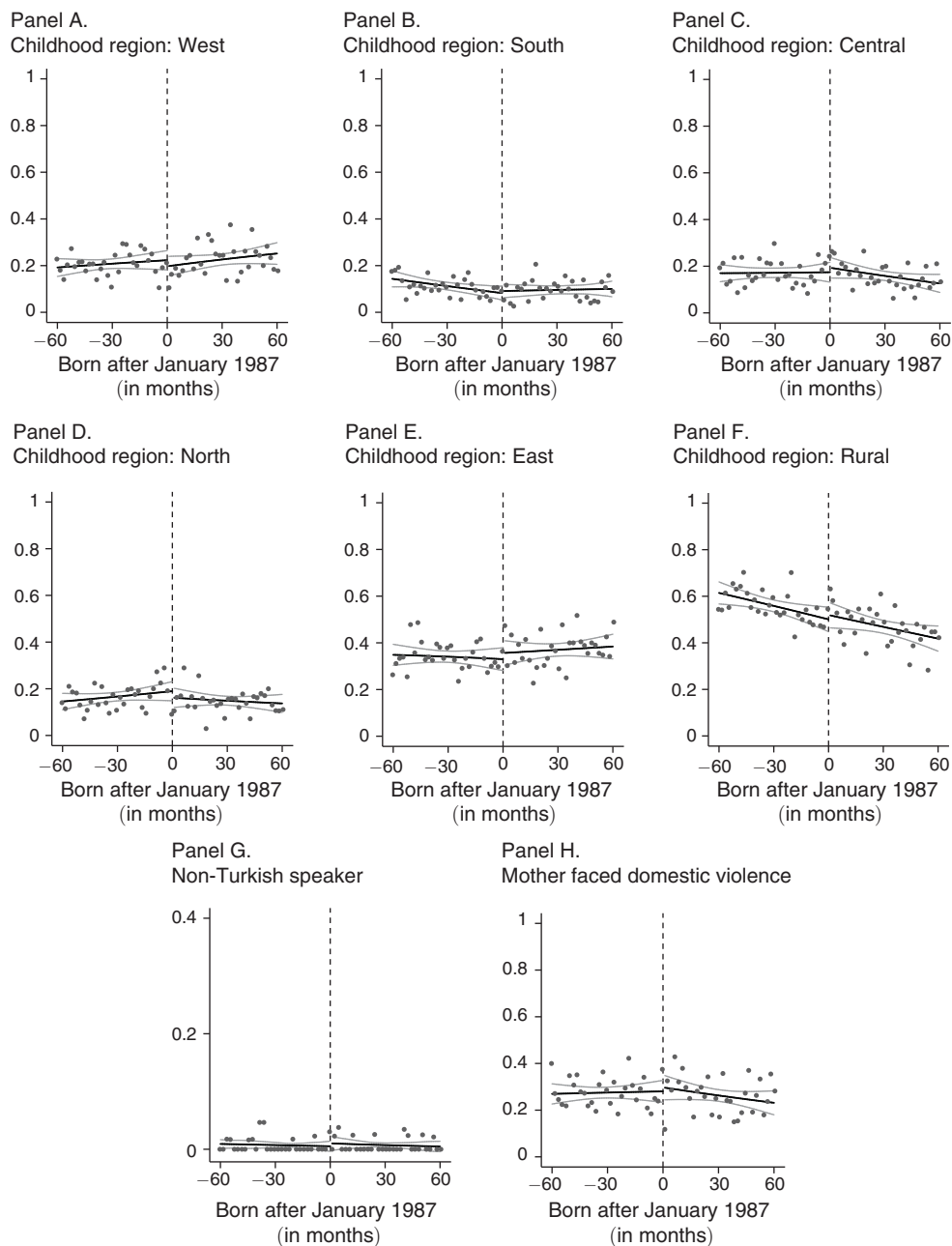


FIGURE 2. BALANCED COVARIATES

Notes: The sample includes all women born before and after 60 months around the cutoff point, January 1987. The figures plot predetermined covariates in monthly bins against the month-year of birth of being born in January 1987. The vertical line in each graph represents the cutoff point. Gray lines show 95 percent confidence intervals around the mean level. Variable definitions are listed in online Appendix A.

Source: Data are from the 2008 National Survey on Domestic Violence against Women in Turkey.

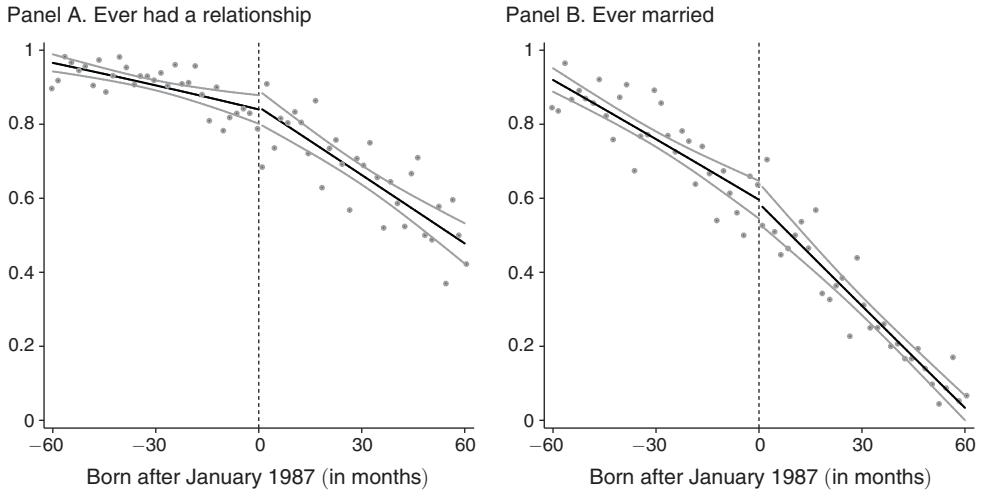


FIGURE 3. RD TREATMENT EFFECTS ON RELATIONSHIP STATUS

Notes: The sample includes all women born before and after 60 months around the cutoff point, January 1987. The figures plot a dummy variable equal to one if the respondent has ever had a relationship and a dummy variable equal to one if the respondent has ever been married in monthly bins. The vertical line in each graph represents the cutoff point, January 1987. Gray lines show 95 percent confidence intervals around the mean level.

Source: Data are from the 2008 National Survey on Domestic Violence against Women in Turkey.

at the discontinuity. In online Appendix B, Table A3 shows that none of the control variables displayed any evidence of a statistically significant jump at the discontinuity. A SUR test of the coefficients' joint significance results in a p -value of 0.71.

Because all of the intimate partner violence-related questions are relevant only to women who have been in a relationship, our RD analysis is based primarily on the sample of women who have ever had a relationship. One concern is the extent to which the treatment had an effect on current or past relationship status in 2008 and, therefore, on selection into the main sample of analysis. To address this concern, we test whether the reform had a significant effect on ever having a relationship and on ever being married. Figure 3 shows that there is some evidence of a trend break resulting from an increasing decline in the propensity of women ever having a relationship in the younger sample. However, given that we find no evidence of a significant jump around the discontinuity, we conclude that the reform had no significant impact on the probability of ever having a relationship or being married. Hence, there is no reason to expect that the reform affected the probability of selection into the sample of women who have ever had a relationship, and this sample will therefore be our focus of analysis throughout the remainder of the paper. Table A3 in online Appendix B also reports regression-based tests that show no evidence of a significant effect on relationship status.

Our focus on the sample of women who have ever had a relationship may imply that our results will be less applicable to the situations of women who had never had a relationship at the time of the survey. A comparison of women with and without a relationship history shows that significant differences between these two groups exist. Table A1 in online Appendix B shows that the women who had never had a

relationship were more educated than those who had a relationship in terms of both the years of schooling completed and the junior high school completion rate. These women without a relationship were also more likely to be employed, less likely to have conservative gender attitudes, and less likely to have grown up in a rural area. Their mothers were also less likely to have experienced domestic violence. However, these women were more likely to have a lower personal income and to live in a village at the time of the survey. Overall, these indicators suggest that women who had never had a relationship come from more educated and less socially conservative backgrounds; this may suggest that they would have continued junior high school even in the absence of the reform. In fact, as we explain below, our results in Table 2 confirm that the effect of the reform for the sample of women who had been in a relationship was larger than the effect for the entire sample.

III. Effects of the Compulsory Schooling Law

A. Schooling Outcomes

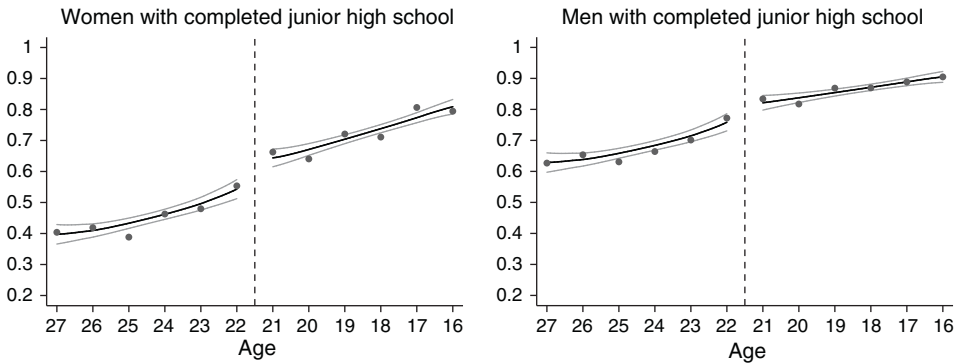
We begin by testing the effect of the compulsory schooling reform on education outcomes. Panel A of Figure 4 illustrates the different effects of the reform on women and men by plotting local averages of female and male rates of junior high school completion against the respondent's age, with a cutoff of 21–22 in annual age bins at the time of the interview. The left graph in panel A of Figure 4 shows that the average junior high school completion for all women in the sampled households is clearly higher for younger women who were affected by the education reform. The graph on the right in panel A of Figure 4 plots the average junior high school completion for all men in the sampled households and shows no clear evidence of a jump at the cutoff point. Because the effects are more precisely seen with data containing month-of-birth information for all women and men, we show the same outcomes in month-of-birth bins around the cutoff using the 2014 Household Labor Force Survey (HLFS) in panel B. The graph on the left shows evidence of a clear jump for the junior high school completion of women¹³, whereas the right-side graph shows no evidence of a significant jump for the same outcome for men.¹⁴ This result implies that the reform had a much smaller effect on men, possibly because the junior high school completion rate for males was already close to 90 percent prior to the reform.

Figure 5 provides a graphical illustration of the RD design by comparing treatment and placebo effects using the 2008 TNSDVW survey and the 2014 HLFS. In panel A, the left-side graph plots the average junior high school completion rates in monthly bins against the month and year of birth, with a cutoff of January 1987 using the 2008 TNSDVW survey. As described in Section I, the education reform required those

¹³Table A11 in online Appendix B presents the regression estimates for the effects of the schooling outcomes using the 2014 HLFS data. The results look very similar to those in Table 2, panel A, which is reassuring. It is important to note that the schooling variable in the 2014 HLFS dataset is a noisy measure of actual years of schooling since the dataset does not have information on the last grade completed, but rather has information only on the last degree earned.

¹⁴Both of these effects are documented for the whole sample of women and men in the 2014 HLFS data. Hence, there is no selection into relationship effects in this sample.

Panel A. 2008 TNSDVW household sample



Panel B. 2014 HLFS data

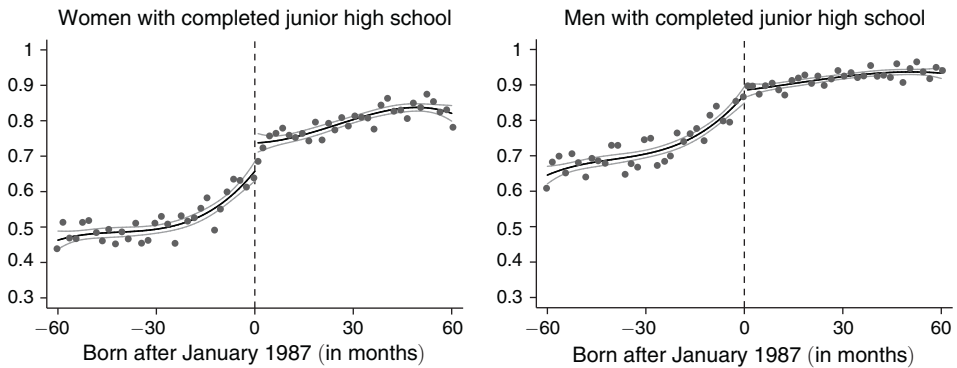


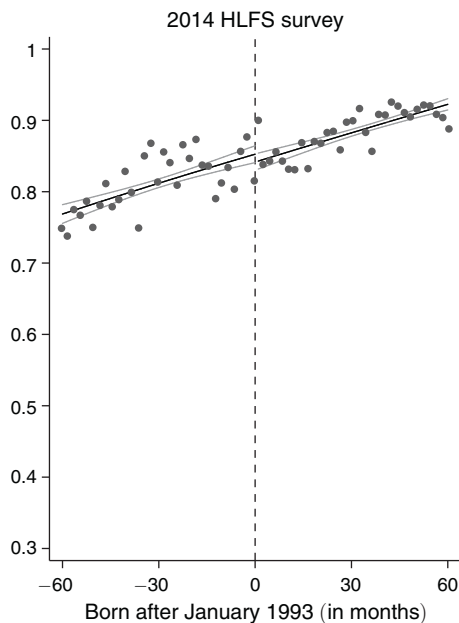
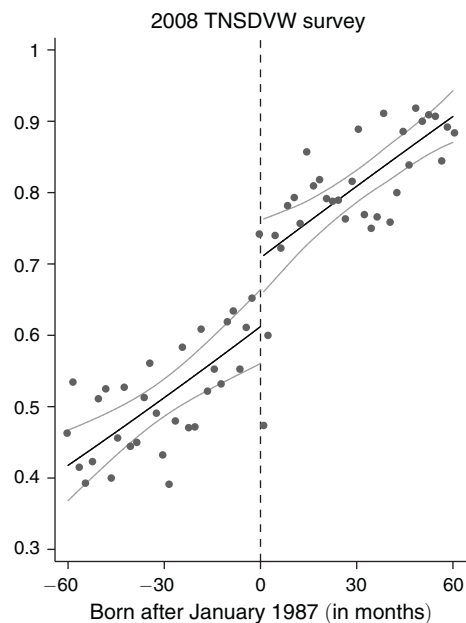
FIGURE 4. RD TREATMENT EFFECTS ON JUNIOR HIGH SCHOOL COMPLETION

Notes: The samples include all women and men born before and after 60 months around the cutoff point, January 1987. Figures plot junior high school completion rates in monthly bins. Gray lines show 95 percent confidence intervals around the mean level.

Source: Data are from the Household Sample of the 2008 National Survey on Domestic Violence against Women in Turkey in panel A, and the 2014 Household Labor Force Survey in panel B.

born after this date to complete junior high school, whereas the older cohorts had the option of dropping out after completing primary school. Local linear smoothers on each side of the cutoff are overlaid on the graph, which shows a clear jump at the discontinuity with an approximately 15–20 percentage point increase in the probability of completing junior high school. We use data from the 2014 HLFS to conduct a placebo test to examine the validity of the RD design. The right-side graph in panel A of Figure 5 shows the same relationship using the 2014 HLFS survey, where the age cutoff is the same age, comparing 21- and 22-year-old women. The same age cutoff corresponds to being born before or after January 1993. The right graph shows no evidence of a jump in completing junior high school for women of the same age in the 2014 HLFS. Thus, the jump that we observe around the discontinuity of the reform implementation in the 2008 survey is not likely to be driven by some underlying relationship between age and school completion but is rather an outcome of the reform.

Panel A. Completed junior high school



Panel B. Propensity to work

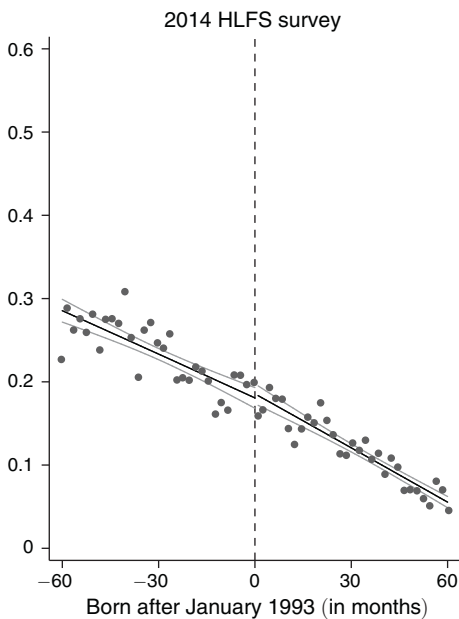
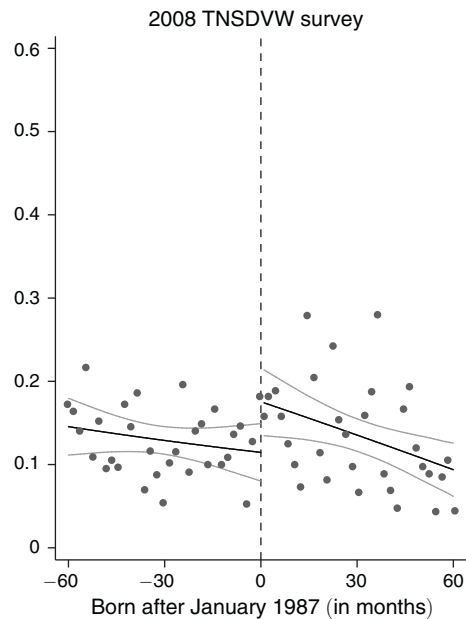


FIGURE 5. TREATMENT AND PLACEBO

Notes: The samples include all women born before and after 60 months around the respective cutoff points, January 1987 and January 1993. The figures plot a dummy variable equal to one if the respondent completed junior high school and a dummy variable equal to one if the respondent works in monthly bins. Gray lines show 95 percent confidence intervals around the mean level.

Source: Data are from the 2008 National Survey on Domestic Violence against Women in Turkey and the 2014 Household Labor Force Survey.

TABLE 2—RD TREATMENT EFFECTS ON SCHOOLING

Outcome	Linear RD \hat{h} bandwidth	Quadratic RD \hat{h} bandwidth	Linear RD $\hat{h}/2$ bandwidth	Linear RD $2\hat{h}$ bandwidth	Bandwidth	Observations	Mean
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A. Sample of all women</i>							
Years of schooling	0.863 (0.317)	0.789 (0.466)	0.804 (0.434)	1.303 (0.247)	54	2,245	8.84
Completed education:							
Junior high school	0.193 (0.042)	0.112 (0.065)	0.141 (0.059)	0.211 (0.032)	69	2,941	0.68
High school	0.135 (0.049)	0.116 (0.070)	0.035 (0.062)	0.297 (0.044)	45	1,865	0.46
Primary school	-0.014 (0.018)	0.000 (0.028)	0.001 (0.025)	-0.024 (0.016)	92	3,822	0.96
<i>Panel B. Sample of women who have had a relationship</i>							
Years of schooling	1.363 (0.322)	1.157 (0.462)	1.063 (0.435)	1.403 (0.265)	61	2,082	8.52
Completed education:							
Junior high school	0.233 (0.042)	0.193 (0.068)	0.208 (0.063)	0.233 (0.034)	77	2,600	0.62
High school	0.185 (0.054)	0.151 (0.077)	0.074 (0.066)	0.314 (0.049)	44	1,475	0.42
Primary school	-0.006 (0.024)	0.031 (0.036)	0.052 (0.032)	-0.017 (0.021)	69	2,349	0.96

Notes: Columns 1 and 2 report local RD regressions with linear and quadratic polynomials in the month-year of birth using the optimal bandwidth \hat{h} , and columns 3 and 4 report local RD regressions with linear polynomials in the month-year of birth using the optimal bandwidth $\hat{h}/2$ and $2\hat{h}$, respectively. The optimal bandwidth, reported in column 5, is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 6 reports the number of observations used in estimations with the optimal bandwidth \hat{h} , and column 7 reports the outcome mean within the optimal bandwidth \hat{h} . Panel A reports the results for the full sample, and panel B reports them for the sample of women who have ever had a relationship. The variable definitions are listed in online Appendix A. All specifications control for dummy variables for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's interview language is not Turkish, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level.

Source: 2008 National Survey on Domestic Violence against Women in Turkey

While these graphs reveal a positive RD treatment effect of being exposed to the compulsory schooling reform, the results could be further refined with regression analysis. Using the 2008 TNSDVW, Table 2 reports the RD treatment effects on years of schooling and the completion of different types of schools for all women in panel A and for women who have had a relationship in panel B. In each row, the last column reports outcome means for the relevant sample. All columns include controls for a dummy variable indicating whether the primary interview language of the respondent is not Turkish, a dummy variable indicating whether the respondent grew up in a rural location, month-of-birth fixed effects, and childhood-region fixed effects. Column 5 of Table 2 displays the optimal bandwidth estimated by the Imbens and Kalyanaraman algorithm in months on each side of the cutoff.

The first row of Table 2 presents estimates of the RD treatment effects on the years of schooling obtained by all women. The optimal bandwidth, calculated using the Imbens and Kalyanaraman (2009) algorithm, results in a bandwidth of 54 months

around the discontinuity. Based on a local linear specification, column 1 presents an RD estimate of 0.86 years for the treatment effect on years of schooling, which is statistically significant at the 1 percent level. In terms of magnitude, an increase of 0.86 years in the years of schooling corresponds to a 10 percent increase relative to the mean. For robustness, we include alternative specifications with a quadratic control function in column 2 and allow the bandwidth to vary by reporting the linear RD estimates with half and twice the optimal bandwidth in columns 3 and 4, respectively. The estimated effects remain significant within the approximate range of 0.8 to 1.3 years. Panel B of Table 2 focuses on the RD treatment effects on women who have ever had a relationship, who constitute our sample of interest in testing intimate partner violence in the subsequent step. In panel B, the linear RD treatment effect is 1.4 years of schooling, which corresponds to a 16 percent increase relative to the mean. In alternative specifications, the RD estimates for the sample of women who have ever had a relationship remain highly significant and larger than those for the entire sample. A comparison of the means of the two samples shows that women who have had a relationship had lower schooling outcomes relative to the full sample before the reform, and they were more likely to comply when the reform was implemented. In short, the compulsory schooling law had a positive effect on the years of schooling of almost 1 year for all women and of slightly more than 1 year (approximately 1.1 to 1.4 years) for women who have had a relationship. The estimates are robust to alternative functional forms and bandwidths used. This implies that the fuzzy RD estimates in the two-stage least squares specification will be smaller than the sharp RD estimates, as we use the sample of women who have had a relationship. In our results, however, we report both of these estimates for comparison.

The remaining rows of Table 2 present the RD treatment effects on different types of school completion. In panel A of Table 2, the second row displays the estimated RD treatment effects for the outcome variable of whether the respondent completed junior high school or higher. Column 1, based on the local linear specification, reports an RD estimate of 19 percentage points, corresponding to 28 percent relative to the mean. In alternative specifications, the estimate remains significant, ranging from 0.11 to 0.21. In panel B, the RD estimates of the reform's effect on the probability of completing junior high school for women who have had a relationship are larger than those for the entire sample, ranging from 23 percentage points in the linear RD specification with optimal bandwidth to 19 percentage points in the quadratic RD specification. In panel A of Table 2, for the full sample, the linear RD estimate of the treatment effect on completing high school is 14 percentage points. Although the effect is not precisely estimated in some specifications, the results suggest that the reform had long-term effects in enabling some women to continue beyond junior high school. In panel B, for women who have had a relationship, the linear RD treatment effect on completing high school is 19 percentage points, suggesting that the reform had long-lasting effects beyond the completion of junior high school among the main reform compliers. As expected, all RD estimates for whether the respondent completed primary school are insignificant. These results for primary school completion constitute a robustness check showing that the reform did not influence the likelihood of completing primary school, which was already compulsory prior to 1997. We also note that the vast majority of the estimated RD treatment effects survive the adjustment to multiple hypothesis testing.

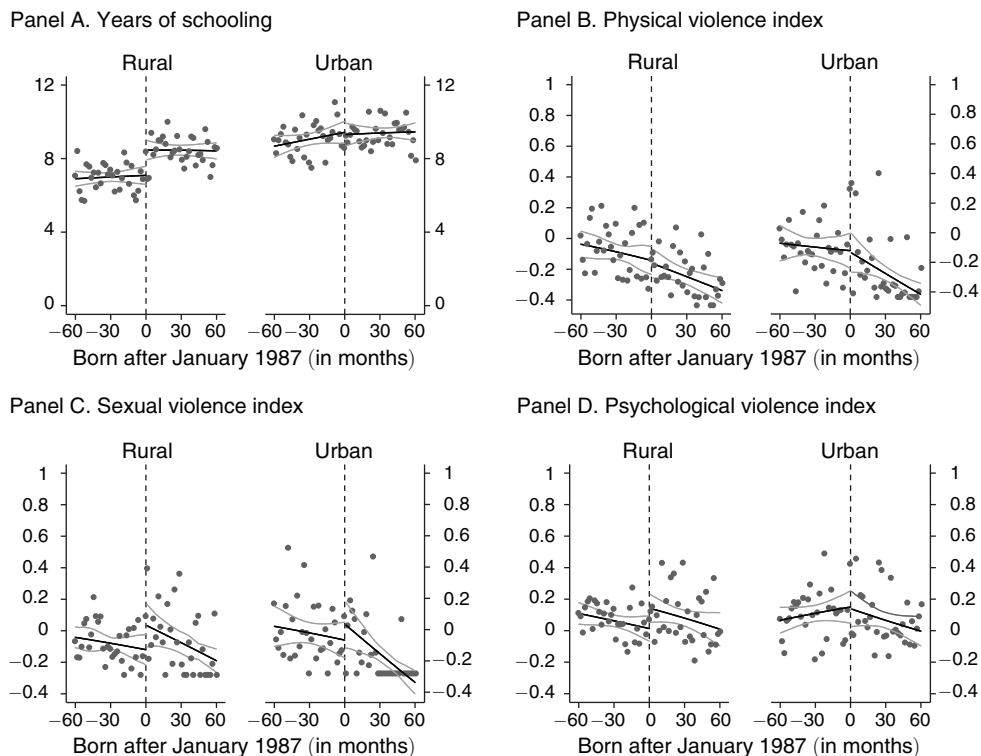


FIGURE 6. RD TREATMENT EFFECTS: RURAL VERSUS URBAN REGION OF CHILDHOOD RESIDENCE

Notes: The sample includes women who have ever had a relationship and are born before and after 60 months around the cutoff point, January 1987. The figures plot years of schooling, physical violence index, sexual violence index, and psychological violence index in monthly bins. The vertical line in each graph represents the cutoff point. Gray lines show 95 percent confidence intervals around the mean level.

Source: Data are from the 2008 National Survey on Domestic Violence against Women in Turkey.

Figure 6 provides an illustration of whether the reform had heterogeneous effects based on the region of childhood. Because the reform affected children who were 12-years-old when it was implemented, we expect the reform to have heterogeneous effects as a result of regional disparities in constraints facing access to female education in Turkey. Whereas some of these constraints result from insufficient schooling infrastructure in rural areas, some relate to the more conservative attitudes toward sending girls to school, which are prevalent in rural areas (Dulger 2004). Panel A in Figure 6 plots the average years of schooling attained by women. For women raised in rural areas, the graph shows that the pre-reform average was considerably lower than 8 years, and at the discontinuity, there is clear evidence of a significant jump of approximately 1.5 to 1.8 years of schooling. In contrast, the graph shows no evidence of a jump at the discontinuity in the urban childhood subsample; this result is not surprising, given that the average number of years of female schooling for women raised in urban areas before the reform was already more than eight. As a consequence, the reform had a significant impact only on the schooling of women who grew up in rural regions in Turkey, with no significant effect on the schooling of women raised in urban regions.

TABLE 3—RD TREATMENT EFFECTS ON SCHOOLING BY REGION OF CHILDHOOD

Outcome	Linear RD \hat{h} bandwidth (1)	Quadratic RD \hat{h} bandwidth (2)	Linear RD $\hat{h}/2$ bandwidth (3)	Linear RD $2\hat{h}$ bandwidth (4)	Bandwidth (5)	Observations (6)	Mean (7)
<i>Panel A. Rural childhood region</i>							
Years of schooling	1.829 (0.371)	1.742 (0.570)	2.168 (0.522)	1.713 (0.318)	74	1,333	7.59
Completed education:							
Junior high school	0.342 (0.053)	0.300 (0.087)	0.356 (0.078)	0.339 (0.046)	100	1,753	0.48
<i>Panel B. Urban childhood region</i>							
Years of schooling	0.614 (0.420)	0.218 (0.559)	0.423 (0.650)	0.804 (0.344)	60	941	9.50
Completed education:							
Junior high school	0.093 (0.057)	0.036 (0.080)	0.072 (0.080)	0.092 (0.044)	76	1,191	0.75

Notes: The sample includes women who have had a relationship. Columns 1 and 2 report local RD regressions with linear and quadratic polynomials in the month-year of birth using the optimal bandwidth \hat{h} , and columns 3 and 4 report local RD regressions with linear polynomials in the month-year of birth using the optimal bandwidth $\hat{h}/2$ and $2\hat{h}$, respectively. The optimal bandwidth, reported in column 5, is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 6 reports the number of observations used in estimations with the optimal bandwidth \hat{h} , and column 7 reports the outcome mean within the optimal bandwidth \hat{h} . Panel A reports the results for the sample of women who grew up in a rural area, and panel B reports the results for the sample of women who grew up in an urban area. The variable definitions are listed in online Appendix A. All specifications control for dummy variables for whether the respondent lives in a village, a dummy variable for whether the respondent's interview language is not Turkish, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level.

Source: 2008 National Survey on Domestic Violence against Women in Turkey

Although the RD graphs in Figure 6 illustrate the rural-urban disparity in the reform's effects on schooling, there is room for a more refined analysis. Table 3 reports the RD treatment effects on the years of schooling and junior high school completion of women raised in rural areas in panel A and the effects for women raised in urban areas in panel B. The linear RD estimate in the first row of panel A and column 1 shows that the reform had a positive effect of 1.8 percentage points on years of schooling for women raised in rural areas. This effect corresponds to a 24 percent increase relative to the mean. The RD estimates in the alternative specifications in columns 2 to 4 remain highly significant, ranging from 1.7 to 2.2 percentage points. In contrast, the linear RD estimate in column 1 of panel B reveals no significant impact of the reform on years of schooling for women who spent their childhood in urban regions. The RD estimates in the other columns remain insignificant, except for the linear RD estimate, with twice the optimal bandwidth, which is likely the result of an artificially large bandwidth that covers observations with much lower values from the left side of the discontinuity. Similarly, we find evidence of a rural-urban difference in the reform's effect on junior high school completion. The linear RD estimate in the second row of panel A in column 1 shows that the reform had a positive effect of 34 percentage points on the likelihood of completing junior high school, corresponding to a 71 percent increase relative to the mean. In contrast, the linear RD estimate in column 1 of panel B shows no evidence

of a significant effect on junior high school completion for women raised in urban areas. Alternative RD estimates remain insignificant, except for the linear RD estimate with twice the optimal bandwidth, which is again likely the result of having an artificially large bandwidth. The point estimates in panel B are also much lower than those in panel A for both education outcomes.

As a robustness check, Table A4 in online Appendix B reports the RD estimates using a static bandwidth of 61 months around the cutoff, which is the optimal bandwidth estimated for the sample of women who have ever had a relationship. The findings in this table are similar to those in Table 3. Whereas the RD treatment effects on the years of schooling and junior high school completion of women raised in rural regions are statistically significant and large, those for women from urban regions are insignificant and much smaller. The differences in estimates are statistically significant for both specifications with linear and quadratic control functions.

Since the findings on education indicate that the main compliers with the reform have been women raised in rural regions of Turkey, in the remainder of the analysis, we focus on results for the overall sample and the sample of women who grew up in rural regions.¹⁵

B. Education and Domestic Violence

In this section, we test whether the reform had a significant impact on domestic violence outcomes. Panels B, C, and D of Figure 6 provide a graphical illustration of the RD treatment effects on physical violence, sexual violence, and psychological violence indices. We expect the reform to have a larger impact on women raised in rural regions because the reform had a great impact on the years of schooling for these women, in contrast to no significant impact on the same outcome for women raised in urban areas. At the discontinuity, we see no evidence of a significant jump in physical violence experienced by women raised in rural or urban areas. However, in the bottom panels of Figure 6, we see some evidence of a jump in sexual and psychological violence, but the jump appears significant only for psychological violence experienced by women raised in rural areas. In contrast, the right-side graph for women from urban areas in panel D shows no evidence of a significant jump at the discontinuity.

To examine the effects of the reform on domestic violence outcomes in a more refined analysis, we provide the results of RD regressions in Table 4. The OLS estimates in column 1 indicate the presence of a negative correlation between years of schooling and the physical violence index, sexual violence index, psychological violence index, and financial control index. The magnitudes of the correlations suggest that one additional year of schooling corresponds to 2.8 percentage points less physical violence, 1.5 percentage points less sexual violence, 2.2 percentage points less psychological violence, and 1.7 percentage points less financial control exercised by the intimate partner. Column 4 illustrates that these negative correlations between domestic violence and years of schooling are also evident in the rural sample.

¹⁵The results for women raised in urban regions are available from the authors upon request.

TABLE 4—EDUCATION EFFECTS ON DOMESTIC VIOLENCE OUTCOMES BY CHILDHOOD REGION

Outcome	Overall sample			Rural sample		
	OLS (1)	RF (2)	IV (3)	OLS (4)	RF (5)	IV (6)
Physical violence index	-0.028 (0.004)	0.036 (0.061)	0.034 (0.051)	-0.027 (0.005)	0.033 (0.063)	0.013 (0.035)
Mean	-0.19	-0.19	-0.19	-0.15	-0.15	-0.15
Bandwidth	63	63	63	92	92	92
Observations	2,123	2,236	2,123	1,606	1,737	1,606
Sexual violence index	-0.015 (0.005)	0.047 (0.063)	0.033 (0.052)	-0.021 (0.004)	0.080 (0.063)	0.029 (0.037)
Mean	-0.12	-0.12	-0.12	-0.09	-0.09	-0.09
Bandwidth	59	59	59	140	140	140
Observations	2,007	2,115	2,007	2,332	2,539	2,332
Psychological violence index	-0.022 (0.004)	0.037 (0.045)	0.022 (0.032)	-0.012 (0.005)	0.123 (0.057)	0.062 (0.037)
Mean	0.03	0.03	0.03	0.02	0.02	0.02
Bandwidth	82	82	82	75	75	75
Observations	2,751	2,904	2,751	1,357	1,462	1,357
Financial control index	-0.017 (0.005)	0.122 (0.080)	0.092 (0.062)	-0.007 (0.006)	0.235 (0.123)	0.150 (0.085)
Mean	-0.07	-0.07	-0.07	-0.09	-0.09	-0.09
Bandwidth	110	110	110	71	71	71
Observations	3,191	3,391	3,191	1,175	1,266	1,175

Notes: The sample includes women who have ever had a relationship. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth \hat{h} estimated by the Imbens and Kalyanaraman algorithm. Columns 2–3 report reduced-form RD treatment effects and two-stage least-squares (IV) RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 4–6 report results for the subsample of respondents whose childhood region of residence was rural. The dependent variables are z -score indices constructed from the components of each dimension of domestic violence. The variable definitions are listed in online Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's interview language is not Turkish, month-of-birth fixed effects, and region fixed effects. In the subsamples of childhood residence, specifications also control for whether the respondent lives in a village. Standard errors are clustered at the month-year cohort level.

Source: 2008 National Survey on Domestic Violence against Women in Turkey

The RD results in the first two rows in Table 4 show no evidence of a significant effect of the reform on the physical violence and sexual violence indices.¹⁶ In the subsequent rows, we find that the reform had a significant impact only on the sample

¹⁶Our violence indices combine different forms of violent acts that can potentially vary a great deal in terms of their cruelty by taking a simple average of the z -scores for each domestic violence variable. Thus, by construction, indices we use assign an equal weight to each violent act independent of its level of brutality. Unfortunately, it is not always possible to rank these acts with respect to their gravity. We tried to construct additional indices to capture the severity of domestic violence experienced by inversely weighting the z -score components based on their means. This way, we can give a higher weight to the acts observed less frequently in our dataset assuming that more brutal acts are the least likely ones to occur. The results obtained are very similar and are available upon request.

of women raised in rural regions. The reduced-form RD estimates in column 5 show that the reform had a positive significant impact of 12.3 percentage points on the psychological violence index. The linear IV estimates in column 6 confirm these positive effects, but the estimates are less precise. Finally, the last row shows that the reform had a marginally significant and positive impact of 23.5 percentage points on the financial control index. The IV estimate is consistent with the sharp RD estimate and is also marginally significant. In terms of magnitudes, the RD treatment effects on psychological violence and financial control behavior correspond to 0.25 and 0.33 standard deviations, respectively. Hence, these RD treatment effects for women raised in rural regions, who are the main group affected by the reform, are relatively large in magnitude. Reduced-form RD estimates for both psychological violence and financial control indices are significant at the 12 percent level after we adjust the standard errors for multiple-hypothesis testing.

As a robustness check, Table A5 in online Appendix B reports the RD estimates using a static bandwidth of 61 months around the cutoff, which is the optimal bandwidth estimated for the sample of women who have ever had a relationship. The findings are similar to those in Table 4. The RD estimates remain significant for the psychological violence index for women from rural regions, but they are estimated less precisely for the financial control index.

A related robustness check involves using an alternative optimal bandwidth selection method proposed by Calonico, Cattaneo, and Titiunik (2014)—henceforth, CCT. Table A9 in online Appendix B reports in columns 1–3 the results for the rural sample using the CCT (2014) optimal bandwidth selection method, comparing them with our original results using the Imbens and Kalyanaraman (2009)—henceforth, IK—optimal bandwidth selection in columns 4–5. The coefficient estimates using the CCT bandwidth selectors reported in columns 2 and 3 are similar in magnitude to those reported in columns 4 and 5 using IK bandwidths, although some are less precisely estimated due to the smaller number of observations included in the narrower CCT bandwidths.¹⁷

Altogether, our results do not indicate that the reform improved domestic violence outcomes for women. While we find no evidence of a significant effect on physical or sexual violence experienced by women, we find that the reform had an adverse effect on psychological violence, as well as the financial control behavior that male partners imposed on women raised in rural areas. In the next section, we will shift our focus to potential channels that could explain these adverse effects on psychological violence and financial control experienced by the main group affected by the reform (i.e., women raised in rural areas).

IV. Examining Causal Channels

We now proceed with an examination of potential channels underlying our finding of greater psychological violence and financial control experienced by women

¹⁷Following Card et al. (2015), we omit the regularization term in the bandwidth selectors since regularized selectors provide bandwidths that are too small for our empirical setting. According to Card et al. (2015), omitting the regularization term does not affect the asymptotic properties of the bandwidth selector.

TABLE 5—EDUCATION EFFECTS ON GENDER AND DOMESTIC VIOLENCE ATTITUDES BY CHILDHOOD REGION

Outcome	Overall sample			Rural sample		
	OLS (1)	RF (2)	IV (3)	OLS (4)	RF (5)	IV (6)
A woman should not argue with partner if she disagrees with him	−0.051 (0.004)	−0.012 (0.053)	−0.008 (0.038)	−0.052 (0.005)	−0.058 (0.061)	−0.038 (0.033)
Mean	0.40	0.40	0.40	0.45	0.45	0.45
Bandwidth	72	72	72	83	83	83
Observations	2,453	2,582	2,453	1,489	1,611	1,489
A woman should be able to spend her money as she wills	0.012 (0.004)	−0.035 (0.048)	−0.022 (0.036)	0.017 (0.005)	−0.041 (0.065)	−0.025 (0.037)
Mean	0.68	0.68	0.68	0.65	0.65	0.65
Bandwidth	62	62	62	70	70	70
Observations	2,070	2,181	2,070	1,270	1,362	1,270
Men can beat their partners in certain situations	−0.014 (0.002)	−0.000 (0.027)	−0.002 (0.019)	−0.015 (0.003)	0.004 (0.039)	−0.002 (0.022)
Mean	0.10	0.10	0.10	0.12	0.12	0.12
Bandwidth	73	73	73	63	63	63
Observations	2,479	2,609	2,479	1,124	1,207	1,124

(continued)

raised in rural regions of Turkey and affected by the 1997 education reform. We divide our analysis into three subsections by focusing on the effects of compulsory schooling reform on the following: gender role and domestic violence attitudes, labor market outcomes, and partner and relationship characteristics.

A. Changes in Gender Role and Domestic Violence Attitudes

A potential mechanism underlying the effects that we observe is the reform-induced changes in the gender role and domestic violence attitudes of women. If additional years of schooling alter women's beliefs in greater gender equality in society, this result might make them less tolerant of threats of violence or financial control behavior exercised by male partners to gain control of household resources. The empirical evidence on the effects of compulsory schooling on gender-related attitudes is mixed. Some studies find that increased female schooling improves young women's attitudes toward domestic violence (Friedman et al. 2011), whereas others fail to find any evidence of a significant change in gender attitudes (Gulesci and Meyersson 2012; Dinger, Kaushal, and Grossman 2014).

We examine this potential channel by testing whether the reform-induced increase in female schooling affected the gender role and domestic violence attitudes of women. Our findings are reported in Table 5, where we focus on the probability of whether the respondent agrees with seven statements reflecting her attitudes toward gender roles and domestic violence, and a composite gender attitudes index, in which higher values reflect more gender-equal attitudes. The

TABLE 5—EDUCATION EFFECTS ON GENDER AND DOMESTIC VIOLENCE ATTITUDES BY CHILDHOOD REGION
(continued)

Outcome	Overall sample			Rural sample		
	OLS (1)	RF (2)	IV (3)	OLS (4)	RF (5)	IV (6)
It may be necessary to beat children for discipline	-0.021 (0.005)	-0.030 (0.039)	-0.013 (0.028)	-0.026 (0.004)	0.016 (0.056)	0.015 (0.032)
Mean	0.29	0.29	0.29	0.34	0.34	0.34
Bandwidth	69	69	69	99	99	99
Observations	2,302	2,423	2,302	1,716	1,859	1,716
Men should also do housework, e.g., cooking and cleaning	0.030 (0.003)	0.096 (0.039)	0.081 (0.032)	0.042 (0.006)	0.142 (0.064)	0.083 (0.031)
Mean	0.71	0.71	0.71	0.66	0.66	0.66
Bandwidth	67	67	67	53	53	53
Observations	2,265	2,385	2,265	956	1,027	956
Men in the family are responsible for a woman's behavior	-0.039 (0.003)	-0.092 (0.044)	-0.072 (0.031)	-0.041 (0.005)	-0.062 (0.056)	-0.036 (0.027)
Mean	0.41	0.41	0.41	0.46	0.46	0.46
Bandwidth	114	114	114	95	95	95
Observations	3,570	3,772	3,570	1,648	1,775	1,648
It is a woman's duty to have sexual intercourse with her husband	-0.024 (0.003)	-0.014 (0.045)	-0.002 (0.034)	-0.024 (0.005)	-0.017 (0.064)	-0.004 (0.036)
Mean	0.22	0.22	0.22	0.25	0.25	0.25
Bandwidth	74	74	74	73	73	73
Observations	2,459	2,594	2,459	1,295	1,395	1,295
Gender attitudes index	0.061 (0.004)	0.057 (0.052)	0.042 (0.035)	0.064 (0.007)	0.084 (0.079)	0.044 (0.042)
Mean	0.05	0.05	0.05	-0.03	-0.03	-0.03
Bandwidth	74	74	74	63	63	63
Observations	2,506	2,641	2,506	1,124	1,209	1,124

Notes: The sample includes women who have ever had a relationship. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth \hat{h} estimated by the Imbens and Kalyanaraman algorithm. Columns 2–3 report reduced-form RD treatment effects and two-stage least-squares (IV) RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 4–6 report results for the subsample of respondents whose childhood region of residence was rural. The variable definitions are listed in online Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's interview language is not Turkish, month-of-birth fixed effects, and region fixed effects. In the subsamples of childhood residence, specifications also control for whether the respondent lives in a village. Standard errors are clustered at the month-year cohort level.

Source: 2008 National Survey on Domestic Violence against Women in Turkey

correlations shown in column 1 of Table 5 indicate that more-educated women are more likely to have gender-equal attitudes. In the last row, column 1 shows that the years of schooling is positively correlated with the gender attitudes index; 1 additional year of schooling corresponds to a 6.1 percentage point improvement in gender attitudes.

The RD estimates for the treatment effect on the gender attitudes index are presented in columns 2–7 in the last row of Table 5. We find no evidence that the reform had a significant impact on women’s gender attitudes. More importantly, in the third row of column 2, the linear RD treatment effect on attitudes toward domestic violence against women—the statement that men can beat their partners in certain situations—is zero and insignificant. The IV estimate in column 3 confirms that there is no evidence of a significant effect on women’s attitudes toward domestic violence against women. Furthermore, the fourth row reveals no evidence that the reform significantly affected agreement with the statement that it may be necessary to beat children for discipline.

We find evidence of significant treatment effects for only two gender role attitudes: men should also do housework, such as cooking and cleaning, and men in the family are responsible for a woman’s behavior. In the fifth row of Table 5, the linear RD estimate in column 2 shows a significant treatment effect of 9.6 percentage points on the probability of agreeing with the statement that men should also do housework, such as cooking and cleaning, corresponding to a 14 percent increase relative to the sample mean. These treatment effects in the overall sample are driven primarily by the large treatment effects for women raised in rural areas.¹⁸ In the sixth row, the linear RD estimate in column 2 shows a negative treatment effect of 9.2 percentage points on the probability of agreeing with the statement that men in the family are responsible for a woman’s behavior.

Overall, we find no evidence of a consistent effect of the reform in improving gender role and domestic violence attitudes. All of the RD estimates for women raised in rural areas, the main complier group and the group that faced a significant increase in the incidence of psychological violence and financial control behavior, are insignificant with respect to overall gender attitudes (last row, columns 5–6). Of the seven gender-related attitudes that we examine, we find only one significant change reported for the attitude toward a more equal division of labor within the household for these women (fifth row, columns 5–6).¹⁹ The attitude channel does not appear to explain our main results.²⁰

¹⁸For these women, column 5 shows a reduced-form RD treatment effect of 14.2 percentage points on the probability of agreeing with the statement that men should also do housework, such as cooking and cleaning, corresponding to a 22 percent increase relative to the sample mean.

¹⁹As a robustness check, Table A6 in online Appendix B reports the RD estimates using a static bandwidth of 61 months around the cutoff, which is the optimal bandwidth estimated for the sample of women who have ever had a relationship. The findings are similar to those in Table 5. The RD estimates are significant for agreeing with a more equal division of labor and disagreeing with the statement that men in the family are responsible for a woman’s behavior. We find no evidence of a significant effect on other gender and domestic violence attitudes.

²⁰Notably, more-educated women may report the incidence of domestic violence less or more frequently, generating a reporting bias in our main dependent variables, such as the psychological violence index. For instance, if education increases women’s awareness of gender equality, we might expect them to report higher levels of psychological violence even if they are subject to the same levels of violence as a less-educated woman. If such a bias were present, we would also expect more-educated women to report an improvement in their gender attitudes. The fact that we found no evidence of such a change suggests that our results for psychological violence and financial control behavior are not driven by a reporting bias in the outcome variables. However, it is important to note that it is still possible that although more- and less-educated women may agree on the ways a woman should be treated, more-educated women might have different views on what constitutes a violent act and report accordingly. Unfortunately, it is not possible for us to account for such a divergence.

B. Changes in Labor Market Outcomes

Another possible explanation for the adverse effects of the reform on the psychological violence and financial control behavior experienced by those who complied with the reform is that the reform improved the labor market outcomes of women. If the additional years of schooling allowed women to increase their probability of being employed and having a higher income, then the women's increased access to resources might increase the incentives of men to use threats of violence or to take women's income without their approval (i.e., financial control behavior) to extract rents from these newly obtained resources (McCloskey 1996, Bloch and Rao 2002). This behavior could, in turn, result in a higher incidence of psychological violence or financial control behavior experienced by these women.

We explore this mechanism by testing whether the additional years of schooling caused by the reform had an impact on labor market outcomes in Turkey.²¹ Panel B of Figure 5 provides a comparison of the treatment and placebo effects using the 2008 TNSDVW survey and the 2014 HLFS for the outcome of the propensity to work. Using the 2008 TNSDVW survey, the left-side graph plots the average propensity to work in monthly bins against the month and year of birth, with a cutoff of January 1987. It shows a clear jump at the discontinuity in the probability of being employed. We use data from the 2014 HLFS to conduct a placebo test to further confirm the RD design. The right-side graph in panel B of Figure 5 shows the same relationship using the 2014 HLFS survey, in which the age cutoff is the same age, comparing 21- and 22-year-old women. The result shows no evidence of a jump in the propensity to work among women within the same age group in the 2014 HLFS. We examine RD treatment effects more parsimoniously using regression analysis.

Table 6 provides RD estimates of the reform's effect on labor market outcomes. Column 1 reports ordinary least squares (OLS) regressions of the outcome variables on years of schooling for observations within the optimal bandwidth as a reference point. We observe a positive correlation between years of schooling and being employed, working in the nonagricultural sector, working in services, having social security, and having a higher personal income. By contrast, we find a negative correlation between working in the agricultural sector and years of schooling. For example, 1 additional year of schooling is associated with a 1.3 percentage point increase in being employed and a 1.7 percentage point increase in working in services.

The reduced-form RD estimate in the first row of Table 6 in column 2 indicates that the reform had a positive effect of 5.6 percentage points on the likelihood of being employed, which corresponds to a 40 percent increase relative to the sample mean. The IV estimate in column 3 is also precisely estimated and confirms this effect. These results are driven by the effects in the rural sample. The reduced-form RD estimate in column 5 shows that, for women raised in rural regions, the reform led to a 8.2 percentage point increase in being employed. Similarly, the IV estimate

²¹ Unfortunately, labor market outcomes are all measured for the seven days prior to the survey date. However, our domestic violence measures capture a much longer time span. Therefore, our findings in this subsection can be regarded as evidence suggestive of this potential channel.

TABLE 6—EDUCATION EFFECTS ON LABOR MARKET OUTCOMES BY CHILDHOOD REGION

Outcome	Overall sample			Rural sample		
	OLS (1)	RF (2)	IV (3)	OLS (4)	RF (5)	IV (6)
Employed	0.013 (0.003)	0.056 (0.026)	0.036 (0.018)	0.006 (0.004)	0.082 (0.031)	0.045 (0.020)
Mean	0.14	0.14	0.14	0.14	0.14	0.14
Bandwidth	91	91	91	78	78	78
Observations	2,998	3,170	2,998	1,391	1,499	1,391
Employed in non-agriculture	0.015 (0.003)	0.054 (0.025)	0.038 (0.019)	0.013 (0.004)	0.096 (0.031)	0.052 (0.019)
Mean	0.11	0.11	0.11	0.09	0.09	0.09
Bandwidth	101	101	101	96	96	96
Observations	3,246	3,437	3,246	1,704	1,843	1,704
Employed in services	0.017 (0.002)	0.027 (0.020)	0.020 (0.016)	0.010 (0.003)	0.077 (0.030)	0.043 (0.019)
Mean	0.10	0.10	0.10	0.07	0.07	0.07
Bandwidth	143	143	143	84	84	84
Observations	4,311	4,588	4,311	1,492	1,615	1,492
Employed in agriculture	-0.003 (0.001)	-0.009 (0.011)	-0.007 (0.008)	-0.003 (0.003)	-0.010 (0.022)	-0.007 (0.013)
Mean	0.03	0.03	0.03	0.05	0.05	0.05
Bandwidth	97	97	97	63	63	63
Observations	3,174	3,359	3,174	1,124	1,209	1,124
Social security	0.019 (0.002)	0.024 (0.023)	0.017 (0.016)	0.016 (0.003)	0.036 (0.021)	0.021 (0.011)
Mean	0.07	0.07	0.07	0.05	0.05	0.05
Bandwidth	120	120	120	99	99	99
Observations	3,792	4,027	3,792	1,742	1,886	1,742
Personal income index	0.024 (0.003)	0.083 (0.043)	0.057 (0.031)	0.022 (0.004)	0.116 (0.044)	0.065 (0.027)
Mean	-0.07	-0.07	-0.07	-0.08	-0.08	-0.08
Bandwidth	98	98	98	112	112	112
Observations	3,194	3,381	3,194	1,918	2,083	1,918

Notes: The sample includes women who have ever had a relationship. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth \hat{h} estimated by the Imbens and Kalyanaraman algorithm. Columns 2–3 report reduced-form RD treatment effects and two-stage least-squares (IV) RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 4–6 report results for the subsample of respondents whose childhood region of residence was rural. The variable definitions are listed in online Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's interview language is not Turkish, month-of-birth fixed effects, and region fixed effects. In the subsamples of childhood residence, specifications also control for whether the respondent lives in a village. Standard errors are clustered at the month-year cohort level.

Source: 2008 National Survey on Domestic Violence against Women in Turkey

in column 6 is positive and significant. The treatment effect corresponds to a sizable impact of 59 percent relative to the rural sample mean.

Column 2 in the second row of Table 6 shows that the reform had a significant positive effect of 5.4 percentage points on working in the nonagricultural sector, indicating a 49 percent increase relative to the sample mean. The IV estimate is also significant and positive. The reduced-form RD estimate in column 5 shows that the reform had a large effect of 9.6 percentage points on working in the nonagricultural sector for women raised in rural areas, where the reform had a particularly strong effect. The IV estimate is also significant and positive.

In the third row of Table 6, column 5 shows that the reform had a significant positive impact of 7.7 percentage points on working in services among women raised in rural areas. The IV estimate in column 6 confirms this positive effect. The RD estimates of the reform's effect on agricultural sector employment are all insignificant and close to zero. The fifth row of Table 6 shows that the reform had a marginally significant positive impact of 3.6 percentage points on the probability of women raised in rural areas having social security, corresponding to a 72 percent increase relative to the rural sample mean. We find no evidence of a significant effect on the overall sample.

Finally, in the last row of Table 6, we report RD treatment effects on the personal income index, as described in Section III. The RD estimate in column 2 reveals a marginally significant treatment effect of 8.3 percentage points for the overall sample, which is again driven by the large effects for women raised in rural regions. The RD estimate in column 5 shows a highly significant treatment effect of 11.6 percentage points for women raised in rural areas. The IV estimates in the overall and rural samples are also precisely estimated.

As a robustness check, Table A7 in online Appendix B reports the RD estimates using a static bandwidth of 61 months around the cutoff, which is the optimal bandwidth estimated for the sample of women who have ever had a relationship. The findings in the table show that the RD estimates for being employed, being employed in the nonagricultural sector, and the personal income index are robust to this specification. The sharp RD results for being employed in services among women raised in rural areas remains significant, while the fuzzy RD estimate is less precisely estimated. The RD results for women raised in rural areas to have social security are also imprecisely estimated.

Overall, these findings imply that, 10 years after the implementation of the eight-year compulsory schooling reform in Turkey, women who attained a higher level of education because of the reform are more likely to work in the nonagricultural sector, particularly in services. As a result, these women are more likely to have social security, which is particularly the case for those who grew up in rural areas, and to earn a higher personal income. Because the reform had a large impact on the schooling outcomes of women raised in rural areas but had little impact on those from urban areas, the reform also had a large impact on the labor market outcomes of women raised in rural areas.

Altogether, these findings indicate that the additional reform-induced educational attainment of women who grew up in rural areas allowed them to work in the nonagricultural sector and provided them with higher personal income. Because we

also find evidence of a higher level of psychological violence and financial control experienced by these women, this result could be interpreted as suggestive evidence supporting instrumental theories of domestic violence. The positive female income generated through the labor market channel may create incentives for the male partner to extract rents from the female partner by using coercive instruments, such as threats of violence and other controlling behavior to gain more influence over the decision-making process in the allocation of household resources.²² In particular, our finding that the male partner exerts more financial control at the discontinuity implies that the male partner has a motivation to extract rents by either taking her income or refusing to contribute to household spending.²³

C. Changes in Partner and Relationship Characteristics

Increased female schooling could change the profile of women's partners. An increase in female education could allow a better match with a "higher-quality" partner who is either more educated or more likely to have stable employment, which could, in turn, affect the incidence of domestic violence faced by the female partner.

We explore this mechanism by testing whether the additional years of female schooling induced by the reform had an effect on partner and relationship characteristics. In Table 7, the OLS estimates show that a woman's years of schooling are positively correlated with her marriage age, her ability to decide who to marry herself (as opposed to her family), her partner's schooling, an index of household assets, and her husband's age, while they are negatively correlated with whether her partner is employed, the difference in years of schooling between partners, an index of her husband's level of religiosity, whether the partner had witnessed violence toward his mother, and the probability of ever having divorced or ever having a second marriage.

The RD estimates reported in columns 2–3 and 4–5 of Table 7 show that the RD estimates are insignificant for all outcomes except the difference in years of schooling between partners and the asset ownership index. The reduced-form RD estimate in column 2 indicates that the reform led to a decline in the schooling gap between female and male partners by about 1 year. The RD estimate in column 5 indicates

²²We also examined whether our results on psychological violence could be driven by men being jealous of the improved status of women within the household. Our survey does not include information on the attitudes of men towards gender equality and violence. However, we have information provided by women about the source of conflict with their partners, including whether the conflict resulted from men being jealous. These questions related to the source of conflict were answered only by women who experienced physical violence, which leaves us with a small and selective sample. We present the results with this sample as suggestive evidence. In online Appendix B, Table A12 shows that there is no evidence that the reform had a significant impact on the probability of men being jealous.

²³Previous studies have also shown that the relative incomes of women play a key role in determining bargaining power, levels of domestic violence, and marriage satisfaction (Aizer 2010; Bertrand, Kamenica, and Pan 2015). Aizer (2010) finds evidence that a decline in the gender wage gap reduces domestic violence against women by improving their bargaining power within the household. Bertrand, Kamenica, and Pan (2015) find that, when a married woman has a higher relative income, she spends more time on household chores and is less satisfied with her marriage and more likely to divorce. This evidence indicates that one way of testing the effect of the income channel in our empirical setup would be to control for the relative income of women while estimating the effects of the reform on psychological violence and financial control behavior. Unfortunately, our dataset does not have actual income variables for any of the partners, and we have no other variables that would allow us to construct a relative income measure. For this reason, we cannot directly test for this effect.

TABLE 7—EDUCATION EFFECTS ON PARTNER AND RELATIONSHIP CHARACTERISTICS BY CHILDHOOD REGION

Outcome	Overall sample			Rural sample		
	OLS (1)	RF (2)	IV (3)	OLS (4)	RF (5)	IV (6)
Marriage age	0.233 (0.029)	0.014 (0.223)	0.033 (0.231)	0.203 (0.037)	-0.067 (0.326)	-0.046 (0.285)
Mean	20.23	20.23	20.23	20.25	20.25	20.25
Bandwidth	68	68	68	79	79	79
Observations	1,564	1,673	1,564	1,123	1,229	1,123
Marriage decision	0.041 (0.004)	0.031 (0.048)	0.018 (0.043)	0.040 (0.005)	-0.023 (0.063)	-0.036 (0.048)
Mean	0.56	0.56	0.56	0.50	0.50	0.50
Bandwidth	88	88	88	94	94	94
Observations	2,122	2,277	2,122	1,355	1,488	1,355
Partner is employed	-0.028 (0.004)	-0.022 (0.038)	-0.021 (0.027)	-0.026 (0.005)	-0.007 (0.052)	-0.005 (0.027)
Mean	0.84	0.84	0.84	0.88	0.88	0.88
Bandwidth	53	53	53	46	46	46
Observations	1,772	1,864	1,772	819	880	819
Partner's schooling	0.597 (0.017)	0.325 (0.299)	0.335 (0.204)	0.513 (0.034)	0.138 (0.452)	0.149 (0.247)
Mean	9.04	9.04	9.04	8.54	8.54	8.54
Bandwidth	137	137	137	81	81	81
Observations	4,121	4,343	4,121	1,415	1,513	1,415
Schooling difference between partners	-0.444 (0.025)	-1.022 (0.348)	-0.811 (0.243)	-0.498 (0.038)	-1.563 (0.435)	-0.881 (0.248)
Mean	0.88	0.88	0.88	1.08	1.08	1.08
Bandwidth	65	65	65	66	66	66
Observations	2,160	2,160	2,160	1,160	1,160	1,160
Age difference between partners	-0.032 (0.042)	0.169 (0.411)	0.064 (0.366)	-0.015 (0.051)	0.346 (0.539)	0.108 (0.430)
Mean	4.01	4.01	4.01	3.95	3.95	3.95
Bandwidth	81	81	81	81	81	81
Observations	1,907	2,042	1,907	1,157	1,265	1,157
Husband's age	0.278 (0.033)	0.128 (0.384)	0.085 (0.351)	0.216 (0.043)	0.368 (0.513)	0.232 (0.432)
Mean	24.45	24.45	24.45	24.27	24.27	24.27
Bandwidth	89	89	89	108	108	108
Observations	2,146	2,299	2,146	1,567	1,718	1,567
Husband's religiosity index	-0.008 (0.006)	-0.111 (0.071)	-0.082 (0.056)	0.002 (0.008)	-0.074 (0.054)	-0.034 (0.032)
Mean	-0.02	-0.02	-0.02	0.03	0.03	0.03
Bandwidth	60	60	60	75	75	75
Observations	1,998	2,106	1,998	1,332	1,436	1,332

(continued)

TABLE 7—EDUCATION EFFECTS ON PARTNER AND RELATIONSHIP CHARACTERISTICS BY CHILDHOOD REGION
(continued)

Outcome	Overall sample			Rural sample		
	OLS (1)	RF (2)	IV (3)	OLS (4)	RF (5)	IV (6)
Partner witnessed violence towards his mother	−0.018 (0.004)	−0.002 (0.050)	−0.019 (0.033)	−0.010 (0.005)	0.031 (0.065)	−0.005 (0.038)
Mean	0.31	0.31	0.31	0.31	0.31	0.31
Bandwidth	97	97	97	105	105	105
Observations	2,354	2,490	2,354	1,360	1,473	1,360
Partner experienced violence from his family members	0.005 (0.004)	−0.067 (0.054)	−0.043 (0.033)	−0.003 (0.007)	−0.056 (0.067)	−0.028 (0.042)
Mean	0.74	0.74	0.74	0.75	0.75	0.75
Bandwidth	86	86	86	60	60	60
Observations	2,354	2,478	2,354	910	978	910
Ever divorced	−0.002 (0.001)	0.006 (0.007)	0.004 (0.005)	−0.002 (0.001)	0.011 (0.011)	0.006 (0.007)
Mean	0.01	0.01	0.01	0.01	0.01	0.01
Bandwidth	87	87	87	99	99	99
Observations	2,891	3,056	2,891	1,725	1,868	1,725
Had a second marriage	−0.001 (0.000)	0.004 (0.006)	0.003 (0.005)	−0.000 (0.000)	0.001 (0.007)	0.000 (0.004)
Mean	0.01	0.01	0.01	0.01	0.01	0.01
Bandwidth	62	62	62	90	90	90
Observations	2,105	2,218	2,105	1,589	1,716	1,589
Asset ownership index	0.047 (0.003)	0.104 (0.038)	0.072 (0.024)	0.045 (0.003)	0.076 (0.044)	0.047 (0.023)
Mean	0.10	0.10	0.10	0.01	0.01	0.01
Bandwidth	92	92	92	143	143	143
Observations	3,023	3,197	3,023	2,396	2,613	2,396

Notes: The sample includes women who have ever had a relationship. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth \hat{h} estimated by the Imbens and Kalyanaraman algorithm. Columns 2–3 report reduced-form RD treatment effects and two-stage least-squares (IV) RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 4–6 report results for the subsample of respondents whose childhood region of residence was rural. The variable definitions are listed in online Appendix A. All specifications control for dummy variables for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's interview language is not Turkish, month-of-birth fixed effects, and region fixed effects. In the subsamples of childhood residence, specifications also control for whether the respondent lives in a village. Standard errors are clustered at the month-year cohort level.

Source: 2008 National Survey on Domestic Violence against Women in Turkey

that the treatment effect is particularly large among women who grew up in rural areas (1.56 years). The IV estimates in columns 3 and 6 are also precisely estimated and negative. The schooling gap declined because the reform led to an increase in female schooling and no change in male partners' schooling, as shown in the fourth row estimates of Table 7. On the other hand, the RD estimate in column 2 reveals a

highly significant treatment effect of 10.4 percentage points on the asset ownership index. The IV estimate confirms this positive and significant effect. The reduced-form RD estimate in column 4 shows a treatment effect of 7.6 percentage points for women raised in rural areas. The IV estimate is also significant and positive. Because the asset ownership index represents the assets owned by the household, this reform-induced positive impact could reflect the additional personal income earned by the female partner, as shown in Table 6.

We find no evidence that the reform had a significant impact on the marriage age of the woman,²⁴ the husband's age, the age difference between husband and wife, and other predetermined outcomes related to the partner, including a proxy measure of religiosity.²⁵ In addition, as Table 7 shows, we find no evidence that the reform had an effect on the probability of divorcing or having a second marriage. The incidence of these outcomes is also very low (around 1 percent) in our sample, which is composed of young women ages 16 to 26. Overall, we conclude that the reform did not have any meaningful effects on partner and relationship characteristics, with the exception of schooling difference between partners, which is due to the increase in female schooling instead of partners' schooling, and asset ownership, which could, in fact, result from the positive impact on female personal income.²⁶

As a robustness check, Table A8 in online Appendix B reports the RD estimates using a static bandwidth of 61 months around the cutoff, which is the optimal bandwidth estimated for the sample of women who have ever had a relationship. The findings in the table closely match the findings in Table 7.

We use an alternative optimal bandwidth selection method proposed by Calonico, Cattaneo, and Titiunik (2014) as an additional robustness check of whether the main outcome variables used in the analysis of channels are sensitive to the use of this alternative method. Table A10 in online Appendix B shows that the RD treatment effect estimates using the CCT bandwidth selectors reported in columns 2 and 3 are

²⁴The estimated effect of the reform on marriage age has been mixed in the literature. Güneş (2016) finds an increase in the marriage age, while Gulesci and Meyersson (2012) find no evidence that there has been an increase in marriage age, similar to our paper. The differences in empirical methodologies (IV using the teacher per child ratio as an instrument in Güneş 2016, as opposed to the RD design using the month of birth cutoff in our paper and Gulesci and Meyersson's 2012 paper) could be one reason for differences in results.

²⁵The latter is a z -score calculated as an average of z -scores of partners' characteristics, including a dummy variable that takes the value of one if the partner never drinks alcoholic beverages, a dummy variable that takes the value of one if the partner never gambles, a dummy variable that takes the value of one if the partner never uses narcotic drugs, and a dummy variable that takes the value of one if the partner never had an affair. Since Islam prohibits these behaviors by categorizing them as a sin, individuals with high degrees of religious beliefs are very unlikely to exhibit these behaviors. We find no evidence that the reform had a significant impact on husbands' religiosity. Although previous studies have similarly found no change in men's level of religiosity as a result of the reform (Cesur and Mocan 2013), they also showed that it resulted in a decline in women's level of religiosity (Gulesci and Meyersson 2012, Cesur and Mocan 2013). The combination of these results would suggest that the reform changed the devoutness of men and women differentially and this could be another source of marital conflict.

²⁶We also examined the effects of the reform on childbearing decisions. Table A13 in online Appendix B indicates that the reform led to an increase in the use of contraceptive methods, a decline in the probability of giving birth, and a decline in the number of children that women had. We also find no evidence that the reform had a significant effect on the probability that the partner disapproves of the use of contraception. Hence, although the reform had a significant negative impact on fertility, there is no evidence that it led to intra-household conflict due to the increased use of contraception and lower likelihood of having children. Moreover, according to Table A12 in online Appendix B, we find no evidence that the reform had a significant effect on the probability of intra-household conflict resulting from issues related to children. Thus, there is no evidence to suggest that the reform led to more conflict over childrearing or the stress associated with it.

similar in magnitude and statistical significance to those reported in columns 4 and 5 using IK bandwidths.

V. Conclusion

The main objective of this paper is to provide evidence of the effect of a change in compulsory schooling that exogenously increased the average amount of schooling on the prevalence of domestic violence in Turkey. Using an RD design allows us to estimate the causal effects of the education reform on domestic violence measures and related outcomes. Previous studies have focused on basic correlations between education levels and domestic violence risk, but such correlations are likely to suffer from omitted variable bias, given that unobservables such as ability, socioeconomic status, and upbringing might affect both years of schooling and domestic violence risk. The key contribution of this paper is the assessment of the effect of an exogenous increase in education on different dimensions of intimate partner violence in a developing-country context with a high prevalence of domestic violence and relatively low levels of women's empowerment.

We find that the reform led to an average increase of one year to one-and-a-half years of additional schooling for women, and this increase had significant positive effects on women's empowerment through the labor market channel. Women in the treated cohorts were more likely to be employed in the last week, particularly in the nonagricultural sector, to have a higher personal income, and to have access to social security benefits. We find that the main group affected by the reform was women who grew up in rural regions with lagging schooling infrastructure and conservative norms against female schooling. The reform led to an increase of 1.8 years of schooling for these women. By contrast, we find no evidence of a significant impact on the education of women raised in urban areas, who had attained an average of eight years of schooling prior to the reform. In parallel, we find larger effects on the employment outcomes and incomes of women raised in rural areas.

Our findings reveal that the reform had an adverse effect on the psychological violence and financial control behavior experienced by women raised in rural areas. In contrast, we find no evidence of a significant impact on physical or sexual violence against these women. We also investigate whether the reform-induced increase in years of schooling, particularly in rural areas, led to an improvement in women's attitudes or in their partners' characteristics. We find no evidence of a significant impact on domestic violence attitudes. Moreover, we find no evidence of a significant impact on the quality of the partner, including his schooling, his employment, and his past exposure to violence toward his mother or toward himself. We also find no evidence of a significant impact with respect to marriage decision rights or the age of marriage for women who had ever been married.

Our results may be interpreted as suggestive evidence for instrumental theories of violence. In particular, our findings imply that the additional years of schooling induced by the reform benefited women through the labor market channel, allowing them to attain higher personal income. This unexpected change in women's employment and income might have increased the incentives of male partners to use threats of violence and other financial control measures to extract rents from female

partners and to regain control of household decision making as indicated by our analysis. However, we cannot rule out the possibility that increases in self-reported psychological violence and financial control behavior are consequences of men's preferences that their partners not participate in paid employment or of the declining religiosity of women in comparison to their partners.

One limitation of our results is that we document the short- to medium-term impact of the reform among women who were between 16 and 26-years-old in 2008, as a large proportion of these relatively young women had not had a long-term partner at the time of the survey. The long-term effects of the reform may be greater than those measured at this relatively young age.

Overall, our findings suggest that the expansion of compulsory schooling in Turkey had significant economic empowerment effects through the labor market channel. However, this economic empowerment did not translate into empowerment within the household. The evidence implies a mixed view of the effectiveness of broad expansions of compulsory schooling in a context with significant limitations on women's rights to achieve empowerment in several dimensions, one of which is the reduction in domestic violence against women.

REFERENCES

- Ackerson, Leland K., Ichiro Kawachi, Elizabeth M. Barbeau, and S. V. Subramanian.** 2008. "Effects of Individual and Proximate Educational Context on Intimate Partner Violence: A Population-Based Study of Women in India." *American Journal of Public Health* 98 (3): 507–14.
- Aizer, Anna.** 2010. "The Gender Wage Gap and Domestic Violence." *American Economic Review* 100 (4): 1847–59.
- Aizer, Anna.** 2011. "Poverty, Violence, and Health: The Impact of Domestic Violence During Pregnancy on Newborn Health." *Journal of Human Resources* 46 (3): 518–38.
- Anderberg, Dan, Helmut Rainer, Jonathan Wadsworth, and Tanya Wilson.** 2016. "Unemployment and Domestic Violence: Theory and Evidence." *Economic Journal* 126 (597): 1947–79.
- Angelucci, Manuela.** 2008. "Love on the Rocks: Domestic Violence and Alcohol Abuse in Rural Mexico." *B.E. Journal of Economic Analysis and Policy* 8 (1): Article 29.
- Angrist, Joshua D., and Alan B. Krueger.** 1991. "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics* 106 (4): 979–1014.
- Bertrand, Marianne, Emir Kamenica, and Jessica Pan.** 2015. "Gender Identity and Relative Income within Households." *Quarterly Journal of Economics* 130 (2): 571–614.
- Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes.** 2008. "Staying in the Classroom and out of the maternity ward? The effect of compulsory schooling laws on teenage births." *Economic Journal* 118 (530): 1025–54.
- Bloch, Francis, and Vijayendra Rao.** 2002. "Terror as a Bargaining Instrument: A Case Study of Dowry Violence in Rural India." *American Economic Review* 92 (4): 1029–43.
- Bobonis, Gustavo J., Melissa Gonzalez-Brenes, and Roberto Castro.** 2013. "Public Transfers and Domestic Violence: The Roles of Private Information and Spousal Control." *American Economic Journal: Economic Policy* 5 (1): 179–205.
- Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik.** 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82 (6): 2295–2326.
- Campbell, Jacquelyn C.** 2002. "Health consequences of intimate partner violence." *Lancet* 359 (9314): 1331–36.
- Card, David, David S. Lee, Zhuan Pei, and Andrea Weber.** 2015. "Inference on Causal Effects in a Generalized Regression Kink Design." *Econometrica* 83 (6): 2453–83.
- Carlson, Bonnie E.** 2000. "Children Exposed to Intimate Partner Violence: Research Findings and Implications for Intervention?" *Trauma, Violence, and Abuse* 1 (4): 321–42.
- Carrell, Scott E., and Mark L. Hoekstra.** 2010. "Externalities in the Classroom: How Children Exposed to Domestic Violence Affect Everyone's Kids." *American Economic Journal: Applied Economics* 2 (1): 211–28.

- Cesur, Resul, and Naci H. Mocan. 2013. "Does Secular Education Impact Religiosity, Electoral Participation and the Propensity to Vote for Islamic Parties? Evidence from an Education Reform in a Muslim Country." National Bureau of Economic Research (NBER) Working Paper 19769.
- Clark, Damon, and Heather Royer. 2013. "The Effect of Education on Adult Mortality and Health: Evidence from Britain." *American Economic Review* 103 (6): 2087–2120.
- Department of Health and Human Services. 2003. *Costs of Intimate Partner Violence Against Women in the United States*. Centers for Disease Control and Prevention and National Center for Injury Prevention and Control. Atlanta, March.
- Dinçer, Mehmet Alper, Neeraj Kaushal, and Michael Grossman. 2014. "Women's Education: Harbinger of Another Spring? Evidence from a Natural Experiment in Turkey." *World Development* 64: 243–58.
- Doepke, Matthias, and Michèle Tertilt. 2009. "Women's Liberation: What's in It for Men?" *Quarterly Journal of Economics* 124 (4): 1541–91.
- Dufo, Esther, Rachel Glennerster, and Michael Kremer. 2007. "Using Randomization in Development Economics Research: A Toolkit." In *Handbook of Development Economics*, Vol. 4, edited by T. Paul Schultz and John A. Strauss, 3895–3962. Amsterdam: North-Holland.
- Dulger, Ihan. 2004. "Turkey: Rapid Coverage for Compulsory Education—Case Study of the 1997 Basic Education Program." http://siteresources.worldbank.org/INTTURKEY/Resources/361616-1142415001082/Compulsory_Education_by_Dulger.pdf.
- Ellsberg, Mary, and Lori Heise. 2005. *Researching Violence Against Women: A Practical Guide for Researchers and Activists*. Geneva: World Health Organization.
- Erten, Bilge, and Pinar Keskin. 2018. "For Better or for Worse?: Education and the Prevalence of Domestic Violence in Turkey: Dataset." <http://doi.org/10.1257/app.20160278>
- Eswaran, Mukesh, and Nisha Malhotra. 2011. "Domestic violence and women's autonomy in developing countries: Theory and evidence." *Canadian Journal of Economics* 44 (4): 1222–63.
- Farmer, Amy, and Jill Tiefenthaler. 1996. "Domestic Violence: The Value of Services as Signals." *American Economic Review* 86 (2): 274–79.
- Field, Erica, Rohini Pande, Natalia Rigol, Simone Schaner, and Charity Troyer Moore. 2016. "On Her Account: Can Strengthening Women's Financial Control Boost Female Labor Supply?" https://www.povertyactionlab.org/sites/default/files/publications/719_On-Her-Account-Can-Strengthening-Women_Rohini-Simon_Nov2016.pdf.
- Friedman, Willa, Michael Kremer, Edward Miguel, and Rebecca Thornton. 2011. "Education as Liberation?" National Bureau of Economic Research (NBER) Working Paper 16939.
- Goldin, Claudia. 2006. "The Quiet Revolution That Transformed Women's Employment, Education, and Family." *American Economic Review* 96 (2): 1–21.
- Gulesci, Selim, and Erik Meyersson. 2012. "'For the Love of the Republic:' Education, Religion and Empowerment." <http://pseweb.eu/ydepot/semin/texte1213/SEL2013LOV.PDF>.
- Güneş, Pinar Mine. 2016. "The Impact of Female Education on Teenage Fertility: Evidence from Turkey." *B.E. Journal of Economic Analysis and Policy* 16 (1): 259–88.
- Hidrobo, Melissa, and Lia Fernald. 2013. "Cash transfers and domestic violence." *Journal of Health Economics* 32 (1): 304–19.
- Huth-Bocks, Alissa C., Alytia A. Levendosky, and Michael A. Semel. 2001. "The Direct and Indirect Effects of Domestic Violence on Young Children's Intellectual Functioning." *Journal of Family Violence* 16 (3): 269–90.
- Imbens, Guido, and Karthik Kalyanaraman. 2009. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." National Bureau of Economic Research (NBER) Working Paper 14726.
- Imbens, Guido W., and Thomas Lemieux. 2008. "Regression discontinuity designs: A guide to practice." *Journal of Econometrics* 142 (2): 615–35.
- Jansen, Henrica A. F. M., Sunday Üner, and Filiz Kardam. 2009. *National Research on Domestic Violence against Women in Turkey*. Ankara: Elma Teknik Basım Matbaacılık.
- Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2007. "Experimental Analysis of Neighborhood Effects." *Econometrica* 75 (1): 83–119.
- Koenen, Karestan C., Terrie E. Moffitt, Avshalom Caspi, Alan Taylor, and Shaun Purcell. 2003. "Domestic violence is associated with environmental suppression of IQ in young children." *Development and Psychopathology* 15 (2): 297–311.
- Lee, David S., and David Card. 2008. "Regression discontinuity inference with specification error." *Journal of Econometrics* 142 (2): 655–74.
- Lleras-Muney, Adriana. 2005. "The Relationship between Education and Adult Mortality in the United States." *Review of Economic Studies* 72 (1): 189–221.
- McCloskey, Laura Ann. 1996. "Socioeconomic and Coercive Power within the Family." *Gender and Society* 10 (4): 449–63.

- McCrary, Justin, and Heather Royer.** 2011. "The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth." *American Economic Review* 101 (1): 158–95.
- NSDVW.** 2008. *National Survey on Domestic Violence against Women in Turkey*. Beytepe: Hacettepe University.
- Oreopolous, Philip.** 2006. "Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter." *American Economic Review* 96 (1): 152–75.
- Organisation for Economic Co-operation and Development (OECD).** 2014. *Society at a Glance 2014: OECD Social Indicators*. Paris: OECD Publishing.
- Rivera, Leonor, Bernardo Hernández, and Roberto Castro.** 2006. "Asociación entre la violencia de pareja contra las mujeres de las zonas urbanas en pobreza extrema y la incorporación al Programa Oportunidades." In *El Programa Oportunidades examinado desde el género*, edited by María de la Paz López and Vania Salles, 69–94. Mexico City: Colegio de Mexico.
- Simes, R. J.** 1986. "An improved Bonferroni procedure for multiple tests of significance." *Biometrika* 73 (3): 751–54.
- Smith, Sharon G., Jieru Chen, Kathleen C. Basile, Leah K. Gilbert, Melissa T. Merrick, Nimesh Patel, Margie Walling, et al.** 2017. *The National Intimate Partner and Sexual Violence Survey (NISVS): 2010–2012 State Report*. Centers for Disease Control and Prevention. Atlanta, April.
- Stevenson, Betsey, and Justin Wolfers.** 2006. "Bargaining in the Shadow of the Law: Divorce Laws and Family Distress." *Quarterly Journal of Economics* 121 (1): 267–88.
- Tang, Catherine So-Kum, and Beatrice Pui-Yee Lai.** 2008. "A review of empirical literature on the prevalence and risk markers of male-on-female intimate partner violence in contemporary China, 1987–2006." *Aggression and Violent Behavior* 13 (1): 10–28.
- Vyas, Seema, and Charlotte Watts.** 2009. "How does economic empowerment affect women's risk of intimate partner violence in low and middle income countries?" *Journal of International Development* 21 (5): 577–602.
- World Health Organization (WHO).** 2013. *Global and regional estimates of violence against women: Prevalence and health effects of intimate partner violence and non-partner sexual violence*. Geneva: World Health Organization.